J. Smagorinsky 3/17/98
start side one with counter at 000

J.S.: Just let me tell you one thing. I have dug up some source material that you may be aware of or may not be aware of. Here...almost the most recent thing is a talk that I gave the American Meteorological Society in 1992. It is unpublished.
P.E.: That I have not seen. I was just looking over this essay form 1983, which is an excellent piece of history.

J.S.: Another one is this...
P.E.: I was planning to get a copy of this but I don't have it yet.

J.S.: One of the things you can do is scan it. These answer a lot of questions of the type that you pose. This is an article I wrote in 1981 which is specific on CO2 and climate. Manabe published something later on which brings it up to date. It is interesting to see where the differences and where the similarities are over a 16 year period. But this is Manabe's paper...

P.E.: May I borrow this, your talk, and Xerox it?
J.S. Yes. I could Xerox it here but it is not a fast machine.
There is one more thing...Years ago in the 60s there was an Academy committee put together, and it was to make recommendations on weather and climate modification. That is where a lot of people see CO2 as relevant. Going back to Manabe's role- when he was hired, he was hired not to do anything on CO2, he was hired to do a radiation algorithm, a comprehensive interactive- it had never been done by anybody- pieces of it had been done, but not the whole composite things. And the way this happened, this follows one of your questions, we were discussing the CO2 role and so on. I was on the committee and I suggested to them that maybe one could utilize the radiation model to answer some simple one dimensional questions. This is in the report of this committee-- it was _______? do you know him?

P.E.: No I don't.
J.S.: You don't even know of him?

P.E. No actually not
J.S.: Anyhow. This is more or less an acknowledgment by the committee. Have you seen this book?

P.E. No
J.S. It is of historical interest. There was one guy on it by the name of ________? who ____?. So I suggested to the committee that we had a model that might be useful and then I went back to Manabe and he did a calculation. I don't remember whether it is the Wetherald joint paper with Manabe or whether it was Strickler.

P.E. Manabe and Strickler I think is the radiation paper.
J.S. Well we used that and that was really the beginning of comprehensive calculations on one or more dimensions.

P.E: Let me start asking some questions which are related to the ones I have posed here. But I got hold of this actually just today and I have spent some time reading through it. This is your paper on the beginnings of numerical weather prediction. This is a fascinating piece of history and I have

to say as a professional historian, very well done. You did a lot of dipping into your personal records, letters and things like that and coming out with....
J.S.: I have a few boxes marked 'Ancient' or something like that. And I was cleaning out things and I came across this box, and I said 'gee well I can make a story out of this'. I happened to have been very orderly in keeping old stuff. I got a Xerox machine and brought it into my office, and I just sat there reading and reading and copying some things. It was a lot of fun.

P.E.: One of my questions is what has become of your papers, and have you made arrangements for their disposal?
J.S.: I haven't done anything. I have got a complete reprint file.

P.E.: But that is all published material?
J.S.: Except for two or three papers that are not published but are still relevant that I have in there. I have reprints that take up three books. This is my bibliography- this copy is for you.

P.E.: I was going to ask you for something like this anyway.
J.S.: Well it is not all on climate.
counter at 123

P.E.: I would like to ask you a few questions about things that came up while I was reading this paper on the beginnings of numerical weather prediction, because this really lays out a lot of the early work very nicely.
J.S.: One thing that really baffled me, is I can't remember seeing Charney's name once in the manuscript. And the man has far more credit than anybody except Phillips, more than ________?.

P.E.: In the beginnings of general circulation modeling?
J.S.: Yes. Then when they moved to MIT in 1955, and changed their emphasis...

P.E.: What happened when they moved to MIT in 55, where did their emphasis go after that?
J.S.: Not on numerical weather prediction. Norman ? Phillips did some things- well, he went to MIT and then left again to Washington into an operational environment.

P.E.: Toward the end of this piece, one of the things that you talk about at some length is your frustration in the late 40's and early 50's with the data that were available then for a large scale numerical weather prediction. And I know that you were heavily involved in GARP later on. One of the things that I am interested in is the interplay between data sources and models. Clearly, you were working on these numerical weather prediction schemes that needed data at, you say in here, at least a grid of 500 km and preferably better than that. Were you involved, before GARP, in any planning for data collection changes like the IGY?

J.S.: I was involved in a study that was done by the WMO. It was with Sawyer from the UK, and Aleason? of ___?. See "Data _____? and data requirements of numerical weather prediction 1960". It is number 7 here (on the bibliography?).

P.E.: What influence did this study have on the course of data collection?
J.S.: We tried to get a plan where ships could launch balloons. See the big problem is that the data goes very very sparse over the oceans and the Southern Hemisphere and then suddenly clusters where there are continents. And this gives problems. We don't want any less on the continents but we definitely have a problem over the oceans.

P.E.: At this point the major source of data over the oceans were commercial ships and weather ships?
J.S.: Well, they were weather ships but only maybe 10 altogether. They were good instrumentation, radio____? essentially. These were internationally run, but ships were the property of their own country. I don't know the situation for weather ships now, but with such gaps you couldn't have had a feasible operational system. You just couldn't do it. So that was given high priority. After awhile money was withdrawn by a lot of countries. So you can have a fine forecast system but you can't use it. So that was a problem. I wrote a paper, incidentally... Anyhow, the big change that really occurred, occurred due to GARP. Platforms and sensors were somehow used to cover the whole works. In order to make them all coherent dynamically, it was necessary to first devise and then use an assimilation program that takes heterogeneous data, data types, and dynamically- by requiring dynamical consistency whether you go frontwards or backwards... it is called four dimensional assimilation- you can build up data within these ____? periods...

P.E.: So it is a model that interpolates between..
J.S.: It interpolates dynamically.

P.E.: So not only in space but in time?
J.S.: Yes. And it is not purely statistical.

P.E.: What does that mean exactly?
J.S.: That you use a dynamical framework explicitly if you do it.

P.E.: So you are using a sort of partial weather model?
J.S.: Yes, a full weather model

P.E: A full weather model to make the data....
J.S.: And there were tricks to going backwards and forwards. What you do is you build up areas of more and more information in them. That was on of the things I worked on.
counter at 258

P.E.: I am very interested in models like that. That is still a major way..
J.S.: Four dimensional assimilation is getting much better. You have to understand that I retired 15 years ago so I am not in touch with everything.

P.E.: When did you complete the first model of the four dimensional assimilation type?
J.S.: About twenty years ago.

P.E.: So 1978, somewhere in there?
J.S.: Yes, it was a subject of GARP. GARP was 1969-74 or something- don't take that literally, it can be checked.

P.E.: In the 50s, when the IGY was happening- wait, back up a step. Another thing you say in this paper- here you are quoting from a 1953 Memorandum to the WMO to Tannahill, you are talking about the fact that observations on the surface and in the upper air happen at different times. And you are sort of wishing for simultaneous observations of the whole system. Well, that was

something that was attempted during the IGY, on the whole global surface. Were those data important for you?
J.S.: Not for me. The only trouble is when we were model developing, we didn't have a good data source to act as the control.

P.E.: What did you use for a data source then?
J.S.: Meteorological? data heavily biased toward continents. You also need surface data. That is definitely not going to be a hell of a lot over the oceans, maybe ten. You can tell roughly what is required at 40,000 data points...

P.E.: Let's go ahead and go down the list of things you prepared there.
J.S.: I also have some notes for the banquet. I haven't even been asked but I should and I use these notes and they answer some of your questions.

I'll start at 1- My early scientific training and when I became interested in meteorology. When I was a young fellow, I already knew I was interested in science. I just had a curiosity about a lot of things that had a scientific base. And I decided that when I was ready for high school that I would go to a science high school, of which there were three in New York- Stiveson, and Boy's High, and Brooklyn Tech or something like this were the three. And I didn't have any specific interests except that I knew that I wanted to continue on. And during my teens, quite independent of anything else except my curiosity, I used to go visit the New York Daily News weather observatory on the top floor. You know what the News is don't you?

P.E.: I don't know where the building is no.
J.S.: It is on 42nd street.

P.E.: In Times Square, that area?
J.S.: It's off Times Square. But I used to go there several times a year. But my real interest was not the weather, but was in the design of ship hulls, which is the same as a hydrodynamics problem except it is more difficult. I elude to this in this paper. But I had financial problems and there were only three places where I could study the design of ship hulls- University of Michigan, MIT, and a place called Web? Institute The last is a remarkable place, they have 50 students complete tuition and everything paid so you can't pay a cent when you are there. The selection process is very very good. They may have 15 slots, 50 is the total population. For the freshman class, they may get something like 500 applicants and only 15 are accepted. And this is after you take a 3-day test. So it is very very well filtered, and there is one meteorologist who is going to be at this meeting that studied ship design when he was there- Mike Wallace.
counter at 383
And that always has impressed me.

P.E.: So there were only these three places that you could study this and you were having financial problems..
J.S.: I was having financial problems and I did not pass the test. I was in the second bunch that took it. That may have been 50 people that took the test. You got to the point where they told you you could take the test. It still exists, it is not where it was located originally, but that is the way to do things. So the Naval architects in general are very well-trained and very effective in what they do. The trouble is, this wasn't picked up as something to try to do- use numerical methods. See, I was interested in numerical methods, to use numerical methods. And even recently, it still isn't done that way. You do it empirically, it is easier to carve something out.

P.E.: Airplane design is similar probably for the same reason.
J.S.: Yes. OK, so that answers one of your questions. Oh, this is how I got connected with the Institute for Advanced?Study...I was at the Weather Bureau since 1949, and within about a year I was off on military reserve activities and heard that some guys from the Institute for Advanced? Study were going to talk about their work. So I went to hear them. And I said this is just what I wanted to do- the way they are doing it. And there were a number of failures before that by other groups. But I didn't know these guys, Charney and Aleason? That is the original group. But after giving a report and really marveled and felt a lot more confident, and they invited me to come work with them at the Institute. So I took a leave from my job at the Weather Bureau. And that was my initial involvement.

P.E.: What was your job at the Weather Bureau then?
J.S.: I was a research scientist. I was not a weather forecaster.

P.E.: And what were you working on at the time you heard them?
J.S.: My boss had some cock-eyed ideas that he put me on and I couldn't make it go at all. The ideas were really bad. So that was almost a two year period- I lost that completely. But I gained it back in ___?

P.E.: So you went to MIT.
J.S.: I went to Princeton. I finished off and did my Ph.D. while I was up here. In 1955, which is when GFDL started by another name.....let's move on.

P.E.: So tell me a little bit about what you remember about Vanoiman?
J.S.: He is not an ordinary human being, that is the first thing you have to say.

P.E.: Everybody says that.
J.S.: He is not just a smart guy. Oppenheimer was a smart guy. But Vanoiman, I would guess, it might be three of five people in all the United States- that proportion, what is it,

P.E.: 250 million or something.
J.S.: Yes, but at any one time, if you could say that these guys are on top of that heap, it is just extraordinary. He had an extraordinary memory. I didn't think he was a completely happy person, but he tolerated fools. It was a real privilege to have been working near him.

P.E.: During the period that you were working with him, was this his primary work?
J.S.: No. He had balls going in the air and everything- running committees, in Washington advising the bomb people, the bomb people and the rocket people. He could do anything. The thing is, the classic that I have heard from a number of sources and you probably will know it- you go to him for one reason or another to ask a question, by the time you are through a few minutes later he knows more about it than you do. He used to do puzzles and computer programs in his head. Extraordinary guy. But you mention him quite often, but I think you should also put Charney in there, and Phillips you do mention I think. It would be a big flaw in your book. Everybody that knows anything about it will say well where is Charney? A lot of his stuff is not directly bearing on climate, but indirectly bearing.
counter at 523

P.E.: The issue of how the general circulation works goes way back. But at what point did people begin to think that it might be possible to simulate it numerically? Is that Charney or is it Vanoiman, or before them?
J.S.: I don't know exactly when they did it, but I can tell you I, in my own private mind, thought weather forecasting was done that way.

P.E.: Yes, you said something here when you went into this line.
J.S.: What I don't remember is the chronology of who did it first, but I can tell you my thoughts were quite independent of anybody else's. As a matter of fact, Horowitz, who was my professor, was very skeptical of the whole thing. He is a brilliant guy, not living any longer. But skeptical.

P.E.: The joint numerical weather prediction group was a collaboration of the weather bureau, the Air Force, and the Navy. You mention in this piece that the funding, the money, mostly came from the military members presumably because the Weather Bureau....
J.S.: No it was one-third, one-third, one-third. But after about a year or a year and a half the Air Force and the Navy pulled out.

P.E.: Really?
J.S.: I don't have that there?

P.E.: No you don't mention that here.
J.S.: They pulled out and the Weather Bureau funded us completely.

P.E.: Why did they pull out?
J.S.: My guess is because they did not have as much control over the program as I was willing to give them.

P.E.: What was their interest in the project in the first place?
J.S.: I notice you ask that question here. Their interest I think was quite practical. Anything that would give them a better forecast they would try to make sure that money doesn't stand in the way of this being explored. I know of no instance where they tried to manipulate anybody. The military supplied fairly senior people-- <u>Lizgarner</u>??, a lieutenant colonel and Stickles, and Navy lieutenant commander. And they stuck with the project right through. I don't know what has happened to them. I didn't feel there was a real problem when we moved into <u>Sudland</u>?, the Navy was already there- they had a place. They wanted to be very close to what was going on. Not so much to manipulate as to carry information.

P.E.: Was there another unit already there in <u>Sutland</u>? Why did you go there?
J.S.: Well, I forget now who was there first. But one of the things we used to joke about is that they are very careful about security and so on. And the big iron pipes going through, and with a little stethoscope you could hear what was going on in another part of the building.

P.E.: So maybe they wanted to keep an eye on you...
J.S. Or an ear, yes. But it was not ominous in any way , or threatening, or ulterior. OK, Three. I told you about <u>Sarkner</u>? and Stickles, I told you that they wanted more skill in forecasting products. You asked whether we used any military computers. We did one, the Eniac?, who is Army.

P.E.: But you didn't use that for very long I take it, two or three years?

J.S.: Well it is an important calculation. It is the first time anybody went out in time. The best that was available were tendency calculations before, that gives you the time derivative at one point. OK, you wanted to know- on four. Well, the basic thing that made it discuss the feasibility was Phillips' model. That was tremendous work, a tremendous work.

P.E.: He did that on the IIS computer?
J.S.: I think so, I think he did some on the Swedish computer Besk. But I think he did...He did the work while he was there, but I am not sure which computer he mainly depended on. You can work on pieces of the problem on Besk without Besk being very fast. OK, now I am not aware of any controversy.
OK, this question comes up several times...Of all the things that require calculations, radiation code, which was very very complicated and consumed time, or any of the other parameterizations--You know what I mean by that

P.E.: Yes
J.S.: is horizontal resolution that almost completely wipes out everything else and I have an analysis of that...
Side ends with counter at 696. Start side two with counter at 000

J.S.: And this family tree that you are talking about, I have one too. But I haven't seen it in a couple of years.

P.E.: Was it ever published?
J.S.: No

P.E.: I would love to see that.
J.S.: It's just the early part, before there were too many different groups involved. You know, maybe the first 3 or 4 years.

P.E.: No, I would love to see that.
J.S.: I'll try to trace it, but it was at least four years ago. You'll have to remind me on that.
OK, now question 5- The most important choice...for the early models...
Well, resolution is one thing. And collectively all of the small scale parameterizations, convection, getting the resolution in the right place. It was all novel. And it is what I call the architecture of the model. There had been no precedent. Well, except Phillips' and my two primitive equation models. But neither of them tried to resolve either the boundary layer or the lower stratosphere that couples energetically. So those are choices that have to be made, but adding one or two levels is trivial compared to changing the resolution by a factor of two. You know why- it is a factor of two because it is two to the third power.

P.E.: Wouldn't that include the level?
J.S.: No.

P.E.: How is it two to the third?
J.S.: Two horizontal dimensions.. In the full spectral atmosphere there are three dimensions, but what we are interested in is the two dimensional part of it, and time.

P.E.: Time, of course. I always forget about that. These models, people talk about them as three-dimensional but they are really four-dimensional.

J.S.: Yes, that is why we call them four-dimensional assimilation: because time enters into it. OK, so where am I?

P.E.: Important scientific choices, and the last part of this question says I am especially interested in the move from baraclinic? to primitive equation models..
J.S.: Well that's wrong, it's not baraclinic its eustrophic?
And another objective, a basic one, was to have stable integrations. No sense having a general circulation model which blows up after a day, a month, or a year.

P.E.: That sounds like the kind of problem that Vanoiman would have been interested in. Did he assist you at all in that?
J.S.: He did some, yes. See, I was only a graduate student there at the time.
OK, six- relationship with Arakawa. I would think that you should call Arakawa.

P.E.: I actually have talked to Arakawa already.
J.S.: He just retired. He was very important in that very small group. And they had a model which was barely competitive, and the truth of the matter was they didn't have any computer time. They got drippings from the rest of the University. But he did a lot of wonderful things including coming up with a very novel algorithm which preserved the energy conservation. And usually if you are not careful, and blindly finite difference a continuum, it gets you into trouble. And that is the thing that I remember about that model. Hardly anything else- I may be wrong, but...

P.E.: Well I think you are right. That is what he himself said he thought was his most important contribution.
J.S.: It turns out that it didn't get as much of a life as I would have thought because..... let's go on.

P.E.: Anything else about your relationship with Mincent Arakawa? How much contact did you have with him?
J.S.: There was one very important contact. I was asked by them, and maybe it is before Arakawa was there... I remember it as December and I remember I flew in a TWA jet and they may have been the first jets, four engine jets or 707s. I hope that is not garbled, but that is as I remember it. I was invited to give lectures on the results of my two level model. And then, I talked about the architecture for the nine-level models. There was never, in any other context with that many levels, used. Three was the most. So that was a novelty in its own right.

P.E.: So you gave this talk...
J.S.: Its purpose was two-fold. One to let them know what it was before being published, what we accomplished so this included the results, not only what went into the model. And then I spoke about what was the series of models, one of which Manabe was prime author, another which I am prime author- there were about five papers produced.

P.E.: So you talked about that to Mincent's group? What were their reactions?
J.S.: They were remarkable. They were not negative results. These were all positive. Charney was not always easily convinced and he thought that this business of putting in nine levels- remember the purpose of that was to get high resolution in the boundary layer and at the stratosphere. I remember very well how he described this- its like the white stuff on top of the chicken shit. Saying that there is a trivial improvement.

And this turned out to be immediately copied by anybody who wanted to do it. And we published that stuff reasonably well. I gave lectures, Manabe gave lectures. You have to

remember that Manabe came as a graduate student. He didn't have his Ph.D. So he did very well for himself. And I am going to say something today if they let me.
counter at 125

P.E.: OK, where are we here.
J.S.: We did five.

P.E.: I'd love to hear about your collaboration with Manabe and how that got started. I am especially curious about the following: It's interesting that of the three major groups doing GCMs by the mid-1960s (your group, UCLA, and NCAR), all three had collaborations between an American and a Japanese émigré who all came from the same group in Tokyo.
J.S.: It was the same group, the same professor Shiyono?, and privately I tell you that he wasn't that smart or had that much influence. But I always felt, and I have been contradicted by some people....

P.E.: Anyway, how did you come to know about Manabe's work and why did you decide to bring him here?
J.S.: The lab has got a bunch of superstories in it, not just one good guy but maybe 5 or 6 world reputations. It may be a little bit more than that now. And I was searching around, and I used to get a Japanese journal that's published in English. So I could do an awful lot of reading about who is doing what and why. And there was one name that caught my eye, Manabe, who had done a study of the heat balance over the Sea of Japan which was very important for cyclo-genesis and weathering and so on. But he hadn't published much himself. But what I was finding in other publications, are thanks to him for giving this idea, for giving this criticism... loads of them. He was just a little graduate student, you know. Co-equally drank beer with his buddies, but he was considered to be an expert. I said, well this guy might be worthwhile getting. And we had to break a very big precedent of no foreigners hired. And we finally did break the precedent.

P.E.: Why did that precedent exist? Was it just...
J.S.: Taking jobs away from Americans. So Manabe was one of our first, maybe the first, visiting scientists. His initial appointment was as a scientist as a visitor. And he took the job. And I had one very big objective for him- I wanted him to write this radiation algorithm using whose work I told him to put together.

P.E.: Was this Muller?
J.S.: No. Muller was there for a year but he couldn't have done this. He would work with simple models and wouldn't want to go into complicated ones.

P.E.: Well, that may be there in the published record. I can look for that.
J.S.: It may very well be. If I take another look at what this comes out to be then I can fix up some things. I owe it to you.

P.E.: I am going to have this transcribed and I will send it to you and you can make any changes or corrections you want.
J.S.: I am trying to fill in behind Muller. You mention Muller because-

P.E.: Well, that's one of Manabe's first publications about the radiative equilibrium with the convective adjustment.
J.S.: It may have been some question of precedent too. But let's not bring that up.

P.E.: So he came and you put him to work on this radiation algorithm...
J.S.: And it has never been done and nobody would ever have a reason for doing it. But then sometimes they work with it in the terrestrial radiation part of it but that's entirely different. I am dealing with the solar radiation. And incidentally, the radiation analog took up one hell of a lot of code. And so one of the compromises is how often do you use it- better to chop it up into small pieces.

You couldn't deal with diurnal variation. To do diurnal variation you essentially have to break a 24 hour clock off into 20 minute segments, and that's very often. If you ever wanted to take radiation into account, you'd have to dominate the calculations. The only thing that's potentially competitive were the demands of horizontal resolution. So that was a tremendous undertaking, and incidentally was absolutely key for the next step. You have radiation and convective overturning. You have a one-dimensional model. Anything that was done before that were zero-dimensional models at a point. This dealt with columns.
counter at 221

P.E.: Now, my impression from Manabe is that you were working on this 9-level model then and he was doing the radiation code. And I am curious about how you worked with the computer at that time. He said that there was a staff of programmers who I guess would turn the equations into code?
J.S.: On stretch?

P.E.: Yes.
J.S.: Well, we had several projects going- a spectral model and we had long range forecasting, short-range forecasting, we had convection, we had planetary atmospheres. In some cases the group consisted of one person. But then we started an ocean group in anticipation of needing oceans- that was a big novelty, a really big novelty.

P.E.: I was asking about how you worked with the computer and he said that you had a staff of programmers...
J.S.: Yes, when we first started out we had a pool of programmers. As soon as the group got large enough, we assigned a permanent programmer. So they would get their programming but it would make it all very manageable. And Manabe was the first- his group was always larger than anybody else's.

P.E.: Did you ever code yourself?
J.S.: Not really. What I did do was on the Eniac. They have function tables on the Eniac. All those switches, <u>not internal</u>? smart switches. And I did that. That is because I was there. But I did not do very much in the way of direct coding. As a matter of fact, when I retired my colleagues gave me a computer.

P.E.: You didn't have one before?
J.S.: No it was 1983. 1982 was when the IBM computer... You seem to be very familiar with all of this.

P.E.: Well, the computers yes. I wrote a book about the history of computers.
J.S.: You know the one thing you might do real quickly is take a look at this.

P.E.: Read through this?

J.S.: Yes. This is what I pose to say to Manabe.
counter at 268

P.E.: I am going to find his book because one of the things you say there is that Manabe's work on the carbon dioxide thing was partly stimulated by a request from this panel for information.
J.S.: I thought it was completely stimulated.

P.E.: Really.
J.S.: I made suggestion that we look into it and they then commissioned it.

P.E.: Tell me, do you have memories of a lot of interest in weather and climate modification during that period?
J.S.: Yes, that was why this committee, this panel, was organized. It is very broad. IT covers everything including CO_2. It was a few years earlier that <u>Rosby</u>? got us put on the TIME Magazine cover.

P.E.: I didn't know that had happened. For what?
J.S.: I may have it upstairs. But if I remember it correctly, I think <u>Rospry</u>? was being cited for his work in chemistry. The Institute in Stockholm was clearly the first to have wed atmospheric chemistry as an important discipline into the broader hydrodynamic context. And he was eventually recognized for it, but we do it now at GFDL, and some other groups do it, but this is many, many years after we've recognized it is something significant. The context for that was how does the chemistry of the atmosphere affect the dynamics. IT is a direct line. You put in more CO_2 and suddenly there is an important change. As opposed to putting in Hydrogen or something like this and you see no change. And this was due to these guys in the 19th century who did it by the seat of the pants and it is pretty remarkable. That you will find discussed in Manabe's paper, and mine. Actually it goes back before this Tendall. And there is a guy who was a professor at the University of Wisconsin.

P.E.: Hmm, that I didn't know-oh yes I do ...
J.S.: I am not sure how it goes chronologically.

P.E.: I think he was one of the first.
J.S.: Yes, they got crazy results because their models were too simple. But they came across with what sort of physics you need and how you design an experiment.
OK, let me take these one at a time. Number 7. We have discussed that. Number 8.

P.E.: Number 8 is about how your relationship with Manabe began and we already mentioned that.
J.S.: Well, the models became more complicated. Manabe's interested broadened to the convective overturning which was key to some of these calculations. Some people may argue with whether convective overturning should be dealt with this way. But he has not made that any more- he has not changed that since the beginning. We had problems with, this was a hemispheric model , and we had trouble with the boundary conditions at the equatorial boundary. You say you put up a wall, and the damn thing was losing 1% per day of mass. That was really frustrating. OK, if you start in the middle of the calculation so you don't get down to a point where it is all flowing away. But we had to scream for help to another Japanese colleague Kurihara. He came up with a new system.

P.E.: A different griding system?

J.S.: Yes called Kuri grid. And you ask somewhere what I was responsible for. I was responsible for the model architecture. Mainly, what processes should be taken into account and make a first estimate as to what you have to do to get it. I worked on lateral viscosity and water dynamics and thermodynamics. And Manabe did the bulk of the work on the radiation package. That is something I realized when I was working on my two level model- that we have to aspire to do, otherwise interference between the upper troposphere and the lower stratosphere get screwed up. The other thing that I was involved in was the experimental strategy and the design.

P.E.: Tell me about that.
J.S.: Well, it was based on my two-level calculations. We assumed that we would need the primitive equations and we used a more general form than I would use in a two-level model. In the two-level model it was no divergence in a column. You complete a column top to bottom. And this permits you to do calculations somewhat faster than pure primitive equations. But we had to introduce a lateral viscosity, non-linear viscosity to keep the calculations stable and once we did it we wondered why it hadn't worked before. But this has made a splash all of its own in engineering.

P.E.: Outside of climate?
J.S.: Yes. I did this work in the early 60s. And aside from meteorological users, I never discussed it with anybody and what was happening was picked up by the engineers. And next time I looked around was maybe 10 or 15 years later, and the world had changed and I didn't even know about it. Ridiculous in these days of massive communications you know.
counter at 409

P.E.: What do the engineers use it for?
J.S.: Engineering flows of all sorts. My name is mentioned many times but it is usually as 'Smagorinsky didn't do a good job, we're going to make it better.'
I've got a big book like this that has maybe 50 or 60 references. And then I did a double-take.

P.E.: Well you know the old line about a prophet is without honor in his own country.
J.S.: Yes, that's right. Well, as a matter of fact my attitude has been, in running this laboratory and has turned out to be a very good thing, is that I would rather one of the scientists in the laboratory do an interesting thing than I do it and he doesn't get a chance to contribute. And I did this with ocean circulation too. And a number of key decisions had to be made and were made by Brian and he was very stubborn about it and it is good it worked out. Manabe too. Manabe has been on his own for a long time. But I rarely go to anybody that I know that well and say 'I want you to do this'. What you say is 'I want you to make this go any way that you can manage and I am not partial to my ideas.' And this turned out to be important.

P.E.: Tell me about the greenhouse effect and how that came to be a big issue for the lab.
J.S.: Well, you know the path that it took. I told you about this committee. They wanted to come up with a more modern calculation after all those that had been done previously, before 1960, Plus. You know about Plus?

P.E.: Yes, sure.
J.S.: There was a regular cascade of flurry in there. But it really needed to be brought up to date, and this comprehensive radiative model is the thing that made it possible. What was also important was the radiative convection linkage that if it's done well this will come out even in a one-level model.

P.E.: What is your memory of how it became a political issue?
J.S.: It started becoming a political issue before this. Before Smic And my concern was that I thought that the political pressures might distort scientific values. And if you are Dick Linson on one hand or Steve, no not Steve Schneider, let me say Lemont, Macneilson, or Jim Hanson who is at the other end of the spectrum. They wouldn't be sensitive to this at all. It is the great unwashed middle that is sensitive to overselling a case because what you do is you tell the politicians something that makes them feels good. And I tend to be that way myself. I am not a yes-sayer and I am not quite a nay-sayer. These calculations are vulnerable, but they are not so vulnerable that they are basically wrong. Some aspects I am sure are not going to be changed much when we get a better model. In this paper, this one...

P.E.: I am asking about your memory on how this became a political issue and you said it went back to before Smic.
J.S.: Oh, there was another committee that I was on with Rivel. See, Rivel was, and Tom Malone, are two people that are great enthusiasts. And they carried an awful lot of weight. What they did instead of evening things out, and the way it was evened out is that it generated people of a type that never existed before, namely Linson and his small bunch of cohorts. And Linson is a smart, smart guy. But I think he is now fighting for his professional reputation. I can't imagine him sleeping with the nay-sayers, but you know the bunch. Jestro, and

P.E.: and Singer and Michaels..
J.S.: Is Singer part of the group now?

P.E. Oh yes.
J.S.: He is the guy I respect least.

P.E.: Steve Schneider doesn't like him either.
J.S.: My daughter used to work in University of Virginia Department of Meteorology. And she was married so she didn't carry her name with her. She went up to him one day and said 'maybe you know my Dad'. And he said 'I don't know your dad but he knows me'. He really had to grope for that one.

P.E.: So you were saying you were on a committee with Rivel and
J.S.: I can give you the name of it.

P.E.: Is that on this bibliography somewhere? Or in this paper?
J.S.: Well, let's see. It was in the middle 50's and I am not sure. Let me make a note to myself.
counter at 580

J.S.: It was a presidential committee.

P.E.: So this was before the IGY?
J.S.: No, I think its after. So that smelled political already.

P.E.: What kinds of things did that committee discuss?
J.S.: Geeze, that is a long time ago.

P.E.: Was Rivel worried about climate change as a hazard?

J.S. Yes, he was one of the people that felt the whistle should be blown. He carried a lot of weight. What did they discuss-- it was CO_2 I am pretty sure. I'm going to have to do some searching but I better be in better shape to do it.

P.E.: Well, then another big question I am interested in is GARP. How that got started and what your idea of the purpose of it was in the beginning.
J.S.: That sort of started politically in its way. Kennedy had an inauguration speech to give and casting around for things to say and do. So he officially made it an international project to talk about it. He didn't call it GARP, but he urged countries to do something about it. And they did it through two international organizations. One was ICSU (International Council for Scientific Unions) and the other was the WMO. And in about 1967, about, after having several meetings, they agreed to recommend to the nations of the WMO- see nations of the WMO may not be very smart, but they have got money, something they try to live down.

So by 1968 it was organized, and in 69 I was on both committees- both the national GARP committee that was set up when you had all the nations create GARP committees in their country, and the international one. The first meeting of both of those committees were right here in January 1969. GARP committee eventually turned over to climate when more or less the ______? had been met. And a lot of the people stayed on the committee. Good committees, I was chairman of GARP for some years and through the transition to climate. And I had to negotiate with the WMO to pull in those horns. And I was the first chairman of the World Climate Research Committee. It went from the joint organizing committee....
side ends with counter at 695, start tape 2 side 1 with counter at 000

J.S.: But that was pretty successful. I left about 10 or 15 years ago and it's grown bureaucratically by leaps and bounds. They turn out many reports and every report lists all of the ___? honors, and everybody wants to take this as an example but I would say the best thing you could do for it is to put it out of business for a year or two and then rename it.

P.E.: It has just gotten too unwieldy?
J.S.: Committees all over the damn place. When in doubt create a committee.

P.E.: Tell me about Soviet or Eastern Block participation.
J.S.: I've got something in there too. When were the Soviets behind effectively. It turns out that numerical weather forecasting in some primitive form was done by the Russians too during World War II. But they didn't exchange any information until the _______? when all these journal they were publishing in showed up. There was a meaningful contribution made even though nobody knew it. I think they had access to American journals. The Russians probably protected their journals from espionage or something like that. Russians always had two people on the GARP committee. The US did too. And the committee was, when I was there, I think about 13. Now it must be 18 or 20. Every time there is a revolt in Africa they want... it is literally true. When I was chairman there was a revolt in one or two of the countries and the next thing we know is 'we now want our quota'. And the thing that was different about the GARP committee that it didn't operate that way because it used as its model ICSU. It purposely, we desperately wanted ICSU on the governing board so that we could use their rules. See, the rules of international organizations differ all over the place. And when you are dealing with the academics you are not dealing with the government in general, you are not dealing with governmental bureaucracy.

But they generally were constructive, sometimes not. I can remember one thing- I think they thought it was a golden opportunity to get out into the open- the individual scientists: travel, be involved in scientific affairs of consequence. And there was only one time that there was a problem. They would pull a very oriental sort of trick where they'd use their connections to get favors of one sort or another and they liked to do this with Americans because Americans were rich and they had a wonderful system of 'let's sit on this tailgate for awhile and see what other goodies I can squeeze out for myself'. But as I said that is kind of oriental. It is oriental with a small oh? Some of the Japanese were saying oh? But otherwise they were very pleasant to work with.

P.E.: Did they make any important contributions?
J.S.: Few and far between. Occasionally. A lot of them were very smart. Their work in turbulence was excellent. Monin was a student of Komogarov's. And Obekov. There are others too, but I mention these are all super sharp people. There were others that were more tangential to this whole thing.
I didn't go through this entire list did I?

P.E.: We are getting close. Smic I also wanted hear about if you had a role in that.
J.S.: Well I attended one or two meetings, I never though it was terribly interesting. It never revealed anything to me of political pressures being exerted although I am sure they were there. It is just that it never became worth any body's time to fight it. So I can't say anything definitive there.
There is one thing that you ask that I didn't discuss at all- that is Bill Burke.

P.E.: Oh yeah, right, from the Australian group.
J.S.: Bill Burke is mainly a mathematician. He may be a meteorologist also. He had done some very innovative work with spectral models. Something that had been proposed many years earlier but nobody every really- I think the best first work on this was George Plods___? And if I remember, maybe Kasahara. I'm not sure. That could be checked. He applied for a visiting scientist slot here at GFDL. And he spent about a year. And what he wanted for this was to convince people here that his modal would be worthwhile having and to carry back with him a new capability to make a full model. It is one thing to deal with spectral fields, it is another to have the dynamics in there. It is sterile otherwise. But he wanted to learn how to model. When we say 'learn how to model' this includes one hell of a lot. It is not like a forecast model, a plain inertial forecast model. And he and his colleagues, we have had quite a few Australians here, have spent time. We gave them the ____ of the first big model, forecast model. Now my son is in Australia. Except he is making money. He is in industry. Burke is very good. If I were to ask what one name would stick out it is Burke.
counter at 118

P.E.: So you gave them their first GCM, but he also influenced you because he helped you with spectral...
J.S.: Yes, yes, that was a two-way street. It often is not. GFDL has often been criticized for not being cooperative. The truth of the matter is if you were indiscriminately cooperative all of the dregs? from all over the world would try to undo it. Grab something for themselves and when they didn't earn something. And our basis of cooperation is not the same as NCAR's. We bring somebody in only if there is somebody has specific interests and what this guy wants to do. It has got to be specific interest. In Burke's? case it was Miyakoda. And the question is what do you do, export the model? Well, these models are so special that you just can't put it in somebody's hands. I don't think you can, maybe things have changed.

P.E.: You are saying it is very difficult to export these models

J.S.: Yes, and we decided- we is really me- that if this guy wants to come and do experiments on our computer, or make model changes and test them out and do it with our model not with out computer, we'll do it only if he takes a year off and comes and does it rather than asking for me to send it to him in the mail. NCAR, which is a different kind of institution, has got to be willing to do almost anything that anybody wants. And that is good and that is bad.

P.E.: Right it spreads them very thin.
J.S.: That's right. But that policy has gotten a lot of people mad saying 'you are a bunch of snobs'. In a way it is snobbery, but it is constructive snobbery. You are doing it because somehow you won't be disturbing something that works.

P.E.: Well, we have been talking a long time, and let me just ask you the last question I had: what do think your three or four most important contributions to climate science have been?
J.S.: I think that is someplace....Well, the whole question of model architecture and experimental strategy and design. I think much of this was, is now taken for granted, but what decided how many levels you take are meaningful, or what processes should be included and how and what is a balanced array of processes, and when is something supposed to be dealt with very simply, and when is it that you only do the most complicated thing because it is so critical, radiation is a good example. And then in terms of more specific things- dealing with the forced quasi stationary monsoonal motion and consequence. The whole question of the water vapor cycle. Precipitation. The research that I did on the primitive equations. Lateral viscosity which is what I was talking about. And I would on top of this whole thing a label 'general circulation modeling'. So I got that one down to one.

And two other things that I might include that other people might not want is my work as GFDL director evolving its scientific program and pulling together a great number of really talented people. You know, know anything about NOAA? NOAA has 15 laboratories and each one has got one superstar at best. GFDL has got 5,6,7 superstars or international figures. And I think it has paid off. And the other thing that I think is the work that I did on the Climate Research Committee and the comparable committee of National Research Council.

P.E.: You are talking about the WCRP?
J.S..: WCRP, I was chairman and I was with them and its predecessor going way back. And the NRC which is the same one. There is a variety of things I did and as time went but it was shifting to more committees. I got kind of fed up with it after awhile, the bureaucracy of it was overwhelming and we didn't have as many kicks. And when I look at reports now it looks much worse than when I was involved. Not because of me, it is just a sign of what is happening. But you know GARP did a wonderful job. Not as good as they say they did, but it did show that a lot of things are possibly with some agreement between nations.

P.E.: Well, let me thank you and close this up.
Interview ends with counter at 232