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Interview of George Platzman
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Interviewer: Norman Phillips

Phillips: I'm interviewing today George Platzman from the University of Chicago, and the interview is taking place in the Chapman room at NCAR. The date is October 22, 1990.

George, why don't we begin by asking you to tell us something about your early schooling, and your family life.

Platzman: Okay Norman. I should mention that yesterday you gave me a little outline of the kinds of things you thought that we might touch on, and I studied that since then. This morning I wrote some notes on the first few points at least that you wanted to cover.

Briefly, my family background is this: My father was born in Berlin in 1885. He immigrated from Germany just before the first World War in about 1913. He worked as what was then called an auditor, I don't know whether that term is still used, but essentially he was an accountant for Universal Pictures, Universal Film Company. They had a branch office in Chicago. Until he retired, and he died in 1966. My mother was born in Wisconsin Rapids, Wisconsin in 1892. Her parents had just a year or so before that, immigrated from Bessarabia, which at that time was part of Russia, and is really Romanian in ethnic background. But now is back within the Soviet Union, since the second World War, I believe, as the Moldavian Soviet Republic. My mother was a piano teacher all of her adult life and that continued until her death in 1959. I have one sibling, a brother Bob, I think you probably have met him, and he was born in Minneapolis in 1918. He was a scientist, physicist, physical chemist, and he died at an early age in 1973. At that time he was a professor at the University of Chicago.

Phillips: I seem to remember that, yes. Where did you grow up?

Platzman: I was born in Chicago, and have lived in Chicago virtually all of my life, with some interludes that maybe we'll touch upon later on.

You also asked about early schooling, well there's nothing remarkable about it. I went to primary school in Chicago, Bryn Mar Grammar School; high school in Chicago, Hyde Park High School (this is all on the south side of Chicago). And college, the University of Chicago. Graduate school, I spent one year at the University of Arizona, and the rest of my graduate work was at the University of Chicago.

Phillips: Your brother, I remember, was a little bit older than you?

Platzman: Two years. Two years older.

Phillips: So he went to the university, and then it was quite natural that you would go?

Platzman: Exactly.

Phillips: Was it accepted in your family that is, roughly say when you were twelve or fifteen years old, some years before you would graduate from high school, was it somewhat accepted that you would probably go to college?

Platzman: Oh yes. That was always implicit in our background, although neither of my parents went to college. It was part of the Jewish tradition that education was a very important element of our life. I can't really pin it down in any more specific way than simply to say that it was just assumed that that would happen. Now, we were then, and I guess at the tail end I think you might say, or midstream perhaps, of the depression, and you asked me whether it was natural for me to pick the University of Chicago because my brother had gone there. That is very true, but at the same time, there really were not any options because I had to live at home because the economy demanded that, although later as a graduate student, when I began to have my own income, I did move to the campus. The fact that the university was right there made it possible for me to go there while living at home, is really what determined that.

Phillips: Did your interest in science start when you were already in high school, or perhaps even earlier?

Platzman: I don't distinctly remember. But, in thinking about this this morning since you put this as one of the points on your outline, the best that I could come up with was the fact that my brother excelled in science, and he was an outstanding achiever and performer. I think my early years were years of trying to emulate him, and I have no doubt that his aptitude in science must have influenced me. As to whether there was any other determinate there, I really don't specifically recall.

Phillips: Well we'll find out in a little while, I imagine, how you ended up in meteorology. You finished high school at Hyde Park High School, and at that time you entered the University of Chicago. This was at the time before Hutchins came or after?

Platzman: No, Hutchins was there. I think Hutchins came in 1929 if I remember correctly.

Phillips: So you took the survey courses at the beginning of your first year there?

Platzman: Oh yes, I did. I remember those.

Phillips: I remember those, but I am ashamed to say that I didn't take advantage of them. Enough said about that.

Platzman: Oh that's an interesting statement.

Phillips: What did you take as subject material in your undergraduate years at the university? What courses?

Platzman: I was a major in mathematics. I took some physics, and thinking about those early years of education, you asked about teachers? I certainly do remember the one teacher who, this was a high school teacher, I think had the greatest influence my intellectual development. Her name was Bulla Schussman. She was a math teacher at Hyde Park High School. She was a remarkable person, as I think back, who got into, as I learned later, public teaching in 1912 in the Chicago School System. So, she was quite an old timer when I entered high school, which was in 1933, I think. I think what I learned from her, perhaps more than anything else was, I guess I should say (before mentioning the main point I wanted to make) I certainly learned a lot of important mathematics, especially algebra and geometry. But, more than that, I think that I learned to train my intellect into rigorous habits, which I certainly did not have at that time before. She was a disciplinarian…

Phillips: You mean axioms and theorems and that kind of reasoning?

Platzman: Well, yes, but also in simple matters of work habits of being orderly, consistent, and in dealing with matters of exposition, for example, and just so many different little things that one ordinarily doesn't think of that are a part of an intellectual activity.

Phillips: If you're true in those recollections, I wish that I had had a Bulla Schussman as a teacher. I had good teachers, I think, myself.

Platzman: Did you have good math teachers?

Phillips: I think so, but I don't remember getting such habits from them.

Platzman: She was truly outstanding, and she had the capacity to inspire.

Phillips: Were you good enough in mathematics though that perhaps she gave you extra support?

Platzman: Well, I wasn't at first, you see, my brother preceded me. And he was one of her outstanding students so she had that to go by when I came along. My performance was, I'm sure, much below average at the beginning; and it was by dint of her persistent pressure to improve that I gradually made headway. For instance, she had honors classes, which I am sure, at that time, was an entirely voluntary activity on her part. Of course it was also voluntary on the part of the students as to whether they wanted to participate, but this honors class met at 8:00 in the morning before the other classes started. This was another one of her ways to stimulate the interest of the students.

Phillips: My recollection of high school mathematics was that the girl students still outshown the boy students on the average in mathematics.

Platzman: In math? I don't have any impression upon that one way or the other. Bulla Schussman had toward the end of her teaching career, received many honors for her lifetime of devotion to teaching. I remember that after I was actually on the faculty of the university, and she had long since retired from teaching, there was a special ceremony honoring her at a local school, that I attended, and she received telegrams from all over the country actually, including a telegram from President Eisenhower congratulating her on her career.

Phillips: Well it's nice to see that she had that reward. When you went to the university as an undergraduate and studied mathematics, what kind of mathematics was it that was pursued at the university? My recollection, when I returned to the university after the war, was that the mathematics department emphasized pure mathematics, and that the applied mathematics courses were actually taught in the physics department. Was this evident to you as an undergraduate?

Platzman: Yes it was. To some extent I suffered from that, because my mental faculties have never been inclined toward the kind of abstractness that is needed to do the kind of mathematics you are referring to, and I had very many difficulties in the Chicago math department partly for that reason. In spite of the fact that I had some really outstanding teachers: I had Adrien Albert in algebra. The one teacher with whom I did have a good deal of rapport, and whose teaching I was better able to appreciate, was Lawrence Graves. Now he was an analyst, and he taught calculus and those other basics. And I think that was extremely valuable, the fact that he taught in the way that I was able to appreciate. In the case of algebra, however, I had great difficulties. And really, my performance there was not at all satisfactory.

Phillips: Well, speaking as a meteorologist, I would say, I could never detect from your papers any weakness. You graduated with a Bachelor's degree from Chicago, and this would have been in perhaps 1941?

Platzman: 1940.

Phillips: 1940. In 1940 you then continued…

Platzman: I went to the University of Arizona, and I did one year of graduate work. You asked in your notes why I did that. Nearly as I can think back, there were several reasons. One was a health factor, which I had been advised for various reasons to seek a more mild climate. And the other was that I had a friend, Armond Deutsch. I don't know whether you've ever heard that name. He was an astronomer, actually quite a well-known astronomer later on, but even in those days he had become an astronomer, and he had previously been at the University of Arizona in Tucson. He was a very good friend of my brother's and mine. We had, I don't remember the details, but I had agreed with him to accompany him when he went to Tucson for the next academic year. As it turned out my grandmother, who lived in Minneapolis, also was looking for a mild climate, and she went down to Tucson, rented a house, and I lived with her during that whole year. So, there was a combination of circumstances.

Phillips: Then you did a Masters thesis there and that Masters thesis was in mathematics?

Platzman: Yes.

Phillips: Do you remember the title of it?

Platzman: Oh, I remember it very well…not the details, but I certainly remember the subject matter. And I guess you wouldn't say, I'm not sure whether it would be considered to be mathematics per se, but there was a man there with whom I interacted very well. His name was Bolderev, Russian extraction. I don't recall exactly what his specialty was, but he was certainly interested in mathematical logic, and he got me off in that direction. The emphasis of this work was to be the application of boolean logic to probability theory. It was then, as I think I mentioned in the notes that I wrote for you on history, that I got interested in the work of George Boole, which is certainly very much involved there. I can't go into great detail here because I simply don't remember too much about this, but that was the general idea.

Phillips: So you finished there in 1941, and as far as I know you then showed up in the meteorology group at the University of Chicago.

Platzman: Well, that is certainly a good first approximation, but the details are this: When I returned to Chicago in the summer of 1941, I took up an interest that I had previously acquired, and I don't remember exactly where, in the statistics of quality control. Now, this may sound rather odd, but to explain, there was a man in Chicago who was a mathematician, and from whom in fact I had taken some courses as an undergraduate, his name is Bartkey, Walter Bartkey. Do you remember Bartkey?

Phillips: Oh I know the name. He was in the Physics Department?

Platzman: No, he was a mathematician. At the time I returned he was actually Dean of the physical sciences division.

Phillips: Ah…that's how I remember him!

Platzman: Now, Bartkey was really an applied mathematician, and his presence in the math department in that respect was somewhat of an anomaly I suppose. But, he had been doing consulting work for Western Electric. They have a big plant in Chicago.

Phillips: My wife's father spent his entire adult life working at Western Electric.

Platzman: Well, that's the same plant. His interest, and their interest in him, was in this field of quality control engineering. Basically, the problem is that you want to test the product in a certain way by sampling so that you know something about the quality of what you are producing. One of the difficulties is that a lot of the tests are necessarily destructive, so the testing design has to take that into account, and this creates certain special problems. At any rate, when I returned to Chicago in the summer of 1941, I had already been in contact with Bartkey in that territory, and he actually gave me a job teaching in what was called the Engineering Science Management and Defense Training Program (ESMTP), something like that. The U.S. was not then actually in the war, but it was certainly very much involved in wartime preparation. And this was one of the elements of that activity. So he put me on to giving lectures in one of the courses being given in that program on quality control matters. While I did that, but, at the same time, (and this is a curiosity) my brother, who was then a doctoral student at the university, had been doing part-time work to earn his bread and keep for the recently formed Institute of Meteorology, which was started in the autumn of 1940.

Phillips: Was Rossby already at the University?

Platzman: I think Rossby came early in 1941. Byers and Starr I think were the first ones on the scene, and Byers was the one who got the administrative apparatus going. My brother had been doing this part time work actually as a map plotter for the Institute of Meteorology. So when I returned in the summer of 1941 he apparently was sufficiently fed up with that kind of work, and probably was also more deeply involved in his own research. So, he said, why don't you take this job? And to make a long story short, I did that. I became an employee of the Department of Agriculture, why the Department of Agriculture I don't recall.

Phillips: The Weather Service at that time may still have been with the Department of Agriculture.

Platzman: Maybe. Anyway, I started doing that along with this other quality control. That continued until December 7th of that year, and then of course everything changed.

At that point, I started to look around as to what I could do. I applied to both the Navy and the Air Corps for an Officer Training program. I forget exactly how you made those applications and for what it was, but I did that, was rejected on account of eyesight from both. I made a couple of other attempts…let's see whether I have those in my notes here. Oh, I took a civil service exam to teach…what I'm not sure, perhaps math or physics, at Chanute Field in Rantoul, Illinois. I actually got a civil service rating to do that, in January of '42. But, I think what happened is that Rossby got his tentacles on me. I had formed an acquaintance with many of the people in the institute.

Phillips: Including Starr?

Platzman: Yes, and Rossby.

Phillips: They were already located in the old music building?

Platzman: No, they were then in Ryerson. That's where the institute started. In fact, they occupied the 3rd, 2nd, and portions of the 1st floor much to the disgust of the physicists who were displaced. That reminds me, I can't help but mention this rather amusing incident: I was sitting in an office that had previously been occupied by Robert Mullikan. Mullikan, you may have heard of him. He was a physicist, spectroscopist, who won the Nobel Prize. He had previously occupied that office, and he came in, he was a very nice person, but somewhat of an odd fellow. And he came in one day in the state of some concern about the medicine cabinet that was still on the wall there. He wanted to be sure that the medicine cabinet still contained the hairbrush that had belonged to Michaelson, who also apparently had occupied that office.

Phillips: Did you leave the hairbrush behind?

Platzman: I don't remember whether he found the hairbrush there or not. For some reason that incident stuck in my mind. So this was in the physics building, in Ryerson. Now what was I saying?

Phillips: You were becoming enmeshed in the meteorology field.

Platzman: Oh yes. I had already met Rossby and Starr. Rossby in those days was very, as I think was true throughout his career, very much of a recruiter of people who had what he thought was the proper background for meteorology, and that would mean math and physics or chemistry. I am sure it was largely through his urging that I decided to throw my lot in with the meteorologists. In February of '42, I entered the third meteorology class. The first one was '40-'41. The second one started in the autumn of '41, and the third was the first overlapping class, and that started in February. It was a nine month course which finished in October, I think. At that point I was urged by them to stay on to teach in the succeeding classes.

Phillips: Ah, so this is somewhat similar to Charney's procedure; his entrance into meteorology, that you recorded with him.

Platzman: Yes, I think so. Right, and roughly contemporaneous. I didn't at first decide to continue doing that. I made some other attempts to find employment elsewhere, but I eventually settled down to do this for 2 years, approximately.

Phillips: I have a vague memory, George, of rummaging through some drawers at the university, when the department was in the old Meteorology Building, overlooking the faculty tennis courts; rummaging through some drawers, and coming across, I believe they were isotropic charts with your name on them.

Platzman: Oh yes! I did indeed…I plotted…I don't know whether I ever analyzed any of them, but I think I did later on. I certainly plotted a lot of them. There was a Teletype room off one of the labs there from which I tore off the necessary data on these yellow sheets, remember those? And I had to learn how to read the code and how to plot the stuff. One thing that I remember about that Teletype is that on December 7th, I was in there, I think it was a Sunday. No, probably it was the next day, Monday, when I went in to the campus, and I tore off the sheets expecting to find weather data, but there were some other things out there. Some open transmissions one of them saying something like this: all aircraft are warned to stay away from some dam (I don't remember the name of it) in Arizona. Now I can't explain this, I don't know why, but…

Phillips: Well it's easy to guess what it might have been, but we don't really know.

Platzman: …some warning about if they violated that airspace they would be shot down. Which seemed to be pretty extreme, for the first day of conflict.

Phillips: Well, maybe that speaks well for some late but nonetheless rapid reaction on the part of the Armed Forces.

Platzman: Yes, it's true. I did plot the data for isotropic charts.

Phillips: Did you ever plot any charts after that, George?

Platzman: Oh yes, I'm sure I did, in the synoptic labs, I was forced to undergo…

Phillips: Another parallel to Jules Charney then. Now, what did you teach in the programs? Well you answered part of it…

Platzman: I thought about that this morning, and as nearly as I can recall, Norman, that the only thing I actually taught in the sense of giving lectures, was dynamic meteorology. But, I and several other colleagues, were more or less in the same boat. Also, we were lab assistants. These labs were not synoptic labs, I wasn't capable of doing that, but labs in math, which was badly needed by many of the air cadet students, and in hydro. Math was taught by a mathematician by the name of Reed.

Phillips: Not the Reed who ended up at Brown?

Platzman: No, not Bill Reed; our hydronamics stability man, who wrote the book with Phillip Crasen. Not that Reed, another one. And the hydro was taught by Michael Ferrance. I don't know if you remember that name?

Phillips: Yes. He had some kind of a popular…

Platzman: Yes. Lemmon and Ferrance. He was a physicist; he later went on to be Director of Research for the Ford Motor Company. We did that lab assistant coaching, I guess you would call it, preparing problem sets and so on.

Phillips: Grading... What was the first meteorology course you took?

Platzman: Oh, I remember that. It was thermodynamics. The first lecture I ever gave for the meteorology program was in the Oriental Institute, Brested Hall, do you recall that place? I particularly remember it, it was on thermodynamics I know, and I am sure that it was much too abstract for the circumstances, but I particularly remember it because Rossby attended. I think he felt the obligation to see that he was not going overboard by putting these greenhorns in charge of lecturing, but probably he was anxious to do it because at that time he was flying off in dozens of directions.

Phillips: Thermodynamics, in the '30's he had worked on thermodynamics, applied to air mass analysis. I taught the first session of dynamic meteorology for many years at MIT in the '50's and '60's and early '70's, and I found then that it was necessary to go back to the 2nd law. Go back to Kelvin's statement of the 2nd law and what was the other statement…Clausius, and to prove equivalents, because the entering students had not had that in their physics courses…in those years they were getting a lot of atomic physics; whereas thermodynamics is somewhat extraneous…

Platzman: I wonder whether that isn't still true.

Phillips: It could be! You did teaching in meteorological subjects. Do you remember any of the students who then graduated?

Platzman: I remember lots of people, some of them certainly were students. Whether they were students in courses that I taught, that part I don't remember.

Phillip: How large were the classes?

Platzman: The classes eventually got up to 500 per class. That was the typical number. During 1943, and the early part of '44, there were always two overlapping classes. Somehow I remember the figure of 1500 students on campus at one time, I don't know how it could get to 1500, but at any rate, the typical class size in those "headache" years was 500. That number of students could only be accommodated easily in Mandel Hall. I remember many lectures in Mandel Hall. Not ideal circumstances to say the least. For example, the students, and I really pity them to say the least, were kept in class from 8:00 am until noon for lectures, and then in the afternoon the synoptic labs were from 1:00 pm until 5:00 pm. Then of course they were expected to study in the evening. On top of that, they had a strict military disciplinarian, Captain Starbuck was the Commanding Officer, and he got them out, they all stayed at International House, and he got them out on the midway at 6 o'clock in the morning doing calisthenics. Then they marched in unison down the distance to the campus singing the Air Corps' "Off we go, into the wide blue yonder…". Well, these fellows were exhausted most of the time, and that showed up in the lectures when a lot of them just couldn't keep awake. It got so bad in fact that the authorities assigned one of the officers to go up and down the aisles with a long feather-stick (that is a long pole with a little feather on the end of it)…I'm not kidding!

Phillips: I understand that's what was done in the Puritan churches.

Platzman: Well, it was revived! On top of that some of students were somewhat distainful not only of the subject matter, but of the lecturers as well. And they didn't mind expressing that distain by, for example, sitting in the front row and reading newspapers. You can imagine that with all these things going on, it wasn't very easy to give a coherent lecture.

Phillips: Then you taught at the university in the Meteorology Training Group for some years, and then you went to Portland, Oregon in '45-'46.

Platzman: Well in '44, things began to wind down in the meteorology training. I think that the primary reason for it at first was the fact that there was an overproduction by that time. So, the orders came through that eventually filtered into the meteorological training program that they should begin to cool it off. So a lot of us began to realize that we were going to have to look for other employment. I tried various things in the course of 1944. I found records of my having applied to the United Nations Relief and Rehabilitation Administration. Now that's surprising.

Phillips: In '44? It couldn't be in '44.

Platzman: What it was…the United Nations was formed during the war actually. I remember the first session, I think, it was in San Francisco.

Phillips: Yes, but I think that was after V-J Day, wasn't it?

Platzman: Well, I am a little puzzled by this, but that's what my records show. And I also applied to NACA; National Aeronautics and Communications Administration, the precursor to NASA. I don't distinctly remember how I got involved with the U.S. Corps of Engineers, which is the basis for my going to Portland. But I did find a letter. Now this is an aside to you, Norman, and to the tape. I spent the last day before coming here to Boulder going through my files frantically trying to find anything that might be of interest.

Phillips: You mean your neat apartment back in Chicago is a mess now?

Platzman: No, these are all files on the campus. One of the things I came across is a letter I wrote in 1944, or was it '45? It may have been early '45, to a man by the name of Meryl Barnard, who then I think was in charge of the Hydromet section of the weather bureau. Now I think it's possible that Harry Wexler, whom I undoubtedly asked for advice, said why don't you get in touch with Meryl Barnard.

Phillips: How did you know about Harry Wexler?

Platzman: Harry Wexler was on the faculty at Chicago University. He's one of the people I formed a very early friendship with. There was Harry Wexler, there was Elman Notsburg; Victor, of course.

Phillips: Was Herb Riehl there then?

Platzman: Herb Riehl wasn't there at first, I think he came about midstream. I don't remember exactly when, '43, perhaps.

Phillips: Dave Fultz, perhaps?

Platzman: Dave was, I think he was with me as a student, in the same Air Cadet class. He later went out into the field, precisely, I think he was in Alaska for a while.

I ended up visiting Washington in January or February of 1945. I visited the Hydromet section. I have a suspicion that the people there suggested that I get in touch with the Corps of Engineers. Because, you see, Hydromet was doing what they called "hydrometeorological storm studies" for the Corps of Engineers. The Corps of Engineers had the responsibility to design and construct dams for flood control, and power generation. The arrangement had grown up, where in the spillway design they needed of course an estimate of maximum possible precipitation, and they turned to the Weather Bureau for that, and the Weather Bureau had this hydrometeorological section which was the place where that information came forth. The hydrometeorological section of the Weather Bureau produced these studies for the Corps of Engineers.

Phillips: These would be studies saying that every 10 years you can expect a storm with this magnitude, and every 50 years you can expect one of this magnitude?

Platzman: No. Well, that's involved. That kind of work was done by the Corps itself. That's purely hydrological work. What the Weather Bureau did was more meteorological. They tried to figure out what factors in the particular region involved would have a bearing on heavy precipitation; analysis of convective storms, and orographic precipitation, things of that nature. I think that the Hydromet didn't have an opening in Washington. But I must have been put onto the fact that there was one branch of the Corps, one office of the Corps, and only one, where this kind of study was in fact done by the Corps itself. And that was important, for the strange reason that there was a man there whose name was Allison, John Allison who had taken a keen interest in this kind of work, I forget exactly what his background was. For some reason he had managed to bypass the normal way of doing business, and had been doing some of these studies himself for the Portland District, producing these estimates of maximum possible precipitation. Well, he was in fact looking for help. And to make a long story short, I was that person, and I spent 2 years there doing that kind of work.

Phillips: That's where your third published paper on **Computation of Maximum Rainfall in the Willamette Basin**. I've learned how to pronounce Willamette.

Platzman: Do you have a relative in that area?

Phillips: Yes, but she has never pronounced the word to me, so I learned it from someone else.

Platzman: I should also mention that one of the other reasons that I left Chicago in early '45 is the fact that Rossby claimed, rather convincingly I guess, that he had the conviction that it was a lot better to go out into the field to get some taste of the real world before undertaking doctoral research, than it was merely just to continue from Bachelor's to Master's to Doctor's as one continuous stream. So he pushed that idea, that I would gain a lot from seeing something of the real world. And I must say that I don't know how I felt about it at the time, but in retrospect, I think he was right, actually. I learned a lot, and I probably learned a lot I didn't have to know later on, but I learned a lot about how things really go. And this, I'm sure, was a very good thing for me. So that's part of the reason I ended up in Portland.

Phillips: You didn't have any relatives you were with?

Platzman: No, but I was then very much involved with Harriette, and in fact we traveled together in February of '45 to Los Angeles. Los Angeles I think simply because that's where the best train service was, the Santa Fe Chief, I think it was. We were married in Los Angeles.

Phillips: How had you met her?

Platzman: She had been working part-time, at first part-time, later on full-time, in the Meteorology Department. The reason that happened was that Francis Day, later Francis Ashley, remember her, who became such a mainstay…

Phillips: …was the secretary for Rossby in Chicago for a while, and then the AMS…

Platzman: That's right, she was the mainstay of the Boston AMS office for many years. But she was staying in the apartment where Harriette lived with her mother. I think it was through Francis that Harriette actually got onto the Meteorology Department. See, why did I say that…you asked me something?

Phillips: Whether you had relatives you were staying with in Portland.

Platzman: No, the answer is no, I did not.

Phillips: Then you then ended up coming back to the university. Was this planned ahead of time, or was it that the opportunity evaporated after a year, or was it you came back voluntarily to the university?

Platzman: Well, I considered the experience in Portland with the Corps of Engineers to be a temporary one. I never intended to remain there. I always had my sights on continuing my Doctoral research. Actually, it had been interrupted by the war as I explained earlier. Now that my field had been irrevocably changed, I decided to go on to a Doctorate Degree in meteorology. I didn't at first decide that I'd go back to Chicago. I found in my rummaging through my papers the other day that I had been in touch with some other places. I found some correspondence with Maureen Iver. I asked him about the opportunities at UCLA. I had known him also in Chicago. I also found a letter I had received from Bernhard Haurwitz who was then at MIT. I met Bernhard during the war when I visited MIT, I don't remember why I was there, and I was quite astonished when he introduced himself. Of course I knew about Haurwitz through his textbook, but I had not met him before having made this trip to MIT, I don't remember, it was either '43 or '44. He introduced himself to me, of all things, as a distant relative of mine. I think I explained this to you probably long ago. And indeed it is true we are related by marriage through my father's family.

Phillips: At this time Haurwitz knew about this, but not you.

Platzman: That's right, he did. What it boiled down to is that we had in common a first cousin who lived in New York as a result of this rather complicated family relationship. So that was the beginning of my friendship with Bernhard. At any rate, I found as I was rummaging through the file a letter that he wrote to me in 1946 in response to my having obviously written to him for advice on what to do at that point.

Phillips: About what school to attend?

Platzman: Yes, well, what school to go to or even whether I should, do what I really intended to do: namely go on for a doctorate in research. He wrote a very (I had completely forgotten about this letter, and in fact I brought it with me on this trip to show to Marian) it's such a friendly, and really helpful letter. But anyway, I ended up going back to Chicago, and was offered part-time research position there as a means of supporting myself while I did the research for my doctoral work .

Phillips: So you returned to Chicago in September of '46?

Platzman: In December of '46, and you asked me whether I overlapped Jules.

Phillips: Jules was there I believe from the summer of '46, I don't know what month, but he and Eleanor left for Norway in I think March of '47.

Platzman: I think that's right. And that's very strange. I arrived in December back in Chicago from Portland, I arrived in December of '46. For some reason I do not have a distinct recollection of my interaction with Jules during those few months involved there. I must have developed an acquaintance with him during that time. How could I not have done so? And In fact, the evidence that I find in my files later on is that I already had a good working relationship with him when he submitted a paper to the Journal of Meteorology when I was an editor in 1949. I worked with him on the editing of that. I strangely can't remember any details of my interacting with Jules during the time that you mentioned, but certainly that must have happened.

Phillips: I came close as a student at Chicago to having some interaction with Charney then, but it was not because I had just come back to the University in September of '46. I took statistics courses, I had not yet been given credit yet for a bachelor's degree, and I had to take physics 201 I think it was, physics 202, which was differential equations, mathematical physics, and I also took statistics. So I spent very little time if any in the Meteorology Department.

Platzman: Well Norman, tell me at what point was your undergraduate work interrupted by the military service?

Phillips: I entered the University in September of '40 planning to major in chemistry, but I frittered away my time a lot, and ended up with what they call later on an associate degree for 2 years.

Platzman: I remember that. That was one of Hutchins' ideas.

Phillips: I do not remember that. I thought of it as a graceful way of terminating what I am sure from the university's point of view had been an unsatisfactory 2 years on my part.

Platzman: No. I think that was part of an experiment that originated with Hutchins having to do with his philosophy of undergraduate education.

Phillips: I then came back, and of course I got credit for the training in the pre-meteorology program, which was at a lower level than where you entered.

Platzman: Is that what was called the C program?

Phillips: That is right, the C program; first at the University of Michigan, and then our group was shipped to Chanute Field.

Platzman: Didn't Harry Wexler teach for a time at the University of Michigan? Did you ever see him there?

Phillips: No. There was another group at the city on the eastern coast of Lake Michigan. There was another course program there; he may have been there. But I still needed to do some formal work at the University to get a Bachelor's Degree after the war, and that was mostly in mathematics, and of course I'd had a lot of meteorology so I did not have to take meteorology for the bachelor's degree. So I spent almost no time in the meteorology department other than to have gotten registered as a student.

Platzman: That you say was in '46?

Phillips: Yes, beginning in the fall of '46.

Platzman: We must have met shortly after that. Shortly after I returned from Portland.

Phillips: Well, except that I spent no time that first year in the department. I never knew Charney.

Platzman: When did you start your affiliation with the department?

Phillips: Oh, I suspect that I got a dynamics course from Fultz or Starr, but it was taught in Ryerson, well it was taught in the Rosenhall. I had no need to go to the department except to use the library maybe, which was on the first floor. It wasn't until the my 2nd year, the beginning of fall of '47 that I spent all of my time there. Now, to return to your life George. In 1948 a lot of things happened.

Platzman: Here are my own notes that I made earlier this morning just off the top of my head.

Phillips: In 1948 you got your doctorate, and that was for the paper on the motion of long waves in the upper atmosphere?

Platzman: Long waves on a jet stream, a particularly idealistic form of the jet stream.

Phillips: You became an assistant professor, and you also became the editor of the **Journal of Meteorology**. Each of those three subjects is worth a few minutes of discussing I think.

Platzman: Well, lets take the editorship. One of the things I came across in rummaging through my files back home the other day is correspondence that I had in 1945, and possibly '46, when I was in Portland, with Harry Wexler, having to do with the meteorological monographs, which was just at that time created by the first number that was issued in that series, a monograph by Woodrow Jacobs called Wartime Developments in Climatology. To try to make a long story short apparently what happened was that the AMS, somebody, had asked Woodrow Jacobs to write something about the war and about the way climatology figured in the war. And he did that, and produced a manuscript that was supposed to be published by somebody, presumably by the AMS. There were some problems with the manuscript; technical problems, maybe some scientific problems, I'm not sure. And it was also the fact the AMS didn't at that time have the means to deal with a substantial piece of work like that. It was too big for the Journal of Meteorology. First of all, they were grappling for a way to publish it, but also they were looking for somebody to do some pretty heavy editorial work on it.

That's where Harry Wexler asked me to get involved, and eventually I did. I spent God knows how many hours working on that thing while I was in Portland. And it was eventually published in the first number of the new series of meteorological monographs. But I mention that because that was really my first introduction to editorial work for the Society, although before I went to Portland, I had assisted Starr who was the first editor of the Journal of Meteorology, and he did that work of course while he was in Chicago. I helped him with a lot of the work involved there. I remember for example making up dummies of the issues of the Gallons. That of course is work that no longer has any meaning, but then it was a pretty important thing. So I had that experience, plus I the experience with the Jacobs monograph.

Now, in 1948, I'm not sure exactly what years are involved here, when Rossby was the President of the Society. But, the second editor of the Journal was Ray Montgomery. I think he started in '46. I may be off there. I think he had it in '46 and '47. And although there had been at one time a very close working relationship with Montgomery and Rossby, who after all co-authored the rather significant paper in the Woods Hole series. I think that that had apparently, to some extent, cooled off, and Ray didn't exactly fit in with Rossby's rather expansionist view of how meteorology should be published in those days. Ray was very methodical, very conservative, and very objective about everything he did, and not inclined to be tooled so to speak, of Rossby's machinations. So I think that at some point or other Rossby was determined to change that situation. How he went about doing that, politically, I don't know. Picking me, I guess partly on the basis of my earlier work with Victor, and on the basis of what I had done with the Jacobs monograph; picking me, I am sure, was his way of creating a situation where he could feel that he really had control of what was going on. Actually as it turned out, I can honestly say that he never interfered with anything that I did. I think that he must have known enough about my attitudes and inclinations to be satisfied that I would not drift too far from what he wanted. So there's that background.

I remember paying a visit to Ray Montgomery when he was in Woods Hole, he later went to [Johns] Hopkins, before I took over the Journal, but after I had been designated to do so. We talked about the transition. I think I probably somewhat indelicately mentioned to him my perhaps apprehensions, but more perhaps just a matter of fact, the statement of the political ramifications of this change of editorship. I think I probably offended him by alluding to that at all…[I was] never really quite sure. So that's how I got into the Journal of Meteorology, which I kept for 2 years. Now, what else did you ask me about?

Phillips: Your assistant professorship, from your point of view, was it something that was logical for you to expect?

Platzman: Oh yes. It was a logical progression from what I was doing. I was a research associate. I had just finished my degree so it just fell into place that way.

Phillips: And the thesis then, on the wave motion in the upper atmosphere, was the topic of your own choosing?

Platzman: I've been thinking about that because I thought you would ask me that, and I honestly don't remember. I don't remember in any specific detail, but I can say a few things about that. Rossby published in 1947 a bulletin having to do primarily with the problem of the solar equatorial acceleration, and he had a theory about that. How he got interested in that I am not too sure, although he had established I think some contact with Koiper, an astronomer at Chicago, but there may be other reasons. No, I think I know. He became fascinated with the monograph (this was a little earlier) written by a man by the name of Maceo Tinski, a thick monograph published in what series…Astrophysika Noveika…I think. On the problem of solar hydrodynamics; he went into all ramifications of it. One of the things he described was this phenomenon, solar acceleration. In other words, equatorial jet stream, you might say.

Rossby developed a theory about that, that as you correctly said a moment ago had to do with horizontal mixing of vorticity. The idea was that there was a strong coupling of the solar atmosphere in the polar regions, which generated the 2-omega vorticity, and that somehow through horizontal eddy motion that got mixed down toward the equator leaving in its wake so to speak a uniform vorticity of that very high value. Of course he had to figure out a scheme of interrupting that at some point because otherwise it would have become inertially unstable I guess. That's also included in this…

Phillips: I remember you made references in your thesis to that paper by the Chicago group, it wasn't only under Rossby's…

Platzman: No. You are thinking of another paper. We are talking about two different papers. I think both were published in 1947, and both were in the Bulletin. The one that you just referred to is the famous so-called staff member's paper, which incidentally, let it be known, that is a euphemism for Rossby. Although, it is true that a lot of people kibitzed on that, and I don't say that everything in there is Rossby, but certainly he wrote it. And certainly the philosophy that's reflected throughout is Rossby.

Phillips: I wonder how much can be said about the 1939 paper by C-G Rossby and staff members…collaborators…

Platzman: I do not know; that of course, I don't have any direct insight on. Rossby did have this, I wouldn't necessarily call it a habit, but an inclination, to get people involved in what he was doing, and I am sure you know that by your own experience.

Phillips: …to quickly learn from what they were doing…

Platzman: Yes, but I mean he was so gregarious, and so wanting other people to share his ideas, that I think this expressed itself (among other ways) and the way in which he published things. This staff members' gambit, and the collaborators' gambit, is an aspect of that attitude and personality.

Phillips: Even in the Rossby volume, for which you were editor by the way, Rossby's paper on the **Problems of Modern Meteorology** or some title like that, has two co-authors, Bergeron and ____; but I am sure that writing was all done by Rossby.

Platzman: Undoubtedly. Now, the paper of 1947 (I don't want to lose track of this point) is the paper on the solar equatorial acceleration. It's only on that topic, and it's only Rossby. It's in that paper that he developed this idea of mixing, and then in the equatorial zone a uniform momentum. That must have influenced me. I think it's also possible that Rossby may have suggested that I look at the stability problem of that kind of a velocity profile. He was there in Chicago at the time, although it was also a time when he was beginning to get disengaged in from Chicago, and re-establishing his Stockholm persona. So that's about the best that I can to answer your question.

Phillips: Well that's true of any such idea; it's difficult to trace back to the ultimate roots of it, even with the best intentions, and the best of filing systems, you can't do that.

Platzman: Unfortunately that's true, and that often leads to a lot of controversy.

Phillips: I imagine, yes. But you evidently gave up the editorship several years later.

Platzman: I had it for two years, and then Werner Baum took it over.

Phillips: Yes, and was that an understanding when you first picked it up?

Platzman: No, but I felt, I'm sure, that I couldn't go on that way or I would never get any scientific work done.

Phillips: I noticed in the papers you sent me, or the list of publications that you sent, (and some of them accompanied by reprint); that you had a 1947 paper on the **Partition of Energy in Surface Winds**. You were actually the only author on that, although in some places in the paper it's clear that you had worked with Starr on this problem or something of that nature. Is that correct?

Platzman: Yes. I think that's roughly true. It was Victor who started me off in that direction. I should say that during the wartime years I had established a very close personal, as well as professional, association with Victor; and I was interested in everything he was doing with the possible exception of the book that he wrote on **The Principles of Weather Prediction**.

Phillips: At that time that was our bread and butter.

Platzman: I imagine so, and it does have some interesting parts to it. For instance, he shows this diagram of the vertical distribution of convergence and divergence in relation to the trough and so on…that I think summarizes…it's partly throwback to Bjerknes and Holmboe. But there also are some new elements in there I think. Anyway, let's not get into that. We were talking about Starr. Well, while I was away in Portland, Starr (and I don't know how this came about from his standpoint) got onto Stokes waves. And he proved some quite remarkable theorems about fine amplitude surface waves (Stokes waves) that nobody have previously had any inkling about, having to do with the momentum transport, and its relation to the wave energy. These just came out of blue, and I think were remarkable, I've always felt, were a remarkable indication (or example) of the high degree of originality that Victor brought to his scientific work. Really remarkable. So he had done that; and when I returned, he was already deeply involved with this kind of work, and naturally it began to rub off on me. How I specifically became interested in this question of energy partition I don't and longer recall, although it is certainly related to (in a very loose way) to his interest in the energy of the Stokes wave. So I wrote that paper more or less on my own. Later on, shortly after that, we collaborated on a paper having to do with energy with group velocity as applied to fine amplitude Stokes waves, and energy transmission. I had a lot of fun with that work on Stokes waves and I learned a great deal.

You asked in your notes to me, whether it could be viewed as a forerunner in my later interest in tides, and I really don't know. It certainly was a forerunner to my interest in oceanography. At that time, I don't think that I knew anything about tides. We did however, have some lectures on tides from Phil Church as part of a nine-month training course for the meteorology program. I have to say though, I didn't understand, not even the foggiest notion, of what Phil Church was talking about at that time.

Phillips: You've spoken now on your early years in the department there. Let's continue then, from the late '40's into the '50's. You had somehow, although you can't remember how, established personal contact with Jules Charney, and you were very affective as one of the members of the team actually carrying out the computations on the ENIAC in the spring of 1950. In my going over Charney's correspondence at MIT, I came across what seemed to me to be a rather anxious period on your part where you were being asked by Jules, and maybe von Neumann, to give talks. This one in particular was in Illinois some place. Tell me what was about to happen in Princeton. You didn't have any actual results. So you were already involved, and they relied upon you enough to be their agent.

Platzman: This was after ENIAC though. After ENIAC, but before the Princeton machine came on line, is that what you are saying?

Phillips: Ah, so my memory is bad now, maybe it was…

Platzman: It must have been after ENIAC, otherwise I wouldn't have had anything to talk about.

Phillips: I think that the computations had been done, but the results had not been digested.

Platzman: That is possible. Yes I remember having been invited by a group down there.

Phillips: University of Illinois was it?

Platzman: Yes. The fluid dynamics group, to give a lecture in some series. I did in fact talk about the ENIAC calculations. That is about the only thing that I remember, however.

Phillips: You had gotten involved with Jules, in being one of the operators in Aberdeen, for the ENIAC. Had you been at Princeton?

Platzman: Now I have to make a slight technical correction. There were operators and programers. The operators were the people who were permitted to put their hands on the machine physically.

Phillips: Yes. That was poor language on my part.

Platzman: I was not one of those. I could not have been, had I wanted to, which I didn't.

Phillips: But, you had 24hour supervision by the meteorologist because there were frequent problems that came up. Had you been at Princeton before then?

Platzman: No. This was my first contact with Princeton.

Phillips: So, it's somewhat of a mystery as to how you got involved with Jules?

Platzman: Why was I asked to join them on the ENIAC?

Phillips: Yes.

Platzman: Oh, now that's one thing I could have looked in my Charney file about that, but I'm afraid I neglected to do that, and I don't think I can tell you.

Phillips: Maybe I should go back to my file of Jules' correspondence and look. So let's continue then. Many computations were done, and I remember at this time that I was well acquainted with you myself at the university because you asked me to assist you in the editorship.

Platzman: Right.

Phillips: And you were serving now as my thesis supervisor.

Platzman: So, excuse me, you assisted me in much the same way that I assisted Victor in the early days.

Phillips: Oh, is that right? You learn in an apprenticeship system…

Platzman: What exactly did you do, if you'll forgive my asking? I'm sure it was important…

Phillips: I did not ever get to the stage where I made up the dummy. I may have gotten close to that stage because I think it was a year and half…

Platzman: You probably read manuscripts.

Phillips: I read manuscripts.

Platzman: Greater service can no man do.

Phillips: The routine editorial editing.

Platzman: I am sure you did technical editing and scientific editing as well.

Phillips: Perhaps, as I got more familiar with it and maybe acted more boldly.

Platzman: Let's talk later on if we can, if there's time at some point, about refereeing in general.

Phillips: Oh yes, okay. Let's come back to that.

You continued this interest in NWP for quite a few years, in fact during most of the'50's, and into the early '60's. Not only on NWP proper, and the actual making of the numerical forecasts, but the theory for some aspects of them, and applications to Lake Michigan and with your students to hurricanes and so forth. But it seems to me that the feature that I associate most with your advance in this field, at that time, is your explication of the use of the spectral form of the equations. Was this in retrospect your main interest in the field at that time?

Platzman: Main, I wouldn't say, no. It came eventually to be what I was mainly working on, that is, in the early '60's. But, backing up a little bit. There are a number of aspects of that decade that I'd like to comment on. The decade of the '50's.

I remember a visit that I had from Charney and von Neumann. They were passing through town (Chicago that is) and they asked me to go down and meet them at some train station downtown, which I did. I don't remember exactly what year that was, but anyway the year doesn't make too much difference. The point of it was that they had been trying to coax me to come to Princeton, and this meeting with them was in a way the last hurrah on that question. This was a very tough decision for me, because obviously Princeton was the place where the action was going to be.

Phillips: This must have been early enough that von Neumann was completely healthy, and was not yet Atomic Energy commissioner.

Platzman: That is correct. And I really agonized over that. But I finally made the assessment, and in a way I've not regretted it. Which doesn't mean that I didn't have trouble making it, or it certainly doesn't mean that I found the Princeton scene either then or later, in anyway undesirable. But I made the judgment that my kind of attitude toward science, and my way of approaching science, was more attuned to academic life than it was to the kind of intense hothouse, shall we say, dedication to frontline research that was going on at Princeton. This was a decision from which there was no retreat, and it was perhaps wrong from the standpoint of my scientific career for me to have done that. But, I don't think that it was wrong from the standpoint of my personality and aptitude and inclinations in the scientific field.

Phillips: If you'd permit me to interject a personal comment…it would seem to me…

Platzman: Because you were in the same boat, in a way…

Phillips: No. There's no comparison, because I had no strong feeling for what an academic career entailed or was, or that I could do it. Of the type especially, that you were becoming familiar with. And furthermore, as it turned out, there were quite a few of us who could do the development type of work in NWP, but there were very few who could do the type of scholarly work that you did. So I think that your decision was absolutely correct.

Platzman: Norman, as always, you're the gentleman, you are very kind in your assessment.

Phillips: And sometimes I'm correct.

Platzman: I cannot argue. However, but not necessarily because I agree with you.

Phillips: Let me return now to some of your papers if you don't mind…

Platzman: But there are some other aspects of this decade that I was talking about, the '50's.

Phillips: Oh yes.

Platzman: Just to cap those comments about Princeton versus Chicago. I have always enjoyed teaching. Down until almost the last year of my academic life I have enjoyed teaching. I can't explain why, but I can also say regarding teaching, and I have no doubt that you can confirm this vigorously, that teaching is one of the most effective learning devices.

Phillip: Absolutely.

Platzman: I refer to teaching not only in the literal sense where you get up in front of a class and have to have well formulated ideas to express, but also teaching in the more general sense where you have students you are counseling and guiding. Two rather different things, but each of them has its own characteristics in being a learning device, as a teacher, that is. I venture to say that most of what I ever came to know in science has been what was brought to me in those terms. So, that had a bearing on it.

You also mentioned my involvement with Numerical Weather Prediction. Well, as it turned out, I feel looking back at it, and this is an assessment that I made long ago actually. Long ago meaning not yesterday, or the day before, but years ago. I feel that I never really became centrally involved in Numerical Weather

Prediction activities. My contributions were somewhat peripheral, having to do with, in the early days, problems of stability. Later this question of spectral integration, and some other matters like discretization of the equation, and things of that kind. These are, I feel at least, peripheral matters, not unimportant, some of them.

Phillips: I don't think you can say that, because the Aurelius work, and what I imagine is a very serious contribution to oceanic tidal saving, involved the development by you of a very carefully designed finite element model for the oceans. One which was flexible, and in some sense consistent and well designed. I can't help but believe that your early interest in the '50's flowered in that effort.

Platzman: To some extent that's true. But I have a certain philosophy about doing that kind of work, and perhaps we can come back to talk about that. The one thing, however, that I did do in the 1950's that I think was more substantive, is the study of the surge on Lake Michigan. Perhaps we can spend a little time talking about that in a moment, but just to go on and try to complete your original question in which you referred to spectral methods; yes, that was kind of the closing chapter of my involvement during that decade of numerical weather prediction. In your notes here, in your outline…

Phillips: Oh, I asked several questions…where did this interest come from?

Platzman: Oh yes, that's the point that I was groping for, I just lost track by my own thoughts. That's a very interesting point, and I can answer that unequivocally, it turns out, fortunately. It came in a very obvious way, which you yourself are all too familiar with. Namely, von Neumann's formulation of the integration for the first ENIAC expedition, which was the purist kind of spectral integration in which the Laplacian is inverted by expansion and ___________. In that case the domain was a rectangle, so you only had sines and cosines.

Phillips: The decoding was done with physics...

Platzman: And that's another interesting point, yes. That fact was in a sense a precursor of the later transformer method. Although I have another comment I'd like to inject at some point about that. But, to answer your question about the origin of the spectral method, that's how it came about. Later on, I asked myself the same question…when my interest first began to heighten in promoting spectral methods. I began to look into the question of whether anyone else had thought of it on those lines. The only thing that I knew about from personal experience was von Neumann's contribution to that, with ENIAC. I looked into it, and somewhere I have some notes about this, I don't recall the details, but one interesting thing that I do recall that emerged from it was that (and this may surprise you) Osborne Reynolds made some very serious, and I think profound, attempts to account for turbulence. And he did it by spectral methods, what we would today call spectral methods.

Phillips: How did you find that out?

Platzman: I don't remember how I found that out. I think it may have been because I came across something else that had been done more contemporaneously with my own work, maybe 10 years prior, where Reynolds was referred to.

Phillips: There was a paper by Boxler, in I think Journal of Fluid Mechanics, or the Journal of Computation of Physics, in which an integration was made, not with the transform method, with the spectral method, with some allowance for aliasing in order to establish the spectrum for two-dimensional turbulence. That's not the paper that you are trying to recollect?

Platzman: No. Who did that?

Phillips: Boxler; a student of Boxler's actually, did the work.

Platzman: That doesn't ring a bell. I really don't recall.

Anyway, coming back to spectral methods. So I had von Neumann's beautiful example to draw upon. At the time that I became familiar with that, during the course of that ENIAC work, I didn't think of it as a spectral method, and it wasn't until years later that the question formulated itself of methodology in solving the prediction equations. And it was then that von Neumann's sines and cosines revealed themselves as the stimulants for this kind of analysis. But, the second ENIAC expedition, which was conducted on very much the same lines served to reinforce that subconscious storage of information. We actually, by we I mean by that time Ferd Baer was a student of mine, and he got interested in this kind of work, and he and I worked together on it. Now, here it's going to be difficult for me to remember precise details, but I remember enough to be able to say that we started a serious attack on long-term integration of the equations (barotropic equations) in approximately 1956. At first our method was not strictly a spectral method, for the simple reason that we didn't have the computing power. In those days I thought only in terms of interaction equations. We simply didn't have the means of doing that. So, the alternative is to smooth in the calculation of products, to smooth the factors before multiplying. This was basically a gridpoint method in which you apply a filter to eliminate the aliasing problem.

The machine that we had available to us at that time, this was in 1956, was an IBM machine at the General Motors Research Center in Detroit. Don't ask me how I go onto that machine, I have no clear recollection of that, but I do remember that Ferd Baer made many trips to Detroit in which these calculations were pushed forward. Much later, unfortunately there was a big lag in publication of the results of that research. Part of what was done there he published as part of his thesis in the Journal of Meteorology in early 1960's, I don't remember the exact year.

This is something that you and I should now, I think, or at some suitable point perhaps this is the point to talk about jointly, because you were also involved in a somewhat similar attack on the aliasing problem, at about the same time. I remember that I went to Washington to report the results of some of our long-term integrations. The reason the long-term integrations come in, I'm sure you recognize, is the fact that unless some kind of filter is applied, truncation errors will overwhelm you. We realized that early on, and in fact, a sense of that fact came out of the ENIAC II calculations. So that approach through filtering is what formed the basis of our calculations on the General Motors machine.

And I came to Washington for an NWP conference in 1958, which you attended yourself to report on that work, and to explain the philosophy of it. Someone told me (before I presented my paper) but I'd discussed it with somebody, that they had heard somewhat the same things from you, and had I talked to you about it? In fact you were there, and you presented your version. If I'm not mistaken, correct me if I'm wrong, you gave your famous example of nonlinear instability.

Well I was quite astonished; first of all that we had been barking up the same tree, that in itself was not as surprising because we both came out of the experience at the ENIAC. But what I was especially startled by was the fact that the truncation errors for which we had designed much the same remedy, namely filtering, could lead to an instability. That's an idea that had never occurred to me.

Phillips: I came across that because I think in this way that at the same time you were looking at spectral approach to forecasting, either Salzman and Lorenz, or both of them, at MIT…

Platzman: Lorenz…I think Salzman was later.

Phillips: Lorenz must have given a talk in the Woods Hole seminars about it. Probably a low order model. I thought, hmmm, maybe I can apply sines and cosines to look at what happens to truncation errors. I suspected I would have to choose a set which was going to fold over on itself (or alias) so I began looking at those, and the simplest ones turned out to, by luck, having an explicit solution.

Platzman: I am sure there was more than luck involved, Norman. I'm glad you mentioned Ed Lorenz's low order model. I first learned about that actually from Jules before he had published anything on it. Jules was visiting Chicago on some occasion, and he knew that I was working on this question of aliasing error. He asked me whether I had heard what Ed was doing, and I said no. So he told me roughly. Eventually when I learned about it from what Ed wrote, I was charmed by one expression that Ed used there that I have always carried with me as a kind of a reminder of the importance of simplicity, and that is his phrase "maximum simplification". Remember that?

Phillips: Oh yes. It's in the title of the article.

Platzman: I think that's a wonderful idea to express in just those terms. Your paper exemplified that to a beautiful degree. But, coming back to aliasing. So we were there together in 1958 and presented our respective ideas on this. It was actually not until Ferd Baer and I got access to the Argonne computer, which was the next generation of IBM machine (this was I think around 1960) that we had a chance to test the interaction coefficient approach.

Phillips: The meaningful barotropic formula…

Platzman: Yes, that's right. We did that, and an example of that's included in our joint paper on that subject, in 1961, I think it was.

Phillips: To back off a bit from the spectral aspect: In your papers, I think one of the earliest ones is on the lattice, well you had the computational boundary conditions; but the lattice structure of the finite-difference equation...I remember that made a big impression on me. It's published in'58, which meant that you had finished the work a year earlier probably, and at that time may have seen a project report a year earlier, but I remember at that time it struck me; it clarified things for me to a considerable extent, in the design of finite-difference systems, and the application I think had a lot to do with my design of the system which is now being used in the short-range forecast model.

Platzman: Nested grid?

Phillips: The nested grid model. It had a lot to do with it; the elimination of computational modes in systems where grids are overlapping, seemed to me to be a desirable feature.

Platzman: Well that's interesting. There are two points there that you've raised that I'd like to comment on. We were talking previously about aliasing, and there is an idea that flitted through my mind, maybe it'll come back. But I did want to mention, since you brought it up earlier, that the paper about the surge on Lake Michigan, which I've said before, I consider it to be the only really serious contribution I made to Numerical Weather Prediction. I say that because it contains a specific forecast, and this is what I mean by serious.

Phillips: I see, okay. This sounds like the old story of the theoretical physicist who believes that all observations are correct, and the observational physicist thinks all theories are correct.

Platzman: And Einstein is reputed to have said, I propose some observation, and that it must be wrong because it doesn't agree with the theory. And it turned out that he was quite right!

Anyway, that calculation is where I first got familiar with the problem that you alluded to just a moment ago. Namely, the decoupling of grids, because I used that very heavily in the formulation of that calculation. Also, I think that may be the first published example of the two-dimensional primitive equation (calculation), albeit a calculation of linearized equations. And of course the linearized equations evade what is perhaps the fundamental difficulty of it.

Phillips: Yes, but this was short-term forecast research.

Platzman: True.

Phillips: You were beginning to recollect something about the importance you attached to the Lake Michigan computation to what you had learned from finite-differences, at that time. I think that's what you were trying to say.

Platzman: Yes, roughly so. What I guess I'm trying to say is that I first understood the importance of understanding the problem of the lattice structure of the finite-difference equations during the course of this calculation for Lake Michigan. I was about to say, I guess I did say, that I think this may be the first example of a two-dimensional integration of the ________ equations even though as I say it evades the fundamentally difficult problem of the nonlinear aspects of that. Now, I should add to that, that at about the same time, or at the same time, unknown to me, Arnt Eliassen (while he was visiting UCLA) was thinking along somewhat the same lines. Actually in a three-dimensional way. I don't know whether he did any calculations at that time, perhaps so.

But, another thing that I wanted to mention in connection with these early numerical integrations (this is strictly from a standpoint of the history of the subject) is I think that we have tended to forget an important precursor to numerical integration of the kind of problems that we're interested in. And that goes way back to hydraulics, and the French School of Hydraulics, back in the late 18th and early 19th centuries is when it developed, and it continued throughout the 19th century. You have people like Boussenesq, for instance, who was central to that activity. Well, these people were very much concerned with problems of numerical integration in a one-dimensional channel. And, a lot of the things, if you read that literature, there are a lot of bells that would ring.

Phillips: I see.

Platzman: This was brought home to me in a very strange way many years ago.

Phillips: Now while you are talking, I'm trying to remember this kind of thing; the hydraulic jump, was that a French concept, a mathematical...?

Platzman: I don't know who put that on record...I think it came out first in acoustics, not in hydraulics. Incidentally, in this connection there is a very interesting book by

Simon Ince on the history of hydraulics where I learned about these things. But a very curious incident happened many years ago when I attended a lecture by Surmack, is that his name, at Colorado State University. Would it be correct call him a hydraulics man, or hydraulic engineer-type person?

Phillips: Yes, I think so, yes. Actually a modeler as well; and turbulence…

Platzman: He gave a lecture there that I attended. It must have been for some special reason that I attended it. In the course of his lecture, he mentioned the Coriolis effect. Now, he was talking about one-dimensional channel flow, and much of what he said after he mentioned the Coriolis effect was lost on me because I immediately started to wonder what in heaven's name he was referring to. Do you know what I am going to say?

Phillips: The slope of the surface or something?

Platzman: No. I went up to him afterwards and I said, "Please, explain to me what is the Coriolis effect." Oh, he said. This is well know effect in hydraulics having to do with the way in which the downchannel flow is averaged across the section in order to give a proper variable for a one-dimensional system of equations. And Coriolis, same man, turned out to be the one who produced some coefficients (and they're called Coriolis coefficients) that enable one to make that kind of average. So there you are. Anyhow, hydraulics, there are some roots that we could find in hydraulics, but getting back to the 1950's. That work preoccupied me quite a bit, and I enjoyed it especially because of the fact that there was this observational payoff, which I feel is so essential to give spirit and life to…

Phillips: Yes, it makes a big difference.

Platzman: …not only makes a difference psychologically, but I think that from the standpoint of scientific progress, it's important if not even essential, to couple any kind of a modeling innovation with a specific context example.

Phillips: And visa versa.

Platzman: Well I guess that's true.

Phillips: …That you should try and explain by theory what you observe. I remember that paper of yours that I used for many, many, many years in my oceanography course.

Platzman: Which one? The surge on Lake Michigan?

Phillips: Yes, it was an example of how the atmosphere would effect the water, and how it was possible to calculate what that effect might be, and how to verify it.

Platzman: I don't know whether you asked me in your notes about this, how I got interested in that problem; but it's not illegal, I trust, to answer a question that hasn't been asked?

Phillips: That's the hope of this interview.

Platzman: I think it must have been in the middle '50's, when Harry Wexler perhaps was visiting Chicago, and he got me to think about this in a very peculiar way. He said that he was sitting next to Sydney Yates at dinner, he's a congressman from Illinois; and Yates (this was in the midst of the cold war), and I say that for the following reason: that Yates asked Wexler whether he could direct him to someone who would know what the possible consequences would be if an atomic bomb landed in Lake Michigan near Chicago. You can imagine the absurd kinds of things that people were thinking about in those days.

Well, Harry mentioned this problem to me. He said that he had talked to some people at Argonne, but didn't get anywhere; and Harry wondered whether I had any ideas on the subject. So I told him that I'd think about it, which I did. I looked at some things like Cole's book on Underwater Explosions, and eventually I got kind of fed up with the whole idea. Perhaps partly because I didn't have the slightest idea how to answer the question. But that got me interested in Lake Michigan in a kind of bizarre sort of way let's say. Not much later I came into contact with Lee Harris. Do you know Lee Harris?

Phillips: By name.

Platzman: He's a Weather Bureau man, now retired.

Phillips: Was he in charge of the station in Chicago?

Platzman: No, he was a Washington man. He was interested in Lake Michigan, I think as a result of the paper written by Don and Ewing on that surge of 1954. They're the first ones who drew serious scientific attention to it, and who proposed this resonance phenomenon, which my calculations substantiated. Their's was essentially a qualitative discussion, with a possible estimate of the propagation speed that is needed for that mechanism. Through my contact with Lee Harris, eventually the Weather Bureau agreed, and I think Harry Wexler supported this, the Weather Bureau agreed to support some research on that particular problem.

Phillips: That was more than the resonance, it was also a focusing…

Platzman: True. There was a trimetrical effect…that's right.

Phillips: It would focus back…very good. Would you like to say more about any follow-ups to that first paper on research?

Platzman: Well, yes I can. That work opened up a period in which I was interested and involved in the Great Lakes. I recall now that probably my first (I alluded a moment ago to this strange incident of the atomic bomb in Lake Michigan) at the moment I don't recall whether that, or one other thing which I'll now mention, were the first stimuli that I had in that direction. The other thing was a visit that Fritz Dapont, do you know Fritz Dapont, made to Chicago. He and his family were there for a year. They later spent some time with Rossby in Stockholm. I became very friendly with Fritz, unfortunately I've lost contact with him. He went to Kiel after Stockholm. Anyway, Fritz wrote a paper on seiches on Lake Michigan when he got back to Germany. I think this must have been something like 1953. He sent this to me to get it published somehow. Well it was in very bad shape. Partly technically, but mostly linguistically, and I was sort of horrified at what I had to do. But eventually I did what was necessary, and I'm pretty sure that I got it published in the Journal of Marine Research. But having done a lot of nitty gritty editing on that, I naturally got involved with it, and I think that also spurred me into that subject matter. Well, once I got committed to it, I realized how little had been done for the Great Lakes of a really fundamental scientific nature on the physical technology side. I don't say that nothing was done, there had been some outstanding work done; for instance there was work by a man who was at the Bureau of Standards by the name of Culligan who did some laboratory experiments on wind ________, very respectable. But in terms of a connected and sustained effort to devote the physical _____ of the Great Lakes, it was practically vacant territory. I thought that was not right, and I would try to do what I could to promote that subject. I think that attitude kept me going at least for a while. Eventually my interest petered out, although I am happy to say that some of my students took it up. The one thing that I am particularly happy about is that after I later got involved with wind set-up on Lake Erie, which is really an aspect of the storm search problem.

Niosaki, a Japanese oceanographer, came to visit me, and he was interested in oceanic storm surges, and was probably the leading exponent of that subject in Japan. While he was with me, he did a model for modelling calculations for the storm search of Carla, which was a Gulf of Mexico storm surge. As far as I know, that was the first such numerical modelling calculation of American Hurricanes.

Phillips: Did you have any contact with Chester ____________.

Platzman: Chester was a student of mine. He got a masters degree writing on another surge on Lake Michigan, but through his contact with me, and I think also with Niosaki, he got interested in the storm surge problem. As you know since you asked the question, I assume you must know, he more than any other person perhaps, established this as an ongoing effort at NMC.

Phillips: I do not know where the actual calculations are made. His office was, and maybe still is, in __________ with the Weather Service Headquarters in Silver Spring, Maryland.

Platzman: I see. Well I should add to that-----------

Phillips: I think that by now all of the ports in the Gulf and the Atlantic Coast have storm searches, and how far they have progressed in tabulating so to speak code finders.

Platzman: I have lost track of the field, but I should add after saying what I did about Joe _______, that Lee Harris was also a moving force, very influential in promoting that kind of work within the Weather Bureau. So that is at least one good outcome of that preoccupation with the Great Lakes.

Phillips: Did you ever have any contact with, what I gather was the Great Lakes Institute?

Platzman: In Ann Arbor? Oh, you mean in Milwaukee?

Phillips: I do not remember, but Jean _______ was director.

Platzman: Oh, that is the one in Ann Arbor.

Phillips: Oh, that is the one in Ann Arbor, okay.

Platzman: That is a NOAA lab. Yes not much, but a little. I know some of the people who are there on the physical side. A student of Mortimer's went there, Everette ______ is the name.

I wanted to add something else. Oh yes. From the standpoint of operational work after doing this thing on the surge in 1954, sorry, published in 58. This was about the last thing I did on Lake Michigan, in fact it was the last thing. It was jointly with another student of mine, as well as Larry Hughes. Do you remember Larry Hughes?

Phillips: Yes, yes, yes.

Platzman: Larry Hughes was then, he is retired now I think, he was then in Kansas City.

Phillips: He was the scientific officer I think for the Central Weather Region.

Platzman: He was. Weren't they responsible for severe storms in the, I do not know, it doesn't matter.

Phillips: Separate from the Central Weather Region in Kansas City. There is a National Severe Storm Center there. There is a center there now administratively part of the University of Washington, but located physically in Kansas City that forecasts the tornado and severe thunderstorm warnings.

Platzman: Yes, that is what I am referring to. Maybe it was partly for that reason, and also because he knew about this work when he was still in Chicago at that time. He knew about this work though. To make a long story short, in Monthly Weather Review, we published three papers as a part of one overall study. One by Larry Hughes, one by me, and one by this student of mine Shirley Irish on surge forecasting. Eventually I put in the hands of the Chicago Forecast Office, some graphs, or whatever, I am not exactly sure, that enables them when they detect a suitable squall line. It has to have the proper orientation and speed, and pick it up say in Madison, to use these ______grams and other formulas to predict what is going to happen. As far as I know they are still doing that now.

Phillips: Very good. Is there any connection between this effort, the 50's, and your more recent effort in the late 70's and early 80's on the title effects of the ocean?

Platzman: I thought about it because you asked that question here, and I though about.

Phillips: Did you put aside as far as your conscious for 15 years?

Platzman: Maybe, but I do not think that there is any conc connection, no series continuity of effort, let's put it that way.

Phillips: It seems to me it would be a very major effort and wonderful study that you made with this series of five or six papers. During that time, and before that time you were personally and well acquainted with Bernard _________, who spent more than the last half of his career doing surge tests.

Platzman: You mean is that what influenced me? Another words what got me started on the oceanic title _______.

Phillips: Yes.

Platzman: No, I do not think that it was Bernard.

Phillips: The first paper published was in 1971, Ocean Tides and Related Waves.

Platzman: Oh yes.

Phillips: That title sounds like it is a review article.

Platzman: It is. Well, it was actually lecture notes for these ___________ that the American Mathematical Society has I think every year.

Phillips: Oh, so this was a publication for this.

Platzman: Right. It is a series, a serial publication, and every year they pick a different topic. That particular year happened to be physical fluid dynamics I think. My lectures, which were given at __________ Technique Institute, were simply one of quite a number of people who were there. Jewel _______ was there, Stewartson, George Backus was there, I do not remember everyone. But anyway.

Coming back to what got me into tides. I do not really know exactly, but when I returned from England after a year there in 1967, for some reason I conceived the idea of doing these two dimensional free ____________, where previously everyone had tried to reduce them to a one dimensional problem. Before too long I realized that, that could be an entrée to the problem of synthesizing.

Phillips: George, I believe we were talking about at the end of the last tape, on your interest in oceanic tides. You were in the midst of trying to recollect how that might have originated.

Platzman: Yes, and what it was, now I am unable to give you a coherent answer to that question. The closest that I can come is that in formulating the normal modes project, I foresaw that it would impinge on tides, and kind of developed the two. What prompted me in the first place to do all of that I am afraid that I cannot explain.

Phillips: My view of that series of papers from the admittedly hurrying that I was able to get in, was that you succeeded in computing normal modes with a numerical model, and tide irrelevant frequency periods range. At the end, a solution of the forced title problem for the, was it the lunar semi_____ tide, and try to recognize in that solution features that looked like some of the normal mode circulation patterns that you had covered earlier.

Platzman: That is right.

Phillips: Did you have the grandiose ambitious view in the very beginning?

Platzman: The very beginning it is hard to say, but if not at the very onset, it developed early on. As soon as it became apparent to me, this was within about a year after starting, I think that I started in 1969. As soon as it became apparent that it was indeed going to be possible practically speaking, to do the normal modes, then I foresaw that I would eventually, if I was lucky, get to the point where that question that you just formulated would be addressed.

Phillips: I was trying to think in preparing for this, whether I knew of any other geophysical phenomenon where this analytic approach has been tried, and has succeeded. I do not know of any atmospheric case that comes to my mind in the Atmospheric Guides that had not been analyzed this way. I think isn't it.

Platzman: Well, I will comment on that last statement that you made in a moment. I cannot think at the moment of any atmospheric example, but I can think of a solid earth example. Mainly the use of free oscillation calculations to simulate seismic phenomenon.

Phillips: This is when there is a point source, and your interested primarily in the _______, which is presumably a free oscillation once the source has stopped. Am I correct in that?

Platzman: That is true. That is an important distinction.

Phillips: The other thing that I noticed about your work on the tides was that towards the end, in the series of papers, the question of energy dissipation seems to dominate the fact that one of them is devoted completely to that. I was wondering the extent to, which this was fostered by you being now in the Department of Geophysics at the University, to what extent ____ exposure to solid earth scientists led you in this direction?

Platzman: No. I do not think that, that had any significant bearing influence on me on this emphasis on dissipation. I think I can say that emphatically. How my interest came about again, I am not entirely certain, but I have been aware for a long time of the problem of the secular consequences of dissipation. The general problem of the lumbar orbit from having studied the book by Malcolm McDonald when it came out, and being very much impressed by the beautiful job that they did. Much later numerical tide models began to begin fielded, this question of trying to pin down the oceanic dissipation became a much hotter topic so to speak. So it is all tied in.

Phillips: It's a pun.

Platzman: If it is it is not intentional. I had a student who wrote quite an interesting thesis on the secular aspect of the problem, and I am not sure to what extent that problem has been further pursued since that time, which was, I am not entirely certain when that was, in the 70's probably. No, maybe it was the early 80's. His name was Kirk Hansen, I think he has since left the field of oceanography. At any rate, he was able to show, you may recall that Malcolm McDonald, or rather McDonald really in another work outside of the book. He had investigated the lower orbit on the basis as most people assumed, practically everyone assumed, that oceanic dissipation was occurring at about the same rate over geological time as it is now. Of course that is a risky assumption for a number of reasons having mainly to do with the changing earth's rotation, and the consequences that, that would have on the periods of oceanic resonance. Also, having a bearing on that is the configuration of the continents and the Baring Sea question, and the question of the extent of the continental shelves, which are very important as to dissipation. Hansen tried to overcome some of these limitations by carrying out the integration of the lunar orbit while at the same time integrating the title problem under the changing conditions of the earth's rotation rate, and even investigated the consequences of different continental configurations. The main thing that came out of that was that in contrast to the earlier theories, which predicted a close encounter between the moon and the earth, essentially in approach to the _______, which was just a ________. He found that the present dissipation rate is anomalously high because of the closeness to resonance that in the present day ocean's have. When that is factored in, then the closest that the earth ever could have approached according to his calculations, was about 50 earth ________, which means that there was no significant problem of title rupture or any such profound geological effects. Whether that will continue to hold up under more astringent modeling, I do not have any idea. That was an interesting question.

Phillips: Maybe this is a good point to bring up this question of the evolution of the Meteorological Group finally into part of the Department of Geophysics at the University of Chicago. I do not remember when it took place. It certainly took place before your tenure in 70-73 as the department chairmen. So it must have been in the 60's that the merger.

Platzman: Yes in 60, I think it was either in 1960 or 1961.

Phillips: And did this merger arise from below, or was it superimposed so to speak from above.

Platzman: Well there were elements of both of those things. There were segments of the two departments that were very much in favor of the merger, and others that were terribly opposed to it. The administration however of the division in which the two departments were located, the Physical Sciences Division, strongly favored a merger. Of course that point of view carried a lot of weight and eventually won out.

Phillips: Has it been successful or worth while in your opinion?

Platzman: In my opinion it has been, although there has been a down side to it. From my standpoint, it was the right thing to do at the right time. The down side is that it altered the perception of both the traditional geology and the traditional meteorology department as it was viewed from outside the university. This changed perception took place mainly among the less sophisticated viewers such as perspective students who were looking for a certain kind of environment. It is a fact that our enrollment, at least in the meteorology component, has suffered from that to varying degrees, fluctuating degrees as you know from your experience at MIT, there are fluctuations that are often simply unaccountable. Sometimes they are part of a national trend, sometimes they are demographically explained, but sometimes they happen without any apparent reason.

Phillips: In the case of MIT, I think one would have to say that the merger which took place much later in the late 70's, was put simply, imposed from above. Neither department as a whole would have agreed to it.

Platzman: Well I will ask you the same question. What are the consequences, and what have been the consequences?

Phillips: I do not know since I left the department before that merger took place, and while I have been back there, I have been more struck by other phenomenon such as the change in the student make up. There are more female students, and more students from abroad, and they struck me much more than any interaction between the two departments. On the other hand at MIT, there is the Science of Oceanography, which, or even more naturally than geophysics and meteorology, encompass both the solid and fluid earth.

Platzman: I was at the time, I still am, an advocate of that merger. My view was that it enabled us to, from a standpoint of our interaction with the outside world, it enabled us to be in a position of attracting to the faculty, people who otherwise might not identify so easily with the more narrowly defined tradition of departments.

Phillips: Yes. I think you could say, see if you agree with this perhaps, that in the 50's the primary focus of meteorology departments at universities was on the forecast problem. That was the most obvious problem with which university faculty concerned themselves. Be it on a large scale or even a very small scale. I think that some of the intellectual challenges of that problem have been successfully addressed, and it has become to a great extent more of an engineering problem. Therefore, has less attraction for the theoreticians perhaps than was the case on a large scale when atmospheric dynamics were first being explored.

Platzman: How do you think that has impacted on the ----------.

Phillips: I think because of that it is much more justifiable now to have meteorology and atmospheric science combined with other geophysical sciences in a single department. The intellectual challenge, which is now upper most rather than the equally challenging, but quite different problem of forecasting the weather. I do not know how far this may be reversed by the problem of forecasting earthquakes.

Platzman: That remains to be seen. We will wait until the next San Andreas earthquake, and then decide that question. However, just let me add one comment there. Some people believed when the discussion of merger was very much in the air that it would promote interdisciplinary collaboration within the university. Merely by the fact that the two groups of people were officially one administrative unit, and as it turned out, eventually occupied one building, as they do in our situation. I never put too much stock in that because I have always felt that where there is a strong intellectual urge to collaborate, administrative boundaries will be totally unimportant, and whether those boundaries fall within the institution or outside it does not really matter.

It is true that there have been some collaborations across interdisciplinary lines in our department that are interesting such as between the paleogeograph's and ________, _______ being interested in paleoclimate as one of his numerous interests. I still feel that this is not a serious motivation for the way in which academic units are structured at the University. So much for the external aspect of it.

Internally, I think that it was all to the good because being a larger unit it gave us more clout. It is as simple as that. I think that the experience has tended to bare that out.

Phillips: This may be the other side of the statement that I have heard made at MIT. The administration was in favor of the union of the two departments because they felt that cases for tenure that came up to the Dean's level, and then gone through more competition, and would be the case in a smaller department.

Platzman: That is a good point.

Phillips: Thereby had more merit, and the Dean's were understandably much happier and possibly even more effective.

Platzman: That is an interesting point. I remember, if I could just give you one graphic example of that, that as you recall Lewis _______ spent quite a number of years in our department, subsequently retired, when his appointment was being discussed in the usual way in the faculty. One of the remarkable performances I thought at the time, and still do, advocating the appointment was by, I suppose at this degree of remoteness from that time and situation, it won't be violating any confidence to mention name, Joe Smith, in our department is a mineralogist. He is quite broad scientifically, and he found little difficulty appreciating the spectroscopic aspects of Lewis _______ work. He gave an impatient statement verbal and written advocating that appointment. I think that is a good example of the kind of support that you were referring to.

Phillips: You had one year service as head of the Physical Sciences Section in the college.

Platzman: Oh yes.

Phillips: Now is this-----

Platzman: I see that you mention that in your outline. No do not attach any significance to that. What happened as best as I can recall is that the head took a years leave of absence. I simply filled in for that person who's identity I no longer recall, and I kind of enjoyed it, but there were too many meetings for my taste.

Phillips: I can imagine.

Platzman: I am sure you know what I mean.

Phillips: So it was completely unrelated to your department.

Platzman: Oh totally, yes.

Phillips: You were department head for three years in the early 70's. I must be frank and admit that I had forgotten that fact since I was, at that time, serving for about the same period, a department head at MIT.

Platzman: Right.

Phillips: I recall that in our case, the most severe problem was finances, and I was wondering if the ground rules became a little bit different from what they had been in previous years in supporting the faculty. I was wondering if you had that thrust upon you when you were in that position.

Platzman: No. I am happy to say that I did not face any serious financial crisis.

Phillips: I would not dignify this by the request, but it was a nagging problem that I had.

Platzman: No. Even as a nagging problem. Not to say that we could do whatever we wanted, but I guess that I would say that there were other problems within the department that, while not serious, I would rank as more formidable than the budgetary problem. I saw that question this morning when I read your outline. I tried to think of what some of these more challenging issues actually were. I do not think that I encountered during those three years, any fundamentally irresolvable issue of grand policy that I was involved in. Perhaps it was just the luck of the draw. I do recall that I had to work hard to justify my appointment as chairman on several occasions. One of them was that we had a, and this is university policy, to have a visiting committee. There is recurrence interval for such committees, and I do not recall what that interval is, maybe seven years, I really do not remember, but it happened to fall in my tenure as chairman.

Phillips: This was not the time that I was out to visit you?

Platzman: I believe it was.

Phillips: In the early 70's?

Platzman: Right, I think it was. So you remember the situation then.

Phillips: Yes. I think Juliann Goldsmith was the chairman.

Platzman: I do not think so. I was the chairman during the visiting committee. He may have played a role, I do not recall the details, in that process having been the previous chairman. He probably joined our discussion, and a lot of the questions that were asked by the committee are questions that he was probably best qualified to speak about.

That was a hard job to deal with that visiting committee. Not really because there were any serious intellectual problems involved, but just to do what was necessary to make the convening of that committee a worthwhile event, and not just some trivial public relations stunt. I think that we managed finally to do that. There are issues probably that still remain that were brought out at that time that we were all well aware of, but emphasized by the committee, and that are probably still somewhat unresolved. The one thing that I think that the committee touched upon that I took some exception to is wanting to emphasize the importance of geophysical fluid dynamics for our department. This is a philosophical issue, and I do not think that we should get into here because the discussion could be somewhat prolonged. That is the only thing that I can think of in the aftermath of the visiting committee that I gave a lot of thought to.

Phillips: My only memory of our deliberations was that rather than emphasizing the need for more geophysical fluid dynamics, it was a suggestion, perhaps there is a word stronger than that, that there should be more seismological type of work. Is that what you meant?

Platzman: No. That is not what I meant. That is a different issue, and it is one that we have been coping with, well aware of, trying to deal with right from the beginning of the formation of the department. We have been totally unsuccessful in all of the efforts that we have launched, and maybe what it will take is the next earthquake, which some people say will be sometime in the next 100 years or so, or maybe a lot sooner.

Phillips: The visiting committee may have been a little off base in saying that, or implying that, that would make a big difference in the future of the department. The department, as far as I know, has been quite successful in its own fields without any added expertise in seismology.

Platzman: Well it is true that on the geological side the department's geochemistry and mineralogy are really outstanding. That is a condition that I am glad to say still prevails, but none the less, I have always felt, and I still do, that the lack of any seismology is a significant omission, and it would help to strengthen us considerably if it were there.

Phillips: We are almost running out of my preconceived thoughts.

Platzman: Are we really?

Phillips: Except that, no, not preconceived thoughts, but ideas on what proved to be ------- be we have covered a lot more. One thing that we have both been preparing for to some extent is a discussion of your interest in the history of meteorology, and even more broadly in science in general.

Platzman: I will be delighted to get into that Norman. First can we revert to some of the points that we have already covered to give me a chance to make some further comments? I am looking at your outline here now, which is really quite useful for focusing the discussion.

You are referring here in point number 8 to the spectral form of the equations. Where did this interest come from? Well, I think we have already discussed that.

Has it been superseded by the transform method in your opinion? This is a question of terminology. I do not look at the transform method as being an alternative in the spectral method, but simply it is one technique to deal with one of problems that arises in the spectral method. I think, at least according to my terminology, I would prefer to use the term spectral method as referring to any method that uses a spectral decomposition to record the history of the ________.

Phillips: I see.

Platzman: The transform method, when it is used in conjunction with the spectral method defined in that sense, does not alter that decision. What it does is to provide an effective means to evaluate the replications of the expansion that occur whenever you have a nominator and interaction. That connection incidentally, and this goes back to my earlier comments about our both being at that conference in 1958 in Washington, where we presented our respective ideas about __________. I subsequently published the work that I presented somewhat elaborated in a paper, The Journal of Meteorology, where I went into a much too lengthy philosophical discussion about this question. It is a paper called The Approximation to the Product of Discrete Functions.

Phillips: It is not a very long paper.

Platzman: Really? I remember it as being perhaps too exotic, but what I tried to do there is to describe what I learned in trying to deal with this ________ question. One of the things that I learned was that one can put that question in context of some aspects of communication theory. The first thing that would come to mind in that connection is the __________ folding frequency, which of course is really another way of stating the _________ problem. When I was setting up the project that I described to you that we carried out in 1956-1958 on the General Motors computer, I looked into these things from the standpoint of communication theory. I read such things as papers by a man by the name of Campbell who worked for Bell Tone Research Labs. Also, the work of a well known name, also a Bell man, who worked in communication theory, his name escapes me now. That was one background for what you and I were respectively doing.

The other was a little bit more mathematically exotic, and it had to do with work on interpolation theory. Now at first site it may sound strange that interpolation theory would have anything at all to do with this question, but think of it this way. This is a thought that I remember when it first occurred to me, somewhat like the story that _______ Ray tells about the flash that he got when he put his right foot on the first step of a bus in Paris. Everything became suddenly clear to him on the problem of _______ groups, or whatever it was that he was working on. I remember I was sitting in my living room reading the newspaper, and somewhat the same thing happened to me.

Here is the idea that if you want to multiply two functions, and avoid the folding that takes place on the spectral domain, all you really have to do is to interpolate each of the functions on a grid of twice the refinement. You have to do the interpolation using the same basis functions such as signs and co-signs for example. Obviously when you do that, then since the signs and co-signs have only the number of degrees of freedom that corresponded to the original number of points, then when you multiplied the two functions, grid point wise, you will not _________. So, interpolation is another way to look at, another window through which you can look at the problem. That led me into reading such exotic works, I use that term somewhat in jest, as by J. M. Whitaker, I believe a son of the more famous physicist. This J.M. Whitaker was a mathematician. E. T. Whitaker who wrote the book on Dynamics of Particles and so on.

Phillips: That is what I was trying to recollect.

Platzman: Anyway, he wrote a book on, I think that he called it Interpolitary Function Theory in which he defined something called a cardinal function. I found that this cardinal function had a great deal to do with this question of the kind of interpolation that I just described, and therefore, indirectly with the problem of __________. These are all things that I have tried to set forth in this article that I mentioned a moment ago. I am afraid that it did not really make an impression because it was more of a philosophical discussion than one that had any real practical consequences.

There is however, the remaining, nagging issue that I have always carried around with me as kind of one of those unresolved uncertainties, and I would welcome your comments.

As I have mentioned to you before, I was quite astonished when you showed that __________ can be unstable. As I said, I viewed it merely as another form of truncation error that was undesirable, but not fatal. I think that misconception perhaps stemmed from the _________ calculations for the very simple reason we never carried them out long enough to make them fit.

Phillips: Incidentally on that point, my mind was just stimulated of course by the fact that I did a circulation computation, which was published in 56, had to be stopped because the computations blew up. I remember at that time, or close to that time, I believe I was at Stockholm in the early winter of 56, and I must have given a lecture at Rosby's Institute. I remember him mentioning rather casually in the discussion after the seminar, if this is random noise, why isn't it like the random noise that you put in to begin the computations? I suspect that thought stuck in my mind for the next two or three years as a stimulus that there must be something organized about this ________ that we were seeing in the computations.

Platzman: And therefore, is this what you are implying its ultimate connection with aliasing?

Phillips: Yes.

Platzman: Well this is just the point that I was going to get at. You referred to noodling, and for the benefit of the listeners, this was an in term that probably no one used outside of Aberdeen as far as I know. Jewel ________ picked that up somewhere, and when he first saw these vorticity noodles, that is where he called it noodling. At any rate, the nagging uncertainty that I spoke of is kind of a two fold question.

One is, are these fine grain structures that one gets in these two dimensional computations truly and completely truncation error, or to what extent can they be physically the consequence of physical effects operating such as the ________ partition theorem? That is one thing. That is an uncertainty that I have never been able to resolve. Incidentally it is probably that uncertainty, but not by any means wholly, that has held me back from beginning a serious effort at writing up the 2nd _________ expedition. If you have any light to throw on that I would welcome it.

The second thing concerns nonlinear instability. The question there is how general is the result, and how general is the example that you gave. To what extent can one generalize from that example to virtually any nonlinear calculations?

Phillips: I think that example was misleading to some extent because it, I believe, associated very much with the time slipping procedure in the ________ equations that we were using at that time. Mathematics needed an explosive growth, which does not happen if you modify the equations to eliminate the computational modes that are introduced by the artificial time slipping procedure. Where by, as you first pointed out, we raised the order of the difference in equation in time from first to second order.

Platzman: Excuse me, but are you saying that you can eliminate the unstable aspect of the -------.

Phillips: I do not remember if I ever demonstrated that, but I do remember I tried to see what would happen with the _________ method of facts that of which would normally be dammed. In later years I recall hearing from people like Michael Gild, and ___________, and that they had established to there own satisfaction that _________ had to have some dissipation _________ system if you wanted to control computational stability.

Platzman: One of the difficulties here may be perhaps a loose use of the term computational instability.

Phillips: Yes. That could very well be.

Platzman: I mean on the one hand you could mean literally that the computation blows up and the numbers become exponentially large. This is what I think of when somebody says computational instability. On the other hand, there are as you know, many situations where truncation error will simply make it almost impossible to see what is going on, and those situations very often the numbers are well balanced. They are not exponentially large, it is just that you have too much distortion.

Phillips: I believe that is __________. The National Meteorological Center in Washington, one of the most intensely observed charts was also the 500 millibar chart, which contained the contours of _________. I found out when I was developing my own model for operational use that in order to present smooth _______ maps from the 36 hour forecast, one had to do extra smoothing of the grid point values _________ that you would compute on the forecast velocities. In my case I was using something like an _______ grid so that _________ were uniquely computable by neighboring u's and neighboring v's with no doubt about where you would get them from. The ________ became very erratic, and I tried to study this casually by looking at more and more examples in the years that I was there.

In my own mind in viewing these vorticity lines as they were printed out on these maps day after day was that in the rather smooth contours of vorticity would be stretched out by the flow in a way which would produce narrow and narrower filaments of vorticity. You could see that happening in the first 24 hours with some degree of fidelity. You lost track of it because the maps that you had to look at in having gone through the smoothing process lost there ____________. Also, the obvious interest from this of course is that the initial maps that we were starting out with have a misleading smooth view of the smoothness on vorticity field. It is really much more ___________ probably than the measurements are able to indicate. We are therefore limited. It is not enough to increase the resolution of the grids to get the correct answer to this problem. You would have to understand more about what is actually present in the atmosphere, and progress beyond the point that we were at the beginning of NWP when you just needed the location of the main blocks of vorticity.

Platzman: What kind of a smoother did you use? Very small scale?

Phillips: It was I think what we called four passes of the Shapiro filter or something like that.

Platzman: How often?

Phillips: Well, it was done during the forecast once every hour, which in our case was the time steps of about every 15 time steps. I could occasionally see this indicated in the observations. I remember one particular case where one day there was a well developed vorticity center in Texas, and it was presumably going to move to the northeast, but it disappeared in the next 24 hours, almost in the next 12 hours. I remember getting a telephone call from New York, what has your model done with this vorticity center? It was forecast to disappear. The next day, the guy called me back up and said the model was right it has disappeared.

In this case however, if you look closely at the observations you could detect the traces of what was probably a pronounced shear line southeast of the United States, which was a remnant of this. I find it very difficult to separate the mathematical numerical mistakes that we were making from the areas that are present in the initial field. I certainly suspect that the __________, which is at the _________ at one aspect of it, the vorticity is going towards higher wave numbers is because it is a reflection of this __________ process, or the beginning narrow filaments of vorticity.

Platzman: When you mention these filaments of vorticity, I was reminded of, and I hope accurately, about a paper by _____________, where he actually mapped these things. I do not know how he did that.

Phillips: He did it two ways. One by looking at forecast winds, and also he had some pictures from a laboratory experiment, a _______ fluid type of experiment, where he had died columns, forecast and got a picture of there displacement from some simple initial _________.

Platzman: We were talking earlier about, you mentioned the computational waves, somehow it reminded me that I think I first mentioned that question in the little note that I wrote on stability of boundaries. Actually, and here is something that I would like to put in a plug for the listeners of this tape, by referring to the log of the ________ expedition. In fact there is separate log for each one. Isn't that right?

Phillips: Yes, I think so.

Platzman: These logs are very interesting. They are part of the charting papers at MIT archives if anyone ever wants to delve into that. I mentioned that because at the moment I do not remember whether if it was the first or second expedition, I think it was the first when, and you said yourself in referring to the ________ expeditions, that for some bazaar reasons we had to maintain a 24 hour shift. You can imagine, you do not have to imagine, you know that this left a lot of loose time. Some of this time was spent during one of the assigned 8 hour shifts in the lab, and the other was spent in the motel room reading the newspaper or whatever.

I think it was in the course of one of those down periods, not down periods, but leisure periods that I began to think about the question of whether one can solve the simple wave equations, exactly and discretely. The example of ____________ was not at that time the ____________.

Phillips: Oh.

Platzman: They do not address this question that we are talking about now. It was during one of those down times that I derived the solution that included the computational wave, and I discussed all of that in one of the log books of the ________ expedition. Strange place to record history.

Phillips: You just mentioned history. Now maybe this is now a time George when we can go with what most people are familiar with you are recognized as one of the lulls of your labors, the history of meteorology and science.

I have asked questions on how you entered upon this field, and what encouraged you to continue your work.

Platzman: Norman, I will first of all mention that yesterday, in preparation for our meeting today, I spent practically the whole day writing out my philosophy of my interest in the history of meteorology.

Phillips: With your permission we will tape that addendum to this interview.

Platzman: Okay. Well then perhaps I should not ________ too elaborately on that, but there are some points in there that I would like to make possibly more forcibly. The first and foremost is that, it is my feeling that everyone of us who is a working scientist has an obligation to make a contribution, however modest it might be to explicating the historical record of what part of the history he himself is personally involved in first of all. It is surprising, once you start to think about that question, how much there is to say in almost any persons career. It is this feeling of, as Richardson would say, public duty that I think has been a large part of my motivation. Not the whole, but a large part. Without it, I think that I would have felt too self-conscious to pursue this effort as seriously as I have. The other part of motivation is very simply that I enjoy that as an intellectual exercise. I think that you can appreciate because you yourself have made some pretty significant steps.

Phillips: Less effort and dedication I think that than ---------.

Platzman: Oh, I would not say that. For instance I look upon your review of _______ motion in a way a good example of how to summarize the threads of the origin and development of ideas.

Phillips: Now that, of course that is based on published papers. So that deals only with the, lets say the official, almost bureaucratic record of where ideas seem to have come from. Where as we are trying in an interview like this, and as I am sure the idealizing the historian tries to do. He tries to go below that public image of events, and what was probably the more human involvements.

Platzman: Yes that is true, and I think that is a large part of the, how shall I describe it, the methodology of the professional historian in science. That is a lot of fun. I am not totally convinced that it has lasting merit in terms of the intellectual history of that endeavor. I am reminded somehow, and this is in a way speaking against my advocacy of history, I am reminded of a little interchange. I think it was between Harris, and someone who was asking him about his encounter with Maxwell's theory with which of course Harris was thorough conversed on, he had to be. As you perhaps know Maxwell's exposition largely probably in his book on, what was it called, Electricity and magnetism, I have forgotten, but anyway. It is pretty opaque stuff in many places.

When Harris was asked how he managed to wade through all of this difficult exposition of Maxwell's, his answer was very revealing and very much to the point. He said to him Maxwell's theory is Maxwell's equations that is what really counted. Everything else was, he did not use this term, __________. I think there is perhaps a lesson to be learned there. At the same time I agreed that the sociology of science is an interesting subject in its own right, and I think very worthy of pursuing.

Phillips: I think that by listening to your interview with Charney, my own exposure to him, and my own reading of part of the archived newspapers at MIT, has been very instructive in that it demonstrates the virtue of being a very open and receptive person. Ideas originate in ways that cannot be planned, and Jewel was very successful in going beyond the lookout and being receptive.

Platzman: Did he actually say-------?

Phillips: No, he did not say that I am saying that. He has demonstrated by papers behavior in you interview shows is a good example of benefits of having that approach to the science. You cannot have too narrow of a view of what you are doing.

Platzman: True, but ------.

Phillips: History shows these ideas are stimulated by exposures here and there.

Platzman: Right. The intellectual interactions are very subtle and very important. You mentioned Charney. One could also say about Charney that he was very well informed about the things that he had to know to do what he was doing. He was perhaps less well informed about some related things to what he was doing that were important, but maybe not so important that they were vital too him.

Let me give you an example. I think it was in the interview with him, and I hope that I am not remembering just a private conversation. When I asked him what he thought of ____________ work. Was that in the interview? Do you remember that?

Phillips: I think that it is in the interviews.

Platzman: Well, he astonished me by saying that he really was not familiar with it. Now that to me is remarkable don't you think?

Phillips: Yes, there was another aspect I think of Jewel that he was yet what a lesser person might be called arrogant. He thought he could decide which subjects he was already a master of.

Platzman: Well, you could put it that way, and I do not know, that is perhaps partly true. I think that it is also true that if you look carefully at the really profound achievers who have put forth half of there ideas it is often the case that they have blinders in varying degrees to what is going on around them. I think there is something of a lesson to be learned there. Perhaps maybe it is a lessen that lesser mortals such as I cannot afford to learn, or a habit that we cannot afford to emulate.

Another example that I am somehow reminded of Einstein. There has been in recent years a lot of discussion in the historical journals and elsewhere on the question how much did Einstein really know when he for instance wrote the special relativity paper in 1905. In particular, did he really know anything about Michaels and Morely. Well, there is significant evidence to point to the fact that he did not, and I think that if I am not mistaking that Einstein himself denied any real knowledge of Michaels an Morely at the time that he wrote that paper. One could probably go on.

Phillips: I might be able to hazard a guess as to why Jewel was somewhat cavalier in his discussion of __________ paper on _________ motion. That may have been because I, with _________ and Bill ________, read and studied _________ paper in 1959 when we did are review of Russian literature, and published a short very adequate summary on it. Jewel must have read that because he was discussing all Russian literature and he made tribute to the Soviet Union in the early 60's. He must have made an effort to get familiar with it. He must have known about ________ paper through that, but that was perhaps not enough to let him answer your question in your interview.

Platzman: He read it after the fact you might say retrospectively rather than ---------.

Phillips: Read it ongoing in our review with him.

Platzman: Incidentally did you use Jim ________ translation of that paper, or did you make your own?

Phillips: I remember that now. No. We had our own translation. I do not remember if we had that translated for us. That study was paid for by the CIA because they wanted to know what the Russian's were doing as far as weather prediction. I think there was a companion study, I suspect financed by Louis ________ on Russian efforts and the rain making. I do not know if that was a fact in this case. They supplied us with some translations. There was an Israel translation project back at that time. There was translated articles on __________ of course. This one I may have read.

Platzman: Further comment since we mentioned Obacoff. At one time I was really deeply immersed in the adjustment ____________.

Phillips: He put out a collection of papers didn't he?

Platzman: That is right. I got together a collection that included Rosby's paper, Cahn's paper, and I think that there was a paper by ________ there. Also, Jim _________ translation of Obacoff's paper. I sent this collection to Obacoff. As I was rummaging through my files the other day I found a reply from Obacoff to that thanking me for the collection, which he found very interesting, and saying that the translation of his paper seemed to be very good. I did not realize that his English was that good.

Phillips: He was quite good in English. I do not know if he stayed at our house one night when he visited MIT.

Platzman: Well, getting back to history. Where are we now and what should I say about that?

Phillips: You had these various studies, and one interesting question is how you were led to do each one? For any of them was the initiative yours, or were you led or seduced into them?

Platzman: That is an interesting question, and I am glad you brought that up. Before I wrote this little discourse yesterday where I discussed each of my contributions, I do not think that I would have realized that every single one of them was done not of my initiative, but somebody else's.

To be specific the first one on Richardson' book was as you know an invitation to write book review. The second one is the Simon's lecture, and that of course was only by invitation.

Phillips: Was the topic chosen?

Platzman: No. The topic was not chosen. So in that sense I made that decision independently. As I mentioned here in these notes, I wanted to do something a little different from the usual technical discourse, especially because Simon's lecture is suppose to be given to a general audience, and is given to a general audience. I am sure you have attended meetings of the RMS, and you know that the general audience is quite general there, and in fact there are a lot of nonprofessional ________ who go to those things.

That coupled with the fact that, this is something that I was not able to remember, I think there was an anniversary of Rosby's having given a paper to the Canadian Brance of the RMS. I am not clear about that. Let's just put it this way. I obviously came to the conclusion talking about the Rosby wave would be appropriate, and that was partly because I felt that Rosby's work had never been fully appreciated in the British School. There was something of an intellectual barrier there.

Incidentally, you will recall the remarks made by ________ in the letter that he wrote to Joe _______ that you put into your article that is now in the __________. Anyone reading that letter would get the idea that they did not care two cents about what Rosby said or did. They went on there own sweet way.

Phillips: The one thoughts I was able to put on that from my own understanding was that Rosby's paper was published in 1939 in a rather obscure journal. He may have sent me prints but I do not know. The British meteorological establishment was occupied with more immediate problems than weather forecasting, and long waves I think. I could understand how ________ might not have really appreciated or maybe even known Rosby's paper.

Platzman: Okay. Fair enough.

Phillips: In 1939 the British were already at war.

Platzman: Oh yes.

Phillips: In the fall of 1939.

Platzman: Yes. The war was not really serious for another year or so.

Phillips: That is true.

Platzman: Anyhow. I thought I would do the opposite of carrying coals to New Castle. That was an invitation, although as you say not the subject. There is an example is the Star lecture, and there again that was obviously an invitation. There again, I picked the subject. There the subject was not really the most appropriate for that occasion.

Phillips: I remember the lecture was extremely well received.

Platzman: Well, well. That is a different matter. As I say in these notes. It would have been nice if the first Star lecture could have been more directly about Star's work itself. I did not really feel able to do that because the best topic would have been the whole subject of eddy transport. That was actually later done, actually more recently. Wasn't it done by Wart.

Phillips: Perhaps so.

Platzman: I have not gone to the subsequent ------

Phillips: You mean undergraduate students?

Platzman: No. As a Star lecture.

Phillips: Oh yes, yes that is right.

Platzman: Okay good.

Phillips: Yes, either last December or the previous December.

Platzman: My non________ is finally overcome. That was the Star lecture. Anyhow, the point is that they were all in one way or another stimulated by invitations.

Phillips: In all of these you had nothing prepared when you received invitations?

Platzman: No.

Phillips: You thought for a few days and said I will now start to do this?

Platzman: Yes. Is that unusual?

Phillips: It takes discipline.

Platzman: How else can it be done?

Phillips: A demand on your brain.

Platzman: I remember having said to Peter Sheppard, he is the one who invited me to give the science lecture. I worked very hard at it for several months before giving it. I and another group of people were invited out to his place in the country, and I mentioned to him on the train going out there that I had worked very hard on the Simon's lecture. I think these were the exact words I used, "an exhausting experience". That was an innocent statement really, but he seemed shocked by that. Perhaps shocked that he had been responsible for exhausting me, I am really not sure, I just remember his reaction of astonishment that I had made that confession.

Phillips: This was in advance of giving the lecture?

Platzman: Yes. In advance of giving it, yes. Incidentally, one of the people who came to the lecture, and I spoke to him after the lecture was Ernest Gould. Did you ever meet him?

Phillips: No. I have seen pictures of him and read his correspondence.

Platzman: I mention him because, this comment is I think _________ our present activity. When I was there I had already become in oral history. I urged certain people connected with the RMS to get into it. I said in particular the one you ought to start with is Gould. I even wrote a couple of letters. I solicited a letter from Spencer Ward at the AIT History of Physics Office in New York describing the basic techniques of doing an interview. I passed that on to, I forget how it was there.

Phillips: This was in 65 or 66 wasn't it?

Platzman: 67 yes. Unfortunately nothing was ever done and now Gould is no longer with us.

Phillips: That is not surprising. In writing up this article on the Emergence of __________ Theory for __________, a fair amount of space is devoted to Sutcliffe who worked in that field before and immediately after World War II. He is rather old. He was in his 80's. I think that I did not have is address to begin with so my communication with him transpired mostly through Brian _________ at the University of ________. I asked Brian to give Sutcliffe this letter from me asking for permission to reproduce his letter ________. Also, sending him copies of those parts of the chapter, which dealt with Israel. _______ wrote back, yes, by all means you can publish that letter. Reggie, I think that he called him Reggie, was flattered by the attention to his work, but did not see why anyone was bothering him on things that took place 40 years ago.

Platzman: Well.

Phillips: Somehow that seems surprising to me in English.

Platzman: No. I think that it is fairly typical actually that --------

Phillips: Dissidents?

Platzman: Yes, right. I am happy to say, and this is _________ of my earlier somewhat pessimistic statement about Gould, and the fact that he was not interviewed, I am happy to say that I think that situation has since turned around despite what you said about Reggie Sutcliffe earlier. The RMS now has a history committee. Philip Grason has been quite active in that, and to the best of my knowledge they have an oral history program. How far they have gone with it I do not know. I do know that they have scheduled sessions in the RMS meetings on historic history topics. I think perhaps the tide has turned there.

Phillips: Undoubtedly your efforts that have fostered the awareness in the AMS and _____, and the historical type of interview that we are doing today for example.

Platzman: Oral history? Possibly. I am really not sure about that.

Phillips: Was there anything existing of this type before you did the Charning interview?

Platzman: If there was I am not aware of it.

Phillips: That of course, maybe you should put on the record, was also something where it was not your initiative.

Platzman: That is true.

Phillips: By having agreed to it, you responded immensely.

Platzman: As a matter of fact now that you mention that I realize that I miss stated that fact that none of my efforts were on my initiative. That is not true. There is one that was, and that was the interview of Harrowitz. I suggested that to him. In that case, he was reluctant at first.

Phillips: Your write up that you gave me a copy of yesterday relates how to your horror you found out on the tape that you did most of the talking, and ________ was content to listen much of the time.

Platzman: Oh yes.

Phillips: Do you have any historical studies underway now?

Platzman: No I don't really. Well no not really. I wrote one little note that I thought was quite nice actually, and I did not know where it should be published. I could not think of any suitable place for publishing it. I eventually sent it to the bulletin. The bulletin editor did not like it so its languish is in my unpublished file.

Phillips: This isn't the ________?

Platzman: No, no, it is a much more recent thing in which a tried to write the history of the development of the idea of what I referred to as the energy product. Now, you have to let me explain that. Since this may be the only record that is ever going to exist on that effort.

An operator has a spectrum, and one of the attributes of the spectrum is often ______ functions that are _________. That is pretty straight forward stuff. It is a fact however, that the manner in which this condition manifests itself in its detailed mathematical expression may differ quite strikingly from one problem to another. In the case of the equations of motion that we have worked with, the so called primitive equations of motion, it is possible to arrive at an _____________ principle without raising the order of those equations, as is almost often done in the course of deriving a suitable operative.

I first came into contact with that idea by reading a paper by Bob Reed, the Oceanographer, on edge waves. I think it is his thesis, which he did under Carl ______, and essentially the idea is really simple. He multiplied the x equation by u, the y equation by v, and the z equation by w if there is a non-hydrostatic equation. You do similar things for the energy equation. This is a process that can lead directly to a scale or product of functions of state, which consist of u, v, w, etc. These constituted an ensemble one vector of state. Simply by working in this way at the primitive level you arrive directly at a scale or product of functions, which is the basis for an expansion theorem, and for the _____________ of the corresponding functions.

The reason that I refer to it as an energy product is very simply that when the two functions involved are identical it produces, or one half of it is the energy. Where did this idea come from? That is the question that I asked myself. It is the kind of question that is often considered irrelevant because the people who know this technique do not feel the need to raise the question. The people who do not know about it are not interested anyway. There you are in a vicious circle.

I wrote a note on the history of that subject showing how it goes back almost to ______, but more explicit to Proudman, and then showing how Carl ________ contributed to it, as well as several other branches. I do not recall all of the details. That is a longwinded answer to your question of do I have any other historical studies in hand.

I do not intend at the moment to do anything else. There are too many irons in the fire as it is.

Phillips: Your wrote me a month or so ago saying you were wrestling with some problem, and you did not see how you were going to be able to solve it. Those are not the right words, but…… You had a very naughty problem in which you did not seem to have an answer for.

Platzman: I wonder what that was?

Phillips: This was when you were writing to tell me that you would not have a complete map of your life because there was not much time left.

Platzman: Oh yes, I was making excuses. What was that? I do not recall exactly.

Phillips: Was that one of the energy questions?

Platzman: What I am now working on, and I will give a seminar here on Wednesday, is the question of energy balance for the lunar atmospheric ________. I will tell you the result, which is very easily stated. This is an observational study.

As you doubtless know from Tony Hollingsworth work, he was your student, and this was the thesis he did under you. The oceans are very important in producing the lunar atmospheric tide that we observe. How can this importance be quantified? One can, as he did, compute the atmospheric response with oceanic input, as well as the tide potential input and gravitational forces directly. Another way to ask the question is, how much of the energy for the atmospheric tide comes from the ocean?

Phillips: Oh.

Platzman: That is the kind of question that I am trying to answer. As far as I have been able to determine so far from this observational study, which has many points of uncertainty, virtually all of the energy of the atmospheric tide comes from the ocean. There is even the possibility, although the dates are not available to answer at this point, that the gravitational force may be driven backwards. That is to say that the atmosphere has to do work against the gravitational force in order to survive so to speak because so much energy is being pumped up from below. In quantitative terms approximately 10 jigowatts are transferred from the ocean to the atmosphere. Practically all of that is dissipated. The atmosphere receives a negligible amount of energy directly from the gravitational potential. You do not need to comment. That is all I am going say.

Phillips: Oh.

Platzman: I have a lot of fancy diagrams to elaborate on this, but you know all there is to know about it right now.

Phillips: That is really amazing George.

Phillips: After an intermission of a few minutes lets resume the interview. We are doing an interview here at NCAR on a beautiful day, and in the past 30 years, since our formation in the early 60's, you have come to NCAR quite a few times, and certainly the fact that you have come back means that you have enjoyed it. What have you found to be the most interesting aspects of NCAR on your trips?

Platzman: Well Norman. I have come to NCAR right from the beginning. From the time that NCAR was situated on the CU campus. The visitors

(Tape 3 of 4 ended after side 1. Side 2 blank. Conversation ended)

Phillips: You got this reasonable prediction of general circulation similar to what Rosby explained in words in his yearbook in the 30's. I remember giving it in a seminar in the Geophysics Institute in Stockholm with Leosin and ________ in attendance. _________ remarked to Leosin on the way out, see you can still do things with cosigns and hydraulic signs.

Platzman: I just want to answer your somewhat self defacing comment about your thesis, that while it is possible that you had some or most of the ingredients. The art is to put them together, and to know that there is something that lays behind or above those ingredients.

Phillips: I was somewhat kind of chef for _________ in Chicago.

Platzman: Earl Berrit. I put him on the list although I do not know exactly to what extent he can be considered one of my students. Perhaps he was my student my student by default since he was not anyone else's student. I do remember many discussions with him and critiques of his research.

Phillips: Did he do this thesis after he was in Stockholm or before? He was at Stockholm in 53, 54…..

Platzman: This was after. This was 58.

Phillips: I see. Okay.

Platzman: I am glad to have a change to mention Earl Berrit because there again was another person was phenomenally creative. I do not know how well you knew Earl Berrit, but he was a remarkably creative person.

Phillips: Very intense person.

Platzman: Yes. His personality was totally different from Pete _______ who had a kind of very easy going and sunny disposition, and Earl was, as you say somewhat tense, but I always had a great deal of affection Earl, and a great admiration for his remarkable originality.

Phillips: He expanded in a certain sense of wider range of phenomenon.

Platzman: Yes, that is right.

Phillips: Dupont thermometer.

Platzman: Sonic thermometer.

Phillips: The thesis problem was the osculation or ________ I do not know what word he uses of the polar vortex.

Platzman: Yes, wave number one.

Phillips: Yes.

Platzman: In the course of that work he had to make a number of _________ analyses, and he concocted a machine, I mean an electronic devise to help him do the harmonic analysis. In longitude. To do the signs and cosigns.

Phillips: Did he patent it?

Platzman: I don't think so.

Phillips: He was at NCAR for a while, isn't that where he went?

Platzman: No, not at NCAR. He was with NOAA actually over here on 30th Street.

Phillips: Oh, I see.

Platzman: In the, what do they call that branch there, I don't remember right now. That is then end of the list of my PHD's. The other people, we mentioned some of them already, Chester ________ we have mentioned. John Lewis is now at the Severe Storm Center in Norman, Oklahoma. I think he is the director of research group there. Some of these other people I have lost track of.

I had a student by the name of Roger Steem. He went to work for an engineering consulting company here in Denver. I am always kind of tickled when I am reminded of his thesis. The title of it is Calculation of Normal Modes of Osculation and Rotating Bases Using Inverse _________.

Well, the problem with normal ______ in a rectangular or square basin is one with wave aptitude. Other people worked on it and so quite a good deal was known about it. Nobody really investigated the full range of parameter space particularly, I guess exclusively in this case involving rotation speed. There is one quite peculiar aspect of that problem for the fundamental mode. I am trying to gather my wits to be sure that I state this correctly. The fundamental mode that starts out as a negative wave. That is to say a wave that propagates opposite to rotation. The question is what happens to that mode as the rotation increases. This is another one he did, and the result is really peculiar.

When it starts out there is single ________ at the center of the square, and the face distribution is such as to indicate a way that __________ in the clockwise direction for the northern hemisphere. As you speed it up, what you would expect eventually, this is just intuitive physical reasoning, is that for sufficiently higher rotation you should get something like a Calvin wave that travels on the boundary. Of course the Calvin wave propagates in the other direction. The question is how can that come about. His calculation revealed that in a really striking way. The first thing that happens is that at a certain point _____________ enter the basin through each of the four sides in the middle of those sides, and start to move toward the center.

Phillips: Separate circulation centers.

Platzman: That is right. You see the mere presence of those, which are moving in the opposite direction. It begins to give you a base in the desired direction of the boundary. To further speed it up, those ____________ continue to move toward the center. You imagine what could possibly happen at this point? They merged. At that point, there is a single counterclockwise circulation with only 1 _________, a positive __________ in the center. That occurs precisely at ________. That is to say when the frequency of the osculation is equal to _______ parameter. When that happens, it turns out, this is something that was first shown by _________ actually, the disturbance, the wave motion, is ________.

Phillips: The vorticity equation does not mean anything.

Platzman: Yes. You see this is a singular--------.

Phillips: Maybe the reason he did not have any _________, but that is had to say.

Platzman: You see it is not a Calvin wave yet. Now, that is only f equals ______. What happens if you go even farther? You speed it up still farther, and then the four _________ points reappear, but instead of moving out along the sides that they came in on, along the mid-point, they start moving out on the diagonals. You still get four ___________ points, which then start to approach the corners, and the corners are limiting points for those four arbitrarily ___________. Anything in between there is going to give you a perfect Calvin wave with four __________ with one on each of the diagonals. Who would have predicted such a bizarre ----.

Phillips: It sounds like something perfectly designed ___________.

Platzman: Could be.

Phillips: Well George, I think this may bring us to the end of our interview. It has been a pleasure to sit here with you. The time is now 4:30 pm.

Platzman: It is 4:30 pm. I thank you for your very kind interviewing style. You have been very gentle, and not embarrassed me with hard questions.

Phillips: I spent sleepless nights trying to --------.

Thank you George.