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TAPE RECORDED INTERVIEW PROJECT

Interview of Syukuro Manabe

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Interviewer: Ronald Stouffer

Stouffer: Okay, you tell me when I should start. Okay, so we're here today to interview Suki Manabe on his research career. We're at the Geophysical Fluid Dynamics Laboratory in Princeton, New Jersey and it's August 23rd 2007. My name's Ron Stouffer and I've been a research meteorologist here for the last thirty years, and for the first twenty years I worked with Suki. And, we're going to do an interview which is going to start with his background, and then look at the first twenty years of his career, and then the second twenty years, and then summarize his career. So, to Suki the warmest welcome, and to start I'm going to ask you where you were born and a little bit about your family.

MANABE: Yeah. The -- I was born in 1931, which is almost 76 years ago, which is three quarter of century ago. I was born in one of four islands -- the smallest of the four large islands of Japan -- it's called Shikoku island. And, the place I was born is very mountainous region. It's a major mountain range in Shikoku Island and mostly (inaudible) farmers. And my father was a general practitioner in this small village, and he was looking after all these people at that time in a small village and following my grandfather who was also a physician. So, I always thought that I want to become a doctor. And, you know that to get in to medical school in Japan you have to do very well at school and do very well in the entrance examinations. So, actually, I studied extremely hard. You know, sometimes I slept, you know, five-six hours every night, and studied very hard and finally I got in to a medical school at the university of Tokyo which is the most competitive school there. And, so I was very happy getting in there. But then I suddenly realized, "Do I really like biology?" And at that time biology was mostly memorizing subjects. So, I usually crammed in the last day -- the day before the exam -- everything I can squeeze in my brain. I remember enjoying studying biology. It actually turned out biology has been a very decent science looking back, what has happened in the field of biology. But, I never thought of that then. And so I thought physical science, particularly physics, was much more interesting because you pose the question, you answer the question logically, you solve the question -- which I thought would be much more enjoyable. And -- so, I decided at that point to change field from medicine to physical science; particularly I decided to go into physics. But, then I realized I'm not that good in math to get in to the difficult physics. I decided to choose geophysics branch of the physics department and -- but then, I did get the [culture?] of the shock because once I got in the physics department, some of the kids were -- you know -- way ahead of me, and I had a hard time even passing the exams. But, then I decided not to just follow lecture in the classroom. Usually, you end up -- because you're in classroom I never understood was the professor is telling me -- so I kept struggling, struggling, but eventually I decided, "I'm not going to listen to this professor anyway. I can never keep up with the pace of these course lectures." So, I decided to study at my own, at my own slow pace, and try to sort out in my mind until I get satisfied. And so the next year I have to take the course again -- I mean -- the exam again. And the next year I did pretty well, I get through most of the exams in flying color. And, so, I learned how to think physically about problems. So, it was never satisfied just getting the solution. Never satisfied. You try to understand what is the physical implication of the solutions. And, so this was a very good thing that happened to me. And, so after graduating University of Tokyo I want to graduate school and geophysics department of geophysics at the University of Tokyo.

STOUFFER: But -- just to interrupt you for a second.

MANABE: Yeah.

STOUFFER: How'd your father feel about you changing out since there was this history of being a medical doctor? And I think your brother's a medical doctor, or –

MANABE: That's right. So, father never appreciate how other professions -- there are so many decent professions in this society other than medical doctor -- but, he always thought that medical doctor was the most [right?] profession. But, you can do everything on your own; you can be independent, you don't have to work for anybody, but still you can make a decent living. So, it was a great disappointment to him because, sending his son to a medical school -- you know -- he was very proud of me. But then, son is now wanting to do this obscure thing of geophysics, and he couldn't even distinguish between geophysics and astrophysics, so he thought I was spending all my time stargazing.

STOUFFER: (Laughing) So you -- pick up your story again.

MANABE: Yeah.

STOUFFER: So, you were going to graduate school, so how did you get in modeling weather and climate?

MANABE: Yeah. So, that is the next thing that -- so, I got in the department of geophysics, the graduate school of geophysics at the University of Tokyo. And, so I have to choose the subject, which one of geophysics. Because you have seismologies, theoretical physics, atmospheric meteorology at that time, and oceanography and electromagnetism. But -- so, which one do I want to do? Seismology I thought you can never see through. And so, although seismology is considered to be a very important topic in that time, I said I eliminate that -- I never go that -- doing measurement, doing measurement and using my hand, so I eliminate it. The electromagnetism I thought I was good at it. Of course electromagnetism was very good. But then, I'm not very good in creating all these sockets, and then trying to amplify all these signal coming from the upper atmosphere. So, I said, "I'm not good at that." So, you know, I eliminated biology a long time ago because I'm not good at memorizing anything. I eliminated theoretical physics a long time ago because I'm not good at math, or good for mathematics. So, it was a process of elimination, I eliminated these theoretic things because I'm not good at using my hands and doing experiments -- creating experimental operators. So, keep eliminating, eliminating. And then, finally, I said, "meteorology may be good because somebody else make a measurement for you, and then you can use these datas, and you don't have to memorize very much because they're doing much at meteorology in that time anyway." Right?

STOUFFER: Yep.

MANABE: And I can see through everything. Cloud is beautiful, sky is beautiful, it's so wonderful! And then, still you use a basic role of physics at that time -- I thought -- although, I don't know at that stage, meteorology is more of an art than a science. So -- but then I thought I would apply physics into meteorology -- make meteorology alike. That's what I said I would do at that time. So, I chose meteorology, and when I got in graduate school it was early 1950s; to be exact it was 1953. And in 1953, the time when the United States, at least for advanced study, some year around 1950 -- it might have been 1949. (Inaudible) [Moiner?] created -- he was looking for -- some theory to apply his electronic computer which he was directing. I didn't know he fathered the electronic computer -- he's the father of the many other things -- but electronic computer is one of the things. And, he created a smaller group, headed by [Jung Chani?], Norman Fritz, and a few other people from the outside. Some of young but very capable visitors. So, started doing this research in weather forecasting using hydrodynamic reports. At that point -- hydrodynamic reports -- only you can solve some linear problems. But, non-linear hydrodynamic equations were interesting. I said, "I'm going to try to predict weather." And, I was so excited by it, and other people like Kiko [Nagoya?] who happened to be a -- he's one year ahead of me at the institute. And my classmate by the name of Kumo Bayashi -- he's a brilliant guy -- I owe him very much in the development of my passing (inaudible) science. But anyway, I happened to have an excellent group of excellent friends and coworkers at graduate school. And, at that time, we heard in the grapevine that [Joe Slovorinsky?], at the institute, was thinking about doing numerical weather prediction of rainfall. And we thought, "Hey, this is a good thing. We would like to try it too." So, in a sense, we copied his idea at that time.

STOUFFER: (laughing)

MANABE: But then, several of us, worked day and night. I remember practically worked about 16 hours a day, every day for about one year and half. We worked day and night and did three-level model baroclinic weather forecasting. With putting water vapor in it. And so we -- you know -- used in [radical pigment?] – we didn't have electronic computer. You remember?

STOUFFER: Yeah. It's so amazing you did all of this on graph paper.

MANABE: Yeah, we moved it graphically, we moved the bolticity around three dimensionally, so in our equation, graphically again. And then, what we come up with -- this was a Kumo Bayashi's idea -- you move the water vapor around, and when it's super saturated, you just (inaudible) you just top them off, and then that is (inaudible). So, it's a very simplistic idea. But, you know, we got a pretty decent -- twelve of our -- we barely did it only twelve hours of forecasting.

STOUFFER: Couldn't have happened in 24?

MANABE: Huh?

STOUFFER: Wasn't it 12 and 24 –

MANABE: Yeah.

STOUFFER: -- the original?

MANABE: Yeah. So, it's just -- kept (inaudible) weighs of moistures of everything. Vorticity of everything, and we did it about, about five of us. Everyday we kept throwing vorticities and this turned out to be a great experience for me because I begin to see how weather works. How vorticity moves around and how rain falls.

STOUFFER: Right.

MANABE: And get a pretty good [percent?] there. And so I published quite a few papers like Moving Barotopic Model -- but moving coordinate. So all domain moves around (inaudible) current.

STOUFFER: Right.

MANABE: And so I wrote quite a few papers at that time. And what is very fortunate is Smagorinsky looked at some of our papers, and then these young [persons?] at the University of Tokyo doing something really interesting. And so he decided to invite one of us -- he send two of us Kiko Nagoya and myself -- the letter of invitation to Professor Shiyono who is my advisor professor there at the University of Tokyo. He thought that, "Kiko is so valuable." Because without Kiko he just can't manage the whole department. Kiko was (inaudible) not only leaving, or he's graduate student.

STOUFFER: He was the oldest of the group?

MANABE: Yeah, he was the oldest of the group, so he was managing there. And also, he was doing, on his work -- which you have to do -- management work for Professor Shiyono, so you can't spare him. So, Suki -- you can -- you are expendable. So, finally it was decided not because I was better, but I was expendable for my professor at least. Right? Because professor thought -- he was very understanding of going away -- so he kept sending, not much job postwar Japan. So, he was sending all groups of people outside of the United States. So, I was one of them. And, didn't have much job in the postwar period. And, knowing what you may be able to do in the United States, took no time for me to accept this job. And so, that's how I decided to come to the US.

STOUFFER: At that time, the lab was in -- D.C. -- Washington D.C.? So you showed up, so then -- what did Joe want you to do when you showed up?

MANABE: Yeah. Now, the thing is -- so I came over. You know, at that time it takes 48 hours to get to -- from Japan to the US.

STOUFFER: Now did you go through Hawaii or Alaska?

MANABE: Yeah, no you go a great circle.

STOUFFER: So you went through Alaska?

MANABE: Anchorage, Alaska; Seattle, Washington; and (inaudible); and all the way to Washington, D.C. It was a kind of tiring trip; and when I finally started out my airplane, which is a Cargo prop airplane, it's not -- the jet's wasn't there yet. And Joe was in sunglasses and waving in the reception area. So, he, realizing he -- you know -- needs some help when I arrived there. So, he took me to his home, which was located in Fairfax County, Virginia. There's a national airport in Virginia, Fairfax County. So, he drove me home first. And then kindly let me stay for about one week at his home. But, as soon as he arrived at his basement he started talking about his grand plans of what he wanted to do. And, by then, already, he was developing the two (inaudible) model, two (inaudible) cyclation. And, no one physics -- he's a pioneer building two radii or more of atmosphere. And, at another front he using that two linear model at least for advanced study, succeeded in simulating basic features of cyclation of the atmosphere. However, it was so -- what do you call beta plane approximation -- it was a zonal delta [altitude?].

STOUFFER: Right.

MANABE: And then he give me -- forced the two linear model with heat source and lower altitude and heat sense and high altitude, and [drive it?]. And then he got this virtual simulation, with western winds and cyclone waves. Now, what Joe tried to do at that time, he discussed extensively with Jung Chani, Norman Fitz, want to extend what normal physics do at that time. And, first of all, he want to change this (inaudible) attitude to a primitive equation, which making it applicable to lower altitude, where corollary force is more, and heat of condensation if very large, so [core?] of the geostrophic equation doesn't work. So, Joe was working very hard. Already, at that time, he was running successfully the two radii primitive equation model.

STOUFFER: So, trying to make the model global, effectively?

MANABE: Yeah. First of all he want to make it global. And then he want to -- if he wasn't satisfied with this (inaudible) model -- so he want to put a -- Jung Chani saw that three's enough, but Joe made a nine digit model. So, it includes not only troposphere, but interactive stratosphere. And, then of course you're always interested in the numerical model predictions of rainfall, as I told you, he pioneered that thing. And so, he wants to put the hydrologic cycle. And so he wants to include in his model transport of water vapor by large cyclation and condensation, evaporation, and these thing. Which, really makes simple, general cyclation model -- transformed that in to theory-climate model. You can't talk about the climate without rainfall.

STOUFFER: Right.

MANABE: And so now that he has nine vertical moving [dissolutions?] he can't -- he can't really fix the static stability. Which [to date?] the model always did. So, he would like to put the convective heat transfer from our (inaudible) atmosphere, through moist and dry (inaudible). And then, that heat, relieved, would have to be given away to the states by radiating outgoing radiation so that [carmoagen?] of this system does work. So, he wants to put the (inaudible).

STOUFFER: Right.

MANABE: And then he want to put a -- heat and water bag of [soil?] which controlled out climate at continental surface. But then -- that isn't enough because we have a big ocean. So, he begin to think at that time -- although about the ocean he was still thinking -- but would like to eventually like to capture this atmosphere with an ocean model in some way. But, with the exception of ocean his plan was pretty specific. And, it was the beginning of [ab-system-morbe?] which we now call a system. That was basically begun -- began -- at that time. Of course, our system (inaudible) probably began by (inaudible). However, [Rich Iverson?] is a little bit of a pie in the sky. But Joe's plan was actually -- he had some special idea of how to get this sky. It's like, specific. Although, I don't know how much he anticipated all of the [overstepping?] which he made encounters along the way. And I was very impressed by this [unhesitant?] nature. Because, you know, coming from postwar Japan, I never did think about such a, sort of, random plan. But, after I think about it, why don't I take this challenge and try to collaborate with him to make this dream come true? So, that being how I get started participating in this group.

STOUFFER: So, is it -- the way you interacted at that time with Joe was kind of supervisor-supervisee kind of way? Or, were you co-equals? Or, how w9ould you characterize that early interactions.

MANABE: So, now this is -- yeah. So, Joe's is a sort of inter (inaudible) idea of supervisors or, they got say as the group leader at that time. And, at that period -- already when I arrived there, he started the end of 1955. When I arrived it was September 1958, so it was about two years. And, he got this extra endorsement from head of research -- weather viewer. His name is Harry Wexler. And, Harry Wexler is supposed it to Joe. Older (inaudible). Of course, regionally, From Norman is the one who want to, sort of, create this general cyclation (inaudible) section which Joe had. But, Harry Wexler is discussing with Joe already, planning to get a super computer at that time, which IBM built it. Built to talk about. And so, he gave full support. One of the things, that by the time I already arrived there, Joe already had a large number of Northern pin programming. Headed by Reese Halloway. And then, he started getting a (inaudible). Another person Roderick Grimm -- Rod Grimm.

STOUFFER: Rodd Grimm.

MANABE: Grimm was really had to been his right hand man, but instead of buy then he hired these programming starts and the (inaudible). But he somehow needs some sort of young [hand?]. So, he was reaching around. And, so, his invitation letter came to University of Tokyo -- it was one of these [lectures?] there. So I came, and then immediately -- and then, almost same week or something, Doug Waylan came. And then, a little bit later, we discuss more about oceanography and atmosphere coupling. So, he mentally, he the army of 60-61 hired to cut riots.

STOUFFER: And he had been in Boston, or?

MANABE: Yeah. Clark was that (overlapping dialogue; inaudible).

STOUFFER: Okay that's great.

MANABE: And, so, we decided to hire Clark. I know him a few years earlier because I went to Roosevelt center and electric institution.(inaudible) that's forced there. So, both guys and I -- so, I got caught over here, it's great to have Joe over here.

So, Joe gave me, to take care of, things he was not doing at that time. He himself was working on primitive equations. So, he asked me to look at how to deal with (inaudible) process. David would transfer -- which he wasn't doing at that time. He got Fritz Mayor -- who is a pioneer of the (inaudible) transfer in Germany -- but he is Fritz's [manner?] is such a big leader in Germany, so he can't stay long to do the glimpse of time. But, Fritz Mayor is supposed to -- usually he could have done (inaudible) transfer...

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TAPE 1, SIDE 2

MANABE: So eventually end up doing [daily?] transfer myself. So, these I was responsible for: (inaudible) process and daily transfer. And then that (inaudible) supposed to do conviction -- convictive heat transfer. And, he put a measure emphasis on cloud (inaudible). Study modeling of cloud top [boundary area?], which now become a major program.

STOUFFER: That's a major problem (laughing).

MANABE: It's extremely difficult. Stratus in the subtropics, all that sort of thing. And so that is supposed to do that. So, three of us personally started doing conviction. So, three of us got a huge chunk of the problem. And so, we worked extremely hard; during light, day and night. And so, during night we think about some way of how to do it. And then come up the next morning and then tell each other what we think about.

STOUFFER: So this is you, Kirk, and Doug?

MANABE: And Doug; yeah. And then you –

STOUFFER: And how much interaction did you do with Joe at that time, or was it just the three of you guys mainly?

MANABE: Yeah, no -- Joe was very close at times in what we were doing. Listening in the seminars and so forth. But, he himself was very busy in designing -- he talks about the day he would come -- designing how to create a (inaudible) finite difference level in the atmosphere.

STOUFFER: Okay.

MANABE: He was working at application of the mathematical __________ formations of (inaudible) algebras of nonlinear viscosity -- he was working on that. And so, everyday he himself was working very hard. We talked about it. And so, I guess, in a sense it's four of us talking all of the time. And, usually when you come up with an idea during the night we shoot them down.

STOUFFER: (Laughing) The next morning?

MANABE: Yeah. So, then you go back drawing board again.

STOUFFER: Right.

MANABE: And so, it was a very exciting, stimulating environment we had. And so, that was really great time in my career. We didn't publish many papers. I hardly published any paper. And Doug may publish one or two at the beginning of the past few years. And yeah. Kirk published that much; he published two papers. But, in terms of doing something, we were doing -- we aren't publishing things in the papers.

STOUFFER: That's the age-old problem of model development –

MANABE: Yeah.

STOUFFER: -- it's hard to publish papers on it.

MANABE: That's right.

STOUFFER: (Laughs) You guys were the first to experience.

MANABE: Yeah.

STOUFFER: But we continue to experience that to this day.

MANABE: So yeah, this is -- I think is a very difficult issue we face. And Joe discussed this a lot. That maybe we can discuss this a little later.

STOUFFER: So, I was curious that, you know, how did you get in to the radiator conductor equilibrium model?

MANABE: Yeah.

STOUFFER: From building a complicated three-dimensional GCM --

MANABE: Yeah.

STOUFFER: -- how did the radiator conductive equilibrium model come about?

MANABE: Lots of time -- theory of radiator transfer had progressed quite far by then, actually.

STOUFFER: Right.

MANABE: And, this is to apply the theory of our atmosphere. And, first pioneer of this is a Fritz Mayor of Germany who happened to visit us around that time. So, Fritz Mayor is one of the pioneers. And the other one is a [Eli Salzer?] -- how do you pronounce it? Eli Salzer?

STOUFFER: Yep.

MANABE: Originating from Germany I'm sure.

STOUFFER: I think so.

MANABE: Eli Salzer -- I think he really made it in the terms of physics what this is all about. And the other person involved was Yamamoto, who was a Japanese scientist. He created excellent rain model transfer (inaudible) into compute heating and cooling by atmospheric radiations. And so, by then, and of course (inaudible) excellent contribution and he also wrote excellent papers -- books, actually. He made the first draft of that book I read, so I studied very hard at that time. They needed transfer which I didn't do -- study much in graduate school. Although I took the course, they talked so mathematically and abstract, I didn't understand a thing. So, I had read [all the books?]. And so these are some of the pioneers -- show us how to complete radiated transfer by usually, actually graphically to complete radiation [dialect?] -- which was a theory in [my subject?] -- and categorize graphically.

STOUFFER: Okay.

MANABE: And heating and cooling distribution in the atmosphere. Also around that time there was excellent measurement of absorptivity of carbon dioxide and water vapor. I think this may be something to do with a [numerical?] application. I don't think there Mayor used absorptivity for the sake of atmospheric radiated transfer calculation -- which we need. But, they did -- so, usually, it's not easy to measure water vapor absorptivity when water vapor amount is very small. And this pioneer knew what had done excellent -- I should more excellent than what was done -- a Ohio State. People like [Hawaii?] or (inaudible) they did excellent measurements of water vapor. They are variables for me to use. And, so I put together all these ideas which are available already. They said they were doing it to review the diagram -- radiation diagram. I want to do it numerically so that it can be put in a computer. But, on the other hand you really don't know if you've put this (inaudible) couple wind hydrodynamically equation in three-dimensions, and then you keep circling all new things, how will you know you end up with something like real atmosphere? So, the best way to do it for me at that time to minimalize the risk, was to do a one-dimensional column model calculation to evaluate radiatable transfer (inaudible) and evaluate possible effect of convection and see whether we can get realistic primary structure of the atmosphere.

STOUFFER: This was in a nine level model?

MANABE: Yeah. Nine level. Actually, we eventually we could do it to an 18 level. So, this is out of screen test of the major component of the three-dimensional model which we are constructing. So, that was basically what we are doing. And following example of [Norman physics?] we sort of did this with usual [variable?] problem. So, you have a one-dimensional radiating convective model. First, you have a pure radiative model. And a very hot initial condition, and very cold initial condition. And start from very hot initial condition -- you run about 300 days. It took me 320 days to be exact (inaudible). And then you get almost final state. Things stop changing and you get radiating (inaudible) of the atmosphere, and you find extremely hot temperatures -- something like 50-60 degrees -- I don't remember exactly, but very hot at the surface, and the temperature goes down very rapidly. And by the time you get to 800 (inaudible) temperature is pretty cold; and then you go more in to the stratosphere.

STOUFFER: There's no heat coming off the surface.

MANABE: That's right. The only way to get rid of the heat from the surface is radiative transfer, so you have to have a very hot surface, and cold air temperature so that net upward [graphs] on long-wave radiation become very (inaudible). That's how you get rid of solar radiation, absorbed by our surface. Now, so whatever happened then is needed for convictive heat transfer -- is very clear. So, that's how we divvy this thing in to convective [rigastinant?] of rhapsody. So, get rid of heat from the Earth's surface by convictive heat transfer. Bring in the heat to our surface to middle and the upper troposphere, which would then in time radiate it in to space. So, that's how convictive heat transfer does right there. So, first we did the radiative equilibrium and then we put the convictive adjustment of rhapsody, and of course (inaudible). But, then you can reasonably good conviction of the troposphere; [troposphere's] height is about right, and (inaudible) stratosphere in which temperature increases slowly with high due to the effective (inaudible) absent and solar (inaudible). So, we get pretty nice, we can (inaudible) structure. And by the time we put the selectable cloud which had reflect the sunshine, had the greenhouse effect, that we get pretty nice surface temperatures. We can put this thing in the 3D model and -- but to get this nice radiative transfer Fritz Mayor was very helpful. By the time he arrived I already had some radiative model developed, but he pulled it out and said, "Suki, this is not going to work." And that really helped. So, you know, for a (inaudible) man to do something like that, it's kind of risky. And so -- and here you are getting in to interdisciplinary area. And so, I think it is very important to collaborate with the people who are the other side of the collaborate -- you know -- to combine thereby getting in to the interdisciplinary science. And -- however, I think that it's not good to just rely on completely: radiative transfer, okay, it's yours. I'll do this.

STOUFFER: Yeah.

MANABE: And then put it together. It's not going to work. And so, in order for collaboration to really work effectively you've really got to understand the other side. Which I can say did happen to me when I collaborated to Kirk Brian. I can't just let ocean sitting in the other side of the fence, and then still try to collaborate in a very clear way.

STOUFFER: It's hard to know what's the right question to ask.

MANABE: Yeah.

STOUFFER: So, what possessed you then –

MANABE: Yeah.

STOUFFER: -- you described -- you developed this wonderful model, you were all happy –

MANABE: Yeah.

STOUFFER: -- and you put the radiative pair in to the 3D model.

MANABE: Yeah.

STOUFFER: What possessed you to double CO2 in the radiative convective equilibrium model?

MANABE: We –

STOUFFER: And then write a paper about it that became a very famous paper!

MANABE: Yeah, and probably that's the best paper I wrote which I will discuss later. But, the -- originally part of the reason I got in to it was I gained invested interested. Because after all, we have the green house gases, this planet is about to (inaudible). So, I want to change all these greenhouse gases. Up and down; try to do sensitivity study before I put all these things in the 3D models. So, it's a kind of numerical exercise. But then, of course, I didn't realize how at that time they fortunately -- there's a paper by Fritz Mayor on the estimating global warming. The (inaudible) pioneers of global warming is [Irraneous?], and he was the first one to do it. And, it was followed by Calendar of the UK.

STOUFFER: And Irraneous was, what? 1900 or some -- 1800.

MANABE: Right, right.

STOUFFER: And then Calendar was 1932?

MANABE: '38.

STOUFFER: '38?

MANABE: Eight.

STOUFFER: Okay.

MANABE: And then [Puras?] 1915 he cross-published several papers. And (inaudible) several papers completed the estimate of the greenhouse warming. And, the -- one of the sanctions is that all of these resources basically assumed this convictive heat transfer which would go from the Earth's surface to the atmosphere. Now, if you put the more CO2 in the atmosphere, more downward flux of infrared radiation congregates -- it's a long story, but anyway -- more downward flux of infrared radiation comes to the ground, Earth's surface. Which, how do we get rid of it? And, so that naturally -- it is natural to think that if you have more infrared radiation temperature at the Earth's surface will become warmer. It should have more upward flux of radiation, which all of these [all said?] thought about. However, if you have more downward flux of radiation another thing is convective heat transfer from our surface to the atmosphere should increase with surface temperature is (inaudible) it should increase. All of these guys assumed that convective heat transfer does not change despite the increase of surface temperature by greenhouse gas increase in the atmosphere. That was the assumption which is done by Irraneous, Calendar, all of these people. They have all of these assumptions. And when Fritz Mayor tried to reproduce these results.

STOUFFER: These results being yours?

MANABE: No.

STOUFFER: Okay.

MANABE: All these pioneers –

STOUFFER: Okay.

MANABE: -- of global warming. And when you put more CO2 in the atmosphere it's a long story short -- he got global cooling rather than global warming. When he tried incorporate the effect of positive feedback affective water vapors. This probably -- basically what happened is the surface temperature increased, atmospheric temperature increases. So, you have more water vapor in the atmosphere. Let me leave more downward flux of radiation -- so what happened is the warmer you make surface temperature, the larger is a net downward flux of radiation. So, in order to make up for the increase in CO2, downward flux, due to CO2 increase, you have to cool the surface. So, to his great surprise Fritz Mayor got to global cooling, so he had to adjust this somehow to affect the other affectant. Eventually he got to global cooling, not warming. But, this time global cooling was about minus ten degrees for doubling the CO2. Now he'd have to plus ten degree for doubling the CO2s. And, then Mayor is smart enough -- actually he and I discussed (inaudible) -- and the reason you get to such a crazy result is because you didn't take in to consideration of the increase of the convective heat transfer from our surface to the atmosphere. And so, I thought at that time, "Hey, this radiative heat transfer, radiative convective model which we have developed the ideal thing for, sort of in this puzzle." Which was the case.

STOUFFER: Right.

MANABE: So, that was the motivation. See, when you are busy in developing a model. But, sometimes it's nice to do some excursion outside of your responsibility which was a development of 3D model. I've been to some mathematical and numerical exercise, but at the same time I was curious enough to see what was the point at that time. And so, despite the fact that I have probably as much stress and pressure to make my -- our model to work, because you've got the huge computer which cost a fortune every day at that time. It was $8,000 dollars a day, but you know, you multiply something major; I don't know ten times, or 15 times. That's a huge amount of money every day we are spending in terms of rental fees to IBM. And, computer was sitting idle for other reasons. It kept blowing up at that time. And so there was a huge pressure. So, in a sense it's the same right now -- the people are getting in the major center for -- are system modeling and the prediction of global warming. It's nice to be curious and do the interesting problem which you are curious about. Using the tool which you are developing. But, the problem is that everybody is so exhausted to make that tool work, and they don't seem to satisfy their curiosity at that point. But I think, looking back, that one more effort to satisfy curiosity would have made a very nice things about their career much more fundamental. You know, this model people are developing is a very powerful tool if they are used right.

STOUFFER: Right.

MANABE: And so, this is -- I think -- the younger generation of climate moderator have to keep in mind we still take these opportunities as their curiosity takes them.

STOUFFER: So –

MANABE: Yeah?

STOUFFER: You've told me many times the stories of you simplifying problems in order to solve them.

MANABE: Yeah.

STOUFFER: Then put just complexity in to the problem that's warranted and no more.

MANABE: Right.

STOUFFER: So how did you adopt that approach to parameter station development, because it impacted virtually everything that you touched for the rest of your career.

MANABE: Yes.

STOUFFER: When did you develop that and how did it come about?

MANABE: That's come out of necessity. Because you have to realize I -- in 1960 with the first IBM machine -- which called the [Spirit77?] came in. Which we used for applying Joe's dream model in.

STOUFFER: Yep.

MANABE: We used a symbolic assembly line. At the beginning we didn't even have (inaudible). And everything that you calculated you did hundred calculations. You had the right hand side of the equation, the left hand side of the equation, and you have a desk top calculation, a cube calculator, a right understand of the equation, seeing whether it's agreeing with the left hand side. And when you had to dig back, after you found out something wrong with your program, usually very complicated [fragmentization?] usually we would do is create a computer (inaudible). So, when you're very proud of creating this parametric deviation of some so-called [theoretical?] processes, put them in, and usually during the famous Murphy's rule, anything that could go wrong would usually go wrong. Because when you repeat these calculations tens of millions of -- tens of thousands of times again and again. Murphy's law applied beautifully -- anything that can go wrong would always go wrong.

STOUFFER: Yep.

MANABE: So, usually, you make a very sophisticated parameter deviation which you are proud of. Usually it ends up with a blow up; and at that time computer is not very curable. Just a small overflow, or leave it to overflow right, and this Spirit machine had the gong. You know, they'd have a gong, every time you blew up you'd get the gong song. And your face -- you know, it's kind of embarrassing that every time you're gonged you come up. And usually, also, gongs were –

STOUFFER: Okay?

MANABE: Yeah.

STOUFFER: Take a break for a second.

END OF TAPE 1, SIDE 2

MANABE: So, there is many interesting findings which this study did. That this three-paper study do to -- by using a couple [modems?] there. And one of them, this great delay in a certain part of the wide ocean, where mixing is very deep, and particularly in the [second floor?] oceans. And there are another place, as you know, it's a sinking region of the north Atlantic Oceans. And maybe (inaudible) another place where it's created a rare subduction of intermediate water is taking place. These are the three places which delayed it greatly. And so, this lead to confirmation of the idea I designed; obtained by idealized sector model ocean, which is inter-hemispheric (inaudible) and the Roman. And, we get a large report on [predication?], that we don't have (inaudible) [predication] in the southern hemispheres. And I think that now I disagree (inaudible), you remember this review we wrote. This hardly centered, this cannot be and, and other -- show that paper -- clearly show confirmation of beautiful inter-hemispheric asymmetry. And similarly tried to obtained by many other model. And, so -- actually, there is the model sensing of tropospheric temperature, also beginning to show similar inter-hemispheric asymmetry. So, I think is one of the biggest discovery we made. Our prediction turned out to be right probably.

STOUFFER: Yeah.

MANABE: There is the other thing that -- we don't know whether it's panned out or not -- but sooner or later we predicted [municary?] may become an inside conclusion will weaken in this state but for the first time. And, I think that [Steel Braggins?] and his collaborator's study suggests that they have a huge natural variability; inside the year there were huge seasonal variations, or inter-seasonal time scale that could (inaudible) all these had an inside conclusion invincibly, and I don't think that if there's a weakening, it's not a disgrace to be weakened. But, this opened up the large fluctuation of the Helen cycles which we discussed with (inaudible) and other papers. And I think it could be a very important issue for a company to monitor the solidities of the north Atlantic Ocean.

STOUFFER: Right.

MANABE: So, these are the -- another thing that we predicated at that time, is rapid reduction of sea ice over Arctic Ocean, which has now panned out. And also, we predicted the slight increase of the Antarctic Sea ice which seemed to be exactly what's happening, that is the people talk about the west Antarctic ice sheet collapsing. Or, you know, ice sheet was used for dramatic pictures of ices pouring down in to the sea, which obtained the near the (inaudible) seas and so forth. But, east Antarctic ice sea -- sea ice -- is increasing slightly so that when you are going over entire things last few decade. It increases by about 1% -- 1% decayed or something like that?

STOUFFER: Something like that.

MANABE: Slightly increasing. And again, in agreement with the results we obtained. So, this study predicted two things which can not be the case. And I'm very proud that we published these statements, and it's one of the best-cited three large papers we had, as we'll discuss later.

STOUFFER: Right. So, when I went to the part of your career where you're using models to study climates --

MANABE: Yeah.

STOUFFER: -- and how did you give in to paleo-planet model where in, or long purple model integrations, and what motivated you to do those kinds of things?

MANABE: Yeah. Part of it is climate has always fascinated me because the changes are so large. And so that, enabled to hit your model by hammer, rather than tinkering with small changes. So, we did that in changing CO2, halving the CO2, or something drastic we'd do.

STOUFFER: Yeah.

MANABE: And so, having a climate study has always been a great pastime for me. You know, it's kind of a great thing. But, the first thing I did was to eliminate all topography. Eliminate all wild mountains there. Which, as you can see, you just experiment, anybody can do. And the -- as a result, we found quite a few interesting things. John [Kudsback?] did, but I think we did ahead of him most of those things, but he is the -- I think -- one of the few -- he is really outstanding climate modelers, in my opinion, better than many others. But, I think that -- let's not talk about that. But, this autography is one of the most -- you can delete some of this -- autography is one of the most interesting experiments we conducted. This one might not be second twenty years -- we already did it during first twenty years -- is to eliminate mountain. And one of the most important one is dominating a (inaudible) Tibetan plateaus. And without Tibetan plateaus you get a rain belt, but never come to the plain of India, but stay at Sri Lanka's near five degree north or somewhere around there.

STOUFFER: So, the summer monsoon doesn't occur in India?

MANABE: No. But then, what we discovered is that when you take that out there's a huge chimney going out the southern half of (inaudible) just southern -- just through the south -- huge chimney going up, and then that's air go over central Asia, all sent down to the central region. So, if you have a huge heat source over the southern parts of the (inaudible) you create south Asian low pressures. You anchor long wave [turf?] -- stationary long wave turf over there -- and so central Asia become a real part of your turf where air subsides. And that is the reason why you get the central Asian desert. And what is really interesting is we get many, many experiments. But, one of the experiments which [Tony Broccoli?] did is -- without soil-moisture interaction. So, Gobi desert, (inaudible) but if you don't have a soil-moisture interaction -- that if you cut the desert in the western, central Asian desert, then the downstream rain is reduced so [soil?] gets drier. Then to get to drier, then it's hotter, therefore this precipitation -- therefore soil gets dry and hotter. This positive feedback extends the central Asian desert all the way to Gobi. However, the original one was because you popped the heat source right at the Tibetan plateau's longitude, that you only can explain by direct effect only the central Asian business. And another thing happened; because of this air just goes around like this, so if you look at how moisture is transported; if you have a (inaudible) here, air just go -- avoids the Eurasian continent, most of them -- south and north and avoid and (inaudible) just air coming down and going out. So, air just goes out these. And, I begin to realize this is one of the important reasons why you didn't have major continental ice sheet in the central Eurasia, because you cut off the moisture supply by (inaudible). So, I think this is one of the experiments you can do, but you have to model by [Hamba?] and create a huge impact. And it's a great pastime to study this. And the -- another thing that you and I work on this one -- is this fresh water -- what you call --

STOUFFER: Water hosing?

MANABE: Water hosing experiment. And I guess Wally Brocker came up with this -- Wally Broker came up with this idea that bounce about the (inaudible) is related to (inaudible) situation. I think we are the first ones to demonstrate by model simulation; connection between some [Helen?] cyclation and (inaudible) oscillations. But, one of the things I think we found is this study we made is Tom [Bedwars?] the multi-federal organization of some [Helen cyclations?] you get to very sudden by an anomaly and multi (inaudible) introvert. And so, you get exasperated even though some Helen cyclations don't change you that much. But, you treat your own and off convection, thereby amplifying and selfish manifestation in terms of SSP and SSS the fluctuation so that multi-federal oscillations of some Helen cyclation is enough to account for the abrupt climate change. So that abrupt climate change is not what Wally Brocker -- in my opinion at least -- going from one model operation to another, but instead it's a manifestation of the multi-federal oscillation of some Helen cyclation which then -- if you actually -- if you are convinced that if you look at them this time sheet is dead over 18, obtained from (inaudible) ice school. High resolution version of it, you get a beautiful multi-decadal fluctuation of some Helen cyclation. And I guess we discussed in our paper -- quite a few of our papers -- and still I don't think that we send the message on this issue to other people. Even though our papers may be cited, but they don't connect multi-decadal oscillation with the abrupt climate change. Instead, still the majority of opinion is that abrupt climate change is the transfer from one model of operation to another.

STOUFFER: Right.

MANABE: And this is something that has to be resolved; and I think we are right that they got to do that.

STOUFFER: Right. The other natural variability thing that you got in to a lot was the Tom [Kanutz?] and another's is the [M-cell?] -- is the multi-decadal fluctuations of...

MANABE: Yeah, no, this is this thing that I have the [Shiaria?] one. I think one of the -- I think you get really involved in to this. This is the first solved in the -- you shouldn't get the Guinness Books' world record for [first 1,000?] for integration of coupled model. And I mean, you have first 15,000 thousand integrated -- a couple more than that. But, I think that the 30,000-year explained it, it's really important event, although people haven't realized. But, increasingly more and more hardly centered people --

STOUFFER: I was going to say, virtually every model group has copycatted this now.

MANABE: Isn't it?

STOUFFER: Everybody goes a thousand years.

MANABE: Now, one thing I realized is that's a beginning. At the beginning it wasn't -- I was watching the citation growing, it was very slow. But now, it's rapidly increasing and people pay attention. But, I think this thousand manipulation is one of the best numerical experiments we conducted. Mainly because it's really successfully simulated the spectrum of the global temperature variability. You know, you characterize the spectrum, or you simply look at how the global mean temperature fluctuates without any forcing by aerosols or anything -- solar constant. You just simply put it in there. You can see that variability of model is comparable with actual. It is slightly small because there's no amplitude as our model is small. But, essentially it reproduced natural enforce of variability of global main temperature which is usually superposed in the warming trend. So, if you did trend it, you'd know that not only (inaudible) run the surface, their temperature, but also models simulate the global mean surface temperatures. And, it is oddly enough that the physical mechanism is basically coming from gaining weather discrepancies. And this is what [Klaus Haserman?] first used it -- tried to explain the Ocean temperature variability. But actually, it's probably worked better in the land variability. And so what happened is that daily weather discovers -- if it comes once every week then you have a (inaudible) of seven days. But weather disturbances never come exactly seven day increments -- seven say increments. Sometimes you'll get the cyclone wave along the occluding front. Front is basically line up the fifth of cyclone waves.

STOUFFER: Yeah.

MANABE: Tiny cyclone waves form their own front, and that goes (inaudible) I think. And so, you'll have the disturbance time scale with all frequencies. So, any of the weather disturbance variables, if you are analyze it, you get the white noise -- which means you get an equal amplitude at all time schools. And so, if you force this from the atmosphere by daily weather disturbance with white noise, you've got daily noise disturbance because a very high frequency, makes the [area?] ocean just can't feel it. But, with an actual time scale of longer than one year or so, mixed [area?] fully feel it. Over continent, (inaudible) at all time scales. And so, you got white noise response to. So, longer than a year or so basically this fluctuation of global mean temperature which we are discussing is basically the response of the Earth's surface to the daily weather discrepancies which we have. That this realization of this is kind of obtained from a 1,000 year integrations.

STOUFFER: Yeah.

MANABE: However, what is really interesting is that (inaudible) ocean have some time scale which you want to fluctuate. In time on your time scale, where (inaudible) is. So, suppose you [know more than?] at least this is the big chance of Texas A&M analyze our result and find out -- is basically what happened is you put the white noise force in your daily weather disturbances. But ocean choose a certain frequencies so that it's kind of a (inaudible) but it's a dumped system. You get a [results?] but [result?] is dumped. So, our modern (inaudible) is basically dumped results after an annual timescale in the tropical Pacific Ocean, forced by the white noise forcing of daily weather disturbances. And the Atlantic Ocean also has multi-decadal normal mode of oscillation of overtime saturation. And when you force them with white noise, then it chooses the normal mode, not he multi-decadal timescale. So, if you do this things and you analyze it. Most risk of Pacific -- you find that it looks more like the first -- this photo of the original (inaudible) series. But if you do it more than north Atlantic, you get a very gulf regencies -- very dominating gulf regencies at multi-decadal time scale. And this was found out by Alex Hall who was also -- not only he analyzed the thousand year experiment, but also he analyzed [motor-ship?] time series of serenity and temperatures. And because a multi-decadal time scale oscillation is produced by ocean cyclation, it's not simply a random noise -- white noise response to white noise (inaudible). But at some scale, ocean cyclations start responding in large amplitude so that at that time scale, serenity and surface serenity and temperatures are correlated at multi-decadal time scale so that [Haseman's?] theory can be extended. The first way to do it is just what he said originally. But then, one can extend at some frequencies also have more than are just response strongly than other frequencies so that majority of SSP variability of the ocean may be able to explain Haseman's but then you have the huge low frequency oscillations -- which is (inaudible) of an ocean which is excited by random weather disturbances. And so, this thousand year integration, as the more we analyze it, more we think about it, it's a profound insight -- at least to me -- to understanding what are the natural variabilities. Now, people try to explain all of these natural variabilities by external forces like sunshine and aerosol, and volcanic aerosol. Yeah, they may -- volcanic aerosol particularly -- may have a comparable magnitude with daily weather disturbance forces. But, try to explain everything with all these forces is emphasis is really misplaced because they are comparable magnitude. That's why temperatures start to fall down before volcanic eruption.

STOUFFER: Okay.

MANABE: And I'm a little critical about (inaudible) centers and other people who try to explain every wiggle by (inaudible) to forcing which is too much wiggle watching in my opinion. In the entire IPCC men come up to me right to bring on [hardly center?] -- but entire IPCC is working extremely hard to do away with wiggle watching. But, when Tom Bayer was first result in it forced him, he happened to get one realization which is almost like the temperature fluctuation of the last 100 years. The -- so, this shows that you don't need to do the forcing in order to get that kind of temperature variability. But that doesn't mean that this forcing is not operating.

STOUFFER: All right.

MANABE: But, I think that people who have seen the two forget about the contribution of daily weather disturbance in the global mean temperature.

STOUFFER: Well, one way to summarize the last twenty years of your career is you've been using the model to --

MANABE: Yeah.

STOUFFER: -- ask interesting questions. And one question is, "Well, how do you find those interesting questions to ask?"

MANABE: Yeah. So, this is where it -- the thing that -- they never use them more than for studying phenomenon which that model is not simulating.

STOUFFER: Yeah.

MANABE: People have actually done that you know. So, this is -- I think -- is, for example, when we got to the simulation of the monsoon, and then we started taking the mountain out. Or, when we start simulating tropical precipitation and then find out that that precipitation seemed to beautifully, qualitatively follow the pattern of SSP variable. Simulation of this is they follow variables of each other. So, we start digging in and try to analyze how ITCZ is -- follows maximum SSP. Almost. There are some exceptions, but it's more risk follows. Why is it? And so, models have almost an infinite amount of data. So, you can analyze what is going on --

STOUFFER: Right.

MANABE: -- and found out that ITCZ is essentially a chain of tropical disturbances -- it's not a two-dimensional, random flow. A tropical cyclone is. It's unstable in the neighborhood of marginal surface temperature, which then, in time, produce heavy precipitation there. And when you time average, you got ITCZ. But, according to studies about (inaudible) bounce out of it, that [most of that skin?] gives you ITCZ, which is almost like two-dimensional without even time averaging. But real ITCZ in my opinion is produced by the wave disturbance which become unstable around these ITCZ because of an inference of SSP distributions and [MASSFLAX] scheme is doing great damage in distorting the way ITCZ forms in this region. That's my opinion. But this is my singular opinion of mine against the entire world. So but anyway this is probably OK to talk about dogmatic opinion about it.

STOUFFER: It's a good forum for it. One thing that happened in the last 20 years was that you had a much bigger group of people working with you here in Princeton than you had in Washington or even early years here in Princeton. How did you manage supervising and keeping track of what everybody was doing?

MANABE: At the beginning -- of course I start working for Joe, but then he become very busy, global atmospheric research programs, and he become a key member of this global atmospheric research program, extending weather forecasting, integrating all these remote sensing, all means of observation. And so I inherited everybody who are working with Joe. And so [port of wide] what I was doing is to get Reese Halloway to translate, not being able to manage people, coming from Japan, and particularly I'm not good at that anyway. So Reese Halloway to translate. I write down, jot down equation, and Reese Halloway translates the --

END OF TAPE 2, SIDE A

MANABE: Finally settled down to offering job to Jerry. Joe and I were discussing. And [less than last time who is] willing to support our efforts through Atomic Energy Commission or whatever. And he has been supporting us. One of the few outside grant or contract we have. So we decided to offer him a job. And so at the beginning I was very excited about this and so I actually prepared a four-dimensional data tape out of Barry Hunt's [GCM] studies. And (inaudible) not very (inaudible) but it's global model, which we later developed. And so four-dimensional tape and each one six-hour average time average flow sheet. Which was very hard to (inaudible) but I am not going into detail. And so by the time Jerry arrived at here he's already have this -- a tape data, which have a few time integration of atmospheric circulation. So that he can do a tracer study. And so I was very excited. And started doing the tracer study (inaudible).

STOUFFER: Was the tracer model called GASP?

MANABE: GASP. See, that tape I used for GASP, which was a very successful project (inaudible) still using. And that was a longtime project. May not be still using but he used it for many, many years.

STOUFFER: Bob Moxom I think still runs GASP.

MANABE: Yeah, and it's very successful. And so he started working on it and publishing several interesting paper, and then we also start developing what we call now Sky High. So by the time Jerry came I was working pretty hard on Sky High.

STOUFFER: Sky High is a middle atmosphere model.

MANABE: Yeah troposphere all the way to middle atmosphere. And so Jerry (inaudible) that Sky High model from me, modified somewhat. Basically I had made a skeleton of that model at that time. And so his project had a nice head start, and I was very happy, and he was very excited about it. So things going very, very smoothly. But then after a few years Jerry said, Suki maybe you better let me do the whole thing. Right? And I was very counting on him, but I wouldn't see why that I would sort of retreat entirely from the stratospheric project at that time. But because I saw the stratosphere had so many interesting features. One of the fascinating problems. And so I have a difficult time to decide what I'm going to do at that time. But then come to think of it, Joe is getting very busy. I have a (inaudible) work to do on the climate model. And handwriting is on the wall, very soon I just can't possibly do stratospheric project at the same time as I do climate research. So maybe Jerry is giving me a great opportunity in terms of focusing on climate model, which exactly happened. So I completely entrusted to him on the stratosphere to Jerry. And then I decided to focus on the climate model. And I think now looking back I'm glad I did it that way. And Jerry was very happy that way too. So it's everything worked out beautifully. So after maybe 1970 or something he was in full charge of the project and started producing many interesting results. So that's how the first part of the interactions with him came out to be. Now the second half --

STOUFFER: Is another story.

MANABE: Yeah. Second half -- of course he ran for director and so he become a director of lab in 1983 or something when Joe retired. When he was around 60 some age Joe retired. So Jerry took over. And of course obviously he really appreciates the kind of thing our project (inaudible) of course he's interested in his own project, but he really appreciates the scientific (inaudible) not only scientific but also practical implications. So he gave us full support of our project. I think I am very happy (inaudible) he supported our project. And then of course he gets more and more increasingly more interested in practical implications as well as scientific implications (inaudible) and so it's amazing that not only he gave full support but also he actually read every paper we wrote. And furthermore I can tell how carefully he read it. And his comment was more useful than anybody I can think of. And excellent comment he gave me. Which was very useful for some places you don't articulate enough. So I think I know what I was talking about, but when somebody else read it it's not clear. And he was masterful in pointing out all these things. And so he was very effective to helping us (inaudible) great to help us in writing a book, help us. I think what was important is I tended not to write enough about the implications of our research and he was very good at if I not discussing enough the implications of our research he pointed out what is the bottom line, what is the punch line. And so I really appreciate what he did. So it was kind of -- somebody you're supervising went up above you, and it doesn't happen in Japan, but it's very fortunate in the United States this kind of thing happens very often, and I never -- I didn't run for director when Joe resigned. I don't want to run because I never good at looking after other people's -- if you become a director of an institution, have to be good at -- reasonably good at look after other people's things. I worry so much about my research, the last thing worry about is personal affairs, and never good at it. And so I never run for director. So it was very fortunate that Jerry become a director. And also I think another great thing he did was to communicate the result we obtained to outside. Nobody was more effective (inaudible) communicating our result to outside in the congressional testimonies and many other things.

STOUFFER: Just to the press.

MANABE: Yeah. So there is some similarity between Joe and Jerry in the managing laboratory. But they helped us in quite different ways, which I am grateful to both of them.

STOUFFER: One thing I neglected to ask you about, I just realized it, was during that first 20 years the lab moved from Washington, DC up here to Princeton. How did that impact you? And what did you think about the move? Were you for it or against it?

MANABE: Yeah, of course I was in a good environment in Washington. So why have to move somewhere else? That was the first reaction. But then come to think of it, this is really opportunity that go to Princeton and you get the topnotch graduate students and teach them and collaborate with them. It's great. And also I thought that working with -- in contact with Princeton University faculty, which considered to be top in many of the field, would raise the standard of our research. Rather than talking about job shops in Washington and then some weather (inaudible) headquarters want something, and then try to do that as they requested, but by being in Princeton you can get away a little bit from the Washington bureaucracy. And at the same time you interact with topnotch scientists. Some of them are Nobel Prize winners. Some of them are Fields Prize winners (inaudible) us in a very nice way. Thereby raising our standard of our research. And we also thought that -- we meaning particularly (inaudible) I think. And the coming here would enhance our ability to recruit some topnotch scientists from other institutions, people who may not come to bureaucratic US Weather Bureau but they may be willing to come to Princeton University. Somehow affiliated although we are not full-fledged faculty here, but I think this arrangement turned out to be wonderful now in retrospect because we never -- I have to tell you I never had to write a single research proposal. And so that I'm not very good at convincing other people how good my idea is or proposal is. For some reason people don't appreciate my proposal at the beginning. Later they may. So it's great that I didn't have to justify my proposal. Now I can think about (inaudible) you can come back next day and start a numerical experiment immediately. And this is such a great thing. Nowhere anywhere research environment is as good as this. And without writing single proposal my group has been spending about $2, 3 million a year adjusting to present dollars. Maybe about $3 million a year.

STOUFFER: Or more than that actually, these are old dollars.

MANABE: Right? And for 40 years if you multiply the $3 million for 40 years, that means add up to $120 million. And that's how we managed to spend without writing single research proposal, which is a really, really amazing thing I think.

STOUFFER: OK, I think we'll get into the 20 years of your research period now that I know, I got to live through. So when I came here you and Kirk had the idea -- I never knew whose idea it really was to build a mixed atmospheric [mixer ocean] model. Whose idea was it? And say why you had that idea.

MANABE: Originally you see that we have the first GCM study of global warming, which was published in 1975. And in that model ocean (inaudible) swamp. So ocean has -- the model ocean is like real ocean in the sense that infinite supplier of moisture. However, it doesn't have any heat capacity. So if you undergo annual variation you get a huge seasonal variation with huge amplitude.

STOUFFER: Temperature.

MANABE: Yeah. Surface temperature goes up and down like crazy. So that's why in that experiment I put annual mean sunshine. But in order to really study the impact and how the impact of increasing greenhouse gases, CO2, without realistic seasonal variation you can't do it. But fortunately we know that ocean is well mixed on the top 50 meters or so. And so that makes it very convenient to have just mixed (inaudible) ocean. So that's how we put that thing in. Now so about that time you came here and this second 20 years as you say, you made a major contribution, because not only you developed the mixed (inaudible) ocean but you developed -- let me put it this way, you made the mixed (inaudible) ocean work effectively. It didn't work before you came here. And you made (inaudible) ocean atmosphere model to work with infamous (inaudible) adjustment and all. And so you made a really key contribution. I think without your contribution our work in the second half here much less than --

STOUFFER: Sure a lot of fun.

MANABE: But (inaudible) some subtle thing, but with you something makes you somehow have some -- what you call -- some ability to make things work. Which you did somehow getting out of trouble. I'm sure you had a fair share of difficulty you encountered but somehow --

STOUFFER: The first [mixler] model I ran, Suki, had a code error in it that was a factor, I had minutes and seconds wrong, and I got sea ice down to Hawaii (inaudible).

MANABE: Somehow to me you somehow miraculously get out of us out of a hole. And in both cases (inaudible) ocean and (inaudible) ocean atmosphere model. And so you got to have some special talent in getting us out. So coming back to [mixler] (inaudible) ocean (inaudible) originally it wasn't cited that well but now recently I look at, it's now getting cited pretty well actually, particularly not the Nature paper but the paper which was published Journal of Geophysical Research. I'm so glad that it had been recognized in terms of number of citation at least.

And this model has one of the important thing is polar wind amplifications. Which is strongly controlled by sea ice. We discovered this very, very interesting phenomenon. Which I think increasingly will people get more and more attention of Arctic. And I'm sure that they will start thinking about this important polar amplification, particularly in winter.

STOUFFER: It's funny you mention that. I was just reading a paper today where somebody rediscovered in AR4 runs that we have an early winter maximum and warming in the models.

MANABE: People I'm sure rediscovered because yeah anyway so that's one thing. And also we discovered that large warming over continents and oceans, also another thing is we discover in the model at least midcontinental summer dryness, which I think is happening. Now people more and more, people talk about subtropical dryness. However -- which we got it for a long time, though we didn't emphasize. But midcontinental summer dryness is one of the important findings of these studies. So there is a quite nice result. However, about five years later, several years later, let's put it that way, Jim Hanson and collaborators of GIS come up with a beautiful paper, which is a really truly pioneering paper. Discussing the issue of climate sensitivity and so forth, which is main conclusion is quite different from, mutually complementary with our earlier papers. But one of the things which Jim Hanson and others proposed is the method called Q-frax. And as you know this mixed layer -- atmosphere mixed layer model simulated reasonably well the climate. But if you look at the detail now I think what happened is the mixed layer model you don't have ocean current, oceanic heat transport. So without some kind of effective ocean heat transport you shouldn't get realistic surface temperature distributions. And I'm sure that if you look at the new -- there is many shortcomings, particularly the temperature -- yeah many shortcomings. And Jim Hanson and collaborators came up with this Q-frax method, which makes temperature realistic. Q-frax has two effects. One is representing effect of oceanic heat transport. Another one is to counterbalance systematic (inaudible) so we thought, hey this is wonderful ideas. And as you know we have developed -- we are developing (inaudible) ocean atmosphere model. But simulation of (inaudible) ocean atmosphere model never right. First of all we didn't have meridianal overturning circulation in the Atlantic Ocean and also sea surface temperature isn't quite realistic. If you have half degree centigrade error in sea surface temperature in tropics your rainfall is completely in error. And model wasn't good enough so rainfall isn't quite right. And I said my God we can use this Q-frax method, apply to (inaudible) model. This solves our problem of (inaudible) ocean atmosphere model we had long time.

STOUFFER: Made the model good enough to use.

MANABE: Yeah. And so the only problem I have is I didn't -- when we write a paper we didn't explain it in a way people think that this is a wonderful method. And it doesn't help that the similar scheme was proposed by Klaus Hasselmar. He quoted (inaudible) adjusted frax is no more correct than unadjusted frax. Therefore frax correction does not describe. Maybe have described his method but not ours. And now I am convinced that this frax adjustment technique is a very ingenious method of doing nonlinear anomaly model so that you make things fracturate around realistic state and without linearizing equation. Usually anomaly model you linearize the equation around realistic state. But perturbation equation you create. But this is a nonlinear equation. Still do the same thing as anomaly model, which perturbs around realistic state. And so what I recommend now is when you do your best improving (inaudible) model without frax adjustment and then at the end then you put frax adjustment, then the model is the same model with frax adjustment is much better than the model without frax adjustment. Making sure that the equatorial SSP is realistic so that you have realistic rain and controlled integrations. Now some (inaudible) circulation comes back does not necessarily say that you don't need frax adjustment. Because tropical SSP have a critical influence on the rainfall. So what I'm saying is it's much better to have it than the one if it was same model do it with frax adjustment because it's better. But anyway so this is I think one of the secrets of -- not the secret, but it's although other people don't appreciate it, because frax adjustment is so infamous in the world. People get paper rejected by using it. But it is essentially same as Q-frax model, which people use without complaint, and because they say it's because representing the effect of oceanic heat transport. So it's critical. But they don't mention the other part of Q-frax, which is systematic counterbalancing systematic bias of the model. And so anyway this leads to this paper, which we published. Three-part paper. One first we published in Nature. We discussed later. And two other paper published in journal Climate, which describes this [pandependent] change. And I think this is probably one of the best contributions we've made, these three-part studies there.

STOUFFER: Right. Then the first by PPC projections chapter is totally based on that model.

MANABE: That's right.

STOUFFER: The whole chapter is devoted to that model.

MANABE: Yeah, what happened is originally Kirk and Michael Schlesinger supposedly author of that chapter, and realized they found hard about our work, and they added Frances Brellatin to write that chapter for us. This is nothing can be better because somebody else with brilliance and skill of English language like Frances Brellatin write about it. So he wrote a beautiful summary of that chapter and since that time I think this become a prototype for these. And some of the most interesting discovery is that there is large delay in those region of oceans, delay in the warming, those region of ocean where there is a deep vertical mixing. And it delays by many decades and more, particularly in the circumpolar ocean.

STOUFFER: We're about done here too.

END OF TAPE 2, SIDE B

MANABE: Jerry maybe already a director. But they have begged Jerry not to send Suki Manabe, because if you get a transcript, when I say climate, stenographer typed it, it's crime. He's talking about crime, not -- crime science, climate. And the whole thing, my testimony, somebody told me, Suki you better look at the senators and then speak and talk to them rather than reading this. But that was a disaster because the instant I start speaking, my English was Japanese broken English, sentences not complete. These senators originally, many of them lawyers. So they learn how to speak logically with perfect sentence and so to them this guy cannot be trusted. Or may not be competent enough. But very fortunate thing about it that Jerry took over and he did a masterful job in testifying, explaining to them in layman's term what is the issue involved in global warming and it's impact. And so it was very fortunate that he become a director around that time. And once he performed well then he was asked again and again. They never asked Suki Manabe again. Which was very, very fortunate thing that happened.

STOUFFER: Another thing that comes along with fame is your getting asked to serve on lots and lots of committees. How did you pick and choose what committee to be on?

MANABE: I just usually said yes to almost all committees. But somehow I have so much thinking during committee meeting, I'm so much thinking about my own opinion about the issue. Then as I said earlier my ear shut. So I don't understand what other -- I don't really recognizing what other people, what is other people's viewpoint, right? So then whatever I have -- finally I come up with a good idea. But explaining your idea without acknowledging other people's comment, then your comment also is not -- they don't listen to your comment because oh Suki is -- he didn't even listen to what I said earlier. And so their ears shut off. So in the committee meeting I never very good at listening to others and understanding what other viewpoint is. Therefore I have great trouble when I got writing assignment, because writing assignment you have to incorporate other people's opinion, right? So then I kept struggling, struggling every night after meeting finished and go to hotel and you start writing and then you don't like it, put your writing in a paper, in a wastebasket, and start writing again, again and again. But obviously you don't have good thing to discuss because you haven't listened to except your own opinion, right? And so I had a terrible time, particularly with jetlag and so forth. So I never -- I was asked many, many large number of committees. I never asked to serve as a chairman of the committee, which is very appropriate. But this gives me ample time again focus on my research. Every one of these, my shortcoming, helped, enabled me to concentrate on my research.

STOUFFER: The other thing that comes with fame of course is awards. How did you handle? You won lots and lots of awards in your career. How did you handle?

MANABE: So actually, come to think of it, Joe was very nice in promoting me at [GFBL] and so (inaudible) I got the GS 16 at the age of 35. So he must be doing great effort promoting me at that time. But outside award, the only award I got (inaudible) is Meisinger Award, the 1966 or something. Which I was in -- because Joe thought that I should be in along with [Arakawaz] and so forth. So I got in but outside [GFBL] I didn't get much recognitions. And as I told you the coupled ocean atmosphere model, it's good, but some of topnotch oceanographers thought that this is really a naive things to couple these two unknown. And the modeling didn't receive the attention it deserved for long time.

STOUFFER: That all changed then in the second 20 years.

MANABE: That's right, gradually. But what it is really, the absence of good recognition, oh geez, Suki and his group is doing great work, this is really excellent work, or something like that, then I would be very happy. But nobody say that except Joe, who already said nice things about it, right? But outside recognition didn't come. For example I got the Raspy Award in 1992 or something. Much much later time. And started getting all the money-producing award like Bloomtenant Prize all coming in 1990.

STOUFFER: When did you become a fellow of the AMS and AGU? That must have been earlier.

MANABE: AGU come very early, 1966 or 7. 6. Because Sigmund Fritz, with whom I worked at the beginning of my career at the Weather Bureau, he said put Suki in. That's how I got in there. But didn't get very much award in the early part of career. But then start coming like anything at last ten years. But I think that the absence of -- they thought that I probably done well enough to give him a fellow of AGU or thing like that. But it's not like some of the star, when they're young like [Ju Chagi] or something like that, you've got huge recognition when they're young, or [Dick Greentent] or any of these people. Very early recognitions. And I don't get that. Nobody thought I'm a very brilliant upcoming star. But yes they gave me some award until 1990. And probably that was very good because that sort of prevent me from becoming too complacent and keep going -- of course the main motivation is if you enjoy your work, whether you get the award or not, that doesn't matter. But which is true, but it's always nice, and if you get (inaudible) big star or something and he's brilliant and so forth at very early stage, you may get more important assignment like committee chairman or directors and all sort of thing, none of which I got, which is sort of -- so that if you look at my citation you can see that citation, I got a total of 11,000 citation now, but they come very gradually. None of them are explosive citations. Over the years slowly keep adding up and adding up. And that is a kind of way my contribution or collaborative, almost all my work is collaborative. I seldom did my single author's work. And so but the recognition of the value of models come very late. Still I think a lot of people think the modeling is something which people who doesn't have much brain does, right?

STOUFFER: I think that's changed. I think in the last few years that's really changed.

MANABE: Modeling is not a science. But you can see that it may have changed, but over most of my career modeling is not considered to be anything which most brilliant person supposedly engaged in, and which probably turned out to have a very good effect on my psychology. I kept (inaudible) pushing, pushing.

STOUFFER: You mentioned citations. And one question I thought would be interesting. Of all your papers, you have well over 100 papers, what would you pick as your top five best?

MANABE: Yeah, so this is a thing I have here. And the one I have here is this first one is a paper which we published in 1965, which is one of the papers which we published, first nine different model, which Joe's big project led to this paper. And this is a 1965 paper, simulated climatology of general circulation model with hydrologic cycle. Published in Monthly Weather Reviews. And this paper's sort of the hydrologic cycle. Chuck Grief did some movies out of it. But real simulation of hydrologic cycle is this is the first one. And I think this is an important step to changing dry general circulation model into climate model. And I think one of the important thing is also it start what is the role of water vapor in controlling dynamics of atmospheric circulation. And this paper for the first time seem to say that moisture, heat and condensation is a source of -- energy source of tropical disturbances. Most of the analysis is mid latitude analysis. So where convergent of heavy (inaudible) potential energy into heavy kinetic energy you get (inaudible) middle latitude. When I saw this paper I see another huge maximum right around the equator. And that's the first time I see this huge (inaudible) of potential energy, which is an energy source of tropical cyclones, which in turn produce the heavy precipitations and the tropical rain belt, so that this is a sort of [eddy view] or ITCZ rather than the zone or lamina, two-dimensional ITCZ. And this is I think -- but people still don't appreciate this kind of viewpoint, I don't think that in a textbook it's really treated that what is role of moisture in the tropical circulations. So this is first paper. Second paper is this pretty well-known paper, it's about this radiative convective equilibrium and this we discussed enough already but this is I think is first study of global warming, which stood the test of modern scrutinies. And key of this is the inclusion of the effect of convection. Convective heat transfer. How they change associated with global warming. And so that issue becomes particularly important when you consider the water vapor feedback. And so this paper is probably the best paper I ever produced. And citation classic papers by ISI. And 671 times. So it's pretty good in this field. Next one is of course this Manabe Brians paper '69. And the interesting thing is this paper is actually -- this paper, the first time discussing how the oceanic heat transport affect not only temperature but also precipitations. And because of sector geography, [Kowp] was able to put a very high resolution in the western boundary. And it clearly shows how sea surface temperature atmosphere, sea surface atmosphere interaction.

STOUFFER: Where are we in this?

MANABE: I think we --

END OF TAPE 3, SIDE A

MANABE: So --

STOUFFER: You're talking about the Manabe weather (inaudible) convective radiative equilibrium paper. Oh no, you were into Kirk Brian, you're right. You're in Kirk's sector.

MANABE: So it shows that how for example [up putting] of equator affects precipitation above it so that the interaction between how the ocean atmosphere interaction control the distribution of sea surface temperatures. And this is the first time we discussed these things. Which we discuss later. So although this paper not cited very well, maybe 80 times at most, but I think to me it's a very important paper which lead to many other studies which come later, and that's why I put this paper here, and very few people know about this paper. Although they may start recognizing it. The next one is this Manabe Weatherall, the 1975 papers. And the effect of doubling CO2 concentration in climate and general circulation (inaudible) published in JAS. And this paper is the first three-dimensional model study of global warming. And it's very interesting. For example it discusses how (inaudible) latitude upper tropospheric warming is very large, and how the near surface temperature warms in high latitudes. It also discuss how not only warming, how hydrology changes for the first time. It discusses in a sense subtropical dryness already. And so this is still -- it's a highly [idealized] geography, but I think this paper belonged to this, one of five papers I have here. The next one is a paper which you and I worked, and this is a paper which we published in Nature, and talk about inter [ham spheric] symmetry, which we obtained by global GCM. And I think that as we already discussed this study has a huge impact, and as you pointed out, it's a major part of this discussion of model projection. First ITCZ report and one of the interesting things is I added the total citation of three papers, which describe these things. And this, the paper which we published in Journal Climate, that paper, 1991 paper, that has the biggest citation. When you add them up you get 874 times, which is probably the best-cited work. One of the best. I think the only person who get this kind of citations is Mike Wallace, you know like what's it called North Atlantic [ressurations] or PNAs, so forth. Very well cited.

STOUFFER: We're coming on to the end. And I was curious what you thought you could have done better.

MANABE: Groups that I didn't do. I hardly do any administrative work at the GFBO or (inaudible) so all my time I spend is research. And then at the place where the ideal environment, I have nothing to complain, and the people I worked with did a wonderful job. Their contribution -- as indicated by my -- all the paper I published, almost, most of the paper I published is collaborative work. I seldom have done none of them by myself. And so beautiful collaborators and beautiful research environment and during my 40 years career computer continued to increase exponentially. Which is amazing phenomenon. So I try pretty hard myself. I don't think I could have worked any longer, any harder. So given my limited ability I don't think I could have done better. That is I give just about everything I needed.

STOUFFER: You had a perfect environment.

MANABE: Perfect environment. And as much computer time or as much money I need, right? Funding, amazing. Joe was always sending me (inaudible) Suki you spent $3 million last year and he was adding personal costs (inaudible) not only computer but --

STOUFFER: Well but you were getting half of the machine time for a long time.

MANABE: Long time. Because allocation maybe 30-40% more recently. However, then we get brownie point, what you call the --

STOUFFER: Unallocated time.

MANABE: Yeah outside allocated time. Huge amount of computer. So I don't think I can do better. So that what my conclusion is.

STOUFFER: Part of that perfect environment of course is your family and your wife. I wondered --

MANABE: Yeah. So she did excellent job. Raising our kid. And both of them doing very well. And older one is giving three (inaudible) more than I am (inaudible). They got excellent education. And the younger one is doing very well. So children education, mostly my wife did it, but she did a wonderful job. And the rest of the family matters, she takes care of it. Because I don't hear anything, I don't want to hear. So she more or less makes most decision and her decision turn out to be excellent in terms of family affairs. I can't -- and I claim very little credit for my kids' education. But she did wonderful job. And so actually without worrying about anything about home finances, she handle all the money, she give me allowance of $20-30 a week, and so I don't know how she's spending our money but she handle it beautifully. Getting through these private school education, sometime two of them overlap. So that's the current money, that is $80,000 a year.

STOUFFER: Lots of money.

MANABE: Yeah all the way to graduate education. So I really my research career would have been much less than what it would be without her help. It's nice looking at my son-in-laws. They work much harder for family affairs and do much more than I did. But she seemed to almost single-handedly take care of it. And when they have a crisis she suddenly gets a sort of what you call you know crisis management, her brain suddenly start working. And remember when we are driving back from Cape Cod in old days, always went to Cape Cod. And my Volkswagen has heavy weight on top of it, it's Labor Day, we are coming back. And then some truck blow the horn because I try to change lane, I suddenly start changing steering wheel and my car gets completely out of control because top-heavy. Start going. And as I go there and truck went by, I go back, another truck went by this side. And at that point she took care of handle. And somehow she managed to control the situation. I was just like this. So really grateful to her. And without her I couldn't have done anything very much, nothing like what I achieved.

STOUFFER: My last question is one. What advice would you give to a young scientist starting out in our field now?

MANABE: Oh (inaudible).

STOUFFER: Well, you want to do that one? OK, go ahead. What's your view of global warming and your role in it?

MANABE: So the thing that -- as I told you that I wasn't that great in communicating in general public. Somehow not that convincing. And furthermore if I say something and then if you print it in the newspapers, something, and if it doesn't come out right, I can't sleep. And so earl on I decided that I really -- I would like to keep as low profile as possible. And by not getting too visible in public in contrast to somebody like Jim Hanson who did beautiful job communicating, which I don't have any talent for. So if I have to worry about what I said in public or I can't stand some people like Dick Vincent, right, gang up on me, or that may not be right person I cited here, but anyway, I just don't -- I don't want to have a sleepless night in getting too public on the issue which is very sensitive to the general public are very sensitive to. And so I decided because I don't want to get involved, and if you do it half-cooked way you lose your credibility and so you're better off given limited ability I have, you're better off not to get involved, but focus on science and if you do good scientific research eventually people will recognize it. And then eventually that go to public and by somebody else who is much better in doing that such as people like Jerry Marman or people like that. So that's what I am convinced of myself that I should do.

STOUFFER: OK. My last question was or is what advice would you give to a young scientist starting out in our field today on trying to find a path to make a career in our business?

MANABE: I think the science of climate is one of the most exciting fields. It's full of fascinating questions such as why you got ice age, right? Or I think global warming is another fascinating issue, which was chosen top ten questions, in the 125th anniversary volume of Science, and people don't seem to realize how exciting this field is. Because they are so busy in daily responsibility which they have to what you call -- which they have to do in order to survive even. And then so they lose sight of big picture, particularly I'm talking about modeling field of climate. Lose sight of big pictures. And very soon they forgot they even lose their curiosity. How fascinating this phenomenon of climate change is. And why climate behave the way it does. And so the important thing is to no matter how busy may be, but be curious. And try to solve the mystery of climate. And yeah that is probably best way to enjoy their life.

STOUFFER: That's a great answer. Suki, I've really enjoyed. Privilege for me to interview you.

MANABE: Oh thanks.

STOUFFER: Been great. Thank you very much.

MANABE: I'm very grateful that you decided to do this for me. Yeah so this is -- I don't know. This --

END OF TAPE 3, SIDE B

END OF INTERVIEW - SYUKURO MANABE