BERNHARD HAURWITZ'S TALK - #4

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At this point in my talks, I am just coming back to MIT (the year is 1941). There weren't any bands to welcome me at the railroad station or anything like that, but at least nobody at MIT strenuously objected to my being back. Though in Canada I was an "enemy alien," as soon as I crossed the Canadian border I became a neutral alien again. So when I came to the United States, to begin with I was at least quite neutral.

Now, MIT had of course changed, and the Meteorology Department had changed considerably since I was first there. I was aware of the changes, especially that there were quite a few more professors than when I had been there in 1932/33. (I think I have some slides there which show some of the people.) (Slide) The chairman of the MIT department at the time was Sverre Petterssen, who is not in the picture because he disappeared very soon after he got me to MIT. Next there was Prof. Willett, whom I had mentioned before when I spoke about MIT. He was a synoptic meteorologist. Then there was a newcomer at that time, Henry Houghton, whom I am sure many of you know or have heard about. Henry Houghton originally was an electrical engineer who somehow got roped into research in fog dissipation, which was done at some site in southern Massachusetts called Round Hill, which was originally in the estate of a rich person who had donated this estate to M.I.T. After this research ended Henry came as an associate professor into the Meteorology Department. His main fields were at that time meteorological physics and thermodynamics; in fact, during the war courses he usually gave the thermodynamics course and a course in oceanography. (I don't know how he came to give the course in oceanography; I think it was largely that he was the most conscientious one of all of us). In addition to Willett and Houghton, there was a young man from New Zealand, Jim Austin, who I think had originally also gotten his doctorate degree at MIT and then later stayed on and worked in synoptic meteorology.

I cannot find one of the other members of the professorial staff on the slide, Keilly, who was working in meteorological instrumentation. But I would like to point out Allan Murphy, who is the father of the Dr. Murphy whom you know, who was engaged in receiving and plotting the weather maps. We also had a fairly advanced student, by the name of Tom Malone. He looks very young in this picture. I think when this picture was taken he had in fact already become an assistant professor. (This picture obviously wasn't taken when I first came--it was taken a bit later.) And Tom Malone later got his doctor's degree. MIT is one of the few institutions who didn't have any particular qualm about making somebody an assistant professor and still letting him work for a doctor's degree.

As I found out later when I went to New York University, you could simply not get your doctor's degree there if you were an assistant professor. The legal justification for that was, in case you are interested, that the whole professorial faculty votes on whether somebody should or should not get the doctor's degree. So, you see, one of these people who is an assistant professor but not yet a doctor, and worked on his doctor's thesis, could go to the faculty meeting and vote for himself--or, of course, <u>against</u> himself.

There are a few other people in this picture whom some of you might know. One of them is Allan Bemis, who was working in radar meteorology later. He, of course, also looks pretty young. And I might point out that there are a few men in uniform. They were Air Force instructors, because when we had these large classes during the war we simply didn't have enough civilian instructors to teach, especially in the synoptic and forecasting laboratories. So the Army Air Force--and the Navy too, for that matter--permitted us to keep some of the crop graduating during the last course, to keep them at least for a year or so.

Some of you might also know Bob Cunningham, who is here. And this is myself; I look quite a bit younger then too. That's just the ravage of the old age--you seem to be thinking it's funny. (laughter) The women whom you see here in front were either mostly secretaries, or they were working on research projects. In particular, this one was working for me for a while, and this is one of the women who worked for Willett. This particular very serious-looking young lady is now Mrs. Raven. She used to be Peggy Whitcomb; she was one of the instructors.

That gives you an idea about the composition of the staff at that time. When I came back, of course, things in general had changed quite a bit. There were at that time quite a few more meteorology departments than when I first came to this country. (I'm talking now about 1941, and I came to this country about nine years earlier.) In the meantime, Rossby had left MIT. He had gone a while to the Weather Bureau to stir things up there, which he did very well--as some of you might know. But later he got tired of bureaucracy and went to the University of Chicago, where he was Chairman at that time.

There was also UCLA, which had started a Meteorology Department under Jack Bjerknes, who was at that time assisted by Holmboe, who had left MIT.

As a matter of fact I was Holmboe's replacement, really. Holmboe preferred to go to work for Bjerknes and stayed there. So, Petterssen needed somebody else and he got me. He didn't like it, of course, but it was his fault--he asked me to come.

Then there was a Meteorology Department in Seattle, of which Phil Church was Chairman at that time. I don't know if anybody here remembers Phil Church. (Walt Roberts: Most of them were on the UCAR Board.) Well, of course, you're really an oldtimer too, Walt, you know. Don't apologize! Anyway, there was Phil Church, who died about five years ago. And finally there was also a department at New York University in the College of Engineering, which had been started by Athelstan F. Spilhaus.

At any rate, my main job at MIT in teaching was, again, as you might expect, to give a course in dynamic meteorology. By that time, of course, I had quite a bit of practice in doing so.

I guess there are at least two people here who took that course. Did you take the course too, Ed (Wolff)? I won't ask for any votes--that would be entirely unwise--but at any rate they survived, as you can see. Well, in a way, at least.

In addition to that course Sverre Petterssen had suggested right after I came that we might plan on giving a course together in climatology. Now, I had never studied climatology, but I thought it might for a change be quite a bit of fun to learn some facts about the atmosphere, and I really must say I found it quite interesting when I learned it for myself. The one thing which I decided immediately was that I couldn't really remember anything unless there seemed to be at least some more or less plausible (or physical, or whatever you call it) reason. So I tried to get things organized so that I could give some sort of a rational explanation.

In addition to that I also found out that there was something called Köppen's classification of climate, which I found sort of interesting. I mean, it has little physical significance; it is just the combination of the various possibilities which you have for variability of temperature and precipitation, the two main climatic elements. At any rate, I decided that this was a good way to learn climatology, by a sort of framework or skeleton on which I could remember the different facts--or at least what I thought were facts. So all the Air Force and Navy students who had to go through my course in climatology had to learn Köppen's classification of climates. I didn't get any protests, but then the students at that time didn't really protest very much. But later, I must say, I had quite a bit of argument with some of the people at NYU when I tried to inflict Köppen's classification on them. Not so much with Prof. London, but somebody else--Ed Fisher, if anybody here remembers him.

My dynamic meteorology classes at Toronto comprised about 12 or maybe 15 people, but at the time when I came to MIT (in the summer of 1941 and the fall of 1941, when I started teaching the regular courses there) though we didn't have very large groups of Air Force and Navy students yet, numbered at least 70 or 80, and sometimes up to 100 students, and that makes quite a lot of difference!

You confront an enormous sea of faces, most of them in air corps uniform, so they don't really look too different from each other. In addition to that we had some naval officers who were sitting there in their uniforms. And then after a while we also got some WAVES--does anybody not know what WAVES were? These were the female officers in the Navy. They were mostly sitting in front. There weren't very many of them--10 or 12--

so you felt like you got to know them, at least. They were mostly much more attentive than most of the men there. And then we actually had four or five civilians, who of course disappeared in the general crowd.

Well, when you were standing in front of these people, as I say, you just saw a sea of faces. You didn't know the individual person at all--but sometimes people stuck out. I remember in particular one time. That was a bit later, after the war had started already. We had a class of about 250 students in the dynamic meteorology lecture, and in this particular class one morning, I discovered that next to one of the naval officers in the back row was a woman whom I had not seen before. You see, the women in uniform--the WAVES--you recognized immediately. There were also a few women civilian students. But this one was obviously something new. And I was wondering all the time what the hell she--what the heck, pardon me--she did in this course. I couldn't imagine that she came there out of interest. It was in the middle of the course, and she certainly wouldn't have understood anything--unless she was a meteorologist. I only found out much later from one of the other naval officers whom I got to know, by the name of Ralph Shapiro--I am sure some of you know him too. Ralph Shapiro was in the Navy and in this class, and I mentioned to him that it looked to me as if one of the Navy officers brought his girlfriend along to my class, which seemed a very poor way of entertaining the young lady. He said, oh yes, he knew about that. What had happened was, his lady friend had just arrived this morning by train from somewhere, and, well, he had to go to class and he just brought her along--and that was that.

In that particular class, we had 250 students. But there was really a class--I think the largest class at MIT--when we had 500 students at one time, who were taught meteorology and, as was the rule, had to learn meteorology in 9 months. Unfortunately, MIT, which after all is a fairly large school, had, however, made no provision for classes of 500 students. There was one very large room which was for convocations and things like that. But this room was occupied by another large class, so I had to give my lectures twice, for 250 students each time. I had to give dynamic meteorology three times a week, once in the morning and once in the afternoon, one hour each.

Of course, I consider dynamic meteorology quite an interesting subject. However, if you teach the same thing twice, in the morning and in the afternoon, it gets a bit boring without some additional challenge. Well, I knew how far I had gotten in the morning, of course; I made a careful note of it, so I didn't get mixed up the next morning with the afternoon of the previous day. So for my own entertainment I would then decide to either try to go a little bit farther or a little bit less far--I admit it's a rather poor sort of entertainment, but it was about the only challenge which I had, as far as teaching the whole stuff.

I think at that time if somebody had to wake me about 3:00 in the morning or so--just when I was most deeply asleep--and had told me to derive the Ekman spiral, I could have started right out without any blackboard or anything and just mumbled the whole thing right through. So that was a way you entertained yourself.

The climatology, as people here who took the course at MIT would confirm, must have been quite an ordeal--not only for me, but also for the class. For some reason the climatology class always seemed to meet at times just after lunch in the afternoon, and especially during the summers it was quite hot. In addition to that, I had to show slides. I had to show slides, because that was the only way for me to remember the facts, say, about the distribution of the meteorological elements--goodness knows where--somewhere in Siberia or something like that. So there must have been a lot of catching up on sleep on the side of the students during that time. (I see Ed Wolff nodding understandingly.)

Another problem with teaching the courses for such large classes was: What do you do if you have a quiz, or if you have a final examination? In dynamic meteorology, of course, you presumably give problems, and the students work out the problems and write down how they do it, and you go through and see how much the student really understood. Say, if he forgets at the end to multiply things by 2 or 1/2, or something like that, it's really not a very serious matter. You would still give him credit for that. But obviously if you have to look over 250 papers, or even only 100 papers, you can't do that. So the grading was completely mechanized at that time even though it was before the time of the computer. You just gave true-false or multiple choice examinations, which are really difficult to make up, either in dynamic meteorology or climatology. They are difficult to make up because you have more or less to explain clearly what you want the student to answer; otherwise you always get arguments. And you have to give the information that the students cannot be expected to know. So for instance, one time when I had a question about the thermal wind in the Southern Hemisphere--excuse me for getting technical--for the location of Sydney, Australia. I put Sydney, <u>Australia</u>, down for obvious reasons, because there are Sydneys in this country and in Canada too, and the main point was that Australia is in the Southern Hemisphere. I didn't mention that, but sure enough one of the students raised his hand and asked if Australia was in the Southern Hemisphere (laughter), and I said, "yes." (I don't know if that student became a navigator--I hope not.)

At any rate, as far as the grading was concerned, the students then got sort of a punch card. They had circles which the student either had to blacken if that was the right answer, or not blacken if it wasn't the right answer; and then these cards were collected and were simply graded by the number of right and wrong answers by aides who were assisting the staff in doing that kind of work. It was really a totally unsatisfactory way to find out how much the people knew. I remember in particular, lots of the WAVES objected very strongly to that, because quite a few of them had been teachers and they knew perfectly well that these true-false or multiple choice questions are really awful.

I remember one of the trick questions, which I liked to give--I think that must been in climatology--a multiple choice question about the value of the solar constant. Now, the quotation for it at that time was that it was about 1.96 g-calories per square centimeter per minute. So the question would run, "give the closest value to the true value of the solar constant," and one choice was 1.96 g-calories per square meter per second; and then something like 2 g-calories per square centimeter per minute (which was of course supposed to be the correct answer). That was just a trick for the students who had just learned numerical value to the last decimal point. But the whole procedure, as a matter of fact, was quite unsatisfactory, as I say. But I don't really see that one could do anything else. The fortunate thing though is that quite a few people who took these war courses stayed in meteorology, and apparently became very good meteorologists in spite of the fact that they had to go through this ordeal.

Another event which had nothing directly to do with meteorology was that on some day in December (I have forgotten which day) in 1941, the attack at Pearl Harbor occurred, and then the war really broke out. Of course it was anticipated in the United States before, because the U.S. started, among other things, to train meteorologists. Well, that event had for me personally the result that I became an "enemy alien" again. I had to register again. This time I think it was with the FBI, but I'm not quite sure any more who it was. And you got a special passport which you were supposed to carry around with you, and you also were supposed to ask for permission if you wanted to go more than 20 miles away from where you lived. Well, you could get this permission, and generally quite simply, by going to a U.S. Assistant Attorney General's office and explain why you wanted to go there. It was a bit of a nuisance--but not really anything very serious.

I remember shortly after that happened, a few days later, we had a visitor, Mr. Schereschewsky. He was in fact the man who organized the meteorological service of the French armed forces. The meteorological service in France, the civilian service, was quite backwards, like in most countries, European as well as the United States. But Schereschewsky was one of the men in Europe who had recognized that the Polar front theory could very well lead to much better forecasts than had been made before.

At any rate, Schereschewsky managed to get out of France in time, and he appeared at that time at MIT. I don't really know what he did. He was there for a few days, and I met him and talked one day, and something prompted me to point out to him: "you know, I'd better tell you; I'm an enemy alien." I explained to him that I was German and had been in Canada and the a whole story. Well, he just shook his head and said, "well, that's one of the rackets I haven't heard about before." (laughter)

Quite funny, too, but a little bit more important was something else. Shortly after the war broke out, a Colonel Jones appeared from Washington--incidentally, that was not our Bill Jones who was here at NCAR--it was another Jones; after all Jones is a pretty common name. He wanted to talk to me about a project in long-range forecasting. What that was about goes back to my time in Leipzig when I studied under Weickmann. I think some of you might remember that I mentioned just briefly that Weickmann was studying pressure waves in the atmosphere, and the way that study in Leipzig came about was that he had noticed first there was quite often a sequence of weather situations which seemed to repeat. So he then started plotting pressure curves, say something like the pressure at 8:00 every morning in Hamburg. He plotted that for say three months, and then made a copy of that on transparent paper. He then took the transparent paper with the copied curve, turned it around, and pushed it over the other curve. He found that if you do that, you can find points of symmetry, or perhaps I should rather say dates of symmetry, where the pressure values which have already occurred will now be repeated after the symmetry point in the pressure. Now, if such points really exist at a sufficient number of stations, it would mean that one could get prognostic charts from what had happened before. Say, you take three months of data, and then, if there is a symmetry point after three months, you just extrapolate from what has happened before.

To analyze the appearance of symmetry points we express the pressure curve by a series of sine functions. Then we find that the symmetry points, these dates of symmetry, occur at times where all the waves (or at least most of the prominent waves of which the pressure curve is composed) have an extreme value--either maximum or minimum. It was also found in Leipzig that there were two prominent waves which seemed to occur from year to year, one with a 24-day period, and the other with a 36-day period. One seemed to have something to do with the outbreak of polar air masses; the other one seemed to be some sort of a monsoon effect. Of course, that all applied to Europe.

Somebody in the Army Air Force had decided that the Germans must have had very good long-range forecasts for their campaign in Poland. Quite a few of you probably don't remember that as vividly as I do, but when the war broke out in September 1939, the Germans experienced very little difficulty with their tanks and armored vehicles to invade Poland--even though the roads in Poland, when there is heavy rain, are very muddy and anything on wheels would get stuck. Nothing like that happened. And the Colonel said that evidently they must have used methods like Weickmann's symmetry points--and perhaps also methods by somebody else, by the name of Baur, who made long-range forecasts in Germany at that time. So the Army Air Corps decided to ask me, since I was the only person living in the United States who had worked with Weickmann at one time, to make a study of these things in the U.S., with the idea of being able to make long-range forecasts.

I told Colonel Jones that I really didn't think that the symmetry points would lead to a new forecast method because, to the best of my knowledge, it had never been tried seriously during all the seven years when I was at Leipzig. In addition to that, I pointed out that I practically come from Poland, because we lived about 20 kilometers from the Polish Border. I grew up there. And when we were schoolkids we knew perfectly well that when our long vacations were over (we had vacations of about one month's time in summer--not as much as here, but then we worked harder)--at any rate, when the vacation was over, about the middle of August, and we had to go back to school, the rains, which seemed to take place all during the vacation, stopped. During that time, the second part of August and the first part of September, the weather was really just known to be very good in eastern Europe. So there was nothing very mysterious about it, and probably the German forecasters used just plain climatology.

That didn't convince the Colonel, so I said, if they really insisted I would be glad to do it; after all this was war and I had to do my duty, and all this kind of thing. So we started out. I suggested he would want about five technical aides, and we would also need five computing machines. Now, at that time, remember, the computing machine was something which was about about as big as that and was pretty heavy, and all it did was adding and subtracting. The more advanced ones would also multiply with a terrific noise, and even divide--with even more noise. But that was high tech at that time. These things which now you can buy (if you really want to spend a lot of money) for $20, which can do all those things without any such big noise, and which in addition tell you all the sines, cosines, logarithms, and God knows what--. Of course they had not been invented yet, unfortunately.

I thought that asking for five machines for five technical aides would raise some eyebrows, but I just didn't know yet that money was plentiful at that time. So that was fine, and everything went quite well--as far as I was concerned. I suggested I would probably need about half time, so the Army Air Corps would pay MIT for half of my time. In addition to that I also suggested--I think I thought of it a bit later--that if I had to supervise these five technical aides, I would need some trained assistants. So the Air Corps let me select from the roster of the last class two officers. I went through the list--it was one of those enormous classes, you know; I didn't really know anybody there. But there was somebody by the name of Richard A. Craig, who looked quite good. And Austin said, "Oh yes, I know him; he is really quite good." So that was one man. The other one was somebody by the name of Ed Lorenz--I didn't know him either. They were both very quiet and not obnoxious people, so I never got to know them before. So they came.

I should also mention that Colonel Jones said, yes, there was something he had to tell me: that the Air Force wanted to classify this project SECRET. That really meant that I would not be able to publish anything. He said he hoped I wouldn't object. So I replied, no, surely, I do not object at all--because I didn't think we would have anything to report--I mean, nothing which we <u>wanted</u> to publish. (I was really a pessimist.) However, I said that I had to point out to him that I was an "enemy alien." He looked a bit startled and said, "Oh my God!" (laughter) and asked, "Aren't you a Canadian?" So I explained to him, no, I never got naturalized. I was in Canada about six years, but they just didn't naturalize any Germans or Italians at that period. He then said, "We could clear you for access to secret material, but that will take about three months and we want to start right away." He mentioned that they had cleared some other people; there was an Italian, for instance (well, that was obviously Fermi) for a very important bit of research. But he said, "I will tell you what we will do: we won't even put you on the project officially. We will put Henry Houghton in charge, but you will be it." So I said, "Well it's all right with me."

Now we started working. Our five technical aides, most of them wives of Air Corps cadets always wanted to know if we were getting any significant results--and, in my opinion and Ed Lorenz' and Craig's, we didn't.

The Air Corps started finally to write to us--when were we going to send the first long-range forecast? (laughter) Well, I mean, this was completely ridiculous, of course. But after that had been going on for about two or three months, I thought, "Well I'll fix them." You see, we made these harmonic analyses of barometer curves for many stations, and of course there is nothing to stop you from computing what the pressure at any station will be three weeks from now, five months from now, and so on based on these analyses. I should say that we actually selected 50 stations in the United States, so it was a major undertaking, you see, even if it may have been completely nonsensical.

I decided that we really must do that right: Let's compute the pressure at these 50 stations from our analysis six weeks in advance. That would take about two weeks, so we could send the Air Corps a pressure analysis--a prognostic pressure chart--about four weeks before it was to come due. Well, that's what we did. (Maybe it was only three weeks ahead, but at any rate, well ahead.) I fully expected that it would be a disaster. Of course, it couldn't be <u>total</u> nonsense, because you started out with real data, and unless something catastrophic happened--like all the maxima and all the minima occurring at the same time at this chosen date--well, you should get reasonable pressure values and pressure differences between different stations. But I didn't foresee what would happen, because, as it turned out, this was really one of the best prognostic pressure charts that I have ever seen. So my smart idea didn't really work out the way I wanted it to, because now it became quite impossible to convince the Air Corps project officers, that there was no skill in the method and the whole thing was a freak.

So we had to go on and make these forecasts for about two months. From time to time a civilian, some statistician, came in to look at the maps and said, yes, the map shows definite skill. And I said, I don't know where the skill came in. It took about two months of completely useless calculations before the Air Force finally decided that there was really nothing to it. In other words, that we were right.

After that work was finished, however, we suggested to the Air Corps that we might try some other methods of long-range forecasting. There had been a paper by the Russian dynamicist, Kibel. It wasn't really published; we just got a translation which probably came through the Air Corps. Kibel had at that time developed a method to make day-to-day forecasts by a graphical procedure, and we decided to try that out. At that time Ed Lorenz had been called to go somewhere else, and I was left with Dick Craig and another man, Lieutenant Jacobson, who apparently did not stay in meteorology later.

So we tried to test Kibel's method of pressure forecasting from day-to-day. We used his paper, and we prepared synoptic charts, and we then compared the prognostic charts with the actual pressure the next day. In order to do more than just look at the two maps and say they showed skill, we tried to use some sort of objectivite criterion. We took the computed pressures and the actual pressures for the second day, and computed their correlation coefficients for the whole set of 50 stations. And they came out quite well. The correlation, which means the forecast of the actual map, was 0.8--sometimes it was 0.9. We did at least 50 charts, and it seemed to work very well.

Until one day Dick Craig had, unfortunately, an idea. Instead of comparing the pressures for one day and the next day, he computed the correlation between the actual pressure on the first day and the actual pressure on the second day, and, of course, that came out at least as good as the correlations which we had computed for the forecast maps. Well, we should have probably known that before we started. What we should have done was to correlate the computed and the actual pressure changes. We did that next, and it turned out that it really didn't give good results. We got correlation coefficients of 0.4, 0.3, and something like that. Once or twice we got 0.6 or 0.7. So we decided that Kibel's method doesn't work.

In fairness to Kibel, I should say that actually the paper which we had was just the start of the method which he developed. We never obtained the paper containing the improved method. At any rate, after the war Dick Craig and I published our results in the AMS Bulletin. In the same issue of the Bulletin there appeared a report on Kibel's method by another Russian, Izvekov, and we published as a sequel to that our results, saying that we couldn't find any confirmation of the results which the Russians claimed. Our results induced Kibel to publish a paper in Russian, of which I got a translation from the scientific attache at the Russian Embassy. It was a paper in which he accused Dick Craig and me of, in effect, "warmongering." He said it was really curious that we waited till after the war with the publication, instead of publishing during the war. The truth, of course, was that we really had very little time to do any writing for publication during the war.

Finally Dick Craig and I decided we would try something else, some kind of an objective analysis of the pressure distributions or the general distributions of meteorological elements. In 1938 Rossby had published a paper in which he discussed what is known today as "Rossby waves," and had derived a formula for the speed of these waves, which was at that time sometimes called trough-formula, or the speed of long waves in the westerlies. When I read this paper in 1938, still during my Canadian time I thought that one of the things that one should take into account was that the earth is spherical. You see, these long waves were really long waves--they have wavelengths of 8,000 km, 10,000 km--and surely in that case one has to take into account the curvature of the earth. So I published a paper at that time on the motion of long waves on a spherical earth. Under the very simplifying assumptions which I made about the atmosphere, in particular that there is no divergence of motion and no vertical motion, you can express the solution for the stream functions by spherical harmonics. So Craig and I decided that the logical choice to represent the distribution of elements on the spherical earth would be a series of spherical harmonics. I'm sorry--that was again a bit technical.

Of course that had been done for a long time in geomagnetism, where the equation which governs the distribution of the magnetic elements requires solutions in spherical harmonics and that was the main reason why it had occurred to me originally. That was about the last gasp of the project. These calculations took quite a while, because--you must remember--that at that time we didn't have a high-speed computer. We had our five desk calculators with which to evaluate the determinants of fairly high order. So mistakes would always occur, and I would often spend a whole night then trying to find the last mistake which somebody had made.

At any rate we obtained some of these harmonic representations, but we decided that at the moment the whole thing simply wasn't practical. I told Dick Craig at one time that later, when he had his grandchildren, and they asked him: what did you do in the Great War, he could always tell them that the thing which he had done was to compute spherical harmonics, because there weren't enough tables which went far enough for what we needed; and then he had made harmonic presentations of pressure and temperature distributions, all as part of the war effort.

So this is the way the project ended. I might just mention that I also did, at that time, start again to do a bit of work at Blue Hill Observatory. In particular, I got better acquainted at that time with H.H. Clayton, who had for quite a long time been interested in long-range forecasting and tried to improve long-range forecasts by means of studying the variations of sunspot numbers. So when I discussed things with him, it struck me that one of the troubles about all this, which of course is now recognized by everybody, is that there is really no good physical explanation--or even a <u>poor</u> physical explanation--for the suspected relation between solar activity and the weather.

I sat down and actually wrote a paper (which I saw last night was still remembered by Walt Roberts) in which I proposed a theory. I think it was a very nice theory. The only thing is that a few years later, when I was already at NYU, one of my younger colleagues shot it down--that was Prof. London, by the way. He made some calculations about the effect of a solar flare on the ozone layer which were involved in my hypothesis. I had assumed a numerical value from an older paper by Maris and Hulburt. Prof. London pointed out that my estimates of what would happen (or, if you prefer, Maris and Hulburt's estimates) were much too high. So that was that.

I might just briefly mention that at about this time, at the end of the war, I got an invitation to come to the Institute of Tropical Meteorology in Puerto Rico. I had done, as I have mentioned before, some work on tropical cyclones. Prof. Riehl, who was at that time the Director of the Institute, which was run by the University of Chicago, suggested that I should come there, for one thing to just see what a hurricane is really like. Actually, as it turned out, this didn't work out, because since I was still an enemy alien at that time (not yet naturalized), I needed a permit from the State Department. I got the invitation in March, and I made the application for the permit right away. And then I didn't hear anything until about September, when things got a bit hairy. By that time the hurricane season is more or less over. So Rossby spoke to General Yates, who was at that time the Commander of the Army Air Force Air Weather Service. General Yates went to the State Department, and I got a telephone call from the State Department. Somebody asked me all the questions which I had already carefully filled out on the five or six pages' application form, and a few days later I received the questionnaire back and went to Puerto Rico and spent three months there.

Of course there were no hurricanes any more by the time I came in October, but it was quite a pleasant experience. I really liked it, among other things because Prof. Riehl suggested that in case I was interested in hiking I should bring hiking boots along, which I did, and we had a lot of good walks there. I also did some work on Puerto Rico, but there is really no time to talk about that.

Before I conclude my talk I might just mention that from MIT I went to NYU. After my return from Puerto Rico I went to Woods Hole for the summer, and there both Panofsky (who was at NYU) and Spilhaus (who was then Chairman at NYU) convinced me that it would be good to try being a department chairman sometime.

I cannot say that I liked particularly being a department Chairman, exept that I think the group at NYU while I was there was particularly congenial. So I really had a thoroughly good time, even though about half of my time must have been taken up with administrative crap. The reason why I left NYU was simply that I figured 12 years in that job was enough; and besides, I got restless again, and I happened to come once or twice out west to Boulder, and I dropped some hints--I think they were fairly broad hints, as a matter of fact--that I could be bought (laughter). (I don't think I put it that crudely.) Of course, the hints were to Dr. Roberts, so here I am.

I think this is a good place to stop. Thanks very much for listening to me. I'm really a little bit surprised that so many of you have been holding out through four hours, because I can't imagine that many of these things are terribly interesting.

Anyway, thank you very much. (long applause)

Roger Olson: I remember that Norbert Wiener was at MIT. Did you have any connection with him?

Haurwitz: No, not in his capacity as scientist, but we had some contacts. You know there are an enormous amount of stories about Norbert Wiener and his absentmindedness. As far as my first wife and I were concerned--she studied physics and had quite a lot of contact with the Physics Department--we had known Norbert Wiener when we first came to MIT, and then, of course, we disappeared--to Canada. Well, shortly after we came back my first wife ran into Norbert Wiener in the hall at MIT. He greeted her, and after talking to her it suddenly occurred to him that he hadn't seen her for a while. So he asked her: "Have you been away on a vacation?" So she just said with a straight face, "Oh yes, six years in Toronto." It didn't really sink in, she said, that she was just being funny. He apparently thought it was the normal thing to do, to go six years on a vacation to Toronto.

When we were back at MIT for the second time around he lived actually fairly close, and quite often Norbert Wiener and I went home on the same streetcar and got off together. I remember one time we were talking about something--as a matter of fact, I think it was about Irving P. Krick--and, well, he got quite engrossed, as he was quite apt to do. So I remember walking back and forth in front of his house while we were finishing our conversation. Finally a little girl came out and came up to him and said, "Say, Daddy, mother wants to know if you're not coming home tonight."

But again, the answer to your question is: No.