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University Corporation for Atmospheric Research

TAPE RECORDED INTERVIEW PROJECT

Interview of Stephen H. Schneider

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Interviewer: Robert M. Chervin

Chervin: [This is] Thursday afternoon, the 10th of January, 2002. We're in a sitting room in the home of Stephen Schneider's, in Palo Alto, California . . .

Schneider: Technically Stanford, California.

Chervin: . . . surrounded by greenery, and we're about to begin the first session of the AMS Tape Recorded Interview Project. And just wanted to begin by thanking you for inviting me here. And to put everything in historical context in terms of full disclosure—as I often say, the two of us do go back about a hundred years—which is getting closer and closer to the truth.

Schneider: That explains your gray.

Chervin: Yes. Beyond gray, it's white now. At any rate, in terms of context, I'd be interested in having you comment on how you got first interested in the whole general area of science, engineering, technology, natural environment. What are your earliest recollections of those kind of interests?

Schneider: According to my parents' probably apocryphal stories, at two years old I was an escape artist. The neighbor called up to announce that I was out on the roof, that I had somehow managed, when having been imprisoned in my room to take a nap (which is the last thing that I as a two-year-old would have wanted to do), somehow I managed to get the windows open, and had crawled out on a ledge about forty feet above the ground, and was happily removed from there. The response to this—rather than not giving me naps—was imprisonment further. They put locks on my windows. So they then also put a lock on the inside of the door, and they put it high enough so I couldn't reach it. And according to my mother, I happily arrived maybe at two and a half or three down in the kitchen, having been locked in the room. And they could not understand this Houdini behavior, and I am told that what I did is I pushed the dresser over and used the drawers as steps to climb up to eliminate the latch. So I guess from day one I had wanderlust and could not be contained. And I had many supervisors at NCAR and other places who had to suffer through that malady of mine.

So, I always like to use, whenever the resources were hanging around me, to do whatever I like to do. But the first real memory I can recall that isn't apocryphal stories from parents (who were both pissed and proud), was that my dad got my brother and me a Lionel train set. (And only that we should have it now, what it would be worth.) But back then—this is probably early fifties—you know, when I was five, six, seven, something like that. And that I could probably have spent six hours a day in the basement working on those trains. Now my brother who's more artistic had the skills to design scenery and paint it and build houses. I was totally clumsy, all thumbs, you know, plus too young. But what I could do is I could wire those trains and I could design them, and I could figure out ways to get the circuitry to work and get all the lights to go. So we immediately divided the labor. He handled the art; I handled the technology. And it really was exciting to be able to do that. I still remember at that age what a kick that you could—if you learned the principles of how it worked, you could make it go. I also remember that I read somewhere how you make soap, so I asked my father, well can't we use that gas burner out in the backyard and boil the lard and get the lye and make soap? So we made soap one day. And I remember that the soap worked, but God with the fat that we used, I can't remember if it was pig fat or whatever.

Chervin: At what age was this? How old were you?

Schneider: Oh God, nine maybe, I mean plus or minus two. And the soap worked but it stunk, it was terrible. And all it told me was all that effort and I can't even do as well as a nickel bar of soap in the five and ten, and that therefore you have to have real skills to design something and make it work right. I think that was the first time I recognized that knowledge really mattered. That, you know, no matter how much you tinkered, and I tinkered with electronics, I tinkered with everything. I tried to build go-carts and racing cars. And I remember at fifteen we built a go-cart, and we couldn't afford to do it right so we didn't have brakes, so what we did is we put an on/off switch on. And basically when we wanted to stop the car [when] it was on full power, well, we just flipped the switch, which shut it off! And I remember my friend and I doing this driving around out in the streets (illegally of course), you know, making hard left turns and you shut the machine off right before you enter the turn, so you slow down and then you lean in the turn. And then accidentally, he was driving at the time, his knee hit the switch, turning the power on full right in the middle of the turn—no helmet, no nothing, skidded out, hit a tree. Amazingly enough, nothing happened to him or the go-cart. But those were the days when experiments were not lethal when you were lucky. I built all kinds of stuff. I remember with a different friend we got a hold of a twenty-two caliber rifle, we locked it in a vise, we took two tin cans.

Chervin: What age was this now?

Schneider: Probably fourteen.

Chervin: Okay.

Schneider: Locked it in a vise that you got from some father, it wasn't my house, it was his. And then got his strobe, connected the wires of the strobe to the two tin can lids which were about a half inch apart, and we put them, and we put a camera on a light bulb which was right behind the tins cans—or right after, the tin cans were right after the light bulb—and we locked the twenty-two, shot the twenty-two, you know with the lights out in the house, went through the light bulb, and as soon as the bullet exited the light bulb it hit the cans and it flashed the strobe. And we have these incredible pictures of y' know this exploding light bulb, you know with the strobe. I remember that was really exciting stuff.

Chervin: What were your parents professionally, and did they encourage these kind of excursions?

Schneider: Well, the last thing they knew about was our go-carts or our little excursions with rifles or the rockets I used to build in the backyard. What we would do is we'd go get very hard cardboard tubes, make my own home-brew gunpowder. I mean, why the pharmacist didn't turn us in—what is this twelve-year-old coming and buying potassium nitrate and sulfur for? The charcoal I ground up, you know, myself. I looked up in the encyclopedia what the formula is, so we made our own gunpowder and I figured I'd enhance it by throwing in match heads. And then we would get these fuses which you could get from the sporting goods store for these little homemade rockets that were around. So we'd get these light fuses, or we'd use a cigarette, which I'd steal from my mother, you know, so that we could get far away from the rocket. And then we'd shoot the rocket up. And we also would put firecrackers and cherry bombs in the nose of these rockets sometimes which would then blow up at the top of the arc. And these rockets would fly away and off they went. Only once at the age of twelve I performed an experiment with a gasoline can that was empty, and I was curious what the vapor would be like, relative to the gas. That was not a good experiment. Two hours later at the eye doctor, my cornea burned and scratched, I learned that sometimes you have to know what you're doing before you fool around.

Chervin: Were there any consequences? Were there any restrictions placed upon you by your parents or by the legal authority in __________? [two voices at once]

Schneider: Well, it was very hard for them to put restrictions on me because these were all clandestine. I wasn't stupid, I knew it wasn't going to be approved, so I didn't tell them. I did it when they were away, you know, and when I was home. So I was always doing these kinds of experiments. I loved doing that sort of thing. And I would take apart TV's. I would actually give myself electric shocks at different voltages to see what they felt like—what did AC and DC feel like? Good thing I was lucky and probably had rubber shoes on, you know, not too bright.

Chervin: Were there any role models in the families, any uncles, aunts that you recall who were active in technology?

Schneider: No, my dad is an economist and philosopher of education. My mother is an English teacher, and my uncle was a businessman. So there was—I was it for the experimental technologist. And one of the things that I have learned in reading the psychoanalytic masters like Jung is that children often fill the unfilled niche in the family. And I had fairly high-powered academic parents and relatives, and therefore it was very tough for a kid to compete, even though nobody is competing with you, in the niches that were already filled. So I went to the one that was empty, I guess. Maybe that was the psychological driver, but I was certainly—my curiosity kept getting reinforced, and my little inventions and games worked, for the most part, in a Rube Goldberg kind of a sense. But there was always an underlying frustration that my lack of expertise and access to resources was not making it as good as it could be. And it was as if everything that I was building was not quite up to the standards that I held for myself. So there was less satisfaction in it, and maybe perhaps a drive to keep trying again and again and again—something which continued to mid-life till finally I realized that this is neurotic.

Chervin: What about formal science training in either junior high or high school? I recall that you had pretty much of a suburban south shore of Long Island upbringing.

Schneider: Yeah, well I was you know the pretty standard kid. By the time I was twelve, thirteen I had a, you know I have a four year older brother who had got his junior license and a car. And my interests switched quickly in the 1950's to hot rod building. He was the driver, I was the mechanic. My parents were the intellectuals who frowned on all this as low brow activity, and I was just, you know, pre-hormonal and beginning to bounce, and what's the last thing a kid is going to do when he gets hormonal is to do what the parents want. So therefore, as far as I was concerned, school was an utter waste of time. About the only thing it could do of value would be to teach me how to make a better racing car. And since I wasn't going to get that out of any of my teachers, I basically cut half my classes. I did whatever I could get away with. Sleeping late, well, see they both worked in New York, my parents, so they didn't know what I was doing, and it was a very simple system in high school. If you were late, you paid the price by detention study hall at the end of school.

So I decided hey they've got a—it's a perfectly good *quid pro quo*. I'm late, I am punished. And then what I would do is I would do all my homework in the study hall after school, and then I would never bring books home, which would entirely infuriate my parents because they were thinking I was doing nothing, not knowing that I had stayed after and got all of it done. So this worked perfectly for my, you know, rebellious adolescent behavior. And then I kept building more racing cars and, in fact, my brother and I won trophies at the West Hampton Drag Race. I still remember we had a fifty-six Oldsmobile from my grandfather and I knew that the idea was not to be the fastest car, but to be there *first*. So I studied principles, you know, and I was probably in physics at the time and said, hey, what you want to do is you want to get the gear ratios best. [Phone rings]. Hold on one second, just put it on pause.

So let's pick up again with the racecar bit.

Chervin: Yes.

Schneider: So I remembered—I had ridden bikes, and I never—we could never afford to get bikes with gears, so when I got into high school my great present was a three gear bike, you know one of those old Raleighs. And immediately I understood that spinning it faster meant that you could accelerate quicker but couldn't go as fast. And then of course in physics I started to understand those principles. So what I did is I went to the manuals in the Oldsmobile plant—the Oldsmobile dealership and discovered that the—I still remember this, the 5298 convertible had a 3.9 ratio rear axle, whereas the standard rear axle ratio was 3.23. So that meant that the engine would spin more times for each revolution of the rear wheels, therefore it would generate more torque, it would accelerate faster. It would limit the top speed of the car, but that wasn't the idea in a drag race, it was to get there first, not to be the fastest.

So I leaned early on that you have to optimize to the objective function in front of you, and not to somebody's notion of what's cool. And so we went to West Hampton and here are these two nerdy kids, you know, me a fifteen-year-old slightly pudgy, borderline pubescent mechanic who didn't look it at all, and my brother kind of a shy, thin guy—not at all the greasy types that you expect in the American Graffiti movies who were our competitors.

And our competitor guys had loud mufflers and noisy things and cars that went shiny and fast. And we had this beat down old Oldsmobile that we had of course shaved the heads to make the compression better and all that. And I even admit a little larceny. They are supposed to be stock. We made all the modifications you couldn't discover, like the rear axle. And what would happen was, we'd start out down the road. My brother would be four car lengths ahead of these guys. They were in disbelief, and by the end of the drag race they were roaring up at high speed ten miles an hour faster and he'd nose them out to the end. Because we had the rear axle. And we also bought butyl rubber tires, which were available at the time, so that the wheels wouldn't spin. They liked spinning wheels and smoke because it was cool. I said, wait a minute that's wasting energy—you want all the energy to go into the brute acceleration of the car. So we got four trophies at West Hampton. Of course my parents knew nothing about this. They just thought that we went out to the beach for the day, but I still have one of those trophies floating around here now. My brother and I laugh about this occasionally.

So those are the sorts of things that I would do. It was not formal science education. My parents had a different idea when they saw my having interest in science. For example, I would watch the programs on TV then like "Hemo the Magnificent." You know, it was a cartoon science creation, when people were beginning to discover microbiology. It was utterly fascinating to me. I could have watched it again and again and again, but there were no recording machines. And I was absolutely fascinated by the space business. And in the eighth grade I tried to make rockets, you know, for my [science] project. But they didn't want you to make a rocket that worked. I already was doing that. My rockets were ugly, but they flew a hundred feet in the air and they had firecrackers blew up. I mean they were functional, they just didn't look very good. So in the eighth grade back in those days you were supposed to make it look like the Vanguard, and so I was a lousy artist. So I was not getting good grades for my science projects, because they were fluff and I didn't want fluff—I wanted things that worked! And I found myself almost always disaffected from school.

The first time I can remember enjoying a science class, was my high school physics class when I was sixteen. And the teacher was terrific. I am trying to remember his name, Paul something. His name will come back to me later. In fact I tried to track him down when I went back to Lawrence High School about five years ago, and they said that he had retired and asked that his address not be given out, which was too bad. I would have liked to thank him for, you know, for letting me go to physics twice a day, I enjoyed it so much, while cutting English or French which I considered a waste of time. And he could have gotten in trouble for that, but maybe he was flattered. A kid actually wants to go to his class twice. And I remember then, that was the first time that I found things academically exciting, because what physics was doing is it was giving me a formalism to express what I had already discovered heuristically by experimentation. And that began to bring in the sense that rigorous analysis can actually eliminate that frustration I had by these heuristic devices that I would build that would work, but would always be below what I thought high quality was. And that probably was the beginning of my formal interest in science and technology, rather than just the informal "build it and hope it doesn't blow me up" kind of background that I had as a single digit and teenage kid.

Chervin: As I figure it, you were graduated from high school in 1962, and then went on to the engineering school at Columbia University in New York City. How did you pick engineering? How did you pick mechanical engineering, and how did you wind up at an engineering school at an Ivy League institution?

Schneider: With my grades?! These are all very interesting questions, Robert, very interesting. Paul Johnson, that was his name, the physics teacher—he's the one who told me to go to engineering. And I said to him, engineering, I'm terrible at math. The only red grade I ever had in my entire high school transcript, despite never taking a book home, was a sixty I once got in one of my geometry classes. Of course, what I forgot was if you don't go to class half the time, and you don't turn in homework half the time (I did it about half the time), that they don't give you good grades, and also you don't learn that much. So I thought of myself as not very good in math, but good in science although it turned out, he said that's not true at all. He said, you're fine in math and that you should think about engineering.

At that time I had a friend, Billy Schwartz, and I didn't think that he was any smarter than I was, he was a contemporary. In fact, he and I had done some crazy things together like the twenty-two rifle routine. And he applied for early decision to Columbia, and his grades weren't much better than mine. My grades basically were about a low eighties average, which was made up of somewhere between ninety-five and ninety-nine in Science and History, and seventies in Math and English and Language. You know, I did what I liked, and didn't do what I didn't like. And when he got into Columbia early decision, with his grades no better than mine, I said, wait a minute, this can't be. So I found out that the reason that happened is that Columbia was in its second year of accepting four-year undergraduates. I think Chervin, was that his name, was in the first class.

Were you in the first class, weren't you—or was there one class before you?

Chervin: I think there was one class.

Schneider: All right, so you were in the second class, so I would have been the third class.

Chervin: Yes.

Schneider: The class of sixty-six. Maybe there was a class, yes, that's right because Ken Harris was class of sixty-four.

Chervin: 1964.

Schneider: Yes, so the engineering school used to be a three/two. You'd have three years as a liberal arts undergraduate, and then you would switch into two years of Engineering. They had just started four-year classes. As a result of that they needed students. So there was this pure random event window of opportunity. They hadn't yet got all the applicants they needed, so there was actually a chance to get in. So because Billy got in, this was so offensive to me, you know, that he could get in to this place, and I guess all that pressure from my parents to be an academic elitist, you know, had been leaking in, so I scheduled an interview at Columbia. And I went there and I still remember the guy, his name was Plaut, the admissions officer. And he looked at my record and he says: oh, so you are the kind of person who excels at anything he wants to do, but doesn't always have discipline.

And I said, yes, sir, I think that's a pretty good honest evaluation of me. He said, I think you could make it here, but you'll have to want to. He said, and I need to know that you want to. I will make you a deal, he said, you haven't taken much math, but you are a smart kid. You have got eight weeks, if you sign up tomorrow, to take the advanced math achievement test (and I had had no advanced math classes). He said, you study it on your own (which you have to do in college anyway), you get over 600 on that test, I will admit you.

I owe this guy a lot. So I did it my usual way. I waited, I waited, I procrastinated, I looked at the book. My parents yelled at me that I was irresponsible—when are you going to do this, when are you going to do this?! Finally, I got a tutor in the neighborhood, about one week before the test they give me a test, they tell me it's hopeless, I can't do this. Nobody tells me anything is hopeless, so maybe they were just really smart strategic characters, so what they did is, I said, give me the book, and does the book have the solutions to these problems? Yeah. So they gave me the book and they gave me a couple of math texts. I was motivated; I probably worked four hours a night for the whole week and took one more practice test. They couldn't believe it, they said, my God you might just make it. Here, and they gave me some pointers and tips about how to be faster and so forth. This was the night before three hours of tutoring. God, that was my whole college career, doing everything the night before. And so I set my tone then, and I took the test and I still remember to this day I got a 637 in advanced math without ever having taken an advanced math course, and Plaut kept his word and he let me in.

I remember the first day at Columbia, when we all lined up and Dean Vreeland looked at us and said, look to the person to your left, and look to the person to your right: one of you isn't going to be here. And that's not like Stanford, where you can't get in, its impossible to get in, but once you get in you can't flunk out unless you, you know, do something very untoward and never show up in class for three years. I mean, they were out to bust out half the people, and they did.

Chervin: And so was Columbia the only option or had you applied to a handful of other schools?

Schneider: Oh, I got rejected at Cornell, and I got into Harper [Harpur College?] in the City.

Chervin: Okay, and were those engineering programs or were those—

Schneider: Yeah, I think I was going to apply to engineering programs. I wasn't completely sure I wanted to be an engineer, but I had a lot of rapport with Paul Johnson, the physics teacher, and he assured me, plus my 637 assured me that he was right. You know, hey—I spent one week studying mathematics and I just made up for three years worth of being a jerk, and not showing up and studying the night before the final and getting a 70. So I decided maybe I could do this after all and math wouldn't be my prime thing, but I could get by.

Chervin: And how did you pick mechanical engineering, was it?

Schneider: I was going to design . . .

Chervin: Because it was close to auto mechanics?

Schneider: Yes! I was going to design the world's best racing car. You know how long that lasted? Till my first mechanic design class, which I thought was ditch water dull, and I was really interested and excited in physics. I had Leon Letterman, now who is a Nobel Prize-winning physicist and a very good friend, because we are both interested in science education and science policy. I have done actually—I have some funny stories about lecturing for Leon at Argonne Labs when he was a director, about ten years ago. And I loved Leon's physics class. It was the only—I couldn't believe I was getting up at 9:00 in the morning to go to a class. I had never gone to a 9:00 in the morning class in high school in my life, I just cut and took detention. And that changed my view. I said, you know I don't want to do straight mechanical engineering. And I went into what they called "Mechanical Engineering Science," which was basically applied math and physics; a lot of EE courses, a lot of courses in the physics department. And I got a very rigorous undergraduate education, heavy in math, which was not my best grades, but I learned it.

Chervin: And so as I figure it, in 1966 you did get a Bachelor's Degree in the mechanical engineering, and you somehow wound up continuing at Columbia in the graduate program in mechanical engineering.

Schneider: I met Don Chu, C.K. Chu.

Chervin: And was that . . .?

Schneider: He was plasma physics and applied math.

Chervin: And was that when you were an undergraduate still or in your senior year?

Schneider: I think it was in my senior year, and he struck me as a really cool guy to have as an advisor. He was interested in basketball; he was interested in politics; he was interested in how science made a difference in the world, not just the science for its own sake. And at the same time he was highly rigorous, because he was a Courant Institute-trained numerical analyst. And he had skills where you could solve equations that would actually let you understand nature or build better devices. And I thought that was all pretty neat and I was kind of hoping that I would go on, and work with him. So I stayed in the department, plus I was about to get married and therefore I wasn't going to go anywhere, and my then-girlfriend was going to be a senior at Barnard, so I said I would stay around and do that. And moving into the plasma physics direction, which I didn't make a firm decision to do until probably my second year in graduate school.

The first thing you had to do in that department was take qualifying exams. And qualifying exams are nothing like they are today. I mean when I think back what Columbia did, it was high stress, almost inhuman. What they would do is they would admit to Ph.D. program, but not candidacy, oh, maybe ten-twenty students. Then after about a year and a half, you know, when you've—I had finished my master's already, you had to take the Ph.D. qualifying exam: eight hours, closed book, all the classes that you had taken in—had questions on it, now you didn't have to answer every one of them, maybe there were thirty questions, you had to answer sixteen, so you know I didn't have to answer mechanical design questions. Everybody had to answer four stars, stars were applied math, and I was pretty advanced relative to a lot of my colleagues in that.

And basically what they did is they flunked two thirds and about half of the flunkies might be allowed to take it again. We are talking Vietnam days. Flunking out of school meant carrying a rifle or however else you wanted to try to deal with that situation, so the stress was unbelievable. And I bit my nails to the bone. I switched my habits; I probably studied eight hours a day, not just my classes. By the time I was a senior I was pretty much all A's; before that I was not even close. And in graduate school I can't remember any B's. And I studied so intensely for that that I ended up being number one. Got through it the first time, and that's when I started to change my perception that I was a mediocre student, that I actually could really do well at academic things. But it took something, you had to bust your butt.

One thing I forgot to mention, I was interested in—I was fascinated by astronomy. And my dad understood the frustration I was having with not having really good quality instruments and things I built myself. He said let's go get good stuff. So we drove down to Edmund Scientific in Barrington, New Jersey, and bought a tube, bought a four-inch subjective lens with a sixty-inch focal length, bought the rack and pinion, bought all the equipment. We took it to a machinist who cut it and trued it and made the holes so that we could screw it in. We painted the inside of the tube black, bought a mount, and I had my telescope. And I remember me, the guy who could never possibly wake up in the middle of the night, waking up, setting my alarm to go out and see Jupiter. So I set it for like five in the morning, go out in freezing weather, its wintertime, it was probably around my birthday, so it was in February, I was thirteen, thirty degrees outside. I am out there, and . . .

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TAPE 1, SIDE 2

Schneider: . . . on the 10th of January and I was in an astronomy aside. Right now the tape must have gotten past the plastic and onto the real magnetic part, okay. So there I was at four in the morning, you know, at thirteen years old, not even shivering, all excited. The very first use of my telescope, other than, you know, looking at the neighbors' windows. And there is this non-twinkling planet which I presumed is Jupiter, although it didn't look as bright as I thought it should be, and I, y'know, adjust the telescope, and get it in there and I start turning the focus and, you know, waiting for the Galileo moment of seeing the moons of Jupiter, and this particular Jupiter had rings. I was so excited I was beside myself. I had seen Saturn, now I had seen a hundred pictures of Saturn, but the absolute thrill particularly since it was blurred and went in focus. It was a Hollywood moment by sheer accident—that that came out and that was so exciting, it just drove me nuts. And I think that was part of what was lingering underneath as an interest in science, not just technology. Plus my telescope was well built, it was machined, it had good parts, I didn't build it myself, which was against my principles but damn-it this one worked. And this was a good machine and I used it all the time. I found nebulae, I then found Jupiter later. I had a really good time with it.

Then my parents said, oh what you need is to go to the Junior Astronomy club. So they dragged me from Long Island to New York. I went to the Junior Astronomy Club, filled with kids who were in high school, Bronx High School of Science and Stuyvesant. What a bunch of nerds, all they were doing was studying valances and chemistry and all this stuff that I was totally uninterested in. They destroyed astronomy for me and in less than two sessions I wouldn't go back. It lost all its interest, it had no context.

Chervin: Very interesting interlude into the science of astronomy. Now did you ever compare childhood experiences with, perhaps Carl Sagan?

Schneider: No, not about that, I never talked to Carl about that kind of stuff. Just found it fascinating and he and I spent much more of our time talking about popularizing science and saving the world and that will come later.

Chervin: Okay, I am sure it will.

Schneider: The other thing that I recall is, when I, earlier than this, when I was eight, I forgot this too, because nature was an important part of me. I would go crabbing on the dock all the time, and my brother and I, and it just amazed me that we could put bait in these traps, drop them down into the murky dark water and up came the blue claws, they were in there. And that I found kind of fascinating. And I also was just beginning to think at the time you know you could get them at some tides and not at other tides, and I was starting to think about how nature worked. And there was a patch of woods still left in this Long Island suburb, that was only twenty miles from New York City, and only a few miles from the Queens border. And this patch of woods was owned by the water company, so they still hadn't developed it yet. Maybe they were trying to have a clean watershed, but you know one square mile amidst the pavement and the housing developments of Long Island in the 1950's. I called it the deep dark forest, and we're talking, you know, a full old growth, y'know, hundreds of year old with hundred foot oak trees and maples and all that. And we used to walk through that woods. I knew every path in that woods. My parents would drop my brother and myself and some friends off there, and we'd spend the day, we'd take a picnic lunch, and it was just, I 'd get dirty as hell crawling in the mud, looking at the ants, doing all this stuff.

Then there was a hurricane. The hurricane came and it blew down half the trees. Oh, I was, you know, a kid in heaven. I was looking at all these giant root balls and the damages that were done, and trying to figure out, you know, how things would grow back. And then the next year I'd see saplings coming up in the old growth. So I was getting a beginning of practical ecology, and little did I know that my conservation ethic was soon to be born, because the next year they locked it off and it was knocked down and turned into Woodmere Park, into a housing development. And there went that patch of green, that, you know, that one area where this young child was able to have transcendental moments with nature that were certainly less important to developers and economists than development dollars. And I didn't know it at the time, but that spirit was going to return with a vengeance later on, and I'd spend a great deal of passion trying to protect remnant patches of forests and other things from not just developments, but from having their climatic niches driven out from under them by human excesses.

Chervin: Okay, shall we return to the graduate school experience?

Schneider: Okay, so end of early childhood aside.

Chervin: As I recall your thesis topic involved fairly extensive, at the time, computational effort, involving the numerical assimilation of an MHD shock tube to try to get some understanding of some experimental results that some plasma physicists found in the lab at Columbia. How did that particular thesis area present itself to you, and how did you get involved in computational activities?

Schneider: Well, I made the selection of John Chu as a thesis advisor, not based on the fact that I had passionate interest to be a plasma physicist or a numerical fluid mechanics person, but because I liked his style. I liked the way he did rigorous science, yet cared about its applications in the world. He was an applied math, applied physics person. He once told me if what you do doesn't make a difference in the world, what good is it? You know, and that was quite consistent with the early passions that I had developed, you know, seeing that forest cut down. You know you have to do something to stop that. I didn't just want to discover something just for its own sake. Yes, I was curiosity driven, but I wanted what I did to make a difference in the world. That was always a deep and abiding passion in me. I remember, for example, this is an aside from the question of the graduate school, but it will lead you to why I liked John Chu, and therefore ended up with the problem that he happened to have had a grant to do. Just the short answer to how I got to do that problem: He had the money; I was his graduate research assistant. I had a 2-S deferment; I wasn't prepared to look this gift horse in the mouth! And I had no way to pick a better problem on my own, so I did the one he assigned me, although I—he didn't—once he assigned the problem I was kind of on my own to do it.

But the old age gray matter, let me see—I was going to tell an aside, oh, yes. My parents, the literary types, loved the theatre, and one of the advantages I had, I was not rebelling by the time I was in graduate school, so when they offered to take me to the theatre I would go and was happy to see them. So they took me to the see the Bertolt Brecht play *Galileo*, and the scene that I am about to describe I now teach in my freshman seminar, "Environmental Literacy." Anyhow, the New York State Theatre had just opened up at Lincoln Center and *Galileo* was being played by the very famous young English actor (oh, hell, put it on pause while I think of the name). Anyhow I believe this was the inaugural year of the New York State Theatre, probably mid-sixties, and I liked Bertolt Brecht, you know the theatre of the absurd, but this was not an absurd play, this was a serious play. And I think my folks knew that I'd like *Galileo*, because it was about the church/science conflict. And this y'know new young actor Anthony Hopkins, you know, that was just beginning to become heard of then was playing Galileo.

And I watched the play and Galileo and his monk friend—whose name I forgot, and although I should remember since I have been teaching this lately—build the tube, the telescope, and they see the moons of Jupiter, and they repeat the excitement that I had when I looked at the rings of Saturn, except they had a bit more political implications, because when they looked at Jupiter and saw the moons, which they didn't know were moons at the time, they thought they were other small planets. And then they noticed they moved. And then they noticed that the four moons, one would disappear for a while and then come back. And it was soon clear that it was going behind the planet. And Galileo was beside himself with excitement, and his great and curious scientific friend the monk was in deep depression. Because they both realized that they had just shattered the celestial spheres rationalization of the Church—infallible doctrine. And they then had a great philosophical debate, and Galileo could see absolutely nothing wrong whatever in explaining the discoveries of the great creation of God. And the monk knew right away that this would be challenging the wisdom of the leaders of the Church and that, what but the people have nothing else, argued the monk—I mean we can't take away from them their confidence in the mother church. And Galileo said, but the information is of the wonders of nature. What could be a better testimony to God?!

And I remember at the time clearly which side I was on, and that anytime you make discoveries which fly in the face of power, you will be shown the torture instruments. It was a very strong and moving play for me, and that's one of the reasons why I think I was gravitating toward John Chu, because he was entirely furious at the political situation at the time, ranging from Vietnam to Civil Rights injustices, and was very encouraging about doing things. He and I and his daughter Barbara went to canvass for Eugene McCarthy. Of course that was later—no, in sixty-eight, right around the time when I'm taking my qualifying exams before I picked my Ph.D. topic. And I remember we went to a small labor town in New Britain, Connecticut. And I had a lot of success in canvassing, we split up, and he had a lot of trouble, until I realized that what he was running into was good old American racism. He was running into a case of "yellow peril." And when he and I went together it was a different thing than when he went alone with his daughter. It was really very disconcerting to me to observe that from the American working class, when we were canvassing, different reactions that we got.

Anyhow I think that's what pushed me toward Chu, and I was going to work with this guy regardless of what topic he suggested. And he said, well you're good at numerical methods. And in fact I should have said first that Barbara Casmin—who became my first wife, who was a senior at Barnard at the time—and I, when I was a first year graduate student, took a four and a half point two semester (four and a half points each semester) class in the math department, a graduate course in math. I never thought I would *ever* take a graduate course in the math department. All the math courses I took were engineering math, where instead of theorem proof you got problem solving. It was rigorous, but you weren't caring about whether or not the equations were right, you were figuring out how to solve them. And I liked that a lot, and I was never enamored at all with the approach of the math department.

And I still remember Sergei Lange, had to take a class from Sergei Lange with his notes, all theorem proof, theorem proof, Sergei Lange taking out full-page ads about how horrible the American administration and value system was. And then in the riots, they said they disturbed his concentration after he helped create this consternation, and then he went off on sabbatical, I mean what a hypocrite. But anyhow, I was totally disenchanted with that math department.

But this class was wonderful. It was taught by a guy named Pat Sturbens who was an IBM adjunct professor. And what he did was theorem proof numerical methods in the class, and the homework problems were solving what we had just learned the theory of in the class on the computer. It was the single most important course I ever took in school. Because it taught me a trade, and the trade was how to solve partial differential equations on the computer. And later on I learned I could solve those partial differential equations in climate, in plasma physics, in ecology and in economics! And that is a skill that has stayed with me ever since, for which I am eternally grateful for that phenomenal, you know, rigorous class in numerical methods.

So I took that class, first couldn't believe that I actually got an A or an A+ in a math class, in the math department! And that I took in the first year and then there was John Chu, a numerical methods person, saying you've got the skills now, why don't you come and do this numerical problem—which is to simulate in 1-D the behavior of the shock tube in the basement, mock twenty thousand electromagnetic shocks, first as a magneto-hydrodynamics problem and then later as a two-temperature, that is an ion and electron, you know, fluid separate equations. And basically that meant that I had to learn something brand new. I had to learn MHD, I didn't know anything about it. So I took the graduate course in NE&M, Clad Jackson, Classic Electro-Dynamics. Had already taken the Carslon Yeager, that was the advanced heat transfer class, which was also solving partial differential equations. But it was solving it the old-fashioned English way. You were solving them analytically; you were leaning about greens functions, learning about how you use linearizations to deal with these problems.

Later on of course, that was wonderful preparation for making me learn how to think. Now they have fancy names like "adjoint methods," but of course I view them as a waste of time, just solve them numerically and solve the whole non-linear problem. What are you doing all this stuff for? People in the, you know, Victorian era didn't have computers so they had to have tricks, so—but anyhow it was very good for training your brain so I learned the—in heat transfer how to solve these equations and I had learned in numerical methods how to solve them by another way. I took advanced partial differentiation equations from Chu, and E&M was pretty easy. You know everybody said how hard it was—I loved it, I thought it was really interesting, you know, good stuff. And that gave me the background to where I could numerically solve my magneto-hydrodynamics equations. In the beginning, I didn't know what I was doing. I was a programmer who knew numerical methods, and I was solving the equations that I was given, in a few papers, for how the shock tube worked. But then the more you play, the more you learn, and then you start doing sensitivity analysis, and varying parameters, and changing things, and pretty soon you start understanding what you are doing. It took me about a year, year and a half.

All of this was taking place—my qualifying exam, my choosing of a thesis topic—in the middle of my second year in graduate school, which happened to have been the spring of 1968 at Columbia, what an interesting time. There was a riot; there was a protest. I was politically oriented. I opposed the war in Vietnam. I agreed with a whole number of things the protesters said.

Chervin: There was an occupation of several buildings on campus.

Schneider: Yes, there was an occupation, I was not part of that. What I could not handle was the "you're with us or you're against us" mentality of the Students for a Democratic Society. I couldn't handle their willingness to rationalize the black occupation as a racist separation that these people have been oppressed and are therefore entitled to be counter-racists—I said no, they're not. They 're no more entitled to anti-white racism than we are to anti-black racism. So I found myself in a small minority of the campus that had sympathies with the goals of the protesters and had zero patience with their intolerance and their rigidity. And I had equal impatience with, you know, we called those "the pukes"—they were so supercilious and sanctimonious, they were disgusting. And then there were the jocks on the other side, the "alrightniks", you know, waving the flag and wrapping themselves up like George Bush today in patriotism, in opposing that, when I thought the basic causes of the protesters were fundamentally correct, but that this mindless "you are with us or you are against us, it's all one cause, and we determine the methods" was a bunch of garbage! So this was what was going on at the time.

So I sat there in fascination, attending the Archibald Cox hearings, because my classes were either canceled or turned into pass/fail, so I knew I wasn't going to fail. To this day I am still lousy at statistical mechanics because my statistical mechanics course in the Physics Department was taught by a left-wing radical who said that none of us should go to class, we should be out there on the barricades. But some of us said we still need credit for this class, so he made a deal that, you know, if we did homework he'd give us a pass. So I had a fair amount of time, and I think that was the only class I was taking then, and I, you know, listened and learned everything I could learn about this. Then the trustees decided under pressure that they were going to have a committee to restructure the university and create an academic senate to deal with grievances. This was their attempt to try to hold the campus together without giving in to the demands of the radicals. And the engineering school had a student council, and it put up a slate of people, and I read what they wanted to do, and I thought it was ridiculous! It was alrightnik; it was business as usual. It wasn't calling for true change. It was calling for getting back to, you know, to quiet as quickly as possible. And I wasn't against quiet, but I was certainly against ignoring the real and fundamental issues that underlie the troubles that still were there. I thought it was just as ridiculous to throw bricks through bank windows because there was a police riot, and believe that, you know, the whole world was against you, as I did to think that we should go on and do nothing more than make money. I felt like a lost generation. I was in a fraternity when I was at Columbia.

Chervin: As an undergraduate.

Schneider: As an undergrad. And the dominant paradigm was getting into the Blue Key and the Van Am Societies, you know, the service societies that were the college equivalent of Rotary and those kinds of clubs. And why did people get into them, because the Harvard Law School admissions liked them. I mean, get into it because it does some good. I mean, what attitudes people had. Well, they would take gut courses so they could get into Columbia Medical School. You go to college because you have an opportunity to work with these great professors. You are at an Ivy League school, and they are taking guts to get into medical school—my God, these aren't going to be my doctors, these aren't intellectuals, these are mechanics. You know, I was really offended by that group, and then in 1968 I go back to my old fraternity, and these students who, all they want to do, is taking a shit was political. They want to throw a brick through a bank window because it was a police riot. It was like—I felt like this lost generation between the group that thought being a Wall Street lawyer or a gynecologist was the only trip for life, and that's why you went to school, and the next group who thought that if you weren't tearing the society down you weren't doing any good. You know, I really felt isolated, you remember that, it was a very narrow window for which you could actually not believe that you had to go to school for the purpose of getting wealthy and having three cars, and at the same time going to school was not a mechanism for the destruction of society.

Chervin: And the age difference was about two-three years.

Schneider: Two-three years. It was—we ought to really do a play on it or a movie. Because I don't think people today could understand what a radical change in values there was, and that, you know, the class of sort of '65, '66 and 67, that was it. That was the transition generation between the fifties values of two refrigerators, cars and suburban houses and the late sixties, early seventies values of rejecting society.

Chervin: It was a radical, rapid paradigm shift.

Schneider: That’s right. I mean I could play the Bob Dylan songs, you know, about you know, get out of the way if you don’t lend a hand, and understand that, yet at the same time I didn’t think that radicalism was the right answer. I thought that fundamental value change is the right answer, but it should be evolutionary to prevent violence, and that was the view that I had. So I read this engineering school palaver from the student council. They were looking for calmness and for, y’ know, reminded me of the “let’s get back to normalcy.” That wasn’t the point of this, this was to take the opportunity brought about by the radicals whose agenda was fundamentally right, but whose methods were inappropriate—so let’s take the opportunity that they gave us, and let’s therefore use this historic chance to restructure the university in a positive way and get quiet, but not normalcy.

Chervin: The real word by the way is ‘normality’—it was that party was an idiot and didn’t know any grammar.

Schneider: So I wrote a one pager, which said exactly that—that this is a historic opportunity for change; we should not be going back to business as usual; at the same time, we have to observe reasonable and civil methods, because when people are tearing at each other and insisting on absolute values the only thing that one can do in a state of absolute values is subjugation of violence, both of which are bad, so let’s have some negotiations and let’s set the senate up. I put this in the box of every engineering student. So here was the slate of official candidates, and there is this one guy who put a flyer on the walls and in everybody’s box, guess who won!

Chervin: You.

Schneider: So I was the engineering school’s representative to the Temple committee. The student council was hysterical and furious—who are you, you can’t do this, well, I certainly can do this, not only can I, but I won. So I then spent this transition second year in ‘68 and then, late in the fall of ’68, when I was still just being a programmer programming MHD equations I didn’t understand, and not very excited by it, sort of finished with classes and too early in my research to know what it means. And I spent about half my time, became vice chair of the committee, met with Allan Temple the president of Chase Manhattan Bank. A nice old guy, initially it was, you know he couldn’t tell us from SDS. He told really fast, I brought him back to the A. E. Pi Fraternity with all those kids wanting to bite his head off. He came with a three-piece suit, there was no communication, and the whole thing was hostile, it was really bad.

And about two weeks later I said, Allan let's do this again. I said, do you have a polo shirt? He got it. He came in a polo shirt, he was looser, students backed away, he came again, it was progress. They actually started evolving a conversation—it was really nice to watch that happen. So then he invites me and the chair to go to the fifty-eighth floor of the Chase Manhattan Bank building over Park Avenue and to have lunch and discuss, you know, the restructuring of the senate. And there we're sitting in this boardroom, now he is in a three piece suit because it's his lair, looking down. I'm just wearing ordinary clothes, looking probably a little nicer than ordinary, but not tie and jacket, looking down at the taxis which looked like model railroad cars below, and having old black men in red bellboy suits come up to serve us and ask me if I wanted my shoes shined. So I had to take Allan aside and explain to him that this metaphor would not be a good way to, for him to be credible in negotiating with the students at Columbia, because that's precisely the image that they had of the trustees and why they were so alienated. That this kind of image was alien to us, and we felt represented a poor system of values, and while I understood that that was his culture and his generation and his particular environment, and I was able to say that and he was able to take it. It was really a very interesting experience.

The other thing I remember about that—I still can identify this as a loss of mentor syndrome—is there were some professors who were very famous who had said really good things during the riots, but when I came to negotiate with them, to help set up the senate, I discovered that just because they said wonderful things and they had ideologies I liked, they could be egotistical assholes of the first rank. And that other people who were less articulate and maybe didn't share my values could be really decent human beings when you work with them closely, and be honest and open. And it was a really major lesson that I learned in the year and a half-two years that we created the Columbia senate in the Temple committee. Of course SDS just kept calling us illegitimate negotiators with illegal authority. And they refused to participate and they essentially took themselves out of the picture. And we were able to effect real change that was made possible by the bloody heads of the radicals, and I never felt very guilty about it. You know, they wanted to get their heads bloodied and what did they expect, a bunch of rich kids protesting against American patriotism, when you had a lower middle class culture of police brought up in parochial schools under discipline—well they think they aren't going to swing batons and hide their badges, what did they expect?! I mean I knew that was going to happen, as soon as the police started on campus I left. I mean you are not going to help the world by getting your head smashed. And on the other hand, I admit if they hadn't made that sacrifice we might not have had the, you know the capacity to get enough apathetic people in the middle on board to restructure the university. So Columbia was one of the first in creating academic senates and some democratization of the academic process, which has remained to this day.

So I had a hell of an experience in graduate education in practical politics and institution building, as a result of the riot occurring just at the time that I was between classes and research. I had finished qualifying exams in the winter. This happened in the spring. I was done with classes, and I was programming all these equations I didn't understand. So I would do them half-time and spent the other half-time doing this. And John Chu approved, because he thought that this was an important thing for me to do at this time in history, and he had exactly the same feeling. The one thing I remember is I already learned about redundancy. I knew that students had taken over the offices of professors and occupied buildings, and I had two and a half boxes of punch cards that I had put my life into, and you can bet, in spite of the fact that I knew it was wasting paper, that every week when I had more than ten or twenty cards changed, I made a spare copy of those punch cards, and brought the box home, because I never knew whether my building was going to be taken over, and in some act of hysterical paranoid rage, the radicals were going to overturn all these cards in the room. Because they had done it in a number of places, like that lunatic who bombed the computing center in Wisconsin, and somehow rationalized that as an act of political courage. We have seen examples of that more recently.

Chervin: Right. And so at what point were you able to reduce the level of your political activity and actually devote full time to thesis work?

Schneider: Oh I can't remember exactly—I basically started fading out because it wasn't necessary. The other members of the committee, you know sort of rubber stamped what we two primary guys, I guess it was Roy Bercaw who was the chair and I primarily did, and we had a few other people. The president assigned a woman whose name I forgot, she was Swiss, to you know from the university to help, so we literally had an administrator who was keeping records on all these things. And we were able to, you know, with the negotiations with Allan Temple and with people from the university, help fashion a plan. I think by the time they implemented the senate I was pretty much out of the process. I was sort of there at the creation. By that time, I kind of understood what my thesis meant, and it was getting pretty exciting. I guess it was by '69 that I pretty well was now working twelve hours a day on magneto-hydrodynamics.

Chervin: And what about interaction with either the faculty . . .

END OF TAPE 1, SIDE 2

TAPE 2, SIDE 1

Chervin: This is tape number two, it's about 5:40 on the tenth of January. We are picking up where we left off. And at the time you were back fulltime on your thesis work, to what extent did you have the opportunity to work with faculty and other graduate students and post-docs who were actually doing the experimental work on the shock tube and compare the computational results with their experimental results?

Schneider: Now remember I was trained in mechanical engineering, applied math, applied physics. I knew nothing about plasma physics. I didn't get to take a course in plasma physics until probably the second half of 1968 and the first half of 1969. So I was solving these equations in a medium I didn't understand. I would then go to the plasma physics seminars, you probably went to them, and I didn't know what was going on, it was very frustrating. I mean, this was good training for the fact that I was going to change careers three or four times later on and I was going to learn all kinds of fields that I didn't know.

And one of the things I learned early is that you can learn a field from text books and basic papers, and I was reading them. But I also learned that you can learn a field by going to seminars. And the best way to know how not to be fooled was to listen to Gross, Bob Gross, who ran the plasma lab and John Chu argue with the seminar speakers. They would argue and argue and argue, and then they would get responses. And in the process I began to learn where the cutting edge problems in the field were, without yet understanding the fundamental underlying principles. You know, I had to take two classes for that to happen and read a bunch of papers.

Meanwhile I am solving these equations. So I wasn't, to answer your question, anywhere near ready to interact with the experimentalists downstairs running the shock tube because I didn't know what a magnetic probe was; I didn't know what a current sheet was. I didn't know what these things were. I certainly knew what a shock wave was, I understood what shock structure was, and I was going to solve for that. And I knew that I was going to solve this by a numerical method which was a transient solution to MHD equations. And I knew that I wasn't going to have to deal with the mathematical problems of multipli-unstable shock waves because that trick that I learned, actually from Bob Towsig and John Chu, of using a transient numerical method would eliminate unstable modes. You wouldn't know what they are. Never mind the mathematics of it. If they are unstable, who cares. It's only, you know, theoretical to know that. By the way, later on Galchen and I used exactly that principle of the first paper that ever was written on numerical stability of climate models. Anyhow, I learned that from listening to Chu and Towsig give a talk and argue with people, when they used a numerical method rather than an analytic method, and people said it wasn't elegant. And they said, but we got the answer—your elegant methods can't get the answer because the mathematics is too hard. Anyhow I always liked Chu's style.

So I went to these seminars, and started learning plasma physics. Initially sitting there lost ninety percent of the time, and then eighty percent of the time, and then seventy percent of the time. You know as I learned more and more, I got more and more references. Finally I started getting results, but I wasn't completely sure how to interpret them, because I still didn't really understand what was going on in the shock tube in the basement.

So it was just about getting time and that's when a good adviser stepped in. He said, okay it is time for you to start applying your results to the work downstairs. So I said, oh so you need [that] I should go talk to the experimentalists. He said no, I think you should talk to Benny Leonard, who is the young post-doc from Australia, who actually was more of a numerical person, but had not spent the year and a half writing an MHD code. So I gave Benny my code, which I believe I did to you as well, and this was my first code, the one that had just one fluid. And then spent a lot of time going over it. And what he did was he took my results and re-plotted it in a way that I didn't know to do, which was to put it in a coordinate system, and in a diagnostic framework that was exactly the same as the diagnostics that were being physically observed in the shock tube below. So when the shock tube—what you are doing is you drill a hole in the side of the tube, and you put a magnetic probe in it, and all the magnetic probe sees is a disturbed magnetic field as the shock wave, and then the current sheet goes by.

So what I was doing is I was showing my shock wave like a movie, but that's not what the probe was seeing because the probe was fixed in one place. So he just did a transformation of what I had already done, and got a magnetic profile from my numerical simulations that were done in now the same frame as the shock tube. And then he got photographs from what the shock tube was and looked at them, and they were almost identical, and they explained exactly what was going on in the shock tube that nobody knew. So it was unbelievably exciting, because I now had numerically explained what the first bump and the second bump that they were always seeing were. The first bump was a shock; the second bump was current sheet. And we could reproduce it. We could fool around with parameters, we could match their probes. So I mean it was Benny that said—knew enough about the shock tube, and what I was doing to say, let's take a fixed port and do it that way rather than the movie-like way I was doing it, and then of course this became exciting. Everybody in the place was all a-buzz, and the experimentalists who were always laughing about the theorists, and the theorists always having their nose in the air about the experimentalists, were each quoting each other. I mean this was really really exciting.

So I went from being this obscure guy over there who was primarily known because he was a really good leader of the reconstruction of the university, to somebody who is now the hero of explaining how the shock tube works, and showing that they are really getting something that they always thought they were. So that was very nice. This was probably by 1969, and we wrote up a paper for the physics of fluids, you know, which was a Schneider, Chu and Leonard that went—breezed through refereeing, and really was a nice thing. Then I began the hard part of my thesis which was the second part, which was not MHD one fluid with the ions and the electrons were together, but recognizing that electrons will have an entirely different behavior. They are light and fast—they will have a much fatter shock, and there will be two shocks. There will be an electron shock and another one. And now I had to do interactive equations. Of course later on, when I built climate models, where I had an upper layer of the ocean and a deep layer of the ocean, it was the same thing, slightly less physics in it, but nonetheless very similar stuff.

So what I was really doing was learning how to solve, you know, typical classes of problems in applied physics. And what basically I did with the two-temperature one is showed that there would be a precursor of electrons going through, and that required more sophisticated measurements. They then even had me calculate how hot it was, you know, so I calculated electron volts, and under various conditions, to see if they would be getting fusion because they actually are observing neutrons. I never put fusion equations into my stuff. I told Chu this is enough. It is time to stop, it is four and a half years, I have a second Physics of Fluids paper single authored here because we were able to get the electron and ion fluids separated. I am not going to do any more. Now something happened at this time that I didn't tell John Chu till many years later. And that . . .

Chervin: What year was this?

Schneider: 1970.

Chervin: 1970.

Schneider: Early 1970.

Chervin: As I recall the first birthday ______.

Schneider: Before that Joe Smagorinsky came around and gave a talk at the Plasma Physics Colloquium.

Chervin: At Columbia.

Schneider: At Columbia on numerical modeling of the atmosphere and climate. By this time I knew something, and I really enjoyed his talk. And that struck me as very interesting. It reminded me of those hurricanes, you know, that I was interested in when I was nine years old that I found fascinating. I forgot to mention, when I was nine my Dad got us an anamometer which we put on the roof, and in the hurricanes we used to look at the needle hoping we would get up to a hundred, you know. Well, everybody else in the world was saying, God the trees are going to blow the house—break the house, I wanted it to go up there because it was exciting. Anyhow so Smag comes and gives a talk, which was interesting about geophysical fluid dynamics. I didn't know anything about geophysical fluid dynamics but basically it was yet another application of basic principles of fluid mechanics, so I knew a lot about MHD but I didn't know how to do anything on a rotating sphere. We didn't do that kind of stuff, but I figure well that's an additional complication, so for—to do this in climate you have to add—I was doing one and a half dimensional models, you have to do 3-D models and you have to have rotations, but then you don't have to worry about ions and electrons and shock structure. So you win one, you lose one, it probably is a tractable problem.

So it started to interest me, when I was thinking about maybe I should do a post doc. on this stuff. This stuff looks pretty interesting. But I banished the thought because my thesis was, you know, very well received. Chu said, oh you can do—why don't you go get a post doc. at the Naval Research Lab or at Los Alamos, and you know you can go anywhere that you have explained how the shock tube works. I was, y'know, kind of a rising young star in this field because thanks to Benny Leonard finally showing me the right way to frame my answers, we were able to explain what was going on downstairs. So we had this theoretical numerical, on the one hand, and experimental, on the other, agreement which is so rare that it really was fun.

Anyhow so then I see Smag's talk and that was exciting, and then I look at the catalogue and I see that there are two classes at Columbia, three maybe, on climate and atmospheres. One is taught by a guy named Ishtiak Rasool (R-a-s-o-o-l) and it's a graduate seminar on planetary atmospheres. Another was taught by Wally Broker on oceanography. And there was this class in geography. Columbia had a geography department then, before they became elitist like Stanford and other places, and declared it a non-discipline and shut them down. Now they probably have to rebuild them because we are getting interdisciplinary again.

But in any case, so I sat in on the climatology course, taught by a guy named John Oliver, found it fascinating and very easy. I mean I just sat in there and it was very qualitative, but at the same time I was learning all the basic concepts. The homework assignments in a graduate course were plotting graphs of temperature. I mean I did not consider this, having come from a plasma physics and applied math background, as very rigorous. Nevertheless it was really interesting stuff. Broker and Gordon, Arnold

Gordon's class which I sat in on, was also really interesting stuff, more rigorous but still not solving partial differential equations, that's not what those guys do. It was, you know, box modeling and qualitative descriptions. But the really fascinating one was Ishtiak Rasool's planetary atmosphere seminar: Why was Mars cold, Venus hot? Why did the earth not run away? How did a cold trap work? How does the greenhouse effect work? That was just absolutely fascinating stuff.

Four people were in the class, one was Zvi Galchen, who was an applied math student, in geology I guess, and geophysics, and me and then two others I don't remember. And Rasool was the number two guy at the Goddard Institute for Space Studies. And largely a satellite meteorologist instrument guy, but also with lots of planetary interests. And he had a phenomenal sense of what was important, rather than being the methodologist himself. And he knew exactly who to hire, and he had just hired a young post-doc. to do ready-to-transfer calculations of planetary atmospheres, named James Hanson. And he was in the institute run by Robert Jastro, that was the Institute for Astrophysics, which everybody called the Institute for Jastrophysics, because Jastro was a tyrant who demanded everything to be done exactly the way he wanted—call people up at Saturday afternoon or Sunday night and say, I need you down here right now because I have an idea! You know, and if people said I'm in the middle of my kid's birthday party—"You want a job?!"—you know, he would do that all the time. He was not a popular guy. But the Institute was an oasis of a NASA lab, right located over Tom's restaurant made famous by Seinfeld and . . .

Chervin: A few years later.

Schneider: A few years later, right. And it had its own IBM 360. Didn't have to wait in the long lines at Columbia, you could get quick access to a machine just as fast as theirs with a hundred users instead of five thousand. And Rasool was number two, and we used to have all kinds of fun meetings there, and they were just getting into meteorology. Because Jastro saw the money dropping off in astrophysics, and hired Mill Talum and I guess Richard Sommerville as a post-doc. Jim Hanson was there by Rasool, not by Jastro, to do planetary atmospheres. And then I was brought in by Rasool. Then I'm doing all this while I'm doing my two-temperature hydrodynamics code. And I admit I didn't mention any of this to my plasma physics senior colleagues because, you know, they don't like to think they are putting all this time and energy into this young hot shot to have him run off into another field. And I well knew that they might not be happy, just like it wasn't time for me to mention to my parents that we were in the basement shooting twenty-two rifles through bulbs and filming them on strobes. So I kept quiet, and amazingly enough for me, and did this as a side job. I have always in my life worked three jobs, so this was my quarter-time side job—learning atmospheric science, climate, oceanography. Taking advantage of the fact that I was in this world class institution, which only had three classes in it, and why shouldn't I sit in from people like this and learn what I can. Then, Earth Day rolls around. And Barry Comodore comes there, and he gives a speech that CO_2 is going to warm the earth, and particles are going to cool the earth, and we don't know who is going to win. And I didn't believe him.

Chervin: Okay and Earth Day was when, April of 1970?

Schneider: Of 1970.

Chervin: Yes.

Schneider: So I am sort of in the phase of writing my thesis and, I confess, in no hurry because I turned twenty-six on February 11th, 1971. And to get out of graduate school months before you turned twenty-six to say, "hello Vietnam" would have been about as stupid as anything I could possibly have done, particularly given my value system plus I was protecting my butt. In any case, and Clinton was proud of it, so was Phil Gramm and so was I. Anyhow the, what was that great book by Al Franken, *Rush Limbaugh is a Big Fat Idiot*, about all the hawks who did exactly the same thing I did. In fact they used pull, I didn't use pull, I did what I was legally allowed to do, I got a graduate degree. Anyhow, in critical skills job. So I heard Comodore do this, right during that quarter when I was taking Rasool's seminar.

So I said to Ishtiak, who is doing this CO_2 aerosol problem? I mean, are we going to warm, are we going to cool, which is going to win? And he said, why don't you work for me this summer. I will give you some codes we have lying around the building; you can modify them. So it was time to teach myself a new field. So he hands me some equations, he hands me some codes, which was an infrared ready-to-transfer code that was floating around using generalized absorption coefficients of Al Sassar. You know we solved it, it's not line-by-line, it was pre-line-by-line. It would generalize absorption co-efficiencies' average absorption over a small delta of wavelength space. And, you know, they had an IBM 360 that I could drop the box in. It ran in a minute, it was pretty fast. So I started out trying to adapt this code. It didn't work very well. I learned that using other people's codes is never a good idea, although it gave me some ideas, and I rewrote the code myself from solving the simple ready-to-transfer equation.

So there I was doing it again—using my skills in numerical methods, solving equations I didn't understand that were suggested by a mentor, by Rasool. Then we needed an aerosol code. Now that was too complicated. I was not going to teach myself in the summer how to solve me scattering. So I had lunch with Jim Hanson and he said, well you know Sagan and Pollack came up with a pretty quick solution to a homogeneous aerosol problem, called the two-stream approximation, suggested that—showed me these two simple algebraic equations, and that's what I did. So I used a two-stream approximation to deal with aerosols, and then I used this ready-to-transfer code that Rasool had floated around which I rewrote, and put it together into what was the Rasool Schneider paper that came out, you know, in 1971 later on, you know after I was finished with my degree. So this was during the summer.

So I was working part-time, clandestined from John Chu and Bob Gross, said over at Goddard Institute. I was also running my plasma physics thesis on the Institute computer because I could drop the box of cards off; I could watch the GCM that was running roll off because I learned how to gain the system. The way you gained the system at GISS, was you had less than one and a half minutes on your program and it would roll it off. So I learned how to load, reload, you know, restart; all the tricks that you learned so that—and you couldn't do this more than every five minutes. So what I was able to do was I was able to, you know, to get through three weeks worth of what it would have taken me at Columbia, where you drop the box of cards off and you get it the next day, I could make, you know, ten/fifteen runs in a day on my thesis model, and I could make a bunch of runs on the other one.

So GISS was great, I had access to resources, and was able to run the system, and everything was fine. I had, this was all legit, I was given computer time to do it. Nobody told me what program I was supposed to run, and I felt less guilty that way for not doing plasma physics, because I was also running my thesis on a machine at about five times the rate that I could have run it at Columbia. As a result, I was actually finished with my thesis by that summer. And I admit it, I choose not to let them know that, because I figured if I finished early they would have given me this neutron problem to work on, and I didn't want to do it. I had done enough, particularly since I had now pretty much made up my mind that I was going to switch into climate because I was really getting interested, and this is by the summer of 1970.

Chervin: In terms of the transition, was it the physics of the climate problem, the time scales of the climate problem, or the application of the climate problem that excited you the most?

Schneider: All of the above. It was a perfect synergism. I'd always been interested, you know, in how the weather worked from being an early kid, especially, you know, extreme events, which were exciting. I always had environmental interests. In 1971 I went to East Africa because Barbara, to whom I was married at the time, worked for TWA and we could fly free all over the world, so where did I go, East Africa to go on a safari. I mean I always had that interest. So this was starting to marry environmental interests with applied physics. It was absolutely exciting to me that I could sit down at the key punch, type up a box of cards and hold in my hands the capacity to simulate the earth, polluted or not. That was an amazing sense of accomplishment to be able to do that. Of course, little did I know how lousy these models were, you know, built on arbitrary assumptions and all of the things that we now know.

Chervin: Thirty odd years ago.

Schneider: Yeah, this was 1970. But nonetheless that was a very strong, powerful feeling. How exciting it was that you could actually simulate something as crazy as the earth, and then pollute the model, and figure out what might happen—and have some influence on policy in a positive way which was something I'd always wanted to do and that was an experience that was made indelible in me through the restructuring of Columbia. So it was a marriage of all of those. And there was a fourth factor, you know if Polonius were alive today he would have told me to do this, which is that in 1970 there were a few dozen papers worth reading in climate theory and modeling, and there were meteorology texts that were readily available, and there were two instruments, the satellite and the computer, both of which were essential for studying the earth's climate or the earth as a system, and neither of which had been exploited for more than a half a dozen years—what an opportunity. Not only was there low hanging fruit, but there were ripe apples sitting on the ground, waiting for somebody just to move in at this time. And the start-up costs were very low, because you didn't have thousands of articles to read like I did in plasma physics—took me three years just to barely get up to speed. Here I could get up to speed in what was done in climate modeling in reading twenty things, so—which I did.

So all four of those factors, not just the ones you mentioned, but the fact that—what an opportunity, I am here by luck at a historically perfect time to study this problem. The problem excites me intellectually, has social implications, and I can look out the window and see it. The plasma _____ shock exciting, as it was to look at magnetic probes, you know, it lasted two microseconds. It might have produced a few neutrons. And I also knew, it was a myth that this was going to produce the unlimited electricity that's pollution free—that we were fifty years away from that, and that we were not remotely fifty years away from having human impacts on climate. That could be occurring now, and therefore it seemed obvious to me that I wanted to move into the more macro, longer term world. The question was how to do that, when my papers were in physics of fluids, and what I had done was primarily noticed in plasma physics. Here I am jumping fields before I'm finished with my graduate school.

Chervin: At the age of twenty-five.

Schneider: Learning—at the age of twenty-five learning something else, and I have to do it quietly. So that's pretty much where things stood in mid- to late

1970, and I had to think about how I was going to deal with this. So I did the calculation for Rasool, really in a way, you know, the Rasool Schneider 1971 paper should have been Rasool and Schneider. It was his—it was my idea to study the problem; his idea how. He gave me the equations, and I just was a fancy programmer, I solved them. And now when I have to read George Will columns about how I was for cooling in 1970, and now I am warming, what a joke you are. In a way, I don't deserve credit for that paper since I was—yeah I did all the work. I earned that paper later when it became a *cause celebre*, and Rasool did not want to go out and defend it, and he sent me. So I was the one who had to go out and give all the talks on it, because people were really trying to shoot it down. So I, boy I learned meteorology and climate very fast later on—but that's getting ahead of the story.

So I sat there, we did it, we wrote up this article. And the aerosols were global, greenhouse gases were global. The model that we used had no stratosphere. And little did I know that that was going to cut in half the sensitivity, the climate sensitivity to CO_2. I learned that later on at NCAR with Jim Coakley, and when we ran it with and without a stratosphere and found out that was why Rasool and Schneider had too low a sensitivity to CO_2. We had about .7°, .8° C for CO_2 doubling. And when we ran the aerosols global, God, global aerosols, they cooled the hell out of the planet.

Chervin: Okay and I assume this was basically a one-dimensional model?

Schneider: One dimensional model, vertical column, and globally average one dimensional vertical column. And so we had the two-stream approximation for aerosols, and we assumed that aerosols and CO_2 were linearly proportional, and that they were both global. And as a result of that, naturally when you start adding aerosols all over the planet, that's an awful lot of square meters and you have only .7 for your climate sensitivity. The CO_2 effect was swamped by the aerosol effect, and we had cooling of three to five degrees by 2100. And Rasool wrote at the back of our paper, 5°—that could trigger an ice age! Now he was quoting Bodiqo and Sellers, whose climate models were just—those are two of the models that had to be read in 1969, both of which said that you could trigger an ice age by 5°, because they had, as I later proved with Zvi Galchen, exaggerated albedo feedback. We later on—I rebuilt those models, did numerical stability with them, and changed the formulation, and we changed the climate sensitivity radically. But I didn't know any of that at the time.

So this paper then was a manuscript that was submitted to *Science* some time in early winter of 1971. Okay, so now I am writing up my thesis, even though I have pretty much completed the work six months earlier, so I am writing up my thesis, you know, in December. The GISS experience had really accelerated rapidly the capacity to make the final runs that I needed to plot the graphs, because I had such wonderful access to computing resources, and learned early on the sources matter. [Phone rings.] Hold on. So I certainly learned at GISS that, you know, resources make a heck of a difference in your productivity rate, plus access to quality colleagues like Jim Hanson saying, hey, here's this paper with the right thing, if I tried to solve the ____ equations from scratch I would have killed a year. And it was great having Jim's, you know, advice on that.

Anyhow, I forgot there was something I did do, and I did the multiple reflections to get the albedo right. I proved that aerosols would not warm the planet, even if they had more warming than cooling in their optical properties, because the albedo of the surface was so low—that is 90% of the radiation would have been absorbed anyway—therefore even although the aerosol was absorbing 60 [%], it was still a net cooler to the planet. And nobody knew that. So I did actually have one value added, by having done, you know, a bounce calculation. So I guess I will take some credit for methodological advance in that paper, but that's all I was, I didn't know what I was doing. I was solving other people's equations.

Chervin: And that ice age comment at the end.

Schneider: That Rasool put in.

Chervin: It was a throwaway line.

Schneider: It was a throwaway line that Ishtiak put at the end, quoting Bodiqo and Sellers, saying 5° could trigger an ice age. All right, now this is important to me, this is going to come back. And this is going to come back in a big way and still does.

Chervin: Thirty years later.

Schneider: Oh yeah, right, well especially when I am dealing with polemicists who don't understand this is how science progresses. Okay, so I had a Krauthammer attack column on me doing Kyoto, you know, citing all that, and I got even with him, and I have that with a column pointing out that we are proud in science of making, of getting the wrong answer for the right [reasons].

END OF TAPE 2, SIDE 1

TAPE 2, SIDE 2

Schneider: . . . [side] two of tape 2. I was discussing my 1997 response to Krauthammer bringing up the Rasool and Schneider paper. And I said that we in science are proud, even when we get the wrong answer, as long as it's for the right reasons—unlike the political world where credibility is whether you predicted it right, not whether the processes that you had were right. Because anybody, any dumb fool can get the right answer by chance. And therefore, what you want to do is you want to get the answer that's correct, given your understanding of the way the system worked—and that back then, for the assumptions that we made, which is that the aerosol was global, we didn't know better than that at that time—it wasn't a bad answer. It was the right answer for those assumptions. The thing I was also most proud of (I will come to this later on) was that I published in 1972, and then later in 1975, what was wrong with the Rasool Schneider paper before any of my critics did. And that's exactly how science is supposed to operate.

But anyhow, so let's go back to this time, which is late 1970. So I wrote up my thesis, and defended it, and went to East Africa for a vacation two weeks before I defended the thesis, because I had turned it in and the committee had to have two weeks to read. And I remember the last three-four days of the trip I was in Moshi, which is in Tanzania. And there is this wonderful mountain, Kilamanjaro, which now has almost none of the ice left that I have pictures of from then, because of warming, and I so badly wanted to climb the mountain. And I was staying at the ____. I found people there who wanted to do it, talked to the two ladies, these famous two German ladies who run these expeditions. And they said, are you physically fit? And I said yeah, I had just been in Colorado a couple of months before, and I was climbing mountains up to twelve thousand feet, and I didn't have any trouble at all, and everything was okay, and so it was all set. And then finally she said to me, is there any reason that you have to be home on time? And I said ,well I can make that, my plane is in five days, and this is a, you know, a four day trip. And she said, son, this is Africa: The cab may not come; the plane may not come; you may not get home. What it is you have to do? I said, my Ph.D. defense. She said, you can't take this trip. You just can't risk it, there's too high a chance that it won't work. I was so disappointed, I really wanted to do that. But I figured, you know, my defense is my defense. Rasool had already told me that I would have a post-doc that would begin the day after I passed my Ph.D. That meant real money, and I didn't climb Kilimanjaro. I to this day am disappointed about that decision, because now I couldn't climb it, I don't think, [I'm] past that capacity to do it.

Anyhow, so I went back, passed my defense, everything was fine. And everybody said, what are you going to do, what are you going to do? Because I had money as a grad student through the end of that semester, so I wasn't going to be unemployed. They assumed I would stay on and write up the second paper of my thesis and so forth. And then I said I am going to be a post-doc at the Goddard Institute for Space Studies starting next week—and what, what is this?!

Chervin: And now is this the first your thesis advisor heard about your moonlighting activity?

Schneider: I honestly can't remember whether I had mentioned it casually. I probably had mentioned it casually before, but I probably did not reveal the full extent to which this is going to be a career change.

Chervin: And this was early 1971.

Schneider: Early 1971. So anyhow, then I got to work with Rasool. I cannot remember the exact start dates, maybe it was in the spring, but I don't think so. I think it was probably shortly after my post-doc. I think John Chu knew that I was at Goddard, working on a project with Rasool and that I was running my thesis there, and since I was churning out work at a high rate, he wasn't complaining, because my productivity looked good.

Chervin: But did your thesis advisor do any inquiries on your behalf for a post-doc position?

Schneider: I wouldn't let him . . .

Chervin: . . . at a research lab or Los Alamos.

Schneider: . . . telling him I needed to stay in New York . . .

Chervin: Because of family.

Schneider: . . . because I was married, and right, that kind of thing. And I said, no I can't go to Washington or Los Alamos now, maybe a year later. I was going to be also offered a post-doc in the plasma physics lab to, y'know, do the next round. They really wanted me to do a 2-D model, more so than neutron fusion reactions. Anyhow, so during that winter I did two things, I polished up the Rasool Schneider paper and I wrote up my physics fluids two-temperature fluids paper. And I started working on Bodiqo Sellers models. Because after all, if 5° could trigger an ice age, and you can get 5° in a hundred years by aerosols, this struck me as *very dangerous*, and that we really better understand this.

So I wrote up the Bodiqo Sellers model, but I didn't do it the way Bodiqo and Sellers did. Bodiqo and Sellers' model was written as a steady state. I learned from Chu and Towsig that it's much better to write it as a transient, and to solve it in a time dependent mode. And that way you don't get unstable multiple solutions and other things, you just get a smooth—if it's supposed to be smooth—transition toward the behavior of the climate system. And anyhow the forcing, that is the aerosols and the CO_2, were transients. And therefore, why would you run a steady state model, which is what these simple climate models were? So I made them into transients. You know, built in the albedo feedback, and so forth. Meanwhile, Zvi Galchen, who was a fellow student of mine in Rasool's class, was now starting to work with Richard Sommerville. He was really interested in applied math, so I invited Zvi to join me, and he and I began working on the stability of these Bodiqo Sellers models, while I was a post-doc at GISS, and he was a graduate student. But this wasn't what he was supposed to be doing. He was supposed to be doing a cloud model for Sommerville. So he was moonlighting, working with me on climate model stability, just as I had been moonlighting, working on climate models when I was supposed to be doing plasma physics. And of course, I saw nothing wrong with that, because I had just spent time doing it, nor did Zvi because he had watched me do it, and saw how successful it was. So we actually had a very nice, y'know, early relationship working on these problems. I can't remember whether he told Richard Sommerville or not. I should ask Richard whether he remembers that far back whatever happened.

Anyhow, in April I went down to Washington to attend the spring AGU meeting and [phone rings in background]—anyhow, I was all excited about this meeting, because there were two talks I really wanted to hear. One was Carl Sagan, and he was giving a talk on planetary astronomy and planetary atmospheres. And the other was a talk on man's impact on climate by Will Kellogg, who had just been the lead editor of the SCEP study, the Study of Critical Environmental Problems, which was one of the first compendia or assessments of environmental issues, and it included a chapter on climate. It is one of the things I had actually read and found exciting. So I had in my hand a preprint of the just submitted to *Science* manuscript by Rasool and Schneider. So I get on the Metro liner and head on down to Washington.

Chervin: Were you actually on the program to present these results?

Schneider: I can't—I don't think so.

Chervin: Or were you just there to rub shoulders and observe?

Schneider: I don't think that we had the abstract in far enough in advance, and the paper hadn't been accepted in *Science*, and *Science* doesn't like that even then. I think I just went to observe. But I am not a quiet type. So I heard Carl's talk and it was just wonderfully presented, as you would expect at a tour de force and very crowded. And I of course pushed myself up to the front and introduced him to—he introduced myself to him, and you know and said I was doing this climate stuff, and he was of course encouraging about doing it. That's all I can remember about that.

Chervin: And I assume that Carl was at Cornell at the time.

Schneider: Yeah I think he had left Harvard by this time, and his up and down relationships with Richard Goody were over. Anyhow, I then went to Kellogg's talk and he basically gave the SCEP talk. You know, the aerosol issue was largely Reid Bryson asserting that biomass burning and desert dust was going to lead to cooling. People were not into industrial pollution leading to cooling. That's what Rasool and I did. We were converting industrial pollution to sulfate aerosol. The thing we did wrong was we made it global, but we had a one-dimensional model. We should have cut the optical depth down by the fraction of the area of the earth that actually was experiencing this, but, you know, come on it was 1970 when we were doing this, 1971. Anyhow, so I hear Kellogg's talk and he was largely talking about greenhouse gases, but also aerosols, made it because of Bryson. And you know he was a very good speaker and a very personable guy. So I walked up to him afterward and hung around, and then started talking to him about what he had said.

And then I told him what Rasool and I were doing, and I gave him a pre-print and showed him what calculations we had made, and discussed it. He said what are you doing, and I said, I was in plasma physics and I took this class with Rasool, and you know I am not shy and I explained all this stuff, and Kellogg is a fabulous listener. He really—I was a post-doc he had never heard of from another field, and here is this important guy who runs the Laboratory of Atmospheric Sciences at NCAR, and the Chief Editor of the SCEP report, and he listens intently to me blathering on about myself for five minutes. And I am explaining this paper. I am like the ancient mariner and he was the wedding guest, and he was politely, you know, listening. And what credentials did I have? I had this pre-print in my hand that I was showing him and I gave him. So he asked me a question or two more, and then he said: What are you doing in July?

And I said, July, this July? I said, I don't know. He said well remember the SCEP report that you were asking me about? We are going to have an entire meeting just on the climate chapter called SMIC, the Study of Man's Impact on Climate, and I need a *rapporteur* and you seem to me to be exactly the kind of energetic young guy who I would like to have. He knew me for five minutes, maybe ten, saw the paper, asked me a few questions, and invited me on the spot to be his *rapporteur* at a meeting that was going to have Bodiqo and Manabe and Herman Floehn and him, and they were going to define the field of man's impact on climate—would I go? I would have cancelled anything I had, including a wedding to go to that! So of course I did it. He said, there is only one proviso, Steve, he said: you're a *rapporteur* and therefore you have to defer to the senior guys. I said, I can do it. I did, I did, I was good. And

Phil Thompson was going to be there. I mean, I was reading his numerical weather prediction book. That was one of the books that I was reading. So this was just as exciting as hell. One month, this was going to be there. It was a three week meeting, in the Island of Lidinge, which is about ten miles from Stockholm, in a retreat that is actually run by the Swedish labor unions, you know, far away, you take the bus into Stockholm to get there. And I was going to go.

Meanwhile, once I had signed up to do this, now I get contacted by Carol Wilson, who is the professor of business administration at the Sloan school at MIT, a colleague of Forester and Meadows of World Modeling Limits to Growth fame, and an absolute interdisciplinary world class policy guy. So he contacts me to you know, just to welcome me in the group. And he had a young protégé named Bill Matthews who was going to be his deputy head of the program, Kellogg was the scientific head. Carol Wilson had the money and was setting all this up. And I was basically the *rapporteur*. So I was in the administration of the meeting, but I was the next to low man on the totem pole. There was one graduate student who was going to work for me, so I was actually now promoted to the point that I had a worker, but I was mostly a worker. Anyhow, they sent me stuff to read and I had to pre-read everybody's preliminary stuff so I could help them design the outline of the book, and I worked that through with Bill Matthews and with Kellogg, and we had a lot of, you know, of e-mails, I almost slipped, a lot of snail mails that went back and forth and packages.

Chervin: Was there any problem obtaining approval from the powers that be at GISS, either from Rasool . . .

Schneider: There were from . . .

Chervin: _________.

Schneider: Rasool was the easiest guy in the planet. He thought this was great. Rasool cared about his people. Rasool was loved by everybody. He was interested in seeing his people succeed. He made connections for us, he paid travel for us. I went down to the AGU because Rasool said, go meet Sagan and Kellogg, great idea! I mean, the guy was what we used to say in Yiddish, a *mensch*, which was completely distinct from number one. The guy who called people up on Sunday and said, if you are not in here, you're fired! Rasool ran interference for people, protecting them from Jastro. Anyhow, so off I went to the SMIC meeting, and it was every bit as exciting as I hoped it was. Manabe was a dynamo, he was, you know, by far in my opinion the most brilliant and pioneering of the numerical modelers. Bodiqo, despite the radical nature of his model, was a broad climate intellectual. He wasn't just interested in climate change *per se*, but in how it affected hydrology, in paleo-climate. If we can't explain ice ages, then how can we trust what we are talking about in the future. He was interested in human impact on climate. He was interested in how climate would impact on humans! I mean he was a broad thinker. Now his methods were trivially simple, and of course he had no access to computers, so therefore he was a data guy, much more so even though he would solve his equations, he was doing them analytically, so he had to simplify them to the point that they could be solved by a hand calculator.

Chervin: And what was his institutional affiliation then?

Schneider: He was the Director of the Mine Geophysical Observatory, and little did I know he was going to get in a lot of trouble, because this was the last time he was out of the country for—fifteen years? And he came alone, which I did not realize at the time, was something unbelievable for a Soviet scientist to be allowed to travel without some KGB gum shoe who was their "programming assistant" or something else of this kind.

Chervin: Of course he traveled without his wife.

Schneider: Yes, she remained captive. I learned this from Carol Wilson, what reality was like. I had wonderful conversations. I met Herman Floehn, the geographer/climatologist who basically knew the climate of every place on earth for the last three thousand years in his head. A wonderful guy, and he also was not reluctant to talk about his role in World War II as a meteorologist, and how he kept a low profile since he was not a Nazi. Very, very interesting meeting. There was Yamamoto from Japan, who had the most exciting calculation. He had globalized sulfate aerosols and they were going to cool us and lead us to an ice age! Yamamoto and Tanaka was the paper that was being written up in this report. There I was, with a manuscript submitted to *Science* with Rasool and Schneider but I was under gag order. This is not your meeting, you are a rapporteur, you are writing it up for everybody else. So I sat there and I didn't tell anybody that Rasool and I had this calculation.

The only thing that I brought into the meeting, and that was only because Manabe insisted that it go in the report, was the calculation I made which showed that what Manabe and Weatherall did in 1967—this I had done in the previous month before I left GISS—Manabe and Weatherall showed that when you increase cloud amount you cool the planet. So clouds looked like a negative feedback. But they also showed that the rate of cooling was radically different for low clouds and high clouds, and that very high clouds actually would warm, because their infrared effects would dominate their albedo effects. So what I did is I showed that if you didn't change the amount of cloud, but you only changed the height of the cloud, and you made the clouds get higher, that meant that if you actually increased the amount of heating of the earth by evaporating more water, but instead of making clouds wider, which is what everybody had heretofore assumed and therefore clouds were negative feedback, I showed that if you made the clouds taller they actually were a positive feedback because they trapped more heat. Manabe loved it. So this guy wasn't jealous, this guy wasn't hostile. He said, wow that's fabulous, you've got to put this in the report!

So therefore I could put my picture in the report, because it was approved by Manabe, since he was one of the guys, and we were in the working group writing up feedbacks, and cloud feedbacks. We basically, I think that was the first time cloud feedback term had ever been put into a paper, was in that chapter of the SMIC report which Suki and I wrote. And of course Phil Thompson was just a cheerleader. He was just wonderful to work with. And I was too verbose, and he was much much more quickly articulate, so he was very helpful as an editor to, you know, crisp it up. Anyhow, so we had sessions of this kind. There were sessions, there was Robbie Robinson, G.D. Robinson from the Travelers Insurance.

Chervin: Sent the first time ______.

Schneider: He was the conservative curmudgeon.

Chervin: Yes.

Schneider: So he and I were constantly battling, I mean friendly battles, they were not unpleasant battles. It was presaging what I was going to have to deal with. I would be talking about human impacts being significant; he would be talking about negative feedbacks off-setting them. I would talk about aerosols, and the Yamamoto and Tanaka leading us to ice ages. He would be talking about how aerosol particles that involve soot would get into clouds and warm the earth, they weren't going to cool the earth. Then there was Sean Toomey who was there, who had this whole radical new idea, which is in the SMIC report, thirty years ahead of IPCC, basically saying if you increase the number of cloud condensation nuclei by having small aerosol particles, you will increase the albedo of clouds the one third power of the number concentration, I still remember it, or maybe it was one fifth power, I got to go look it up again, my brain is getting old. And that that would increase the albedo of clouds, and therefore you could not just take a look at the direct effect of aerosols in-between clouds, but there would be an indirect effect of aerosols on cloud albedo, and he thought it would be a radical cooling, more dramatic than what Yamamoto and Tanaka had calculated (or me and Rasool, which nobody knew at the time). And then Robinson said, oh no no you guys can't neglect the soot. So all the arguments of the year 2000 about the indirect effects of clouds, of either the reflecting particles like sulfate, or the absorbing particles like soot, were all in the SMIC report. Nobody knew how to get the answer, but the report was two decades ahead of its time, by having people like Robinson and Toomey present, and people can go back to that report and find the historical intellectual antecedents to the modern aerosol debate all laid out. Anyhow, so all of my excitement about being at that meeting, and that it would define the field of human impacts on climate was right. It really was just an unbelievable intellectual experience.

Chervin: And who actually sponsored that meeting?

Schneider: MIT through the work.

Chervin: __________.

Schneider: Yeah, it was Carol Wilson the peripatetic intellectual who did it. Julie London was there, and Julie you know was involved in radiative transfer calculations. He was involved in—he was always a skeptic. There was nothing he wasn't skeptical about. Anytime anybody had a positive suggestion, he said what was wrong with it. That's exactly what a good scientist is supposed to do. It was such an exciting meeting.

And my job was to write all this down and then try to produce text, y'know, within two days that would summarize those sessions. So and then of course I would be writing the text in the evening, sitting in the music room, which was the room that Phil Thompson took over and played classical music. And I remember there I was, vomiting on the page writing at high speed, crossing things out. And there was Phil sitting there listening to Mozart, scratching his head, doing nothing for two hours, and then turning around sitting and writing five pages, handing to me say, what do I think, and it was perfect prose that didn't need to be changed. It reminded me of seeing Amadeus years and years later, when he would sit and write it down and he said why should I change it, it was perfect the way it came out. Phil would write like that. I could not believe that somebody could sit there and think, work out in his head, text. Now he would talk to me about it before. What did you think of this, and what did you think of that. And I had no idea that what he was doing was formulating this wonderful set of words to do that. I was way too impatient, I couldn't wait for that formulation.

I would write at high speed, cut, paste, chop, put back together, rewrite three different times—remember no word processors in those days, it was a pain. A pain to your hand, a pain to the typewriter. There was nothing, you know, everything was cut and paste and redo. So Phil's method of think first and write second was vastly preferable, but his brain worked that way. You know, he had the good sense. He also could go through half a dozen Swedish beers while getting his thoughts going, and it did not stop him from sitting down writing in legible long hand, this beautiful prose. So we made a great team because we were so different. I was fast, quick, and not always careful. He was in-depth, careful, but didn't catch everything that went on. So between the two of us, it was a great collaboration writing that report.

Chervin: And projecting ahead a little bit. Besides Kellogg and Thompson, were there any other NCAR people at SMIC?

Schneider: I don't think so, Julie London from Boulder, I am trying to remember back, I 'd have to look at lists—no there was Lester Mockter from Air Resources Lab who, you know, was great on CO_2. Chris Yunge, another fabulous guy. You know, the Yunge distribution on aerosols, he was there, but he was an aerosol physicist. And here's this famous aerosol physicist, and he was completely encouraging about doing these calculations the simple two-stream approximation. I think Yamamoto and Tanaka derived their own two-stream approximation. Anyhow, the SMIC meeting was three weeks long, late June, early July. And the paper Rasool and Schneider came out in *Science* about four days before the end of the SMIC meeting. Victor Cohen was the reporter for *Science* at the time, called up Rasool, and Rasool said my co-author Schneider is at the Study of Man's Impact on Climate conference in Stockholm presenting our results, which happens not to be true. I was a rapporteur who was under wraps, and I was sitting there frustrated that my results were not being presented. The only results of mine that were being presented were cloud height feedback, and that was only because Manabe wanted it in.

So I go off to Stockholm that day, it was a Saturday. It was the day before the press conference in which the meeting would present to the international press its conclusions. And I spend the whole day in Stockholm, my first day off in three weeks. I come back for dinner, and I walk in, and the room sees me and they start buzzing, and Julie London says, "The ice man cometh." What? And then Kellogg and Manabe come over and say, "Why didn't you tell us?!" Tanaka is sitting in the corner fuming! What's going on?! I mean not Tanaka, Yamamoto—fuming, you know polite, but fuming. What's going on? What's going on is that there is a newspaper called the *International Herald Tribune*, which is an amalgam of major stories in the *Washington Post* and the *New York Times*. And the day before the *Washington Post* carried the interview with Rasool about our paper just published in *Science,* saying that I am presenting these results to the Study of Man's Impact on Climate in Stockholm, and I hadn't told anybody about these results, and they had just been published in *Science,* and it would have been an embarrassment to this report not to mention it. Plus Yamamoto and Tanaka had just made this discovery that is now published in *Science*, and in *Science* they were scooped by the invisible rapporteur, and he is the senior professor. So of course Phil Thompson thought this was all very cool, you know, that he loved this. And after Julie got finished teasing me, and so forth, I had to explain to everybody, gave a little mini-seminar on what I had done, and what Rasool and I were doing, and so forth. And you know, Chris Yunge was wonderful about it, Bodiqo was really nice. I mean, it was nice to have mentors like that, early in your career, who were really supportive of that.

And they said, of course this will be in the report. So even though Yamamoto and Tanaka were featured in the report, Rasool and Schneider was in there because how could they not put in the article from *Science*. Then I got a foretaste of what was going to be my career, constant media. So there was a press conference the next day, and I am still a rapporteur. I am sitting in the back of the room, and Phil Thompson is presenting for the theory section, and I think Lester Mockter on the observations, and Will Kellogg is overall running it, Floehn on paleohistory. Reporters come in the room, they make these presentations, first question—"Where is Dr. Schneider?"

Chervin: Now these are reporters from the European press?

Schneider: European press, but also stringers from American press. And they walk in front of me, and they have pads of paper, and they are sticking microphones in my face—When is the ice age coming? So if I had any media experience, it was baptism of fire. I had had a little media experience because of the Columbia thing, but that was talking to the *Columbia Spectator*, y'know, this wasn't exactly . . .

Chervin: The campus paper.

Schneider: . . . this wasn't *Time Magazine* and the, y'know, the Swedish national newspaper, and those kinds of things, the *International Herald Tribune*. But I had been smart enough to learn to be careful, and said, no we did not predict an ice age. We did not say that this was going to create, what we said is that these were uncontrolled emissions that went on for a hundred years, and . . .

END OF TAPE 2, SIDE 2

TAPE 3, SIDE 1

Chervin: . . . think that it's still the 10th of January, closing in on 7:00 p.m. in the evening, and we are picking up at the press conference at SMIC and the issue of presenting caveats when dealing with the media.

Schneider: Right, you know, the where was Dr. Schneider and at least I had the good sense then to say, no we did not predict an ice age. You know, what we said was that 5°, according to professor Bodiqo right over there, would trigger an ice age—ask him, you know. I was pretty proud of me for coming up with that, you know, and that way also got Bodiqo in it. So I was starting to feel like I was getting too much attention, plus a problem I have not had since, but I was nervous about this. I had no experience doing this stuff. This was quite a trial by pen and microphone. Anyhow Rasool loved it when I went back. You know the next week was fourteen hour a day work. Phil Thompson, Will Kellogg and I, and Bill Matthews, but since he wasn't the science guy, we sat there and we wrote the report.

We basically wrote the report, and I think I flew it home with me and took it up to Boston to MIT to the press to do it. I was the one who corrected the galley proofs six months later, went up to MIT to do that, and it came out. Still, that book is still, in my view, current in that it set up the basic problem, even though lots has happened since. Anyhow, the next phase of this, which we will pick up after we eat, is when I went home and Rasool had many angry demanding invitations from places like University of Washington—Bob Charlson and other places, to go and explain our irresponsible paper. Because Charlson and Pilat (P-i-l-a-t), his colleague, had published that aerosols were going to warm rather than cool, because there were soot components on them, and we had them cooling. And in fact Bob later told me he ordered one of his graduate students to find an error in that damn paper! Actually, I'm the one who found the errors in that damn paper, but that's good. Anyhow, that was a whole other phase, and Rasool looked at me and he said, you did the calculations—you go out and defend it.

Okay, well one of the first places I went to was out to University of Washington, in fact, to see Bob Charlson. But I knew that I had to be prepared for this "aerosols are warming." So what I did is I—one of the first things I did was I went back to the original part of my calculation in the Rasool and Schneider, which is the multiple reflections between the aerosol and the surface. And went and showed by simple averages that even if an aerosol absorbed as much radiation as it scattered, as Charlson and Pilat asserted, they would not warm because they were floating over surfaces with reflectivities like ten to twenty percent, you know oceans and forests, most of the time, unless the aerosol was above clouds. But that was unlikely, because aerosols were concentrated in the first kilometer or two of the atmosphere, so they weren't very likely to be over clouds, maybe over snow fields they would be warming. And I basically showed that since an aerosol that absorbs half and reflects half is actually reflecting considerably more back to space than the surface, which would have reflected ten percent and absorbed ninety. So I said almost all aerosols, except pure soot or unless they are over snowfields, are basically going to cool the climate—and direct effects, not indirect effects and clouds like Robby Robinson.

So I went to Seattle and I gave a talk on the Rasool and Schneider paper, and on why aerosols were gonna—I showed this multiple reflection paper. And the thing that was so neat about Bob Charlson is he kind of said, oh I think I am going to throw in the towel, and then invited me out in his boat. So they really are a great bunch and I had a terrific time. But I admit I was scared to death, because I am going to this meteorology department—I don't know any real meteorology, I was basically the computer programmer, and when Rasool gave me the assignment I had about two months before I went to give talks, and I read every meteorology text, every Manabe and Smagorinsky paper I could get my hands on, so that at least I wouldn't make a fool of myself in answering questions. So just like I had to learn plasma physics when I didn't know any to do a thesis, and I was using the skills I had in solving numerical fluid mechanics, you know, like a programmer, and then learning the science afterward, I had to do that again. I had, you know, the programmer who was working for Rasool and now I had to learn the science because I got the job of defending it.

So already I had to do this twice in about three years, and I started to realize you know you can learn phenomena. If you have good grounding, and you have some methodological skills, and I was very appreciative to John Chu and the Columbia Engineering and Applied Science School for giving me grounding in basic science and math, so that I could pick up phenomena because I had had enough methods. That was really something I later learned and I still give to my students. I make my students take a numerical methods course over in Civil Engineering here at Stanford now, even when they are in Biology, and I have them take a fluid mechanics course not because they are necessarily going to use it, but that way they are not fooled. And they all come back and tell me that it has really helped them to be able to do better in the literature than they otherwise would have, and certainly it's impressive to their friends that they know something, and they don't have to sit there with their, you know, fingers in their nose, when complex methods get discussed.

Anyhow, the other place that I went to was NCAR. And I guess that was in, I still remember it, I gave a talk at the invitation of Will Kellogg and Phil Thompson, and this was a talk which was not *how dare you publish Rasool and Schneider*, this was *why don't you come to NCAR* talk. And they wanted me to be an ASP post-doctoral fellow at the time which Phil Thompson used to run the program, and he had just turned it over to the then young new director of ASP, Peter Gillman. And I had actually applied to NCAR, as I now remember, in 1970 for a post-doc, and I got an honorable mention. And I am not sure I would have taken it, because it would have caused considerable consternation in the marriage situation. But by 1971 it was clear that I had to do this for reasons which will be apparent in a minute. Right before I left for NCAR in mid-September of 1971 to give the talk and to visit with Will and Phil, I saw an op-ed piece in the *New York Times* that outraged me, written by a guy named Bob Guchoni who later turned out years later to be editor of *Omni*, and I think he was involved with *Playboy* too, I can't remember.

Chervin: *Penthouse* I think.

Schneider: *Penthouse*, *Penthouse* thank you. In any case, he was working for some mining magazine, and he wrote this really tongue-in-cheek mocking attack on global climate change saying, well it could be warming from greenhouse gases or it could be cooling from aerosols, but don't worry folks, if they don't cancel each other its neither. And I wrote back a letter, and I said it's real cute, but we don't know whether the warming or the cooling is going to dominate, and the big problem we have is rapid change in the present, because both agricultural systems and ecological systems are adapted to the present—and we don't want large changes, and this is not anything that should be mocked, because we'd have to be mighty lucky to have them exactly cancel out.

And the *New York Times* wrote back to me a day or two before I left, and said, yeah we want to use your piece—we have edited it as follows, do you approve the edits (and they mostly shortened it) and by the way, where are you? Because I sent this in as a citizen, private citizen, because I was a National Research Council post-doc at Goddard Institute for Space Studies at the time that Rasool had arranged for me. And I said, well I am an atmospheric scientist who is working at the Goddard Institute for Space Studies but this is—you know I am doing this just as myself. Oh no, no, we only need that for identification purposes, they said. So off I went to NCAR, and I gave my talk, and I—it was fine. I met Bob Dickinson, had a really really, you know, nice set of interactions with Thompson and with Kellogg, went to the Kellogg ranch.

I still remember that it snowed eighteen inches on the 18th of September, and I said, gee this is a very interesting place, and three days later it was eighty degrees, you know, welcome to Boulder. And I think on the second or third day of the visit I was in Will's office, and he was trying to convince me why I want to, you know, apply to ASP this time. Don't worry, you won't get an honorable mention this time. And I am saying, yeah but Peter Gilman's running the program. And he sort of looked at me, and he says, we'll talk to him. And I am thinking, oh, Peter is going to love me, you know, these old guys are going to be coming and tell him who he is supposed to take. In any case, the phone rings, and Will ignores it and Barbara Hill, I have forgotten what she was called at the time.

Chervin: Kendall.

Schneider: Barbara, no it was before that even. Well, in any case Barb says—comes in and says, Will I think you better take this call, its from Bob Jastro. So I am sitting in Will's office, yeah hi Bob, how are you, and all of a sudden I see Will, you know, one of the nicest guys his—he gets a frown on his face and his brow wrinkles, and he says, yes yes he is here. Well, we'd love to, but what's going on? And he says, Bob that sounds a bit strange, maybe you ought to rethink this and talk to him, and well you don't want to talk to him?—all right, okay, and he hangs up. And I look at Will, and he sits there silent for about twenty seconds—now what's going on? He said, well apparently Bob is angry.

I said, why what happened?

He said, well you wrote a letter to the *New York Times* and used the Institute's name without clearing it with him.

And I said, no I wrote a letter to the *New York Times*, and they asked me where I worked. And I told them that, and I said I was writing this as a citizen.

And he said well, Jastro said that only he approves everything that goes from the Institute.

I said Bob, Jastro is writing to the *New York Times* all the time—well I guess he is approving it for himself. And I said, did he have any objections to what I wrote?

No that wasn't the point.

And I said well what was this business about it's okay?

Well he told me—is Schneider there?—and when I said yes, said Will, he said, good keep him, I just fired him. So he called up the NRC and undid my post-doc that day. So needless to say, I am now out of a job, and I call up Ishtiak Rasool and I explain all this, and Rasool is furious, and gets together with several of the other people in Goddard Institute who were equally furious at this, and Rasool being the consummate and brilliant political tactician, calls up Maurice Tepper, who is in NASA headquarters, and who has actually been encouraging GISS to get into climate studies. He explains all this. (I found this out, by the way, months later, what actually happened.) All I knew at the time was that about four hours later Rasool called back and said that Jastro's calmed down; we have had a discussion—I said it was my fault for not explaining the rules to me, and I've been reinstated but it will be on a post-doc through Columbia. And basically what happened was, Rasool called Tepper, explained all that, Tepper turned around and called Jastro to congratulate him on my letter in the *New York Times* and to say how really good it is, and how much the headquarters approves of hiring guys like Schneider and getting into the climate business.

So they pulled an end-run around it, and by that time he had already removed me from the rolls, and then he apparently forced me on Wally Broker's grant that he had given to Broker—something which later on Jastro was fired for, is my understanding, for improper use of government money, not this time but exactly that kind of deal—where he would give grants, you know, for certain things and then take some of the money back for his own people. But anyhow, that was when I started to discover I was very controversial. And when I went back, Rasool just shook his head and he said: You didn't do a damn thing wrong. I thought it was a good letter, it was perfectly okay. He is just a paranoid. He is in control of all things in this Institute, and we have got the problem fixed.

But I knew that I wasn't going to be able to stay there, because I knew that my style was not going to let me clear what I have to say with that paranoid Jastro, and that I wasn't going to tolerate that crap from him. So when the offer letter from NCAR did come through I took it, but that was, you know, three-four months later. Meanwhile, I had a ton of research I wanted to do, in particular working on the stability of the Bodiqo Sellers models which I was working on with Zvi Galchen on that very easy to operate Institute computer that I could put in two boxes of key punch cards and get the answer out, y'know, in a minute and a half later. So I still maintained a productive environment there. Richard Somerville came at a very good time interacting with him over the improvements in the GIST GCM.

Had lunch every day with Jim Hanson and then he hired an assistant to Andy Lacy, _________. And we used to go over to the high in the food chain Tad Steakhouse everyday to have a, you know, five dollar steak for lunch. And I still remember, and I tease Jim about this—telling me, why are you working in this climate stuff for, it's such an impossible problem. It's got so many dimensions, you never can solve it. Do something tractable like radiative transfer and planetary atmospheres and clouds. And I love reminding Jim of that conversation that we had back in 1971 and 1972, when he was telling me that climate was an impossible and intractable subject, which of course it is. And he has done among the best work anywhere trying to make dents at it, so that was a cute memory from that time.

In any case, it was a great group that Rasool had, you know, working with Jim, and Richard being there, and working with Zvi Galchen. And it was something that I was going to miss, and everybody trying to tiptoe around Jastro and his outbursts and ego fits, which I knew I would never be able to tolerate very long. Now I remember the summer of 1972, about a month before I was going to move out to Colorado, this was an intimidating thing, you know, I hadn't spent much time west of the Hudson. And some guy name Chervin, y'know, who went to graduate school with me, walks in the office and he said, my fellow plasma physicist, hey how did you convert into this business? I wonder if there are any jobs for me? So far I am not being interrupted by this guy, so I must have—am I getting this right? And then I told him about Will Kellogg and I said, you know they have post-docs out there. So who should call but Kellogg. Doesn't he call me or did I call him, I can't remember?

Chervin: I think you picked up the phone.

Schneider: I picked up the phone and called him, okay.

Chervin: As I remember.

Schneider: Spoke to Will and it was . . .

Chervin: 1972.

Schneider: Right, June okay, June of 1972, I thought it was maybe July. It was a couple of months before September 1, which is when I was due to start my post-doc. And I mentioned to him that I have this friend who is a plasma physicist just like I was at Columbia, who is interested in numerical calculations and so forth, and he said tell me about him, and I gave him a minute or two rundown. And I says, as a matter of fact he is in my office. So I put Chervin on the phone, and I can't remember, two minutes?

Chervin: It was about that length.

Schneider: Two minutes, he hangs up the phone and he looks at me, white as a sheep because he hasn't been, y'know, west of the Hudson River, and he said, I am going to be working in Colorado on September 1! Will offered him a job on the spot, because apparently they had something called presidential interns, which were post-docs, and that somebody had just turned them down and had this job which if they didn't fill they were going to lose. So he offered it to Bob, and he took it. And here now twenty-eight years later he's still in Boulder. Anyhow, Will was that kind of guy. Will was intuitive, and he would talk to you, and he'd make a judgment, and he'd go with it. So come September, we went to NCAR.

Chervin: Right. What were, you know, as you indicated, you had spent basically your entire life in and around the City of New York at that time.

Schneider: I had done some world traveling at least.

Chervin: Ah.

Schneider: But that's all.

Chervin: What were your feelings about vacating the life that you had known? And two—what were your expectations in terms of long term possibilities at

NCAR? Because as I recall, at that time the post-doctoral fellowship program in the advance study program, ASP, at NCAR was for one year.

Schneider: One year.

Chervin: One year only.

Schneider: Right, they didn't have the renewable. I didn't know what to expect. I didn't honestly think I'd be there long. I figured that I'm still converting into atmospheric science, I have two papers now, and a third one in the pipeline which was the cloud height feedback paper. And that I need to do some more, and I was working on the stability paper with Zvi, and that this would give me an opportunity to become more firmly established so that I could find some other job. I also was married still in New York, and Barbara encouraged me to go out to Colorado, but wasn't anxious to move there. And so I assumed that it would basically be a year. When I got there, Will said oh, Barbara works in an airline, I have friends in United in Denver, she could do the same job. And when I called her to tell her that, she said she wasn't interested, and that's when I realized that her shipping me out to Colorado was not just what was good for me, but what was the end of our relationship. And that this was actually a convenient way to get me out of town. And that was painful, but it in a way cut the need for me to immediately return. I guess I knew this within a month or two of having gotten to Colorado. It became completely clear that that was the situation.

So as it turned out, a month, two months after I got to Colorado, some rather major events occurred which dramatically changed my life and also NCAR, and I will get to that in a minute. You asked—so how did I feel about the move? I was very excited and I also had high trepidations for the reasons I said. I had gone to Boulder twice before that, once as a tourist in 1970, and then visiting with Kellogg and Thompson in 1971. Really enjoyed interacting with Bob Dickinson and Warren Washington.

Chervin: So there was the—only that single one visit post-SMIC and . . .

Schneider: For about a week, yeah.

Chervin: . . . and arriving as a post-doc.

Schneider: Yeah in fact I forgot to mention, when I was there and I showed Warren Washington what I was doing on cloud height feedback, he said it would be good to try that in a GCM. And therefore Warren and I sat together and evolved a whole series of GCM experiments, where I increased and decreased the fixed ocean temperatures—because they did not calculate ocean temperatures in those days, they were fixed—and I increased them by 2°, and decreased them by 2°. And we calculated out what happened to cloudiness. In fact, we discovered a positive feedback. Then I decided that this positive feedback was a bit suspicious, and I did two strips. I did a strip of warming that was a January, perpetual January model, meaning that the sun was held fixed in January position. And I hated that, because what that meant was the model was non-energy conserving, because there was not a balance between the incoming solar absorbed and the outgoing infrared. Because when you have fixed ocean temperatures that's an infinite heat sink.

So I was very unhappy with that component of the model, and I wanted the model to be energy balancing and to calculate its own ocean. And that was in fact—we had long conversations about the need to get an ocean model in there. I probably discussed that with Chuck Leith and Dickinson, as well as with Warren. And so I put a strip of warming between zero and 10° S, which in January is the rising branch of the inter-tropical convergence zone. And I was curious what that was going to do to the already cloudy ITCZ. What it did was very interesting. It changed almost nothing in that zone because it was already saturated with clouds, but it so intensified the circulation in the northern hemisphere, the Hadley circulation, that it increased the sinking air in the subtropics, and actually decreased the clouds in the ten to twenty degrees north zone. And that was a tremendously interesting regionally displaced tele-connection and feedback that I thought. So then I got another idea which is to put a warming strip between ten and twenty north, which is the normal sinking branch of the Hadley cell. And what that did is it weakened the Hadley cell, thereby reducing the cloudiness in the ITCZ, that is between zero and ten south, and increased it between ten and twenty north.

So what I learned immediately was if you are going to do clouds you can't do it in zero-D or 1-D, you got to do it in the 3-D model. And you have to have very clear regionally heterogeneous forcing, otherwise you won't know what you are doing. And that was a nice collaboration that I worked out with Warren, and when I told Warren what I wanted to do, he gave it to Gloria DeSanto, later Gloria Williamson, to program. And Gloria had—I didn't do history tapes, I had those little microfilms. She had them mailed to me within months.

Warren really came through, so I was analyzing those while I was at GISS, and Warren was kind enough to fund me to go to the International Radiation Conference in Sendai Japan in the spring or early summer of 1972 on NCAR, on Warren's budget, where I presented a joint paper with Warren on our GCM work and on my one dimensional work. And that's where I, you know, had the fun and the pleasure of meeting people like Tom Vandahar and John Gilly, and in fact we went traveling to Kyoto, about six of us went down there. Garth Paltridge, Vergil Kundi who had a big red beard, and you know I had to teach all these guys how to use chopsticks. I do remember that when we were riding on the trains. So we went down to Kyoto on the fast train. And I remember sitting in Japanese restaurants, having been in New York I was already pretty acculturated to Asian food. So I was, y'know, lecturing on how to do that to my friends there, which was fun. And I remember at one stage, six-foot-six Tom Vandahar, big big guy, now this was not a standard, you know, guy in Japan. He was trying to fold his legs underneath one of these Japanese tables where you sit on the floor, and the waitresses were sort of looking at this and they were looking at each other and trying to be polite and not to giggle out loud, and we sat there for about five minutes and then all of a sudden like an undone spring his legs uncoiled; they flapped under the table, the table went bouncing, the sake went flying. And all the waitresses in the place just looked at each other, covered their mouths and giggled hysterically. It really was actually kind of cute. Tom took it very very well.

Chervin: And at that time was this table the only western group?.

Schneider: Pretty much the only western group in the restaurant. We also went to a Japanese bath. And I remember Kundi was there with his red beard, and we were sitting next to each other in these baths, you know, and you sit there with nothing on while you are getting scrubbed by people. And he was looking in a mirror and trimming his red beard, and all of a sudden he sensed a presence. And I remember looking, I noticed it first. There were about forty people crowded around staring at this guy trimming his red beard. So we had wonderful cultural experiences. We went to these places because, oh come on you know—because Takashi Sasamori, who was at the radiation meeting, and what a sweet guy he is. He is the one who sent us to the baths and all the other things.

And so I started to develop a sense of community within the radiation wing of the meteorological community, and they were terrific people. You know, we would talk about feedbacks, and Vandahar was just beginning to get satellite energy budgets. And it was just a super exciting time, because we were beginning to put together from the meteorological tradition, climate oriented science. And this was in the summer of 1972, right before I went to NCAR, and what I was hoping to do, and what Kellogg and Thompson and Dickinson and I and Warren had talked about, was creating a stronger and more visible presence for climate science at NCAR. And when you asked, what was the long term, I figured it would be one year, but maybe, the way Will and Phil were talking, if we could create a climate entity at NCAR, I might be able to actually get a real appointment. And after I lost the connection to New York personally, that freed me to be able to think that that would be a nice possibility, since I enjoyed most of the people with whom I was interacting.

Now I remember a dramatic event occurred in early November of 1972. The laboratory of atmospheric science, which actually included chemistry, physics and dynamics as three separate groups run by Will Kellogg, would have a retreat. This particular retreat in this year was held in Winter Park, and it was a very early snow year, I guess it was a big El Niño year, and for some reason they had a lot of heavy snow. I still remember trudging around in cross-country skis up there, and Chervin refused to risk it and was trudging around in snowshoes.

Chervin: Snowshoes for the first and only time . . .

Schneider: Right, walking up a hill at ten thousand feet.

Chervin: Slowly.

Schneider: And this was ten thousand feet, this retreat. And boy, the red blood cells get tied up with the alcohol molecule real quick up there, and there was wine a plenty, as I remember. And I was asked by Bob Dickinson to give a ten minute talk about why NCAR should have a serious expansion in climate research, and there were several other people proposing other expansions and I had forgotten what they were.

Chervin: Now was this possibly in the format of a debate?

Schneider: No, there was a debate right beforehand between Dave Baumhefner and Bob Dickinson. And Dave Baumhefner, you know who is involved in the weather forecasting group, was arguing that we needed to get a giant computer at NCAR to be able to compete with NOAA and so forth in weather forecasting. And Bob Dickinson, while saying he wasn't against getting a giant computer, was sort of forced into the negative because somebody had to take the other side . . .

END OF TAPE 3, SIDE 1

TAPE 3, SIDE 2

Schneider: . . . side 2, of tape 3?

Chervin: Exactly right.

Schneider: All right. And we were talking about the Dickinson-Baumhefner "debate." I put debate in quotes, because the recording machine can't see my fingers doing quotes, and because in a way, you know they really didn't radically disagree. Baumhefner wanted a big computer to run initializations and weather forecasts, and Dickinson was a little bit suspicious that that kind of stuff wasn't really science. And he was arguing that we don't want to lose our science by just getting into number crunching. And of course ironically Bob built a GCM later on, but Bob did science with the GCM. So that was the position he was thrust into, and then I was asked. And I didn't discuss climate from the point of view of GCMs alone, I was talking about it—first I said there is a hierarchy of models. And in fact, I should have said earlier that the hierarchy of models was a concept that basically Manabe, Phil Thompson and I evolved in the SMIC report. And later on, two years later, Bob and I refined in depth in a survey article that we wrote in *Reviews of Geophysics and Space Physics*, where we tried to define, in a way—the field of climate modeling had never had a survey before, and had no single, you know, set of definitions about where it should go. But the basic ideas originally came from Manabe, Phil and me at—from our long discussions at SMIC.

Anyhow, so I argued that where there is a hierarchical approach, and I remember everybody was a little bit drunk, and I was putting on overheads with—well we didn't even have overheads, they were opaque projectors—and I wrote the equation on the back of an air mail envelope, and some people were laughing and some people were hostile. I was trying to figure that out. In fact, some of the chemists were not thrilled with this. I probably was a little bit arrogant and facetious, as I was wont to be at the age of twenty-six or seven, of course not anymore. And, you know, the talk was well received by some, and others didn't like it at all. Because I was arguing that we don't need to understand the problem in all of its depth, that we can run a hierarchy of models with the simple ones addressing basic questions, and then I didn't see a contradiction, for example, between Baumhefner and Dickinson. I said let's do basic science on the simple models that we can do analytically or simple numerical ones, and then we need the large-scale 3-D models I had already learned from the work that I had done with Warren Washington, that you get all kinds of interesting displacement behavior you know and tele-connections when you run GCM's, and that we couldn't avoid the dynamics, and we needed to have the number crunching as well. All of that was fine and, you know, other people made other presentations. Then either later that evening or the next day, I can't remember which, Walt Roberts, who I really hadn't had much chance to meet, but had admired because I liked his style, he was interested not just in science but in the applications of science, the societal problems. He got up and he was dead serious, no clowning around, and he announced that we are going to have a radical reconsideration, a navel contemplation of the institution.

Chervin: Okay and at that time, as I recall, his title was President of the University Corporation for Atmospheric Research.

Schneider: President of Corporation for Atmospheric Research.

Chervin: UCAR, which was the consortium of, at that time probably fifty odd . . .

Schneider: Fifty odd universities, and remember Walt Roberts founded the institution originally as the High Altitude Observatory, and then his vision was to broaden it to include atmospheric science. And now he wanted to move in the direction of climate, because he saw that as an evolving problem. And he also had to satisfy the fifty-odd customers, namely the universities, who apparently had had complaints from some members of the universities, based upon individual NCAR scientists who were complaining about the scientific management at NCAR moving too much toward big science, and moving away from basic science. So individual NCAR scientists had complained to their friends.

And the trustees appointed the JEC, the Joint Evaluating Committee, which was made up of largely administrative meteorological luminaries—deans, people of that kind, and they did a quality and programmatic review of NCAR. Now let's start right out by saying these are university people, at a time in 1972 when the Nixon administration had just for the first time since Sputnik cut the budget for science. And here were the people competing with the NSF for exactly the same pot of money as NCAR, applying constraints on the very institution with which they were competing. I always saw this from a legalistic point of view, though I wasn't a lawyer, as utterly absurd conflict of interest. And I still think that there's a conflict of interest, and if I had my druthers I would have had UCAR managed completely differently than the universities who compete with NCAR for the same pot of gold. But that was not how it was originally set up, when it was set up by Walt Roberts. It was expansionary times, and there was plenty of money to go around for the universities and for NCAR. And NCAR was set up to be both an independent research shop, and one that did work at a larger scale, and provided services and computers and things for the universities.

And it worked very well in times of plenty. And it didn't work so well—it was starting not to work so well in times of scarcity. And this was the first real time of scarcity. So the JEC report was schizophrenic—Walt Roberts reported on this. On the one hand, it said that in order for NCAR to maintain true high quality, they had to do it in the university image which meant singly authored papers, individual scientists, and a university-like promotion system. Yet on the other hand, so it wouldn't compete with the universities and therefore degrade its mission to help the atmospheric science community in general, it should have projects. Now it never explained how at the same time it was to have projects which don't work very well at universities, yet they were supposed to have a university based promotion system. And that incommensurate problem started in the JEC, and in my opinion still plagues the institution to this day. But that's what was announced. And this was serious business. And Walt Roberts said, this is a stinging indictment of this management, including myself, including you John Firor, and you, Will Kellogg.

Chervin: At that time John Firor was . . .

Schneider: John Firor was the director, and Will Kellogg was the head of the Laboratory of Atmospheric Sciences. So it wasn't in slightest clear, I mean the whole place was turned on its ear. Nobody knew what was going to happen. Because there was a challenge to the quality of the institution, and to the mission of the institution. And the irony was that people who complained that the institution was, you know, selling quality down the river by not looking for people who were doing what I would call either the IKX science. On the other hand, the universities first complained that the scientists should be of that quality and then said they should do projects.

So as I scratched my head I also realized that when a structure is shaken, there is new opportunity for alternative management and organization that was not there before when the structure was stable. Exactly the same thing as Columbia in 1968, when after the riot there was now an opportunity to create a senate and democratize the institution—here there was a JEC report saying the institution needs to organize around projects. And at Dickinson's request I was out there trying to push for climate, and there was no place to put it. All of a sudden, what happened after JEC was there was a request that individual groups within NCAR propose projects, and that they were going to reorganize the institution, and there was going to be an open competition for these projects. And people proposed chemistry projects, and they proposed a variety of them.

So what happened was is that Will Kellogg, Phil Thompson, Bob Dickinson—I am trying to remember who else—me, possibly Akira Kosahara, (Warren was not in this because Warren was off doing the GCM. For some reason Warren Washington was not in this particular group.) got together, maybe it was Sasamori who was in it, I actually forgot, and we proposed a climate project. We worked long and hard on it. I remember Barb Hill typing this up. And the very opening line, back in these days before word processing, she handed—she gives it to Will and Will just starts laughing. I said, what's so funny? And it began, "We propose a conceited effort at climate research at NCAR," and we said no Barb, that's "concerted." And she looked at me, she said I was just thinking about Steve, you know. I said, thanks a lot, good friends. Anyhow we proposed this effort. We suggested it be based on the hierarchical approach that we would work on simple models, that we would work across the hierarchy, and we would, you know, connect ourselves to the observationalists to validation and derivation of parameterizations, and that we would make cloud feedback a central component of the issue. And we wrote up our proposal. It wasn't very long, maybe ten fifteen pages long. That's all there was supposed to be.

Well the NSF composed a committee of themselves and UCAR trustees, and a couple of members from the scientific community at large, to evaluate these proposals, and to rank them. And somewhere in the winter of 1973 when I was a post-doc for all of six months, the proposals were in, the rankings came back, and the climate project proposal was number one. As a result of that, our "conceited effort" was now going to be an NCAR project. I was then asked to be the deputy director of the climate project, working with Phil Thompson. And I moved from being a mechanical engineer, to a plasma physicist, to post-doc, to deputy director of a project all in a few years. And this was very heady and very flattering—at the same time it was very intimidating. And the other thing that I learned is it also caused a lot of enemies, particularly those people who over in other towers' projects came out at the bottom of the heap, and they lost their jobs or their friends lost their jobs. And they were furious to the point of violent, about how we, just because we could write a good proposal, were displacing these real scientists who make measurements, and we were just going to do this theoretical modeling stuff. And I was really disappointed at that kind of back biting that was going on, but it was part of life, and I learned early that that's what happens when you make a splash, particularly if the splash is going to win, you are just going to make good friends and good enemies by virtue of your existence. And that was a lesson that I guess I learned as a twenty-seven or twenty-eight year old.

Chervin: It was probably also the case that these were tight budgetary times.

Schneider: For the first time.

Chervin: For the first time in the history of the organization, which at that time was a good twelve years or so, and by having been founded in 1960 and apparently it was clear that some long term activities will be cut, and other fresh activities would begin in their place.

Schneider: Right.

Chervin: And so I think all of that put together added to the uncertainty and possible resentment, as you indicated.

Schneider: Yeah now I forgot, there was something that I did which I concede, I even conceded at the time, was a politically strategic move to push the climate project. I walked into Will's office one day and I said, we need to demonstrate—I don't remember if I actually used this phrase, market pull. We need to show that there's a genuine interest in this institution in a climate entity. And therefore, instead of having the usual stuffy scientific seminars, I propose we have Climate Club. And Climate Club would send out an abstract of a talk, and a title, and we would always try to get interesting speakers, controversial subjects. Bring in Murray Mitchell—is it going to warm, and is it going to cool? You know, bring in Sellers—are we going to get the ice age?

And then Will said, well if we want to make it interesting, and we want to make it a club, why don't we have the talks late in the day so that when they finish at five o'clock, we'll have wine and sherry and cheese. Will was very clever. So between the two of us, we worked out sexy topics, and a bribe to get everybody in the room, and it worked like a charm. We had eighty to a hundred people coming to these meetings. They were fun, I would introduce the speakers, and always with trying to be humorous and a little bit facetious, and everybody had a really good time. And they were outdrawing the regular seminars very significantly. And that also probably had some influence on the management that this was a topic that, you know, had a large constituency. I think once I even invited a couple of years later, well after the climate project was established, Paul Erlich to come in and talk about how climate was just a piece of the so-called world predicament, the club of Rome problem, because Walt Roberts was very interested in issues of this kind, and Paul, having been a Johnny Carson regular, not only filled up the room, they had to put speakers outside the room. There were probably two hundred people who were sitting there.

And I told Paul that his job was to try to convince my somewhat reluctant disciplinary very atmospheric science oriented colleagues that for us to move forward into the world of real climate applications, that we needed to do more than just study meteorology or oceanography, but we needed to have ecologists, economists. We needed to have a broad multidisciplinary moving toward interdisciplinary program, which of course Walt Roberts had long advocated. But Walt was losing his grip because the JEC attack on NCAR for quality, plus usurping the universities' prerogatives had weakened his position. So Paul came there and dutifully did his job for a while, you know trying to convince people, but Paul being the direct and cynical character that he is, couldn't resist—someone from the audience got up and said, well, Dr. Erlich, all of what you say may be interesting, but there are (I am trying to remember precisely what it was—you may remember this slightly differently, the way I remember it is there were probably two questions that I am lumping into one, but one of the questions was), there are members of the National Academy of Sciences who do not share your pessimistic views, to which Paul quickly snapped back as I remember, cream isn't the only thing that rises rapidly to the top! And I looked at Paul and I said yeah, thanks a lot you are supposed to help me! How do you remember it?

Chervin: As I recall, there was an expectation stated from the audience that, you know, these are the difficult problems, and perhaps we should allow our elected officials to solve them for us.

Schneider: Oh, that could be right.

Chervin: And then Paul uttered the infamous statement that it isn't only the cream that rises to the top.

Schneider: Yes, I think he went on and got more graphic after what it is that floats.

Chervin: Floats.

Schneider: . . . that also floats, yes that was true. It brought the house down while enraging others. And I guess I was by that stage indelibly going to have to live with a mixed reputation of people who really liked it, or people who really thought this was an anathema to science, what we were trying to do, by broadening the institution, moving away from the meteorological paradigm and trying to—what was way before the phrase "global change," move the institution in the direction of being a main presence, as a global change institute. We were too far ahead because it was just not possible to do it.

Chervin: Now as you indicated as a time for rethinking the direction of the institution and paths, changing a paradigm or two. You were in effect a lowly post-doc at the time.

Schneider: Right in 1972 and early 1973.

Chervin: And in that era the one year of post-doctoral fellows were free to do anything that they wanted in the institution—they did not have to attach themselves to any ongoing effort. And as I recall, more often than not, they would spend about half their time writing up papers based on their Ph.D. thesis, and the other half would be to try to find a real job for . . .

Schneider: Right.

Chervin: . . . for the next year. Who exactly was it that encouraged you to play a major role in this program development activity?

Schneider: Well I kind of backed into it. When I first came there, I was going to learn about atmospheric sciences. I was attending all the seminars, and I was you know working with Galchen long distance on the climate stability paper. And I was working more with Warren on the cloud feedback paper, but I immediately had realized that the cloud feedback paper had a lot of noise in it, and how are we going to, you know, describe whether this thing was significant. I remember talking to a guy named Chervin about how to do that. I had heard a talk by Larry Gates, where he was arguing that we need to deal with the statistical significance so that we could separate out, you know, what was signal from what was noise in the model. Larry hadn't actually done anything analytical at the time or numerical, he had just made a comment, and in fact I thought that comment gave him a prior claim to work in the field. And as you remember Robert, as a result of that, we actually a year-two years later invited him to join us in a joint paper using the then Rand model and the NCAR model to do some joint, I guess probably one of the first papers Jerry Spar at NYU had talked about that also. I think that was the only other person that talked about the need to look at statistical significance.

So that's what I started out doing. But I was there all of two months, when the JEC report came out, and all of a sudden we are supposed to prepare a climate project. So I didn't do any science probably from November to February. Every day it was, are we going to have, y'know, setting up the Climate Club? Are we going to have a climate project? Constantly writing and rewriting—I guess it's not fair to say I didn't do any science, but it went to, y'know, twenty percent. Little did I know that I was going to be a presage of the rest of my life, trying to deal with all the other things that I was doing. And back then I didn't have any graduate students and post-docs and junior scientists to, you know, do the work on my ideas. If I didn't do it myself and sit in front of the keypunch, it didn't get done. So I was worried. The good news was that I had enough of a head start, and enough papers in the drawer that I could kind of get away with that then. And it was clear if I was going to be proposing a climate project, and that worked, that I was in a way making my own job. So that was my job search. And it wasn't that anybody encouraged me, it was just basically happenstance or random event that the confluence of my early post-doc and the JEC report were, you know, within two months of each other, was just pure luck.

Chervin: And was it that experience that began the collaborative activity of Bob Dickinson, in terms of both trying to define the field of climate modeling and climate science, and also to establish a program or a project to pursue those goals?

Schneider: Yeah, as I said earlier, when I came out and visited in 1971 I had a very good chat with Bob. And we had an absolutely compatible philosophy about the need for hierarchical approach in climate, and that the sort of the arrogance of the general circulation model community—this does not include Warren. Warren was not like this, and nor was Manabe. But certainly there were others, I don't need to mention names, whose basic views were, if you weren't writing down three-dimensional Navia Stokes equations, you weren't doing science. And my view is if you weren't understanding what mechanisms were causing these models to give you the answers they were, you weren't doing science. And therefore we needed to do both. We needed to explore with stripped down, you know, highly parameterized models, and then ask the same questions across the hierarchy and see what we could learn. And we used the 3-D models to do nonlinear interactions that were tough to do by intuition, and we would look at the disaggregated models to see if we could figure out process by process. So Bob and I had this compatible philosophy and we would, you know, talk about how to do that—how would we couple that to ocean models? When are we going to put in ecosystem models? You know, we were just beginning to think out loud about Earth Systems Science before it really had even gotten that name.

Chervin: _________.

Schneider: Right. And Bob asked me at the time, it was November, that fateful November of 1972 when there was a JEC report. A couple of weeks after that report, I went with Bob at his request to Fort Lauderdale, Florida to the SPONS, the Scientific Panel on the Natural Stratosphere, the very first CIAP, Climatic Impact Assessment Program. Remember at the time, the Boeing Corporation had proposed a supersonic transport. And they proposed that it be federally subsidized to compete with the European Concorde. And a lot of environmentalists were upset about this because in the SCEP report they had shown that ozone depletion would be possible from the injection of water vapor in the stratosphere. Of course it turned out later that wasn't the problem, it was nitrogen oxides. And work done by Harold Johnston and Paul Krudsen essentially nailed the issue of the NOX from the jet engines.

Anyhow, the SST was already sitting on the fence, just because some people didn't like government subsidies, others thought it was a bail-out of industry. It was interesting that the right wing which doesn't like government subsidies actually was going to give the money to industry. The left wing which does, didn't like it because it was, you know, money to a rich corporation. There was a very interesting political debate going on, and the vote in the senate was very close, and the environmental risks of ozone depletion pushed it over the top—deny the SST. So the Nixon administration didn't like that outcome. So they had the Department of Transportation commission what basically was the very first of a long series of assessment projects, you know, which IPCC is the legatee.

So CIAP took place, and Bob Dickinson was in charge of, since he was really a stratosphere person, he wasn't even in the original climate project, because he was in the stratosphere group. And even though he was working with me on issues, he wasn't actually in the group. So he asked me to go with him, and I remember we flew to this meeting, to this very first CIAP, where there was a young post-doc named Mike McCracken, and then two young scientists, Jerry Malman and Richard Lindzen, and there were some other people. And we were going to be involved in the assessments of the climate part. There was going to be plenty on ozone, that wasn't my thing, I was on the climate panel with McCracken and Dickinson. I think Jack Gisler, I can't remember who the others were, there weren't that many. And at the outset of this meeting I began my long and happy relationship with Richard Lindzen. He got up and he denounced the enterprise on day one as irresponsible. And the reason it was irresponsible is this report had to be written in two years, and he claimed that the science will not be definitive on the timeframe of two years, and anytime scientists suck into political pressure to try to provide answers that cannot be given, that they are violating their scientific integrity.

Chervin: Okay and at that time he was a professor at . . .

Schneider: Chicago.

Chervin: University of Chicago.

Schneider: And basically he had done work in the stratosphere, but his claim to fame was tides in the atmosphere, but he worked on some stratospheric chemistry and dynamics—and clearly a smart guy!. I completely and totally disagreed with his philosophy, as did Mike McCracken, as did Bob Dickinson. Bob's not a fighter, and he is not into this kind of hot debate, and he does not like unpleasant confrontation. So they, you know, they kind of looked at me, and McCracken got up there, and he basically said: Dick, we are not arguing that we should tell people we know the answer, what we are basically saying is that we have the best information there is, and that we should explain what we know, and you know, what the likelihoods are, and how much of this we can fathom. And we should say what research needs to be done, and you know, he basically outlined beautifully this post-doc who had been at Livermore working with Chuck Leith, what a good assessment should do. I thought it was an excellent speech. Lindzen got up there and he just excoriated him. I don't remember precisely what Dick said, but it was basically, you don't understand what science is, and that's not what it is, and then these politicians should never be pushing us around, and we should not give in! We should demand that we scientists are in charge of the scientific agenda. We are a better enterprise than politics. So at that stage I said, Dick they are going to vote on the SST in a few years. They need the best science that's available. Do you think it's more responsible for us to guess with caveats attached, or to have Barry Goldwater guess for us? A couple of people applauded. Lindzen turned around and said, that is the most scientifically irresponsible thing I have ever heard, and he stormed out of the room.

Chervin: At the time of this event, was there any federal agency program managing . . ?

Schneider: Allen Grovebecker, Rene I'd forgotten his name, Goodstein? I actually think that the guy who's running IPCC was there at the time, Ron, who is now sort of the head of the bureau there, it will come to me—Ron something. It's alright, we will get it when I edit this transcript. Yes, there were plenty of people there, and they completely agreed with McCracken and with me, and with the bulk of people who were there to do an honest job of assessment. This group was so far ahead of itself, it had Ralph Dodge from the University of Wyoming, who was an economist. They were going to actually take a look at the economic implications of climate change, and the economic implications of ozone depletion. They had structured it right. It was way ahead of anything that had been done. It was much more multidisciplinary. Of course, y'know, nobody in the room knew anything about economics, other than the economists. Then the economists knew nothing about climate.

Now, when one has global change meetings, everybody in the room has a pretty good understanding of the wide range of topics. We are vastly more multidisciplinary and even moving toward inter-[disciplinary], but that's taken thirty years. I mean it was not respectable back then. It was viewed as it's impossible, if you are not a pure disciplinarian, you must be shallow and incompetent. How dare you get out of your field, y'know, that was the view.

END OF TAPE 3, SIDE 2

TAPE 4, SIDE 1

Chervin: . . . four, it's about 11:30 p.m. still ________.

Schneider: Yeah we will go part way through this. Well there's a lot of history here.

Chervin: And we're picking up with CIAP . . .

Schneider: Did you check . . . ?

Chervin: . . . Ft. Lauderdale, the fall of 1972.

Schneider: That was the fall of 1974. Okay. So anyhow, CIAP got together and we said we'd write this report. Meanwhile, I had some science to do, you know, NCAR was now under the gun from JEC to have individual scientists judged by the single authorship rules of the universities. So you can't just sit there and do assessments and expect to get promoted. And in fact they had a senior review group which had ranked all the scientists on five criteria, and of which scientific originality and productivity were the two main categories. There were some service categories, but just like universities perfunctorily say you must teach, the main thing they look at is what your research is. And NCAR was, you know, despite its project oriented officialdom, was going to still define quality in the image of university professors.

So I knew that we needed some help. I couldn't just do this CIAP report alone. And Chuck Leith, who was going to also be involved in it, wanted to work on turbulence, and wanted to work on normal motor initialization—he didn't want to do all this alone either. And there was a young post-doc in either my year or the year after named Jim Coakley, and he wanted to stay at NCAR. And I thought Jim would be a great guy to work on this program. And we didn't have enough workers in the Climate Project, so I proposed that we get a grant from Allan Growbecker of CIAP, and we use that grant to hire Jim, and then Jim would be kind of a project leader on that grant. And he'd be responsible for helping to write up the chapter we needed on climate. And Chuck and I would sort of be involved as the PI's on this, and that's what we did. Now back in those days there were almost no grants at NCAR. This was one of the first. And because of the budget constraints, and because I knew how hostile some people were just about the hiring of me, when others were getting fired, I didn't want to sit there and bring a new person into the Climate Project at the expense of somebody else from the old guard, it was already nasty enough. So getting the CIAP money was a pretty good way to solve that problem. And I'd have to go back, and have to ask John Firor or somebody, but I think that there were long and hard discussions at very high levels within NSF and the trustees about whether this was a good idea to get outside money into NCAR, whether we should stay NSF pure. In any case we got the money, it wasn't that much. But we got, y'know, enough to hire Jim, and to do some overhead support. In fact I think we weren't allowed to charge overhead, because that would be—you know, look like a university. I can't remember very well what the terms of the deal were.

Chervin: Are you able to recall if Jim Coakley was hired on as post-doc or _____________?

Schneider: No, I think we hired him on as a project—I think we probably hired him on as a project scientist. That's how we were going to also—in fact as a result of that, I was starting to reconsider the CO_2 aerosol problem, that's what I was getting back to. And Jim built a radiative convective model that we could use in CIAP because we had to discuss—we had to, in order—remember CIAP was about stratosphere, and the model that I had built for Rasool had no stratosphere. So Jim built a radiance convective model that had a stratosphere in it, so we could throw aerosols in there and do the CIAP job. That's how I discovered what was wrong with my calculation with Rasool. Because when we used basically the same kind of model that I had built, but we had a stratosphere in it, instead of getting .7° for climate sensitivity, it was getting one and a half, like Manabe and Weatherall.

And I realized the reason for that was that by not having a stratosphere we were missing the downward infrared radiation and enhancement that occurs when you add CO_2 up there. Plus you warm the stratosphere. You get twofold: You get the downward IR from the extra CO_2, but when the stratosphere heats, when the stratosphere—no I get that wrong, the stratosphere cooled. No, it was just—let's just take that one from the top.

The extra IR emission from CO_2 in the stratosphere was sufficient to make a substantial increase in the climate sensitivity. So I immediately realized that the Rasool Schneider paper, you know, was flawed. Because by omitting the stratosphere it missed half its climate sensitivity. Also, by that time we had experts like Jerry Grams in the chemistry division, who I was talking to about aerosols, and Kellogg was beginning to work with Jerry on what he called GNP, gross national pollution, where they were actually drawing maps of regionally heterogeneous aerosol loadings which were concentrated around the industrial areas, not globally widespread. So it became completely clear to me by 1973 that the Rasool-Schneider calculation couldn't be right, because we had global aerosols. Plus the CO_2 calculation that I had made was wrong, because we didn't have a stratosphere. And what I then did was read all the papers that had been done on climate sensitivity, who had sensitivities ranging from point six or five, up to 9, Mullar was 9, and then plotted them all out. And basically said that in my opinion the best guess was probably something between one and a half and three-three and a half, based upon the literature, and published that later on in 1975 in a paper called "The Carbon Dioxide Climate Confusion."

And I am very proud of the fact that it was not my current and past critics who found out what was going on, but I did and that I think that I operated in the best tradition of science, which is you do what you think at the time. Then you reexamine the assumptions. You decide the assumptions lack. You recalculate, and then you publish all that without any shame. That's how science proceeds. In fact to flash ahead, to being attacked by George Will and Krauthammer and others about what a joke you were cooling in the 1970's and now you are warming. I said, imagine the doctor who makes a preliminary diagnosis before the blood test and the X-rays are in, and then they are different than the preliminary diagnosis, but sticks with it to be politically consistent. I said, this is not what we do in science, maybe that's what you do in politics, said Ida, Will and Krauthammer, but we don't do that in science. And we're not ever ashamed of getting the wrong answer for the right reason. The right reasons were the best models that we had at the time, and as we refined them we improved them. And you know that was a style I learned from John Chu and Bob Dickinson and Phil Thompson.

So I felt I had fine mentors, you know, on the right way to approach it. But CIAP got us some funds, set the tone for external research, and was an assessment. And I learned how you begin to approach assessments, and why you ask broad questions, thanks to that program. Later on, in fact at the same time, we evolved the concept of climate sensitivity. It was in doing CIAP, you know, Chuck Leith and I and Coakley, I guess, you know, kicked this around with Bob. And the first time climate sensitivity was ever defined in a paper was in the appendix to the Schneider and Mass paper in 1975. And basically the concepts came out because of that work that was funded by CIAP that we, you know, kicked around in the climate project.

I have to say that in the early 1970's that was the place to be. It was the most exciting climate research in the world. And while Geophysical Fluid Dynamics Lab may have had the best GCM's, and among the best dynamics, they were not looking at the broad, wide ranging sets of questions. Each summer, we with ASP held a climate colloquium. We invited people like Bill Sellers, Jerry North when he was coming from physics in 1973 as an ASP fellow, Bob Sess, Ramanathan; what brought John Mitchell out there in 1973 or 1974. And what we did is we just kicked around feedback concepts. We looked—Michael Gill came out and spent a month with us. Ed Lorenz would visit, and he would join in every day, and taught me how to hike. And it was just—it was a time that was intellectually magnificent. And we were growing, and were learning together, and we were broadly multidisciplinary, still focused on climate. We hadn't yet crossed quite into ecology; we were looking a little at it, but at the margins. We certainly were not ready to put in economists. Didn't have much hydrology yet, so we were not by any means as global change oriented as we are today. But that's where we began this process. And as a result of that, I was asked by the editor of *Reviews of Geophysics and Space Physics* in 1973 from Rice . . .

Chervin: Alex Desler.

Schneider: . . . Alex Desler, thank you—if I would write a survey paper on climate modeling. So I took him up on it. And I, you know, did the hierarchical approach, and wrote the paper up, probably had eighty references, it must have been fifty pages long. And I gave it to my favorite critic, Bob Dickinson. About a day later I got back a paper with so many red marks on it, and he looked over to me and he says, I am crabby!

I said, what's the matter Bob?

He said, you left out this, and where's this, and that, and have you read this reference, and I don't think that's exactly what they said.

And I said, I don't know, I would have to go back and check. This is an early draft, Bob.

You know, remember how I described the way SMIC went, I would throw up on the page, and get everything out there, and do a draft, and then start fixing it later. Well that wasn't Bob's style either. He liked to be very thorough and careful in advance. I liked to get the concepts out, and then let the community help, y'know, evaluate them before I published them. So Bob started going to most of the references I had, and he started reading them. Within one month he had read every reference I had in there. He then read the references of the references. And he was telling me where I should expand this and this, and I said, Bob why don't you just become a co-author and let's do this paper!

And that's what happened. We had about a five month collaboration, where we wrote close to a hundred typed page survey article that defined climate modeling. And I kept trying to make it readable by humans; and he kept trying to make it as scientifically sound as possible. It turned out we were a pretty good combination. I was making the paper accessible, and he was trying to make it as technically thorough and broad based as possible.

And I remember one interesting side-story. By 1970-late 1973 Bodiqo was becoming *persona non grata* in the Soviet Union, and my understanding was that he had a number of Jewish scientists in his institute. And when he was ordered to fire them he refused on the grounds of he is not going to play politics—these are fine scientists. And apparently he was heavily censored, was not allowed to go in the western literature room, was banned from travel. And I sent him a copy of this paper for review, and didn't hear from him. And I was really surprised, because we had maintained a correspondence. And I had sent him other articles and gotten back good comments from him, since we met at SMIC and got on very well. And about the time the galley proofs were coming back on the climate modeling paper, Chuck Leith came to me and said, I was in Russia last week. And here is a—I am playing mailman. And he handed me a crumpled up set of two or three sheets of papers which were from Bodiqo, handwritten review and comments on our climate modeling paper. And Chuck looked at me with very sad eyes and said, he gave it to me in the bathroom where we thought we weren't being watched. It was very sad. That began a long period in the Cold War which when we talk about nuclear winter, we'll come back to. And of course I incorporated a number of Bodiqo's comments in the galley proofs, and was very pleased to have them. And I know that he was pleased that he was able to get quoted and thanked in the acknowledgements of that survey paper.

Chervin: And what was the general reaction to that paper after it appeared in print?

Schneider: Bob and I got an honorable mention in the NCAR publication prize for it. It was very very well received. Smagorinsky, who doesn't believe in simple models, grudgingly said well, this is a fine didactic work, he said, but all the real modeling is going to be done in 3-D. And I said, yes Joe we have to use 3-D models at some stage, but we still have to understand what we are doing with the hierarchy. And I still believe that to this day, that you don't just add resolution, and add complexity for its own sake. You still have to disaggregate and see how processes interact, and do that at the same time. I later on called that SSCM, simple simulation of complex models—working with Starly Thompson and Dave Pollard, where what we do is we would do models of glaciers, ocean and atmosphere. Each of those models were very very low resolution, but we would use exactly the same coupling physics as the GCM's used, and we would then use those as simple simulations of complex models, to be able to explore the coupling behavior. I mean that's exactly the kind of thing that comes out from the hierarchical approach, and it's still in my opinion necessary. And people who don't do that are going to be using models like their observations. They are going to get lack of interpretation and understanding of what results they get.

Well anyhow, I get ahead of myself, because I need to go back to 1973, because something else happened then that was very fundamental in my life. And that was an invitation by Bob White at NOAA to give a talk on human impact on climate at the AAAS in Baltimore, I think it was in 1973. Helmet Lansburg was on that panel, and he was not a fan of mine, as I learned there. He was very hostile to modeling. He thought modeling was no damn good, and that you just measure. And I argue, you can't measure the future if you don't model, you don't know anything about the future. Modeling is about—a measuring is trying to understand processes.

When you understand processes, you then build a structural model, you use that to predict. Because the future is going to be disturbed by humans in a way not observed in the past, therefore we have no alternative but models.

That was a very controversial view at that time, because it was non-empirical. Science is empirical, I was told. I said science is not empirical when it's about the future. There is nothing empirical about the future. There is only empirical about the present and the past, and what you do is you use empiricism to derive the understanding of the sub-components of models. I still have to give that speech, even today, having done one just this week. Anyhow, I didn't then understand Basian versus frequency statistics, but in fact that was what it was about, you know objectivity and subjectivity in modeling and in projections.

So I went and I gave a talk about warming versus cooling. And by that time I had already realized that aerosols were not globally distributed, and that I could no longer make the claim that Rasool and I had made that the cooling dominated the warming. And I pretty much said that depending upon what assumptions we make, we could get substantial cooling, substantial warming, and that it wasn't yet clear, we didn't know enough about the distribution of aerosols to know how to do it. And I made a quip, and I said Mark Twain had it backwards. Nowadays everybody is doing something about the weather, but nobody is talking about it. And there was a white haired gentlemen sitting in the front row who was writing all of this down on a relatively small sized pad, which I later learned is a reporters pad. You know they can—they don't use 8 by 10 sheets, they tend to use 5 by 6, 5 by 7. And that white haired gentlemen was Walter Sullivan, the dean of science writers at the *New York Times*. And I was written up in the *New York Times*. He quoted that and then said how it could warm and cool. And this was not front page stuff, it was probably back in page forty. NCAR at the time had a press intelligence service, and that not necessarily being an oxymoron. What they meant by press intelligence is . . .

Chervin: Is that a clipping service?

Schneider: Yes. Anytime NCAR or UCAR's name was mentioned, they were sent a clipping. And the clippings were sent back to NCAR and they are actually distributed in xeroxes. They distributed them around the institution, so people had them.

Chervin: That's right—as I recall that was a monthly product.

Schneider: Right, later on. Back in the early days, I think that was not quite that systematic. So what happened was I came back from the AAAS, there was a *New York Times* story. And I found it posted on the bulletin of the map room, the place where everybody congregates, with a big blue stamp from a rubber stamp on it stamped "bullshit." Of course, whoever did that didn't have the guts to do it to my face. But I then began hearing all kinds of stories about people who were furious and hostile about this Schneider because he is always out there seeking publicity. I was seeking publicity by a wise quip that caught the attention of Walter Sullivan. And then later on he interviewed me, and we talked about these issues, and he did a damn good job of honestly reporting the session as you would expect from a fine writer like Walter.

What I began to realize is, you know, there are people in the world—let's be real blunt and call it what it is—who are just jealous because in science people derive reputation from the publications and the works that they do, and they become increasingly famous, y'know, in proportion to their side of equality. Well all of a sudden now it's a media age, and I was getting a lot of notoriety. It already happened in the Rasool-Schneider paper, now it was happening from Walter Sullivan. And I was getting this notoriety not necessarily on just my science works, but on the fact that I was articulate and could use a turn of phrase that was a sound bite that got attention. And people were getting hostile, some people were getting hostile.

Chervin: But as I recall this particular event, this was an actual scientific conference.

Schneider: It was a scientific conference. I was invited.

Chervin: AAAS.

Schneider: That's correct.

Chervin: And the press, as they are allowed, attended ______.

Schneider: In fact encouraged at the AAAS, because of the function of the AAAS. And the president of the AAAS that year was Walt Roberts. So I was getting from Will Kellogg and Walt Roberts high level encouragement to communicate science to the public. They viewed that as absolutely essential to the health of science, particularly in a time of budgetary crisis, when if we didn't explain to the public and to the political leadership why science had something relevant to offer them, why should they support it, just because it's curiosity driven. This Victorian paradigm that Dick Lindzen and others were pushing, that we do only curiosity driven science, and we never respond to the pressures of politicians, was not the philosophy of Walt or Will, or me. And therefore, we were carrying science to the public and trying to explain it. So was Carl Sagan, which is, you know, one of the things I greatly appreciated about the clarity of his communication, and the wit and use of analogies. And I learned from Carl how to do that. So I go to this meeting. At the meeting I met Jessica

Tuchman, Barbara Tuchman the writer's daughter, and she had been a biochemist—I was interested in environmental chemistry problems at Cal Tech—and she had just partaken in the brand new program of the AAAS called Congressional Fellows, Congressional interns for young PhD.'s.

And I was smitten with the idea that I would get to spend a year working for a Hubert Humphrey, or a Teddy Kennedy or somebody in the senate. Because already I was realizing that I needed to explain to them what we could do in science, that climate science was going to be critical, that humans wanted to disturb the system, that it was very likely, if we continued our population, economic and technology growth—that we were going to be by the end of the century a major player. I genuinely believed that—still wasn't sure about warming and cooling then. Although I was moving toward warming, because as I realized aerosols were increasingly regional and therefore they weren't going to outweigh the CO_2. But somebody had to look at this problem. We had to, y'know, do the research, and that was what I thought I would do.

So I came back all determined that now having worked on the survey paper with Bob, and having gotten four or five more papers out, that I could take a year off, I could afford this. And I'd apply to the AAAS program. So I march into the UCAR office in the Fleischman building of Walt Roberts, who by now I have gotten to know very well, because he was a strong mentor and encourager to me, to popularize science and to broaden. And I said, Walt, you are now the past president of the AAAS, so I need you to write a letter of recommendation, which of course will get me into this program.

And he looked at me and he said, yeah it might, he said, and you are just dumb enough to do it.

And I looked—that was what what what?

And he said, you know, let me compliment you and insult you. You are so articulate and so broad visioned, he said, you will get in. Not only will you get it, but you will immediately become the science advisor to a Humphrey or a Kennedy, just exactly what you want. And it will be so enticing that how can you turn it down. You will have power and influence, you will be watching legislation influenced by what you are doing. And then five or ten years later when they retire, you will be viewed by the world as a partisan political hack. He said, let me make you a deal, you stay in science, you stay here, and I will give you so many contacts in science and science policy that you will have trouble keeping up.

And I thought about it, went home, and I said, I think he's right. And he kept his word. Three weeks later I was invited to spend the summer, a couple of weeks in the summer at the Aspen Institute, and about two weeks after that I was invited to a meeting at the Rockefeller Foundation to discuss the Sahilian drought, food climate, and sat next to a young guy who is a political scientist, who was dynamic and fun named Mickey Glantz.

Something we need to add back to the Walt Roberts story. When he told me that he'd get me so many things, he also said something which I don't think I said. Which is, he said, if you stay in science, you will be much more creditable and much more influential than you could ever be in politics. Because what ultimately counts in this world is the quality of your arguments, maybe not in the short run, but over time. And therefore, people will get used to you're being on top of the information, rather than associated with a partisan point of view, and that that will have more influence over time than the short term power trip that you get in Washington. So he used a very good argument, which I had seen in Columbia, with short term power trips. And that good arguments *are* what carries the day over time, and as frustrating as it is to watch the world where in the short run politics always wins, in the long run truth starts to rise to the top. And Walt was right, and he kept his word.

Chervin: Okay, we ready? So we are beginning now on Friday the 11th.

Schneider: We had just discussed Walt Roberts' promise to me that he would get me connected with policy people, and going to a Rockefeller Foundation meeting where I met Mickey Glantz. And Mickey was a political scientist, then at Lafayette, I believe, and interested in the politics of environmental issues. Soon thereafter, perhaps 1974, we'd have to look this up, Walt brought Mickey to NCAR, I think initially through ASP. There already had been a group at NCAR that was ostensibly looking at implications of atmospheric science, but in truth it was really an extension, and if I were to be less kind I might say an apologist, for the Hale Research Program that was looking at the social implications. Although there were good scientists in it like Allan Murphy, who was a fair and honest statistician, these are not social scientists. And people weren't really asking penetrating questions about whether these technologies were desirable, and who the winners and losers are, and who was paying, and who were the beneficiaries, and whether it was appropriate to be messing around with nature. Those are tough questions, and you won't expect to get them from people trained in meteorology in most cases.

And when Mickey came to NCAR, he was the first *bona fide* political scientist, despite his metallurgy undergraduate degree (his graduate degree was in political science) who was asked to address honest-to-goodness social science questions in the atmospheric sciences. And Walt Roberts, again, with his visionary broad views, saw that as a positive influence. Not everybody in NCAR shared that vision of Walt—who is this social scientist nosing around? And when they became somewhat suspicious, sometimes Mickey reflected their suspicion back on them. And the implementation of that kind of multidisciplinarity to NCAR, while still fundamental and I think a critical development in the atmospheric sciences in general, nonetheless was uneasy. I was also doing some social science on the side, but I was doing social science by asking broad policy questions of what kind of research were we doing, and was that research actually relevant to what people out there in society needed? I was talking to people in agronomy, for example, and finding out that they cared much less about whether climate changed by 2° than whether it changed the variability. Because the variability was what hurt them currently, and if climate change is going to change variability, that's even worse to them than any change in the mean. And nobody was looking at variability in the 1970's. And people didn't believe they could trust the means that were being predicted and modeled, so how dare we look at the higher order moments. Nevertheless, that's what people wanted.

And Mickey and I, you know, spent a lot of hours discussing those kinds of issues. And ISIG began to transform from basically a statistical outfit that was justifying the Hale project, to a legitimate social science group asking questions about the value of information and the distribution of winners and losers. In the 1970's there had been an international program called GARP, Global Atmospheric Research Program. It was UN, that is out of World Meteorological Organization, as the umbrella institution, but very strongly supported by most countries and . . .

END OF TAPE 4, SIDE 1

TAPE 4, SIDE 2

Schneider: Now we are talking about GARP. The Global Atmospheric Research Program had two objectives. The prime objective was to improve the quality and reach of long range weather forecasts. By long range they meant six to ten days. And in order to do that one had to collect a globally comprehensive, more accurate data set. Lots of regions in the oceans, and especially in the southern hemispheres and some developing countries, didn't have the richness and data that occurred in the more wealthy instrumented places in Europe and North America, for example. And if you wanted to do weather forecasts, you have to know what the initial state of the atmosphere is in order to run it forward. And there was a lot of people who believed that if we just could get a better data set, we'd be able to have the value of giving farmers six to ten days notice on, y'know, when they are going to get weird weather.

GARP had a second objective which wasn't really pursued heavily in the beginning, not until it became clearer and clearer that the first objective was not likely to work very well, that the limitations weren't just data but they were fundamental process questions, the resolution of models. And it was going to be a very tough nut to crack to get improved accuracy in six-to-ten-day forecasts. But the second objective was to improve the forecasting of climate. Now it wasn't completely clear whether it was meant by that, was months and seasons ahead, like if you had an El Niño, could you forecast the, you know, the heavy rain in California better?—or whether they meant the human impact on climate, which is over decades to centuries. And it was wonderful that that vagueness was there, because in the early 1970's, and especially after SMIC, the world community was now primed to take on a new task. Plus, with the existence of satellites to get data, and computers to process it and run models over periods of time as long as a hundred years, it was becoming technically feasible to actually ask questions about century long climate change.

As a result of that, the GARP second objective really started to rise. So there was almost a seamless transition from the first to the second objective among the world scientific community. And in 1974 in the summer, in the very same Lidingo island away from (that's "L-i-d-i-n-g-o, I believe)—island ten miles from Stockholm in the very same retreat for the Swedish labor unions, there was a GARP sponsored meeting on defining the science that was needed for the second objective. It was a very interesting meeting. It was chaired by Bertha Leen, but the person who was nominally put in charge of writing the report was John Kutzback from the University of Wisconsin, who was a very broad-minded meteorologist, trained originally by Reid Bryson, and therefore had already learned how to ask broad questions, yet had meteorological pedigrees that didn't frighten away mainstream meteorologists the way

Bryson would with his, y'know, forays into anthropology, agronomy and his certain belief that the world was cooling because of dust generated by the goats of Asia and Africa.

So this meeting was both international and multidisciplinary. Although it wasn't multidisciplinary to the extent of including social scientists, and it had very few people looking at ecology. But it certainly included oceanography; it included meteorologists; it included chemists looking at a broad range of issues, including the stratosphere and air pollution chemistry. And in that sense it was already setting the tone, at the international level, for the kind of multidisciplinarity that would eventually grow to include, you know, biological and social sciences a decade hence with the Vilot series of meetings, and then later on with IPCC. But this early meeting was really important, it was two weeks. It also had a comparable representation of measurers as well as modelers. And the modeling contingent was lead by Joe Smagorinsky, and the measuring group, by Pierre Morelle and Vern Suomi. Now those who know Joe and Pierre know that these are not shrinking violets. Their personalities are very strong, and they clashed constantly. Even the good offices of Suomi, who tried in vein to maintain the peace, didn't stop what went on at the meeting.

The climate modelers got together, and they defined what they thought were requirements for the kinds of observations they needed to derive paramaterizations and validate models. They involved high-resolution highly accurate global data sets. Immediately Morelle said, this is not necessary, this is too expensive, this will never be supported by national governments—and if you claim that's what you need we'll get nothing, because they will say we can't meet that requirement, so why give you anything. So of course Morelle's argument was essentially an economic/political argument. And Smagorinsky then just shouted back, we're not challenging the manhood of you measurers just because you can't measure up, this is what we need! And you can imagine the response from Morelle. I don't remember enough French curse words, but it was very entertaining. And Suomi would try to jump in and say wait, wait—what we need to do is point out that what we can do, and what we will be able to do in the next five years, is more valuable than what we have done, and that that will improve the situation, even though we could go beyond that in the ideal world. Which of course was the right answer, and Kutzback wrote that down, but the meeting itself was almost entertaining in the clash of these Titan personalities—each one zealously pushing the sub-tribe of modeling and measuring.

I hadn't actually realized how unusual my experience was at Columbia with the shock tube people, because when I started there was this gap between the modelers and the experimentalists, but then when they became convenient for each other, we had a very happy marriage for a while. And here this was definitely a shotgun wedding, or less. The other contention at the meeting was my fault. I had by this time had enough exposures to people in agriculture and ecology, thanks to Walt Roberts sending me to meetings in the Rockefeller Foundation, and also an Aspen Institute (a story I will tell shortly). But I knew that the questions that we are asking in atmospheric science aren't always the questions that are defined by the scientific needs as seen by meteorologists. But rather, what are the variables and the time frames at which these variables need to be presented, that impact on agriculture, ecology, water supplies, coastlines and so forth—the same argument basically that Smagorinsky and Morelle had.

The question isn't what we are comfortable producing, it's what does the political world need? In fact, later on this became known as science for policy. Whereas what was going on before that was really policy for science. Well, what do we need to produce in the form of research? The scientists were answering the question based upon their needs and their beliefs as to where the weaknesses were in the fundamental physics or in the observations. But that might very well be producing results that nobody really needed. Whereas what we really need, particularly things like variability and the time evolving path of climate change, that's what the society needs, that what we needed to know for ecology. It was really tough to do, and the scientists didn't want to even look at that.

So I asked Boline and Kutzback if I could have ten minutes to address the meeting and raise the question of, although I didn't use the phrase then, because that's a phrase that we evolved eight years later, of science for policy. And I went up, and I showed the world food situation, that food reserves by 1974 had dwindled to less than 10%, food prices had skyrocketed; that we were going to face famines if we continued, if there was weather variability; and that we needed to ask these questions. And it was really controversial. I was the only one in the meeting arguing that we had to take a look at the social implications of what we were doing. Moline angrily attacked this as utterly irresponsible because we should not address any questions before we scientists are ready. He reminded me of Dick Lindzen at the 1972 meeting. Other scientists like Burke Boline were wonderfully supportive and said that the science has to evolve, so that we not only address questions we think are important, but we address questions that the people who fund us think are important.

An interesting comment from Burke Boline in 1974, as he then became the general chairman of the IPCC about fifteen years later, and he carried that philosophy right with him to IPCC. When the rest of the world was ready for somebody with his vision, he was able to be there to step in. But he already had that vision. And it was interesting to see it early on. And of course, you know it wasn't fun being attacked by famous guys like Monine and others, I don't remember who, for even raising the issue about addressing questions before we were "ready" in science to address them. And I said, I didn't say that we should say we know them for sure, but we have got to look at the questions that people care about.

And Paul Crutzen was at that meeting, and he didn't say anything. He came up to me later and he said, you know you are fundamentally right, it is going to be tough to do that, let's work our way toward it slowly. And Paul Crutzen, seven years later, and we will tell a story about that, moved toward nuclear winter, which could not be a problem that is more difficult to establish scientifically or more important socially. So I was very proud to be part of pushing that early agenda, and I was really pleased that a lot of people agreed, although they didn't want to go right out front saying that, because still in the early 1970's arguing for the social implications of science was viewed as suspect by the bulk of the established community.

Chervin: And so in spite of that controversy, are you able to recall if any of those concerns actually appeared in the text of the report of the meeting?

Schneider: I would have to go back and read the GARP 16 report, which by the way, despite being now twenty-five years old, is still a damn good guide to the fundamentals of this problem, weak on ecology and absent social science. It's possible there's a one-liner in the beginning, you know, about how we have to help solve the problems that the world confronts, but I don't think it was very well represented. Basically, my contribution was a condensed version of the Schneider-Dickinson paper defining the hierarchical modeling needs. And Leith had a contribution, and Lorenz did, which were looking at theoretical questions like predictability and so forth.

Chervin: And so the review paper had appeared in print by that time?

Schneider: If it hadn't appeared in print it had been so widely circulated, and the community had so broadly seen it, that it already was having its impact. And I presume I was invited to the meeting because of being the author of that review.

Chervin: What other NCAR people were in attendance then?

Schneider: I would have to go back and look it up. I don't think Crutzen was at NCAR yet, 1974 he was just about to be at NCAR.

Chervin: He was probably still at the University of Stockholm then?

Schneider: Yeah it could have been, could have been, we would have to go look it up.

Chervin: Okay.

Schneider: One other meeting took place in 1974 that had high social relevance. Again, one of the very early assessments that presaged the kinds of controversies that would pop up routinely in the 1980's. And this was an invitation by the Arms Control and Disarmament Agency in the U.S. for the National Academy of Sciences to evaluate the environmental consequences of nuclear war. Crutzen, I believe was there. Mike McCracken and I were the two primary people who were involved in looking at the climate aspects. We all blew it. We basically were looking at scenarios where multi-megaton bombs, ten-twenty megaton bombs, would be used. And that's because missiles weren't very accurate then. And if you wanted to destroy a hardened target, and you could not assure yourself of a high probability of a direct hit, then you needed a massive explosion with a large fireball in order to take the target out. So what we were basically looking at, were these massive explosions dumping megatons of dust in the stratosphere.

So McCracken and I dutifully calculated optical depths from the numbers that the Defense Department scientists gave us, and concluded, boy this is going to be one hell of a volcano. We might cool a degree over a year or two. But it did not excite us to think that the weather forecast of a degree cooling after a nuclear war was even remotely comparable to the direct effects. So I think we basically said, this isn't really the prime problem, but it would be yet another after-effect. Crutzen, on the other hand, was working on the ozone question. And he and colleagues concluded that there would be a tremendous reduction in ozone. And the reason for that is these ten to fifty megaton nuclear weapons are so explosive that their fireballs were more than ten kilometers in diameter. As a result, they would reach up above the tropopause into the stratosphere, and they would dump the chemicals that were produced from the heat. Well one of the chemicals you produce from that kind of heat is nitrogen oxides.

So the nitrogen oxides would be dumped in profusion into the stratosphere, and nitrogen oxides, thanks to the work on supersonic transports that have done earlier, Harold Johnson and Crutzen's earlier work four years earlier, were known to deplete ozone, so they calculated out a dramatic ozone reduction after a nuclear war. I can't remember the exact numbers, fifty to eighty percent, something like that, lasting a few decades. You know, lots of skin cancer. Again I suppose if you are a combatant you can't get excited about skin cancer, relative to having taken out, you know, a couple hundred million people by direct effects, and then probably a couple of hundred million more by taking away the infrastructure that provides food and health and so forth. But it was not a completely trivial effect.

What we all missed, and this came up fifteen years later, was fires. And that, when we talk about nuclear winter, we will come back [to], very interesting story. In fact, while we are on the subject of fires, Paul Crutzen is the one who, about eight years later, introduced the fire issue. And it happened, ironically, because of the climate clubs that we talked about earlier. Climate clubs did not just stay in the first two years, and they weren't simply a ploy to demonstrate market pull for people. They were very popular, and I continued to invite guests from all around the world to give these talks. And they were very informal with lots of interruptions, and Q and A, and we scheduled them an hour and a half so there was a lot of time.

And I got a phone call one day from John Haldren, the energy analyst at UC Berkeley, the energy and resources group. And he was spending the summer at the Rocky Mountain Biological Laboratory, a place where I would go every summer to teach for one week in a population environment course there that he ran. He called up to say they wanted George Woodwell, the ecologist from Woods Hole, to come and spend a week with them, but they had no money. So could you invite George to give one of your climate clubs, because John had actually given one on the energy situation that was almost as well attended as the Erlich one, and a little bit less controversial. And I said sure.

I invited George because I knew that he had a very very controversial theory. George argued that the five billion tons per year of carbon that was injected by burning fossil fuels was being dwarfed by a factor of two or three by the injection of carbon from the burning of tropical forests. Wally Broker at Columbia considered this to be outrageous extrapolation. In fact at a Department of Energy meeting in 1977, he sat there and he screamed at George, you have one data point in Venezuela—you've just extrapolated from Venezuela to the world! And I remember, since Wally had just published a paper in *Science* about two weeks earlier, where he used the camp century Greenland record, and then extrapolated it to the world, I said nobody here would extrapolate from Greenland to the world, would they Wally? You know, and he could take it, he smiled, and he said, okay okay I did it too, he said, but he's wrong.

In any case, he probably was wrong, but, and Wally probably was right. But the key is that it was very controversial, and nobody had enough data to really answer the question. Norman Meyers, an independent scientist who lived in Oxford, was at that time beginning to call the world's attention to the carbon emissions from deforestation, and not only carbon emissions. He was very interested in the loss of biodiversity associated with that in the book he wrote called *The Sinking Art*. So these trends were in parallel, and these communities who did not know each other began to learn of each other, the atmospheric science needs for understanding what's happening in ecology, and the ecologist needs to understand there is a climate connection.

And so I brought George Woodwell out in the summer of 1977, and he gave his climate club on the fifteen billion tons that he thought was being emitted from deforestation in the Amazon. Well in the audience was Paul Crutzen who was then directing atmospheric chemistry at NCAR, and one of his visiting scientists and colleagues from the Max Planck Institute, which Paul was then later on asked to head in the 1980's, and that was Wolfgang Seiler. Paul and Wolfgang were not working on CO_2, they were working on CO, carbon monoxide, and the prime source was deforestation. So they were, needless to say, passionately interested in George Woodwell's deforestation numbers, because that was going to dramatically affect their CO budgets. So they attended the talk, and they were stunned by his numbers. They genuinely did not believe them, because it didn't balance the books on their CO, but they could not rule it out, it was possible. Supposing George's extrapolation was right, then they were missing something in air chemistry. So Crutzen did what any good scientist did. He organized an expedition to the Amazon. And then they were running in and out between burning trees, weighing the logs before the fire and after the fire, and so forth. Well of course, that's the way Paul told it, but the way the graduate students told it is, he sat in the Jeep directing them to run back and forth between the burning logs. But in any case, they got a massive data set on chemical emissions, and also discovered two things.

A significant fraction of the carbon was not going in the air as either CO or CO_2, although most of it was CO_2. It was going into charcoal. It was being converted to charcoal, getting dumped in the soil *and soot*. So there was a large soot aerosol that was produced from the flaming biomass burning, and that could account for at least 25% of the carbon emissions. So therefore, he was already beginning to reconcile his CO numbers, which had to have lower biomass burning, with Woodwell's numbers. Woodwell's numbers are not based on chemistry, they are based upon the actual deforestation rates. And therefore Crutzen said, well we can make up half the difference, maybe a quarter at least, maybe half the difference, just by recognizing that burning those trees doesn't automatically convert to carbon in the air. It could be soot, aerosol, and charcoal.

So that was a nice side effect that came out from the Climate Club. And even more fun is the fact that when Crutzen finally went to Europe in 1981, to head the Max Planck Institute for Chemistry in Mainz. This was a time right after Ronald Reagan was elected. Ronald Reagan had a radically different philosophy than Jimmy Carter. He was a cold warrior. He believed, or at least Richard Perle, his deputy, which we used to dub the hawk prince of darkness in the Department of Defense, believed that the proper U.S. strategy to stop the Russians was not to meet them with conventional weapons, because they could out-do us in manpower and ground forces, but basically to threaten that if they came near Europe we would hit them with tactical nuclear weapons. And this was called "theatre warfare." And of course the theatre was Europe.

Casper Weinberger, Reagan's Secretary of Defense, gave a speech in England in 1981, in which he said that such a nuclear war was winnable. This was essentially heresy, because up until this point the doctrine had been MAD, Mutually Assured Destruction. This is the first person who uttered from their lips that you could actually win one of these things, and therefore it would be rational to consider having one. It rekindled almost overnight the anti-nuclear movement in Europe, and to some extent in the U.S. And sitting in Europe was Paul Crutzen. The Swedes, who have always had their pacifist bent, immediately commissioned a study on the consequences of nuclear war, and decided to revisit the question of after-effects. So they went to Crutzen, the man who gave them ozone depletion from the 1974 National Academy Studies, and they asked him about this. He had a visitor from the University of Colorado at the time, John Burkes, who is a chemist. And they started looking into this question. But technology had radically improved from 1974 to 1981, if you consider that your missiles can be much more accurate than they used to be. As a result of that, the nuclear arsenals of the super powers, particularly the U.S., were now filled with many more but much lower megaton bombs. Because they could deliver them much more pinpoint precision accurate than they used to, because of the new electronics. As a result of that you didn't need the big clunky bombs, so if you have hundred and five hundred kiloton bombs instead of ten and twenty-megaton bombs, the size of the fireball is much smaller. It doesn't necessarily reach the stratosphere.

Therefore, Crutzen discovered, when he started studying the problem, that these fireballs won't be injecting much ____ in the stratosphere. And he did not want to reduce the after-effects or the seriousness of the after-effects of nuclear war by saying, we don't have a big ozone problem anymore. So he and Burkes started looking more deeply. And then Paul remembered his trip to the Amazon, and that when you torch a forest, what you get is soot. So he and Burkes calculated that the soot that would be generated from nuclear war from all these fireballs, from the collateral fires produced by the explosion of these weapons over all the various sites in the world, and he was largely thinking of forests in this case. And they calculated it out, there would be so much smoke and soot put in the air, that it would create a photochemical smog that would be toxic. And they were looking for the toxicity. So they were arguing that there would be this toxic pall that would be created, it would be damaging to lungs, and so forth.

And then they realized, but wait a minute, to create a toxic pall from the emissions of the biomass burning, you would have to have sunlight, because of its photochemical reactions. But the soot's going to block out sunlight. So their new effect wouldn't happen, because the sunlight not getting low enough into the atmosphere to create the photochemical reactions needed, would prevent them from occurring. And then all of a sudden they had their *eureka*. That's the effect, its going to block out sunlight, and that's how nuclear winter was born from that. And it really traced its way back in a funny and ironic way to John Haldren's phone call to get Woodwell out to do something completely different, while Crutzen and Seiler were working on CO. But then they learned about biomass emissions, which led them to the soot/ lack of sunlight at the surface hypothesis. It's a strange way that science works. Science doesn't always work by planning what you are going to do. It works by serendipity, but serendipity only works in the mind of brilliant people who can see the meaning of their serendipitous discoveries, and certainly Crutzen qualifies for that.

Chervin: And also who are not wedded to preconceived ideas.

Schneider: Yes, that's certainly true for Paul. In fact, I should have learned the lesson of Crutzen when nuclear winter finally came around, and Kurt Kovi and Starly Thompson and I turned it into Nuclear Fall, and there was a headline in the *Boulder Camera* with my smiling face on it, saying "Scientist Says Nuclear War Not So Bad." I can tell you how many hate phone calls and letters I got from people in the peace movement about that. And no, it wasn't smart trying to lower the seriousness of nuclear war. On the other hand, I've never been an 'ends justify the means' person and we felt we were right. But that's a long story to tell later. That's a mid-1980's occurrence.

Okay, let's return to the GARP 16 meeting in Stockholm in 1974. Because at that meeting I was asked to consider being on the working group on numerical experimentation, which had largely been involved in GARP 1 activities. It was a GARP working group, it was headed by Axel van Nielson and Leonard Bangston, and Leonard was Axel's young right hand; Fred Bushby from the U.K. med. office; and Larry Gates from, I believe by this time now, Oregon State. And Larry was largely—had been the climate person, and they wanted an additional climate person. And so I got involved in this, I guess, as had been my typical experience at that time, you know, ten-fifteen years younger than anybody else on these committees, maybe not Leonard, maybe only eight.

I went in there and I arrived day one not knowing what the committee does. Usually a committee reacts to requests. And I came there with a proactive proposal, which I had already written up. And my proposal was for a beauty contest for climate models—that I felt that it was borderline outrageous that five or six modeling groups around the world would double CO_2 from the pre-industrial value of 270 to twice that, and other groups were going from the current value at the time of about 340 to twice that. And therefore people are both calling those two times CO_2 experiments and they weren't. And I said, can't we standardize? People were running experiments on a variety of things, where whatever their particular group's random selection of how they wanted to run their experiment was happening. They would not plot in the same variables, there would be slight differences. So I just basically said, why don't we inter-compare the models, not just on how well they do on reproducing seasonal cycles and the regional distribution of climate, but let's also look at paleoclimate and let's look at their sensitivity performance to a standardized set of sensitivity analysis. Oh boy, several people representing various groups really got angry. Oh you are going to prescribe to groups . . .

END OF TAPE 4, SIDE 2

TAPE 5, SIDE 1

Chervin: . . . tape—its now Friday the 11th of January 2002, about 6:30 in the evening.

Schneider: Yeah, right before we light up the grill to have the hamburgers. Okay. So I was kind of stunned at the reception I got for my very first working with numerical experimentation in 1975. I suppose I can now, in retrospect, smile about this, and think who is this thirty-year-old kid who marches into this existing establishment that largely answers questions asked by various meteorological services in the world meteorological organization, coming in with a proactive plan that was going to tell groups that they had to abandon their own prerogatives on exactly how they want to double CO_2, and precisely how they want to be producing the diagnostics, and try to get a world standardization. So I can see, you know, in retrospect that I probably would have been a lot smarter to at least attend one meeting first and see what it was about, before trying to come there and propose a—what at the time was a radical, although now it is considered standard—way to do international science across multi-groups. So after I got finished being told that I was compromising the independence of science, there was support from Axel van Neilson that this was still a relatively good idea that we do inter-comparisons.

And Larry Gates stepped in as the peace maker and said: I think we should pursue this, but we shouldn't ask people to do sensitivity analysis, that's too much. Why don't we first just compare how the models do at reproducing current climate? And lets not tell them what variables to do, why don't we ask them what they like, and then we will try to build consensus.

Larry was a good politician, and he knew how to make that happen. And of course I could see that that was working, so I shut up and felt—okay, good I'm glad I pushed this. It's fun because when Larry went to Livermore in the 1990's that is precisely what he pursued, what he took him, I guess almost fifteen years to get adequate resources and cooperation from the community. And not only do they look now at what the models do in common sets of units, and on common grids for their performance on today's climate, but that was called what, AMIP Atmospheric Modeling Inter-comparison Project. But then there is a CMIP, that's for Coupled Models, Atmosphere Ocean models, there's a PALEOMIP, and . . .

Chervin: EMIP, right.

Schneider: And what's the next one that's coming, there's a new one which is going to even have ecology models in it. And it takes a long time to get what seems like obvious ideas implemented through world institutions that each have their own agendas. And Larry gets a lot of credit for pulling it off. I was utterly frustrated that this took a decade or more to happen. I thought it should have been instantaneous. But we did have a meeting that came out of that first meeting of the WG&E, which by the way I still remember well. It took place in Devon, in the U.K. And it was a four hour train ride to the university there. And Klaus Hosselman was invited, and Klaus is an amazing peripatetic broad intellectual, interested in everything from the development of stochastic climate models, to oceanographic coupled models. And now he and I actually have had a lot of communications in the 1990's on how to incorporate economic models into climate models, and develop an integrated assessment. So I loved Klause's quick and nimble mind, and his absolute volubility and lack of inhibition to just jump all over anybody with a dumb idea. I haven't talked yet about Francis Bretherton, Klaus was the German version.

And Klaus came to this meeting for an agenda item, and he only had one day. He had to go, the work wasn't finished. And Larry Gates being ever the problem solver said, well there's more to talk to you about Klaus—I'll ride the train back with you to London. And Larry went with pad and paper in hand, rode back with Klaus, and came back to the meeting about ten hours later with five pages filled up with pads, milked him for what we needed for the project. And I really admired Larry's capacity to solve problems by whatever it took to do it. And that was also an interesting experience for me, learning about how some of the more senior people there in their calm ways could accomplish things, even with volatile characters like Klaus Hosselman, or in those days me. So the WG&E, you know, was serving its purpose, but it still had its primary function of responding to questions and helping inter-comparisons in the GARP 1 context, and it slowly moved toward GARP 2. I didn't last very long. I didn't really, I guess, have the right personality for their slow and deliberative process. I think I was on it for a couple of years and then moved on to other things.

Chervin: I'm trying to recall exactly when that first inter-comparison took place. I believe it was 1977?

Schneider: No, it may have been 1979.

Chervin: In Washington, D.C.

Schneider: In Washington, D.C.

Chervin: In the _________.

Schneider: So here it was three years later, Gates ran the meeting and he asked me to help him co-run it, in recognition, I guess, of that initial initiative. And that was a very interesting meeting, because we had not yet had a formal

MIP. There were no model inter-comparison projects, so we brought everybody together to discuss how to do it. That was actually a very exciting meeting, as I recall. We invited a whole host of characters, not just modelers, but we invited house critics like Dick Lindzen, and also people like Ramanathan to discuss observational components. And Ram also had done, by that time, some fundamental calculations where he showed that people who were calculating the CO_2 greenhouse effect had missed a number of hot bands. And Ram had made a fundamental contribution to that effect.

I mean there were all kinds of characters there. There were careful and sober presentations at low key and articulate from Jill Yeager. And then there were highly mathematical tour de forces from Michael Gill. And then there was scorn and cynicism from Dick Lindzen, who I still remember—and a number of us to this day still laugh about this—who sat in the front row reading the *Wall Street Journal*, and loudly turning the pages, particularly during Ram's talk, mine and somebody else's. And I remember Larry, Larry Gates is rolling his eyes. But Dick had contributions that were worth hearing, and that's why we invited him. And the meeting, however, set the tone and it is a GARP publication for making it legitimate to have model inter-comparisons. And Larry was able to build from the report of that GARP report into getting funding eventually to run the MIPS.

Chervin: Okay, so we have kind of branched off a few years, but I don't think we've adequately wrapped up the full consequences of the JEC report, and the transition of the Laboratory of Atmospheric Sciences to a new divisional and project structure. I believe the division was called AAP for Atmospheric Analysis and Prediction, and I believe there was a full-fledged climate project, in addition to the more informal Climate Club.

Schneider: Yes. Well, in fact, we also need to talk about the leadership change, both in personality and in style. Because remember the JEC had intimated that there were quality problems, and suggested that there was occasional lack of rigor. And while nobody doubted the vision of Walt Roberts—Walt was not a bench scientist, or the classical paper publishing scientist based on analytic, numerical, y'know, or experimental techniques—that was not what he was doing. He was the person who saw how things fit together. At the same time, Will Kellogg was a manager, and a person who looked for the relative importance of things, and he also did not himself generate complex equation oriented papers.

So there were those who fit the view of what I like to call E to the IKX science—that one needs complex methods to be a truly good scientist. I personally do not agree with that. I think people who deal in concepts, when those concepts are fundamental and connect them right, are just as good a scientist as people who deal with complex methods. But that wasn't the dominant paradigm in those days. And in the wake of the

JEC report there was a clear search to change the leadership. I can't remember the exact dates, but it was probably late 1973 that the President of UCAR, which was Walt Roberts, the Director of NCAR, which was John Firor, and the Director of the Laboratory of Atmospheric Sciences—which was the combination of mesoscale, chemistry, atmospheric physics, and dynamics and what was, you know and the fledgling climate program, got split. And first at the top, they brought in a new president, and also a new director, all the same man, Francis Bretherton, who was a very analytically strong geophysical fluid dynamicist, oceanographer, and a completely different style of character from Walt Roberts. While Walt tended to trust people and dealt at the level of how science fit into the scheme of things, Francis dealt at the level of "show me the equations," "show me the data," and in your face kind of oversight. Now that was a radically different style. So not only was Francis the president of UCAR, but he also was made the director of NCAR, quite a hefty job. And he was completely hands-on, to the point that anyone doing anything had to explain it to him in technical detail, which was quite a different issue. Now Francis had a habit of reacting in sort of explosive phrases: "Well what do you mean by this?" and "Why didn't you do the different scheme that way?"

I actually found that entertaining, because he was so smart and could learn so quickly, that I found it fun debating with Francis. But I am that kind of personality. I like in-your-face, and I didn't mind it at all. I had big battles with Francis because I thought he had a narrow view of what constituted good science in his early days. But I thought, y'know, that he cared a lot and wanted people to do quality work. Many other people who were quieter bench scientists (classical definition of working quietly alone) were utterly intimidated because Francis would shoot from the hip with both guns blazing, not because he was being mean, but because that was his style. And he would forget sometimes that he was the director and the president, and that this was very intimidating to people just by virtue of his own position. So there were some rocky days with this management style. We would, at the time, debate how to proceed beyond the second generation GCM that Warren Washington and Akira Kasahara had built. And the question was, should it be a fourth order model that Bob Dickinson had taken from Dave Williamson and proceed? And then later on Chuck Leith got involved, and very strenuously argued that it shouldn't be either of the second generation or the fourth order model, both of which were finite difference models, but rather they should be spectral models. But I get ahead of myself.

First the Laboratory of Atmospheric Science was broken up. And instead of having one mega-division headed by Kellogg, instead there were, I think three—AAP, Atmospheric Analysis and Prediction; chemistry division; and then a mesoscale division, or that may have come later. We will have to check the history on this to make sure it was right. The first AAP Director was Chuck Leith? I am stumbling because I am having trouble remembering what happened. Robert, you think that it may have been Doug Lilly that was the original AAP Director. The structure was there were projects. There was a climate project; there was an observational project.

Chervin: It was Chester Newton's empirical studies group project. There would be Jim __________ small-scale analysis and prediction project.

Schneider: Newton's empirical studies group, right, with Chester Newton and Madden and so forth. Deardorff's small-scale analysis prediction [project], which probably later on became the mesoscale division.

Chervin: Exactly.

Schneider: Before a right, right, became the mesoscale division.

Chervin: That became first the mesoscale research section.

Schneider: Research section, right.

Chervin: And I believe the original director of AAP was Doug Lilly.

Schneider: Right. But let's be honest, and say that everybody believed the Director of AAP to be Francis Bretherton. Because almost nothing happened in that division that Francis was not involved in, hand and glove, equation and observation. And it was impressive how fast he could learn many things, and he could see through shallowness really quickly. He just didn't have a very effective management style about bringing his people along, and many people were intimidated.

I remember in 1974 I began to get students coming to me. Cliff Mass, a student of Carl Sagan's, as an undergraduate came and worked with me for a summer and then continued on for I think another year, I had forgotten which, before he went on to get his Ph.D. at University of Washington. And we built a simple climate model, which actually was the first one to that date that actually looked at the combination of natural forcing, such as volcanoes, potential solar forcing where we used the Kondratchiev and Nikolski, the Russian balloon observations of solar energy, which by the way we didn't believe was real, but we did it anyway, and the sunspot observations that Jack Eddy had generated from his recent history study where the _________ minimum, the period from 1750 to 1800 that had no sunspots. And we combined that with CO_2 forcing, and wrote a paper in *Science* in 1975 that suggested that in order to figure out how to deal with the observed record, one had to combine both natural forcings and human or anthropogenic forcings. And we said, we are not sure we have it right, but this is the way this problem should be approached. And we also said it had to be solved as a transient, or you wouldn't get the sequence right. And we set that up with a simple model. And we had an appendix which really was the first place that climate sensitivity was ever formally defined in the literature—although in truth Chuck Leith and Jim Coakley, who we hired through the CIAP grant, and I really evolved that in the CIAP program.

I began to discover that working with people like Jim Coakley and Cliff Mass was a way to substantially multiply my effectiveness. And Zvi Galchen, he and I continued to work on our climate stability work. In fact when Jerry North came, he and Coakley solved analytically the Bodiqo Sellers model to find out about why it was so sensitive. Zvi and I solved it the year before that, in fact we got an honorable mention for the NCAR publication prize in 1973 for that paper. And then Jerry and Jim used gander functions, not cercoharmonics, they used the gander polynomials, you know, the 1-D version as sort of two polynomial fit. And they basically came up with the same answer we did, but shown analytically that there was an unstable branch. And I always was amused that they got the whole prize, we only got the honorable mention. We solved it first, but we used the dirty numerical method, and they did an analytic trick, and anyhow the powers that be liked that better. Nevertheless, it was fine work and there was a lot of excitement in the group about that kind of thing.

And I started to realize it was essential, if we were going to do climate modeling and climate theory, that we not only have a hierarchical approach, but that we could not possibly have enough resources internally within NCAR, having one climate project that was only started in the wake of the JEC, and we needed to have university collaborators. And I set up summer workshops, where we brought many people in from the universities, Jerry Namias for example, and John Mitchell, and Bob Sess and Ramanathan. And we had one to two week long discussions about climate sensitivity, and about modeling and stability. And formed basically an expanded cohort for which NCAR was doing exactly the service I thought it should have done to the universities—providing some intellectual environment and a scale of operation that was bigger than the university could do. But we could bring all these kinds of people together.

And that's why I would argue that the second half of the 1970's at NCAR was the place to be: the most intellectually exciting place for the development of climate theory and modeling, not so much from the 3-D point of view, but from the point of view of understanding processes and interactions using that hierarchical approach. We had Bob Dickinson in the mix all the time keeping all of us honest with his, you know, analytic brilliance. And Francis, who came to the meetings and was a wonderful participant—here's the president of UCAR, sitting in there with the graduate students, you know, and slugging it out on the equations, and it really was exciting.

The problem came—no, let me before I say that, one thing—then the survey paper that Dickinson and I did on climate modeling came out, and we got our honorable mention for the NCAR publication prize, as I said earlier. And it also attracted a lot of attention, because it was a systematic approach to climate modeling, and helped explain to the meteorological community that this was not some voodoo, that there really was method here, and it could be built upon what they already understood, for the workings of the atmosphere. And we went beyond that and said, and the oceans.

I got a letter from Mr. Ridel of the D. Ridel publishing company, a main publisher of journals and academic books, and he said I would like you to edit a new journal for us on climate modeling, and the new results. And I was very flattered because this was probably 1974 or early 1975, you know, twenty-nine to thirty-year-old getting asked to edit a journal _____ was certainly something that, you know, was a nice ego feather in my cap, but I thought long and hard about it, and decided it was a bad idea. Because I felt that climate modeling, while a nascent field, would be built on the integration of good disciplinary science from radiative transfer, from atmospheric dynamics, from hydrology, and oceanography and atmospheric chemistry. And that at the moment, we needed to know enough in depth about all those fields, those multidisciplinary subfields, that they needed to be integrated in models, and that if we had a journal that just did that, we would become disconnected from the disciplinary journals that the societies were publishing and it's too soon.

So I wrote back to Mr. Ridel, thanked him and said that I thought that it was not appropriate to have a corporate journal with no page charges, but with charging a lot for the journal competing with the society journals and disaggregating itself from the disciplinary journals. Because climate modeling probably belonged in the mainstream journals, and I wanted to see the *Journal of Atmospheric Sciences* and the *Journal of Geophysical Research*, where Galchen and I, and Coakley, and North and others were publishing—broadened to include climate modeling, rather than to move it off into its own journal. So in a way I was a conservative and a traditionalist, and made that decision.

In 1975 I came up for what was called the five year appointment. And this appointment I got—it required letters of recommendation on the outside—and I was apparently the youngest person to ever be appointed. And while that was flattering, Francis came in to congratulate me for the appointment, and it was a classical good news/bad news interview. The good news was, you got the appointment—congratulations, you should feel proud. You're young and we think that you have a great future, and that we want your work here. And then the bad news was: We don't like your style kid, you're spending your life with students, and you are talking about policy. Remember, Walt Roberts had brought me to Aspen, because after Francis took over, Walt's next job was heading an environment and energy program at the Aspen Institute. And he was interested in [the questions]: Could the world feed itself? Could the world provide energy? Would we have adequate housing? And how would climate change affect all these things?

And I went to these meetings, and Francis said: We're really worried. You are spending too much of your time dealing with these applications and implications, and then a lot of your science is being done by students. You know we expect to see you single-authored, sitting down there at the keypunch, working like everybody else. And I looked at Francis in utter amazement and I said, are you kidding?! You expect that I should be—stop training students; reduce my productivity by not having multiple numbers of people working on issues; and stop addressing the great problems of the world, which is why the society gives us money, just because it is the tradition? And he said, I didn't say you were wrong, I am telling you [that] you won't get senior scientist because people are really worried about that style. These days I would call that "defining quality by looking in the mirror." It wasn't the most pleasant conversation. And the good news was swamped in my head by the bad news. In fact, I remember being really angry about it.

A few weeks later, I went off to England to attend the Norwich workshop, the brand new climatic research unit had just been established, run by the senior climatologist Hubert Lamb, but in truth it was really its intellectual luminary was Tom Wigley, who was a physicist from Australia, who had just been brought over. And he was very very broad and eclectic, as comfortable working in climate and history as he was in data, as he was in building models. And that was an exciting meeting, and many many NCAR people were there, and we defined again a broader spectrum of climate subdisciplines, now including history and paleoclimate much more directly than had been done before, because of the influence of Lamb and Wigley.

When I finished that meeting, I flew over to Holland to meet with Mr. Ridel. Because I had asked to talk to him, and I said I have reconsidered your offer, and I want to talk to you about it. And he said, please come over. And I went over to see him, and I said, you asked me would I run a new journal that would publish results of new and exciting research. And you had suggested climate modeling, and I gave you reasons why I thought that was not a good idea. I've come up with another idea, which I think is absolutely essential and I wonder if you would be interested. I would like to publish a journal that will publish the integrative interdisciplinary results of combining the various subdisciplines of climate science—atmospheric dynamics and oceanography; hydrology and climate models. For example: history and climate; economics and climate; agriculture and climate. And he looked at me, and he said, will people publish papers? And how will your handle quality? And I said, we have to evolve something new. We have to figure out how to define quality in an interdisciplinary context. And I said, before we will call for papers, we will do that, and we will insist on peer quality. And he said, go ahead, let me know how you are going to handle it. So I put together an editorial board that consisted of people like Margaret Mead, Carl Sagan, Bob Dickinson, Jack Eddy and a number of other—Ken Hare—of other senior people—Murray Mitchell—who were super helpful in defining what it means to have "interdisciplinary quality."

And we defined three steps. First, communications have to be much better than they are in the average disciplinary article. That is, the jargon has to be defined, specialized terminology or fancy math has to be appendicised, at the level that multiple disciplinary people can understand. So cross-disciplinary communication was step one. Step two was, if its inter-disciplinary, that means it will be combining information from multiple disciplines. So the multi-disciplinary components had to be state of the art. That is if you are doing an agriculture and a climate model paper, you had better be state of the art in the climate model and state of the art in agriculture. Now, no quality academic program would promote somebody for being state of the art. They expect you to advance the state of the art. Our requirement was not that. We didn't demand disciplinary originality, we demanded disciplinary accuracy—state of the art in multiple fields, but not necessarily original.

And the third component, which we thought was our most creative, innovative was we agree that originality is essential, but the originality should be in how the knowledge is combined to do one of two things. Either solve a systems problem, which requires multiple disciplines to be able to approach it in a way that no discipline could by itself do; or solve a practical problem, like a climate policy issue, or a conservation problem in climate change [such as]: What do you do to design a nature reserve?

So I went back to Mr. Ridel, he was very satisfied. He said this is quality peer review, and he gave the go ahead to create climatic change. So in a way, climatic change was born out of my frustration at being told I shouldn't be inter-disciplinary. And I guess I said: Oh yeah, not only am I going to be inter-disciplinary, I am going to start a journal just to publish the stuff that you don't want me to do. Not exactly a very mature way to deal with things, but then again I don't think I was exactly handled well. The irony, and I will jump way ahead of the game now, is that Francis Bretherton, by 1980, when he was deeply involved in the creation of Earth Systems Science and the so-called Bretherton Wiring Diagram, was one of the most inter-disciplinary people around. And he and I evolved to have a very very good rapport and understanding about exactly those issues. In fact on more than one occasion, over the time I have been asked who was the best and who was the worst NCAR director, and I unhesitatingly said Francis Bretherton. And they looked at me, what do you mean—how can he be the best and the worst? I said, first year or two of Francis—the worst, shooting from the hip, narrow disciplinary view, not aware of his status and dealing with people and the last year or two . . .

END OF TAPE 5, SIDE 1

TAPE 5, SIDE 2

Chervin: . . . fifth tape.

Schneider: All right, and we were talking about Francis. And I said the best and the worst director. And the best director was late Francis, late meaning in the end of his tenure at NCAR in 1980-1981, around that time. Because not only did he have a brilliant and detailed set of insights to the science, but by that time he had grown in intellectual stature, where he, like Walt Roberts before him, was seeing how science fit into the larger scheme of things, and was actually proposing quality interdisciplinary work. And I was very disappointed when Francis, for health reasons, needed to let go of those positions that he was jointly holding as Director and President at UCAR, because he had really earned the right to those jobs through learning by doing and on the job training. Anyhow, lets go back. So in the earlier days, we had AAP, it had a bunch of projects. Do you remember the projects?

Chervin: Well as best as I can recall there were about half a dozen. There was of course the climate project, and then a new and separate oceanography project.

Schneider: That's right.

Chervin: . . . that was headed by Bill Holland, and then there was an empirical studies project headed by Chester Newton. There was small-scale analysis and prediction project headed by Jim Deardorff, and then it was either a large-scale dynamics project or a—no, it was the NWP project.

Schneider: Yes.

Chervin: Which was, as we discussed earlier GARP 1.

Schneider: Right.

Chervin: So that was the NWP project, headed up by Richard Somerville, and that was concerned about adding large-scale analysis to create initial conditions, and then do order the five to ten day forecasts.

Schneider: Right.

Chervin: And finally there was a tropical physics and dynamics project that was associated with GATE, the GARP Atlantic Tropical Experiment.

Schneider: Right, the measurement program . . .

Chervin: Right.

Schneider: . . . in the off Africa.

Chervin: And so I think those were the six projects.

Schneider: Right. And these had emerged from the competition following JEC from internal proposals that were written and evaluated by the NSF trustees committee. Good, I think we may finally have gotten our history straight, although we can check this with others.

Chervin: We are only talking about [on the] order of twenty-seven, twenty-eight years ago.

Schneider: Yeah, indeed. The key was that the project orientation was there, yet there was still the demand that promotions be university-like, and that tension which we talked about earlier on, still remained between your incentives being to publish individual papers, and your official job being to look project-like so as not to compete with universities. And a set of senior review group was set up to try to evaluate quality to do this, and it was always controversial. And there was always a debate about this tension between large-scale service oriented work, which we officially had in our lexicon, and the truth, which is we only valued things that looked more traditionally academic.

Chervin: And as I recall towards the end of the 1970's, I believe it was in 1978, Chuck Leith agreed to become the division director of AAP, and as I recall about the same time the projects transitioned into sections. The climate project became the climate section, and in those few years of its existence, it became the biggest of all of the sections within AAP, and for organization purposes it was broken up into three groups. There was the Climate Sensitivity Group, there was the Cloud Climate Interactions Group, and there was the general circulation model coordination and operations group. And I believe Bob Dickinson was head of the Climate Sensitivity.

Schneider: And I was deputy, right. And I think there was only one other person in it Coakley, but I can't remember.

Chervin: Right and I believe Ramanathan was head of the cloud climate interactions group.

Schneider: Umhm.

Chervin: And what can we call, in terms of the focuses and thrusts of climate sensitivity? What were the key issues at the time?

Schneider: Well, in order to answer that question, let me first say that in going over this history, and we will remind our readers that this is an oral history, we don't swear by the names of these groups, the acronyms of the leaders. But the one thing that was absolutely clear from the very first presentations at the JEC meeting, when there was a question about whether we should have a formal climate research entity at all at NCAR, we now had the largest section in the basically analysis and prediction division on climate. It had indelibly become acceptable scientifically for climate to become a subject. There were oceanographers around too, you know, it wasn't—I don't remember where they were—they were not in the climate section, but they were in the same division.

At that time I continued my style, the style I was told not to, of bringing in outsiders. In fact, I accelerated it to the point that I remember in 1976 the very last computer program I ever wrote solo. I sat down on the keypunch, I wrote three or four pages of code on the multiple reflections between the surface and cloudiness, depending upon whether the clouds were patchy, that means distributed in a random order around a grid box, or whether they are unpatchy, as if a front, you know, one portion of the box covered completely, the cloud, the other cloud free. And I showed that there was an indeterminacy, I should have called it an uncertainty principle in climate modeling—it would have been a famous paper. But I just, y'know, was talking about the indeterminacy in determining the average or parameterization of reflectivity, depending upon whether the clouds were patchy or not.

And it was the only program I had ever written in my life, of the hundreds I had written by then, that compiled the very first time I submitted the box of cards. And this was perfection, I knew I could do no better. And that, as it turns out, was the very last code I ever wrote completely myself. I was very proud of this code having worked the first time. I wrote a paper for it, showed it to Bob Dickinson, and of course Bob had his usual seven criticisms, five good suggestions, and was already rewriting the paper for me about how it should be better. So I then rewrote the paper the way Bob suggested it. It was a double-authored paper with Schneider and Dickinson, too. He smirked and said, okay I accept—he earned it. It was easily accepted at *JGR*, it wasn't one of the world's best papers but it was fun.

Why was that the last time I did a sit in front of the keypunch all by myself write a code from scratch? It wasn't because it compiled for the very first time ever, it was because Starly Thompson had come to me, Cliff Mass was there. And I was getting students who were so good at programming. And it was clear to me that that wasn't the best use of my time—that I should be spending my time dealing with ideas; dealing with where we should be going; arguing about what sub-systems should be coupled to answer important questions; looking about how to validate climate models, and training students at the same time. Starly I later hired as a support scientist. And in fact I remember well Nusep _______ was there as a student at that time. We worked on stochastic models. Hosselman had derived the stochastic model, which was a zero-dimensional model. It was a world average, where the fluctuating term was an additive term in the right hand side. And I didn't think that was correct, because what causes the fluctuations in the real world is not an additive term at the planetary radiation balance level, but rather the fluctuations due to storminess.

So [what] we did is we did a one-dimensional model. Basically we took the code that Zvi Galchen and I had used for our Bodiqo Sellers stability work, and we put a stochastic term in the heat transport part of that. And what we found out was that the relative magnitude of fluctuations was a factor of three or four less than Hosselman had calculated when you put it in the right place. That was the kind of things that we did, to answer your question, in the climate sensitivity section. And it was largely being done by students. I had a style where I have two or three students in the room, and they would be making a run. They would be dropping off—there were still boxes of cards, we hadn't quite yet transitioned to terminals, it was just about to come, the VT100's were on the way. And I would say, what are you running? And they would describe what they were running, and I would make everybody go to the board and guess what the model was going to say. What I was trying to do—aside of have fun, was to develop our intuition. And the best way to lay it out is to admit in front of your colleagues what you think it is, and then of course when you are wrong, that really gives you a visceral reason to dig in that model and see what's going on. And it was a very exciting group. We had a lot of fun doing it, and we learned a lot and worked collectively.

I did something, and I later discovered [that] university professors do all the time. I couldn't keep up with the literature, it was too big. I was too busy, I was involved in a lot of national and international programs, which I will talk about in a minute. And as a result of that, I would assign blocks of literature to various students to read. And they would read, and then they would present lectures on it, like a reading group. And then I would do a few as well, and we'd divided it up. And that way we could digest the literature much more quickly than if we had to do it alone. So I not only didn't listen to the advice I got in 1975 to back away from the style, I adopted it and embraced it wholeheartedly.

At the same time I was working with climatic change to broaden the paradigm to include multiple disciplines. And the larger world of atmospheric science was beginning to recognize that part of its next phase would be as a partner, particularly with people in agriculture, oceanography and hydrology. And Bob White, who had been the head of NOAA, who now when he lost that position or retired, I can't remember which, set up through the National Academy of Sciences the Climate Research Board. And he then had summer workshops, two to three week long workshops in Woods Hole in 1978, 1979, in which he tried to define the new evolving direction of climate research. And I remember being invited by Bob to the first workshop. I was the only one there, out of mostly traditional meteorologists, atmospheric scientists, arguing that we needed to have a multi- and eventually interdisciplinary approach. Wow, what shout-outs, what fights, what threats that was. Bob, you know, sitting there above the battle adjudicating it, sort of sticking my neck out, y'know, into the chopping block to fight these battles. And it was not very pleasant. I can remember some pretty nasty events. Bob Dickinson was about the only guy there backing me up in these kinds of fights.

Then we came back the next year, and Bob White had added Mickey Glantz, and added Ken Hare, and added Norman Rosenberg. And all of a sudden there were now four or five of us fighting to include integrative, interdisciplinary components. And it wasn't so controversial anymore. People have to get used to things. And Bob was absolutely brilliant in his capacity to evolve group consensus slowly over time. And I was the sacrificial lamb initially, and I was of course very willing. I was in fact thrilled that he asked me, although these were stressful times. They also kept me on the road, they kept me away from NCAR. And my productivity was maintained by large measure through having people from the outside—students programming the computer, based upon the discussions we had in my office when I was around, the half time that I was around—for going over how we ought to do things.

Now in 1976, I decided [that] I wanted to learn how to teach, and I hadn't really done more than lectures. So I was invited by Jim Hayes and Wally Broker at Lamont-Doherty Geological Observatory to spend a quarter with them. So I became a UCAR affiliate professor, and evolved a course in climate modeling, which was a sort of zero-dimensional theorem proof class about how feedbacks worked, and what transients were, and so forth. And I taught this to the students of Wally and Jim Hayes. They included people like Bill Rudiman, was one of the students in that class. And later on when I did it three years later, Alan Mix and Doug Martinson, the people that were involved in SPEC map. And these were geologists who had had some math, but didn't know much about atmospheric science. So starting with zero-dimensional models and explaining things was a good ground up way to do it.

Meanwhile, while I am there I am getting taught by these people all about geology, and all about paleoclimate, and the absolute essential need to use the paleoclimatic record as the backdrop against which we calibrate our understanding of the tools that we used to make predictions of the future. We don't use paleoclimate, that is historic situations, to give you analogies to the future. Because the global change stresses—the land use changes, the atmospheric CO_2 and aerosol forcings—they are all going to be different than anything that's ever occurred in history. What you do is you use them to derive the models and to test the models. And I began again to broaden myself increasingly as an inter-disciplinarian, plus there was the venue of climatic change that was sitting there, waiting to publish these papers when people met the review criteria of cross-disciplinary communication, multidisciplinary accuracy, and interdisciplinary originality.

So the process began to emerge, as the development of integrative studies and climate was going to be defined much more broadly than atmospheric science. And at the same time the journal was there to help publish these things. And now we had official support in the form of Bob White and the climate research board at the academy to bring it forward. And Francis Bretherton began to work with NASA in developing Earth Systems Science. Just around that time in the late 1970's I got a phone call from Bill Hay from the University of Miami, who I had met as an NCAR—I think he was a members rep.—and he, as a geologist and paleoclimatologist said, I have a young student who is very interested in climate, and he has been doing paleogeographic reconstructions, you know, where the contents were in the Cretaceous, and wants to figure out what that means climatically. I can't really help him do the climate, will you take him in your group, I'll pay. So I was delighted to have Eric Baron join our group. And he was there, and I introduced him to Starly, and they roomed together.

And that led to many collaborations between Starly and Eric and me, where we now began to incorporate geologic information into our multidisciplinary perspective, and evolved toward even broader integrative studies. In fact Eric then later was hired by AAP, and worked with Warren Washington after he became an ASP post-doc, on using the general circulation model for Cretaceous simulations. And of course Eric then later on was hired by John Dutton to create the very broadly interdisciplinary Earth Systems Science Center at Penn State—one of the few universities in the world that actually would allow a split between departments, where Eric controlled the money for slots and the departments controlled the tenure. But if the departments insisted on the disciplinary narrow minded person Eric didn't have to give them the money. It was a brilliant design to get around the university disciplinary narrow mindedness, where they only wanted to promote people who did disciplinary originality. So Eric spent a number of years with us, and he broadened us and we broadened him. I think it was really NCAR at its finest—you know, helping to create a new direction for the community, while at the same time improving our research product.

In the late 1970's there were a number of other events. I had mentioned Starly Thompson arriving, and you asked, Bob, what the climate sensitivity group was doing. I began increasingly to realize that it didn't matter what CO_2 doubling in equilibrium meant, the classical thing that people working on it—what really mattered, and I learned this from ecologists and agronomists, people I had met at Bob White's conferences and the Department of Energy had set up several workshops, and I was part of them, to do integrative studies. David Slade at DOE had inherited the CIAP work, and was setting up a massive program to look at a full-blown cross-disciplinary assessment of the greenhouse problem. In fact, he had Roger Revelle, with whom I worked very closely, directing this program. And we evolved what we called integrated assessment. We evolved studies which are just now taking off.

Unfortunately, in 1981 when the Reagan administration took over, and they ran on the basis of eliminating environmental and social sciences, they looked over this program and they cut this program simply because it was environmental and social. And in a great ideological scissors which then led to a very controversial hearing in which Joe Smagorinsky, Lester Lev, Roger Revelle and I testified before the subcommittee in the house of junior Congressman Albert Gore, which was his first ever climate hearing in which we had an ugly confrontation with the new Department of Energy people, who had just cut the Roger Revelle program. And that was the beginning of Al Gore's involvement in climate, because the integrative program was being cut. Because it involved study of what—of so what if the climate changes, and what are the policy implications? And the Reagan administration was hell-bent against letting anybody discuss a policy implication that might affect the fossil fuel industry, which they were patrons of.

But I get ahead of myself, because let's go back to what Starly and I were doing. We recognized that from agronomy and ecology, that they didn't care about whether the temperature warmed up 2°, they cared about whether it was 2° in twenty years, fifty years or a hundred. The rate of change was much more important than the amount of change. Because the capacity of a system to adapt, whether it is farmers who have some, you know, foresight and some resources to plant different crops or irrigate differently, or nature which does not have those resources—they only have the genetic endowments that they have, but that gives them some degree of flexibility. The more rapid the changes, the more damage is done to nature. So I understood right away that what really mattered was the transient.

So Starly and I began to think about this problem, and with Nuset Dolfus and Starly and Eric, I set about making homebrew wine. We would have about every other week an ethnic food party. Bob Chen from MIT was working with us as well, and he would make dim sum. And Nuset would make baklava and Turkish things, and we all would make, and Starly would have to do some kind of Texas ribs. We would all have a different ethnic meal every other week, you know, at the house. So then we got this whole wine apparatus, and we were sitting there and scrunching grapes, and making also plum wine from the backyard plums, and having a good time. And while we were sitting around doing this, we were talking science. And I remember over some very bad wine that we made, Starly and I thinking, you know this transient is really more interesting than we had realized. Because it isn't just the rate of global average temperature change that matters, but that the middle of continents are going to warm up much more rapidly than will the middle of the tropical oceans, because the upper mixed layer, a hundred meters thick, is going to respond on the timeframe of years, whereas the middle of continents would respond on the timeframe of weeks to months. But what about the high latitude oceans, where the cold and salty water sinks to the bottom, and it's hundreds of meters deep or greater? Its thermal relaxation time could be on the order of decades to centuries. So what that means is the middle of continents warms up rapidly; the middle of oceans, an order of magnitude slower; and the high latitude oceans an order of magnitude slower than that. And therefore during the transition toward equilibrium, what would happen is the temperature difference from land to sea and equator to pole would be skewed, relative to that in equilibrium. If you skew the temperature difference, you skew where the storms are. You skew the nature of the circulation systems.

And we looked at each other and we said, that's the problem. The CO_2 problem will not be handled by equilibrium calculations anymore, everything else must be a transient. And this evolved with students, with the kinds of discussions that took place informally over making wine, and in the sort of atmosphere where everybody was free to guess what each other's computer model was going to say the next day. And there was no such thing as a wrong answer, we all laughed together when we were wrong, and we all strutted a little bit when we were right.

At the same time that this was going on, the international community was beginning to think about climate as more than just a research problem. And there was at that time Bob White again was the co-chair with Federoff from the Soviet Union of the First World Climate Conference, to take place in Geneva in February of 1979. I remember well that there were going to be a number of formal talks, one given by Sir John Mason, who was then heading the British Met. Office. And he had just published a paper in which he had said that the atmosphere is resilient and is "Want to Make Fools" of those who don't understand the resilience, somehow implying that there would be no climate change from human activities, completely inconsistent with the work of people like John Mitchell and his very laboratory, let alone Tom Wigley or us. And I knew there would be confrontation at this meeting. There was an energy and climate presentation by Wolf Hafla and Jill Yeager. There was agriculture and climate, and there was even Ralph Dodge, the man who was an economist from Wyoming, who remember we mentioned in connection with CIAP, who was going to present a discussion of economics and climate. So this meeting was well structured.

Now I remember going, and Federoff getting up and in his speech delivering an ideological blast, claiming that there's a world food crisis, and that's due to climate fluctuations that have induced changes, but the underlying cause is not climate, it was "the manipulation of monopolistic capital" he said. I remember it well. I was sitting next to Mickey Glantz, and I remember saying something to the effect, I said, that son of a bitch! The most monopolistic capital block ever manipulated was the Soviet purchases of grain in 1972 and 1974, driving up the prices and starving people in India when they couldn't afford to purchase on the markets. And I start to raise my hand to skewer this guy, and Glantz grabs my hand and puts it down. He says, no wait, wait, this is the Co-Chair—you got to give him a chance to bluster, he is a Soviet. This is Mickey, who had skewered anybody he could ever see in sight, calming me down. So I said, okay I'll wait. That night Bob White had a reception, and Mickey and I are there. And we are at Bob's apartment, and I am fulminating about Federoff's crap and Mickey says, yeah I shut him up. And Bob said, thank you Mickey, I couldn't believe that you would shut him up. And he said, Federoff has to say that, that was the price of his going. If he says it again, Bob looked at me, skewer him, but he probably won't—he never did. We didn't have to have the confrontation, and I learned a good lesson. I hope Mickey learned that lesson too. He was the one who stopped me. I have had to, on occasion, constrain him. So it was very entertaining that I needed the constraint at the time, and it was appropriately done.

I remember a couple of other serious issues at that World Climate Conference. Mason got up and strutted across the room and gave his want to be—make the atmospheres want to be resilient and is going to make fools of people who don't understand it. This time I could not be constrained by Mickey. And I raised my hand, and I was really angry, because I had privately talked with him about the fact that we had lots of climate sensitivity experiments. And there was no way you could argue that there was no sensitivity to the climate, to several watts per square meter of forcing. Ramanathan had shown clearly how that was distributed between surface evaporation, downward re-radiation and so forth. We understood it pretty well. And Mason is out there spouting, and doesn't know what he's talking about. So I raised my hand and I said: Professor Mason, I'm sorry I didn't know the atmosphere is resilient to climate changes—to forcing, because if I had, even though this is February, I should have brought my bathing suit instead of my skis. And he said: Well Steve, you would be a bad climatologist. I said, no maybe the other point, John is it's not resilient, because it's a hundred watts of energy per square meter different between winter and summer, and the fact that winter is cold and summer is warm is exactly proof that the system isn't resilient—that the system responds, and what we have to do is figure out quantitatively what's going on.

Mason also said at the meeting that the SST's we used to think were going to be an ozone depleter, but now we know from new and improved chemistry they slightly improve, they slightly increase ozone. He said, wouldn't that be an expensive way to maintain the ozone layer—ha ha ha! And I got up and I said: But John, you correctly mentioned that the complexities of nonlinear dynamics and chemistry made the calculations of SST's go from an ozone reduction to a slight increase, but you forgot to mention that those very same complexities made the original calculations of fluorocarbon decreases, which were small, very large. Why don't you be symmetrical and say that when we fool around with the system without understanding that some things are going to get better, and other things are going to get worse.

So that was where already the battle lines that later on became the contrarian debate with IPCC and National Academy studies was already getting set up. And I can remember going to a cocktail party that night, and a British scientist who was a little bit inebriated, a very senior person, was so offended by my rather snide way of dealing with his hero John Mason, that he started shouting and cursing at me, you know four letter words: You think you're so smart, and we all know that this is not the way that any civilized person behaves. And as—this is as Chuck Leith and Barney Boville and I were about to step into the elevator. And I remember very proudly Barney stepping out, grabbing me and pulling me out of the elevator, and looking at Gasgill who had said this. And he said: Sir, you are loud, drunk, and have insulted my friend, and I won't ride with you. It was really just . . .

END OF TAPE 5, SIDE 2

TAPE 6, SIDE 1

Chervin: . . . part of tape number 6. It's now 12:01 a.m. on the 12th of January, 2002. We are still in Stephen Schneider's living room in Stanford, California and we are continuing on the First World Climate Conference, which took place in 1979.

Schneider: In Geneva.

Chervin: Okay.

Schneider: I was setting up that the beginning of confrontation, rather than friendly discussion over climate was starting. Ten years later it was honed to a high art, but we will leave that contrarian debate discussion for later on. Two more stories from the World Climate Conference to really drive home how passions have become inflamed by this subject. First, there was the question raised by Bob White whether this should be a ministers' meeting to discuss policy to deal with climate. There was Jill Yeager, me, Mickey, Walt Roberts, and others who felt that was a good idea, if the meeting were carefully structured, and the agenda was relatively benign and agreed to in advance. There was John Mason and a number of others who absolutely insisted with passion and anger that a ministers' meeting is vastly premature. How dare we think about policy to a problem that has not yet been scientifically solved! It reminded me of the debates at CIAP with Dick Lindzen and Mike McCracken almost a decade earlier.

The second exchange I remember is when Ralph D'Arge gave his talk on economics, and again Mason, but others joining him excoriated D'Arge in the nastiest tones for daring to calculate the costs of climate change when "we haven't got a clue what the climate change is going to be." So what Mason missed was that you need to begin the multi-disciplinary process of asking the question—"So what if the climate changes?"—to begin to set up the question about whether or not we want to take the risks, long before we have the final "definitive answer." And as we now know, what's the probability of a definitive is the fundamental question. It's a stupid word that has no meaning in the context of climate, because nothing is ever definitive. It's all differing degrees. So the questions were not well framed at the time. The debates were not very competent, but they certainly were passionate, and they were presaging exactly what was going to happen in the late 1980's, after the heat waves turned climate into a world political problem. But the first World Climate Conference was where it began.

Shortly after this evolution toward Earth Systems Science thinking, and the beginnings of the evolution of global change, Francis Bretherton for health reasons decided that he was going to step down from his dual job of President of UCAR and Director of NCAR. After the early confrontations and arguments, some might have thought that a number of us would be glad to not have to have Francis to fight with. I was exceedingly disappointed, because Francis had grown not just in stature, but in breadth, knowledge, and in support to those of us who had a broader vision than just solving straightforward problems. And I was of course concerned about who was going to fill this vacuum, and where are we going to go? And what was going to happen to the movement toward integrative and interdisciplinary studies, since they still were relatively controversial, and there was still a widespread belief that interdisciplinary quality was an oxymoron. Climatic change had turned the corner, and was beginning regular publications, and was developing the reputation of being able to publish quality papers in interdisciplinary science, by enforcing its standards of multi-disciplinary accuracy and interdisciplinary originality. But it was still early in the game, and it had not been widely accepted as a leading intellectual venue quite yet in the late 1970's and very early 1980's.

In the vacuum between the designation of a new president of UCAR designate, and Francis' announcement of retirement, it was suggested to me by several in leadership capacity at NCAR that, rather than my applying to be promoted to Senior Scientist, the very thing Francis had warned me I would have trouble achieving if I continued my integrative studies and having students do my work, rather than single authorship papers—his warning was very prophetic. Because I was told, you're really a university professor, you are not a bench scientist, and this isn't the place you should be, so why don't you move on. And this is just when I was beginning to believe that NCAR and places like NCAR needed to serve that integrating function that was so difficult to do in the universities, because of blocks in promotion and disciplinary tenure obstacles, that there was no way I was going to move on. This was precisely the place that should be doing this kind of thing. And in fact, when members of Congress came to NCAR, or there were NSF presentations, I was routinely wheeled out, placed in front of them, and used as exhibit A of how relevant NCAR was to addressing real world problems. So I quite bluntly told these powers that be: you know, you can't turn around and use me as an advertisement for a false premise, because if you do I am going to tell the people in power precisely what the game is. This led to very unpleasant interactions—ugly would be a mild description.

Then all of a sudden the word came through that the president designate for UCAR was none other than Robert M. White, and immediately all hostility ceased. My nomination for Senior Scientist was put forward, was successfully completed. Bill Hess was brought in as Bob's director, and what they decided to do with me was to acknowledge that my strength was in dealing with students and in ideas, and I was asked to head the very Advanced Study Program that brought me into NCAR in the first place. And they said, would you head the visitors program, the post-doc and the graduate research program? And it was a wonderful opportunity that I accepted with relish.

I forgot to mention one thing in the 1970's, and when we type this up we have to shove this backward. One of the most entertaining guys I ever worked with was not even an atmospheric scientist, but a biophysicist named Steve Warren. He walked up to me on the street in 1976, when I had just written my book *The Genesis Strategy*. And he had read the book, and he said: I want to work in climate, how do I do that, I've been doing biophysics? And I said: you apply to the advanced study program, just like I did, and just like Jim Coakley did, and just like Jerry North did, because it always takes one or two people from other fields every year to be converted to atmospheric science. And indeed he applied, and he got in. And when he came there, what he and I worked on was a refinement of the Bodiqo Sellers models; and then a paper which showed that Richard Lindzen and his student Brian Farrell, who had tried to show that North's model was no good because it was a diffusive based model and did not have a proper Hadley cell. So they proposed the method for dealing with the Hadley cell.

And what Steve and I did is we said, I don't care if your method looks theoretically interesting, how well does it perform in the real world? So we used the seasonal cycle as a test, and what we did is we used the North model, that is a diffusive model (really it's Bodiqo Sellers), and we showed that when you use seasonal cycles, it does a very bad job of predicting fluxes in the subtropical zones as a function of latitude and season, but does very well in the mid-latitudes. Then we used the Lindzen and Farrell model and showed it was even worse. So our conclusion was: You're absolutely right, Dick, to criticize diffusion because it doesn't work in the tropics, and what you substituted is even worse than what was there before. And that further continued my happy relationships with Dick Lindzen.

Steve then got interested in radiative transfer, worked with Warren Wiscombe, who we had hired in the climate sensitivity group to develop radiative transfer models. And Warren and Steve evolved some Delta Edington models, which then later on became standard use in aerosols. And they also applied them to figure out the albedo of snow with impurities—something that then turned Steve into a snow expert, for which he later on became a professor at the University of Washington, where he is now a world leading guy on cryosphere and climate.

One other item about the 1970's that was partly responsible for my unpleasant interactions with some NCAR higher-ups about my style being inappropriate to what they believe research scientists should be doing. And that is when I wrote *The Genesis Strategy* in 1976, in which I still proudly remember on page 11, I predicted that greenhouse gases and perhaps aerosols would cause a—and the very word I used was "demonstrable" IPCC, about twenty years later said "discernable"—it would cause a "demonstrable" change in climate by the end of the century. And that book had substantial influence on a number of young scientists, and later on several of them applied to ASP. And I did not know until after they were there that it was because of that book. And not just that book, but they found out about that book because Paul Erlich and Carl Sagan had both independently mentioned to Johnny Carson that you shouldn't just have us as your two scientists, you should get this guy Schneider. So I did in the summer of 1977 four Carson shows. The very first one he was not there and Steve Allen was there. And it was the thrill of a lifetime to sit there with a guy who I respected as clever and funny and discuss in popularized terms the nature of why the public should care about atmospheric science.

Then I did three programs with Johnny that really worked out very well. One thing that people didn't know about how Carson worked is you work out with his producer in advance the questions. You also give the producer the answers. They have the questions and they have the answers. And what I observed the first time I went on with Johnny, he would ask a question, I might get half-way through the answer. And he would sort of hesitatingly stop and say, "but wouldn't it be . . .?"—and he got the answer, and I would say, "Yes!" and the audience would be amazed at his perspicacity and intuition. He actually was a smart guy, and he really did catch on quickly, but it was the ultimate in showbiz. Even though we didn't stage or modify the truth, the relative order in which things came out, remember it was entertainment. I had to join the union, oh, not Screen Actor's Guild, it was . . .

Chervin: AFTRA?

Schneider: AFTRA, the American Federation of Television and Radio Artists. I was paid an honorarium of the AFTRA minimum of three hundred bucks for this appearance, and that didn't even cover my AFTRA dues, to show up on this program! And I was reminded this was entertainment, and therefore unlike the news program we could stage it and do it. So this was—it was a very good romance. Johnny and I got on very well. It was good programs, they liked it. And then my last program was in September of 1977, because I made the ultimate mistake, unwittingly. We were going over the usual array, and I had slightly different questions from before. And he started going *back* to earlier questions we had done before, and I didn't like the idea of repeating stuff, because I had already been getting some comments from some of my atmospheric science colleagues, about: Hey, you are saying the same old stuff, can't you say something new from program to program? So I changed the format, and I asked the audience, I said: *Let's take a vote, ladies and gentlemen, let's see if you know—how many of you think the world is cooling?* Most people thought the world was cooling. *And how many people think it's warming?* And I said no, the world actually is warming, it's just that we had a short-term cooling trend.

We went over all this, and I did it sort of like a "Directors Now" with the audience. It worked out great, Johnny participated, I thought everything was fine. At the end of the program, you know, we shook hands and all that, and he walked away. You know, in the past he didn't walk away, we had a conversation. I was wondering what was going on. The producer comes over to me and he said, Steve that was a brilliant show but it will probably be your last. And I said why? He said, you deviated from the script! You took over his program—that's not for guests to do! Never got called back. However, as I later discovered when I had ASP post-docs, and they told me that they got interested in climate because they saw me on Johnny Carson and read *The Genesis Strategy*, I decided that that interlude was worth every minute.

This is out of order, because I forgot to do it when you asked me the question, Robert, about the things we were doing in climate sensitivity, and in the climate project. One of the things that was clear, even as early as 1973 and 1974, was that the real atmosphere is noisy, there's a lot of weather variability. Therefore, how do you know whether a trend, even a ten-year trend, is noise or the signal of being forced by something like anomalies and ocean surface temperatures, volcanic eruptions or greenhouse gases, or aerosols for that matter, or the sun? In order to do that you had to distinguish the signal, which is the trend you want, from the noise, which is the background variability.

So I remember Larry Gates talking about this problem. And I remember that you and I were thinking about how can we formally do this in GCM's? And I remember saying: you know Larry had an idea, and I got it from him, so it's only right that we invite Larry to join us. So we did and the very first paper, the Chervin, Gates & Schneider in 1974, I think that was the actually—the first paper that used general circulation models to estimate signal noise ratios. No, we hadn't gotten signal noise ratios, we were talking about time averaging as a way of reducing noise. So we used NCAR GSM's, and we used the Oregon State GCM. And published a joint paper that set up the idea that you could use time averaging to reduce noise. But that clearly wasn't good enough, because Jerry Namias at Scripps had long been arguing that anomalies in the North Pacific Ocean surface temperatures were causing tele-connections way downstream, and would have some possibility of predictability for season ahead weather anomalies. Not the GARP 1 objective, but part of the GARP 2 objective to get at least medium range—short range climate forecast, long range weather forecast.

And in order to do that, what we had to do was look at a very noisy flippy-floppy jet stream, and find out if we could see a statistically significant signal. You may remember better than I. So what we did is we evolved techniques to generate just pure noise in the model. That meant grabbing runs in which errors had been made. They were perfectly good noise sources—or to run the model by just flipping initial bits in the initial conditions and temperature, and allowing the Ed Laurenz predictability period to be exceeded in a month in which the system forgets its memory of its initial state. And then from there, we would generate noise statistics from which we could come up with a standard deviation of the background. Then we had something which we could compare, the observed delta or difference between a control and a disturbed experiment, and then compare that delta to the sigma, that is the variants in the model. And then we used T tests, as I remember, to try to determine statistical significance.

It was a very simple method. It was a point method. It assumed that there was no correlation in space from one point to the next, which was clearly a simplification others criticized later. But that method is still fundamentally what's used today, with spatial modifications. You can fill this in better than I. But just publishing a statistical method isn't very interesting, and therefore evolving some science, which is climate sensitivity, was needed. So there were two papers I think that we did. First we published the statistical methods by themselves, right in the *Journal of Atmospheric Sciences*. And then we published applications. And the applications were the Jerry Namias warm spot anomalies. And I remember when we had a plus and minus two degree anomaly in the North Pacific, it caused a statistically significant signal right over the anomaly, but that was it. The tele-connection downstream was absolutely buried in the noise. Now if we had run a hundred year ensemble maybe we would have seen it. But we didn't have enough noise data, and we didn't have enough anomaly experiment data to be able to do that.

So we invented a new trick, which others have since copied, which was the super anomaly. We took the dipolar pattern of temperature, and instead of making it the 2° or 3° that it was supposed to be, we multiplied it times three—that worked. That created a really clear statistically significant signal, but even that super anomaly was a very very weak signal downstream. Do you remember whether there was anything at all much past the West Coast that was statistically significant?

Chervin: I don't think we got any significantly significant response over the continental [area] of the United States, perhaps in Canada but not in the U.S.

Schneider: Well I remember we sent this to Jerry Namias, and he wrote back the angriest most hurt letter. How could I do this to him?! I said: Wait a minute—Chervin's the first author of this, why are you blaming me?! He only thought you did the calculation. I got blamed for trying to show that his work didn't work. There must be something wrong with your model, he said, that it's not picking up these anomalies that we see in nature. And we wrote back and we said, Jerry we're just trying to demonstrate a method, and in this particular model it doesn't show up. Then I remember Bob, you worked with Paul Julian and did a similar experiment where you put anomalies in what are now Nino 2, the region that people were just beginning to think about, El Niños back then. And it created a whopping statistical significant signal even outside of the region. And basically, using those simple T test methods to demonstrate statistical significance, and using the second generation model—we hadn't even gone to third generation models let alone spectral models.

Chervin: And this was still run in perpetual January mode.

Schneider: Perpetual January mode without . . .

Chervin: _________.

Schneider: . . .with frozen oceans. Without real—these were not true climate models, because they were not energy conserving. And what we were able to show even back then was that you would expect much more highly significant signals from the tropics than from mid-latitudes. And that was exactly what climate sensitivity was supposed to be about: Where was it sensitive and where was it not? So and we were never claiming that these were the truth. It was just that we felt that what was going to be robust from those conclusions was that it was going to—you were going to fight the noise anytime you were dealing with anything in the mid-latitudes, but when you are in the tropics you could get stronger signals, and I think that that was prescient.

Chervin: And as I recall, with those tropical anomaly experiments we did not have to resort to the super anomaly approach . . .

Schneider: Right.

Chervin: . . . to get a significant response.

Schneider: Right. Then the second paper you and I did was to take Warren's model, and Warren by the way was I think the middle co-author of each of these, because it was his model, you know, and he did the runs for us, and he was a good collaborator and discussant. And take the runs that he did for me in 1971, which I had still not yet published, on cloud feedback, where I had put the plus and minus two degree warmings over all the ocean grid points, and where I also put them in the tropics and in the subtropics. And this time we did statistical significance experiments, and we showed that those cloud feedback signals were highly significant. Using that method it was very easy to demonstrate that these tele-connections that were generated by those particular experiments—they may not be true, but they were highly significant in the model. And again that was the kind of work that came out of that heady atmosphere that took place at NCAR in the 1970's, with all those visitors from universities and other scientific laboratories around the world, where we would get together and kick around what needed to be done, and divide up the work among ourselves.

One more 1977 flashback. Because of the Carson appearances and *The Genesis Strategy*, I got a phone call from CBS. There was a very famous CBS program still on called "60 Minutes" and Dan Rather was involved in it, and he had a new idea. He was going to do a 60 Minute-like program, but it was going to be biographical, and it was called "Who's Who." And they had done a couple of programs, and they wanted to do the fourth program. And the fourth program was going to include Lilly Tomlin, Rosalynn Carter and *me*!

So out came Dan Rather and producers, and this was in 1977, the height of the drought. And I had said in *The Genesis Strategy* that we'd had a long lucky streak with the weather, and we had to expect nasty weather one day. I did not predict that we would have a drought that year. I simply said we were due, because we hadn't had one for a while—and guess what, it happened to have occurred in that year. So I was given credit as a prognosticator [that] I did not deserve, and I didn't claim the credit but I got it anyway. So out he came, and we are having a conversation, and he said, haven't we got anything more visual than you talking here with the mountains in the background? I said, Dan, there's a lake up in the mountains, which happens to be Barker Dam up in Nederland, and I said that lake is so low that you can see how the drought has really drained it down. And he said yeah, okay what do you think we ought to do there? I said why don't we go fishing.

So I gave him—I had two fishing rods mine and mine, and I gave him one. And we headed on out to the place. Well the camera crew set up about a hundred yards away, and we were wired with microphones, and they actually had radios then, so we had it, y'know, in the back pocket. And I still have a photo of the two of us sitting there fishing that one of them took, y'know, somewhere, and both of us looking very much younger than now. And I said, it's a bright day, so we should be using a copper cast master.

And he said, "I've been fishing, son, since I was a young guy—I know what kind of lure to use, show me those lures."

So he put a silver lure and I put the copper one on. So we are fishing and fishing, and then in about five minutes later, guess who caught the fish? I caught the fish. I'm reeling this fish in, he says, "oh you've got one," and he was pissed because he wanted to catch the fish. Anyhow it was all in good fun. And I am reeling the fish in, and I get the fish. And it's squirming around, it's on the end of the hook. And then, y'know, we cut.

And he yells out to the guys, "Did you get that?!"

And they said, "No we were changing the film!"

So this was horrible, I mean how could I not catch a fish on national TV! So I cast the fish back in the water. This is staged, this is news, we are not supposed to stage—it's okay on the Carson show. I reel in and I reel out this fish which has no fight left in it, so this dead fish coming up on the line, which they put on the air. Now the irony is that I was never much of a fisherman. I used to crab as a kid, but I was never fishing. And I had just learned to fish literally the summer before in Rocky Mountain Biological Lab, with a fanatic fisherman John Haldren had taught me how to fish in lakes, and I was beginning to learn to fly fish. So John is watching this program, and he says, "What did you do put a dead fish on the end of the line?!"

Chervin: Well, would you categorize that as entertainment or news or what category?

Schneider: It was—I viewed it as entertainment; they viewed it as news. But the one thing I will say is it was the very last "Who's Who" that was ever on the air, so Rosalynn Carter, Lilly Tomlin and I buried the program. Apparently the higher-ups at CBS either didn't like it or it wasn't getting ratings, and I wasn't on the cutting room floor like I was on "Meet the Press" once, but this one buried the program.

Chervin: Is there a videotape of that anywhere around?

Schneider: I have videotapes of all of this stuff on 3/4 inch videos which I haven't seen in twenty years, and I have no idea if they still are archivable quality. One day I ought to probably transfer them.

Chervin: Well, note to NCAR archival office: We might consider trying to copy these to a different media.

Schneider: If you have a three—you need a 3/4 inch video VCR.

Chervin: Right.

Schneider: One more aside. I remember what else Haldren said. He says, you son of a bitch, I teach you to fish last summer and you go catch a dead fish on national TV!

Chervin: Okay we are still on the A side of tape number 6, its now Saturday the 12th of January, about three o'clock in the afternoon. And can you recall any other mentors on the international scientific scene over the years?

Schneider: Yeah before we proceed into the decade of the 1980's and the Advanced Study Program years, I forgot to discuss a person who really was very important in my life, another very powerful character, and that's Margaret Mead the anthropologist. I got to know Margaret through the mentor I've mentioned many many times already, Will Kellogg. After Will was no longer the head of the Laboratory of Atmospheric Sciences, and was just a scientist in the climate project and then later in the climate section, he still retained his phenomenal sense of important projects to get involved in. There was SMIC, there was SCEP. This time he got involved with Margaret Mead. Now Margaret was the president of the AAAS at the time and very much the eclectic scientist, interested in broad ranges of issues, interdisciplinary . . .

END OF TAPE 6, SIDE 1

TAPE 6, SIDE 2

Schneider: So now we are on side 2 of tape 6 on the 12th and I was talking about Margaret Mead. And I said Will Kellogg, being the good mentor he was, brought me into a project that he had just evolved with Margaret Mead, who had a grant from the National Institutes of Environmental Health Sciences in Research Triangle Park in North Carolina, to run what was called the Foggerty Conference. Now most of those are about health issues. And she being the eclectic character that she is, decided that the health that she wanted to talk about was the planetary health. And the director of the—_______, I can't remember what his first name was, Randall was his name, was broad enough, plus Margaret Mead was a pretty hard character to argue with, accepted it. So Will ran a meeting with Margaret, Will was sort of the scientist on the content, and Margaret was the visionary.

And her view was—it was called "The Atmosphere: Endangered and Endangering"—and her view was that you should bring together a very very diverse set of what she liked to call "analogical thinkers." She said she hated digital thinkers, people who only operated on the basis of methods and couldn't think by analogy. And so the people that she brought together ranged from Robby Robinson, who I had mentioned before as kind of the conservative curmudgeon at SMIC; Wally Broker who is a curmudgeon who can't be labeled in any other way; and a strange guy named James Lovelock who had a new idea called "the Gaia hypothesis" that he had evolved with Lynn Margulis, that the earth was a self-controlling automatic cybernetic—that is, negative feedback system—that optimized the environment for its own good. And he in fact had been attacking people who were worried about fluorocarbon effects on ozone, arguing that the system was self-healing. And he was extrapolating that to climate.

On the other side, arguing that humans were multiplying out of control and using technology and organization in a dangerous way, were the two young turks, John Haldren and Steve Schneider. So the two of us were there, and we were going to argue that side. The rapporteur was Dana Thompson who was an NCAR post-doc, had just been in oceanography, and also had worked with Mickey Glantz, and as a result of that broadening experience was interested in social issues, and Barbara West who had worked in Harrison Brown's organization after Harrison moved to Hawaii. Harrison was the first person to be a futurist stepped out of science. He wrote a book called *The Next Hundred Years* in 1956 and *The Next Eighty Years* in 1976.

All right, so this was quite a group of characters, and then there was Margaret. And she got up there and she said, basically if we can't solve the problem of the planetary commons when it's as obvious as pollution, [then] we can't solve any problem. And of course there were people in there who denied that we needed to solve a problem. The battle lines were drawn, and it was really quite a meeting. When somebody said, “We don’t have enough data to make any definitive statements,” you could count on Haldren or me jumping in and saying, “Well how can we take a chance when we can’t rule out catastrophic outcomes?” [Those were] almost the same kind of arguments that we still hear today about whether you are risk–averse, and worry more about the potential legacy of pollution and biotic impoverishment, or whether you are worried about wasting resources on a problem that may not be so serious that you would rather spend on some other social need. So all of those issues were drawn there.

Lovelock was among the most interesting because he said, “Well let’s not worry so much about what we are going to do to the planet. After all, when the microbiota in the oceans produced oxygen it was a poison that eventually leaked into the air and relegated them to the anaerobic niches of the planet. And therefore life is very resilient.”

And Haldren and I jumped down his throat and said, “Jim we’re not talking about the survival of life, we are talking about the survival of half the species, and the well-being of humans. We are not interested in the abstraction about whether DNA replication continues.”

And [it was] quite an interesting set of confrontations. Margaret Mead I later invited in 1977 to join CS Kyong who used to be at NCAR in chemistry and then had gone to Georgia Tech; Lee Shipper and Jerry Weingart from Lawrence Berkeley Labs and the Energy and Resources Group; and me—on a panel on how do you do interdisciplinary things. This is right about the time that I was getting going in climatic change. I invited Margaret Mead to be on the editorial board, as I had mentioned earlier. And we examined the stories of each of our interdisciplinary groups, and we invited Margaret to be the commentator.

She gets up at the end of it, and she sort of looks over at these four young thirty-year-oldish guys and she says, “You young physicist guys are all alike, you just only know what you know, and you don’t know the old story.

And we said, okay Margaret, what’s the old story?

She said “It’s an old story, she said, how do you do anything creative in a hidebound elitist institution like a university?” She said, “It takes three things to create an interdisciplinary group or to create a creative new idea. First, a charismatic leader—and that charismatic leader has to have enough credentials in the disciplinary world to fight off the narrow minds, and to do number two. And number two is,” she said, “attract a staff of young brilliant analogical thinkers. The digital thinkers will kill you. The methodologists will drive you nuts. You want people who at the margins of their intellect want to solve problems and want to make connections. And the third thing you need is a sugar daddy. You need somebody to come up with the bucks, because you are not going to get them out of the establishment.”

That was a wonderful model. I sat there and I looked at Margaret and I said, "Margaret that's pretty tough."

And she smiled and said, "Okay, you've got to have all three over a ten year period, but in any one- or two-year period you need two out of three to go on."

And I take a look at the creative institutions that I've seen, and most of the time, when the charismatic [leader] leaves they fail. And her model is still good to this day. And what one needs to do to institutionalize creativity is to take it beyond the charismatic. And if we look back to the early days of NCAR before there was climate research, I think her model worked. There were charismatics. There was Phil Thompson, and Bob Dickinson, and Warren Washington, and Will Kellogg, and even me. And we were able to broaden it to where that became institutionalized. The sugar daddy was switched to the NSF when the trustees and the community at large went along with it, and we had a mix of analogical and digital thinkers. Because to make climate work, you can't just have people who think at the margins of their intellect about connections. You also have to have people who can build the models, run the data and so forth. So maybe she was a little bit too harsh on her point two, although getting things started that's not true.

I would argue that later on, when in the ASP era Starly Thompson and I tried to create broad interdisciplinary studies at NCAR, and we created the Global Systems Group, that we started out non-digitally. But even that's not true, because Starly was one hell of a good computer programmer and an equation solver, and then we brought in Dave Pollard and other students. And without having that mix of people who both have first-rate methods and at the same time ask fundamental intellectual questions, I think you end up—you don't have greatness, you have mediocrity.

That brings us to 1981 to the era of Bob White and to, in my own personal sense, having been asked by them to run the Visitor Program and the Advanced Study Program. [Dog barks.] When I went to run ASP, I was very excited. I took a look at what was there. I knew that program had brought in the likes of Jerry North, and Zvi Galchen, and Jim Coakley, and Jeff Keel, and Phil Rash, and even me, and I think Peggy Lamone too.

Chervin: Right, and Joe Clamp.

Schneider: Joe Clamp.

Chervin: __________.

Schneider: And a lot of people who were, y'know, bright young folks, some trained in meteorology, but others from other fields that were enriching it—Steve Warren—and that was terrific. But I also remembered my own experience in the early 1970's. It was a one year post-doc. And most post-docs hit the ground with papers to write from their previous work in the first six months, and a job to find in the next six months. And I immediately set out to find a way to expand the program so that people could be renewed for a second year. But here was the problem, there was a fixed pot of money. So how are you going to be able to do that without cutting the number of post-docs in half, which was an unacceptable solution. So what I came up with was the idea that instead of having the post-docs groveling to find work elsewhere, they should start out as intellectual free agents, working on whatever they want. And their selection would be based in the first year not upon how well they fit into the programmatic interests of NCAR projects and divisions, but on some "absolute standard of quality"—very difficult to evaluate when you are talking about somebody from ISIG on the one hand, a turbulence person on the other, and somebody who does measurements of atmospheric chemistry yet on the other hand; or a climate model on a different hand.

So that was my job, was to try to weigh those incommensurates. And I asked NCAR scientists as individuals to rank these people on absolute quality grounds in the particular fields that they were in. Then I would ask sections or divisions to rank them in the programmatic order that was of interest to those sections and divisions. But I gave that less weight in my decision making than the three or four or five individual letters I was getting from the NCAR scientists or from the scientists who wrote letters for them. So I tried to make absolute quality the prime criterion, but not the only criterion. And the reason for that was, I wanted people to be able to stay for a second year, and the deal was this, and the division directors all agreed to it. They would pay out of divisional funds for the second year of the post-doc. That put intense pressure on the post-docs to make sure that they interacted with key and important scientists in those divisions, and made themselves indispensable enough that the division felt that picking up their relatively low salary, relative to a regular staff member, but still twenty-, twenty-five thousand dollars (I don't remember what it was in those days, probably less than that then.) but something that was half of what a support scientist, a regular scientist got, but still not a trivial amount of money—that they would be willing to kick that money in.

And the other thing I felt was that if they were going to survive in the real world, they can't just be entirely curiosity driven intellectual free agents a hundred percent of the time. They better figure out how to sing for their supper at the same time. So I thought that was sending the right message. And that's exactly what we got the director's committee to agree to. And Bill Hess was a very strong supporter of ASP, and he told me if we have any real crises, come to him when the crisis occurs and he'll look at the director's contingency fund. I think I was in his office about five times a year. He would say, "Oh no I am going to be bled again!" every time I walked in the door. But he liked post-docs, and it was a very nice prejudice.

Well Bob White brought in somebody who was a manager but had the philosophy that bright young people were an important and an essential ingredient for the health of the community. So that was the first change that I made, and I am really very proud of that in retrospect, because I think about eighty percent of people who stayed did it through that route. They were not hired immediately as scientists in those programs, but they transitioned relatively gently into a compromise between absolute quality in some sense, and programmatic interests of the projects.

The second thing that I did is I felt that the ASP program needed to advance some social and intellectual agendas that I felt were important. I took the job, and I did not hide this from Bob White or Bill Hess when I took the job, that I had two agendas that I was going to push and use ASP to do that. One was that I was going to broaden the program and bring in more ecologists, more social scientists, geographers and so forth, to move us in the direction of earth systems science, and do it in a way that only had a one year commitment of NCAR funds. And after all if these people were going to stay, they were going to have to convince somebody to pay them for the second year. And the second agenda I had is I wanted more women in the business. Both of those were happily agreed to by Bill Hess. Now how can you do that without cutting down, that is paying what economists call an opportunity cost, cutting down the number of traditional slots that went to the sections and the divisions?

The only way to do that was to expand the program. When I started the program it was ten to twelve people. Every year I appointed no less than eighteen and sometimes as many as twenty post-docs. So where did this money come from? Well I was bleeding Bill Hess dry. I also would go to Warren. I would go to people in divisions and I'll say: "You know, you ranked this person as really really programmatically important, but they were number two on the list from intellectual absolute quality. Don't you want him anyway?" And then they would come up with half the money, and then I would go to Hess for the other half [of] the money. Or Starly Thompson and I by that time had a bunch of grants. I would kick in some of my own grant funds. We were able to, by continuous visits that I would make to people stretched all around the building, especially Hess, to come up with the way to fund about twenty post-docs. Now not all twenty of those could stay on, but more than half stayed on for the second year because they got funded.

Now this wasn't frictionless. There were times when a division director or a program manager would come into my office shouting and screaming at me because in the past they always got the post-doc they wanted—and we ranked them, and you didn't give us the guy we ranked, and what's the matter with you?! And I would have to say— excuse me, he was number one ranked in your programmatic interest but was third or fourth in an absolute sense, that is not good enough for getting into round one. And then next year they would not be angry at me, because they got two post-docs because they were really highly ranked. So over time I think the statistics averaged out and most of the complaints disappeared.

The other thing I did that generated complaints is I did not use the limited funds that we had in the Visitor Program for this summer colloquium. A lot of people in the UCAR institutions liked the summer colloquium. And I felt it was a little bit of a vacation for people who wanted to come and spend a few weeks at NCAR on some topic. I didn't think it was a waste of time, but I felt that to spend a hundred thousand dollars, probably that was five post-docs-worth, on one summer colloquium was a horrible trade-off, relative to bringing in five bright young people for whom this experience would dramatically influence their career and at the same time enrich, y'know, the institution and the field. And I made the value judgment that I wasn't going to spend the money on that, even though I was getting complaints from outside committees.

The other complaint I got was from a few members of universities. We had an external review, and one of the university department chairs came in all loaded for bear, because the Graduate Research Program, which I really was high on, in which students from university departments would find a machine, like an airplane, a computer program, like a general circulation model, a laboratory instrument, or just an intellectual character like Bob Dickinson, y'know, at who's feet they could sit because there was nobody like that in their school. And they would come in for two years, and they would write their thesis as a member of some section at NCAR. This person would then be given an adjunct status at the university, and be a member of the thesis committee, and the thesis advisor would have to approve it. Well we didn't have an anniversary where we had a hundred applicants for which I had to choose twenty, like the ASP post-docs. But these would come in every—they used to come in once a year, and I changed it to come in every four months or six months, I can't remember which. Because these graduate students sometimes needed to move on a more rapid thing, and it was on sort of an as needed basis.

I had money for six or eight of them, and I was using money that used to be in the colloquium for that. And as usual, I would bleed Bill Hess or the divisions because when there was a student they really wanted, and there was an advisor at NCAR who really wanted them, we found a way to get the money. Between our grants and internally loose funds we were able to do it. Then this university chair comes in and says, our review—I don't like this program. Our best students are applying to this and it's depriving our department of their presence. And I turned around and I said: But Dr. X, isn't it true that their advisors have approved it? Isn't it true that this is improving the education of the student, and providing an opportunity? And he turned around and said: But you are NCAR, it is your job to be supporting the interests of the university. I actually knew about this, because I had a little advanced intelligence that this was going to happen. So I'd had a few minutes to think about the nastiest thing I could say if this happened. And I turned around and I said: "Let me tell you my priority system. Number one is what's in the best interest of the student. Number two, what is the best thing that enriches our field, broadly defined? And number forty-two is what's good for the Meteorology Department at your institution." Yes, that was designed to piss him off and it worked. The good news was [that] a member of the review committee who had just been nominated to the National Academy of Sciences, a senior man jumped up and said: "I agree, this is healthy for the field, and we shouldn't be provincial. We're thrilled when our students come here." End of discussion. We kept the graduate program. Although my understanding was when I left in the late 1980's, shortly thereafter, it was eliminated and they went back to the colloquium.

Chervin: Are you able to recall any particular successes of students who passed through that program and went on to great accomplishments in the field?

Schneider: Well, there were low power people like Susan Solomon, Diana Liverman, Phil Rash, I mean there were just excellent people. Nuset Dolfus worked with me. I guess I was the thesis advisor for about a quarter of them. I remember when Diana came. Now she was a geographer. She fit two of my agenda criteria, a woman and a social geographer. And what she wanted to work on was a world model. This was the intellectual legatee of the Club of Rome models, but they were regionally disaggregated. Time doesn't permit going into exactly what that means, but basically those are population resources and environment models. And she was working with Dick Warick who was then a junior scientist in the other section in the Advanced Study Program.

Remember, the Advanced Study Program was headed by John Firor who had been the Director of NCAR. And as a good manager, I guess they were also looking for a good home for John, and this was kind of natural, and it had two sections. It had the Visitors Program that I headed, and it had the Environmental and Societal Impacts Group that Mickey Glantz headed. And Diana would work with ISIG and with Dick Warick. Unfortunately, in one of the external reviews, there were some negative comments about ISIG, a budget cut, and Dick Warick was a victim. So Diana was left floundering within months of her arriving there. So she came to me, explained it all, and well, had to have an advisor. So I felt, hey it's time for me to learn about world models.

And what she did is she showed me that she had this model that she got from the Denver Research Institute, which had originally come from Milo "Mike" (everybody calls him Mesarovic) at Case Western Reserve. Well I had known Mike about eight years earlier. Because in 1974 he published a famous book. I think it was *Mankind at the Turning Point*, which was the second report to the Club of Rome, where they used a computer model to demonstrate that the original model, which said that somewhere in the middle of the 21st Century there would be a world collapse, was flawed. Because there wouldn't be a world collapse. What they did is they regionally disaggregated into nine regions, and showed that the rich regions would probably get by just fine, but that the famines would be restricted to, you know, to isolated areas where there was insufficient growth—which is by the way looking more like more what's happening now, is that the world becomes increasingly fractionated. While in aggregate the world gets richer and better off, the fraction of poor people decreases, but the total number stays about the same, and in fact they fall further and further behind as the world globalizes. So the model was actually pretty good.

Mesarovic called me up around 1976, and I visited Case Western Reserve, and he said, "Steve, can you help me put environment in the model?"

And I looked and I said, "Well one of the routines in the model has crop yields. One thing we could do is say that climate change might influence crop yields."

And he said, "Oh good give me some formulas."

I said, "Well, we can't do that yet because we're not sure whether it's positive or negative, because CO_2 fertilizes crops and improves it. Some areas with short growing seasons will get better; the hot regions will get worse. It will be a redistribution. And anyway, how do we know that the model is accurately and adequately translating changes in crop yields into changes in food productivity, and changes in trade and famine." All these things were predicted by the model. I said, "So why don't we do something else. Why don't we put in the famines of 1974 and 1976—not by putting in the famine itself, by putting in the droughts in India and Pakistan when the monsoon failed, and then by putting in those crop yields as a forcing. Let's see how well the model does as a validation test, and that way we will know if it makes any sense. Take the next step.

He never called me back. They weren't particularly interested in validation. So Diana comes, and she now has this code neatly in her hands. It was very difficult to get it adapted to the NCAR machine. And another very interesting character who was a GRA, Nuset Dolfus, a physicist from Rice (actually he is from Turkey, but he was working with Joe Chamberlain at Rice University). Nuset was interested in every thesis but his own, so I later discovered that he was becoming the programmer for half the other students. He was solving all their systems problems. So I finally yelled at him one day that if he doesn't solve his own he is not going to have his thesis. But anyhow, he helped Diana get this model adapted to the NCAR computer, and it was running. And she said to me that she was interested in running some cases. And I said, well no, and I told her the story about Mesarovic. And she said, yeah let's do a validation.

So what she did is she changed the crop yields in India and Pakistan and the Soviet Union, which had the big grain trades. And when she put them in, the model didn't predict at all what happened. And then realized that—I said why? And she looked further and she said, “That’s because the model didn’t predict the trade. Because the Soviet Union went into the grain markets.” So now she picked in the trade. It got better, but it still didn’t predict the famines and the trouble that went on that actually happened. Why? Well because there was an OPEC embargo, and the OPEC embargo raised the price of fertilizer, thereby further reducing yields, and giving countries less foreign exchange to go out on the grain market. Then she put in that, all of a sudden now the model started to look a little bit more like reality, but was still under-predicting significantly the tremendous food price rises that occurred in those years.

And finally it occurred to me what might be going on. There was no futures market. What happened in those years was [that] the amount of grain on hand that was stored dropped below about 10% of world use, so the supplies became less than 10% of usage. And when that happened, the speculators would go in, into the futures market, and that was driving the high prices that were killing people. And she didn’t go far, as far as to try to build a futures market into her model. But it was exactly the kind of thing that I believe you are supposed to do in models.

And Diana, you know, became actually quite well known as a young grad student, because what she showed is how we use a model as a sensitivity tool, and how you can actually develop what we now call integrated assessment—end to end models that deal with physical, biological, and social interconnections. And she was doing that oh, ten to fifteen years ahead of most people. It landed her in a position in the Geography Department, initially in Wisconsin, where I think her problem was they had her teaching four classes and she didn’t have enough time for research. And when Eric Baron finally left NCAR in the middle 1980’s to run the Earth Systems Science Center, he immediately grabbed up Diana, where she then picked up the cudgels for the Social Science wing of the Earth Systems Science effort at Penn State. And was there for quite a long time and really helped to build that into a center of excellence, before in the 1990’s she went to run the Latin American Studies program, which is actually one of the few Latin American Studies programs that actually considers environment as part of development, which she now does at the University of Arizona.

In the early 1980’s, Starly Thompson who had been the support scientist who had worked with me on the CO_2 transient, and on defining how the Melankovich mechanism—that’s the way by which orbital wobbles change the amount of sunlight that comes in-between the equator and pole, and winter and summer. We did a theory on that. Starly had by that time had gone to get his Ph.D. at the University of Washington, and was no longer working directly in the group. And only would come back to NCAR periodically for computing use, and so forth. He was busy writing his thesis and kind of told me: Okay, enough of you. Don’t give me any ideas, I’ve got to finish my work.

Another young post-doc came in called Kurt Coby, and he primarily was interested in the atmospheric dynamics of Venus, had worked with Jerry Schubert at UCLA. But he also had put down on his application that he wanted to work not just with Bob Dickinson on this, but that he had some interest in getting into climate modeling. So after Kurt was there a couple of months working with Bob, he came in to see me, maybe he was worried about how he was going to get his second year and thought I might be a good way to do it—you know, that's what I wanted people to do. And he came in and he said, I want to talk about, you know, any climate modeling projects you might have. I'm guessing this was 1982.

And it turned out that I had just gotten the pre-print from Paul Crutzen on the Crutzen and Burkes paper on nuclear winter. And I knew that they had simply asserted that throwing dust in the atmosphere was going to cool the earth. They had not calculated anything. And I thought we needed to apply climate models to do this. And I said, Kurt you're interested in planetary atmospheres, and therefore in very big forcing and very large signals like the difference between Mars and Earth and Venus—here's one for you. So we talked about this for a while, and he said he was actually interested in it, and he would study it. We tried it with a simple model, the model that Galchen and I had used, and it didn't really work, because the only way to make it work was to look at the vertical distribution in the atmosphere of heating.

And we then got a pre-print from Carl Sagan that he was working with a group called TTAPS—Turko, Toon, Ackerman, Pollack and Sagan. Turko was then at R&D Associates, which is a defense black, meaning largely secret classified research institute in Marina del Rey. And Brian Toon and Jim Pollack and Ackerman were all at NASA Ames and Carl was Carl.

END OF TAPE 6, SIDE 2

TAPE 7, SIDE 1

Chervin: . . . [This is side] A of tape 7, Saturday the 12th of January, about 3:30 in the afternoon. And we were just getting into the nuclear winter topic, and you had just said something about the TTAPS effort.

Schneider: Right. That there was—in addition to Crutzen and Burkes saying that smoke would be involved, there was this pre-print—or available from TTAPS, suggesting that in the one-dimensional model it was going to lead to freezing. Crutzen was not arguing about cold as much as he was arguing about dark, killing off photosynthesis. So I knew that the TTAPS effort was a one-dimensional model and I wondered what would happen in a 3-D model, and asked Kurt if he would do that. So Kurt started looking into how to modify the code, how to, y'know, change radiative transfer. And he and I worked on this. We asked Ramanathan to help us. And we were using at that time, I believe, the CCM0A. When Chuck Leith took over the atmospheric analysis and prediction, he didn't want grid point models. He wanted there to be spectral dynamics—that is, the model still had good points for solving the so-called physics, the thermodynamics, the radiative transfer, but that the motioned equations would be solved by spectral methods. There were two models, one was from Australia which was the Bork model. And then the other one was from the European Center for Medium Range Weather Forecasting. And there were two partisan groups that both were working on. And I believe Warren was working with A, and who was with B, I can't remember?

Chervin: That was primarily Dave Williamson.

Schneider: Williamson, yeah. And there was a rather intense and not always friendly competition about which was the better model. Meanwhile, Chervin, right—wasn't it Chervin?—had been using model A and was doing the statistics. He had, y'know, basically pioneered the statistical packages for doing signal noise ratio, and now was analyzing the seasonal cycle and the model performance. So therefore he had the code, he had the data, and we went to Bob and got all that. The question was how do we modify the radiative transfer package. We went to Ramanathan and others and the radiative transfer package was very very opaque. It was very difficult to deal with. Now there was sitting Starly in a room that I had assigned him as a visitor when he was writing up his thesis, and Kurt, an excellently trained planetary dynamicist, wasn't really a radiative transfer person. And I knew that Starly had built radiance convected models, and I knew I had to get Starly in this game. Now how am I going to get Starly in this game when I promised to leave him alone?

So Kurt would be—he and I were trying to figure out how to modify the routine, and we were getting some preliminary results that were interesting but internally contradictory. And I remember there was a hall upstairs on the fifth floor right by the ASP office which had long windows. And right in front of these windows there were heating ducts. And since we had about ten or twenty pictures to look at, and they were in sequence like a movie—all of these were microfilm printed out in those days. I went over in the heating ducts and I laid them all out, all twenty pictures in a row, and Kurt and I started analyzing them. Of course I laid them out in the window right in front of Starly's office. So we are discussing all these things, and we are *loudly* talking about how the model is doing this. It worked! He couldn't stand it. He came out, he said, "Wait a minute, how did you get this result and that result?" Soon we had him in the code, and he put in a simple aerosol routine, and then we had a 3-dimensional model that could actually begin to analyze the nuclear winter question, and become a supplement to that which was done in 1-D by TTAPS.

Now I remember the meeting we had in 1983 in the spring in Cambridge. It was called by Carl Sagan. And Carl brought together two meetings—one for a few days on the physical science; and then one on the impacts, the biological science. And I attended both of them. The physical stuff he was in charge of. The biological stuff, Paul Erlich was put in charge of. And like good assessments that were done before, they recognized that it needed to be multi-disciplinary, and it needed to include the "So what?" question. Carl certainly recognized that. And it actually reminded me of a time when in Denver in 1977, at the same AAAS meeting that Margaret Mead was at, Carl and I had lunch. And I remember saying to Carl that he was one of the best communicators anyone could ever imagine, but that he hadn't at that stage taken on the politically tough issues. And I was saying: Come on Carl—we need you! And boy, he jumped in with all four feet on nuclear winter. The meeting in Cambridge at the American Academy of Arts and Sciences that Carl had arranged was really quite an important event. His, I guess then third wife, Ann Drewyen, was probably the main influence in Carl moving from the wonderful communicator of 'science for its own sake' toward 'science for policy and social relevance,' because Annie had a very strong sense of arms control need. And Carl shared that intellectually, but I think probably the emotional boost was a main driver for him.

In any case, and Annie was there—Carl was recovering from a very serious appendicitis peritonitis that he had, where they had to drive from Cornell to Syracuse Hospital, and you know, it was not even clear he was going to make it. This was a really courageous thing for him to go to this meeting, when he was still in pain when he would bend over from the surgery. That's how passionately he felt about this need. I also think that part of his motivation was enhanced by the Casper Weinberger loose lips about winnable nuclear war, which really had absolutely reinvigorated the anti-nuclear movement, as I had mentioned earlier on. So the first few days we talked about science.

And the TTAPS results were presented, and it was quite different than Crutzen and Burkes. Because what they had looked at was the collateral fires, forest fires associated with nuclear bursts. Rich Turko, who worked in a defense agency funded by the Defense Nuclear Agency, that is in a defense company funded by the Defense Nuclear Agency, had access to classified data, and knew what the DNA (which is what the Defense Nuclear Agency was called—I always thought it was kind of ironic, since they were the people who were going to hurt DNA), what the DNA was interested in. It wasn't trees, it was cities. And when you look at the amount of fuels—and by fuels I mean tables, chairs, asphalt, roofing tiles, building materials that burn in cities—it dwarfed the total numbers that were available in the collateral damage in forests. And Turko had calculated how many millions of tons, and tens of millions of tons, of soot aerosol would be injected. Toon and Pollack had the one-dimensional radiance convective model they had used to study volcanic eruptions. And then they had to modify that to add soot or absorbing aerosol, because the volcanic eruptions were generally considered to be reflecting aerosols, and that was Tom Ackerman's job as a young radiative transfer guy.

So how did Carl get in it, other than having been the advisor to Pollack and Toon? The answer is, he was a planetary astronomer. And he had been observing in connection with the Voyager mission when it went by Mars and Mars was obscured, and people wondered if there was something wrong with the resolution of the cameras. And what they discovered instead was a raging dust storm. And he had recognized that what was happening on Mars was self-lofting. In other words, as the Martian climate moved towards summer, and the sun began heating a—I've forgotten which hemisphere—this would generate strong winds, which would then cause blowing dust. But the dust, being large particles, absorbed enough energy that it was actually heating the middle of the atmosphere instead of the surface, thereby causing rising convection currents, in other words, self-lofting.

So what Carl's contribution to the nuclear winter was, and this was a very important scientific component, was to by analogy (Margaret Mead would have loved it, an analogical thinker)—what Carl did is by analogy he said: Could we get self-lofting of soot, where the sun would then be absorbed not at the surface as it is now, but will be absorbed in the middle of the atmosphere in the soot, and therefore that would cause it to rise and continue to rise, and thus spread globally? So they made the assumption —they did not have a three dimensional calculation, it was an assumption—that there would be a Martian analogy, and that the soot would spread around the globe. Therefore, that would justify the use of a one-dimensional model, so that they could then calculate the effects. And then the 1-D model with the soot being in the upper troposphere and the stratosphere, most of the sunlight could not get through, as a result, nuclear winter.

Now this model was running in what was global mean sunlight. Global mean sunlight is roughly equivalent to what you'd get in March, or what you would get in September. It was not an extreme season, a winter or a summer. And in fact, unlike those seasons, it would have a little bit of energy—have more energy at the poles. Because remember the poles get a tremendous amount of energy in the summer, and nothing the other rest of the year. So the global average amount of energy is different than the amount for using [the] mean annual. So it was not, in a way, a really good analogy to a three-dimensional world. And they didn't know that, but they had it.

So the reason they called this meeting together was to invite people to shoot down the model. Carl said: Before we go public and make a big splash, which I want to do on Halloween day (about five months later, five or six months later), not an accident, Carl was clever, in 1983, in a meeting in of course, Washington—where there would be a downlink with Russian scientists, because after all it was the Russians and we who were going to annihilate each other, and now take the world down with them. And his concern was that we'd created a doomsday machine, like in *Dr. Strangelove*, and we didn't know it, also as in *Dr. Strangelove*. And it was built in to the very nature of nuclear war when he heard the loose lips of the Reagan administration about winnable nuclear wars. This was going to take the debate away from the politicians and the ideologues, and put the scientists squarely in the loop by saying that there's no significant political objective that could justify this kind of risk for the annihilation of the planet. He wanted to make sure the science was okay.

But he also wanted to take a look at what it would mean, and the scenarios that were then handed to people in the second week of the meeting—excuse me, the second component of the meeting, which is two or three days later. Carl Sagan didn't run that, Paul Erlich did, but it included Bob May, who was then at Princeton, and a mathematical ecologist who later went to Oxford and then became the advisor to the prime minister of two U.K. administrations, two British prime ministers, both the Conservatives and then later on the Labor Party. And Steve Gould was there, looking very gaunt from his cancer treatment for mesothelioma, which we are all glad he is in full remission thereof, and many other ecologists who were looking at these questions.

I remember one night in Carl's hotel room, when we were planning that everybody was going to go away in the summer. They were going to do their best to reinforce scientifically the credibility of the TTAPS scenario, and if necessary point out flaws so that we—rather than our critics who were ideologically motivated, who wanted to push nuclear war fighting—wouldn't be the ones to find flaw. I thought that was perfectly reasonable, just as I had been proud to find what was wrong with the Rasool and Schneider calculation before my critics, I thought it was best that we internally do this before anybody went public. So I asked at this meeting in Carl's room, I can't remember who was there, I know Erlich was there, I think John Haldren was there, possibly Bob May.

And I said: Let's make a pact, nobody goes public, nothing gets discussed until we meet in Washington. And let's go to Washington two days early, so that we can have it out internally and make sure that we have a consistent story that's scientifically straight. Everybody agreed to this. That was a good idea, that we would get together, we would have that meeting. I went back. Kurt, Starly and I worked on the three-dimensional model. We put in an elevated soot aerosol, and immediately got a stunning result. We ran the model in perpetual January and perpetual July mode. We were not running it coupled with an ocean—is that right, or did we have a simple mixed layer?

Chervin: No it was uncoupled to an ocean, it was an atmosphere alone, but I'm pretty sure that you ran through the seasonal cycle.

Schneider: Yes, but we had a fixed ocean temperature.

Chervin: That varied according to the seasonal cycle.

Schneider: Seasonal cycle. But it was not an energy-balanced model. That is, the oceans did not get colder. I was not worried about that, because I knew on the time frame of, y'know, a couple of months, that the oceans wouldn't get more than a few degrees colder, so it wasn't such a bad approximation. What this was going to do was underestimate the severity of nuclear winter. I didn't mind that. Nobody could accuse us of exaggeration. Later on I think we did couple in a mixed layer ocean, but I can't recall, I'd have to go back and check. The stunning result that we got was even when we snapped on the dust cloud in the middle of the northern hemisphere, or maybe the whole northern hemisphere, probably down to 20° North, we didn't do the self-lofting to the southern hemisphere that Sagan had done. We found that within one or two days there was—this is in July now, perpetual July, or it was stepping through the seasonal cycle, but in July temperatures drop well below freezing. In other words, we were validating the one-dimensional model result. One day later the very same spot that was well below freezing was well above freezing. Coasts like California almost never went below freezing, except in the very rare occasion when the meteorological conditions had the winds blowing from the east, and then the cold interior air would blow over it. So instantly we realized it was a highly heterogeneous pattern. The very very warm oceans which remained warm because of the high heat capacity of the oceans, even though in this particular model the oceans could not physically change—if you did an off-line calculation with a simple ocean model, that would be a very good approximation for several months, which is what it would take for the dust to fall out. So we immediately discovered that there would be no uniform response.

The second thing we discovered was that one of the TTAPS runs, which was the threshold where Carl said that even a hundred-megaton war, which is a very small war, very small fraction of the existing arsenals, he had a thousand weapons of I believe a hundred kilotons each, if those numbers add up, and that was sufficient, the illogic went, to "trigger" nuclear winter. Because that was the threshold at which enough smoke went in the air to drop the global mean temperature in the one-dimensional model below freezing, in this perpetual one-dimensional sense world. So we varied the amount of smoke, and reached the conclusion: there was no such thing as a threshold. That for the real world what would happen is that plumes would drift around, and that after the war the combatants would have large blobs of smoke over them, and those blobs of smoke could within a day drop the temperatures well below freezing, as long as there wasn't strong winds from the ocean to the land. Then the blob would drop away, and move out over the ocean, the land would instantly go back up. This was not a perpetual deep freeze, it was fluctuating freezes. In fact this could occur for wars much less than the hundred-megaton threshold that would trigger nuclear winter, yet the planetary average temperature wouldn't get to zero, because the oceans would prevent that. And it wouldn't even be perpetually below freezing in the land because of the very strong on-shore winds. So we recognized immediately the threshold was a non-concept, that it was only a concept that was arising from a one-dimensional model.

We discovered something else, which is when we put the dust cloud, the smoke cloud in in January, almost no cooling took place. First we thought there was an error. The only place cooling took place was in the subtropics. And then it was completely obvious what was going on. There isn't very much sunlight coming in in the winter, therefore depriving the planet of most of it didn't make much difference, because the temperatures were maintained in the middle of continents by very strong winds blowing over the warm oceans with their large heat capacities. That's precisely why Europe and the Gulf Stream are so much warmer than Hudson Bay. It made only a little bit of difference. I remember, I can't remember if it was Kurt or Starly, one of them looked at me and they said, "Great, now we've told them when to push the buttons." We went through a soul crisis as to whether we should even discuss this in public. And I remember thinking: But it's what we found; not to discuss it is to play the same 'ends justify the means' game of people that we don't like their ethics. We have to discuss it, but we don't have to mention it today.

So I went to Walt Robertson and asked him what he thought. And he said, "Are you sure you're right?"

And I said, "We're not sure."

He said, "Do some more experiments. Yes, you have to tell people, but you don't have to tell them right away. You can give yourself more time to be right." And he said, "Can you put it in perspective?"

And I said, “Well, it’s really like fall, and nobody can grow crops in the fall!” It’s in a sense essentially nuclear fall rather than nuclear winter. Because of the fluctuating on and off. And so my line was, you can’t grow crops in nuclear fall. But I wasn’t going to use that phrase “nuclear fall” because I was media savvy enough to know that that would be immediately grabbed, and it would be contentious.

So as summer wound down, and we went into the actual fall, the dates for the meeting in Washington came closer and closer. And I had called Carl and told him what we were learning, and it was kind of quiet, you know. And a week or so beforehand, I get a copy of a *Parade Magazine* article that Carl had written on nuclear winter on TTAPS, on the threshold, and on why the nuclear arsenals had to be reduced by a factor of a hundred to drop below the threshold that would trigger nuclear winter and eliminate the doomsday machine. And Kurt and Starly and I looked at each other and said, but there is no threshold. It’s a scientific fiction from a one-dimensional model. I was in a bind, what was I going to do at this meeting? I remember walking down the street explaining this to Pat Zimmerman and Ralph Cicerone and saying, these guys have created a scientific extrapolation from 1-D, and we had an agreement to talk before we went public!

I got to the meeting, we had our session, as promised. And Carl begins the session by showing a video that he had produced with chilling electronic music, “The World of Nuclear Winter, the Deep Freeze,” and he even went so far as to say this could not rule out the extinction of humanity. I called up Starly and I said: Do you think this would lead to the extinction of humanity? And we both agreed, not nuclear fall. I’m more worried about the combatants being deprived of food, medicine, shelter and national civic order, than I am about the direct effects. I’m more worried about the dust plume drifting over India and shutting off the monsoon, killing more people than live in the rich and combatant countries. But I couldn’t imagine any scenario that was an extinction of humanity. There were way too many people living in way too many circumstances that are relatively low technology that that would happen. And if the war occurred in the winter, it would make very little difference anyway, and the noncombatants wouldn’t be particularly affected. So I was left with an ethical dilemma about what to do about this. So I said at the meeting, “Wait a minute, we have this problem!” But it was too late, because the video had been made, the *Parade Magazine* was coming out, and Carl had written an article in *Foreign Affairs* as to why nuclear policy had to be revised, based upon the threshold.

That began one of the most unpleasant chapters in my life. I knew that Paul Crutzen didn’t want to make nuclear war less severe, and sound a new effect in the process. And I didn’t want to make nuclear war less severe, but the science was the science. And I remember Starly, Kurt and I having the conversation—the credibility of science matters. Because we have to deal with global change, and global warming. And if we do ‘ends justify the means' now, even for nuclear war, we will be reminded of that when we talk about global change. So the question became how to do it. I went to the meeting and I explained there was no threshold and so forth. It was met with mostly stony silence. Nobody told me I shouldn't say that. They just basically said, well these are very preliminary calculations. And Alexandroff and Stenchakoff in Russia have done it, and they get the same basic answer we do. They were using a very very low-resolution version of the _______ model. And it turns out when you looked at them, they were not getting the same answers as TTAPS. TTAPS, remember, was using mean annual sunlight, not July. We got the maximum effect in July, as did Alexandroff. And we were getting very large coolings over the continents, comparable to TTAPS, but only for a day or two in patches. We had a much higher resolution than Alexandroff, and Alexandroff used a very very large scenario of smoke, much larger than we were using when we were identifying in the absence of threshold. So I didn't think that it proved that point. I made the comment, and I asked Carl to stay away from those other concepts so that we don't have a public dispute that will only force the media into detracting from the fundamental message that we all agree, namely that the idea of a winnable nuclear war is insanity of a warped ideologue like Richard Pearl and the Reagan administration officials that he represented. We all agreed about that.

Went to the meeting, I remember sitting behind this familiar looking character who is best known now to the younger generations as the producer of salad dressing, Newman's Own and his wife Joann Woodward, and it was a show. Carl knew how to put on a performance. There was a downlink via satellite to Yuri Israel, and the Hydromet service from the Soviet Union. And of course this was viewed as disloyalty and cohabiting with the enemy by the Defense Department establishment. Don Kennedy, then the Stanford president, gave the introductory address. Walt Roberts supported the general notion, but had already been pre-warned by me and was careful in saying that the details will change. But the basic idea would remain the same, which I genuinely believed. When I got my ten minutes, I went over the GCM's. I emphasized the fact that even smaller wars than the threshold trigger could create fluctuating what I called "quick freezes." But that it was hard to find a threshold. I did not interpret for people that therefore the threshold was wrong. I was hoping that Carl would do that for them, the same way I had to do it for Rasool and myself ten years earlier on our trigger and ice age paper. That's not what happened.

Months later, after a number of interviews, I got a call from Carl. And he was very unhappy at the reporter quoting me as saying there was no threshold. And he kept saying: What have you guys done wrong on your model? And you have to be careful of how you say this.

And I said, "Carl, you have to back away from the 1-D model result. You have to do it, and what you should say is something got better and something got worse. What got better is that we don't have a trigger of nuclear winter. What got worse is even smaller wars than your thousand-city hundred-megaton-war could create quick freezes. And that fall-like conditions, you can't grow crops in it."

There was no agreement. I was supposed to change, I couldn't change. I remember when the *Boulder Camera*, the newspaper, called me up and wanted to know about this. Now the media was covering me versus Sagan, even though Carl and I were much closer in our philosophies to each other than we were to the Richard Pearl Pentagon officials. This was the debate, it was the debate between the milder version and NCAR, and the radical version of Sagan. The *Detroit News* wrote up Carl as an exaggerator and me as the good guy. I had to write a letter back to them saying, I have very few fundamental disagreements with Carl Sagan; you have exaggerated this difference.

Meanwhile, a check arrived at NCAR. This is about early 1984 by now. And I get called into the office by my friend Bill Hess, the man who I bled for money for ASB, and he said: Steve, you know how sensitive the trustees in the NSF are about people writing grants—why didn't you follow the procedure when you wrote a grant to the DNA (the Defense Nuclear Agency) for this money, I mean the NSF just called up furious that they got a check, and they had no paperwork to go with it.

And I said, "I don't know what you're talking about." Apparently what happened is the Defense Nuclear Agency, the people responsible for this had been attending the . . .

END OF TAPE 7, SIDE 1

TAPE 7, SIDE 2

Schneider: So we're on the second side of the seventh tape, and I was talking about how the DNA, the Defense Nuclear Agency went around and they decided who were the people doing the good science. And they separated out about six groups, including NASA Ames. Before I continue the Hess story, let me say that Ackerman, Pollack and Toon were told by NASA Ames higher-ups that they were to stop doing this work. This was the Reagan administration. This was embarrassing the nuclear war fighting strategy of the Defense Department. And they thought that this was going to hurt the NASA budget. I remember having an angry conversation with Bob Schiffer in NASA headquarters a couple of months later, when he said that he agreed that this was a threat to NASA. And they had already said what they had to say, and they should stop before they get into trouble.

And I said, "But Bob, did anybody in the DOD or the White House call you up and say no?"

He said, "No, but they would have."

I said, "This was a preemptive strike to protect your budget, and you could care less about the truth of science?!"

And he said, "It's my job to protect the institution."

So these guys at NASA Ames were writing their home address. This was not a paper of NASA Ames. They had to do all the calculations on their own, and they claimed not on those computers, although how could they have done it without running it on those computers? That was the intimidating nature of the Reagan consensus with the landslide election, and the conversion in 1982 of the Congress switching over to the Republicans. They thought they could get away with anything. And that was very intimidating. And I felt really pleased that Hess called me in the office once and he said before I went to the Sagan meeting, "Steve, I am not going to tell you what to say because you wouldn't listen to me anyway, just be careful that you don't go overboard."

And I said, "Believe me, you are going to find me on the wrong side of this problem, Bill, I'm not going overboard. If anything, I'm going to get attacked from the peace groups."

So the sixty thousand-dollar check arrives, and I said, I don't know what's going on. So I quickly call up the guy who I had met at one of the meetings of the Defense Nuclear Agency, and he told me, "Well we liked the work you guys were doing, so we sent the checks to your agencies."

And I said, "But that's not how it works here. At NCAR if you want to get money, you have to write a proposal; it has to be reviewed; it has to go through a whole series of things."

And he laughed and he said, "And they think that we're bureaucratic in the DOD, you guys are worse than we are!"

So what I had to do for this lousy sixty thousand dollars is I had to write a proposal. We had to send it out for review. We had to respond to the reviewers. We had to get it signed off by the trustees committee that was supposed to do this in the NSF. And about seven months later we got our sixty thousand dollar check, which then later on turned out to be a few hundred thousand checks for the next several years, and those were very helpful. As a receiver of DNA money, I then had to go to DNA meetings, that was part of the deal. These meetings were—and I take my hat off to the DNA—unclassified. Anybody could come, but they had to be invited, you had to be on the guest list.

One of the guests was a guy named Russell Sites who is the nephew of Fred Sites, the former president of the National Academy of Sciences and the Rockefeller Foundation, a very conservative guy with CIA affiliations. = He had actually visited me with two CIA guys, and talked to Starly and me, and wanting to know everything we knew about Russian computing and Alexandroff, and so forth. And Sites was there, constantly trying to nibble around the edges of denigrating nuclear winter. And he thought that I'd be an ally because I was pointing out problems. I wasn't an ally. I thought the problem was absolutely real, it just wasn't the way it was described in the one-dimensional model. At a meeting in Santa Barbara—I remember the meeting very well, because it was about eighty-five degrees in February, they were breaking record highs, and I wanted to get back and work on my other problem, y'know, back to climate change. And I gave my talk. Starly was there, we went over what we were doing.

We had now by this time modified CCM0A by putting in a transport calculation that Starly had done. Essentially, it was to put in a parallel equation to the water vapor equation, and move the smoke around so we no longer had to put smoke in as a block that magically appeared at time zero and stayed fixed forever, but could actually be transported by the winds. And then we used Filipo Georgi, another GRA who was working with Bob Dickinson independently on the question of aerosol removal. And we got Filipo interested, and he helped us write a code for the coagulation and wash out of smoke aerosols. As I was showing this results—an aside, I remember taking Filipo and Kurt and Starly to that meeting in Santa Barbara, we flew into L.A. and I treated them all to Universal Studios. Oh Filipo, he just really loved it. In any case, back to this meeting, so we are at this meeting, and I showed how we could get quick freezes, I showed that when the blobs of smoke were put in and transported around, you know the work that Starly and Phillipo had done, that instead of getting average temperature drops that were significant that we had very little average temperature drop, five to eight degrees at most. Nothing like the 20° the one-dimensional model was getting, but we were still getting quick freezes.

And I said, "It's like fall, we really should have called this nuclear fall."

The audience laughed, remember it wasn't a classified meeting but it was a by invitation meeting, and everybody had agreed that this meeting was private and not to discuss with the press.

Two days later I went home, I get a phone call from *Time* or *Newsweek*, I have forgotten which, I think it was *Time*: "Well, Dr. Schneider, we understand that you were at a meeting of the Defense Nuclear Agency and you described this problem as nuclear fall, rather than nuclear winter, is that true?"

And I said, "That meeting was private. I'm not going to discuss what was there."

He said, "Well, we are going to report this as a source. (It was Russell Sites, I later found out.) "And you can confirm or deny that you believe that, we just wanted to give you the opportunity."

And I said, "Well, it is inappropriate to do that."

And yes, I did say and believe that fall is a better analogy than nuclear winter, but I do not want people to misinterpret this. You can't grow crops in a nuclear fall. You don't have an Asian monsoon in the fall, you have it in the summer, and that this is still a serious disaster scenario. Of course, *Time Magazine* confirmed that I called it "nuclear fall" but they did not put in the rest of the story. I got a phone call from the *Boulder Daily Camera*, as I was mentioning earlier. They called me to ask me about this. And I described everything. There was an excellent story written by Todd Malmsbury. He then called me up that morning, because the *Camera* comes out in the afternoon, it did then. And he said, "I'm not responsible for the headline, I didn't have anything to do with that."

I said, "Why, what's the problem, Todd?"

He said, "You'll see."

The paper came out; my phone rang off the hook because I was in the office at the time. Members of the peace community cursing me out, saying, "You must have money from the Pentagon, you've sold out, you are a shell for them!" And the front page story with my smiling face from a photograph from their previous records was: "Scientist says nuclear war not so bad."

I sure understood why Crutzen didn't want to take down nuclear war's seriousness. And by that time the debate was now full-fledged between me and Carl over the seriousness of this. No matter how many times I had tried in private to get *them* to be the ones to say: *We were basically right but the scenario has changed.* I ended up now full-fledged in the public on it. There was nothing I could do, I could own it or not own it, and I owned it. Lots of stories took place. They were ugly, they were polarized. People were getting violent on each side.

Erlich called me up and said, "You are losing all your friends in the environmental movement."

And I said, "Do they care more about the truth? Do they want to have credibility later on, when we have issues like biodiversity loss and climate change? Isn't nuclear fall bad enough? Don't you go morally numb when we kill two billion, do we have to lead to extinction of humanity? Isn't that enough? Aren't you deterred after the first hundred million?"

And of course Paul said okay, and he became a strong defender, and was fighting with other people to say no, you gotta hang in there. I saw Carl on "Nightline" shortly thereafter, and Koppel was interviewing him, and said, "What do the Russians say?"

And Carl said to the effect: I'm really ashamed that I have been able to talk to the equivalent of the Joint Chief's of Staff in the Soviet Union, and they are showing humanitarian instincts and are greatly interested in this problem, yet my own country is in official denial at the Pentagon about this problem (Not that that was entirely true.), and I can't meet with them.

A few days later I got a phone call from a lieutenant colonel who happened to have been an attaché to the Joint Chiefs and he said: The Joint Chiefs want to have Carl Sagan meet with them, but they do not want him alone, they want you there at the same time, so you can have a debate.

And I said, "Oh, that would be very interesting. They are going to be surprised that Carl and I agree a lot more than the media would let you think. You know we are going to be arguing about global mean temperature drops of twenty versus five, but then I am still going to be arguing nuclear fall is not a nice thing."

And he said: That's fine, we want to hear what the story is. He's alleged that we won't talk, and we will. And he said by the way, what do you want?

I said, "Excuse me?"

He said: Well, I mean, you know, what kind of honorarium do you want to do this?

And I said, "Well it would be a great thrill to talk to the Joint Chief's of Staff—I'm sure I can get my director to pay the airfare, or I hope you would cover that, but whatever honorarium you pay Carl, I want exactly the same thing. I don't care if it's zero, but I'm not going to, y'know, have one paid and not the other.

And he laughed, he said: Oh I guess you want twenty thousand dollars and a private jet to fly you here then.

The debate never took place.

Chervin: Do you have any idea what caused that debate to be canceled? Was it the price tag or some other development?

Schneider: The Lieutenant Colonel also told me that Carl was very strongly against having anything other than a private meeting. A few weeks or months, I can't remember the sequence, but around that time I got a transcript of a Congressional hearing. I had testified to Congress many times, and I know how Congressional hearings work. You get five minutes, and then there's lots of Q & A. If you are very lucky, there's more than one or two senators or members there. And because this was nuclear winter, and because it was Sagan and Richard Pearl, it was a hot number and it was well attended. One of the senators was the liberal Republican Bill Cohen, who was later a Defense Secretary for Bill Clinton.

And he asked Carl point blank: Dr. Sagan, you have been accused of exaggerating. Work down at NCAR and other places suggests that the effects are much smaller than you've said, and that they're nuclear fall, not nuclear winter, and that it would be unlikely to be as severe. What do you say to these critics?

And he said: There is no difference with us, we and they. We get 20° cooling, they get 20° cooling, what's the difference?

I saw that, and was frankly shocked. They got 20° cooling as a global average temperature decrease for a mean annual amount of sunlight. We got five under those conditions. Our 20° were one- to two-day weather coolings in the middle of continents in the July case. I couldn't believe it. All the conversations we had, telling him the truth was bad enough. He was out there saying nothing had changed, the threshold was still valid. It was clearly not scientifically tenable.

I walked into Walt Roberts' office, I plunked this down and I said: Walt what do we do about this?

And he looked at me and he said, Let me think about it— I'll talk to you in a few days.

Two days later I got a phone call from the editor of *Foreign Affairs*. He said, "Walt Roberts spoke to me, I would like you and your colleague Starly Thompson to write a *Foreign Affairs* article to follow up on the Sagan one, telling us what the science and the implications really are of this problem."

Starly and I had a long debate. We asked ourselves how vilified we are willing to be and we said: Hey, it's got to come out. It's better to have us explain it than to continue to have the media distort this into Carl's end of the world vision, and somehow as if we're saying 'it's not a problem' when that's not what we say.

And we wrote our *Foreign Affairs* piece. We said extinction was a vanishingly low probability, but that killing a few billion people was enough to make anybody who had any ethics at all morally numb long ago—and that we don't need the early version to be deterred. And even in the winter, when we found relatively little effects, we said: Imagine getting through regular winter when you have no deliveries of food, no heating, no electricity and no medical services. You don't need to know the weather forecast after a nuclear war to be deterred, unless you are already so insane that you can't be saved. And that's what we wrote.

We got criticized immediately by people who said *Foreign Affairs* isn't a peer review journal—you should have submitted this to peer review. We actually thought about that. We knew that the modeling that we were describing was still being reviewed at the *Journal of Geophysical Research*. And Starly and I said to ourselves damn it, we better be right. Because this is a political issue. We are going to stick our necks out. And we are going to say what results we got. And we're very very confident that we did this calculation right.

The other problem was that the way we were transporting smoke in our model was based upon the water vapor equation of a so-called spectral model, as we talked about earlier. Spectral advection—that means the way you treat water vapor—is fitting fifteen waves, two-dimensional waves, so-called spherical harmonics, to a very patchy pattern. When you do that there are going to be ripples that are unreal. So what would happen is one minute literally, or twenty minutes—the first time step, after the smoke was injected, little unreal blobs of it, small blobs would appear over the Antarctica and other places, and thereby causing quick coolings in those places that were unreal. So the trick that Starly and I evolved was a borrowing and payback scheme. We simply took away a significant fraction, like 20% of the smoke wherever it was, and then put it all back into the heaviest patch. So we conserve smoke that way, and we eliminated all these fluctuations. We knew that this was not the satisfying way to do this problem, but we also knew that the optical depth of the smoke was so large that it wouldn't make any fundamental difference to the overall qualitative conclusion. We knew that because we intuitively understood climate modeling.

We explained what we were doing to our friends in Livermore, Mike McCracken and colleagues, and our good friends in Los Alamos, Gary Glatzmaier who had been at NCAR for a number of years, and worked on solar dynamo models, and Bob Malone who was one of the developers of CCM0A. And Bob had that model, plus they had extended it and they also had the resources of Los Alamos, and had already calculated a transport model that didn't use spectral advection. It didn't have the problem of borrowing and lending. And as a result, about six months later they calculated it with much higher resolution, with a much more "methodologically credible" model than we had. And I was very happy when I got a call from Bob saying: Hey, we get basically the answer you guys did, I bet you're happy about that. I said, "I am unhappy for the world, but I'm glad for me." And it was nice that we were able to be vindicated in that.

And our gamble that we went with the *Foreign Affairs* piece on the basis of our intuition, that the science was going to be at least qualitatively upheld by further modeling, and that we would get our papers accepted by peer review, which we did, was all true. Unfortunately, it led to a very significant schism between me and Carl which was personally very painful, because I admired Carl as the single most influential popularist. I had personally been pressuring him to get involved in public issues back in the 1970's, and he did—and look at the thanks he got from me. And because when I wrote *The Genesis Strategy* and sent it to Carl, he wrote a wonderful jacket comment, and gave me a nice set of things, and I almost felt, you know, like the ungrateful son who was turning on a mentor. And at the same time it was personally painful that somebody who I had such admiration for was, in a way, so caught up in 'the ends justify the means' that he had lost his own objectivity—and was hurting his own credibility when he didn't have to. And I didn't wish to be the executioner of his credibility, and found the entire episode painful from start to finish.

Chervin: Did you actually get any explicit comments from Carl Sagan on the *Foreign Affairs* piece?

Schneider: He wrote . . .

Chervin: . . . to you.

Schneider: Carl wrote a critical response to it in *Foreign Affairs.* And Starly and I published a rejoinder, within which we were as polite and as praiseworthy of their initial effort as possible, while defending what we had. There were other comments written to *Foreign Affairs* by political scientist George Raptins at MIT, who just excoriated Sagan for jumping into a field in which he new nothing, namely arms control, proposing minimal deterrents which he believed to be—he, Raptins believed to be a discredited doctrine, and then using a now discredited scientific claim of threshold to support what he believed to be a discredited doctrine.

We stayed fairly clear of that, but I can remember when I first was working on this problem, how the whole nuclear weapons issue prays on your mind. Flying in airplanes, and all of a sudden at twilight a thunderstorm flashes a bright light in a cloud, and for a microsecond you think it's a nuclear burst. It really is tough working in this problem, it really does something to your head. And what bothered me more than that was [that] after a few years of working on the problem, not only did those fears disappear, but it became a theoretical exercise: How do we model this better? How do we get the transport? And I can remember talking to Starly and Kurt about it, saying, *I start to understand now how people working in these nuclear establishments get themselves inured from the problem. You get—you convert it from an emotionally horrible and unacceptable thought into just another piece of your profession. And that therefore it's a good thing to go out in the public and get your smiling face on the newspaper, so that the people from the peace movement can call you a shit, because even thought you did it because you thought it was more ethical, you need to remind yourself what you're working on.* And I wish that more people who were involved in those kinds of defense projects, y'know, that are hidden by black screens, and get involved in the mechanics and forget what it means, were forced occasionally to go in public and see how their fellow citizens look on with horror at the stuff that they think about, what Herman Collin once called "the thinking about the unthinkable."

There's a lot more to the nuclear winter story, I don't need to tell it all. Vladimir Alexandroff from the Soviet Union, one of the few scientists who ever traveled alone since I saw Bodiqo in 1971, obviously was connected with the right agencies, came over to NCAR once. Got called up by the Director Bill Hess to his office. And he looked at me and he smiled and he said: "I've got to do this, they made me say I would tell you."

I said "What, Bill?"

"Alexandroff is coming, and you have to deny him access to the computer."

Remember this is the Cold War and the Reagan administration.

I said, "Bill, I am not a CIA or an FBI agent. I am not going to follow the guy around and see if he visits the terminals and the keypunches. If they want to do that, they should bring an agent here and do that. I am not going to give him my ID number, and I am not going to help him get on the machine, but I will not be a policeman."

He said, "I knew you would say that. I already told them you would say that. Frankly, I agree with you."

I said, "Thank you."

Vladimir came, we gave a talk, had a nice barbecue out in the backyard, I think you came to that, Robert—yeah you were there, I have a picture of it somewhere, if I could find it, flipping hamburgers, being a very American guy.

Chervin: I was probably also doing the ceremonial cutting of a watermelon.

Schneider: Yeah, I think you got involved in the whole thing. The American Meteorological Society was having a nuclear winter session. So we went to that. There was also a AAAS session on it shortly thereafter. And we went to the AMS session. And we flew together, I guess it was Starly, Kurt—Kurt may have been at Livermore by then, I can't remember—Vladimir, a couple of us went. And we went down to the baggage claim to pickup our luggage, and all of our luggage came except for Vladimir's. And this thing went around and around and around and around, and we waited five, ten, fifteen minutes.

I said, "Well, we better go check and find out what happened."

And Vladimir was looking very suspicious. And he said, "Hmmn, well I know there are some people who are concerned about my visit."

I said, "I don't know, Vladimir, maybe they think you have a supercomputer in your luggage."

You know everybody laughed nervously. We went over to the United people and they didn't know where this was. And they said, "Well you know I think that one baggage cart didn't get unloaded yet, and just still wait." About ten minutes later his bags came off alone. So obviously they were inspected. [We] go out to the rental car, we drive over, we give the talk.

Meanwhile I am having a long argument with Vladimir. I said, "You know, Vladimir, people don't think you are credible."

And he said, "Why?"

I said, "Because all you ever do is the worse case. Why don't you guys run some nuclear fall cases? You can't just put in the maximum amount of smoke—it just makes it look like you are not trying to do the science straight. You have to do heavy smoke cases, and light smoke cases, just like all the rest of us are doing."

And he kind of looked at me and he said, "I will take that up with my people."

He was direct and honest and he said this is under political control. That's what you expect in the Soviet Union. At least he was traveling alone, we could have that conversation.

About two weeks later I was at a meeting, and Mary Rickle, my secretary, called me and said, "I just got a phone call from the FBI. They want to know why you rented a car for Vladimir Alexandroff."

I said, "I never rented a car for him, what are they talking about? Call them back and ask them what they meant."

So she called back the agent, and he gave the dates and said that Alexandroff was seen getting into an Oldsmobile something—no a Pontiac 6000, I remember it—and they had the license number, and they traced it back, and it was rented by NCAR.

And I just laughed out loud, I said, "That was my car! I rented it and I drove him to the hotel. For God's sakes, can you tell the FBI agent, you know, that I'm an American. If they want to know what happened, why don't they call me up and I'll tell them!"

This guy was hanging around with a camera, hiding in the corner and snapping shots. So I could understand the paranoia of some people in dealing with that. And here I am trying to sit there and influence the Russians that they should join the rest of us and stop doing this case. I mean, you remember, Bob, we did a joint paper with them at AMBIO back in 1984. And it was tough to get the Russians to run anything other than the worst case, or to say this is anything other than the end of the world. And they were the good guys who believed in the arms race, and that the Americans were denying this because they didn't believe in the arms race. That was as much ideological nonsense coming from Russia as we were hearing coming from Richard Pearl and the Reagan administration.

One more nuclear winter story I can't resist. Fred Singer, the perpetual contrarian—who believes that all science that denigrates the power of the United States or individual rights must be wrong—had come on a number of occasions, trying to convince Starly and me and others that the nuclear winter scenario was false, that it was truly nuclear summer because of infrared radiation. Three or four times we showed Fred why, even if it had high infrared optical depths, it still—and the planet were like a black body, that the black body with zero reflectivity would still be below freezing, because it would eliminate the greenhouse effect. There was no way to convince Fred that this was not going to happen. So there was a nuclear winter session at the AAAS. I can't remember whether it was 1984, possibly 1985. I give a talk, Fred raises his hand, I call on Fred.

He gets up and he says, "Well Steve, you are moving us in the right direction. You have taken us to winter nuclear and nuclear fall. Why don't you just go all the way to where the real answer is, which is nuclear summer?"

And he went on about how infrared—and I remember the line that popped in my head, and I couldn't resist and I said, "Fred how many times are you going to keep waving the infrared herring in our face?"

Oh, the place—they laughed in his face. I went on and explained why it couldn't happen; why the physics was fundamentally sound; why Los Alamos, Livermore and other such left wing establishments were getting the same answer that we were, which was basically the nuclear fall scenario; why patchiness and infrared radiation did offset the cooling substantially, relative to TTAPS, but nonetheless still left us with a nuclear fall scenario. And that's where the answer was converging, even though we wouldn't be able to perform the experiment. There was no deterring Fred then, just as there's no deterring Fred now, that the only measurement of truth is a short satellite record, which everybody knows is contaminated by stratospheric and surface emissions, which he still insists in the only true record in global warming. So his antecedent routes for denial and poor science were demonstrated then, as they are continuing through the 1990's and the decade of 2000's for the climate change problem.

One other thing—absurdity wasn't just on the part of conservatives like Fred Singer. I was invited to join the highly classified Defense Science Board, which Walt Roberts had been on. In fact, Walt had co-chaired it once, and told me that a military officer was flying in airplanes a few seats behind him, to make sure that he was not commandeered and captured for secrets.

And I said, "Well what were they going to do?"

He said, "Shoot me—what else?"

And yeah I laughed, I wasn't sure it was the truth, but it might very well have been. In any case, I asked Walt whether I should be involved, he said sure. And the lawyer from the ______ and General's office from the Pentagon called me up to find out what my conflicts of interest were, you know, what my—well I said, "I am getting money from The Defense Nuclear Agency, does that mean that when that issue comes up I can't talk about it?"

And he said: "Well how much money are you getting?"

And I said: "Oh, you know, probably two hundred thousand dollars a year, and you know, this is an average sized grant, and we are going on. And do I recuse myself, take myself out of the room?"

And he just laughed out loud.

I said, "What's funny?"

He said: You know, the Defense Science Board has guys from Lockheed and Boeing, and so forth. They are into us for two billion, and they don't seem to have any model compunctions. I don't think we got . . .

END OF TAPE 7, SIDE 2

TAPE 8, SIDE 1

Chervin: First side of tape 8, it's Saturday the 12th of January, 2002 about 5:00 p.m. here in Stanford, California. We are continuing on some of the ramifications of the nuclear winter research, and you had been talking about your . . .

Schneider: Defense Science Board.

Chervin: The Defense Science Board.

Schneider: So the Defense Science Board lawyer said to me, he said, "We got guys from Boeing and [from] North American, and they are into us for two billion. We're not going to worry about you for a measly two hundred thousand."

I guess what he said is they don't have compunctions about talking about anything, so we don't have to worry about you for a measly two hundred thousand, that's what he said. And I guess I realized I was in a different world, different stakes. The other thing that was entertaining about that group was they did not—they set up a task force that I was on, but they didn't call it the "Nuclear Winter Task Force," even though that was the name by which this was still known, and I still use that phrase to describe the problem. And they called it the "Defense Science Board Task Force in Atmospheric Obscuration." And I had to get a top secret clearance to be on it, and I was very pleased that they checked out my background and decided I didn't look very subversive, because I got my top secret clearance. Although every meeting we had was open.

I remember one which had a Deputy Under Secretary of Defense, another Reagan appointee out of Richard Pearl's office. And he didn't like this problem much, and he came, you know, to tell us that. And the DNA, which now was at full loggerheads with Richard Pearl's strategic office. In Congressional hearings the DNA was going and backing up the Livermore, the Los Alamos, the Ames and the NCAR scientists. It was very interesting to watch that one part of the Pentagon, the Richard Pearl shop, was viewing this as a conspiracy against American policy. And the other part, the Defense Nuclear Agency, which is responsible for nuclear war issues and fighting strategies, was defending this as good science and part of the understanding we should have. It made me eliminate all the notions about Pentagon conspiracies. The only conspiracy is the old-fashioned one, which is that each subagency wants its own agenda to be flourishing. So therefore they follow the prime directive of the bureaucracy--the bureaucracy's mission shall thrive and grow. And that was the only conspiracy there, a very healthy conspiracy, by the way, relative to the kind of paranoia that I was hearing from some of my left wing friends about how the Pentagon was involved in a conspiracy, and how they were funding us, you know, to try to diminish the problem. That was all a bunch of nonsense.

The other thing that was funny is this guy, who was the Under Secretary, came in to complain about this problem, and he didn't like the fact that the Defense Science Board's scientists, who were outside people, were endorsing the basic concept. And the DNA was funding an experiment in the chaparral of California where they were going to actually ignite bushes and see what fraction of smoke is removed. There were lots of technical arguments about how high an altitude the smoke clouds would be. Would they be self-lofting?—all these arguments that we still had no real experimental verification of.

And at one stage, as the DNA guy explained what was going on, he said, "And here we will have the fires [and there was this map] and over here on the hill will be the observers."

And he said, of course the Pentagon guy [said], "What do you mean observers, isn't this classified?"

"No this isn't classified, you know, this is a—we're just measuring smoke, there is no reason to do that."

"Well what kind of observers do you have in mind?"

He said, "Well, you know, dignitaries and the media."

"What kind of dignitaries?"

"Well, people like Carl Sagan and the media."

He said, "I am going to get Carl Sagan and the media on one hill at the same time?! Give me the time and the coordinates."

That was pretty good—I liked the guy after that, that was funny you know, it was okay.

One more involvement with the Pentagon that I recall, and each time I got involved I became less worried about the Pentagon conspiracies that I grew up with during the Vietnam War, listening to people proclaiming them. And this time Walt Roberts and I were invited to go to Fort Leavenworth, Kansas to discuss the nuclear winter issue. Probably this was about, must have been 1987 by now, when the science had matured and was settling down around the nuclear fall scenario. And we met with some majors and colonels, who turned out to be involved in the tactical nuclear weapons groups that were on the front lines in Europe. So we gave the usual, that this is a worse issue than would otherwise have happened; that the prime people who would be killed would be the starvation of people in the Asian monsoon zones, and it would make it even worse for us and so forth.

Nobody was writing anything. They were polite but kind of yawning, they didn't really care much. Finally it occurred to me—these guys deal in tactical weapons. And I said, "Gentlemen (because that's all there was in the room), I don't know what [is] the optical depth that your radar can see through, or your cruise missiles, or your optically guided bombs, or your battlefield management cameras in space. But I'll bet that if there were ten-kilometer plumes of thick black smoke with optical depths from three to ten, that you would have an already impossibly difficult battlefield management situation, even more uncertain than it was before. Half a dozen pens picked up, notes written on pages. Then we had a long discussion about that question. Now I had my top security clearance and so did Walt Roberts, but it wasn't a classified meeting, so they didn't answer the question, what the optical depths were, but obviously that wasn't a bad guess.

And I said, "A satellite can't see through that, even if it's not under attack. And how are you going to control the battlefield when you don't know what's going on? You don't know where the gains and the losses are, where the Soviet tank columns are, and anything, because you can't see. And you can't hit them with optically guided weapons because they can't get through the cloud.

And one of the, I guess it was a Lieutenant Colonel, said, "You know, we are on the front line. We already thought this situation was out of control and untenable. You're right. The last thing we need is more uncertainty."

And then others made comments like that. And it was as if they now were free to say: We have a job to do, we understand that this is part of deterrents, but if we ever have to do our job, everything we believe in has failed. So the guys who were in the front lines and in the middle echelons of management in the Pentagon all believed in MAD, in Mutually Assured Destruction. They did not believe in winnable nuclear war, they were not crazy. That was limited to a few ideologues who were political appointees. And I came away believing that we're a lot healthier than I ever thought. These are *not* insane people. It was a nice discovery.

Two more nuclear winter stories, and then it will be enough. One is, there were national and international committees of scientists called to assess the problem. The international one was called Scope Eunuwar, headed by a chemist from Exeter University (his name slipped out of my head, I can look it up). A nice guy, he put together a number of reports. Barry Pitzoc was deeply involved in it, a name that will surface again and again in connection with IPCC, and a number of others. And they were, y'know, asked by SCOPE, the Scientific Committee on Problems in the Environment, to assess the seriousness of these questions. They had a meeting in England, and then we had a meeting about six months later in Thailand in Bangkok. The meeting in Bangkok was by this time maybe 1987. Russia was now under the control of Gorbachev, glasnost was taking place. Alexandroff had disappeared. There was a question as to whether—he had disappeared right before glasnost, by the way—and there was a long series of questions as to what happened to Alexandroff. He was last seen getting out of a Soviet vehicle that had taken him dragging to the embassy in Madrid, to the Russian Embassy. And we just assumed that he had been purged, because he was beginning to argue against the Soviet system. They of course issued a denial and said that he was a victim of the CIA. To this day that mystery is still not resolved. We have not yet to my knowledge found out from Russian defectors what happened to Alexandroff, I would love to know.

His co-author Stenchakoff had never traveled, was not politically connected. At the meeting in Bangkok, there was Stenchakoff, traveling alone. Not only was Stenchakoff there traveling alone, but he now had made all the kinds of runs we were trying to talk Vladimir into making. He would run weak cases; he would run strong cases. He had now become an active and normal participant in the international scientific dialogue, not ideologically controlled. It was so stunning to see the difference between the behavior of the Russians that I had observed at the GARP 16 meeting in 1974, or the World Climate Conference in 1979, or the constraints put on Alexandroff in 1984. And it let me think that there was going to be a real change in the world regime—when you could see at the working level that these people were now free to be the honest intellectuals they were internally, but they were not allowed to be when they were representing a regime which had political control over information.

The report basically was concerned about the nuclear winter scenario, largely endorsed the nuclear fall result. But in my view quite correctly said that the absence of trade, the shortening of the monsoon and so forth would be the biggest effects, and I think they got it right. The U.S. is always suspicious of international assessments. And the National Academy of Sciences did its own, headed by Harvard engineer, I guess applied scientist, George Carrier, a very bright and honest guy. And I was not on the committee, but Starly and I were invited frequently to testify and be guests. They were supposed to be independent, I don't think that the TTAPS are also on the committee.

And one day we had a session at the Academy that was a public session, and it was pretty widely attended. Bob Jastro, who I hadn't seen for years, now was involved with a group called the George C. Marshall Institute that was affiliated with Fred Sites, and was opposing serious work concern of global warming. Nuclear winter was a phony issue, they were now embracing the contrarian agenda. And while Jastro was standing, on the way in, standing next to him was a former student of mine, Bob Chen. And Bob overheard Jastro saying: Oh those sons of bitches, they have weak science. I am going to get up there and I am just going to just show how bad what they are doing is!

So Bob of course told me to expect this, and then strategically placed himself one row in front of Jastro, listening to Jastro talk to the person next to him, cursing out the presentation from Starly, cursing out the presentation from Toon, as we went on in discussing these issues. But this great man of courage, when we were in the room at the front, never once stood up to object, or to give us a scientific challenge. Unlike Fred Singer, who was completely willing to make a fool of himself, Jastro sat there in his seating, and kept quiet.

But what happened at that meeting was more disturbing. Because a very fine meteorological theorist, Norman Phillips, one of the pioneers in numerical weather prediction, a member of the National Academy of Science, was present. And he got up, and he looked at Starly and me and Brian Toon and others, and kind of askance at George Carrier, and he said, "Aren't you all ashamed? This isn't science. This problem can't be verified. Real scientists only work on problems they can test."

I had quite a few answers to that, and as I was about to take a breath, my right arm was touched by Bob Case, who is a social geographer and a member of the National Academy of Sciences, basically telling me, like Mickey Glantz did at the World Climate Conference, shut-up and let somebody else handle this.

And he got up and he said to Norman, "I am ashamed, I am ashamed for the National Academy of Science, and I am ashamed for you. If you think that the 19th century beliefs in what makes good science should dominate our agenda and the importance of the problem has nothing to do with it, I can only be ashamed." He said, "Well, imagine a doctor who, confronted with a patient with a rare disease for which there is little empirical information and not enough to come in in the next few years, who comes in, are you going to not treat him because the problem isn't "scientific"? It will be one day. We have to give our best estimates based upon the best science we can do. And to argue whether or not one is pursuing a problem because it doesn't fit a scientist's definition of traditional science is an outrage."

Phillips, I can't remember precisely what he said, he just basically said: *Well, good science is what really matters to me.* And I guess what bothered me is, good science is not about waiting for the empirical results to get in. That is a complete misunderstanding of scientific epistemology. Good science is about using data to validate the subcomponents of theory that fit together in a coupled systems model that we then use to estimate future events. Our estimations will always and necessarily be subjective, because you cannot have data before the fact. And Phillips is correct about that. That doesn't make it not be science. It just makes the science subjective. And it's up to the scientists to be honest in explaining its subjective opinion. But it is also expert opinion because data is used, both to derive the equations and the structure of the models that we use, and to test the models on analogies, on additional problems—on volcanic eruptions, on asteroid collisions, on other things for which you can find out whether the model gives rough answers, and therefore is likely to be in the ballpark of giving you a reasonable estimate for the future. And that's precisely what George Carrier's committee was doing, and precisely why Bob Cates was right. That it was in the best tradition of science, even though we weren't going to get the definitive empirical validation anytime soon. I wish that that attitude were in 19th century physics, and had passed.

Norm Phillips was not alone, in fact in IPCC in the year 2000 I still had that debate over and over and over again with many colleagues who did not want, and still do not want, to assign probabilities to future climatic events because they haven't happened, even though the world of policy—because we are talking about science for policy in IPCC—the world of policy has limited resources with which to choose what problems to spend what money on. It *has* to know what the likelihood is! Otherwise, we should spend all our resources preventing the great asteroid collision with the earth, like the kind that wiped out the dinosaurs—more unimaginable than any nuclear winter scenario, global warming, or Al-Queda put together. The only reason we don't spend all our resources on it is because it is one in ten million per year to have such a collision. Probability matters, it is scientific to try to estimate the probability of events that you can't know before the fact, by using the best estimation techniques that we have, and then explaining what they are worth and not exaggerating the confidence.

And that's something that I began to see and understand clearly in the nuclear winter debate—that there was going to be an epistemological, that is a philosophical debate, with many scientists who still were living in this 19^{th} century idea that we only deal with directly validatable scenarios, and that scientists define their own agenda, and don't take cues from the people who fund us, as to what it is that we ought to study, or whether we are willing to talk about subjective possibilities of issues that are needed for science for policy. This presaged what was coming next.

Chervin: Okay, as I recall, it was some time in the 1980's that you also added the Natural Systems Group to your responsibilities in ASP.

Schneider: That's right.

Chervin: How did that come about, and who was involved, and what were the scientific topics that you attacked initially there?

Schneider: The Natural Systems Group was this really large, cumbersome bureaucratic structure that consisted of Starly Thompson and Stephen Schneider. It was a name that we gave to what we were already doing. But actually I am glad you asked that, Bob, because there is an old tradition that I haven't talked about yet, from which it was developed. Part of it was the nuclear winter story; part of it was the CO_2 transient. But there was another aspect, and it goes back to Walt Roberts being involved with the Aspen Institute, and with a number of international ventures. One of the ventures he was involved with, which was funded by the Royal Swedish Academy and pushed by the Swedish king, was called IFIAS, The International Federation of Institutes of Advanced Study. Walt dragged John Firor to that group, and somehow managed to come up with some money, a couple of hundred thousand dollars. I would say this was around 1981, right about when I was taking over the ASP Visitors Program. And I was also the overall acting director of ASP because John, as soon as he became ASP director went on sabbatical, to his credit actually, to Resources of the Future in Washington, because he realized that he was going to move in the direction of understanding policy having to do with atmospheric science and climate in particular, and therefore a year with the economists would be a good way to do that. In fact, he was ahead of me in doing that, I did that ten years later. In any case, John, y'know, came back and said: I have this money, what do you think we ought to do with it?

And I had, by that time in my involvement with the Department of Energy program that Roger Revelle was running, the one that was cut by the dentist who was the Governor of South Carolina that Reagan had appointed to the Department of Energy for the purpose of destroying the Department of Energy and eliminating its environmental research. He failed, because the Congress didn't let him. The—let me think—I learned from these groups that what agronomists, ecologists wanted, as I said earlier, is not average temperature changes of 2°, but what happens to extreme variability. It's what happens in the variance [that] matters much more. You know, crops are not destroyed by average temperatures, they are destroyed by a frost. So threshold crossing events are what's important. And what's the probability of changing the crossing of thresholds that are damaging?

So that's what I said to Firor, that we ought to look at that question—and that most of the people in climate modeling are not even thinking about variability, they were already nervous enough about saying anything about the regional effects on average temperatures and precipitation. My God, I didn't want to get into variability. And I said, "Look the models are producing variability, why don't we at least see if it will do any good?"

So John said: Okay what do you need?

And I said, well at that time I was traveling so much and running ASP, there was no time for me to sit and do it myself, and by that time I was not programming the computer. So I had mentioned this to the ASP groups. What I did in ASP is I instituted an every other week seminar series, where three of the post-docs or the graduate students would present their work for twenty minutes, and we would discuss it. And we would try to develop an *esprit de corps* and I tried to make the multi-disciplinary group a little more inter-[disciplinary] by understanding each other. Plus I learned what everybody was doing. And I gave a talk too, and I think I gave a talk mentioning this, and Diane Liverman told me: "Hey, I have somebody who would be good for doing that for you, who is a fellow geographer, physical geographer, at UCLA" (where she was getting her Ph.D.) and her name was Linda Mearns.

So Linda came out, and I talked with Linda and she seemed like a good person to do it, her training was strong in statistics, and [I]made her into a support scientist for a year or two to work on the IFIAS project, which was looking at variability. Linda started out as Linda is wont to do, very slowly, not saying much. In fact I began to wonder after a few months if she was going to do anything. Little did I know that she was reading all the references that I had, and making herself familiar and comfortable with the issues. She also had taken the liberty of going to ISIG, because she was interested in ISIG. And not in this case to talk to Mickey, but to talk to one of Mickey's scientists, to Rick Katz, the statistician who had worked with Al Murphy back in the days of the hail era, and was now working with Mickey on a number of statistical issues related to social questions.

So Linda and Rick were thinking about this problem, and we got together and talked about it, and evolved a very interesting project, where we published in 1984 in the *Journal of Applied Meteorology* the first that I know of paper which said that from the climate impacts point of view what really matters is variability. We looked at agricultural productivity in the U.S. Midwest, and we looked at heat waves in Washington and in Texas. And what we argued is what really matters is the probability of crossing high thresholds, like 95° Fahrenheit, which non-linearly hurts corn plants, and looking at 100° Fahrenheit which non-linearly kills people in Washington or 105° which non-linearly kills people in Dallas, because of the summer acclimatization. And we calculated the probability of crossing those thresholds.

Well first thing we assumed was that the climate was normally distributed, because we had data to show that, and we used the distributions from our actual data in these locations. But then we went on and said: But when you look at theory, it is not at all clear that all you should do is take the normal distribution and yank it to the right 5° to account for a certain CO_2 warming, and then calculate the area under the curve that crosses the threshold. But that's very non-linear and it causes a dramatic increase in the probability of threshold crossing, and those we listed in our paper. But we said two other effects could happen, and this really in a way was attributable to Linda and Rick looking at more sophisticated higher order moments of statistical properties. They went on and they said: Supposing the climate change changes the variance. You increase the probability of higher variability. Then we recalculated how change in variance, either increase or decrease, would affect the probability of crossing those thresholds. And we went even one step beyond that. Supposing that in the process of increasing the heating, you might decrease the daily variance but you increase the probability of blocking, or some long-term change associated with *El Niño*, say, in the jet stream. That could change the auto-correlation. So that means how long a particular anomaly would persist. So you might not have more variability from day to day, but maybe you get two weeks worth of extended heat, whereas before you used to get one.

And we therefore showed that this problem was vastly more complicated than simply a change in the mean or a change in variance, but it was the combination of the change in mean, variance and auto-correlation that would have dramatic impact on the probability of dangerous, damaging threshold-crossing events. And then what's dangerous and damaging would have to be defined in the context of each sector, whether it is agriculture, health, for example, or hydrology, in each location.

And that paper, you know, to this day is still widely cited as having set up the need for variability paradigm in climate impacts analysis. And Linda Mearns has gone on to be, in my opinion, one of the top two or three people in the world in carrying forward the impact assessment from climate change, based upon looking at variability—and not only looking at variability, but Linda's worked extensively with Filipo Georgi, who later joined the Natural Systems Group when it became the Interdisciplinary Climate Systems Session after I left ASP in the late 1980's/early 1990's, by coupling mesoscale models to downscale the results of GCM's. And Linda and Filipo's work, originally by the way started by suggestion of Bob Dickinson, who also was in the Interdisciplinary Climate Systems Section, to particularize in terms of location where the impacts were. Because we knew that we could make more credible statements with general circulation models at the scale of continents and hemispheres, but that to have real impact in the impacts world we had to get regional to local. And one of the ways to do that was translating through mesoscale models.

And Linda has continued the tradition of looking at variability and looking at regions, its just that she did it through additional methods. And again, they got their birth in ASP. Linda, by the way, became a graduate student in ASP, and I was her advisor. And she stopped the variability work for a while and worked on crop climate models. But then I hired her back as a junior scientist to reexamine the variability question and to work with Filipo on the downscaling problem in the late 1980's and early 1990's. And now she is in ISIG and a prime contributor to IPCC, in both Working Groups one and two, one of the very rare people who is viewed as skillful enough to be in both of those areas at the same time, based on her roots. And that's partly what helped us define problems for the Natural Systems Group.

The Natural Systems Group evolved when—I can't remember the year, it might have been 1986 or 1987—when it was clear that what Starly and I were doing was trying to create a global change broadly defined interdisciplinary program. And I felt it was time to come out of the closet. Let's call it what it is. And we're not so interested in just the meteorology, or the oceanography, or the chemistry, or the agronomy, but how things couple. Let's call it Natural Systems, Systems with an "S"—that's what it's about. I remember John Firor was convinced, and he was supportive. I think Mickey was a little concerned, because he was worried about whether we'd actually be able to couple all these systems and come up with anything credible. Mickey preferred doing analysis, that is picking a very particular problem, doing a detailed case study and analyzing it all the way through. He was uncomfortable with synthesis. And perhaps because as a political scientist he was constantly being viewed with suspicion by his colleagues, that he got a little paranoid about any of their attacks on the quality of science, and stayed on the analytic, rather than on the synthetic side. I guess by that time I had been attacked so long and so often that I didn't care very much what anybody said. In fact, I should now say what Margaret Mead told me about that. Let me go back to the meeting that Kellogg arranged with Margaret Mead. This is an aside back to 1974 or 1975. And I guess it was late 1974. I went to visit Margaret in the Natural History Museum in New York, and she lived in one of the towers that you can see at the very top on the fifth floor, that you can see from the street on Columbus Avenue. And it's a . . .

END OF TAPE 8, SIDE 1

TAPE 8, SIDE 2

Schneider: . . . Anyhow, so [I'm] visiting Margaret Mead in these Victorian towers, and it was a kick. First of all you have to get, you know, an escort to go through this because this is the back room of the museum. You walk by all these old cabinets. They have more dead birds and carcasses floating in these drawers. There's probably species waiting to be discovered in the collections back there in the museum, that nobody even knows about that haven't made it out to the floor. Anyhow, this was right before the meeting in North Carolina so I, you know, this is why I went to meet her. And she was at first kindly, but she was a feisty old lady. And, you know, I guess I told her the story about how my *New York Times* story with the Mark Twain quip occurred, and they stamped bullshit on the door, and how my promotions were being threatened. And I guess I was expecting this grandmotherly-like lady to put her arm around me and tell me that I was a good boy.

And she just sort of looked at me, and basically put on her Harry Truman hat and said, "Listen kid, if you can't stand the heat get out of the kitchen!" She said, "That's exactly the way it's going to be. You are going to have to deal with this, because these people are being threatened—because you're saying the way they define quality and the way they see their lives is not the only way, and that's not going to float for them. And then she kind of smiled and she said, "Have a generational perspective. Don't just think about it from day to day; it will change. Your students and theirs will not be locked into their paradigms. They will have minds to make up for themselves. Things will change. Be tough! Hang in there."

And there are many a time when I had frustration, particularly in the nuclear winter era, when I said, we have to do this because we have to protect our credibility to deal with global change, you know, in the next decade. And I was thinking about Margaret then, and boy was she right. Because when I go to Congressional hearings, or newspaper reporters talk, and recently [in] a Charles Krauthammer attack column on me he said this was nuclear winter without the nukes. I wrote back and said, "And by the way, who was it who made nuclear winter into nuclear fall, because that's the way the science fell?" And it's just the same reason why I am telling you now that this is a serious problem, global change.

And it really did a lot to enhance my credibility, having the history of having stood up and taking the heat from the left when it was necessary, when now I was taking the heat from the right, because they didn't want to constrain the activities of their fossil fuel buddies.

Back to the Natural Systems Group. So Starly and I knew that we needed to make an official come out of the closet and say, we are doing Natural Systems, we are not doing disciplines. We are doing paleoclimate; we are doing ecology; we are looking at variability, we are doing all those things that Norman Phillips said we can't validate in advance, and he was right, and so were we because those are the important problems that the world needs the answers to, at least in my value system that's what should dominate. People like Margaret Mead and Bob Cates were wonderful mentors in giving me the courage to continue to do that. In any case, John Firor told me and Mickey that we were going to get a review from the UCAR review committee fairly soon. And he said, "But since the Natural Systems Group's only six months old and there's only the two of you, it's not necessary to present that to the group."

And I said, "John, what are you talking about? Not necessary—it's essential to present it to the group, because there is only [the] two of us and Starly's here on soft money from the guest (probably then still the DNA). I said, I want to get him appointed; I want this group to expand; I want to be able to hire Linda. There are all kinds of things I wanted to do. I said, the only way that's going to happen is if we get the committee saying this is what we need to do."

And he sort of looked at me and he said, "This is a brinkmanship strategy, supposing they don't like you?"

I said, "Hey if they don't like me, I am right where I am now." I says, "So why don't we just—we're still going to be here. We are still going to do what we're doing. Let's give it a try.

And he basically said: your funeral, it's okay. And he backed me, and John went along with it. And Starly and I presented to the committee precisely why we called it a Natural Systems Group; how we had a vision of global change as becoming eventually the dominant paradigm. I did not see two points skill score in weather forecast as being the 21st century problem. But rather working with ecologists, economists, hydrologists, development specialists, in which atmospheric science and climate more broadly becomes a central and key discipline, but not in charge. And I was very pleased because the committee endorsed it, and said that this group was sub-optimal and needed to have more people. And as a result of that it grew to where, by 1992 we had almost a dozen people. Bob Dickinson joined us, Pat Kennedy, Filipo Georgi, Linda, and later on Dave Pollard. And we got a phenomenal amount of things done in integrative studies. And in fact, Natural Systems Group, when I left ASP, evolved in AAP under the leadership of Warren Washington, who was very strongly supportive of this kind of work into the Interdisciplinary Climate Systems Section.

Chervin: Right. As I recall that actually happened in 1987, when AAP evolved into the Climate and Global Dynamics Division, and Warren Washington was the first director of that.

Schneider: Yeah and I guess I was kicked out of ASP, so to speak, either that year or the year after him, I can't remember exactly what the year is.

Chervin: Right. 1987 was when the Interdisciplinary Climate Systems Section was formed.

Schneider: Okay. So it evolved from the Natural Systems Group, it didn't last very long. That was just sort of the precursor. But the fact that the review panel from UCAR gave it a strong and ringing endorsement gave Warren the ammunition he needed to support it, and get it through. By the way, almost everybody in it was soft money. I—the only time—Jack Eddy joined the group too, when he left HAO. Each appointment—Filipo, Linda, Starly—was made when a senior person left. When Jack Eddy left there was enough money in there for me to appoint Starly and half of Linda.

Chervin: Bob Dickinson left . . .

Schneider: When Bob Dickinson left there was now enough money to appoint the other half of Linda and Filipo. So what we were doing—and when Dave Pollard called me up—that's an interesting story because Dave Pollard applied for the ASP post-doc back in 1982. And I thought Dave Pollard was a brilliant young guy from Cal Tech whose Ph.D. thesis, working with a dynamist with a full beard . . .

Chervin: was Andy Ingersoll.

Schneider: Andy Ingersoll, a very very interesting character, lived in a giant Victorian house in Pasadena in sort of a commune-like atmosphere, open-minded guy. And what Pollard basically did is he built a model of the ice ages. It was the first model I had seen which could actually explain the saw tooth behavior of ice age interglacial cycles. And he did it by asSuoming that as the glaciers got bigger, they compressed the bedrock, creating puddles in the wake. And as they started to move south and eventually started to melt, these puddles would float the ice to corner, and like a hatchet would chop off the glaciers, and created a rapid deglaciacian. It was a brilliant model. So I immediately offered David an ASP post-doc. And he didn't want it. He decided that science was too filled with nasty people who were more interested in ego and attack, and that his personality found that so stressful that he would rather be a programmer. And he disappeared, and I hadn't heard from Dave until probably 1988. And then he called back and said, you know that—and I told him don't do this, you have a fine career in science where we will protect you from these kind of narrow-minded guys.

And he called me up and he said, "You know, I have been thinking about it, I just miss science too much." And he was a programmer and he had been doing very very well. Have you got a job where I don't have to be visible?"

And I said, "How would you like to work with Starly on developing a GCM for paleoclimatic purposes?"

Because it became increasingly clear that the GCM group was going to increase resolution, increase parameterization skill, and not do applications. And that we were going to have to develop our own model, if we were going to do applications or we were going to wait ten years, and ten years was too long. And in my view that was how I read the tea leaves. I then called up EPA, which had given me a grant to fund Linda Mearns, and I called up DOE, people who I had done favors for for years. And I said, you guys owe me. I got the opportunity to get this guy Dave Pollard, the best in the world. I need a hundred fifty thousand dollars for three years. And they came through. So we hired Dave Pollard to work with Starly, which was the genesis of Genesis. That's how that model came about, the Genesis model which was then used extensively by the paleoclimate community for years, because the other model was never ready. And that's what the evolution of the Interdisciplinary Climate Systems was from the Natural Systems Group, whose origins really were earlier on from the IFIAS money and the nuclear winter studies that we did.

I've mentioned Margaret Mead a number of times, and then her meeting in 1974 or 1975 on the atmosphere endangering, and the presence of Jim Lovelock. Also during the 1980's I was in a large office, and when I was in the ASP, and I brought up some ficus benjamina plants that were too big to keep at home anymore, because they were so tall and the office was high. And I had them there, and film crews were increasingly showing up because of nuclear winter, and every time there was a drought or a heat wave they would come and talk to me about climate change. You could never get the attention on them for science or policy, but you could get them on weather events, then talk about climate. And were we going outside and film? No, film crews were thrilled, we had the flatirons in the background, and I had these big ficus trees sitting right behind my desk. So I would sit at this big desk, and they would frame my head in the trees, and there would be the mountains in the background. And it served—soon learned that I had a studio there, so I actually rearranged things so they could get cameras in. This was happening, you know, sometimes in the 1988 heat waves it was once a week.

So when I got kicked out of the upstairs office and moved downstairs, it was on the ground floor in the back, but you could still see the mountains from the ground floor. So I set my desk up so that the big picture window was behind me. So I didn't get the advantage of looking out the window, and the big ficus trees were sitting there in this frame, and again I used that as a studio dozens and dozens of times.

One of the events that occurred was the arrival of "Nova." "Nova" came to me and they said, we want to do a program on the Gaia hypothesis. That's Lovelock's and Margulis' argument, which is that the biota of the earth control the planetary atmosphere and the climate for its own good. There's negative stabilizing feedback. And Gaia was paid attention to only by two groups, by polluters who were using it as an excuse, the great negative feedback and the, you know, in the soils and the oceans; and by [the] sort of people who were looking for oneness in nature, the Buddhist philosophers, that this is a great global being, mother earth, Gaia. It was largely ignored or disdained by the scientific community. And I thought that was wrong, because I thought it was a brilliant and clever insight, but probably not right, but certainly worthy of discussion.

And BBC had done a "Horizon"—that's their equivalent of "Nova." It is a forty-minute program on Gaia hypothesis. WGBH in Boston had bought the program, but it needed to be fifty minutes to fit the NOVA format or I admit—those numbers may not be exactly correct, but that'd add about ten minutes. And they asked me would I look at the program, did I have any comment. I looked at the program, and basically the program was a point of view journalist piece. It contrasted the radical scientists Lovelock and Margulis against the liberal establishment that was almost like the Church opposing Galileo. And that was nonsense, because it was completely clear that the critiques of Gaia were strong and powerful, and at the same time Gaia should be taken seriously.

So the NOVA people asked me if I would be the science advisor to the program. So I became the science advisor, and I changed the theme of the program. I changed it away from "Lovelock and Margulis are the valiant and correct scientists," to "They are the daring people proposing new ideas," and that science advances by daring and ideas, but that those ideas don't always survive intact after further analysis and debate, and that's precisely how science is healthy. And I gave examples of the critiques, and one of them was mine. I said planetary self-control was never defined. You can't say what's good for the biota. By biota do you mean the longevity of species, the diversity of species, the biomass? Every single one of the those could be completely different. One could go up and one could go down. That was a fuzzy concept; we had to define it better. We had to be able—I know I sound like Norman Phillips—but we had to be able to define our terms; otherwise we can't do hypothesis testing. It just was a vague concept. So then they would film me, then they went back to film Lovelock, and they were going back and forth.

And at one stage I said, this must have been 1986, this is outrageous. Why am I debating Jim Lovelock via BBC and "Nova" producers? Why don't we have a scientific meeting? So I proposed to the AGU that we have a Chapman conference on the Gaia hypothesis. Well, there were a few people inside, there was Ron Rodera, a former trustee and Aronamer he was an aurora physicist from University of Alaska. And there was Ralph Cicero. And they carried the torch to the AGU council to have a Chapman conference, the most prestigious scientific meetings that you could get at the AGU, on the Gaia hypothesis that I would co-chair. When I invited Penny Boston to join me, because I wanted to have a biologist, Wally Broker was on the committee—he was outraged. "Gaia isn't science!" "This is ridiculous!" It took the committee about a year and a half before it finally got around to approve it. I guess Ralph and Ron were sufficiently persuasive that in the end we got the meeting. The meeting took place in San Diego, it was actually one of the most spectacularly dramatic meetings that ever took place. The very first talk was Lovelock. He went up and gave the history of how he studied physiology and medicine, and how he then came to evolve physiology as a metaphor for planetary systems—you know, think at a different level, think out of the box. I thought that was all great, but he ducked the questions about definition and hypothesis testing. Then Lynn Margulis got up and gave a rousing speech about how anybody who doesn't see how a planet controls things after all the biota produce oxygen, and they radically change things. And she ignored my critique which was yes, but the biota that produced oxygen ended up relegating themselves to the anaerobic niches of the inside of termites and cows, and they didn't exactly do anything for themselves. It was the fitness of the creatures that could breathe oxygen and evolve—sounds more like Darwin to me than like some new change. In any case so we had this debate. I invited Paul Erlich to come to the meeting, and Paul loves to be heterodox, but this time he got the orthodox role. It was his job to defend Darwinian selection and traditional evolutionary theory.

He got up there, sat at the podium, he looked at me, and he said: "Schneider you bastard, I love attacking things and you are going to force me to be doctrinaire, so let me start. There are a hundred thousand examples for Darwinian evolution and none for Gaia."

Margulis went nuts, it was really great, the audience was abuzz. This was how the meeting was. We had a session on a Tuesday night called "Epistemology." David Hawkins, the philosopher of science, was there. And a young graduate student working with John Hart at the Energy and Resources Group named Jim Kirchner came. And he gave a philosophical and historical *tour de force* that took the meeting upside down. He basically reviewed the history of Vernatsky, Hucksley and others, and showed how people arguing that the earth worked as a planetary system was not new. He also showed, by quoting Lovelock and Margulis, that they had five Gaia hypotheses, ranging from weak, which was easy to defend, to a teleological, that is purposeful strong one which is more like religion. And he said, "I don't know whether to be for or against Gaia. The weak ones have already been established, and the strong ones are kind of nuts." Well this just brought the house down. It polarized the community. I remember beginning it by reading a very laudatory comment from Roger Revelle how about it is about time that this thing got out of the media and into the real science, and a comment from Wally Broker saying how dare you have this meeting, I won't go because Gaia isn't now and never will be science. It was, you know, science shears Wally that also brought the house down showing that to everybody.

So this meeting took place—a large you know MIT published book evolved after the AGU spent one year not being able to decide whether it wanted to publish the proceedings. They sent it out to review to members of the AGU, two of whom called me up and said, what is this Gaia thing? And I said, why are you being asked to review something you've never heard of? So I called back the AGU and I said, if you don't make a decision in the next six weeks, I am going to take this property and consider my obligation to you is over. They never answered, it went to MIT press and they published the book after Penny Boston and I spent about two years getting the articles peer reviewed, getting the sloppier statements out. And that I was actually very proud of. And Wally Broker called me up after this meeting and he says, "Steve, you bastard."

I said, "What?"

He said, "How dare you put on this meeting. I heard it was one of the best meetings in the world, and I wasn't there!"

I said, "Wally why didn't you come?!" So I liked Wally because he could call up and say okay, I was wrong I should have gone. He thought it was going to be a whitewash for Gaia; it wasn't.

It said that a number of fundamental and important things—like Steve Warren, Bob Charlson and Andre Andreas' extrapolation where they found the DMS produced in phytoplankton in the ocean could leak out into the atmosphere in the form of sulfate, and then become condensation nuclei, increase the albedo of clouds. Back to Sean Toomey and SMIC! They found a biogenic mechanism whereby Toomey's concern, which originally was based upon anthropogenic SO_2, actually could influence cloud albedo and may have amplified or not—we don't know—ice age interglacial cycles. So Lovelock was doing the job. And I felt kind of proud that the way I transformed the BBC into the "Nova," where the idea was that Gaia may not survive intact, but in the end it is going to advance science because people are passionate about the ideas. And when they are passionate they are going to go out and study, and they are going to look at the earth the right way—as a system, not as a disconnected set of disciplines. And I think Gaia was very helpful in doing that.

Penny and I and Jim Miller ran a second meeting in 1990, no I don't mean 1990, that was only two years after the first one. I mean in 2000 in Valencia, Spain. The reason for Valencia was Lynn Margulis talked us into it, since it was the four hundred fiftieth anniversary of the University of Valencia, and there were a number of people there who were interested in this problem. And by the time we were at that meeting Gaia was no longer controversial. A number of things had been discovered. In fact Ty Lavock and Dave Schwartzman from Howard University, Volk at NYU, had shown that when vascular land plants, with roots and veins, were able to get into the soil, they broke the soil up so much that they exposed the mineralized carbon in the soil to erosion from carbonic acid, which is what happens when rainfall falls through a CO_2 atmosphere. And then that radically increased the draw down of CO_2.

And as a result, Gaia was doing what it was supposed to do. It was leading to interdisciplinary earth sciences. Although there were as many examples of Gaia being a positive feedback as a negative feedback. And what I argued for is that the right paradigm was, I guess the title of the book I had in 1984 with Randi Londer, it was *The Co-Evolution of Climate and Life*, which could be a positive or a negative feedback. And that depending upon the time scale and the particular variables in mind, life would either accelerate or constrain change in the direction it was going, and that we should abandon the notion that we can assume that it's a self-controlling negative feedback. That's still controversial. We still haven't got all the results in, but I think that now Gaia has become more mainstream in the scientific community. And the people who used to use it as an excuse to say we will go on polluting no longer do, because even Jim Lovelock in his later book said: I was wrong about that. That was a poetic notion I was only thinking about life surviving. We could easily threaten individual components of life.

So I think that a lot of those issues have now resolved. And the earth science community is enriched for having, you know, clever thinkers like Lovelock and Margulis proposing outrageous hypotheses, which again, may not survive intact, but they've sure advanced the rate at which we have made interesting discoveries.

Before we leave the ASP years, there are a few other stories that come to mind that are fun reminiscences. I remember that it was ASP, rather than the computing facility that actually was leading in getting color terminals, and in doing video graphics of computer models. The cabal of computer jocks was students and post-docs—Starly, Nuset, Michelle Verstrat was there, and to a lesser extent, he was more of a user than a pioneer in this was Danny Harvey. Danny Harvey actually is exemplary of the kinds of ways that I could increase the student population at no expense.

Remember I had mentioned earlier how Bill Hay from Miami had sent Eric Baron over at his expense to work in my group, this was before ASP. And Eric by this time now was working with Warren, although unfortunately the main AAP division didn't really see fit to give Eric [the] full set of support. And therefore he didn't hang around, feeling that the meteorological establishment at NCAR was not fully prepared to deal with a geologist and a paleoclimatologist with the same degree of resources and respect that would be afforded somebody working on atmospheric dynamics. It was very unfortunate, and I also felt, a major error on the part of NCAR management to allow somebody like Eric to escape by virtue of having more attractive offers on the outside, when in fact he probably could have advanced his career more rapidly at NCAR by modeling. In any case, a similar situation came up when Kenneth Hare, the renowned geographer from the University of Toronto, who also by the way was one of those who was a strong supporter of interdisciplinary studies, an early member of the Editorial Board of Climatic Change, and one who joined the Woods Hole Workshops that Bob White had arranged in the late 1970's, and fought with me and Mickey and others to add multiple disciplines to the global change agenda, and not define atmospheric science strictly in classical meteorological terms.

Ken called me one day and said, "I have a graduate student who wants to do climate modeling, he is very advanced mathematically, relative to almost any other geographer I have ever seen. He is exceedingly energetic, and I think you are the only one who can handle him. I'll pay for him."

And indeed he sent Danny Harvey. And Danny came out, and he was every bit the dynamo that Ken said, and he worked with me for a number of years. We built box evection diffusion models. And then we used them to run a number of climate experiments, such as examining the effects of various ocean mixing regimes on transient responses. And then to simulate atmospheric ocean coupling models, we showed that a number of the numerical schemes that people used to couple atmosphere and ocean models—which when you do it with full-blown three-dimensional models is exceedingly expensive, and hard to find out what's causing your simulation to behave the way it was behaving—we were able to show that one really needed to synchronously couple the atmosphere with the upper layer of the ocean, that is to run them on the same time scale. Otherwise you'd have major errors. We also showed where you can reduce those errors. But by and large what we were doing is the old hierarchy of models behavior that Bob Dickinson and I argued for so strenuously in our survey article in 1974—the same idea that evolved from the SMIC meeting in 1971. And we were again trying to show that while three-dimensional models were essential eventually for accurate simulations, that one would learn a great deal about them by doing simple simulations of these complex models. And Danny really took the lead in that.

Danny also had broad interests in other aspects of global change, and when he eventually became a professor of geography in Toronto, has branched out to work on everything from carbon cycle modeling, to a greenhouse plan for the City of Toronto, and has actually written the best textbook there now is on climate theory and modeling from the energy balance simpler model side. So that again was another aspect of ASP.

One final recollection I have is the egg drops. I think it first began with the private school, with Bixby School where my son was going. And I always used to go help and teach in these schools. My daughter was at the Mapleton Elementary School at the time. And I was often asked to go in and be, essentially a science mentor in these classes. And one of the things that I got involved in, which I had heard about somewhere, was egg drops—where you would drop a package containing an egg off a high tower or building. And the idea was the kids should design it so the egg would survive the fall intact. And they cleverly designed parachutes, and Styrofoam, and springs, and all kinds of things. It really was fun and they had to decorate them and so forth. And what higher tower is there than the

NCAR tower over the parking lot? So I remember arranging an egg drop. And I am trying to remember who it was inside the NCAR community relations office (what is her name?) who, when I asked permission to get this done joined in, and then told all kinds of people at NCAR. And it was just absolutely fun, because that tower had not been used for a social purpose since 1973.

In fact, in 1973 these crowsnests towers were the scene of a rather unusual event. There was a short balding guy with long hair who was tied up in a strap and dropped off the tower. Of course he was supposed to be wrapped up in computer tape, and he really was Woody Allen, who made his movie *Sleeper* partly at NCAR. This was probably one of the most entertaining disruptive events in the NCAR history. Everybody turned out to see it, because Woody Allen and the entire Hollywood crew arrived with the citymobile trucks and Diane Keaton in tow. And they took extras from the NCAR staff to become Gestapo–like security guards.

And I remember when the lady recruiting them, who was recruiting the extras, had a meeting in the Damon room where everybody was supposed to be selected. I was going to be in this movie. This was going to be my Hollywood career, so I put on the tightest fitting, brightest turtleneck with stripes I could do, something that they would notice; wedged my pushy self to the front of the room; sat there, and it worked! I got picked to be an extra. Of course I had to take vacation days from NCAR. They paid us the whopping rate of twenty dollars a day, but the agent who booked us took a quarter of it. So she took five of it, so I was getting fifteen dollars a day, which I of course soon realized meant that I was being utterly ripped off. I was being paid more than fifteen dollars a day that I was losing in my vacation days. Nonetheless, this was the chance to be in Hollywood, it was really fun. Woody Allen was the great idol of those of . . .

END OF TAPE 8, SIDE 2

TAPE 9, SIDE 1

Chervin: . . . side of Tape number 9. It is about 10:45 p.m. on Saturday the 12th of January. And as I recall you were talking about your Hollywood in Boulder at NCAR experience.

Schneider: Yes, my first and last Hollywood experience. Well anyhow, we were all excited about Woody Allen, having enjoyed his movies, especially his self-deprecating New York shtick, which for those of us who had just come in from New York, this was really great fun. Anyhow we were recruited as extras. I was dressed up in Gestapo uniform. I remember going into the truck where they gave me the black clothing that I was wearing, the arm bands called security. I think me and Julian Shedlofsky, a chemist, and then there were a couple of people they brought in from the outside who I didn't recognize—we were dressed in black or white. Generally, the black people were dressed in white, and the white people were dressed in black, a little more Woody Allen humor. And at least the future was racially integrated. And this was supposed to take place two hundred years hence, and he had slept through it, and then was discovered. And there was a leader *a la* Big Brother, and the leader had been assassinated. His nose was left, and his nose was going to be used to clone, and then of course Woody Allen gets involved in the underground to steal the nose and prevent the cloning.

So what happens is a few of us were chosen. The black and white and me the white and black were chosen to chase Woody Allen and Diane Keaton as they went running out of the NCAR front door down the driveway. We chased them, and they threw the nose down and a trained hawk would swoop in, pick it up and go off, that was the shot. Well, the first four or five times the hawk didn't find the nose. I might add that the shoes that I had on were the only ones they had in the property shop. They were about size fourteen, and running in those was not exactly a lot of fun, to say nothing of "clippity-clop." I was waiting to do a header right in the middle of the show, but fortunately that didn't happen, although just blisters on the feet.

Then finally the hawk landed, picked up the nose, and did nothing. About eight cuts later, and takes later, I began to realize that Hollywood was not all it was cracked up to be. That everything was "Shoot it over, shoot it over, shoot it over, shoot it over"—so that you can get thirty seconds of film, you have to go through half a day of filming. I watched that actually happen in other scenes that Woody and Diane were in. So finally they decide to have a clever idea. They go inside and they get hamburger meat. They stuff hamburger meat in the nose, what does the hawk do it flies down on the tenth take, picks up the nose, picks the hamburger meat out of the nose and sits there. Exasperation is beginning to set in everywhere. Exhaustion is setting in in our legs.

And finally the trainer of the hawk, a young woman with large deep scars on her face, came along and had another idea about where to put the hamburger meat and so forth, put the hawk in from a different angle. And finally it works! We are chasing Woody Allen, the hawk picks up the nose, turns around and flies about twenty feet until it hits the glass walkway, which is one floor high. It was an arch where you could actually see through from one glass panel to the next. And the hawk obviously didn't know that there was glass—it looked like it was an opening like flying through a canyon—hits the glass, falls crashing to the ground, nose and all, and is stunned on the ground, lying there. People run over there and go, "Poor hawk!" and Woody Allen has the presence of mind to say "Get away from that bird, it's dangerous!" Anyhow the hawk did not revive well. It recovered, but it was not able to do any more takes. We had done thirteen meaningless takes. And they thought they might be able to get something from that last take.

That ended my Hollywood career after two days, until about a month and a half later when the phone rang and I was asked to do it again. They were going to come back, because they decided they did not like the scene, so the same black and white and me white and black were then chosen to chase Woody Allen and Diane out the door, except this time they had a new idea. There was a giant steamroller made to look high tech that was driving up the driveway toward the main Mesa Lab. And we chased them straight down the driveway, he throws the nose in the ground under the steamroller, the steamroller runs over it, and we stop and pick up the nose which is now squashed into about a two-foot wide flat pizza piece of rubber in the shape of a nose. It really was a very funny substitute idea.

So again about five or ten takes, and after one of them I remember sitting there and staring at the camera, as opposed to looking at the nose, and Woody Allen yells, "Cut!" And he looks at me and he says, "Come on, your grandmother's going to see you anyway, just look at the nose, you don't have to look at the camera." It was pretty good. Oh, about six months to a year later we all with great anticipation went to see it, and lo and behold, he was right, I was there between those two clicks which are less than one second apart, you could see my smiling face looking at the camera. He left it in, I was not entirely on the cutting room floor, but our entire chase of him down the driveway was missing. The chase of the hawk was missing, and I learned what Hollywood was like. Lots of film, very little on screen.

Anyhow that was the last time the tower was used for any such frivolity, and now here we were in the mid-1980's getting a bunch of kids, late 1980's, dropping eggs off the balcony. The kids loved it, the teachers loved it, and lo and behold, so did the NCAR staff. Who was the person, I can't remember her name, who was handling community relations?

Chervin: Probably Rene Muñoz.

Schneider: Rene Muñoz, Rene really liked it. She told everybody around the building. And when I did it again the following year with Mapleton Elementary, in my daughter's class, there were all kinds of NCAR programmers and post-docs and others. They came along, they had their own eggs, and we ended up having the second grade class from Mapleton along with other NCAR people all dropping eggs off the tower. And it was a celebration, it was in the staff notes. It really was one of the fun things that I remember. The students liked it, and it was, again, highlights before the nastier times took place in the 1990's.

Earlier I had mentioned how my office is a TV studio at times. By 1988 it became a TV studio three times a day. In May, June and July the super-heat waves gripped the eastern two-thirds of the U.S., and the media discovered global warming. Essentially the problem moved from the left brain of we intellectuals arguing about whether the problem is real, to the right brain of the political establishment now deciding whether or not it actually had to do something about this issue. The media was all over it. They were out at NCAR all the time, and there was just a phenomenal amount of coverage. The NCAR press intelligence service was going crazy. I probably, and Warren Washington and others were in fifty clips a month around June, July and August on this problem. It also gave rise to the belief given that Senator Tim Worth, the Colorado senator and a friend of ours, who had had a number of people testify, first and most famously Jim Hanson, when he said it was time to stop waffling around and accept that the green house effect was responsible for the warming. And then later on a number of others, including myself, who went to several hearings held by Gore and Bradley and Worth. And as a result of that, people were talking about actual legislation.

There was a meeting in Toronto called the World Climate Conference. And although they called it the first World Climate Conference that was incorrect, because there had been a World Climate Conference in 1979. This was really a conference run—and this time for the first time—not by the scientists and the government establishments, but by NGO's, by non-governmental organizations. And they had an agenda, and their agenda was to push a protocol or a treaty, in which there would be cuts of 20% in the emissions of the countries of the world, in particular the rich countries. And this was a radical agenda, because it involved something that would actually cost interests real money.

And then what happened is people like Pat Michaels at Virginia, and Dick Lindzen and Fred Singer, who had long been the opponents of public science and the ones who denied there was any seriousness in global warming or nuclear winter and other such issues. They were essentially handed a very big and loud megaphone by the fossil fuel industry, and by the ideologues who did not believe in protecting the commons. And that's when a lot of ugliness began. Because one of the tricks that was used by the so-called group of contrarians, the formation of the Global Climate Coalition and industrial lobby against climate policy, was character assassination. They followed the line of what the chemical industry had done a decade earlier in the 1970's, by attacking Roland and Malina, you know impugning their character over the ozone depletion area. And they began doing the same kind of thing with scientists such as Hanson, and me and others who were discussing the issues. There was lots of Congressional testimony, lots of media—and lots of nasty and growing contention about this problem, as it switched from an intellectual discussion to one of whether there should actually be policy.

In the background of this, Bert Boline, remember Bert from discussions we had about his meeting fifteen years earlier, was asked by the Union Nations Environment Program and the World Meteorological Organization to convene an international study. I remember running into Bert in Washington at a National Academy of Science study, one of many. I think this is one that Bill Nordhaus, the economist from Yale, and Ramanathan and Bob Sess and I and a number of others were on, Jerry Mellerman. And we had for about the fifth or the sixth time reaffirmed the standard line that nobody could be sure, but that it was likely to be a one and a half to four and half degree warming for CO_2 doubling. And we were ordering the things we knew well, and separating from things which we had some idea about, and separating those from things that are speculative; beginning the subjective probabilistic analysis not in a sophisticated way, but trying to distinguish between those things that we could know for sure, and those things which we had a good intuitive grasp could occur or might occur. And I remember seeing Bert Boline and he told me about the possibility of creating IPCC, and he asked me what I thought.

And I said, "Bert, I think it's a terrible idea."

And he said, "Why is that?"

I said: because this is now the Bush administration, and John Sununu and the Bush administration was strenuously arguing that we should be doing research, and until we have more information we should have no policy. And I said, "This is just going to be another two years to do another assessment. It's going to provide another excuse for the Sununus of the world to call for delay. And what are they going to learn that we don't already know from assessments made in Australia and the U.K., and it's at least five or six times now from the National Research Council."

And he looked at me and he said, "But how many of those are convincing to people in India or Indonesia or developing countries who don't trust the science that comes out of it?"

I said, "Yeah Bert, I guess you're right." I said, "But we have had so many assessments and the scientists are all leverage, when are we going to get time to our own work?"

And he said, "And what happens if we don't do this, and we do not have an international consensus? Will it be possible to have climate policy without having a scientific group in which various countries in the world have some political ownership?"

And I realized he was right. I said, "You're right, I guess it has to happen."

And indeed it did, and the IPCC then over the next two years met, and it was phenomenally successful. Because when the IPCC report was in late craft stage, it was the prime basis for the credibility that could be established at the Second World Climate Conference in Geneva. I went to that conference and I was absolutely stunned at its difference from the First World Conference. Not only had Sir John Mason lost. (He argued that it would be premature and irresponsible to have political leadership involved in the climate issue until we had resolved to a very high degree of certainty all the scientific issues.) This meeting was dominated by political leaders. And in fact about the only really good science there was the IPCC. Because by this time the NGO's of both the environmental ones and the industrial ones were in an "end of the world" and "good for you" false dichotomy debate about this problem. The battle lines of contrarians versus deep ecology was drawn. And various governments were lining up on various sides and the IPCC was about the only credible fresh voice—and the reasonableness of Bert Boline to try to hold together the credibility of the meeting to maintain the integrity of the science, and at the same time fight off the wild overstatements coming from extremes on all aspects.

This then led to the next stage, which was the United Nations Framework Convention on Climate Change, which emerged from the real meeting, the World Conference on Environment and Development, where the very famous phrase was that it was the objective of the framework convention to "prevent dangerous anthropogenic interference in the climate system." We are still to this very day arguing about what that means, because the UNFCC did not define it. And it's clear that what's dangerous is not a scientific judgment, but a value judgment about what's important. On the other hand, in order to make that value judgment, we have to know how much climate change will take place. And we have to know what will be impacted, what will happen to wildlife, what will happen to fisheries, what will happen to coastlines, to human health, to agriculture and so forth. And how do we weigh a change in agriculture in one country versus a threat to health in another? So what could be dangerous to one country could be a benefit to yet another. None of that was defined.

And this was to set up the agenda to occupy IPCC in the 1990's and still does to this very day. But the UN Framework Convention on Climate Change set up the COP process, COP, the Conference of Parties, which then was all throughout the 1990's a series of meetings of national governments, in which a climate protocol was to be hammered out. And these meetings were highly contentious. They were punctuated by the presence of a large number of media; by NGO communities; and by independent scientists and academic organizations who registered as non-delegates, and then would have information sessions that were discussing all various aspects of it. All of this derived from the Second World Climate Conference's pushing of the real meeting. The real meeting was prime one.

One more recollection about the real meeting was that President Bush, George Bush Sr., had not decided whether to go, right up until the meeting. And there was a high degree of pressure. And I was invited to a meeting in Washington in the Spring of 1992 to try to help put pressure on Bush to go. The meeting was called by Carl Sagan and Al Gore, then a senator. What they had done is assembled the leadership of the major religions in the United States. And the premise of the meeting was that we may not agree about the origins of life, but we agree about the need for stewardship. Climate change was one of the prime issues there, and we all got together at this meeting and wrote a joint statement about how important it was for the U.S. to be a prime player in continuing its world leadership that it had been losing since the Reagan administration's denial of global and environmental problems, continued by the Bush administration to a slightly milder degree. This was really powerful, getting major church leaders from the Baptists, and the Catholic Church, and rabbis, and Muslim leaders and others together to do that. At one stage Steve Gould, who had been fighting a long battle with these very religious leaders about teaching of creationism in schools, came to the meeting.

He got up and he looked at the scientists and he said to us, "How can you be talking to these people? These are the same very people who are trying to rewrite the nature of science; do not understand that science and religion are different; and who have been leading pernicious campaigns against intellectual values."

It brought the house down. At which stage Carl Sagan got up and gave an impassioned speech saying that we are not here to argue origins, we are here to argue common interests.

And Al Gore took over the microphone, and in one of his most articulate moments got up and said, "I am a man of deep religious faith," he said. "Yet I believe in stewardship. I believe that God's requirement of us is not a literal interpretation of the Bible, but rather an interpretation of our stewardship responsibility. And I am not going to let anyone get in the way of that, and I am thrilled that the bulk of scientists of goodwill and religionists can put aside their differences about origins and move forward in protecting the planet."

And it really saved the meeting. Well the next day I was invited by Al and Carl—in fact it was nice, because I was able to have a reconciliation for the first time since the nuclear winter era with Carl. We were on the same side, and it was fun fighting together again. And they put me on at the last minute to replace Steve Gould in lobbying members of Congress and the Senate the next day. Because they decided that his presence would not be a helpful influence in the group.

And I can remember the leader of the Catholic Church saying to George Mitchell and to (oh come on, he ran for president) Dole, Bob Dole, the leaders of—the Senate Majority and Minority Leader: I just want to let you know that we are going to read the following sermon on Sunday, and there are more people in those churches than there are people in schools. He said, "I think you would do well to listen to us, and I think you would do well to inform your president," he said to Senator Dole, "that if he continues to deny his participation in this meeting, that he will find that the church groups will not consider that to be an appropriate ethical behavior."

It was amazing. Shortly thereafter President Bush announced that he was going to Rio. And he did indeed sign the treaty, which committed the U.S. to voluntarily cut back its greenhouse gases in the 1990's to near its 1990 level. But of course by the end of the decade we were up by 15%, not down by five.

So around 1990 it was a very busy time. In fact it was busy for me because I wrote a book called "*Global Warming: Are We Entering the Greenhouse Century?*" where I essentially described what had happened in the 1988 heat waves, and how frustrating it was to me, having tried for fifteen years to get people's attention on this problem. And then essentially an irrelevant random fluctuation comes along and the problem becomes too credible, whereas before it wasn't credible enough. And I found myself in the ironic position of saying, "No, ladies and gentlemen, this is not global warming, because the world has only warmed up about a half degree C, and this is many degrees warmer than normal. We've only been responsible for a few percent of it. However, if you don't like this, stick around, because this is the kind of event that we are going to have increasingly frequently in the future. And therefore it is symbolically important of the reason of the need to move."

I remember environmental groups calling me up and saying: No, you can't take this away from us. We have to tell people this is global warming!

And I said: You know, next winter when it is abnormally cold, then the contrarians are going to tell us that that's global cooling. I think we have to play this one straight. The truth is bad enough. We've got very good evidence that we are a half a degree warmer. We have increasing evidence that humans are responsible for it. We have very strong evidence that we will continue to increase the pressure on the system, and very likely cause significant, if not dangerous, change. Let's just stay with 50% to 80% chance of significant change. We don't need it to be 99%, especially when we can't defend it as true.

And that was basically the points that I was making in global warming, as well as telling the history of the story. I did have one chapter that I was very bittersweet about writing, which I called "Mediarology," about how difficult it is to communicate honestly complex science through the media. And I told the stories about the distortions that go on—the fact that the media, trained in political reporting, generally has the view that if you get the Democrat, you get the Republican. You give equal time to all claimants. And I was arguing [that] this "doctrine of balance" is pernicious when applied to science. Because science is rarely just two-sided. And what scientists do, like they did in the National Academy studies and like the IPCC, is they winnow out the relative likelihoods of all of the various outcomes. And therefore we are not in the business of equal time, we are in the business of quality. And therefore what we need to do is report the relative strength of the arguments, not give equal time to all claimants of truth. That is an anathema position to advocates, elite lawyers who get equal time in a courtroom between prosecution and defense, and in political debates.

So I became very controversial for opposing the doctrine of balance. People accused me of being against the other side in fairness, whereas what I was arguing for was peer reviewed assessment, much more so than trial by op-eds, where you go back and forth from one end of the world to one "good for you" guy from one day to the next, leaving the public more confused. I also decried the sound bite system. I gave a very cynical interview with a famous journalist called Jonathan Shell who had won a Pulitzer Prize for a book called *The Fate of the Earth* when he was writing on this for *Discover Magazine*.

And I somewhat tongue-in-cheek said, "Look, we are forced by the sound bite system in which you get twenty seconds on the evening news and this "doctrine of balance" to conjure up scary stories, to leave out the caveats, to get media attention."

I didn't say I was in favor of it. In fact, I was decrying it. I said, we end up with a double ethical bind between telling the whole story or not getting any attention.

And I then said, "I hope we'll do both." I went on to explain that the way you do both is you can talk in sound bites, but only if you have a hierarchy of back-up products. That is if you also have op-ed pieces which explain in more than twenty-second length what your views are. You still can't put in all the ifs ands and buts, or all the scary stories, but you can put in some. Then you write *Scientific American* length pieces, of which I had written several by that time, in which you can give the twenty-minute length talk—something in the length of a "Nova" for the factor of ten fewer population that is going to read that, relative to the op-ed pieces. Then finally you can write full-length books in which you *can* put in all the if's, and's and but's, and all the scary stories. That's what I was trying to do in the global warming book, and had tried to do in *The Coevolution of Climate and Life*. So that was my argument.

What happened was a big mistake. Because my enemies, the ones who were attempting to discredit the problem by discrediting the witness, using those legal tactics, took the interview in *Discover Magazine,* which by the way did not give the full context. I put the full context in my chapter, my meteorology chapter, where I discuss the double ethical bind. And what they did is they quoted out of context the sentence where I said we have to conjure up scary stories, get media attention, and forget the caveats. And then they put that in end of quote, as if what I was doing was arguing for people to distort. And they said you see, Schneider has a secret plot to advise his colleagues to lie for the sake of getting attention, and that's what all environmentalists do. This was then quoted everywhere. There's a web based organization called Lexus Nexus where legal and news was. And people quoted it, and they quoted each other.

And this distortion, this mis-quote took on a life of its own. It was quoted by Lindzen, and quoted by Michaels, and quoted by the Climate Coalition. And I was even being told by people that it was getting hard to invite me to go to give Congressional testimony, because if I showed up, the people who wanted me there were afraid that the other side was going to start attacking me as advising people to exaggerate and lie, because didn't I say this. No, I didn't say that. I said something different and broader, but that's not what was quoted in that article. It taught me that when you are being honest and open about the nature of the process, and also, never be tongue-in-cheek was my lesson. I learned that that was not a smart thing to have done, because now I had to live, and still to this day have to live with people who still quote that out of context, who still leave out the last sentence in the *Discover* story when I said you have to, y'know, balance being honest with being effective, and I said I hope that means doing both. And the story, as I mentioned, that Jonathan Shell wrote still left out how to do that. At least I put it in my books.

And I remember once when the *Detroit News*, the same *Detroit News* that said I was the great hero in the nuclear winter era, and Carl Sagan was the villain, then had me as the lying villain who was just typical of all the global warming people who were trying to wreck the auto industry, you know, and scare people by quoting this. And of course they quoted out of context. They deliberately selected the things that made it look worse, and totally distorted and turned upside-down my meaning. I wrote back and got an op-ed response in which I said it's hard to imagine how I could be accused of trying to lead a clandestine plot to get my colleagues to distort and exaggerate, when I am forthrightly explaining what's wrong with the sound bite system to a Pulitzer Prize winning journalist in the form of Jonathan Shell, who is going to write about it in *Discover Magazine*. I would be about the stupidest guy on the planet to be revealing my conspiratorial secrets to him. I was attacking the horrible system into which we are dropped by a media brought up in the world of political reporting trained to get the other side, and too lazy to find out who's more credible. To this day, I still am deeply embroiled in that battle, most recently in a book written by a Danish statistician Bjorn Lamborg.

I still have this battle repeatedly. I've don't at least ten debates with Richard Lindzen in BBC in front of the World Bank economists and others, still arguing these very same points that began to heat up in 1989 and 1990. So those heat waves and those books, even though they have my full explanations, they have me talking in subjective probabilities, not in absolute truths, are only read by tens of thousands of people, not by the millions who see these distortions, lies, advertisements and character assassinations of the Global Climate Coalition and their ilk. And when I see it from the environmentalists, I get just as angry in response. Because what it does is it dums down the debate into a series of polarized character assassinations. And what gets lost in the middle is any semblance of truth. And truth is not the answer. Truth is a series of subjective probabilities and possibilities, because that's all science can offer about the future.

Chervin: Well at the same time that you were thinking globally, what were you hoping to affect at the local level in the NCAR establishment?

Schneider: Well I remember that I was asked by Rick Anthes to head a hopeful program at NCAR, called the Climate Systems Modeling Project, CSMP. And I told them no, I said I am not going to do it, it's not going to work. We had established a proposal that was—this is I am now talking fifteen years post JEC, which Starly Thompson and I proposed to John Firor, and he proposed to the trustees, to a committee, and this is when we were invited to have more internal proposals for new starts. I can't remember what we called it—Climate Systems Modeling? It was a name which slips out of my head. I will have to talk to Starly and get the name. It was the number one proposal again.

Chervin: It was the Climate System Modeling Initiative.

Schneider: Climate System Modeling Initiative.

Chervin: CSMI.

Schneider: CSMI—we got money promised to us. And we tried to hire Turko, Toon and Ackerman, who by that time had had enough of working where they wanted to work. And I thought they would be a great addition. We recruited them; we were going to build up this program. What happened? Right when this initiative was about to expand, I can't remember the years, it must have been 1988, 1989.

Chervin: Right, it was the late 1980's as I recall.

Schneider: And Rick Anthes had been . . .

Chervin: Anthes at that time was . . .

Schneider: . . . just become President of UCAR—right, and Bob Serafin had become the Director. And this was the number one initiative based upon write-ups. We had to write them up. Firor did a great job of presenting it, and Starly and I wrote it up. And we were supposed to proceed. We had this hiring set-up. We were going to broaden it and include ecology, and maybe even economics, trying to do that although we had a lot of resistance, quite frankly, from ISIG, about anything that would change their own self-driven agenda. So I just felt that we just do this in spite of them, and they are a fine group that did their own work, and if they . . .

END OF TAPE 9, SIDE 1

TAPE 9, SIDE 2

Chervin: Now we're on the B side of Tape number 9, it's 11:20 p.m., still the 12th of January. And a bright light just lit above my head. And as I recall that initiative, that you had to go through all the hoops and hurdles thereof, was called "Coupled Climate Systems."

Schneider: Right, right good memory! As you can see, readers of this text, I'm entirely unreliable about such names, but at least I got the concept right. So we proposed Coupled Climate Systems. And we were going to use low order models to do it. And what we argued (I now remember it well, thank you for reminding me.) was [that] there was no contradiction between the GCM group of Williamson and (Who else was in there?) Hack, people like that—and some of the oceanographers who wanted high resolution, lots of physics, models that would take forever to run. You could never run more than a few months of simulated time without busting the computer budget. And what we wanted to do, we wanted to run for dozens, if not hundreds of years with low-water models, more physics in terms of subsystems included, but in much less complexity.

We did not see this as a contradiction, because we said what we will do is we will run the applications and the coupled systems, and we will explore what happens to the behavior of the emerging properties, as it later was to be called by complexity theorists, of coupled models while the other group developed the high resolution models. They did not have the taste to run these applications. They didn't like running low order models over long periods of time. They wanted to get the dynamics "right" and improve the parameterizations, and that was fine. Because as computers got bigger, we'd take their model just as soon as they were finished with it. And as soon as they were finished, they wouldn't be happy and they would now go to higher resolution and more physics, and we would run that in coupled climate mode. So that was basically what the proposal said that we would do. And we wanted especially to have land use in it, because Bob Dickinson, who had been in the Interdisciplinary Climate Systems Section, had developed the BATS, the Biosphere Atmosphere Transfer . . .

Chervin: Scheme.

Schneider: Scheme. That was the first biophysics scheme. And he showed that if you don't have stematol resistance of plants and so forth in the GCM, you are going to get your vapotransporation wrong, you are going to get your surface temperatures wrong. And I remember people, especially the dynamists, laughing at Bob when he was showing pictures of trees and leaves—ha ha, the guy is putting leaves in the GCM. Yet Bob was right, and he showed how significant they could be on meteorological factors.

Now this wasn't Gaian negative feedback, but it was certainly coevolution of climate and life. It was a main interactive component. And it was clear that coupled systems were essential. So our proposal that John Firor presented in the NSF, that Starly and I wrote, was number one, and therefore we were supposed to have this initiative. What happened was we tried to hire Turko, Toon, and Ackerman. It was almost essentially approved. Anthes was supportive; a number of people were supportive. And then low and behold, NCAR got a budget cut. So what did NCAR do? It protected the usual classical, traditional components—and the initiatives were lost.

So at one stage I went to Rick Anthes and I said, "What happened to the promise?"

He said, "Well we didn't cut your group, did we? We left it the way it was."

What, all two or three of us? Most of us were on soft money anyway, what was to cut? So I had a rather disenchanted frustration with NCAR. I know that Bob Dickinson was really upset by what happened, and it essentially drove him to go elsewhere. Because he was so frustrated that he had been promised, and we had been promised that we would be funded to do this kind of coupled systems, and including the biosphere work. And when push came to shove, they backed away and went back to traditional old stuff, away from the new global change research. Bob wasn't a fighter and a confrontationalist, and when he was offered the opportunity to leave he took it. And none of us really talked him out of it because he had essentially made the decision before we even knew it was happening.

I know that Ralph Cicerone also was frustrated by exactly that same kind of management decision, where traditional things were protected and the innovative was not. And when he had the opportunity to leave, he too did. Ramanathan also had left. I was really becoming concerned. In my opinion, the prime intellectuals in the building were leaving, and there were too many technicians staying behind, and not enough of those charismatic leaders that were so essential whom Margaret Mead had defined earlier, if you want to do creative work. I was still there, and I was still hopeful that we could do something. About a year later Rick Anthes asked me would I head the Climate Systems Modeling program. I said, "Hey."

Chervin: The initiative.

Schneider: The initiative. And I said, "You know, 'fool me once, shame on you; fool me twice, shame on me.' I've been there, done that, nothing happened."

"No, no this is going to be different. This time we are going to get the community to participate."

Rick was honest, he wasn't trying to fool me. He believed it. We got a large group of people together. We had a three-day meeting. And basically what happened was the universities looked at this as an NCAR grab for money. The design that we had was that you can't build large coupled models easily in universities. That's not the way people get promoted. That's not individual science. So that would be NCAR's job. And we would want these codes to be accessible to our university colleagues who we had hoped would provide the scientific credibility on building each of the subsystems. It was a good idea, it was well received. The budgets weren't good. And I think that people on the outside just felt too threatened, saw this as yet another NCAR attempt to capture too large a share of the budget. Remember since the JEC there was high tension between the very people who ran NCAR, who were competing with NCAR for the same pocket of money.

Unfortunately, my original cynicism proved to be correct. After months of working on this initiative getting meeting after meeting, getting the goodwill of both university scientists and NCAR scientists to put it together, it didn't have a political prayer. And by this time I was genuinely becoming disenchanted. I guess because of that degree of frustration, when Paul Erlich called me up and he said that we are going to have a slot at Stanford soon in the Biology Department, and the ecology and evolution subgroup which consisted of scientists whom I knew and had worked with for a long time, particularly starting out in the early 1980's over the nuclear winter problem—Hal Moony for example, Peter Vertusic, and Jon Roughgarden, and Paul. These people I knew to be brilliant ecologists, and very broadly interested in large-scale problems.

And they said to me, we don't have a globalist; we don't have a climate person; our students are being deprived of being connected to the largest component of the biosphere, namely the climate system.

I wasn't sure that I wanted to go to a university. I was no longer running the ASP program. I genuinely missed that interaction with the students. I didn't really have time and energy anymore to go teach classes like I had at Lamont three different times, because it just would take too long to be away. And I had too many committees, too many testimonies, too many obligations.

I had the Interdisciplinary Climate Systems Section to not just run, but to protect against carping people trying to cut it down, claiming that it was an indulgence at a time of budget cuts to do this, when we should be putting all the resources into the prime issues, such as the GCM. People in the main GCM group were furious because Starly and Dave Pollard had created a parallel model of low order. We were basically doing the coupled climate system initiative on our own, with DOE and with EPA money. And we were interested in paleoclimate, transient climate applications. And we weren't going to wait for the high resolution "give us all the physics" guys to spend another ten years to be satisfied that their model was sufficient. We had very unpleasant debates with the other parts of AAP, quite literally at retreats' ugly shout-outs about people accusing us of squandering the precious resources of the division, on a less than state of the art model, because it wasn't the highest resolution possible.

And I said, I'm not going to run a model that can run for a month. We want to run for ten years or a hundred years, and if that means lower resolution that's what we are going to do. We will try to make sure we are asking questions that that can responsibly answer.

And the paradigmatic response I got back is, "You can't do anything responsible at low resolution." So it became increasingly clear that there was not going to be an agreement in principle about dividing up the work of the Interdisciplinary Climate Systems Section which was applications and coupled systems oriented, and of the other GCM group. They just were going to view us as dangerous competition. That was not endearing me to NCAR. The defection of what I considered to be the best and the brightest of the charismatic intellectual leaders was not a thrill, and I did not see the trustees who were still viewing NCAR as competitive with them for resources as a good sign. So when Paul invited me to consider applying to Stanford, that was the first university offer I actually really seriously considered in a long time. I allowed him to proceed with the recruiting.

Around this time, it was November of 1990, I was invited by an ecologist named Edward, that is Ted LaRoe, from the Fish and Wildlife Service, to go to Oregon State and give a talk to a group of ecologists and wildlife people who were interested in how climate would impact on wildlife. As I was then learning the science of biography—which you know, determines how the range boundaries of plants and animals are determined by temperature and precipitation, and so forth—suggests that as climate changed, that the plant and animal communities would be moved around. And the question, as to whether the nature reserves that are in fixed locations would be able to protect the very animals and plants that they were there to protect, was raised. This actually was a question that was first posed in the early 1980's by Tom Lovejoy when he came to NCAR, had lunch with me, and we said: How can we talk to each other?—and essentially evolved the synergism of climate change and fragmented habitats, that is, humans fragment habitats into smaller and smaller patches. Wild animals and plants get squeezed into these patches. Now we change the climate, forcing them to migrate, but this time they don't have free range, they have got factories, farms, freeways and urban settlements to cross and this would threaten extinction.

We had a meeting in the National Zoo in the late 1980's over this. And Linda Mearns and I and Peter Glick had written the chapter on the climate component of this potential synergism. Well anyhow, Ted LaRoe, now working for the Fish and Wildlife service, was interested in putting into action in that mission agency a plan where they could take a look and find out whether their collection of wildlife reserves would actually be able to protect the fish and game and wild animals that they were supposed to. So I was invited to give a talk at that meeting. And I gave the keynote talk.

And I remember Ted saying, "But I thought there was a problem with the thermometers, and that they weren't perfectly accurate."

I said, "Well that's true, they are not perfectly accurate. That's why we have, you know, a half a degree warming, plus or minus two tenths."

He said, "Well why can't we use animals as an indicator of climate change? After all, they have been moving around, and we know the range boundaries. And shouldn't we do that?"

And I said, "Well, I think that you don't want to use animals as a thermometer when we have thermometers. What you want to do is find out how the animals are moving around; compare that with temperature so that we can come up with a forecast scheme to say how the animals will move around further when the temperature changes a lot."

And he said to me, "Well there is a young woman here named Terry Root who has just shown with her thesis in Princeton, she is now at the University of Michigan, that there is a very tight correlation between temperature and bird ranges. Isn't that right, professor Root?"

And she said, "No, I am afraid I agree with Schneider about this one."

So I liked her right away. I mean, how if she agrees with me, and we had long conversations. And I discovered that what Terry had done was to show that the physiological requirements of birds determine how far north they could literally shiver their way through the winter. And that therefore, if the climate were to change, Terry pointed out, these birds would be able to move rapidly north, based on their physiology. But the other point that she made is that it isn't just physiology that constrains their ranges; it's the physiology of the vegetation that they need for nesting and for food. And therefore if the climate would change as rapidly as degrees per century, the vegetation could not keep up with that. So the birds would be able to move, and those birds that did not need vegetation would move right away. And those that did [need vegetation] would have to hang behind and wait for the vegetation to move, thereby tearing apart the communities of species.

And it occurred to me—wow, that's the real problem. We have been looking at the wrong problem in climate and ecology. We have been looking at the problem of how individuals would move, and we really have to look at what happens to the communities—what later on was called the ecosystem services movement. And I suggested to Terry that we write a grant proposal to the Winslow Foundation to do that, and that she and I would then write a paper that would try to explain how climate modelers who work at scales of five hundred kilometer squares could talk to ecologists who work in tennis court-sized plots, and how we might be able to have some predictive interactions.

I went out to visit her in Michigan in March of 1991, at exactly the same time that I was being heavily recruited by Stanford. And when I went to visit, I gave a talk. And one of her colleagues, Jim Teary, who was running the biostation, called me into a room. And he and Tom Donahue, who was well known to atmospheric science types, he was in Michigan at the Michigan Engineering School and the Atmosphere Ocean and Space Science Group, and had been working in stratospheric dynamics and chemistry—they both called me in, and they smiled and they put on the board the virtues of living in Michigan. And I thought this was, something was up. And they said: We want to create an environmental institute at Michigan. We'd like you to join with us to do it. And there was a group of four people—Henry Pollack, a geologist very interested in these questions; Gail Ness, a demographer interested in these questions; Jim Teary, an ecologist; and Tom Donahue, an atmospheric scientist. And they said we need a charismatic integrator, and you are it. Come and join us and create an environmental institute at Michigan.

So now all of a sudden not only did I have the Stanford possibility, but now there was a possibility to go to Michigan. I, in spite of all the rapid things I do in my life, do not make moves that quickly and easily. I may not be as constrained and sedentary as Chervin, but I still, I'm not ready to make instant decisions. There were also family considerations and other problems at the time, as well as family stresses that were occurring in my marriage at the time. So I decided to try it out. I set up a scheme in the fall of 1991, where I would spend twenty percent of my time in Stanford, twenty percent of my time in Michigan—no I am wrong—forty percent in Stanford, forty percent in Michigan, and twenty percent at NCAR on sabbatical, because I was entitled to a sabbatical at NCAR. And that's what I did, I commuted back and forth across the country, trying to get a feel for each of those programs.

Stanford had just created an Earth Systems program in which I then taught, I think, the very first class of their very first course; and also had just created a Senior Honors program in Environmental Science, Technology and Policy that was going to be co-taught by the former president Don Kennedy, the same person who had given the keynote talk at the Carl Sagan Halloween party in 1983. Don was a former head of the Food and Drug Administration, an exceedingly articulate and broad intellectual. Another person was Roz Naylor, a young economist from the London school who was organizing the program and work with us; and Larry Goulder, an environmental economist and public finance economist, who was interested in questions like carbon taxation and efficient ways to solve the problem. I found the Stanford students brilliant kids; I found the college brilliant; and I found a tremendous frustration. It would be impossible at Stanford to get a professor tenured outside of an academic department.

The Earth Systems program was broader than any I had ever seen. It had three classes that were required of all students. One was called Geosphere, which was a geology course, basic geology and plate tectonics and so forth, with calculus. It had Biosphere, which Peter Batusic taught, which was basic ecological methods—again, with calculus, ___________ equations, population dynamics equations, and so forth—and the Anthrosphere course that Larry Goulder taught, where cost benefit analysis was done, again, with calculus. These students were getting rigorous multi-disciplinary background. The problem was every professor was in a department. It was a lend-lease faculty. I then taught several lectures in the Earth Systems opening course, which was not one of the core courses but called the Introduction ______ course. And I found it a very attractive program with superb students, but I was concerned about the incapacity of the program to hire faculty outside of the standard rigors of disciplinary narrow promotion system.

Michigan, on the other hand, claimed it would have an Institute. So the bargain that I held out with Michigan was: If I'm to come here, we are to have the approval of the president and the deans that the Institute will grant tenure to people, perhaps half time—half appointment in the Institute, and half appointment in the academic departments. It looked like it was going to be a go for a while. There was lots of campus/cross campus excitement. But I began to discover that there was a different problem at Michigan than at Stanford. The problem at Michigan wasn't the departments; the problem was the schools, the deans. Michigan was dominated by deans who were predominantly interested in protecting *their* administrative unit, and they weren't interested in sharing power and resources, let alone joint tenure. As a result, when the committee of four, when Gail Ness and Jim Teary and Tom Donahue and I went in to see the President of Michigan, I guess it was Duderstat at the time, he enthusiastically supported our effort, and said that he would put all the resources in his office at our disposal to write a science and technology center proposal.

And I said," Excuse me with all due respect, President Duderstat, but that's a fishing license. What will the university guarantee with regard to the capacity to generate tenured slots in the department?"

"Oh, well once you raise the funds, and once you demonstrate the success of the Institute, I am sure that we will be able to get our deans to consider what departments these people can get their formal tenure in."

I slumped down in the chair, and realized that Michigan was not going to happen—that this was again another academic idea killed by the narrowness of the combination of dean-oriented schools and discipline-oriented departments. In the end, I went to Stanford because of the high quality of the students and colleagues, and also because at that time it was clear I was going to get divorced and that Sheryl, my then about to be ex[-wife] was willing to go to the Bay Area, and we therefore could have joint custody of the children without a legal battle. And that, plus what I had seen made it clear that Stanford was going to be the choice.

What about NCAR? When I told the management at NCAR that I was seriously considering going to a university, they said: No no, you should stay here. We really don't want you to leave. They did a very flattering thing, they created a new position which they called the Super Senior Scientist, with a substantial salary increase which I was given. And given that NCAR salaries are published, this generated even more hostility than I had experienced in quite some time. The second [thing] that they did was ask me, what did I want to stay?

And I said, "I don't want money, I don't want power. I want us to have a global systems division. I want us to have a couple of climate systems programs. I want us to be able to hire economists and ecologists to work on integrated assessments, so that we can move forward and look at the coupling across the disciplines necessary to deal with climate policy and environmental policy issues."

I guess the answer I got back I think what Rick Anthes said is, "It's a really brilliant idea, but it's just too radical. We just can't do that at this time. Can we do something a little bit less? Is there something else?"

And I started to think: I am going to end up in disciplinary frustration at Stanford to some degree, but at least I will have colleagues from these professions that I want to work with. I am going to end up with disciplinary frustration at NCAR, and I won't have colleagues from ecology and economics to work with. I might as well just go to the university, and at least I will have students, and I really miss the students. The real hesitation was what was going to happen to the Interdisciplinary Climate Systems Section if I left and wasn't there to protect them from the hostility that existed from other elements in the institution who saw us as a threat to their access to resources. Nevertheless, it became clear to make a choice. And I was one of the last ones left of the original cohorts that I had so enjoyed interacting with—Dickinson and Ramanathan and Eric Baron, and I just was the next casualty.

I remember the last day that I was at NCAR as a fulltime person. Oh, by the way, I negotiated a deal where I would stay on as a ten percent senior scientist, in order to run a grant I had with the U.S. Forest Service. And that was agreed to, and that lasted about three more years before Maurice Blackman terminated it. But that way I still tried to maintain the NCAR connection, I guess secretly hoping that we would be able to get that global systems division up, although it didn't appear like that was going to happen at any time. I remember the last day, I think it was the 1st of September on the 20th anniversary of the day that you, Robert and I arrived at NCAR. And it was the retirement day for my secretary Mary Rickle in her thirteenth year. And I got up and gave a speech about Mary's loyal service, her defacto assistant editorship of climatic change, her being a press agent, a secretary, a computer operator and all the things that she had done, you know when people congratulated her on her last day.

Chervin: And a gatekeeper as well, as I recall.

Schneider: And a gatekeeper. She was the wolf at the door who protected my schedule, and an absolutely phenomenal asset to me. In fact I remember that getting her to work for me took a little bit of doing thirteen years ago. My argument was: Wait a minute, I'm a scientist. I guess I was a Senior Scientist at the time, I can't quite recall. No, I may not have been a Senior Scientist. I may have just been a Scientist-5. And every Scientist-5 or Senior Scientist was generally entitled to a support scientist or a programmer. I didn't ask for one, I only asked for an administrative assistant, which costs less, and that was the argument I used, and very fortunately John Firor and others agreed to it. So I had Mary. So I gave Mary the farewell speech. It was a wonderful party, it was very well attended. And I sat there as I was doing it, saying goodbye to her on her thirteenth anniversary of arriving, and realized this is the twentieth anniversary of [my] arriving and I'm leaving. And I think you, Bob, were the only ones who said, hey you are leaving too. That hurt. I remember it well.

Chervin: Yes, as I recall there was a farewell reception on the tree plaza.

Schneider: Right, for Mary, that I led. And it was my last day too, although in some senses I hadn't resigned, I kept my ten percent position. So in a way, I could rationalize that. But it still reinforced in me that maybe I made the right decision, and it was time to go.

Chervin: Okay, so in the late summer/early fall of 1992 you made the commitment to Stanford University.

Schneider: No, actually I made the commitment in March, but I went . . .

Chervin: Right.

Schneider: . . . in the early fall.

Chervin: How did you adapt to the change in lifestyle of a professor, as opposed to a researcher, with obligations of regular undergraduate teaching, and pursuing contracts on grants, and establishing a research program on campus basically from scratch?

Schneider: In a Biology department.

Chervin: In a Biology department, especially keeping in mind your own academic training, which involved Engineering degrees.

Schneider: Right.

Chervin: How accepted were you when you finally arrived, and to what extent were the promises that you received in the courtship phase realized when you had signed on the dotted line?

Schneider: Well with ten years of retrospect, they are beginning to happen, but it was by no means a honeymoon. Let me start by discussing the fact that at NCAR, even with its problems, I had my secretary; I had my travel resources; I had my computer time, Xeroxes, mail, overhead all covered.

Chervin: Right, and that was all covered by the umbrella base budget from NCAR . . .

Schneider: That's correct.

Chervin: . . . from the NSF and it required limited amount of paperwork and pleading.

Schneider: Correct. I also had substantial grants which was keeping about half of the Interdisciplinary Climate Systems section on soft money, but the other half we had converted to hard money by the attrition of Bob Dickinson and Jack Eddy, and so forth. And then after I left I think that freed it for several others. But I wasn't going to have that at Stanford. In fact, I was quite worried about how I was going to make ends meet over time. I was quite worried about how I was going to recruit graduate students who might work on climate models, or the connection of climate models to the ecology or other things, in an Ecology department and how we would work the priorities on that. And I had a start-up package from Stanford, but it was pretty limited. It was going to cover a secretary for a few years, some office expenses for a few years, and that was pretty close to it. There wasn't really any travel money.

Chervin: And also was it clear in your mind at that time which were the appropriate federal agencies to approach, or would it be better to go the foundation route?

Schneider: Yes, and I had become a rather controversial character. The continuous lies about the double ethical bind, the fact that I was getting increasingly interested in the policy implications of climate and interdisciplinary connections in running the *Climatic Change* journal, which by . . .

END OF TAPE 9, SIDE 2

TAPE 10, SIDE 1

Chervin: Okay, it's now it is 12:01 a.m. on Sunday the 13th of January, 2002.

Schneider: So says the clock.

Chervin: The fourth day we've been at this, and . . .

Schneider: Exhaustion is setting in.

Chervin: Exhaustion is beginning to set in. I have a plane to catch in about fourteen hours, so that's a boundary to this.

Schneider: Yeah, I have a hospital procedure in about twenty-four.

Chervin: So—all right, you were talking about issues of funding a research program.

Schneider: Right. And I said that *Climatic Change* had just become the journal of record for interdisciplinary climatology. And that was something of which I was very proud and working, you know, about a quarter time job editing it because the journal was expanding so fast [that] it took a lot of time and energy to do it. All of those things mitigated against the capacity to get federal grants, plus I had had the style at NCAR, going back to the first relatively unpleasant conversation I had with Francis Bretherton warning me that if I don't do my own work and I keep having students do it for me, you know—this isn't the style of the research scientist that we want right now. Even though Francis changed his views of that shortly thereafter, I did not have with me at Stanford the very people in the sections I had at NCAR where I could spill out ideas, write grants, and have the folks who could do the work. I wasn't going to sit there in front of the terminals and code the codes anymore, that wasn't what I did. So I knew it was going to be difficult to go back to the tradition, plus I had grants with EPA that I could have taken with me. But what would Linda Mearns have done for her work, and what would we have done with other members of the group? So I wasn't going to take the grants with me, I left them behind.

So those were concerns that I had going in. I just felt that I would find a way around them—and you suggested it in your question, Bob, when you said, "What about private foundations?" I knew that while there was a cost to me for becoming increasingly public and controversial, making it tougher and tougher to get federal grants, it would become easier to get some foundation grants because they liked people with that kind of profile. Indeed that did happen eventually. But I didn't have them at the moment, and I was sort of asSuoming I would hit the ground slowly at Stanford, and work my up towards such grants over a period of a few years. And I would just operate without the large staff that I was used to having for a few years and see how it would go. Well the work that I was doing with Terry Root progressed very well, and when the work transformed, because the more we worked together, particularly when I was in the divorce proceedings, the more our relationship transformed from one of professional colleagues to a personal relationship. And then on top of everything, I now found us directly involved in a personal relationship, and now a commuting relationship to Michigan, just as in the early part of 1992, just at the time that I was about to move further away to Stanford. So that was even more daunting. Anyhow, that was the situation that I faced in the spring of 1992, as I was packing up and getting ready for the move.

I was called by a producer at CBS News, who actually met me at the Second World Climate Conference, and told me that he was writing a screenplay for a made-for-TV movie which he was calling *The Fire Next Time*, which was a global warming dramatization for CBS. And he asked me a whole bunch of questions, and I gave him lots and lots of information. He called me back and he said, we actually would like to use you as an advisor. And I told him that his script was crazy and wild—that he could maintain its dramatic impact and at the same time not make a million mistakes with the science. So he sent me the script, and I did a lot of rewriting and then became the science advisor to the program. He invited me to go to Louisiana, where the shoot was taking place, in early June of 1992 to Morgan City.

And the reason for Morgan City is it was going to be hit by a super hurricane. In fact, that was something I suggested to him that was credible. I said the super hurricane could hit tomorrow. I said what climate change will do is soup it up, make the storm surge a little higher and make it a little bit stronger. So they filmed it in Morgan City. They sandbagged the streets and then flooded the streets with an artificial flood, you know, to represent the hurricane. It was actually tremendously exciting going through the filming process. They even did something silly. They put me on as myself, twenty-five years in the future or fifty years in the future, saying that because we missed the opportunity after the real conference to do something about climate change, we now have to live with it—something that critics of mine have later really called me names for doing. But I thought that was kind of cute.

Anyhow, while I was there my office told me that I had had several phone calls from Kenneth Hope. I've known Kenneth Hope for many years. He's the program officer at the MacArthur Foundation who generally was involved in the selection process for the MacArthur Grant Fellows, the so-called genius awards, where people get a couple of hundred thousand dollars just because they exist. And I had written letters of recommendation for dozens of people over the years, and Ken has called me on various occasions to discuss the letters, and compare various people, and this sort of thing. So I was told by Mary that he was, y'know, pretty anxious to talk to me because he had some decision to make right away—could I call him? So somewhere around the 12th take at the set for my pretending to be interviewed (and by the way, they brought in the actual weatherman from Baton Rouge to interview me as if I was me) I said oh, okay I better go call Ken Hope.

I said, "Hi Ken, what's up?"

And he said, "Where are you?"

And I said, "I am in fact doing my science advice thing at this set in Louisiana."

And he said, "Are you sitting down?"

I said, "No, I am standing up. Why, what are you calling about, what do you need to know?"

He said, "This time I am calling about you."

Now that wasn't bad news. And I said, "Wow that's really nice, I am really glad to hear that."

And he said, "Yeah, I kind of figured you would."

I said, "But Ken, over the last five years I have been telling you that people like me shouldn't get these awards. We are well funded with two million dollars in federal money. I had a good salary at NCAR. I had all those things," I said, "and therefore the money wouldn't help very much. But I have got to tell you, a couple of months ago I agreed to go to Stanford and I was not, I was clueless how I was going to fund myself, so this has really come along at a good time."

He said, "Yes, we know that and that's why you got it now."

So they had a pretty good spy network, and indeed my question as to how I was going to survive at Stanford was answered. Well that night the producer and Terry and I went into New Orleans and went to Commander's Palace, and we had quite a celebration, which I agreed to pay for over the MacArthur. In any case, it proved to be a godsend because I funded graduate students with it, I funded my travel with it, and I ended up funding student assistance and other things over the period of five years that I had it. Also [I] met lots of people in the MacArthur Fellows who really proved to be wonderful colleagues and giving me good ideas. So that was the biggest positive I had had in quite some time.

Anyhow, back to what to do. [I] got to Stanford, and had a half time appointment in the Department of Biology and the Ecology Evolution Group. I had a half time appointment in the Institute for International Studies, which had an environmental forum, and was the place where research was to be done and where the Senior Honors program was to be taught. And I was asked by the Civil Engineering Department, by Bob Street and Jeff Koseff, if I would be willing to consider being a courtesy professor in Civil Engineering, so that if I taught a climate theory and modeling course, I could cross-list it in CE, since they had a very well respected Environmental Fluid Mechanics program. I agreed to do all of that, and spent most of the first year teaching the undergraduate Senior Honors. That's where I put my energy in year one. [The] second year I taught a class on climate theory modeling and applications. I also continued to do about twenty lectures a year that were single stand lectures in courses ranging from running a mock press conference in the Journalism School to teaching in Economics, Environmental Economics classes, about how modeling works in climate and comparing that to economic models; teaching a number of Ecology classes, several Engineering, Geology, Geophysics, and especially Earth Systems. So it was literally like doing a course that wasn't my course. Although of course it didn't take as much preparation to do that, because a lot of the lectures had similar content. And again, I was one of the few globalists on campus, and it was my job to present a global perspective.

I remember giving a lecture in the Earth Systems introductory class. And I was showing on the blackboard how irrational it was that people were buying fifty-cent light bulbs that were a hundred watts, when they could have bought *ten* dollar light bulbs that were twenty watts for the same power. And I asked the students, would you do that? They said of course not, because they are in a dormitory and Stanford pays the electricity. This is what we call a split incentive—you don't have an incentive to save the money. And then I went and showed the arithmetic of how, if you ran the bulb ten hours a day at ten cents per kilowatt hour, you actually would save twenty dollars a year in energy, and you would save half a ton of carbon dioxide, and some hundreds of pounds of nitrogen and sulfur oxides, and so forth.

So I remember doing this while being illuminated by incandescent light bulb light, and mentioning it to Terry over lunch, and she said, "Why complain, why don't you ask the students to go around Stanford and see where the inefficiencies are?" So I called for volunteers and got six or eight students, and sent them out against the University—and did they find inefficiency. They found it in the Student Union; they found it in the dormitories. And we wrote up reports, and I demanded that they be rigorous—that they do not just complain, but that they actually do the engineering calculations of how much it would cost to change the energy systems; how much would be saved; and how many tons of CO_2; when it would become cost beneficial. Supposing it were cost beneficial with a given carbon tax, and so forth. And that then led to teaching a class on energy efficiency in the real world, which I did the following year in Earth Systems and in Civil Engineering.

And I found those very rewarding, and that was exactly what I had been missing the past four or five years in NCAR, which is that broad, y'know, exciting new set of things to do with students. I certainly thought the Interdisciplinary Climate Systems Section was doing the kind of work that I liked. What was missing is I didn't have any workers, I had students. And it's very hard to have them run differential equations for you. So I had learned that there was a difference. It was essentially students versus workers. And I had the advantage of one in one place, and the other in the other. And they weren't different; they weren't better. I'm sorry, they weren't better; they were different.

However, I had an experience soon that really proved to be exciting. One of, in fact two of my students who were involved in the energy efficiency project became advisees of mine for their Senior Honors thesis in the Institute for International Studies, which is called the Goldman Honors course (the course I taught with Don Kennedy and Larry Goulder and Roz Naylor). And I had never seen undergraduates this good. They were as good as a lot of masters' and better graduate students that I had. And one student, Chris Yang, who is now at Princeton, and another, Eric Sellman, who eventually went on to Harvard Law School and got a law degree.

We actually wrote papers in journals, which was the senior honors projects of these students. Now the writing was way too turgid, and not at the professional level. And I used the money I had from MacArthur to hire them to stay over the summer and into the fall. Both of them were getting co-terminal masters, so they were still around the year after their senior year. And I did a lot of rewriting to get those papers out. But I soon found out that I was going to be writing my papers with undergraduates, rather than with graduate students. Because it was difficult to get graduate students who would apply to work with me in a Biology department. I was on the committee of many many of the Biology graduate students, but I was again the consultant on global issues and climate issues and modeling issues, rather than the prime person who would be publishing papers with them. I was able to publish papers with Terry on connecting in climate and ecology across the scales. And I began a collaboration that was very valuable for me with the economist Larry Goulder.

Now that came about in an interesting way. In the 1980's I spent a great deal of time debating, and enjoying my debates with a very brilliant economist, Bill Nordhaus. Bill put together an economic model in which he weighed the costs to the economy of climate change, which is so-called "climate damage" against costs to the economy of fixing the climate change via imposing a carbon tax. And then he tried to balance those in a so-called "optimization." Well in order to do that, he had to know how much the climate would change, given an amount of CO_2 emissions. So he invented a very simple carbon cycle model. And he called me and asked me, "What would be a good climate model?"

And I said, "Well, your economic model is globally average, so let me give you a globally average climate model."

And I gave him the two-box, the upper and deep ocean model of Schneider and Thompson 1981, which was our paper that first argued that the CO_2 problem required a transient, rather than an equilibrium calculation. Bill was able to simply code these differential equations, and coupled them into his model, and came up with the first sort of simple integrated assessment model that coupled the climate model and a carbon cycle model and an economics model. The problem was that he assumed that the damages to the earth's economy from climate change would be minimal. He reasoned that the 1988 heat waves cut out one-third of the agricultural productivity in the U.S. And if that was going to be a typical global warming year in the future, that he should figure out what the costs to the economy were. But agriculture is only 3% of the U.S. economy, he reasoned, and it cut out a third of it, so let's just assume the damages are 1%.

So immediately I started fighting with Bill: Well what about nature, and what about coastlines, and what about the boat people that would be created in Bangladesh which might destabilize Asia?

"Oh that's all speculative, and we don't know what that is."

And I was unable to convince Bill to change away from his 1%. I think he upped it to 1.25%. Anyhow, we were both on the Social Science Research Council's committee on environment. And he presented the results of DICE, that's his model, the Dynamic Integrated Climate Economy model, to the committee a little bit before the model appeared in *Science Magazine* shortly thereafter. And in it he went over the recommendation of his model, which is a minimal carbon tax of five dollars a ton, rising to maybe twenty-five or fifty dollars a ton at the end of the twenty-first century. And this was only going to offset a few percent of the global warming. In other words, the optimal solution of this economic model was to essentially do nothing. Obviously, I didn't like it and we had long debates.

He also ran a case with what he called "the Draconian 20% cut"—precisely that which in 1988 the World Climate Conference, that is the NGO World Climate Conference in Toronto, had called for governments to do. And he claimed that it would cost trillions of dollars to do this, and it was not optimal and too expensive. Finally he showed a picture. And this picture was of the growth rate of the world economy over the hundred-year period from 1990 to 2100. And it was about a 450% growth rate in personal wealth. And what absolutely stunned me is when he plotted on the same graph what the growth rate in the world economy would be in the model for the Draconian 20% cut, which actually reduced global warming by about 40%, whereas his optimal model reduced it by something like 4%. It turned out that instead of getting a 450% increase in personal wealth in 2090, as he got in the optimal run, it was delayed to 2100.

And I said, "You mean to say the insurance premium for cutting 40% of global warming away is to delay getting 450% richer by only ten years? Don't you think that's pretty cheap?"

And he said, "Oh but it's not economically efficient."

And I said, "Sounds like religion, Bill."

So we had this argument. And I remember Tom Shelling, who was also on the committee in the room said, "You know, Bill, if you drew those two graphs with a thick pen, you wouldn't even be able to tell the difference between the two of them!"

So then the argument became whether it was in fact a cheap insurance policy. Well when DICE came out a couple of months later, in *Science*, that graph was not present. So I wrote a letter to *Science* and said to the readers: Let's go to the graph that was in Bill's longer pre-print, and let's discuss the fact that, while he is advocating a small carbon tax, it makes very little difference. The larger tax, which he quite responsibly ran in his model, showed very little economic impact. The statements being made by the fossil fuel industry and Sununu and others that this would bankrupt the country were utter nonsense. There was almost an indistinguishable growth profile, with or without the large carbon tax that could make a big difference in global warming.

Nordhaus wrote back saying essentially the same thing, which is: Yes, but efficiency is important. There are all kinds of other projects we could do which could make the same claim.

Back to Stanford—there are a number of economists at Stanford connected with the so-called Energy Modeling Forum or EMF—John Wyatt, Alan Mann, and then some non-Stanford economists such as Rich Richels from Epry and Steven Peck from Epry, as well as other Stanford economists who were not directly involved in EMF but who are interested in environmental economics. One was Larry Goulder, and one was the Nobel Laureate Ken Arrow, a brilliant man about seventy at the time, but with all his faculties and articulateness. Every month they got together (and it also included a Business School professor and someone from the Hoover Institution) to discuss various environmental economic issues. And they called it the NFL, which stood for "No Free Lunch." Everybody had to pay for their own. After all, they were economists, so there was going to be no free lunch. And I attended one or two of these. And then one of the economists, I guess it was Alan Mann, saw my letter in *Science* and he said, "Okay Steve, what are you going to say about us to complain? Let's have it out here in private before we have it out in public."

And I said, "Okay, I have a problem. I don't understand something. When you people run your economic models, what are you asSuoming for technology?"

Remember, I was trained in Engineering, and I also had one course in Economics where I remember being told that if you are in an industry with multiple competitors, if the price of one of their products goes up, then the other competitors will invest more money in research, development, advertising and whatever, because they'll see an opportunity for a better market share.

And I said, "So how do you account in your model for the fact that if the price of fossil fuel went up because of a carbon tax or some other policy, that people would invest more heavily in alternatives, like solar or wind or fuel cells, or even nuclear?"

And Alan Mann said, "Well, we deal with that with a special parameter which we call AEEI."

I said, "Well what is AEEI again?"

And they said, "Well it stands for Autonomous Energy Efficiency Improvement."

I said, "What do you mean [by] 'autonomous'?"

"Well, that means that over time people make more discoveries as they learn more."

I said, "Did these discoveries occur in the shower, or did they occur because of investment?"

"Well, they occurred because of the investment."

I said, "Well what is the feedback that you use in the model, which determines the rate of investment as a function of the price of conventional, that is carbon energy?"

Everybody around the table smiled. I said, "What's so funny?"

And Alan said, "Well, there is no empirical data on that."

I said, "You don't need data. You have to think like a physicist."

They said, "What do you mean?"

I said, "If we know that two processes are functionally related, but we don't know the function, we make one up!"

And they all looked at me in horror, these empirically driven economists, and I said, "For example, we write down a diffusion equation. We don't know what the diffusion coefficient is, so we write a high one, a medium one, and a low one. We run it in the model; then we go out and do experiments. We gather data, and we find out oh we were wrong, it really isn't diffusion—it's really double diffusion. And then we do some more experiments, and we find out no, it wasn't really double diffusion; it was invection diffusion—and then we start to converge on a much better model. But you know, twenty years later when we finally learn that, we discover in the end it is still better if we had picked an average diffusion coefficient, rather than nothing!"

And they just said, "Well, you know we are empirically driven."

I said, "But you just admitted that these processes are real. Why can't we make up a reasonable function, use some empiricism to bound it, and then do sensitivity analysis?"

So Larry Goulder came over to me afterward. He said, "Let's do it, I've got a model. Let's work together, and let's do it."

So for the next several years we came up with what we called an ITC model, an Induced Technical Change model, which was really nothing more than the kind of work I did with Galchen or with Starly Thompson years earlier. It was putting in first order feedbacks that we knew intellectually had to be there, that were difficult to define empirically, but that we could do sensitivity analyses with. So Larry and I put this into his general equilibrium model. Now I didn't know what general equilibrium was at the time—I had actually read about it, but I didn't deeply understand it. So we would have monthly lunches for the next several years, where I would explain how climate worked, and he would explain how economics work. And then I was invited by Alan Mann and Rich Richels and John Wyatt, who was the head of the Energy Modeling

Forum, to go to workshops in snow mass and they asked me to be the house critic. I was to listen to the integrated assessment model talks, and then I was supposed to critique them. And I critiqued them for missing induced technical change. And I critiqued them for using very high discount rates that were higher than historical growth rates in capital. And I would critique them because the only things that they considered in climate damage were things traded in markets. They left out altogether what would happen to nature. They left out combat that might be induced by boat people. And slowly over time, they learned from me, and I learned from them.

This multidisciplinary community then began to invite more ecologists, more climate modeling people. Mike Schlesinger became a regular; Tom Wigley was invited; Steve Pacala from Princeton was a regular in ecology; Terry Root was invited. And slowly the disconnection between these people who didn't know each other's traditions, over a period of five to ten years, was changing into an interdisciplinary community. I remember complaining that we can't possibly know what the damage is, because we don't know what the climate change is; we don't know enough about regional distributions; we can't pick one damage function and optimize, which you economists love to do. We need a distribution of damage functions.

Well Alan Mann and Rich Richels said next year they would come in and they would run a distribution. Of course I complained that they picked too low a number. And we've continued these kinds of debates, and I found that to be just super-stimulating. It reminded me of the early days of the Climate Project when we would have two week summer workshops at NCAR, bringing in people from all around the country and the world to think together about how to proceed and how to develop the hierarchy. We were beginning to do this for the field of integrated assessment. About three or four years later, I guess this was by the mid-1990's, John Wyatt said, "Okay, Steve, it is time for you to start writing all this up."

And Jan Raatmits, a Dutch integrated assessment modeler, said, "I am going to be involved in editing a new journal called *Environmental Modeling and Assessment* and I would like you to write a survey article for integrated assessment." Well, it is one thing to go a summer meeting and give off the cuff, think-on-your-feet critiques. It is another to write up a meaningful survey. So I remembered what it took to do the survey with Bob Dickinson. It took years of reading papers. Fortunately, at the time there weren't that many climate modeling papers to read, but there certainly were a lot of papers in meteorology, oceanography, hydrology, and so forth to read. Well there again, fortunately, were not a lot of integrated assessment models to read—there maybe were a few dozen. And there was a large related literature that began with the Club of Rome world models and the international future simulation model which I still remembered a great deal about, because of Diana Liverman's thesis, and I knew that tradition. And I spent about a year reading that literature, arguing with people, going to meetings. And by 1996 had prepared a fairly long draft, in which what I defined was a hierarchy of integrated assessment models, beginning with what I called "pre-methodological models," in which I included those from, for example, Lee Bryson or Mickey Glantz, which were based on case studies. And they were how it began. Although the one that Bob Chan and I did, where we looked at the coastlines and coastal flooding, where we redrew the maps of the U.S. with storm surges from fifteen to twenty-five feet. But those did not include economic feedbacks.

Then the next class of model had elementary economic feedbacks. Then the next class of model had induced technical change. It had statistical distributions of uncertainty included, subjective estimates of the probability of redistribution of equity—that is, when the climate changes it could improve the agriculture in cold northern countries and reduce it in hot southern ones. And even though that may not in the net sense lead to a net change in planetary income, it would be a redistribution of winners and losers, with the rich getting richer and the poor getting poorer. There is a cost associated with that. That cost had to be accounted for in models it had not heretofore been accounted for.

And so I was classifying the hierarchy, and then in the end we had to include changes in variability and outright surprises. The more rapidly a nonlinear system is forced, the more likelihood to have surprises. That is the very last paragraph of the IPCC Working Group One 1995 study. It was fashioned in Madrid. I wrote it at 2:00 in the morning—when a delegate from France said, "There's nothing on surprises in this report."

And I said, "That's not true, the chapter on advancing and understanding, chapter 11 (which I was a co-author of, a lead author of) had a paragraph that made this point."

He said, "Well, if its in there, it belongs in the summary for policy makers."

And we wrote it, and actually as contentious as that meeting was, we were able to avoid contention on that issue. So I had the IPCC to quote that "Nonlinear systems when rapidly forced are more subject to unexpected behavior," was roughly the IPCC language that was approved by the delegates. And I still believe to this day that's correct. And it's one of the reasons why I believe personally in slowing down this experiment on what—my 1997 book's title was called *Laboratory Earth* and the experiment I don't like to perform, that Roger Revelle in 1957 called when he said we were performing a great geophysical experiment by dumping CO_2 in the air. And he didn't deal so much with the impacts of that experiment. I was much more interested in the impacts, and whether we should be risk averse in slowing them down. So I then prepared a survey article on integrated assessment models. Sent it out just exactly as I had with the Schneider and Dickinson climate modeling paper to about thirty people. Got about twenty sets of comments back, which then took me another six months to rewrite, because they caught all kinds of things, because this involved many disciplines, a lot of economics.

And [I] slowly evolved a survey which, you know, has now become sort of a standard definer of the field. I was very proud of that, but in a way I did not go into it naively, because I knew from the work that Bob Dickinson and I had done earlier what it was going to take to do that. And I knew it was going to take a lot of reading on my part. But I also knew that no matter how much I did, I could not make myself an expert of the seven disciplines needed to do it. But I was going to have to rely on those forty people out there to keep me multidisciplinary accurate in the phrase of climatic change review policy. And I think that was probably the single most important part of my intellectual growth in my Stanford years—was learning how economic models worked and learning how they can be integrated with physical and biological models to do integrated assessment.

And I owe a lot to my Stanford colleagues for their open-mindedness and willingness to entertain me as a non-specialist house critic who would have a fresh point of view to give them a hard time. And it was a role I relished, and one they appreciated and they were willing to respond and change the nature of their modeling efforts on the basis of that kind of criticism. Now of course I was wrong at least as many times as I was right in criticism. And they would laugh at me and correct me, and so forth. But it was really a wonderful collaborative effort that was very reminiscent of the early days at NCAR when it an intellectual center for the development of climate theory and modeling, not so much a place that was interested in just simply devising tools.

While I was working on an integrated assessment paper, a very brilliant undergraduate came into our senior honors program, Tim Roughgarden the son actually of Jon Roughgarden, my ecologist colleague. Tim was a computer scientist very well trained in mathematics. And he said, "You know, I've never really run an actual model on a practical problem—do you have any ideas?"

And I said: Well yeah, I've been fighting with Bill Nordhaus for years about his, in my opinion, underestimate of climate damages. And Bill, being a brilliant economist, had an answer. In 1993 he brought in twenty economists and other scientists who were mostly involved with the 1991 National Research Council's study called *The Policy Implications of the Greenhouse Effect*, I was one of them—mostly conventional economists, but there were a few ecological economists and three natural scientists, Gordon Orians, an ecologist, and Bob White. And he asked us to fill out a sample survey where we would estimate a probability density function for what the damages to the world economy would be from climate change of two amounts. He actually had three scenarios. He had 3° warming in a century; 6° warming in a century to represent a very radical case, and 6° warming in two centuries. So let's just concentrate on the three and the six degree warming in a century. He said give him your 10 percentile, your 50, and your 90 percentile estimates. He recognized completely that nobody could come up with the answer, that you had a range of possibilities.

I thought that was an excellent set of questions that was in the tradition of decision analysis, which is a discipline that tries to elicit from people the best understanding they have in the most consistent way; which is a discipline that deals with industrial engineering, cognitive psychology, and statistics—particularly Basian, that is contingent statistics. So Bill did the survey, and he sent it around to us for comment, and a spectacular result concluded. He found that the conventional economists had estimated damages that were an order of magnitude less than the natural scientists. Their damage numbers are on the order of a few percent of GDP; and the others were on the order of ten. Even the 6° warming in a century, the temperature difference between an ice age and an interglacial [age], occurring in a century, not thousands of years, did not motivate these economists to see more than a few percent loss in GDP.

Bill said, “Well you see?!” he said to me quipping. “The people who know the most about the economy aren’t so worried.”

I said, ”Gee Bill, the people who know the most about nature are!”

The paradigm gauntlet had been thrown. So why was this going on? So I mentioned this to Tim Roughgarden. And I said, “It would be really fun to take Nordhaus’s survey, fit a statistical distribution to it, and instead of producing a single optimum carbon tax profile over time, why don’t we produce a probability density function of optima that each depends upon which particular value of climate damages occurs from the distribution that we get from the Nordhaus surveyed experts.”

So the first thing that Tim did was he followed up on one more question that Nordhaus answered. And make no mistake about it, I love to argue with Bill Nordhaus, but he is a brilliant inventor of new techniques. And he does ask the right questions—even if he answers them in my opinion wrong, he asks the right ones.

And he said to us, ”Tell me, in the end what fraction of your climate damage estimate comes from the SNA (that’s the Standard National Accounts—in other words, things traded in markets like the yields of grain which you can translate into dollars, or timber. What you can’t translate into dollars is species lost, conflicts, heritage sites lost when small island states are flooded. Well this is a personal value judgment on our parts about relative value, and it’s also a guess as to what people a hundred years from now are going to care about. How we can’t possibly know that in a definitive way. So when Tim Roughgarden plotted up the damages, absolute damages, as a function of the percent and SNA a spectacular thing occurred. Those people who had very small damages generally thought the bulk of them would occur in things traded in markets and the Standard National Accounts. Those people who had large damages, like the ecologists, thought the bulk of the damages would occur in things not traded in markets like ecosystems. So there was actually less difference between the ecologists and the economists than we thought. The difference was the belief of the economist that you could substitute for the services of nature relatively inexpensively by human invention; and the ecologists were basically thinking, no you can't, these are not simply substitutable, and things not traded in the markets will be real damage. Now obviously this is an empirical question that will be answered over the next hundred years. It will not be answered before the fact, however.

So what Tim and I then did is we took a look at these results and we found out [that] there was one thing that the economists and the ecologists had in common. They all were right-skewed. By that I mean [that] they felt there was a higher probability of nasty surprises than nice surprises; while the economists felt that there was a substantial chance, 10-20% chance that there would be benefits from climate change—CO_2 fertilization, longer growing season. They also felt that their 90 percentile guess was much more damage than their 10 percentile guess was benefit. The ecologists, while their 10 percentile guess was generally not benefit, their 90 percentile guess was substantial damage. One of them actually had as his 90 percentile guess as a hundred percent loss to the economy—in other words, the end of the world. And his rationalization was associated with war and conflict generated by environmental resources. I was radical but not that radical. My 90 percentile guess for the 6° warming was a 30% loss in the economy. Nordhaus said, "Well that would get my attention, that was a world depression." Although he didn't believe that any such thing would happen.

In any case, we plotted up this distribution. And we could not of course get a normal distribution because it was right-skewed. So we fit a _____ distribution. We then did a Monte Carlo simulation using the DICE model, where instead of plotting out a single optimal trajectory for carbon taxes, beginning at about $5 a ton and going up to $40 that Nordhaus recommended, which had almost no offset to climate change, we sampled from the probability distribution that was represented by the damage functions from his survey participants. And we ended up with probability distributions for optimal carbon taxes. Not only was there not a single optimal tax, but there was about a 10% chance that the optimal tax should be something like minus five or ten dollars. That is, we should be subsidizing the coal industry, just exactly as they argued. We also had a 10% chance that the tax should be $150, which would be the end of the coal industry. Optimal—because that would occur if there were very severe climate damages. It would actually be economically optimal to spend a large amount of money preventing them, because they would be so harmful to the economy.

In order to know what those actual damages are, you have to know how to value lost species; how to value inequity generated by rich getting richer and poor getting poorer; how to value loss of heritage sites; how to value loss of life. None of these things were included adequately in economic models. So Tim Roughgarden's thesis, in my opinion, was a spectacular piece of work for an undergraduate, better than Ph.D. theses I've seen in many places. He hung around to do his co-terminal Master's. Again I used my MacArthur money, plus a grant now I had had from a private foundation, from the Winslow Foundation to hire Tim. And we wrote it up, and it was accepted in the journal *Energy Policy.* And it's had quite an influence in a lot of the thinking in integrated assessment. Because what we did is we fought with a conventional economic paradigm which says "let's produce an optimum." We said: No, an optimum has no meaning because you can't define the natural science, the physical and biological sciences, except by probability distributions of the kind Nordhaus elicited. And therefore lets do probability distributions for outcomes, and transform the debate from one of optimization to one of risk management. And that's been the emphasis that I've tried to push on IPCC. In 1994 when I was a lead author on IPCC, Working Group 1, I went to Sid Tuna for the first meeting in Sweden. And I got up and I mentioned exactly this case study.

And I said, "In order to do this problem right, we shouldn't just have statistical distributions for the damage functions. We should have statistical distributions for climate sensitivity. We should have statistical distributions based on the uncertain parameters on the carbon cycle model. And we should have statistical distributions for how humans will behave over the next hundred years to emit carbon dioxide. And we should end up with joint probability distributions and put them all together and that's the final answer that we should be giving to the political world."

Well that was viewed as radical to almost crazy by most of my colleagues, who would say, "How could we come up with probability distributions in the future when we have enough trouble understanding the present?" And the argument that I kept using over and over and over again is: If we don't assign probabilities to climate sensitivity and to other things that we have expertise in, then the economists who will build models based upon the ranges that we give will have to *guess* what they think we thought the probability distributions are, because their models demand probability distributions in order to calculate these distribution of optima. So I can't see how it's more rational to have them guess what they think we think the distributions are, than to have us do a job of being honest admitting the confidence levels that we have. I failed. I was not able to convince my colleagues to do that.

Turns out that Working Group 2 was run by Bob Watson and his chief assistant, the head of the so-called TSU, Technical Support Unit, was Richard Moss. Richard was trying to do the same thing for Working Group 2 that I was trying to do for Working Group 1. One day I met Richard and we both discovered that we were working on the same problem. And it was obvious—I said, "Richard, we need to join forces. There will be a third assessment coming up in 1998 and we have got to confront our colleagues with the fact that they cannot continue to duck the question of adding subjective statistics. Because if they don't, then either the economists will do it for us, or even worse the politicians . . ."

END OF TAPE 10, SIDE 1

TAPE 10, SIDE 2

Schneider: Even worse than the economists, the politicians will do it for all of us. The political leaders *want* us to tell them what can happen, and what are the odds. If we don't tell them, they have to guess. Richard and I decided that we should convene a group of lead authors from all three working groups, and address the question of how uncertainties were treated in IPCC's second assessment report, the SAR. I suggested that we go to John Katzenberger of the Aspen Global Change Institute and have a two week summer meeting on exactly that topic. I suggested that it be the week or the week and a half after the Energy Modeling Forum, because that way we could capture a number of the scientists that were brought over from Europe and other places by EMF, particularly the economists. And that way we could get them to do it at cheaper airfares. Katzenberger liked the idea, was able to convince the U.S. Global Change Research Program that was helping to fund him to do that.

And in the summer of 1996 we held a session on uncertainty. We also invited Granger Morgan and Elizabeth Pathe-Cornell from Stanford who were decision analysts, formally involved in how to make decision-analytic protocols and deal with eliciting information from expert groups in a consistent way. Granger Morgan hammered over and over and over and over again that the worst thing anyone writing an assessment report can do is use terms like 'likely,' 'confident' and so forth without linking it to a quantitative scale. He said, "Every human has a different probability in mind when they use those words, and therefore we will be talking all past each other." Richard and I found a hundred examples in the IPCC reports where exactly that perversion had occurred. Granger knew what he was talking about.

So what we evolved was quantitative scales, where we were trying to define terms like 'likely,' 'unlikely,' 'confidence,' 'medium confidence,' 'low confidence,' and so forth. We wrote the report in the AGCI, sent it to Bob Watson, who then invited us to come and talk to some IPCC groups. It was controversial, we said that this cross-cuts all three working groups, and it was a cross-cutting theme. It turned out that Bob and the co-chairs of IPCC, people such as Tommy Tamaguchi from Japan and Pachi Pachori, the head of ____ Institute from India, and others who were involved thought it was necessary to have multiple cross-cutting themes. One would be on uncertainties; one would be on sustainability and equity, something that the Third World countries were demanding. They wanted no longer to have neoclassical economics, its cost benefit calculus where a dollar lost in the Third World was equivalent to a dollar gained in the rich countries. They wanted distribution and equity to be an explicit variable. Another cross-cutting theme—they wanted how to cost to be a cross-cutting theme, because after all, you don't just cost things straight into markets, but you have to deal with lives lost and heritage sites and equity.

And also [there is] the fact that there are more than one decision analytic framework in economics—that is, cost benefit analysis is not the only thing. There's 'Do no harm.' There's precautionary principles, there are other such principles, that could also apply and that have different methods for making policy suggestions.

So these were called guidance papers, and four of them were prepared, and Moss and I were asked to write up one on uncertainties. We worked about a year and a half on this draft. And there were three rounds of reviews by e-mail of lead authors from Working Groups One, Two, and Three. And over time it converged to, by about 1998 to 1999, right when IPCC was meeting, that we sent out the drafts to the Working Groups. They suggested quantitative scale—that we would define low confidence as a less than one-in-three chance; medium confidence, one-in-three to two-in-three; high confidence, above two-thirds; very high confidence, above 95%; and very low confidence, below 5%, for example. It took a long time to negotiate those numbers and those words. There were some people who still felt that they could not apply quantitative scale to issues that were too speculative. So we had a qualitative four box scale, where we used phrases such as "well established" if there was a lot of data and a lot of agreement between theory and data. We used words such as "speculative" when there wasn't much data and there wasn't much agreement. We had "established but incomplete" and other categories.

And then for the next two years Richard and I became what were called "the uncertainty police." I probably read three thousand pages of draft material in various working groups, where people were using uncertainty terms not in accordance with the guidance paper. They would do something like write a sentence where they would say that because of uncertainties, the range of outcomes could be anywhere from one to five degrees change. And then they would put parenthesis "medium confidence"—that's completely incorrect. It was very high confidence, because they were talking about the fact that between one and five degrees was a very very likely place in which the outcome would occur. But they didn't want to say "very high confidence" because nobody felt very confident about the state of the science. So I would help people to rewrite, and say that we have low confidence in specific forecasts, but we have high confidence that the range is likely to be one to five. Simple things like that—it took a long time to get the community used to be consistent in the use of confidence phrases that were linked to quantitative scales, and the kind of standard language that existed in assessment reports. Every single time phrases like "we cannot be definitive" occurred I would put big red letters: "What is the probability of a definitive? Strike this. It means nothing to say we can't be definitive, that's an irresponsible copout."

And again we had many hot, contentious go-arounds on that issue. Working Group One initially balked at the notion and then embraced it, but then said that they needed to have finer gradations, because they had real data, not just subjective data, and they wanted to have a 99% and a 1%. Of course they ended up never using it. But they defined it: “We in Working Group Two refused to do this on the grounds that most of our estimates involved a high degree of subjectivity and it would be outrageous to try to pin down subjectivity to a 1% or 99% level.” In fact, 5% and 95% were already finer than anybody felt they could legitimately justify. So this process went on. It emerged from the EMF discussions, from the work with, you know, debates with Bill Nordhaus, from working with the senior honors people, and from conversations with Larry Goulder and other economists. And in the end the guidance paper was very popular with the governments that attended the plenary sessions. And there is in each summary for policy makers an explicit description of the quantitative links, terms like “likely” or “confident” and that the report addressed these issues for the first time in a consistent and quantitative way.

However, before I declare victory I must say that (a) Working Group Three did not use the lexicon; and (b) the special report on the emissions scenarios, which produced six story lines that suggest what the future emissions might be, refused systematically to assign probabilities to their story lines. They called them all equally sound. Also, Working Group One, in citing eighteen general circulation models or six representative ones to give different climate sensitivities, refused to discuss whether these models were independent, or what the relative probabilities were. Therefore, when the IPCC in the end gave its now famous estimate that warming by 2100 would be between 1.4 and 5.8° C—the difference between 1.4 and 5.8 being the difference between relatively adaptable and probably catastrophic. But politicians of the world all said: Well what’s the likelihood of this? If it is going to be 5.8 we better do something about it right now. If we are going to get away with 1.5, we have got some time. IPCC never addressed the question, because they still would not assign probabilities to those outcomes.

So, my job and Richard’s job is not done. And the current battle lines are to try to get the next assessment to deal with the subjective probabilities, events a hundred years in the future. It is understandable the reluctance that the analysts have to do that. Imagine someone in the Victorian era being asked to assign probabilities to what the emissions were going to be right now. You might find it laughable. On the other hand, how is a political leader to know how much resources to invest in a problem if you don’t have some idea about the likelihood of the problem? So what I have called for is a double strategy, where you not only provide subjective probabilities, but you also provide the degree of confidence that you feel in those forecasts. And I am certain that our degree of confidence will be low on most of these forecasts, unless we have broad enough ranges that we can have higher confidence that it’s likely to be within that range. But I think that’s more responsible than ducking the question altogether, and leaving it to every polemicist out there who wants to grab the IPCC numbers out of context to support a radical view of the world that's going to have catastrophic or mild outcomes. And that's already happening, as people are grabbing these story lines from the report on the emission scenarios; grabbing models with either high or low sensitivity and claiming that those are the most likely outcomes in the future, because IPCC never constrained them by suggesting what the experts believe to be the relative likelihood of each of those either modeled sensitivities or emission scenarios. We've got plenty of work yet to do.

Chervin: As I understand it, IPCC is assembled every two-three years?

Schneider: Five.

Chervin: Every five years. And so the next assessment is supposed to appear in print in 2005 . . .

Schneider: 2006.

Chervin: 2006.

Schneider: Basically it will be a 2004-2005 process. However, it is not clear that will happen. Because the hundred or so governments in the world that care about this process meet in plenary and they redefine it. And they haven't met yet to decide what they want to do next. There has been some complaint that the IPCC reports, you know, a thousand pages long are too thick. And even though the last report was not supposed to repeat the information of the first report, it had to a considerable degree have to resummarize where we were, otherwise the report would not be self-contained. And you can't ask somebody to read the thousand page report now, and also go back and read a five hundred page report from 1995 that isn't state of the art of anymore. So there's been some frustration that these reports are repetitive, and it is hard to separate out the new information. And there may very well be a call to have the reports shorter and more on the most recent discoveries. But then that calls for a sophistication on the part of the reader to be aware of what happened before, or least a good introductory chapter that in a short way summarizes the previous reports. It might be possible to have your cake and eat it too.

The second problem is the reports have been largely disciplinary. Working Group One has been climate science, largely climate modeling and observations. Working Group Two has been climate impacts and adaptation—agronomy, ecology, hydrology, and to some extent the social science of adaptability and vulnerability to change. And Working Group Three has been largely economics and technology: What are the costs of developing alternatives to the fossil fuel world? And what are the implications of both having no change and letting people suffer through climate, or the implications of imposing policies which cost money, and how will they impact people as well?

What I have argued for is what occurred in a fourth report that was part of the TARR, namely The Synthesis Report, where we answered ten questions posed on us by government which primarily went across working groups. It asked what would be the differential consequences of stabilizing the atmosphere high, medium and low levels of CO_2. Well in order to address that you have to look at the physical, biological and social sciences. So The Synthesis Report began to do that. And my own personal argument is that the next round of IPCC should be organized around great questions, not in disciplinary clusters. I was told that it was a little too radical, a very familiar sound to me from what I've heard at NCAR in the 1980's about creating a global systems division. But I'm hopeful that a number of government representatives, especially from New Zealand and Australia and the U.K. and some European countries who don't think it's too radical will continue to push to change the emphasis in the next IPCC on asking synthetic questions at the outset, and then having the disciplines being brought in to help keep the answers to these synthetic questions honest.

In fact it's like the climatic change editorial policy. Let's address the broad questions the way they lie. They are integrated questions, but let's make sure that the disciplinary subcomponents are brought in at the state of the art, but that the originality is in the integration across those disciplines. I think that would make the next set of IPCC reports truly unique and more valuable to policy makers. And they have to deal with the question of subjective probabilities all at the same time. But whether that's too big a leap for this community remains to be seen, and the national governments are going to iron it out in the next few years as they decide what form they want the next set of reports to be in.

Chervin: And another possible issue is the one of possible burnout of the participants. I'm sure there's a substantial set of people who have been involved in all three reports.

Schneider: I should show you all the skin that sloughed off me from being burned out. Not only was I a coordinating lead author of chapter one of Working Group Two, and was I a lead author of chapter two of Working Group Two, and was I the co-author of the uncertainty guidance paper, but I had to read thousands of pages of three different drafts of Working Groups One, Two, and Three as the uncertainty cop trying to find, you know, and help deal with uncertainty language. And I don't think I could do it again. It was a half-time job for three years and it was *pro bono*.

Chervin: Right, so that's another issue that I've been thinking about of late. In this field, historically there's a lot of *pro bono* work. For example, reviewing papers for publications, serving on thesis committees, serving on advisory panels, serving on evaluations.

Schneider: Doing Congressional testimony, talking to the media, being on National Academy of Science committees.

Chervin: And on and on, and at best your travel expenses are covered.

Schneider: Yeah for travel in cattle car.

Chervin: So how long can this paradigm continue?

Schneider: Well, most scientists aren't truly motivated by money. We are motivated by community. We really enjoy interacting with each other. We enjoy having a reputation based upon the quality of our work and our performance. And I think those motivations are their own rewards. But, the economists aren't wrong, you need some incentives as well. And whether we should be paid I don't know, maybe we should have fewer such meetings—I mean those are all possibilities. At the moment the IPCC does not have trouble recruiting people. It's become such an exciting venture and an important venture that people are willing to undergo it.

But in the last round, not only were there three rounds of review, which was normal in the round before, but there was something new added to deal with the politics of those who accuse the IPCC of being a conspiracy of scientists who want to scare the world so they can get funded by governments. So what they did is they brought in review editors to make certain that the authors were not ignoring the comments of the industry or of the contrarians without just cause. And that meant that not only did we have to prepare revisions based upon fifty or a hundred review comments three times, but we had to prepare a twenty-five page document that detailed how we dealt with each comment. And the amount of effort was just odious. And it was necessary politically, I agree, to give the report credibility. It did not stop the detractors from still asserting that the report was a political cabal, but it gave them less credibility in their assertion, because of the review editors process. And I think they are going to need some fresh blood in next time. And it's one of the reasons why I was proposing a synthesis rug built around fundamental questions, rather than the usual disciplines. Because maybe that would bring in some new people, and the disciplinarians would be able to be brought in as consultants, rather than to have so much of their time leveraged writing the report where they are saying largely the same things they wrote in the previous report, just updating the literature. Maybe that would relieve some of the stress on the communities, I don't know.

I've said that the frustration at Stanford has been that it's a disciplinary institution. And therefore it has been very difficult to convince people to do integrative hiring. It's hard to hire somebody who is not looking like they are National Academy of Science profile scientists. But there are also some big pluses. One plus is the incredibly good crop of students, even undergraduates. And the second plus is [that] nobody questions what you do when you are in what I call the consenting adult population. You are a post tenure faculty member, and if I want to work with an economist, and I want to teach a class in Civil Engineering or in Earth Systems, nobody in Biology says what have you done for me lately. That is not as likely to happen in a state university—or at NCAR. And therefore, that has been one of the strengths of having come to Stanford. I have been able to learn a tremendous amount about ecology, economics, and integrated assessment because of that free flowing capacity across departments at schools for individual collaborators, just not for hiring young people who are integrative.

Another example of that is [that] I have long argued that one of the problems with traditional integrated assessment models is they neglected variability. What I learned in working with Rick Katz and Linda Mearns in 1984, which was actually driven by working with ecologists and agronomists, is that it matters more whether there is a change in extremes than a change in the mean. Of course, a change in the mean would be associated with a change in the structure of the system, which would change the extremes. And what about nonlinear systems unrapidly forced being particularly subject to unexpected behavior, so-called surprises? I remember when I was giving Congressional testimony in 1988, when Senator Bradley read precisely a statement from me and from Bob Watson where we were both talking about surprises. And he said, "Well, gentlemen, what kind of surprises do you have in mind?"

And I said, "Well sir, a surprise is something you don't know."

He said, "Aw, come on, you got ideas!"

And then of course we rattled off four or five things. And one of the things, of course, is a flip-flop in North Atlantic Ocean circulation, the thermohaline circulation. I was thinking, all the damage functions that Nordhaus uses are smooth, and all the damage functions used in integrated assessment models—they are typically quadratic to represent the fact that you get further away from the present and you get more damages. But they are still relatively smooth. And most of the damages occur far away, a hundred years away, when the quadratic starts to really take off. And those damages have very little present value because they are discounted by when you discount the world at 5% or 6% per year, the standard return on investment of money. And as a result of that, the models recommend very little climate policy for the simple reason that climate policy implemented now costs you immediately, and therefore is not discounted, whereas the benefit which has reduced damage fifty to a hundred years down the future, when the damage is nonlinear because it goes with the quadratic of the temperature difference, is discounted by factors of a hundred or more because it is so far away. What happens if it turns out that the world really has abrupt nonlinear change, and that you can't even know whether you are going to have it until you get close to the event? Therefore, no model run by economists with a perfect foresight into the future, for example, (And perfect foresight is an absurd assumption because we can't have it.) really can represent long term surprises.

So I decided it was time to modify the Nordhaus approach by adding abrupt nonlinearity. So I called up Starly Thompson, who was at this time no longer at NCAR (because as I had feared when I left the Interdisciplinary Climate Systems Section that it would lose its rather aggressive champion that I represented for the detractors in the institution) and said, I have an offer from the Pew Center on Climate Change, (again, a private essential foundation) to look at abrupt change. I said, let's take our 1981 climate model (which was a smooth model that's coupled to Nordhaus and many other integrated assessment models) and let's add an abrupt nonlinear change. Let's put in the Stonell equations for thermohaline circulation that Stephen Ramsdorf had recently shown could be used if you Toond the coefficients right to match the behavior of the general circulation models. Starly did that with a grant I had from Pew. And it was fun recollaborating with him after about ten years. In fact, he is now out at Livermore, working on a couple models again. And I then got another undergraduate, Mike Mastrendrea, to couple DICE to the SCD—that's what Starly called this model, the Simple Climate Demonstrator. We called it a demonstrator because we claimed no originality. We simply used the Stonell equations and then Toond the coefficients to get it to behave like a number of GCM's.

And when we coupled the two models together, we found out a very strange and interesting thing. We found out that if you had a typical DICE run, it produced a tripling of CO_2 by 2100 and maybe a quadrupling by 2200. That was sufficient to trigger the flip-flop in the ocean current. So we took the profile of carbon generated in DICE, fed it into the SCD climate model, and asked what happened to the thermohaline circulation. It would weaken and sometimes it would collapse.

So we said, let us assume that there is an enhanced damage, in addition to the damages associated with warming, the square of the temperature difference that Nordhaus used, which we called enhanced damage, that's proportional to the reduction and the strength of thermohaline circulation. We have no idea what that damage is. I mean if you collapsed the thermohaline circulation completely and sent Europe into the cold, that could certainly be a nasty situation. So we used values of 1%, 5%, 10%, and so forth. And when we put that in, we then let DICE recalculate the present value of climate damage with the enhanced damage. Now we decided since its nonlinear, let's add the enhanced damage not as an additive term but in the exponent of the damage function. So the damage function, instead of being temperature squared was temperature to the two plus *epsilon*, and *epsilon* then was the enhanced damage. And we Toond *epsilon* so that when the thermohaline circulation collapse occurred it would be equal to whatever our specified number was—5%, 10%, and so forth. It turned out that that caused a substantial increase in the amount of carbon taxes the model considered optimal in order to prevent this enhanced damage that could foresee, but it still had trouble preventing the collapse from occurring. Because the carbon taxes, even though they were larger, were not large enough to radically prevent the economy from relying on fossil fuels.

So we then experimented with another parameter in the model. We experimented with the discount rate. And we found out, very interestingly, that for a mid-range set of numbers, for example (The exact value of these numbers is not important, but it is the idea.), when the discount rate was less than about 2%, that meant that enhanced damage a hundred years from now was sufficiently valuable today that the model's optimal called for a very large carbon tax. And that very large carbon tax reduced the emissions sufficiently that we didn't cross the nonlinear threshold in the climate model that led to the collapse of the circulation. In other words, the policy worked! It actually prevented the collapse of the circulation, even in 2150, because the low discount rate meant that the future was foreseeable, and had some value today. When we increased the value of discount rate beyond about 2% (and most models run at between 5% and 6% or 7%), then even that unimaginable catastrophe of flipping off the Gulf Stream, being so far away had so little present value that the model's optimal is to let it happen.

Now the trouble is, what would happen fifty or eighty years from now if we got close to the event, and we realized that this is what we were creating, and we pulled a Gilda Radner on the old "Saturday Night Live" and said, "Never mind, it might be too late." It might be beyond the point that you could reverse the affect. Therefore, the next few generations will be making decisions that will affect the sustainability of fundamental environmental systems in the generations beyond that. And the point that we made is: No integrated assessment models to date are dealing with abrupt nonlinearities. And we showed that it can make a very big difference in what the model recommends for optimal taxes. And therefore we said the entire literature has to be reconsidered, based upon a range of these kind of imaginable abrupt events. And how to value them is very speculative. We just simply bracketed it by sensitivity analysis. And that's going to be part of the daunting challenge that the IPCC and the scientific community will have to deal with in the coming assessment report. And the fun thing was, when we coupled DICE to the climate model—the most sensitive parameter in the climate model typically being the amount of greenhouse gas forcing—the coupled model, the most sensitive parameter governing the behavior of the coupled model was the discount rate. And therefore we got, in the words of the complexity theory crowd, an emergent property from coupled models that was different than you would get by running either model alone.

And apropos of our conversation, Robert, it reminds me of the coupled climate system initiative that we never got funded. Because this was precisely the kind of things that we wanted to do back in the 1980's, when we wanted to couple low order models from multiple systems and find out what kind of behaviors we would get. Because you can't learn them from just a complex model of one subsystem alone. And I have been actually able at Stanford to do that with undergraduates and old collaborators, using very simple tools. And while I wouldn't believe that any of the numerical results that we get should be taken literally because there are too many assumptions, I think the overall basic message should be taken seriously, namely the analytic tools that we have been using so far are quite inadequate to be able to assess what the real risks are of these geophysical, and now social and geophysical experiments that we are performing on the earth; and that we really need to seriously think about whether we want to slow this down a little bit and buy ourselves some time to figure out what it is that we are actually doing to the planet.

Chervin: It sounds as if, when you enter the realm of integrated assessments, you have complications upon complexity, upon almost intractability.

Schneider: Um hm.

Chervin: What sort of advice would you offer for any high school students, undergraduates, or brand new graduate students contemplating trying to do some good?

Schneider: Yeah, it's not like when I entered climate modeling when I was working with Ishtiak Rasool and there were thirty papers worth reading. You know, there are thousands in multiple fields, and no human can keep up with this. Of course the simple answer is collaborations. You need colleagues from all around. And you have to learn what they know, and they have to learn what you know. And you have to integrate and evolve together, and it takes time. The real difficulty is—who rewards this stuff? Let me tell you what I say to the Earth Systems 10, that's the elementary beginning course in Earth Systems here. I always teach the first day, and just did it a few days ago. I will begin by talking with the students about how pleased I am that they are interested in, you know, environmental systems, and dealing with problems that the world has to face.

And then I'll—a couple of minutes into the class I'll say, "Now tell me" (and about a hundred people out there, so this is not a good seminar, so I will do it by hand raising) and I will say, "How many of you want the world to be a better place?" And they all raise their hands.

And I say, "How many of you would like to work to improve the environment?" They all raise their hands.

And I said, "How many of you would like a good job?" And they all raise their hands.

And I say, "How many of you think this instructor is an idiot for asking dumb questions?"—and they cheer.

And I said, "Okay, there is a trick here, you know it, right guys?"

And they say, "Yeah."

I said, "How many of you think you can do all three of those things at once?" And now there aren't so many hands in the air.

I said, "What is it that in the Victorian Industrial Revolution made the rich countries rich?—specialization. Where are the rewards? Be the very best in your narrowly defined field. On the other hand, if all you do is content in one field, then you are ignorant of the crosscutting components which define real world problems. They are multi-[disciplinary] and in fact, interdisciplinary. Therefore, you need *context* as well as content. Unfortunately, you are no smarter than your colleagues who are biologists or economists or geologists or engineers. That means if they spend a hundred percent of their time doing just the discipline, they are going to have more content than you are. If you are also learning economics and biology and geology, and even though you may be a track, as we call it a track, in biology, you are going to still learn less. There is no way to avoid what the economists always remind us, that every single thing that you do reduces your opportunity to do something else. They call it an opportunity cost, it is a trade-off. And therefore, you are trading off content and context. You can opt to go back to the old way, spend all your time being the best specialist you can, you might get a good job. But then you won't be well equipped to understand what it takes to address real world problems. You won't be literate in what the fields are that you need to have some knowledge of. So how can you do it? My first advice is, stay on for your master's degree. We have here a co-terminal master's degree in Earth Systems. And that way you end up with the best of both worlds. You end up with that extra year which allows you to escape with about the same disciplinary content as a regular undergraduate, but you have the extra *context* about solving broad scale problems. But you have to pay for it, it is not free. You have to pay for it with an extra year of your life, and with an extra year of tuition.

The second thing is, when you move into your specialization and your job later on, don't forget that you *can* work on a detailed and specific problem, but you should always be asking the broader questions about how it fits in. And when you can't answer them for yourselves, get colleagues. *The key is community.* When I have to debate somebody like Dick Lindzen, as I frequently do about whether global warming is real and so forth, I will say to the audience: "Ladies and gentlemen, it may be difficult for you to discern whether he or I are more likely to be right, this is not a two person issue. The only way you can have any confidence in what is being said is to ask what does the community say. This is not a me versus him." You have to get to broad-based communities; you have to find out what people think is reasonable. You have to find out what disciplines do you need, what do the disciplines know, and how can we put the knowledge together. And there are now specialists in integration. And even though it is hard to find them jobs in academic locations, with a few exceptions, like the Earth Systems Science Program in Penn State or the Engineering Public Policy Program in Carnegie Melon—and I hope, a program that we will have here at Stanford, an interdisciplinary program in environment resources which will have graduate students soon, but still yet lend-lease faculty. You can combine disciplinary knowledge for multiple disciplines in an original way *only* if the knowledge that you are combining is of high quality, and if you have some understanding of the context. So let's make that trade-off between content and context—not too much of each. And if you need more of one kind or the other, find the specialists who are good, work with them, and let them help to keep you honest. It's not hopeless, you don't have to know everything, but you sure do have to know a lot, and it is going to take a lot of work.

Chervin: Okay. At this late hour, is it fair to ask, well I will ask it anyway, are you optimistic? Are you pessimistic? Will technology help? And do we have to redefine the term "community" that you used before?

Schneider: The answer to all your questions is "yes."

Chervin: In other words you are both optimistic and [pessimistic] . . .

Schneider: I'm optimistic that there is a lot we can do, and a lot of damage that can be prevented. But I am pessimistic that there is a lot we will miss doing because of the combination of, unfortunately, ignorance, greed, denial, tribalism and short-term thinking which prevents us from seeing problems until they tend to be too late—and the narrowness about sticking in our disciplines and not getting broad. But I think that's changing, and I think that people are demanding that we adjust our educational systems and our institutions to fit the nature of real problems. And that will, and already is making a difference. It is just not as fast a difference as we need, that's why I can be both optimistic and pessimistic. I think the community is key, because the community is really a set of interlaced communities that have different traditions, but the more they learn about each other, the more they learn they need each other.

And that's precisely what I like about integrated assessment is the merging of communities into an approach to questions of mutual interest. And that is a driving force, a powerful driving force to get us to learn enough about each other to be able to have collaborations. Technology is essential. We cannot repeat the Victorian Industrial Revolution with sweatshops, coal burning dirty power plants, and internal combustion engines in India, China and Indonesia. There are simply too many people in them, and when they start getting to the levels of income that we have now, we will have way too much residual damage to the atmosphere, land and so forth. We need to leapfrog to high technology. We need to use our inventive genius to help provide these people an opportunity to develop, without at the same time producing the same kinds of damages that we produced. That's only going to be credible if we work in partnership with these groups. We're wonderful at competing; we have to learn to be wonderful at cooperation.

Chervin: All right, Stephen, I wanted to thank you for one and all for your participation in this marathon session over four days. This is the end of the tenth tape. We have been involved in this process for in excess of ten hours. And I'm sure that the American Meteorological Society will gain a lot from this particular activity and I thank you again.

Schneider: Well, you are welcome and I have enjoyed the retrospective and thinking about these issues and I hate to say this, but this is just the tip of the iceberg of the issues that we need to talk about!

END OF INTERVIEW

INDEX