

Interview with Harry van Loon



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Harry, what would you say nowadays about the quality of the weather maps you prepared in the 50's and 60's on the Southern Hemisphere? How much were those dependent on data, and to what extent was that fantasy?

HvL: Well, we – my good friend Jan Taljaard and I – wrote a paper that came out in the *Bull. Amer. Meteor. Soc.* in 1964. We assessed the reliability of the maps. Actually, we assessed the reliability of the historical maps of the IGY (International Geophysical Year), but it goes for the earlier maps as well. If you look it up you can see how much confidence we had in the various areas, because it varies from area to area.

I remember you saying once that there was a problem with somebody who would always draw anticyclones...

HvL: Before Taljaard and I got on to the historical map series, there were two colleagues analyzing the Southern Hemisphere maps in Pretoria. We took over in 1954. All I can say is they had a wonderful imagination.

In the South Pacific Ocean south of roughly 30° there were no stations. Our colleagues drew daily maps with very few observations. North of 30S there was string of stations toward Tahiti and south of there: nothing. They would show you the trades north of 30S and south of there draw

a big anticyclone reaching almost to the Antarctic coast – so the mean maps of the Pacific in those years are useless.

What happened when you and Taljaard finally got access to satellite images? You had one paper.

HvL: That was with Aylmer Thompson. As an experiment, we wanted to see if the analyses with satellite data would be improved, first analyzing a map without the satellite data and then adding in the TIROS satellite data. It was just for fun.

What do you think what was the major result?

HvL: We could place systems like fronts and vortices accurately on the map but we couldn't get the intensity.

Did you analyze the central pressure of a low by drawing contours until you ran out of room?

HvL: We would have a ship, say, at 45 south. It had a strong westerly wind and we would then use the pressure gradient inferred from the geostrophic wind relationship. But of course we had many surprises when ships came in later where we had had no data. There could be a pressure difference of 40–60 millibars between our analysis and what the ship showed later.

Taljaard and I wrote a paper about that.

You finished university when you were 21 years old.

HvL: I didn't finish. In order to be able to attend the university in Denmark in those days you had to have matriculated, to have graduated from a high school that gave access to the university. Then as part of being allowed to study you had to have a small philosophy degree, including logic, and that's all fallen away today. Then I took some courses in chemistry, physics and mathematics to make up for the fact that I had not learned much of that in high school. Then I got to MIT.

I graduated from High School in 1943. Also I didn't start studying meteorology right away, I studied prehistoric European archeology. During the period when the ice withdrew after about 12000 years ago from Northern Europe, there were enormous climate changes, as you know. They were analyzed through various means: pollen analyses, lake varves, etc. I got interested in these climate changes, particular since I took a student job in the Weather Bureau in Denmark, just to keep a roof over my head and clothes on my body. It was a flex job. We worked Saturday, Sunday, at night, and holidays. And I had to go to lectures in the daytime. It was hard to stay awake. I worked every day then as a technician in the Weather Bureau. Learned a bit of meteorology. I had a wonderful chief

in the forecasting section, Leo Lysgaard, who was very interested in climate change, he taught me a lot. So I got interested in climate change, at a ripe young age.

I started in the Weather Bureau in October 1944 more than a year after I had graduated from high school and the job was for 36 hours a week. Then I had to disappear for a while and came back during the summer of 1945, after the war. I began archeological studies; on the side, as it were, I read about climate change, about the climatic optimum, the Iron Age cool period, the "Viking" climatic optimum, etc.. Because of my interest in that, I started taking courses in mathematics and physics. My boss Leo Lysgaard had written extensively on climate fluctuations. He got me a research assistantship with Hurd C. Willett at MIT, because at that time you could not study meteorology in Denmark. They did not teach meteorology until a Norwegian, Fjørtoft, came as professor, that must have been in the fifties. There was no professor, no department of meteorology. You had climatology within geography. That was all.

So, did you decide to forget about archeology at that point?

HvL: Yes, I got more and more involved in climate. Also I took a course at the Technical University, it was called Modern Meteorology, but actually it was quite old

fashioned, even for then. It was given by the then director of the Weather Bureau who had fought against introduction of air mass and frontal analyses in the 30s. I had a little background from my daily routine, from working with maps, studying climate, and from just being in the Weather Bureau.

Which were then the favored theories and authors in those days about climate change?

HvL: Nobody really had a clear idea of the causes of climate change. There was talk of solar influences, of volcanoes, the Milankovitch theory and so on. Still today we don't know everything about climate variability and the causes of changes. Lysgaard wrote a good book on recent climate fluctuations which was meant to be his PhD thesis but it was turned down, unfortunately. He really knew about all theories, but he didn't point to a specific one. This was a period when continental drift was still rejected by some.

Say, before we go too much further. Would you say a few words about the time you had to disappear.

HvL: No, not really.

It was the underground?

HvL: It was. But it wasn't dramatic. Neither dramatic nor romantic.

You said you came back in July. That was well after May 1945.

HvL: That's right. Because I was part of a wartime organization called "Free Denmark". They used us after the war to guard German fugitives that came to Denmark from the Eastern areas, from the Memel area, from East Prussia, and from what is now western Poland .

Close to 400,000 came into Denmark, more than 10 % of the whole population. They were housed in schools and other public places. And we had to guard them. That's why it took me a little while to get back to the Weather Bureau.

I was drafted in '46, There was a lack of officers in the Army, because none had been educated since '43 and many Danish officers had joined the SS Divisions and had been killed or kicked out of the Army. They took any suitable human they could – put them in officers' cadet school, and made them second lieutenants.

How long were you an officer?

HvL: At first you were an officer's cadet, a sub-species of lieutenant, then finally, you were a lieutenant for the last nine months or so. Then I got out. Thanks to that uniform I met my wife. That uniform was certainly a great draw.

You had this interest in climate, and climate change. Nevertheless you went to the Army.

HvL: I HAD to go to the Army. You were

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conscripted. I had to go to the Officers' cadet school. Although I did my best to avoid it.

ment. What they did in those days was to take in mathematicians and physicists, and



Harry's Officers' Cadet School unit in Denmark (ca. 1946. Harry is second from the left, middle row.).

After this time was over, after two or three years, you went back to climate?

HvL: I went back to the Weather Bureau to work as an assistant there. Then I got more and more involved in courses, and daily weather.

So, then you went to MIT?

HvL: In 1951. I talked to my benefactor Leo Lysgaard in DMI about climate change and I told him I would like to learn more about it. I couldn't learn more in Denmark, there was no faculty, no depart-

train them as meteorologists, in the Weather Bureau. So, Lysgaard said, I have a good friend in MIT, named Hurd C. Willett who is interested in climate change, and we correspond frequently. I could try to get you a job there so you can study there. He did it for me and another fellow called Hans Buch.

We went over there. First we started working for Willett, both of us, then Prof. Victor Starr took Buch away to work for him.

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Tell us about the flight over when you went to Boston. It was very different

HvL: My wife, Kirsten, and I took the train down to Schiphol airport in Holland. We almost did not make the train, because the driver chose to go through the vegetable market. In those days when farmers came in the early morning to sell their produce, it was crowded with wagons, and we were stuck in there. We got to the train one minute before it left. So we got on board the plane and flew first to Lerwick in Scotland, then to Keflavik in Iceland. Then we flew to Goose Bay in Labrador, and finally to New York.

We stayed a week in New York, fascinated by the city. Then we took a Greyhound bus out to Boston.

We rented, with Hans Buch, a couple of rooms and a kitchen on Massachusetts Avenue, in a house owned by an old Italian woman.

That would be not far from MIT then? Is it on Boston side?

HvL: On the Boston side. We walked every day across the bridge to MIT. After a year in this apartment, Buch moved into a dormitory and Kirsten and I rented an apartment on Beacon Hill.

You mentioned Starr and Willett. Were there any other professors?

HvL: Yes, Austin gave a synoptic course, and he used Petterssen's books. Tom Malone, I think it was Malone, did a climatology course using Bernhard Haurwitz's book "Climatology". Then in synoptic lab, there were two teaching assistants, one was Dick Reed and the other was Fred Sanders. Every day, four days a week, all afternoon, for four hours, we had a synoptic lab, for two semesters. That was actually wonderful. I loved that, because both Dick Reed and Sanders are fabulous synopticians. We had forecasting competitions. Starr was a fantastic teacher of dynamic meteorology, and Willett in what you might call descriptive meteorology.

Had Jule Charney arrived?

HvL: No, this is before Charney and Norm Phillips. Murray Mitchell, a well-known climatologist, came the year I left, in 1954 to work for Willett.

Was Namias there?

HvL: No, Namias had left. Then, of course we had a connection with the Air Force. Starr had a large general circulation project with the Air Force Cambridge Research Laboratory.

There was a strong interaction between them and the department at MIT. A lot of people were associated, like Ed Lorenz and Bob White, for example. Starr's students

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participated, and Phil Thompson came as a PhD student too.

It was wonderful to study there, everybody was enthusiastic. There was a lot of GI Bill people there. You probably don't know any of them. Stu Muench and Pete Leavit were in my class along with Dan Lufkin, and several others. Stu did some very good early work on wave propagation in the stratosphere.



MIT dance, Kirsten and Harry (center) relaxing with friends Dan and Pat Lufkin during their MIT days, Boston ca. 1952.

What about Larry Gates?

HvL: In my last semester Larry Gates was doing his PhD on a one-dimensional numerical model. Our class was used to compute for him.

Did you compute on adding machines then?

HvL: Yes, those things you turn a handle on.

How about Joe Smagorinski?

HvL: Yes, he was with Starr at that time.

They started running models at that time.

HvL: This was 1951 to 1954. All the activity was at Princeton then. Phil went right from MIT to Princeton. Ed Lorenz was also a PhD student at MIT at that time.

At that time you hadn't seen any computers?

HvL: MIT certainly didn't have computers in the Meteorology Department. This was taking place at Princeton, under von Neumann. Experimental model building and so on.

But had you heard about computers in those days? What did you and your colleagues think about this?

HvL: We had not heard much as students. This was in 1951–54. It had not “seeped out” as it were.

You have been a computer, you have been one of Larry Gates' early computers.

HvL: I have been a cog in a computer.

It was on a very small scale like Richardson's idea about using a whole theater.

When I look back at the teachers I had in my life, Starr ranks very high among them. He was an absolutely wonderful teacher. He and my Latin teacher in High School probably were the best teachers of my life.

This wonderful time came to an end. You got a Masters degree then?

HvL: No, I didn't. You know, what a tuition fee was in those days? \$600 per semester. I got \$167 per month working for Willett. It could just barely take me through life as it were, because the IRS took 35 %, since we were nonresident aliens. Willett had started a Southern Hemisphere Project. He wanted circulation statistics from the Southern Hemisphere and none existed. I worked for him and a guy called Mort Rubin.

Rubin was at MIT?

HvL: He was going for his Master's degree. He was overseer of this small Southern Hemisphere project. This project had gotten some funding for, a couple of women assistants, Buch, Mort Rubin, and me.

One day we were visited by the then assistant director from the South African Weather Bureau, who told us that he was starting a similar one in South Africa.

What is the name of this guy?

HvL: M.P.R. van Rooy. Dyed in the wool Afrikaner. A real gentleman. So he said to me, what are you going to do when you finish here? I said I am going back to Denmark. I hope to get a job in the Air Force Weather Service. He said, why don't you come and work on our project? You

know, this was before South Africa was recognized as a skunk among nations. Nobody really paid attention in the fifties to what they did there. The Afrikaner nationalists had come to power in '48. He said we will send you a contract. You can sign it in the embassy in Washington on your way back, or you can put it in a pocket and think it over. I stuck it in my pocket. We went home. It took us about a month to get home. We were on an old Italian liner from 1926 called Volcania, – it had three large smokestacks. We lived down in the hold for about a month. Our son Mikael was half a year old. A cabin in the hold cost \$460 for all three of us. We stopped in Azores on the way, in Portugal, in Casablanca, in Gibraltar, in Barcelona, in Palermo, in Genoa, in Cannes, then we got off in Napoli. We stayed in Napoli a couple of days, then we went to Rome for ten days. We took the train to Firenze, stayed in Firenze, then took the train to Milano. Finally we got on the train back to Denmark.

The following anecdote has nothing to do with that what we are doing here. It was an 18 hour train ride from Milano to Copenhagen. We got into a little compartment with Mikael, and Kirsten and I were lying down on the seats. I thought we could lie and sleep all the way. Then we heard two voices, one voice saying, "Da liegen nur zwei ganz ausgestreckt." Two Germans came in and chased us up to sit

all the way through Germany. They wanted a seat. Couldn't really blame them.

It sounds like your first paper "Aspects on circulation of the Southern Hemisphere" was written at MIT.

HvL: I wrote two papers at MIT. One was never published. One was based on the daily analyses. We had always been taught that the southern circulation is very zonal, with nothing like the anticyclonic polar outbreaks you get in the Northern Hemisphere. During the analyses I noticed there were lots of polar outbreaks in the Southern Hemisphere. The zonality of the mean comes of the constant movement of pressure systems eastward, so the mean maps look just like bulls eye's. I saw this big high form in the Scotia Sea, move up across Tristan da Cunha, and finally south of Cape Town, bringing very cold air all the way. I told Mort Rubin, "There are polar outbreaks. This one is associated with a high of at least 1025 mbar." He agreed, and I suggested that we write a note on it. And that was the first paper.

What happened to Rubin, do you know?

HvL: He got into the Weather Bureau, and became an administrator. In the IGY he was sent as the American representative to the Russian station Mirnyi. He was there for a year. When he came back he spent a year at the Scott Polar Institute and he

wrote some papers there, then he became an administrator again.

What kind of data were you using for these analyses?

HvL: We had all land and island stations, and the ships that sent in weather reports, but of course nobody sent in weather reports south of 40S. So there was a fairly big open space from the Falklands eastward. There was no Gough station. We had an occasional ship going across from South America to South Africa and back. We had all the ships in the trades. However, that was out of the westerlies. There were two one-year stations in Antarctica.

Willett got it all sent from WMO and other places. And the same thing happened then in South Africa, we had a very fine port officer in Cape Town. He got us data from all the whaling ships in summer.

Did you do your own quality control of these data?

HvL: When you have so few data, you are very careful about it. You study every aspect of the observation, pressure, pressure tendency, clouds, cloud heights, cloud types, temperature, dew point, wind, weather, every aspect is closely scrutinized. So you get the most out of every observation. Of course, if you have only one ship in thousands of miles without any other observations, you can't say for sure

whether its pressure is correct. There is nothing to compare with. You must take it at face value.

Where have we been? You were on the way on the train with the two German tourists.

HvL: I came back to the Weather Bureau in Denmark. Analyzing upper-air maps, 500 millibars, 700 millibars. Actually I invented a thermal wind machine to derive quickly a thermal wind. I got a little award for it. I think I still have the machine somewhere. I also learnt Fjørtoft's graphical method which was, I thought, very elegant. Where you take the vortices out and you have the basic flow field in which you advect the vortices. I liked that. It is probably almost as good as any 24 hour forecast you can get.

For two reasons we left. Not, because Denmark is not a fine country, which it is. But – while I was away, even when I was in the army, they had pushed others ahead of me. You know how it is in the civil service. If you are there long enough, you are promoted. That annoyed me. Also the general attitude of the Weather Bureau vexed me; I asked the then head of forecasting if I could take part in the forecasting. He said, no, you haven't got an education that would justify it.

I asked Kirsten if she would like to go to South Africa. She was all for it, she loves to travel. So, I went to the South African

Embassy in Copenhagen and said I have this contract; I'd like to sign it, if you could arrange our transportation to South Africa. Yes, I signed the contract, got to Southampton, onto the mail boat and sailed to South Africa. I got there on one of the coldest days they had ever had. -7°C in Pretoria, I think.

You were taking classes and working toward your degree. But you did not get a degree.

HvL: I wasn't really serious about a degree, I just wanted to learn something.

When I got to South Africa, I thought, I had to have something. So, I got a geography degree in South Africa.

I got into the Southern Hemisphere project. Part of my work was to forecast for shipping. It was for several shipping routes, first for the coast of southern Africa, from Angola all the way round to Mozambique. Then for the southeast trades for ships coming down from or going to the Northern Hemisphere, and also for the shipping routes to South America to somewhere west of Tristan da Cunha. And we forecast for the shipping routes in the Indian Ocean, one going to Indonesia, one going to Australia; and then in summer, on top of it, the whaling ship forecast, all the way from the Scotia Sea to the center of the Indian Ocean, south of about 45S.

These were 24 hour forecasts. We issued them twice a day, in the morning and in the late afternoon, and they were broadcast from Cape Town. It was hard work, because you started early in the morning, went home in the heat for a siesta, and ended about 6:00 or 7:00 in the evening and it was seven days a week, because there were only three of us who did it.

You have published several papers in Notos.

HvL: *Notos* was discontinued, after about 18 volumes.

You wrote a paper in 1956, which got a lot of notice, with many citations – Blocking Action in the Southern Hemisphere

HvL: It was part of my realization that there was blocking in the Southern Hemisphere. Nobody thought so, but there was blocking. So I defined the areas and the duration. I used Dan Rex's *Tellus* papers as an example.

They were also a few years earlier so you weren't really isolated....

HvL: In those days there were few journals and few papers, and you could manage to read everything. Nowadays I've given up.

Which journals were these?

HvL: *Tellus* was one. The American Meteorological Society Journal of Meteorology, what is now divided into many journals. It came out only four times a year.

That's where we published that first Southern Hemisphere paper. The Quarterly Journal of the Royal Meteorological Society, and the AMS Bulletin of course. It was different in those days from what is now, less social. Monthly Weather Review and Met Magazine were also around.

Meteorologische Zeitschrift had disappeared in a way, it came back later. But Archiv, we read. It was an Austrian journal, published in Vienna.

Which year are we now?

HvL: We are in 1956.

The paper on the blocking action in the Southern Hemisphere gave at least 44 citations.

HvL: That was the only one written until Kevin Trenberth wrote on the same theme and until Harald Lejenäs wrote a good paper too.

You did these papers at the same time you were serving as a forecaster?

HvL: Yes, and also at the same time as I analyzed historical Southern Hemisphere maps. We had to do everything, we had to go out to the telex room, tear off paper, take it in, plot the data, analyze it and issue the forecast. It was tough.

This was still pre-computer time.

HvL: Certainly in South Africa. It was going full strength in Princeton and other places.

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I am interested in this paper: 700 mb mean maps from the Southern Hemisphere -- and it was in Miscelania Geophysica ...

HvL: It was the tenth anniversary of the Angola Weather Service and we were invited to write a paper for it. I wrote that paper, because we were then preparing for the IGY. South Africa's duty was to analyze the Southern Hemisphere historical maps of the IGY, from 20 south to the South Pole. The Germans did it from 20 south to 20 north, but we overlapped by 5 degrees so we analyzed to 15S. There were very few upper-air observations in those days. So I devised a method to construct 500 mb maps and the beginning of that method is in that paper: how from surface observations together with a few upper observations you could build thickness maps. These you could then add to the 1000 mb height which you got from the sea level pressure. Later I wrote a paper on the whole method. The method, which was taken over by the Australians, consisted of anomalies before and after cold front, strong cold front, behind a low, in front of a low, in the middle of a low, in a high and so on.

About this time you actually went to Antarctica..

HvL: What then happened was that I had met Harry Wexler at MIT. He came to MIT to see our Southern Hemisphere pro-

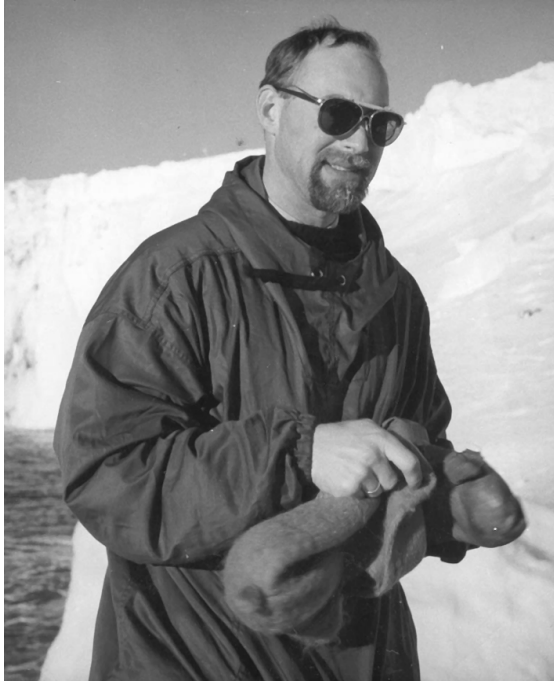
ject. He was very dynamic, a very friendly, very nice guy. He said, in the IGY we have to have an International Weather Center in the Antarctic. That was established in 1956, before the IGY, and called Little America III, near the old Little America Byrd stations from the late thirties and just after the Second World War. It was a truly international center, there were Australians, Russians, Argentinians, French, and American meteorologists. They invited the South African Weather Bureau to send someone too, but nobody wanted to go. They were used to the sunshine and warmth. In any case, I was keen to go. Since I could both forecast and analyze historical maps, there was no problem. I went. I came down there at the end of October and left at the end of March. That's when the summer activity takes place. We had to forecast for the flights down from New Zealand, and for the ships, for the snow trains, and all that.



Harry briefing colleagues at little America, Antarctica (1958).

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I had a great time there. We worked around the clock, as you can imagine. Ate four square meals a day, saw movies, and skied for hours and hours.



Harry in Antarctica 1958.

You stayed in South Africa until 1963.

HvL: There were several German scientists there, German meteorologists. The Afrikaners kept us all down. The system of Afrikaner nationalism totally dominated the country. The civil service was at least 95 % Afrikaans. And they were all promoted all the time. The Peter Principle was truly at work. There were two Englishmen, me, and about half a dozen Germans in the Weather Bureau. We were all kept at a low level. Most left after a while. I think, only one drunken German stayed, one died while there.

My contract expired in 1960 and I was going to leave. At that time we were really getting into analyzing the IGY. We got all the data after '58. IGY ran to the end of '58, and started in July '57. The data were streaming in and we were right in the middle of that. They asked me to stay. I said, OK, I'll take a contract for three more years.

The IGY was planned for a year, but lasted 18 months.

HvL: Yes, and then there was an extension into '59, but not full-scale. The IGY was planned because it was at a solar maximum. It was actually the first very large solar maximum since at least 1750.

This whole business was without computers for you and there were no satellites.

HvL: Absolutely. It was all hand-work. It took us a day to analyze a surface map, a day to make a thickness map and a day to add the two to get a final map. So, it took each of us three days to make a day. It was hard work. Two women plotted the data for us. We had to be careful, since they would plot anything as it was in the message.

This culminated in the 1964 paper, published in the Bulletin of the American Meteorological Society?

HvL: Taljaard and I wrote this paper on the reliability of the IGY analyses in the

Southern Hemisphere.

After this publication the type of publication changed and that is because you moved NCAR.

HvL: Yes, but first I applied to Australia. I knew Bill Gibbs who was director of the Australian Bureau of Meteorology. He had gotten his master's, while I was at MIT. He was very interested in our analyses there. We often talked about it. I met him again on my way to and from Little America in 1957/58. So I wrote to Bill, and he said, "Yes, send an application, we'd love to have you". I did so, I waited a little and got no response. I knew Phil Thompson was at a place in Boulder, Colorado. Aksel Wiin-Nielsen, whom I knew vaguely from Denmark, was also there. Thus, I wrote to Phil and got an immediate answer. Yes, come. I waited a little while, months actually, for an answer from Australia. My case was in the Public Service Commission, that's where all such things went, and finally I gave up. I accepted Phil's invitation. We went to Boulder in 1963.

Nowadays, when somebody gets a job, it seems you need to have a contract, they know this is going to be paid for and that is going to be paid for

HvL: Phil hated administration. Just come over here and bring your family and your furniture if you have any. That's it. I went into his little office in Cockerell Hall,

where he and Axel were sitting in a dense fog of cigarette smoke, on the first of July 1963. I asked Phil, "What do you want me to do?" He said, "Is there anything you'd like to do?" I said yes. "Just go ahead and do it", he said. I was put in the synoptic group, which did not yet exist because the chief, Chester Newton, didn't come until a month later.

Was Paul Julian there when you came?

HvL: Julian was in HAO and came to NCAR to the synoptic group, along with me, Henry van de Boogaard, Jim Fankhauser, and Chester. It was only a question of a month. Warren Washington and Akira Kasahara came at about the same time.

Did you know that Chester would be the boss of that group?

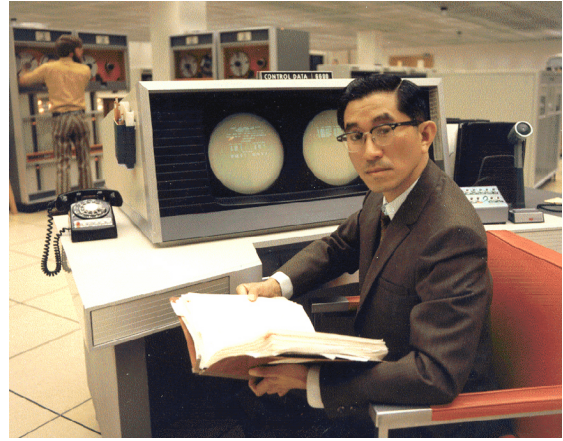
HvL: I had no idea who Chester Newton was. When he came and introduced himself to me he said, "Oh, you are the guy that writes two-page papers."

I think Roy Jenne came in 1966/67. He knew to handle computers, he had been in the Air Force. He had done a lot of data handling.

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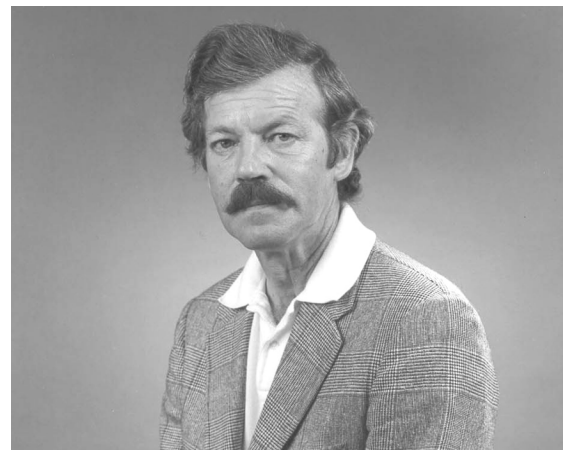
Will Kellogg, director of NCAR's laboratory for atmospheric research about the time Harry joined the synoptic meteorology group. The synoptic meteorology group was a part of LAS. (ca. 1963).



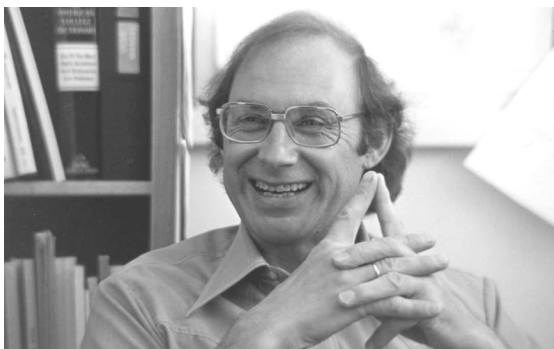
Akira Kasahara (ca. 1967) at the console of the NCAR CDC 6600 computer. The 6600 had a clock speed of 10 MHz and a memory of 64 KBytes and was the fastest and biggest computer devoted to meteorological studies at the time.



Chester Newton (ca. 1967), head of the synoptic meteorology section at NCAR. Harry was an original and long-time member of the group.



Phil Thompson, associate director of NCAR about the time Harry arrived in Boulder (ca. 1963).



Harry in his NCAR office.



Warren Washington checking output of the NCAR general circulation model (ca. 1967).

What did it mean for you? Did you consider this as a new opportunity?

HvL: Absolutely. We could do a hundred times more. When you calculated derived data from maps by the old method, it took several months. Now it could be done within a few seconds when the program was written and when the grid points were read. It was a totally new world.

Did you do any programming yourself?

HvL: Well, I tried, but I never really did. Not like Madden, for example. The programs were ready for everything I wanted to do: geostrophic wind analysis, standard deviations, harmonic analysis, and so on.

Somebody had to write the programs. Was each scientist assigned a programmer in those days?

HvL: Not really. I cannot say for sure, because I worked closely with Roy Jenne for many years. He and Will Spangler, and sometimes Dennis Joseph, would do the programming. I don't know what other scientists had in terms of programmers. I closely associated with these guys through research, computation, and analysis.

We see now from your list of publications that something else happened, namely satellites. In 1966.

HvL: That was a brief, peripheral thing.

You saw for the first time images of cloud

cover, it must have been very impressive, wasn't it?

HvL: Yes. But I remember a paper by Harry Wexler, in the Bulletin when he had taken a weather map and he had drawn a cloud cover as he thought it should look for that weather map and then he compared with the satellite's clouds for the same day, and it looked almost the same. The cloud distribution around a low, along a front in a high and in the trades and so on.

Weren't weather maps produced with the help of satellite images?

HvL: I am not knocking the satellites. On the contrary, I was admiring Harry Wexler's skill. Satellite images are good to have, absolutely. In the Southern Hemisphere nowadays they are indispensable. For hurricane forecasting they are indispensable.

You never really much worked with satellites.

HvL: No, I wrote a paper, actually two. I did it with Aylmer Thompson from Texas A&M. We talked about him earlier. Just to see how much I could improve the analysis of given days. The position of things were nailed down by the satellites.

Thereafter you had left it to the synopticians and the operational meteorologists

HvL: I never used satellites for analysis again. Nowadays, a lot of data you get are

from satellites, vertical soundings, winds, etc.. That's different.

Then in the late sixties or so you did a comprehensive summary of climate in the Southern Hemisphere.

HvL: Taljaard came, and we began a climatological analysis. We started with the IGY maps. Then we collected data, made the atlases and derived data. All of it was aimed at making a monograph, Roy came in the middle of it. We had the grid points read by Crutcher's people in Asheville. We asked Roy to check for time and hydrostatic consistency in the data. To quote George Platzman's words, "If it couldn't be accurate it could at least be precise." Roy did this marvelously. He did a very good job.

We analyzed the height and pressure maps, the temperature maps, and the dewpoint temperature maps, and then all the derived maps were computed by Roy. They were sent to Asheville and gridded in Asheville. The numbers were punched on cards.

The maps were all hand drawn. I still have the sheets. We plotted everything. We drew every line on those maps. This was before machine plotting.

The computer was just used to provide consistency and do the derivations.

HvL: In the atlas all grid points are printed on the right hand side of a map. And zonal

averages. These were very useful maps.

The maps were printed so beautifully, in color. They were drafted. We did the analyses, then they were drafted in black ink in Asheville, then they were printed.

This explains that then Hal Crutcher appears in your publication list. This was the cooperation with Asheville.

HvL: Yes. We made enough volumes so each of us could have our name first on one of them.

In these days, there was also that semiannual wave paper.

HvL: Werner Schwerdtfeger, a German meteorologist, left for Argentina after the war. After Argentina he came to Wisconsin together with other Germans like Lettau and Wahl. He was a very fine synoptician, he had been forecasting in Germany during the war. I think he went out in submarines too and took observations in the Atlantic. In any case, he had got to Argentina, and being interested in climate as well, he made some very good climate analyses. But he didn't discover the semiannual oscillation. It was in the first analyses by Meinardus analyzing the maps of the polar year 1902–04. Meinardus and Mecking published atlases and papers. From the derived data like movements of cyclones Meinardus saw the semiannual component in the winds and cyclone movement. That

was the first step. Then Reuter in Germany wrote a thesis in the thirties, where he analyzed all pressure data available in the Southern Hemisphere and he found a large semiannual component in the few stations he had. Then Wahl made an even a better analysis 1943 in his Diplomarbeit or thesis.

Schwerdtfeger took off from there. He had already shown the rough outline of the semiannual oscillation. He made several very good papers on the semiannual oscillation. He did some very detailed climatic analyses of ship data in the Drake Passage where it is very strong. He wrote a whole book and a smaller book on the Antarctic Peninsula where it is all very well described, plus some papers in German and some in English. I got to it, when we did the IGY analyses, I made some time sections and I noticed the thing. I hadn't read any of the old papers. Later I found Schwerdtfeger's work, and there I found Meinardus and all the other guys. They are all referenced in the 1967 paper.

The 1967 paper won the NCAR publication prize. It was the first paper to receive such an award.

HvL: That's right. \$600. I got a new engine for my jeep.

And it had important implications for the development of meteorology in the United States because of certain people who did not get it and moved...

HvL: Nomina odiosa sunt, as we said in Rome. Names are odious.

Did you know that your paper might win that award and so you just....

HvL: I didn't have the slightest idea. Chester had nominated it. I know now why he did it. I was making a heat balance study. I wanted to explain the semiannual oscillation physically, and then how it worked dawned upon me. I was enthusiastic, and Chester and I were having coffee, and there was a blackboard, and I explained it to him. I could see, if you know Donald Duck's cousin, the bulb light up on top of Chester. From that point on he thought he would nominate it, apparently. But it had a very funny fate. Is the name Clarence Palmer known to you?

That tropical meteorologist was a very fine meteorologist, but as alcoholic as you could be. He was out at UCLA. I submitted the paper in late 1966. I heard nothing from them and I was a greenhorn in those days, I should written in and said what the hell is going on. After eight months it was too much for me. I wrote to AMS "Why haven't I gotten the reviews of my paper?" They called Clarence Palmer. He must have been sober at the time. He wrote a wonderful letter saying that this is the best paper he had ever read. Publish it immediately without changes. So it came out without any changes.

That was Clarence Palmer. He died not long after this. He was the one who wrote the Southern Hemisphere chapter in the Handbook of Meteorology in 1944, a very good chapter, considering how little was known at that time. He also wrote a very nice paper on solar influence on low latitude pressures. He had to test the results against a random series. For the random series he took the thick telephone directory in Los Angeles and took the last number of all the telephone numbers.

Your publication list in the sixties reveals something else, namely the first paper with Karin Labitzke in 1965.

HvL: Karin Labitzke had been brought over here by Walter Roberts, who was very keen on solar influence on weather and climate. He went to Richard Scherhag who was professor in Berlin at Freie Universität, asking if he had any good doctoral students interested in the stratosphere who would be willing to come to NCAR for 18 or 12 months. He needed help in analyzing solar influences. Karin had just gotten her PhD and she accepted. When she came she must have been about 28 years old.

Nobody could speak German. She spoke very little, broken English. I was the only one who could speak German. Her husband was with her, too. We invited them home, played poker on Saturday nights, etc. I got interested in her stuff on mid-

winter warmings. She had done some work on that while in Scherhag's outfit. I said to her there is not much we can do about the Southern Hemisphere and its stratosphere, but let us look at what little there is. So we did and wrote that paper.

This is, important and interesting, because you really continued to work with her for many, many years. You produced more than 30 many papers with her. You worked with her for almost 40 years.

HvL: Yes, we still work together a little. We are working on a paper right now. That will be the last one then. Well, I will be 80 next year (2005) for heaven's sake.

Let us get to NCAR. These were the golden times. At NCAR you were really free to do whatever you wanted to do.

HvL: Yes, but in the 70's it started changing. It began in 1972 with that

Rol Madden: ...Joint Evaluation Committee Report. NCAR got re-organized because of the Joint Evaluation Report and Harry and I were put into a climate section. After the first meeting of the climate section everything seemed OK to me but Harry said "We have to get out of this group".

Then we moved to a different group of which Akira Kasahara was in charge.

It turned out Harry's insight was true because we flourished in this new group

much better than we would have in the other.

HvL: Let us go back to an old French proverb which I cannot say in French: The more it changes, the more it remains the same. For Madden and me it has been like that. Not for us alone, but for other people, too. There was an urge to change things every now and then. The people reorganized, reorganized and continued as before.

Does it mean that in 1972 the Joint Evaluation business had no implication, no impact?

HvL: Oh yes, it did. First of all we got a terrible hierarchical system with scientists 1,2,3, senior; and up or out promotion system. And secondly, two categories of scientists, where we usually had only one before. Associate scientists and ordinary scientists. The whole thing became structured.

You were not affected by this change, because you were a senior scientist.

HvL: I became a senior scientist in 1976. Madden became one too. You cannot fire a senior scientist, if he doesn't fit in, as long as he publishes and doesn't commit illegal acts. Unless you abolish the whole group he is in.

We are now at the end of 1972. The next paper we should address is the Jenne, Labitzke piece, on zonal harmonic stand-

ing waves, which got 136 quotes. How did you think of displaying the waves in this way?

HvL: People still hadn't realized that the Southern Hemisphere was not just a zonal circulation. Through synoptic work one notices features such as blocking, the meridional movement of lows and highs and so on. There must be something that steers these things, if that's the right way of putting it. I decided to look, I already knew the difference between the South Atlantic and the Pacific Ocean in terms of mean sea-surface temperature and pressure. So I thought let me see if there are any quasi-stationary waves. Wave one was obvious, but then wave number 3 popped up which I hadn't expected to that extent.

Well, I think the 1973 paper on zonal harmonic standing waves in the Northern Hemisphere which appeared in the Journal of Geophysical Research had a big impact both on observationalists and theoreticians.

HvL: Do you really think so? I did some work with Jill Williams in 1976/1977 which showed the role of advection in climate variability and which actually was straightforward. Francis Bretherton once came in, looked over my shoulder and I showed him how the changes in wave number 3 affected the temperature trends in the Northern Hemisphere. He said, "I

never thought of that but it is very simple minded." I took this as a compliment.

Harry, we forgot to speak about the book in 1972.

HvL: I rate that as one of my few real accomplishments even if it is not the first book on the Southern Hemisphere. In 1938 Meinardus wrote one on the Antarctic, which took in a lot of the Southern Hemisphere too. It was very good for that time. You have always to judge things in their own period.

How does an AMS Meteorological Monograph come about? Do the authors approach AMS ...

HvL: Jan and I had done all the work with the data, and also written it up in papers. It was nice to get as complete a picture as one could in those days. So, we decided to ask the AMS to print the Monograph. I looked around for authors for those chapters that I did not write myself. Jan was an obvious one. It's a very fine synoptic chapter that he wrote. Never been superseded. Obviously Takashi Sasamori would be good, he was a very knowledgeable person on radiation. He wrote a very good chapter with Julius London and Doug Hoyt. Chester was the obvious candidate for the general circulation. I had first asked Paul Julian to write about the stratosphere. He said one couldn't, there was not enough data. So I said to Karin Labitzke, let's take

a look at it and see if we can write a chapter together. It is a very modest chapter. We didn't have many data but there is still a lot of information in it.

So I collected these guys and I swung the whip over them so that it would not take too many years. Then we got it out. I am happy about it. It was a good monograph for the time.

There is a lot more in the new one, which came out 25 years later, obviously since there are a lot more new data since then.

You are not a member of the AMS. Could you just say that why.

HvL: After we had done all these analyses, published all the atlases, papers and the book, Chester nominated me for fellow of the AMS. He said, now it is time, its going to be easy. Three years in a row I was voted down. So I wrote to Spengler and said I want to leave the AMS.

In your publications list a few more new names come up. One is a young fellow with the name Madden, then Jill Williams and Jeff Rogers. Do you mind saying something about these papers?

HvL: Jill – I wasn't really her advisor, she got a PhD with Roger Barry. But I was sort of semi-advisor together with Warren Washington. She is very clever, good at programming too. I asked her to help me

with some analyses, so after that we wrote a series of papers together.

Jeff Rogers was my first graduate student in the Geography Department at the University of Colorado. Roger Barry was Professor in geography. A climatologist, he has written several synoptic climatological books. He asked me to join the faculty as adjunct professor, and take some of their graduate students. I had a good topic, the North Atlantic Oscillation, so he referred two students to me: Jeffrey Rogers for his PhD, and Jerry Meehl for his Master's degree. We started on the NAO, although lots had already been written on that long before me.

These papers with Jill looked mostly at the Atlantic and temperature over Europe. So that was sort of the beginning on NAO work.

HvL: That's how I got into it. It was on the Northern Hemisphere as a whole, because we looked also at the importance of changes in wavenumber three. Out of that came the association between long waves and climate variability.

Would you say that the North Atlantic Oscillation was the first oscillation you had really worked intensely with?

HvL: I knew about the Southern Oscillation. We talked about it in South Africa and Willett was very keen on it. Mort Rubin wrote a paper on it when we were

together at MIT. But I had been turned off the SO by Robert Montgomery, who in 1938 went through Walker's correlations and showed that many had fallen by the roadside and even changed sign. So I was not too enamored by the Southern Oscillation at that time.

You thought maybe it was a statistical artifact?

HvL: No, I didn't. We knew the effect of the 1957 classical warm event. We had a gigantic and classical Warm Event. I read a paper written by an American and a Japanese. Do you know that paper of 1958 about the warming and the abnormal equatorial rainfall and SST? A very nice paper. So it was in the back on my mind. Also, I'd gotten a letter from Jacob Bjerknes. He had gotten interested in the Southern Oscillation. He asked if I could get him some pressures and winds from the South Pacific Ocean. I gave what I had from the IGY. All geostrophic winds of course. It was all lying there, waiting.

This work with Jeff Rogers about the North Atlantic Oscillation is the most quoted paper of yours, more than 350. How did it come about that you really concentrated on that as opposed to the Southern Oscillation which was much closer to you in a sense.

HvL: Don't forget I was born at one end of the NAO and lived for 26 years at its re-

ceiving end. Some of the correlations with Copenhagen are the highest. In any case, also historically it interested me. I dug out all the old stuff.

I knew about the Southern Oscillation and its possible use in inter-seasonal forecasting, and I thought the North Atlantic Oscillation might have some potential as well. But as far as I can see, it has no persistence in the same sense as the Southern Oscillation. You can't really use it in long range forecasting. There are also no clear precursors. Therefore, I gave up on that. Also, my interest in the NAO goes way back to my time with Lysgaard, who had done lot of work on the condition in the North Atlantic although he had never published much on it. He examined the North Atlantic Ocean, to see whether there would be characteristic sequences of events in fall, winter and spring. He found nothing of value.

Would you say that you in a sense had rediscovered the NAO? At that time only very few people spoke about the NAO.

HvL: No, Loewe had dealt with it in the 1940's–50's. Much later, I encouraged Jim Hurrell to work on it. He sent a good paper to "Science". Science had sent reviews among others to me but to also some Dutch meteorologists. They wrote back that we know all about the NAO. There is nothing new you can tell us about that.

This is in the nineties.

HvL: Yes, but this just shows you, even then, people thought they knew all about it. the Dutch said yes when the low is deep the we get strong westerly winds, cloudiness, and rain. We know all about it, but they didn't know all about it.

Where does the name NAO come from?

HvL: From Sir Gilbert Walker. It's a pair – the North Atlantic and Southern Oscillations. The Pacific Oscillation is basically part of the Southern Oscillation.

What do you say now that your adopted daughter has received a new name, namely that it was rediscovered as Arctic Oscillation.

HvL: There was a paper by Clara Deser who found a 0.96 correlation, which is very high in meteorology between the Arctic Oscillation and the NAO, and you can take it from there.

How would you then understand that both – the Antarctic and the Arctic oscillation have received very much attention?

HvL: If I really get into this I will have to insult people that I like. But let me just say – just as Clinton's people said, "it is the economy, stupid" – it is the waves, stupid. Both, in the Northern and in the Southern Hemisphere. The North Atlantic Oscillation is part of a long wave pattern and you cannot disregard that.

So few people take the trouble to go back and see what has been done before on a topic that they deal with. They would have found, for example, the older Defant in 1925, wrote an excellent paper on the NAO, but even now, they don't refer to it.

There is a story with a missing minus sign?

HvL: John Walsh and I were commissioned to write a report on "Climate change in the Arctic". I had seen the these sort of abrupt changes – step functions in 1920 and in 1976. I had got the idea that you get this very sudden change of climate within two/three years and you are in another mode. So I was working with Greenland stations. There was one in the far northeast, but there were no other stations near it. I noticed that that station suddenly had a jump in its mean temperature. I wrote it down and sent it to John. He wrote back and said, Harry, look at that decade in the World Weather Records, they forgot to put the minus sign in front of the temperatures. That's terrible. Such is life.

This paper about the SO in 1981 is also one of the papers which had been quoted very often, namely 180 times. The Southern Oscillation. Part I.

HvL: Madden's contribution to it is very important, because he showed through co-spectrum analysis from one period to another that the correlations change, the

frequencies change in importance. He was the one that suggested dividing it into four periods and see where you always have the same correlation, and where it fluctuated from period to period. Most of what is good in that paper is Madden's contribution.

Who was responsible for the first line of that paper: "the Southern Oscillation needs little introduction"?

HvL: I was. To me it needed little introduction, but to a lot of people apparently it did.

The real revival came with Rasmussen and Carpenter's paper in 1982. This is one of the best ever written on the SO. That one really got people thinking. It was well written and methodologically sound. I would say that really rekindled the interest.

It is remarkable that you actually have this series on the Southern Oscillation. Part I to part IX, which began from 1981.

Are you aware of anybody else who made a series of ten papers? Over 25 years.

HvL: Sure. They may not have numbered it. I numbered them.



Members of NCAR's synoptic meteorology group, Paul Julian, Roland Madden, Dennis Shea, Chester Newton (left to right) listening attentively to Harry (ca. 1980).

Part II also was received very well with more than 70 references. That was with Jeff Rogers.

HvL: He did a good job. And he continued with the topic from then on. Much of what he has done comes out the work that we did together. But, of course, he has done it independently. Some very good papers.

Is it about that time or was it even earlier that you started to interact with Christos Zerefos?

HvL: Zerefos had been at NCAR as a visitor. We had been talking together about various things. In Greece they were interested in the stratosphere. So he just said

why don't you come to Greece for a while, which of course I jumped at. We had a nice half year in Athens in 1981, and came back to Thessaloniki in 1993. This latter stay was not quite that successful because he didn't have time when I was in. I had only one graduate student. We wrote one small paper

published in Argentina. and that is totally forgotten.

Christos Repapis was head of the group in Athens. Actually, there was an old Greek in his 90's they treated as head of the department. That is out of the Greek attitude toward old men. Repapis was the real head, and he had several graduate students. We had a lot of interaction.

During these years you also regularly went to Berlin.

HvL: Every year, sometimes several times a year. Roy Jenne and I got Karin to collect the stratospheric data, and with her we published a stratospheric climatology in the *Meteorologische Abhandlungen*. That was later improved by Steve Pawson and his collaborators. A few years ago they

collected all the new data. It is an excellent work.



Southern Oscillation conference in Dehli, Roy Jenne, Harry (ca. 1985).

You wrote a book chapter with Taljaard in 1984.

HvL: Volume 15 of the World Survey of Climatology. I also edited that volume. That is another thing which I like. It is 760 pages or so, and really gives a thorough review of the climate of the oceans.

It was sold out quickly, went like hot cakes. The only book I ever earned money on. Although in terms of hourly pay it was very little.

When I was working in Berlin in 1974, I got a call from Helmut Landsberg, who was the chief-editor of the World Survey of Climatology series. He said to me, this is terrible, volume XV, which is on the climate on the oceans, is in bad shape. He said, "First I gave it to DeRuyter, a Dutch oceanographer, and he died. Then I gave it to a Danish oceanographer, and he gave up. Would you mind taking it over?" I

thought a little about it and said "OK, I'll do it." He sent back all that had been written up to that time. I was not satisfied with a lot of it. The one on South Pacific Ocean had been written by a well known climatologist. A junior in high school could have written it. It was terrible. I got John Zillman and Neil Streten to write it. They did a wonderful job. Brian Tucker had written the chapter on the North Atlantic Ocean. He wrote back and said throw it away, it is too old now. So, I called Roger Barry and said, listen, Brian Tucker has written a very nice chapter on the North Atlantic but he doesn't want it published. Would you mind helping him revising it. Barry did that. It is Tucker and Barry now. It is a good chapter. The worst problem I had was with Colin Ramage. He was supposed to write about the climate of the Indian Ocean, but he had written only about the tropics, and two or three pages about the parts south of the Equator. I said to him, Colin you got to extend it to the Antarctic. No, he didn't want to. Ok I said, we are making two chapters. North of the axis of the southern subtropical high by Ramage, then Jan Taljaard and I will work from 35 South to the Antarctic. So we did. A nice German, Höflich – the name itself says it all, had written a long chapter on the South Atlantic Ocean, 100 and some pages, a whole book, in German. I said to him, we'll translate it. Since I could not find anybody

to do it, I did it myself. It is a very thorough chapter. That book cost me so many thousands hours of work, you can't imagine.

The Japanese chapter I almost gave up on. I had to rewrite the Japanese English into English. The Japanese is still shining through. On top of it, the chapter on Iceland which is in there should have been in the *Survey of Climatology* that Svinn Orvig and Vowinckel wrote. It got submitted too late. They had just published that Volume, so I agreed to put it in my book.

It is interesting when you say how many hours it took you, because it came out in 1984 and I just counted 7 publications in 1984.

HvL: That was routine stuff. I worked at home on that thing every day for eight or nine years. I got the nicest letter from Landsberg when it came out.

Now we are in the mid of the 1980s, right? There were the papers with Kingtse Mo.

HvL: Yes, we wrote two papers, one on trends and the other on interannual variability in the SH. Also a couple of WMO reports. I really enjoyed working with Kingtse, she is very skillful with data analysis and computing, and has good ideas.

You started to do things with people like von Storch and Kiladis. This paper was exceptional since it was the first time that you were engaged in modeling of climate.

HvL: Yes. I liked that. I am sure, it doesn't show anything startling. We wanted to find features that might be important in the development of a warm event. It was nice to see that a model could reproduce the observations.

The next big thing is in 1987 with Labitzke.

HvL: She had been fiddling with some data and she saw a solar influence in the winter stratosphere in the Northern Hemisphere if she divided the data into the phases of the QBO, east and west phase. I said, why don't you write a note for GRL. She sent it in. The editor sent it back: A solar influence doesn't exist. I went to Ray Roble, who was a friend of this editor and said to him that this is very interesting stuff. It has high correlations and there might be physical links to equatorial stratospheric dynamics. So Karin got it printed. After that we just continued working with probable solar effects in the stratosphere.

We got a wonderful review from Jim Holton who said these correlations are too high to ignore. Now it is referred to quite often.

Also that had meant that you came in contact with a very different community. You can see that on the type of journals you were sending this. I guess there were also very different people.

HvL: Karin and I wrote a book together that came out in 1999. That was a book on the stratosphere for general audiences. There were also several review articles on solar relationships with Karin.

In a sense Karin started other people working on the sun, she reintroduced the interest in solar climate relationships. It was that little 1987 note. It is rolling along so much now that some people have forgotten how it started.

Back to NCAR, you said before you stepped down from the position as scientist IV in 1991.

HvL: I was 65 years old. It was about time.

But you continued. You have been on this part-time position from 1991 at NCAR until 2000

HvL: They gave me an office and \$12,000 a year. But personnel had decided that people like me, Holland and other retirees shouldn't get \$12,000 a year, but should get \$22,000. So they gave us \$22,000 a year. Then one year, after ten years or so, a year, when Karin and I had published a book, and I had three good papers out and 119 first author references, and the Divi-

sion Director needed \$42,000 dollars to remodel his office, so he took my \$22,000 away

Then you moved to CoRA, Colorado Research Associates..

HvL: I really didn't know anything about Colorado Research Associates at first. Ralph Milliff, who is a fine scientist, very thorough, insightful, and reliable in what he does, was not treated very well by the same NCAR Division Director, because he didn't want to take part in the team modeling effort, so he moved to CoRA. The problem with CoRA is you have to bring your own money. It has no funding. So everybody there writes proposals like mad and I wasted almost two years, writing two proposals. But the spirit and working conditions at CoRA are phenomenal.

Finally Jerry Meehl wanted to work with me. He persuaded Warren Washington to employ me as an independent consultant for \$1,000 a month. Jerry and I have worked together since. For which I owe Warren gratitude. Not that I need the money, but it is the principle: if you are productive and still useful you should also get some remuneration, not necessarily \$100,000 a year, but \$12,000 seems fair.

It seems that you have not been engaged in what people call anthropogenic climate change research.

HvL: No, I haven't....

Interview with Harry van Loon



Myanna Larsen, Harry and Kirsten's son Mikael van Loon, Kirsten van Loon, Harry and Kirsten's daughter-in law, Dana, and Harry (left to right) at Harry's retirement symposium, NCAR 1996.



Jin Song von Storch (back to camera), Harry, Will Kellogg, Roger Barry (back to camera), Karen Labitzke, Byron Boville, Roy Jenne, Bob Chervin (left to right) at the van Loon symposium NCAR 1996.



AMS Award presentation, Jill Williams and Harry, behind Tim Hoar at the van Loon symposium NCAR 1996.

Did you have in your career ever communicated with the public, with the media or with policy makers?

HvL: Not much. I remember an occasion when I was called by Reader's Digest some years ago. They were going to have an issue on sun and climate. He asked me what do you think of anthropogenic global warming. I said, you know, if you had called me twenty years ago, you would've asked me what do you think of global cooling. He said yes, in those days I wrote a book called "The Cooling". So I said, "now you can write one called 'The Warming' and you will be just as right. Climate changes on all time scales. Because the change happens on our watch doesn't necessarily mean we are responsible.

What do you think what is the role of people who are called very often by the media and who are actually influencing the public opinion. You have not participated in this debate with the public. On the other hand the public deserves some, needs to have a few people like you.

HvL: The public deserves reliable, proper information on politics, science, health, etc., but it is not so that everybody, who has a strong faith in an issue is necessarily the one to give the public information. He or she may be very biased because of their conviction or faith in this issue. But how

do you sort them out? They have to be able to say "I don't know."

How would you do that? How would we do that? You have the opinion some people do better than others. How do you judge that?

HvL: I refer to Plato who said, don't ever give power to those who wish power. Give it to those who don't want power. Because they will do their duty and then they will relinquish the power. Also, don't let those who are keen to be in the limelight be the ones who tell the public what *they* believe is going on.



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Unfortunately, this list of publications may be incomplete.

