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TAPE RECORDED INTERVIEW PROJECT

Interview of Joseph Smagorinsky
John Young, Interviewer

16 May 1986

Young: This is the beginning of an interview with Joseph Smagorinsky, of the Geophysical Fluid Dynamics Lab. We're at the University of Wisconsin, in the faculty lounge of the Department of Meteorology. It's May 16, 1986. I'm John Young, Professor and Chairman of the Department of Meteorology, currently.

Our discussions today are aimed at talking with Joe about his career in meteorology, focusing on the events that he has had an impact on, but especially from his personal point of view.

I guess, Joe, when one sort of starts at the very beginning, you were born in 1924. I guess you grew up in downtown New York City. How does one get interested in weather, or were you interested in weather as a boy?

Smagorinsky: I was, and I was helped along very much by coincidence that was forthcoming by living in a big city such as New York. There's a well-known tabloid newspaper in New York called the **Daily News**; it probably has the largest daily circulation of any city newspaper in the country. And they have a building of their own, and on top of that building, they have a big weather observatory. I don't know how it is today, but this was true when I was young, in the 1930's. On numerous occasions, I used to wander uptown--I think it was to 42nd Street, to the **News** building to go up to their weather floor in order to see the instruments and see what they were doing in the way of meteorological activity. So I had a rather unusual opportunity to see something about the nuts and bolts of weather observation, and in following the weather on a day-to-day basis, and in this particular case, readying it for newspaper issuance.

Young: Were you employed there?

Smagorinsky: No, no. It was purely my own personal curiosity as a teenager.

Young: So your first job related to meteorology might have been when you were in the service?

Smagorinsky: Well, perhaps it was, but actually there was quite a bit that happened in between. I was interested in weather, and I didn't think of it in terms of career. When I was ten or twelve years old, I already knew I was interested in technical things, in science and engineering. And I took the unusual step of taking an examination to enter one of the three science high schools in New York City. The New York City school system is such that one normally attends the regional school in the area where one resides. But there were special high schools set up for children who were somewhat more talented in the humanities and in the sciences. The three schools in the sciences were the Bronx High School of Science, Brooklyn Tech--I'm not sure what its full name is--and Stuyvesant High School. I took the examination for Stuyvesant High School, which happened to have been fairly close to where I lived, about a mile and a half away, and I was admitted there. So I had the opportunity to take enriched courses in mathematics and physical sciences, and developed not only my interest in meteorology, but even more so, my interest in naval architecture. And I had visions of going off and studying naval architecture, which was really my first love, but it turned out that my parents couldn't afford to send me to the two schools that existed out of town. One of them was M.I.T. and the other was the University of Michigan. The third school was in New York City. It was called the Webb Institute of Naval Architecture. It was the most exclusive of all; actually, it was a school of naval architecture and nothing else. It had something like fifty or sixty students in it altogether, and those students were there on full scholarship, everything. It was a very, very selective school, perhaps one of the most selective in the country in any field. They normally had something on the order of five hundred applications; they selected perhaps fifty to take their three-day examinations, and from these selected a class of twelve to fifteen. I didn't make it. But I'm very happy to say I know somebody, who's a colleague of mine, who did. His name is John "Mike" Wallace, of the University of Washington.

So I didn't make it, and my next choice was to go study meteorology because there was a place that it could be done, where I could live at home. This was New York University. So I entered New York University in the Department of Meteorology, which had a new undergraduate curriculum in meteorology. I was one of the first enrolled in it and I think it was the first year it was offered--1941. This was in the summer of 1941, and I was accepted and I studied there. The war started, of course, in December, 1941, that is, the American war, and by February of 1943, after I had spent a year-and-a-half at NYU as a freshman and a sophomore, I entered the Cadet Program run by the Air Force in this country. That was the last cadet class, the one that entered in, I believe, approximately March or the spring of 1943. I had to take some basic training before that, so that's how I ended up in meteorology in the Air Force.

Young: Question on that though. Were you able to select the field--?

Smagorinsky: Yes. That selection was made in advance, and one had to apply, and then they determined who would be accepted for it. It turned out to have been a very selective program, especially the classes before mine. In general, they were dealing with people who already had BS's. They had begun to run out of quality people that they wanted by 1943.

This course of study ran in two parts because, [since] we didn't have a BS, they felt we should spend six months studying mathematics and physics. And so I was sent to Brown University for six months and thereafter, after some trauma because the whole program was threatened to be phased out--they thought that they may not need more weather officers--but eventually they decided they did, and I was sent off for nine months to MIT, attending the last war course that they had there. Two of my instructors who stayed in the field, later on became prominent in it. One was Ed Lorenz, who was two classes earlier than mine; he was our instructor in dynamic meteorology, and Bob White was one class behind me--he was my laboratory instructor. I received my commission the day before D-Day, June 5, 1944.

Young: Were any of the instructors in the program "heavies" in meteorology? Did any of the faculty members at M.I.T., for example, give instruction, or were people who were to become famous in meteorology, instructors?

Smagorinsky: Yes. It was the regular faculty that taught the war course. Bernhard Haurwitz was there, Tom Malone was a young assistant professor, Austin was there, Hurd Willett was there.

Young: Henry Houghton.

Smagorinsky: That's right. Henry Houghton was chairman. Because the load was so great, they also were assisted by officers who had graduated from the course a year or two earlier, and that's how people like Lorenz and White and others got involved. Of the sum total of the large number of people produced by these war courses, only a small percentage of them remained in meteorology after the war. But I was one of the few people that entered the war course who was interested in meteorology to begin with. Admittedly, it was my second best interest, going way back to my early years. Nevertheless, I fully intended to stay, and when I left the service in 1946, I returned to complete my Bachelor's education at N.Y.U. and then to take a Master's degree. Then I got married, went to work for the Weather Bureau in Washington for two years and in 1950, was invited to join the Institute for Advanced Study, where I stayed for two and a half years, during which time I commuted to N.Y.U. and did my Ph.D.

Young: OK, I want to get into that in a couple of minutes. Let me just ask you a couple of other questions about, for instance, the MIT days. I somehow had the impression that many of the prominent instructors and people that came out of the World War II cadet programs perhaps were not people who were initially interested in meteorology, in contrast to yourself. Is that a wrong impression?

Smagorinsky: No, that's a correct impression. Many of them returned to their fields of original interest and became very prominent. I can only mention one or two offhand, but I have come across many who label themselves former wartime meteorologists. People like Kenneth Arrow, professor of economics and I believe, a Nobel Prize winner at Harvard; Jack Townsend, who used to be at NOAA, was actually a wartime meteorologist, but never worked at it afterward, but he was really more an engineer and physicist. A lot of people in mathematics and a lot of people in the humanities.

Young: In fact, I can think of one person that you introduced me to in 1978 in London, at a cocktail party at one of the embassies there--Robert Cowan, the science writer.

Smagorinsky: Yes, yes, in Boston, the **Christian Science Monitor**.

Young: Right. And he also writes for **Technology Review**, the MIT publication.

Smagorinsky: I think one of these days it would be a good idea to try to go through a roster of all of those that attended the war courses, the so-called "A" schools, the meteorology school courses such as at MIT. These were done at the five schools giving meteorology at that time: NYU, MIT, Chicago, Caltech and UCLA. And it would be interesting to trace through all of the people who have gone through those courses to see how many we can now recognize as one way or another having become prominent in other fields.

Young: The timing would be perfect; that's a great suggestion.

Smagorinsky: All of these people are now, like myself, in their sixties.

Young: On that theme of the war courses, that's a special period in history and it influenced society in very strong ways. It tremendously influenced meteorology in a singular way. From your description, I have the impression that the level of these courses in the cadet program was pretty high, pretty intensive with high expectations of performance. Is that right?

Smagorinsky: Yes, I think that the selectivity and the standards of excellence were set high because they realized that these people would have to spin up rapidly to the highest level. Even though most of these people were supposed to be forecasters, they wanted them to be technically very capable. So the selectivity for just coming into the course was very great. It was mainly enforced by Rossby, who was guiding the Air Force in setting the rules for this program--in curriculum and things of this sort. So, for example, by the time I got to MIT and was a few years off from my BS, I had enough grounding to be able to take a good course in dynamic meteorology. Haurwitz was assisted by Lorenz, who had learned dynamic meteorology just two years earlier himself. But I would say they were a pretty smart bunch of people.

Young: I think it's very interesting that a kind of urgent national situation with very practical needs--namely forecasting weather--had an operational program that still was able to include as much theory, basic theory, as many curricula probably had for graduate school in those days. I am a little surprised that in the heat of the war developments, that they would not have just simply emphasized synoptic forecasting alone.

Smagorinsky: Well, I think it was done in a fairly orderly way. At the very beginning, the earliest war courses, which were only about three or four years earlier, took people who were already very highly qualified academically and they figured they could spin them up within nine months and the amount of time that was involved was not terribly different from what would have been required to create an infantry officer or pilot. So I think the investment of time was comparable and consistent. It's just that they realized that in order to ensure a fairly large yield of good people, that they needed to be very selective at the outset.

Young: Looking ahead, I guess there was one question I had. Now you said you graduated just before D-Day, that wasn't the end of the war, it was a turning point in the war. Did you actually serve as a forecaster?

Smagorinsky: Oh yes, oh yes. My first job was to have been sent out to Nebraska. In Nebraska and Kansas, there were about twenty some-odd airfields, whose job it was to train B-29 squadrons, groups and wings. At that time, the B-29 was a very secret aircraft and even though I was an officer, I wasn't allowed to go anywhere near one. But I was supposed to make forecasts for it and these were supposed to be very high-performance aircraft, the first of their kind, relatively high-altitude pressurized aircraft for the first time, and aircraft capable of going on very long missions. In particular, they wanted them to be able to go on bombing missions to Japan from the island bases in the Western Pacific, in some cases island bases that weren't even held yet at that time by the Allied forces. In order to train these squadrons and groups, the Yucatan Peninsula was selected as a simulation target. It was about the right distance and what was required were forecasts 24 and 48 hours in advance, of the cloud variations level by level, something that we couldn't even do today, right? On top of the fact that there were no observations except those of the last training flight. You have to remember that I was brand-new at this at the time and I made my biggest mistake the first day I was there. At four o'clock in the morning, I was typing out a forecast from the sequences, the telegraph sequences, and I said: "Rain starting at 11 a.m. in the morning." I felt eyes upon me and sure enough, it was a sergeant standing behind me, a very old man, he must have been about 29 years old (I was 20), and he said, "Say, Lieutenant, have you looked out the window?" Of course it was raining already. That's when I learned my first lesson. Always make sure you at least get present weather correct.

Young: That's still a lesson people are learning in these current generations.

Smagorinsky: Well, this business of making these forecasts of these cloud variations--that was a tough one, obviously. How you said it mattered, and after awhile, the pilots and crews thought I was doing a reasonably good job. I thought to myself, it's not what I'm telling them, but how I'm telling them. There was one substantial part of the forecast that they required that was quite important. When they came back in the evening, after a long flight, they had to have very good terminal conditions to land. They were low on fuel and if you decided that they couldn't land at that time, then this was very serious because they would have to be diverted and lose two days of training. So this was critical, especially in this part of Nebraska where I was, in southeastern Nebraska, which in the wintertime or the fall was highly susceptible to upslope fog. I remember one day I was sure that it was going to get socked in by evening and I made the forecast: "You won't be able to land back here tonight." This was a whole group of airplanes. As I remember it, a group consisted of something like 27 B-29's. At about six o'clock that evening, they were due back in a few hours, I went into a movie. The sky was still pretty clear but when I came out I could hardly see anything in front of me.

There is one other thing I hardly ever talk about which I might mention. One morning I got up and one of the three squadrons in the group had disappeared. It had been there the day before. Nobody knew where it went to. It turned out that the squadron was sent to a base in Utah and it was to form the nucleus for the bomb group that ultimately dropped the atomic bomb.

Young: My goodness.

Smagorinsky: That was such a specialized bomb group that it only consisted of one operational squadron. All of the other squadrons in the bomb group were support squadrons.

Young: Had they seemed like a special group before?

Smagorinsky: They must have been evaluated as being good. I think this was the pick of the litter. But they were trained specially at this base in the Rockies. They didn't know why they were being trained either. I didn't find out about this until after the war. It's always sort of been a little bit on my conscience.

Young: You were half a year in Nebraska?

Smagorinsky: Yes. Let's see, I graduated in June, and I got my commission in June, I ended up at the base in July and I remember in February, I was transferred to a weather reconnaissance squadron, but before I went to the reconnaissance squadron, I went to its parent wing, which was headquartered in Denver, and I was there for a couple of months. While I was there, I was asked to give lectures on meteorology to pilots. Then I went to Grenier Field in Manchester, New Hampshire, which is where the reconnaissance squadron was headquartered, and where I was trained in reconnaissance. Then I was sent out to Newfoundland, where I flew reconnaissance, eventually accumulated many hours in the air. I flew from Gander and from Goose Bay, Labrador, and from the Azores. In the Azores incidentally, I met Norman Phillips, who was a forecaster on the ground.

Young: So Phillips was about the same generation as you?

Smagorinsky: Phillips is a year older than me. He was also in the last class, but the one that was being generated at the University of Chicago. So he got his commission at the same time as I did, but at a different place. He was in forecasting for the entire period of time. I was in the Azores when the war ended, and I went directly to be mustered out at Fort Dix, New Jersey.

Young: Soon you were on your way to your Bachelor's degree with a meteorology major. Did you take a lot of physics and math at NYU for the Master's degree?

Smagorinsky: Well, I needed another year to get a Bachelor's degree, and I got my Bachelor's degree with a great deal of graduate credit because some of the courses we took at MIT were graduate courses. So I had a rather lopsided Bachelor's degree, and it was one with a lot of graduate credit in mathematics, physics and meteorology, but it was short on the humanities; it was not well-balanced.

Young: Were there any professors who were particularly influential as an undergraduate to you in that year?

Smagorinsky: By that time, Haurwitz had gone to NYU and another one who was quite influential was Hans Panofsky, who was still at NYU at that time and there were some younger faculty members like [Jerome] Spar and Al Blackadar, but I was probably less influenced by them than I was by Haurwitz and Panofsky. Jim Miller was there, also.

Young: Now you stayed on and went straight out of the Bachelor's degree at NYU into a Master's program. Was that partly a result of the fact that you had so much graduate credit already accumulated?

Smagorinsky: Well, I still had to take a year--a Master's worth of graduate credit, so I took a lot of courses that would have normally been taken by somebody going into a Ph.D program. It was kind of lopsided, but there were enough courses that I never ran short.

Young: Was it a clear decision on your part to pursue the Master's or not at that point, or was it just kind of automatic--or did you just like what you were doing--?

Smagorinsky: I knew I didn't want to stop at that point. For one thing, the custom in those days was definitely to go through a BS in meteorology and into advanced degrees unlike today. Some universities just don't even bother with an undergraduate program, and they take their graduate students from mathematics, physics or engineering.

Young: Was that master's degree one based entirely on courses?

Smagorinsky: No. I wrote a dissertation.

Young: You did. . .

Smagorinsky: In retrospect, it wasn't a particularly exciting one, but---

Young: What was the subject?

Smagorinsky: Oh, I was trying to determine the divergence fields. In those days, large-scale divergence and vertical velocity were new and one was trying to find ways of calculating them; the "omega" equation hadn't quite come in to use yet. So there were all sorts of ways of trying to estimate the vertical velocity. I must confess I haven't thought about it in about thirty years. I couldn't even tell you what the title was. But I did want to come back to get a Ph.D, and I also wanted to get married. I met my wife, who was a graduate student on a scholarship at NYU.

Young: In meteorology.

Smagorinsky: In meteorology. She was a statistician.

Young: Margaret--what was her maiden name?

Smagorinsky: Knoepfel. She was trained as a statistician, and was working at the Weather Bureau, and she and several others were on scholarships from the Weather Bureau. She was the first woman, and probably the last woman because she eventually got married and quit. She was there and we took courses side by side. I decided I wanted to get a Master's degree and she went back about the time I got my Bachelor's degree.

Young: She went back to the Weather Bureau.

Smagorinsky: She went back to the Weather Bureau in Washington. I took only a year to do my Master's, and then after doing that--this was in 1948--went to Washington. We got married, spent two years, as I said earlier, at the Weather Bureau, and that's when I started getting involved with Charney.

Young: After the Master's degree, you were already somehow running into Charney?

Smagorinsky: Well, I can tell you roughly, I'm not sure I can remember exactly the chronology, but after I was in Washington for about a year, there were two things that happened. One was Charney came and gave a seminar at the Weather Bureau, and another, I heard him, Eliassen and one other guy give a talk at one of the January AMS meetings, it must have been 1949. And I had some questions on the scale analysis of large-scale atmospheric motions. This was the first time these ideas were put out, concluding that the large-scale modes might be predictable despite the noise of higher-frequency modes. I was very interested in what he had to say because I didn't think it would be possible. I had heard about the failure of Richardson's work without really knowing why his work had failed. I asked some questions of Charney, and he later on asked me if I would come to Princeton on visits to work with them. This was just on short visits, a day or two. Harry Wexler encouraged this. Then after doing this for about a year, during which time the ENIAC calculations were done, the first ENIAC calculations, Charney asked me if I would come for a continuous interval without an end point stated, the implication being at least a year. He said he knew that I was interested in getting my Ph.D, and said you could do that too. I was on the G.I. Bill, so in 1950, my wife and I went to Princeton. She was already pregnant at that time with our oldest daughter, and Margaret started programming for the I.A.S. computer; she was one of the first programmers ever to work on the I.A.S. computer, the I.A.S. machine being the granddaddy of all modern computers. As a matter of fact, what she was doing was a barotropic system of equations--that was in 1950.

Young: Let me ask you a question there: it's rather interesting to me now Jule Charney had only recently established the meteorology group at the Institute for Advanced Study. It was not a part of Princeton University. Presumably he was not any sort of faculty member at Princeton University, but he was eagerly giving out advice as to how you could essentially do some research that would be appropriate for an academic career there, or for a Ph.D program.

Smagorinsky: Well, he was an exceptionally bright guy, much more mature than his years let on. He was about seven years older than me. He had been a student of Holmboe, and Rossby was the one that recommended him to von Neumann. Charney came to the Institute in 1948, to start the meteorology group, and it was a year later that he asked me to occasionally come and work with him; already by that time, he had done a scale analysis. He laid out a rationale for prediction using the barotropic vorticity equation. He made contact with Eliassen when he worked at the University of Oslo on a post-doctoral visit before 1948. Charney was ready to roll. I was just a young guy, and he wanted to have somebody assist him.

Young: I want to get into that in one second. I think we probably have twenty minutes, is that right? A little less? O.K. Let me just stop at this point and turn the tape over. . . **END OF TAPE ONE, SIDE ONE**

INTERVIEW OF JOSEPH SMAGORINSKY

TAPE ONE, SIDE TWO

Young: O.K., this is Side Two of our continuing interview with Joseph Smagorinsky on the 16th of May, 1986.
Before we get more into your period as a graduate student at the Advanced Study Institute in Princeton, I just wanted to ask you a couple of questions. I had the impression that first of all you had gone to Washington to get a job because you had a family growing, you were newly married. Did you anticipate a lengthy stay there, when you initially went to Washington after the Master's degree?

Smagorinsky: I don't think I had any specific pre-conceived notions. I knew that eventually I wanted to get a Ph.D. Margaret did have an obligation to remain on for awhile at the Weather Bureau and the first job I sought was not in the Weather Bureau in Washington--as a matter of fact, I was trying to avoid that.

Young: Why?

Smagorinsky: Well, I thought it would be a good idea if husband and wife didn't work side by side. So I looked for jobs, I remember, in two places, one at a Naval Research Laboratory (and I probably would have ended up in weather modification or cloud physics) and another was at the Air Force in Washington. Those didn't materialize and Harry Wexler offered me a job. The first job I had there was due to a temporary vacancy. A fellow by the name of Morris Tepper was on leave and they had a temporary vacancy and they were able to hire me. Eventually, when he came back, they had to create another position for me or fire me. There were two main things that I did while I was there. One was: I was handed a tall file cabinet stuffed full of carbon copies of letters, which was the "crank" file of the Weather Bureau. This was a file that had been in the custody of many people, even some distinguished ones. Whoever had custody of it currently was responsible for answering crank letters. The object was not to be so nasty that you get the guy sore and have him get in touch with his congressman, but not to be so friendly that it invites a second letter. After awhile, I learned to do that.

Young: Good training for the career you later had, as Director.

Smagorinsky: Yes, well, you're right in retrospect, I never thought of it that way. I finally turned it over to somebody else. The other job was as a research assistant to Harry Wexler. In those days, Harry Wexler was very interested in the effects of solar flares and he wanted me to help him prove that they had important dynamical consequences in the atmosphere. That cost me about a year of my life. I never produced a paper that was worth trying to get published. In retrospect, I don't think one could do any better today with models and new observations and everything else. I still don't think there really is a very strong valid basis for saying that we understand how activity on the sun has identifiable consequences in the lower atmosphere. The upper atmosphere yes, but not the lower atmosphere.

Young: Now some people who have tried to push that idea of lower tropospheric influence by solar flares have sometimes been controversial figures in meteorology. You probably drew some conclusions about some of these characters yourself, didn't you?

Smagorinsky: Yes, well, I don't want to get into personality statements, but that community is a very small one in the United States and has remained small. It is interesting to note that in the Soviet Union, it is a very large and very powerful community, a very vocal community made up of many academicians who have had some noticeable effect on Soviet meteorology and even on the relationship of Soviet meteorology to Western meteorology. For example, in the bilateral collaboration on the environment between the United States and the Soviet Union, the Soviet Union insisted quite strongly that this be an important element of the agreement. When I was involved with it, I pointed out to the Soviets that it was hard to find a community of interest in this country to interface with.

Young: Very interesting. Did it also--it has always seemed like within meteorology, especially when there is the old and the new and they were just beginning to develop their distinctive personalities, it has always seemed to me that there is a pretty strong sociological stratification that developed within our field, and people started not talking to each other or not respecting each other's work, increasingly, and I think probably for good reason. Was that starting to happen in those days, was there a fringe group starting to develop, for instance, in your estimation, on topics such as this?

Smagorinsky: No, I think not. There were some pretty respectable people in those early days--people like Hurd Willett, who was actually within the meteorological community. He felt that the newly discovered sudden stratospheric warming was the result of extra-terrestrial radiative activity. I remember in the early fifties, there was a conference in which I participated, and this was perhaps five years after that I did my early work. He gave a paper and he wasn't booed. But certainly he didn't convince many people despite his very heavy reputation at the time. I think as time went by, that camp was ignored more and more. Well, you know there were others. For example, Walt Roberts didn't start out as a meteorologist--he was an astrophysicist. And then there were a lot of other guys, some of them very capable scientists who still strongly believe there is a connection. Of course, there are these occasional empirical studies which correlate solar activity with events on earth, such as rainfall, and so on, and say, "Well, there is a connection [and] now we have to understand it. Your job is to explain it."

Young: Earlier, I was going to ask you about Harry Wexler's influence. Verner Suomi's chair here at the University of Wisconsin has been named the Wexler Chair by Verner's choice, partly because Verner gave him a little bit of credit about foreseeing the possible weather applications of satellites. You did interact with him as a collaborator on this solar flare work. Were there any other influences he might have had on your career at that point?

Smagorinsky: Well, he was the one who encouraged my going to the Institute for Advanced Study in Princeton and working as an assistant with Charney. It was Wexler's way of getting the Weather Bureau involved at an early stage with Charney's effort. This was before there were any real tangible results there, more or less on the promise of it. After all, I was a pretty low level guy at the time. I was interested in dynamic meteorology and had some training in it. It wasn't a very substantial connection between the Weather Bureau and Princeton. Even at the time, Charney invited me to come to Princeton on a continuing basis, although not on a permanent basis (there was no permanent setup there--even Charney didn't have a permanent position; he was a long-term member.) The question was, would I take leave from the Weather Bureau or would the Weather Bureau pay my salary? And Wexler wasn't willing to pay my salary.

Young: So he was interested in trying to keep up with a new group that was starting and keeping some contact there, but he couldn't come up with the money to support it.

Smagorinsky: Well, he didn't want to. That was his choice. I said I did want to go because it was an opportunity that I didn't want to miss. I thought it was in the interest of the Weather Bureau, I said, but what I'll do is take leave and after a year, if you [Wexler] want to change your mind, you can turn down a request for a renewal of leave (it had to be renewed every year). They did renew my leave, but when I finished at the Institute for Advanced Study--this was March, 1953--the question was, would I go back to the Weather Bureau?

Young: I want to be talking about those days at the Institute when we renew ourselves next week. One last little question before you get to the Advanced Study: did you have contact with Rossby at all before you went to Princeton?

Smagorinsky: No. My contacts with Rossby were mainly through his interests and involvement with the Princeton project.

Young: O.K. Well, we'll talk more about that when we continue this. So basically we have talked up to the point where you're about to head for Princeton and really where some meteorology was beginning some important revolution, and when we come back, I want to talk about Princeton days and then forming your major sustained career in the Weather Bureau in the fifties. Thanks a lot, Joe.

Smagorinsky: You're welcome, sir.

[Interruption]

Young: Today is May 17th, 1986, we're at the University of Wisconsin and this is John Young, chairman of the Meteorology Department, continuing our interview with Joe Smagorinsky. This is the second part of this interview which we continue on this tape.

O.K. We want to begin talking now about the days from roughly 1950 on. At this time, you had been sent up from Washington, D.C., to the Institute for Advanced Study at Princeton, and I think what we want to talk about here are some of the things that maybe haven't been covered in other descriptions of the Advanced Study group, maybe descriptions you've written in other articles, but we can cover some of that, too. It would be nice maybe if we heard a little bit about the personalities there. You were in a pretty unique position, Joe, when you arrived there, because you were really just beginning a Ph.D program, whereas the other people that were there were in more senior positions and did you feel like a student in this group or did you immediately feel like this was a totally unique experience?

Smagorinsky: Well, certainly, it was both. I was a junior person and I was there primarily as an assistant, and I worked about a third or half of the time, so that I could spend the rest of my time either commuting to New York in order to take courses, or to attend lectures, or to discuss my dissertation. But my dissertation was largely inspired and overseen by Charney. He suggested the original project. I got it approved by Haurwitz, who was my thesis advisor, although my original thesis advisor had been Panofsky, but then he took a job offer at Penn State.

Young: Panofsky in those days was doing more dynamics than micrometeorology and turbulence?

Smagorinsky: I think he was sort of in transition in those days. But in his earlier years--I guess people have forgotten--some of the earliest ideas in objective analysis were his. I worked with him on that, as a matter of fact, and brought some of these ideas into what was going to be the Joint Numerical Weather Prediction Unit. Then this was elaborated on by Cressman. But, to get back, in many ways, Haurwitz didn't have as much to do with my dissertation as he probably would have preferred. He didn't see it until I had a second draft. But I have to thank Charney for, as I said, bugging me on the original idea. The original idea, incidentally, was to try to elucidate the competition between his and Eliassen's paper on the importance of orographic effects on the large-scale, and Sutcliffe's ideas, which were published earlier, on the influence of continentality. They were competing ideas and Charney thought it would be a good idea if Sutcliffe's stuff was done over again, but now with more modern techniques. So you can see how broad Charney was.

Young: Well, I was going to comment on that. It seems interesting, there was a real mission as I understood it, for the Advanced Study program to essentially get this barotropic forecast model going, and yet here's a guy that's a key player in it--Charney--who is on the side advising a person on a full quasi-geostrophic analysis with thermodynamic forcings, and it just shows that Charney was not interested in doing just one thing. He was being a professor and he was ultimately wanting to explore the range of problems.

Smagorinsky: Well, he was thinking ahead. You were right, he was worrying about the validity of barotropic modeling to atmospheric phenomena. He was thinking ahead in terms of baroclinicity in his own work, and his interest in Phillips' work. Phillips' earlier work for his dissertation, which was on this multi-level model, ultimately became the prototype for the first baroclinic geostrophic model which was first run at the Institute. It then became the first operational model at the Joint Numerical Weather Prediction Unit. It didn't last very long. After I left, it was changed back to a barotropic model.
But he was also thinking ahead in terms of longer-term evolutions in the atmosphere, and what maintains the general circulation, and its general features, in particular, the normal patterns of the atmosphere. Say, if you were to have an Earth that was completely uniform, either completely ocean or completely continent, and orographically uniform too. If you were average for many Januaries or many Mays, you just get zonal flow.

Young: Sure.

Smagorinsky: And you don't. So the question is, why are the normals perturbed? Sutcliffe originally thought it was due to continentality, Charney and Eliassen showed that just purely by barotropic forcing, the orography could explain a great deal of the variance. My final conclusion was, based on my dissertation, that the two are indeed competitive and that the continentality dominates in the lower levels, but by the time you get to the upper troposphere, the baroclinic effects poop out and the barotropic orographic effects can still be seen. As a matter of fact, we know that they can even be seen in the stratosphere.

Young: I'd forgotten that you'd also looked at the effect of the surface orographic--

Smagorinsky: I did do some calculations which were unpublished, some one-dimensional calculations. But I finally was able to interpret other people's results and my own results to reconcile the relative accordance. A lot has happened since then; it's been over thirty years since the early fifties. Numerical models have been brought to bear, much more sophisticated models, where linearization and stationarity are removed as driving approximations. I would say the conclusion I drew at that time has evidently stood the test of time.

Well, anyhow, the Institute was a bustling place. There were a lot of itinerant visitors, for a day, or a few days: Lorenz came, Starr came, Rossby came. Then there were other visitors who came for extended periods, for a month, a half-year or a year, people like Bolin, Eric Eady, who incidentally transmitted my dissertation for publication in the **Quarterly Journal**. You see, you can't publish in a quarterly journal of the Royal Meteorological Society without being a member, and I wasn't. He transmitted it for me, and it's the only paper I ever submitted that wasn't bucked back to me for revision.

Young: Fantastic.

Smagorinsky: The only thing--they did revise it themselves--they changed the spelling, to "colour," you know--things of this sort. Well, that was an interesting paper and it was a paper that in the early days was a little controversial. Evidently Rossby had been thinking along these lines, too. I was told, I think by Aksel [Aksel Wiin-Nielsen] that in a lecture that Rossby gave, he talked about these quasi-stationary components. This was in the fifties, and when somebody said, "Say, isn't this what Smagorinsky did?" And he said, "No, no." Somehow he didn't want to (he was apparently a very proud man) be competitive with a young squirt. But I think over a period of time, there have been several papers that have been offshoots of this, that have built on it. Doos wrote a paper some years later, Salzman also did, and then there have been a variety of others. As I said, once we started getting into non-linear, time-dependent models, it could be studied in a far more general way.

Young: Now that theory was essentially using quasi-geostrophic tools--mid-latitude confinement of the disturbances. Were you ever tempted, when you were working on that problem, to extend it to lower latitudes, or was it just so far beyond the scope of the emphasis?

Smagorinsky: It was. You know, meteorology in those days was rather chauvinistic. If you were interested in the tropics, you were a tropical meteorologist, and there were a few institutions that were either wholly devoted to that or partially devoted to it. But there wasn't yet the feeling that we had to understand the whole global circulation. When I went on to my two-level model--my two-level primitive equation model, it was the first time we could look at non-geostrophic modes. When GFDL started in 1955, one of my objectives was to understand the non-geostrophic modes, including those that were particularly tropical, within the limitations of what I did: namely, it was bounded by a wall at the Equator and only had two levels in the vertical. But nevertheless, it was a beginning of being able to style that type of analysis and then probe into the interaction of the tropics with the extra-tropics. Even in a model of that sort, you had to look real hard for the Hadley circulation. I remember when we did our first calculations, the first primitive equation calculation of the general circulation, the first thing I thought I would see right away was an explicit Hadley circulation. I looked and I couldn't find it. We had to very carefully average, so that we didn't let truncation error noise obscure it. We finally were able to find the Hadley circulation despite these pre-conceived notions. Also, the whole question of these non-geostrophic modes on the energetics of the system had never been done before. That's one of the reasons why, although a good part of the work had become completed by 1958, I didn't publish it until 1963. That paper was really about five papers, and I should have published it in segments.

To return to the people at the Institute, a number of people were there from time to time. Fjortoft was there, Eliassen...

Young: Did they have clear-cut roles when they would visit, or were they just treated as smart visiting dignitaries and could kind of latch on to whatever...

Smagorinsky: They worked on problems. Fjortoft was involved in the early ENIAC calculations. As a matter of fact, he was a signatory to the paper that was produced together with Charney and von Neumann. So these guys were much more senior than I was but they were all young men, they were all, at most, in their late thirties, in many cases, not too well-known. Then there was a young fellow who was a student of Eady's, who quite independently did his dissertation on the same subject I did mine, except that I used Charney's baroclinic instability model and he used Eady's baroclinic instability model. It was interesting because we got complementary results, in some cases duplicating, in other cases, different.
His name is Gilchrist, Bruce Gilchrist, and his paper was published in the **Quarterly Journal**, I believe, in the fifties. Bruce left meteorology immediately, he was only one of two students as far as I know that were produced by Eric Eady. The other was Green, John Green.

Young: Was Gilchrist an Englishman?

Smagorinsky: An Englishman, and he came to the United States as a visitor to the Electronic Computer Project, Charney's group, and stayed. And he's now head of the federation of computing societies in the country headquartered at Columbia University.

Young: About twenty-three years ago, when I first started my Ph.D thesis on a two-level model including thermally-forced circulations and baroclinic waves interacting, I was curious about Gilchrist's work, and I called him up. Jule Charney gave me his number and I called him up to find out more about what he had done because somehow I didn't have the reference--

Smagorinsky: He may have been working for IBM at the time.

Young: I think so. He was very pleasant. But anyway, there was that little link for me that has some meaning.
Let me ask, Joe, you referred to yourself not only as "junior" to these other people, but also as an "assistant" at the Advanced Study. Were you being asked to assist with various calculations of the barotropic model, or anything like that on the side or were you able to essentially work on your thesis problem?

Smagorinsky: On both. I was supposed to do both, although they were very generous in letting me work on my thesis problem, and I was able to complete it in two years' time. I came to Princeton in the summer of 1950 and left in March, 1953. In that period of time, I completed all my requirements for the Ph.D. in addition to working there. You have to remember that during this interval that I was there, between '50 and '53, the IAS computer was being built, and a number of us were doing programming in anticipation of use of that computer; and that was programming in machine language. There, of course, was no such thing as software, no such thing as Fortran, I dealt with the instructions of absolute octal--it was a different world. Margaret was a programmer until the time when she finally had to quit to birth our first child.

Young: And that's Anne.

Smagorinsky: That's Anne, right.

So the computer was completed, and one of the first problems that went on it was Phillips' baroclinic model, some variant on it. By then, it was a variant and pretty well bore Charney and Phillips' name both.

Young: Now is this essentially the model that Norman subsequently applied in his famous general circulation experiment, which was published--?

Smagorinsky: Not quite. Well, I guess it had some similarities to it, yes, but the big innovation in the general circulation model was that he had built-in sources and sinks of energy; so that the model could sustain index-cycle type evolutions over long periods of time and not just short extrapolations. It took advantage of what he had learned since he wrote his dissertation. And as I remember it, the building of that general circulation model was actually done on a visit to Oslo, maybe in 1954.

Young: I know Phillips published a paper, I think, in **Tellus**, around 1954-55, on some nice calculations he had done about baroclinic waves and their feedback onto the general circulation, which was done by linear theory, really.

Smagorinsky: Yes.

Young: So there was Norman, there was Fjortoft, you covered most of these people. What did Arnt Eliassen work on when he was at Princeton? Most people are aware of Charney and Eliassen's collaborative work which I always attributed to Charney's visit to Oslo before Princeton. What did Arnt do and how long was he there?

Smagorinsky: Well, I must confess I don't exactly remember, but he was there on a couple of visits and then around 1954 or so, spent some time at UCLA. I remember that because it was there that he produced an unpublished report which outlined a two-level primitive equation model which didn't admit external gravity waves. The model was constrained so that the vertically-integrated divergence (and therefore the surface pressure tendency) was identically zero. Only internal gravity waves were possible. He never actually programmed it to run.

Young: I think I saw the report and I believe it had variable static stability, possibly, as opposed to--

Smagorinsky: No, it had to be specified. It appeared as a specified co-efficient. It was only a two-level model. After I had left--I'm jumping ahead a little now--after we started the predecessor to the GFDL (it was called the General Circulation Research Section), in October, 1955, we were casting around to decide what model we would use, and Charney at that time suggested that we take a strong look at the Eliassen model. And we did, and it turned out that nobody had successfully run it. And we had to come up with a couple of alternative ways of solving the system of equations. It depends on how you want to satisfy the constraints and I wrote a paper on this at one time, on the method of solution. It was published in **Monthly Weather Review** and eventually we got it to work. One of the people who was key was a programmer-meteorologist whose name was Bob Strickler. He had been Eliassen's assistant temporarily at UCLA during Eliassen's visit, and then on a visit that I made in early 1960, no, it was before that, maybe in '59, to UCLA, Bob Strickler agreed to come to work for us. And so he was the first programmer on that project.

Young: Yes, I remember him. In fact, he's even appeared I think as an author on some of the early model papers.

Smagorinsky: He worked with Manabe on some of the radiative convective models and parameterizations. He's retired now, retired about a year or so ago.

Young: Well, maybe we ought to see whether any other things we want to talk about on the Advanced Study before we go on to the Weather Bureau days. Were there any other personalities you want to mention?

Smagorinsky: No, except that one of the things that was happening that ultimately had an important effect on the group was that von Neumann was getting interested in national political and military affairs. And he had always been very active as a consultant to the Defense Department on anti-ballistic missiles, and involved also with the AEC. He was somewhat typical of a number of Hungarians who left Hungary in the thirties--they were also avid anti-Communists; Teller is another example. But von Neumann wasn't nearly as vociferous as Teller was. Von Neumann was offered a job around 1954 in Washington as an Atomic Energy Commissioner, and surprisingly, he took it.

Young: My goodness.

Smagorinsky: It was just around that time that he suggested to the Air Force, Navy and the Weather Bureau to set up a group to do general circulation modeling. I wasn't asked to head up that group at the beginning, it was actually Wexler, effectively, but within a year, I don't quite remember the details, I ended up being the head of it.

Young: This was the old JNWP?

Smagorinsky: No. This was GCRS, General Circulation Research Section, this was later. There were two groups: one, JNWPU, was the operational forecasting group to do short-range weather forecasting numerically that was organized under the Air Force, Navy and Weather Bureau, that had started in '54. Dan Rex and I were the people who essentially drew up the organization for it, the budget, the staffing, space and everything else. I was involved in getting the computer, which was an IBM 701. I was head of one of the sections, the computing section, which may still exist as an entity at NMC, the successor organization. And in the middle of all of this, there was a second move to form another group which would be a general circulation modeling group.

Young: Wow. That was quite a double-barreled responsibility, potentially.

Smagorinsky: Yes. So it was the second group which was the predecessor to GFDL.

Young: The GCRL. This was out of--

Smagorinsky: GCRS first. Then, "L" a few years later, then GFDL.

Young: Did you--"L" for laboratory, as opposed to section? Did you change the bureaucratic terminology there?

Smagorinsky: I didn't, no. Somebody thought it would be more respectable to call research groups "laboratories" rather than "sections." Section sounds very bureaucratic. So they said, "Do you mind very much if the 'S' were replaced with an 'L'?" I said, "I don't care."

Young: I always assumed that you were the one who instigated that.

Smagorinsky: No, no I didn't. As a matter of fact, I was rather conservative about the whole thing.

After Bob White came in, he sort of wanted to re-cast the Weather Bureau in his own image, a natural sort of thing to do. One of the things he wanted to do was to change the name of our laboratory. And this was seven years after it had been formed, about '62. So he said, "Joe, you know, General Circulation Research Laboratory, this doesn't mean a goddamn thing to guys up on the Hill. Why don't you change it to something that's sexier?" So I said, "What?" and he said, "Geophysical Fluid Dynamics Laboratory." He was the one who suggested it. I said, "Is that supposed to be more understandable to the people on the Hill?" I said, "I'm rather reluctant to do it, Bob, because we're already known all over the world by this one name--General Circulation Research Laboratory." He said, "But you're doing more than general circulation research." I said, "Yes, but there's no one name that is descriptive of everything we're doing and if it were, it wouldn't be for long. The Great Atlantic and Pacific Tea Company had been selling other things besides tea and yet they cling to their name because they just want to have a unique identity." Well, after a year of nagging, I finally gave up. But these were not my ideas.

Young: And now we've had twenty-four years of GFDL.

Smagorinsky: Incidentally, the name wasn't original with Bob. The name, as far as I know, was originated by the Woods Hole Summer Study, which was started in the late fifties. And of course that name is not unique any longer. There's a group in the British Met. Office under Hyde with that name, not the laboratory part of it, but geophysical fluid dynamics. There's a group in Florida with that name under Dick Pfeffer. I think there were others too. And "g.f.d." is now a generic term.

Young: Absolutely.

Smagorinsky: Small letters.

Young: Better change tapes.

END OF TAPE 1, SIDE 2

INTERVIEW OF JOSEPH SMAGORINSKY

TAPE 2, SIDE 1

Young: This is the beginning of the second cassette. This is 21 May, 1986, John Young and Joe Smagorinsky. This should be the last side of the interview.

We were just talking, Joe, about the name, really, of the modelling group that you were the first head of. It started with the General Circulation Research Section, and after seven years of existence, it had been changed by Bob White's suggestion to the Geophysical Fluid Dynamics Lab, GFDL. I want to backtrack just a moment now, to get a clearer feeling of how the evolution really worked in those first seven years of your leadership. The Weather Bureau in the mid-fifties when you were getting your start with the General Circulation Research Section was not very much a hotbed of intellectual revolution. Maybe you care to comment a little bit about some of the kinds of support you got, maybe the role of Reichelderfer, the head of the Weather Bureau, etc.

Smagorinsky: I think Reichelderfer's role often doesn't get enough emphasis and credit. The whole thing came out of the success of Phillips' general circulation calculation; it was obvious to von Neumann that something should be done to exploit that and with a separate dedicated effort. It was already evident that the group at the Institute might not last long because von Neumann was leaving, and the Institute was anxious not to have that activity perpetuated there. As it turned out, none of the modelling work that was started at the Institute followed Phillips and Charney to MIT. They did dynamics, but not computer modelling. And I think von Neumann sort of saw that this might happen. He thought there should be an effort devoted to exploiting Phillips' findings, which were really remarkable in their day.

He approached the Air Force, Weather Bureau, and Navy, and wrote up a long document which I have a copy of, to justify an effort--a special effort that he thought the Air Force, Navy and the Weather Bureau should support. And early on--this is now in 1955--he was also working a deal for this group to be set up at the University of Maryland, but an agreement couldn't be reached with the Institute of Fluid Mechanics there, which was a new institute. It still exists, I think. So ultimately, it was put within the Weather Bureau, with support by the Air Force and the Navy. My condition for heading it up was to have certain freedoms that were not common to the Weather Bureau at the time. You have to remember up until the middle fifties, the Weather Bureau was a very conservative organization, it didn't dare use the word research in its job titles, or in its organizational titles. For example, Harry Wexler's organization was known as "Special Scientific Services." It was Sputnik that changed that. And Reichelderfer has to be given credit for a number of innovations that he really was not at home with. One was to set up the satellite organization; another was to set up JNWPU; another was to set up the GCRS. I asked Reichelderfer, I said:

"Dr. Reichelderfer [or "Chief," as we always used to call him], there are two things I need besides the monies. First of all, you were going to give me a certain number of positions that I am going to have available. If I don't find the guy I want, I don't want that position taken away from me at the end of the year." And he said, "What?" I said, "Yes, it's absolutely necessary in order to ensure that I get only the best people, and that I not be penalized for waiting for the best." And he said O.K. And it turns out, that of all the initial vacancies I had, the last one was used up about five years later.

The other thing I said was, "I don't want to be forced to use cast-offs from elsewhere in the Weather Bureau, I want to be able to hire from the outside." Most all of the scientists were hired from the outside. These were both tremendous departures and Reichelderfer honored them with full backing. He and Harry Wexler and some of the people who worked for Harry Wexler like Bob Culnan, whom you knew, and a fellow by the name of Ted Gleiter who was head of personnel at the time, ultimately was head of administration for NOAA. They all backed us but left us alone to set our own standards and to do our work. And there were some elements in the Weather Bureau, I was told, that were throwing stones at us. They were jealous of our position, and the way we operated, but we were protected. And in those days, we needed the protection. Now one might be able to acknowledge that the laboratory has demonstrated that [it] can do good work and therefore has earned special consideration for the future. In those days, it hadn't produced anything yet. And so there had to be a strong article of faith built into the whole operation. One important thing that happened within the year was that the Air Force and Navy withdrew their money. In part this was connected with the fact that Charney moved to M.I.T. You have to remember that Reichelderfer's original contribution from the Weather Bureau was not money that had been appropriated for this purpose--this was money that was bootlegged from elsewhere around the Weather Bureau. He said, "Smag, don't worry about it. I'll make it up."

Young: That was very bold on his part. This idea that there was Defense Department money, like Air Force money being lost to the project after a year, was that because Charney went to MIT. and he was getting grants to--

Smagorinsky: He needed grants, yes. I think he made a request for more and they said, "We're already pouring money into two joint groups."

Young: But the net effect was to take money from one group and give it to another university group that was starting up that in fact didn't really do the kinds of things that you had been doing and were to do.

Smagorinsky: Were to do, yes, that's true, I never thought of it that way.

Young: There was never an MIT GFDL-type activity that went on, that was just a mainline university research activity under Charney and Phillips, and never a modelling group that I ever thought of at MIT.

Smagorinsky: Charney, I think, probably also felt that our group would be a service group to him all along. I thought that we should cooperate and collaborate with them, which we did at the beginning. As a matter of fact, our initial successes were very heavily dependent on Charney and Phillips. No question at all about that.

Young: I knew Phillips had been involved with the JNWP activities--

Smagorinsky: No, he was involved with us, too. He was a great help, gave a lot of good advice, specific advice and for the first couple of years at least, the first two years, anyhow, their advice was essential in getting us started. We eventually weaned away and were able to continue on our own, develop our own ideas and our own thrust. We did start off on the General Circulation project, and that's why the title of the group was quite appropriate.

Young: At what point did you drop your computing section responsibilities so that you could concentrate just on the general circulation?

Smagorinsky: Oh, I had to change organizations to do that. JNWPU was a separate organization from GCRS. I was involved in the organization of JNWP, that's the predecessor of NMC, and also its founding, and the choice of a computer and its delivery, and the supervision of programming the first operational model. Then the group started, I believe, in May--March or May--formally started in 1955. From the day that it started, I was head of the computing section. There were three sections: development, computing, and analysis, I believe.

Young: Was this out in Suitland at this point?

Smagorinsky: This was in Suitland.

Young: So you left the downtown Weather Bureau offices at--

Smagorinsky: Right. Before the year was out, by October, I was asked to change jobs.

Young: I see.

Smagorinsky: And to head up this other group.

Young: O.K. So you actually didn't have head titles for both groups--

Smagorinsky: Not at the same time.

Young: I see. I see.

Smagorinsky: No, these were separate, different jobs. As a matter of fact, when GCRS started, we needed space and Namias made some available to us. In Suitland, about six or seven offices. And I was the first employee.

Young: Namias' project in those days was the extended-range prediction section, or something like that?

Smagorinsky: Yes. I forget the exact name of it. JNWPU had uniformed people and civilians from the Air Force and Navy that were seconded. GCRS was not, it consisted of Weather Bureau employees. But very quickly, it became whole by a Weather Bureau organization, within the year.

Young: That's very interesting. Your portrait of Reichelderfer is a very sympathetic one in that this wasn't so much a man who somehow saw something exciting happening in his own eyes and therefore saw that you could pull it off, but he was getting some advice from someone else and he was willing to respond to that advice.

Smagorinsky: Well, it was his neck. If he made a mistake in responding to that advice, his advisors wouldn't be hung, he would have been hung.

Young: But the pushing on that advice was coming from people like, ultimately, von Neumann?

Smagorinsky: Von Neumann and Wexler.

Young: Well, you must have greatly respected them to have done that. It's quite a statement. I think it's very appropriate that he was able to show up for your twenty-fifth anniversary celebration a few years ago--

Smagorinsky: I would have gone to Washington and driven him myself, if necessary. He was that important to us.

Young: That was a real surprise to me, very pleasant. Is he still alive?

Smagorinsky: No, he died. He died about two or three years ago.

Young: Died around '84?

Smagorinsky: In his eighties, yes. Fantastic character.

Young: Why don't you tell me a bit about the specific recruitments you made to--you told how Reichelderfer gave you the right kinds of arrangements for your finding the people. What about some of the personalities? You mentioned Strickler, what about key scientists for the lab--as a long-term investment--?

Smagorinsky: The first guy who was key, is someone you probably don't even know. His name is George Collins, who was in JNWP, and I recruited him away from there. He was the only one I took. George was really essential in helping us, he wrote this precipitation paper with me, which is one of the first attempts to lay a dynamical framework for precipitation.

Young: Which journal?

Smagorinsky: **Monthly Weather Review**. Historically, it was quite important. George left a few years later.

Young: Where did he go?

Smagorinsky: He went to work for a private programming firm--he was a meteorologist. He originally had worked for Roger Allen at the Weather Bureau. I probably hired him for JNWP. He also was the one who started programming this two-level model, the primitive equation model, and unfortunately, he left in the middle of it.

Young: I see. Did you have a name for that model, by the way? Was it the Mark something-series?

Smagorinsky: No, the Mark series were the nine-level models.

Young: O.K. This came later.

Smagorinsky: The nine-level models came later; we started them in 1958. The two-level model was something we started in 1955, this was the model that was based on Eliassen's dynamic baroclinic framework.
Then I hired two guys, one is Leith Holloway, who ultimately became our sterling, premier programmer, and the other was Rod Graham, who ultimately became my chief administrative assistant. Both of them had a history in computation, and they took over that model, that two-level model, and very systematically got it together.

Young: Now, Holloway, wasn't he a statistician when he went to--

Smagorinsky: No, he was trained as a meteorologist. I believe he had worked for Allen, too. Graham, I think, I hired from the outside. I think he was in California at the time. I wish I could remember these details. Then I was casting around for somebody to work with me on the idea level of general circulation modelling. So far, I hadn't done that. I had been reading some papers about Japanese scientists, came across two names that seemed to crop up time after time. One was Manabe, the other was Miyakoda. The thing that intrigued me about Manabe's name is that it didn't so much appear on papers that he had written, but on papers that his colleagues had written where they were crediting him with some of the basic ideas. He didn't have a Ph.D yet, he hadn't finished his dissertation, I think, at the time. So I made an offer to him--

Young: He was a student at the time?

Smagorinsky: --he came as a visitor, well, he had just about finished, close to finishing everything, but didn't have his Ph.D yet. I couldn't offer him a permanent job, he was an alien. I needed again very special permission to hire a foreigner. It really probably had never been done, but that started a precedent.

Young: This was what year now?

Smagorinsky: He came in 1958. Also, I had read a little bit about an attempt by Joanne Simpson and a fellow by the name of Witte in Stockholm who had tried to model with convection.

Young: Yes, right.

Smagorinsky: And I read their papers and I thought that this was something we should look into. And I heard about a guy who was a bright but brash young graduate student at Florida State University. I went down there to see him; his name was Douglas Lilly. And I told him I wanted him to work on convective modelling--he had been working on tropical meteorology. He had no experience at all in modelling. Modelling was an entirely new enterprise; nobody had much experience with it, especially going into these new areas. So I also told Doug that I wanted him to model turbulence. He never did do that at GFDL. He did it later on, but never at GFDL. So I hired him, and he actually finished his dissertation in Washington.

Young: I recall his dissertation was not--modelling--

Smagorinsky: No, no--

Young: Kind of a theoretical treatment of moist convection, kind of the slice method. Worrying about the non-linearity and the heating and what-not--

Smagorinsky: Manabe's work up until that point, his own personal work, had been on air mass transformations over the Sea of Japan.

Young: Yes, I remember. That was published--

Smagorinsky: Manabe also had no experience at all in modelling. So all these people had to learn modelling. In those days, we had to create programmers. There was no such thing as hiring a programmer.

Young: What about Miyakoda? What year did he come?

Smagorinsky: He came later, in the early sixties.

Young: O.K. We'll get to him in a minute.

Smagorinsky: The next thing I wanted to do was oceanography, for two reasons. One is, I felt that our techniques could be applied to the oceans in modelling, even though a lot of the basic physics wasn't understood and dynamics wasn't understood the way it was in the atmosphere. The other thing is, I knew that if we were going to be interested in longer-term evolutions of the general circulation, then we would have to worry about the oceans, too.
So the first guy I went after was Kraus, who was a German oceanographer. He led me on, then used my offer to get himself a promotion. I think he may be retired now. I was looking for a two-year visitor. Then I went after a Japanese oceanographer by the name of Yoshida--

Young: Oh, yes, of Yoshida Jet.

Smagorinsky: --who died a few years ago. He said he had a problem with his wife--she couldn't be away from Japan for too long because of her health. And I said, "It's going to take a minimum of two years to do anything. It'll take you a year to learn the techniques. It'll take a minimum of another year to get to the point where you have something going, and that's optimistic. So I didn't see how you could do anything in a year."
Meanwhile, Manabe and Lilly had gone to a GFD summer program course at Woods Hole. And they met a guy by the name of Kirk Bryan who was a meteorologist, and a student of Lorenz, I think.

Young: He had done a Ph.D around 1956, at M.I.T.

Smagorinsky: He, Pfeffer and Saltzman were contemporaries. And the oceanography he had learned was by osmosis. He had never done any modelling, he had never really done much in the way of theory either, I guess, at that point, and he was now at Woods Hole. And we started talking and by 1960, he showed up.
Meanwhile, I was talking to a Danish fellow by the name of Aksel Wiin-Nielsen, who at that time was still at JNWP as a visitor and who was destined to go back to Europe. He was very much interested in coming to GFDL. I had arranged a visa for him and everything else, and then I went to the Tokyo Conference in 1960. When I came back, I found a letter on my desk saying, "I decided to go to NCAR instead."

Young: This was nineteen--?

Smagorinsky: Sixty.

Young: December, 1960. NCAR was practically a piece of paper at that time.

Smagorinsky: That's right, that's when NCAR formed. He was one of the first people there. So he never did come, and one of the things about Aksel was that he said when I was courting him, "Well, you know me, Joe, if I can't see the end of a piece of work, the real end, namely finished writing it up and sending it off, in six months, I don't start it." He said, "I know that the basic principle that you operate under is long-term commitment to projects", which was a novelty in those days. It was still quite common to publish or perish, and so it was the practice to work on projects that you could have quick turnover, not because of your personal temperament, but because this is the way the project monitors wanted it. Which meant that if there were a difficult problem that required a big investment of your career, it was not undertaken. So this was the first thing I told people when they came: I said, "We're problem-oriented. Essentially, this is the problem that we want to solve and I'm willing to tolerate your not publishing at the beginning provided you set your sights ahead." And Manabe got a couple of promotions before he published his first paper. I was able to prove to the senior promotion board, that he was worth these promotions. This was Reichelderfer's Weather Bureau--

Young: This is still Reichelderfer's--because White came in--

Smagorinsky: White came in '62 or '63. Kirk Bryan didn't quite start off the way I wanted him to, but it turns out that what he did was better. I wanted him to start looking at the baroclinic ocean right away and he thought it would be better to start out with the wind-driven ocean, a barotropic problem.

Young: Which was very much the rage of the theoreticians in those days.

Smagorinsky: In other words, he wanted first to see if he could duplicate some linear results. So he started off with that and went on to the heat-driven ocean and then to the thermohaline ocean, idealized basins first and then real basins. It wasn't until the late sixties that I finally got him and Manabe together. There was a little difficulty at the beginning getting them together to work on the combined problem, coupled models.

O.K., so those were some of the very early initiatives. There was one other guy involved early on--he had been a Weather Bureau meteorologist by the name of Wayne Sangster. He did some of the first work on forecasting models, which was not our primary business at that time. He was involved in the paper that ultimately produced the 1965 results of four-day prediction. Strickler was also involved in this.

Young: I associate Sangster's name with some analysis papers.

Smagorinsky: Well, he finally went to work for the Kansas City Severe Storms Center.

We were also beginning to develop a visiting scientists' program. Our first visiting scientist was a very distinguished scientist by the name of Fritz Möller, who was a radiation expert; he's now been dead about four years. He was supposed to show us how to build radiation models; it turns out Manabe did most of it himself. But he came, and he was getting paid much more than I was. I was able to arrange that.

Young: You violated a lot of codes.

Smagorinsky: He was two grades higher than me.

Young: Möller came in what year?

Smagorinsky : Around '58 or so. He was the first visiting scientist we ever had. Then we started getting a few others. A young fellow by the name of Gareth Williams had just gotten his Ph.D in Wales under D. R. Davies.

Young: Davies had written a totally unintelligible and overly-ambitious paper trying to explain all the dishpan circulations, I remember.

Smagorinsky: Williams came as a visiting scientist, too. Incidentally, in most cases, most of our permanent people originally came as visiting scientists and about one out of every ten became permanent. What I wanted him to do was to see if he could reproduce dishpan experiments on a computer and then to tell us why. Well, that was moderately successful, but not completely. And when he got through with that, I said, "Now you're ready for planetary atmospheres." And he started doing that essentially after he got to Princeton some years later.

In the mid-sixties, we started seriously to go into small-scale phenomenology. In just about every one of the fields that I've described--general circulation modelling, extended prediction, ocean modelling, dishpan modelling, planetary atmosphere modelling--we were virtually the innovators. But there was one area which we didn't start that we originally had no intention of going into, but where our institution sort of twisted my arm. First was hurricanes. Bob White said to me, "Joe, I want you to do hurricane modelling." (Despite the fact there was already work going on in two different places in the Weather Service--one in Florida, and the other one in Washington.) That's when I persuaded Kurihara, who had been there on a visit and I hired him back on the second visit. Miyakoda came in two visits, one a short visit and then permanently. Miyakoda eventually took over the forecasting that I was temporarily head of until about the middle sixties. Kurihara came and started hurricane modelling. Then a few years later, Dick Hallgren, who was assistant administrator in those days--I forget what his portfolio was--came to me and said, "Joe, I think you should go into mesoscale modelling. If you don't do it, somebody else will do it and I want you to do it." In none of these cases, did we ever ask for or receive additional money or additional positions as a condition. Those resources came into the lab unmarked. And one of the reasons for our success is that flexibility given to us. We were given a computer, we were given money, we were given positions, and told to do whatever we thought we could on our own initiative. These were the first times there was any programmatic pressure. Oh, there was one attempt early on to change the name of the lab to the Weather Modification Lab.

Young: Oh, boy.

Smagorinsky: And I refused. Anyhow, the mesoscale work started under Isidoro Orlanski. He came after his getting a Ph.D from M.I.T.

Young: Yes, he had quite a reputation.

Smagorinsky: Maybe I should say a word or two now about what finally took us to Princeton. Up until 1962, the lab had been using other people's computers--JNWP's computers, the 701 and the 704; the computer over at the Bureau of Standards--we were buying time. Eventually we got to the point where we could justify a big computer for ourselves, and that was STRETCH, an IBM 7030. It was a big jump. It was going to be delivered around '62, and space in Suitland was not suitable at all. We started looking around, we looked in many places. Eventually, I.B.M. offered their premises on 615 Pennsylvania Avenue, which had been the Vanguard Space Computing Center. So it was built for a big computer, in terms of air conditioning, false floors and power. It was an ideal space for us, a lovely spot just opposite the National Gallery of Art and the Federal Trade Commission. We often wondered when people came to visit us, whether they really wanted to see us or our neighbors.

Young: I can remember going across the street to lunch with you, at the Federal Trade up in that triangular room, looking down Pennsylvania Avenue to the Capitol.

Smagorinsky: Well, 615 Pennsylvania Avenue no longer exists. It is a new building there, the F.B.I. took it over after we left. The building has been knocked down, there's a skyscraper there about seven stories--which is a skyscraper in Washington. But we were there from '62 to '68. Getting on to the middle sixties, well, we changed computers, we went from STRETCH to the CDC 6600, which we accessed remotely. And then we went over to Univac 1108's, and there were several things happening. First of all, we already knew that block was destined to be razed. It turned out that it would take about fifteen or twenty years for it to happen, although it was already being threatened then. There were riots in Washington in about 1966; we had to doublepane the windows and do all sorts of things like this. It was hard for people to come to work at night (we ran around the clock), especially women programmers and computer operators. But I think these were things that were pushing us away from Washington, but there were other things that were pulling us away. The laboratory from the very beginning had a very strong postdoctoral program, and this is how we acquired many, if not most, of our scientists, and in a way, we were training people. We often thought that it would be better if we could get people to be trained at an earlier stage in their career, namely at the doctoral level. This wasn't being done anywhere else in the country. People were not being trained in modelling anywhere. As a matter of fact, when you get right down to it, they still aren't in many ways, you know, comprehensively. Although it's much better than it ever was then. So I had discussions with Bob White, and he was very supportive of the idea of renewing the notion of doing something jointly with a university.

Young: Renewing the notion?

Smagorinsky: Yes, renewing the notion--

Young: --seems to be a pretty radical notion--

Smagorinsky: Well, renewing the notion because you'll remember that I told you that von Neumann originally wanted us to go to the University of Maryland.

Young: Oh, O.K. Fifteen years before.

Smagorinsky: This was around '66 and '67 now. And quite unsolicited, I had gotten a number of invitations, one of which was from the University of Wisconsin. There were several others. When it got to be known that I was having informal discussions, in some cases I got a letter and was asked to come visit, maybe give a talk and maybe talk to people. Others came and informally asked. So, formally and informally, there were about seven involvements, the last of which was the Princeton one. That was purely accidental. Princeton had turned down Phillips and Charney when they were free in 1955, and were looking around. They had turned down the notion of having such a group, and it was only by accident that I was talking to a faculty member by the name of George Mellor, who came to visit me at 615 Pennsylvania Avenue. He said, "Did you ever think about an academic involvement?" I was telling him, "Well, we're in the midst of informal discussions now." And he went back to Princeton, talked to senior faculty, administration and all sorts of people. I don't want to go into the reasons for it, but there were many criteria which we considered in trying to come to a decision as to which to choose of the many enticing offers we had, all for different reasons--some very fine institutions.

Young: Let me flip the tape.

END OF TAPE 2, SIDE 1

INTERVIEW OF JOSEPH SMAGORINSKY

TAPE 2, SIDE 2

Young: We were talking about the decision to go to Princeton in the late sixties.

Smagorinsky: Well, based on many discussions, many visits, the final decision was ultimately made by the senior scientists at GFDL. And, well, let me say it this way: all I could make was the recommendation. That recommendation was never tested by those who had to give final authority to make the move. Incidentally, making that move today would be much more difficult to do. But it was done. It turned out to be awkward because the Secretary of Commerce, who had to sign this thing, was also a Princeton alumnus, so we had to get him out of the circuit somehow. So Bob White ultimately signed. He and the president of the university signed the memorandum of understanding and we moved in '68, about August to October of 1968. And part of the agreement was to set up an academic entity; that was our primary purpose. We were not just going to sit prettily on a campus. We could have done that in hundreds of places, maybe even nicer places. That was not the object. The object was to establish an academic entity which could produce students of the type that would be needed around the country, and that we thought we had a special ability to do.
Just about the same time, CIRES (Cooperative Institute for Research in Environmental Studies) was set up in Boulder, the first of the cooperative institutes to be set up. There are about eight or nine now, including one here in Wisconsin. Almost all of them looked at the GFDL setup in Princeton and rejected it. So that one remains unique. It remains unique in terms of its academic commitment. It's not just an occasional professor giving an occasional lecture. Ten percent of each involved GFDL member's time is devoted to this program. Most all of the others are ways of enlarging the flexibility of money.

Young: Sure.

Smagorinsky: This is also a side-product of the Princeton arrangement with GFDL, but it is certainly not the primary one. And if it were to disappear as a motivation, the program would still live, and still have a reason for living.

Young: Is this totally funded by the government, or does Princeton kick in any--

Smagorinsky: Princeton kicks in several faculty positions and salaries.
Young: Facilities?

Smagorinsky: Princeton's getting a good deal. For one thing, the program now is nominally an eighteen-year program. It stands completely on its own, and is very creditable to the university. Instead of living off the university's reputation, it contributes to the university's reputation, and I think everybody knows that.

What I think is most important is that our field--the international field, of modelling geophysical systems, has a source of well-trained people. These people are going all over. We don't produce many; a typical admissions class is about four or five, of which about 80% survive, so it only produces about four, or three, Ph.D's each year, not many. But we think they're pretty well qualified. Most of them can work in oceanography and meteorology just as easily, although they wrote their dissertation on something specific. But that has really been a great source of satisfaction, and it's a quality effort that's developed a good reputation of its own. I think it has a comfortable home, administratively, at the university. I was very careful at the beginning. One of my conditions at the beginning was that the program not lose its identity before it starts by being buried administratively in an existing entity. So first of all, I insisted that the program be involved in several different departments, sponsoring departments. There were three primary sponsoring departments and about three or four others that were secondary. The primary ones were aerospace and mechanical sciences, civil engineering, and geological and geophysical sciences. Secondary ones were astrophysics, chemical engineering, statistics, and applied mathematics, which also is a program in its own right. And those departments lie in two different schools at the university: one in the school of engineering, the other in natural sciences. It was only after about ten or twelve years that they started saying, "Well, it's really untidy for us--we'd like to put you in a single department." So I said, "I'm not concerned, because we have our own reputation. We're known as the Geophysical Fluid Dynamics Program to the outside world. We have developed a base of our own, and now I'm not uptight about it." I said, "The only thing I insist on is that program retain its identity within the department, and attract its own students, approve of its own students, maybe with an oversight by the department if it wishes, but the responsibilities for teaching, curriculum and dissertation supervision." They said, "O.K."

Young: So what department?

Smagorinsky: Geological and Geophysical Sciences, for the reason that we had more of common interests with them than anyone else.

Young: Has that worked out?

Smagorinsky: Yes--well, every once in a while there's a test of tranquility. But it has worked out well. Our postdoctoral program is very strong. I still feel I have a strong stake in it and certainly a strong interest and there are still many remnants of my being involved in the past.

Young: Absolutely. You probably don't miss the administrative responsibilities.

Smagorinsky: When I was younger, that was part of the whole challenge of it and I think I undertook the job with zest and I think I did it reasonably well. And I didn't do it at the sacrifice of the intellectual objectives--which is one thing that most administrators do. The first thing they sacrifice is the intellectual objective in order to have an administrative victory. The idea is to do both, and to succeed at doing both.

Young: Do you think it would have been harder to protect the lab with tight money times coming up?

Smagorinsky: No, I have a pretty positive attitude on money. Over the years, I could have gotten three times as much money as we had, but I purposely turned it down because it had strings attached to it, in terms of programmatic control. The lab could have been three times larger, and I think it would have impeded the ability to communicate.

Young: But you would have been hiring out essentially scientists to--

Smagorinsky: Job-shop, yes. And I think is, again, one of several reasons for the success of the lab. The lab retained its integrity, not arrogantly. It's one thing to say, "I'm not interested in being pushed around, I want to make up my own mind," and then march off and do nothing. I think we did something.
As good as GFDL is, it's got its weaknesses. And I knew what they were, in terms of weak programs and weak people. The two usually went together. During tight times, I would use that as the opportunity to clean house, and make a virtue out of a necessity.

Young: Did you have to do a lot of outright firing...or is it more the message getting through that there's no further future?

Smagorinsky: Practically, it is the message. Pay him his salary, but put him where he doesn't incur any other costs. He occupies a position, but at least you're not wasting other resources, which go well beyond his salary. That's in the government. At a university, similar situations exist where you can't easily fire, especially tenured faculty; it can't be done.
Some directors say, "These are hard times, I can't manage, I'm doomed to failure." Those guys shouldn't be in a management position. Too many people were brought up during easy times where they didn't have to think very hard and they could hire indiscriminately or create programs indiscriminately and not oversee them in a very responsible way. Just based on the last thirty years of good times--well, it's not quite thirty years, it's less than that--a lot of mediocrity has crept into our field, and I think a lot of that could be cut out. And there's room for cuts to be made. It can only be done once, but it hasn't been done once. But I think the science would be healthier, actually, provided that could be done in a very creative way.

Young: You're speaking there sort of as a--the perspective of a director of the lab, and in the government you also find that universities would sort of be the same way.

What about your current job as President of the American Meteorological Society? Do you see that kind of philosophy that you just espoused about "leaner and meaner" professional staffing through improved standards of--do you see that as something you have as one of your goals with the AMS presidency?

Smagorinsky: Well, I don't think the situation is comparable at all. First of all, the AMS presidency is almost a non-job, and it's carefully devised that way. Anybody going into it should take the time to find that out. There's virtually no power to the presidency at all; as Suomi says, "It's like a Tokkaido Express--oops and you're through!" I'm halfway through, there's hardly anything I can do--there are no interim powers that the president has between meetings of the Executive Committee.

Young: How often does the Committee meet?

Smagorinsky: Four times a year. That's only by custom. It could meet for longer periods of time, and it could meet more often. It could meet in extraordinary session, by telephone conference. The power in the Society realistically is vested in the Executive Committee. The Executive Committee consists of the president, the future president, the last two past presidents and two other council members. They are six voting members. There are two more people who are non-voting members, that's the executive director and the secretary-treasurer. So the Executive Committee does have powers; its power is given it by the Council that legally has the corporate authority, like a Board of Directors. The only role a president plays is in how creatively and persuasively he presides over meetings. I don't see how the Society president can do very much compared to a laboratory director or a chairman of a department, in government or otherwise. They're just different kinds of things.

Young: It seems like you're accepting this. Is this just the realist in you or do you think that--

Smagorinsky: What, in the Society?

Young: --the limitations of the presidency--

Smagorinsky: Yes. I think it's a good idea, because--well, let's put it very simply: Say the wrong guy gets elected. It's very easily done because the way nominations are done and the way the election takes place has very little to do with the guy's measured competence for the job. It's a beauty contest. Say the wrong guy gets in. If he has power, he can do a hell of a lot of damage.

But yet, there are opportunities for some leadership. It's just not what you think it is, it's more an honorific thing, it was designed to be that way. For a guy to have more influence, he'd have to be in the job for about three years, so that anything he starts, he stands a chance to see through. He has to know the job.

You have only one year to spin up for it and that's at four meetings of the Executive Committee as the president-elect. Unless you've had some miscellaneous experience before that, and I had very little actually--I was only a councillor once, you have very little preparation. A lot of power de facto resides in the executive director of the Society, and I think a lot of people don't understand that. And it depends on the executive director to know what his bounds are, because he can go beyond those fairly easily.

Young: And of course we've had the same one for forty years now.

Smagorinsky: Ken Spengler has been very good. I think what happens inevitably in being around for forty years is that it's hard to distinguish the man from the job. Forty years is too long. I would say, ten years, fifteen at the most, would make more sense. Don't take a guy who's in his thirties. Take one who's in his early fifties, maybe.

Young: This will be a very interesting transition coming up within the next couple of years with Spengler leaving.

Smagorinsky: That's right. Also, you know the Society grew by at least an order of magnitude since he's been in the job. It was very easy to be a full-time employee and chief of staff of, to begin with. Ken still tends to do many things the way he did forty years ago, which means that a lot of commitments aren't written down, a lot of things that may not be known to anybody but Ken--not because he's hidden them from anybody, but because he's able to handle the knowledge and the operation that way. Yet if something were to happen suddenly to Ken--you know, he's around seventy years old, approximately--it would be difficult to dredge out what the Society is involved in. Some things would be very easy for people to know, but there are others where there was purely a verbal exchange. So that gets to be a problem and I think that anybody coming in has to recognize that the Society now has to do things on a more formal and businesslike basis. It makes it easier for a transition, makes it easier in case of an emergency and I think, most important, where there are legal issues that come up, you have something to base a legal confrontation on.

Young: O.K. We've pretty well taken us into the Princeton years. We haven't talked too much about key staff additions in the later sixties and early seventies. Are there any individuals you want to mention there as far as the GFDL is concerned?

Smagorinsky: Well, in the last two or three years, despite hard times, I still followed the principle of trying to add very, very good young people at the bottom. And I think I succeeded. Some I added myself, others I put in position for it and the final action occurred after I left there. One is Isaac Held, another is George Philander, who was a foreigner and that was a special problem. There was a Catch-22 because he came from South Africa. He's a "colored," and we couldn't hire him for a while because he came from a country that discriminated against Blacks.

Young: What a paradox.

Smagorinsky: Yes, isn't that. He finally made it.

Then there was Gabriel Lau, who didn't make it until after I left, but I had gotten it all set in motion for him. He's another case of a foreigner, he's a Hong Kong national. And then Ray Pierrehumbert. So those are four very good guys that we got in say, the last six, seven years.

Young: You're happy about the intellectual future for GFDL. You sound very happy.

Smagorinsky: No question at all. I think that even in hard times, the people who are GFDL's bosses, such as Joe Fletcher, Vern Derr, Tony Calio--I think they understand GFDL and they're proud of it and want to protect it. Some hard decisions have been made in NOAA, and I think GFDL has been reasonably well-protected--not completely, but that's just one of those things.

There are pressures within NOAA to take an important step backwards, and to put the research under the control of the operations. There are pressures from the outside, there are pressures from the inside.

Young: That's what I worried about when I heard about your retirement much earlier than you needed to retire, because I always felt that you were a person who had carved out a wonderful institution and you knew how to protect it and you could play hardball if you had to and I wondered whether the directorship of GFDL would require somebody with those kinds of skills.

Smagorinsky: Let me tell you. Jerry Mahlman is already proving that he's got it. I used to think he'd have it, but I wasn't sure how he'd show up in a pinch. You know, he might suddenly try to compromise his way through, and there are some people that just wait for compromisers and pounce on them. But he's proven to be very good and I have utter confidence in him.

Young: Of course we have no control over the appointments of people like Tony Calio and the replacements. How many more years is Fletcher in place?

Smagorinsky: Just maybe one or two or so.

Young: So you can be critical and things can shift against us.

Smagorinsky: But look: one of the things I'm very proud of is that, despite the fact that people said I was very strong on that job (strong not always being a complimentary term), if they had watched very carefully, they would have seen that the laboratory in the last fifteen years of its life, while I was in it, pretty well stood on its own feet, dependent on the reputation of its scientists and not the director. At the beginning, it was the director. But I built it and sustained it so that it could stand on its own, so that it makes its own case, and its own case speaks for itself. This wasn't true at the beginning. There was no case to show.

Young: Certainly in the academic world, that's a sufficient condition to pretty much assure a long-term future. I still worry about the government world.

Smagorinsky: Let me tell you something. I'm going to go way back now to the beginning. When GFDL was first being formed, I spoke to Bob White. He said, "Joe, that lab will never fly. It's impossible in the government, but especially in the Weather Bureau." One of the important reasons it worked was Bob himself, later on as my boss. But I think what's happened is that it has worked for many reasons that I didn't give. One of them, for example, was in the middle fifties, there was an absolutely superb Chief Commissioner of Civil Service: a guy by the name of John Macy, who wrote job standards for scientists that completely captured the environmental needs for a scientific activity in the government. Absolutely superb. You or I couldn't do a better job. And that was one of the reasons that it was possible to do a lot of the things.

Now admittedly, not all labs resulting were good. It's rarely the case that something's impossible. You have to know what you want, you have to be single-minded about getting it, and eventually it stands on its own merits. Now you can't just shout out into the wind "I'm good, therefore you should treat me well." If you don't have substance, that's not going to work either. You do have to produce. And what is the legacy of GFDL? It's not that building, it's not that computer, it's not the Princeton University connection. It's the fifteen or seventeen guys who are known as the scientific staff of the laboratory. That is the legacy of GFDL.

Young: So when you think that legacy has been established, what makes you decide to retire?

Smagorinsky: Well, I had a number of reasons. There are many, reasons, some pulling me, some pushing me. I think I wanted greater freedom over my own time, something I've desperately wanted over the years. But I also had a number of problems and personal things that I wanted to get out of the way. I pretty well had them done. There were other problems waiting to come up and they did eventually. But I made a calculation as to where the windows were that I could retire prematurely. I could have gone on to the age of seventy, I suppose, but I was only fifty-nine. I woke up one morning in October, of 1982, and I said to my wife, "Marg, today's the day I'm going to announce my retirement." And she said, "What?!" And I gave four months notice. I wanted to retire on my fifty-ninth birthday, which was January 29, 1983. I gave four months notice because I thought that would be enough time for them to at least identify a replacement.

Of course, it took more than a year and four months to do it. I don't regret having made that decision. I've enjoyed myself thoroughly, GFDL is doing well and I keep out of Mahlman's hair, but still maintain relationships with everybody. I come down to find out what's going on, occasionally they invite me to program reviews. I still maintain a lot of continuity. You can see in the lectures I gave here that I still keep in touch with what's going on, take a lot of pride in it. I enjoy my existence thoroughly, almost as if I had been doing it all my life. I fell right into it, no difficulty.

Young: Those are great words to hear. And thank you very much for participating in this interview, Joe, I do appreciate it.

Smagorinsky: Thank you very much, John. I appreciate an old, old friend who looks so young, young, young, and answers to the same name. You've been a wonderful host here. You know, I'm leaving in a couple of days, it's been a month, and you have been great, and your colleagues have been just superb in their hospitality and their warmth. It's just been a magnificent month and I just have to stack this up as one of the things that I couldn't have done at any other time, if I was still at GFDL. I did it and I enjoyed and I savored it and I'll always remember it.

Young: Great. We want you back. Thanks again.

END OF INTERVIEW