

**American Meteorological Society
University Corporation for Atmospheric Research**

TAPE RECORDED INTERVIEW PROJECT

Interview of Lester Machta

31 October 1993

Interviewer: Julius London

[Twelve minutes at the beginning of the interview were not recorded.]

London: Could you name some of those colleagues?

Machta: Yes. The one who helped me out most was a person I'm perpetually indebted to, and that was Harry Wexler, who was one of the instructors at Grand Rapids and for a while at Chanute Field. Will Kellogg, who was formerly at NCAR and may still be at NCAR, was another.

London: How about John Leighly?

Machta: Oh, yes, John Leighly was a climatologist. I certainly do remember him. He was also a civilian for most of the time that I was a civilian, and taught some climatology. The students I don't think appreciated his scholarliness quite as much as they should have. He should have been in an academic setting.

London: Well, of course he was at Berkeley, both before and after Grand Rapids.

Machta: I may also point out that while I was involved with the forecasting school, the Army Air Force also had an observers' school at Chanute Field which turned out as many or more students. The Army had a system whereby the technician-type work was done by observers and only the forecasting was done by Chanute Field graduates.

London: This was meteorological observing.

Machta: Meteorological observing, that's correct.

London: Do you remember who was in charge of the program at Chanute Field when you first came there?

Machta: Yes. There was then-Major McNeal, who became a full colonel thereafter. Colonel McNeal was a person I found very difficult to get along with. He came from a school which believed that anything that was ordered down from above must be true, and you can never question it and in teaching, this is not always the case. I must say that I had very great difficulties as did every other instructor there because he was a very difficult man to get along with.

London: Weather forecasting by the rules?

Machta: By the rules. Now, on the other hand, we did have another civilian who did become an officer and in charge of the school. George Taylor--who wrote a book on weather forecasting--

London: **Aeronautical Meteorology.**

Machta: He was a very good man, I may say. I got along fine with him and when he was in charge, things went quite well.

London: Is there anything else that you wanted to say about the program at Chanute Field, and how the training program itself started? There were some people who were involved with the training program. First of all, meteorological training was not just at Chanute Field during this time, but it was at a number of universities.

Machta: That is correct. Before the war, there were a few academic institutions like NYU, MIT, University of Chicago, UCLA which granted degrees in meteorology. The Air Force used virtually only the forecasters coming out of Chanute Field. But after the war broke out, there were many other institutions providing an officer corps to go into the Air Force.

I can make a comment about why there were so many. It is pretty evident if you look at the statistics that during World War II we trained far too many officers in meteorology. The reason for this, I believe, is the fact that this was a deliberate policy on the part of the United States government to keep some very high I.Q. people out of the firing lines. It was not only done by over-training people in meteorology but in intelligence and in languages and probably in some other fields as well. The person I think had a major hand in some of these decisions was Carl-Gustaf Rossby of Sweden, who was an advisor to many military people in the United States at that time.

London: At that time, he was also a professor of meteorology at the University of Chicago.

Machta: That's correct. Even before then, he was with the Weather Bureau. After Harry Wexler passed away, I looked at Harry's papers and I found one rather unique one

which I think was dated 1925--he was a director of research at that time under Reichelderfer, and he sent a letter to him--

London: By "he" you mean--

Machta: Carl-Gustaf Rossby was now writing this letter and describing to the Chief of the Weather Bureau, saying, "You've given me \$25,000 to conduct research. I've spent that money; I now want to leave and do something else." Grants for \$25,000 for research for the entire year for the Weather Bureau.

London: Of course, at that time Rossby was at MIT and Harry Wexler was one of his students, wasn't he?

Machta: Yes. Rossby had a profound influence on meteorology all over the world for many years before, during and slightly after World War II.

London: You had mentioned in 1946 or so you went back to MIT to continue your graduate work. You got your degree when?

Machta: In two years, in 1946-48. I don't think anyone these days is able to get a doctor's degree in two years. But after World War II I think academic institutions made life easier for veteran graduate students.

London: You did a thesis there with whom?

Machta: I did my thesis under Professor Austin--well, it really should have been under Professor Starr, but it was not. Victor Starr, who since passed away, and who I liked very much. In fact, all who had contact with him really loved him very dearly.

My thesis there was on analyzing ageostrophic winds, winds out of balance with the pressure gradient and the Coriolis force. One of the things I discovered, in fact the most profound thing, if you can call it profound, was the fact that when people draw upper air weather maps, they draw the isolines more west to east than the winds would indicate so that there is an ageostrophic component in the observed wind. This misanalysis still goes on. The upper air isolines are too west to east.

London: Of course, the ageostrophic wind was determined largely by the pressure field rather than just the wind field. Then, as you have already said, after you were graduated from MIT, you went to the Weather Bureau to work. This was then the start of a very longtime professional career with the what was then the Weather Bureau, and later became what?

Machta: It became ESSA, then became NOAA.

London: It is still NOAA after many, many years. It's interesting that it hasn't changed its name despite the fact that it's had many new directors in recent years.

When you went to the Weather Bureau at that time--NOAA now--what was your role there?

Machta: Backtracking just a bit, the history of meteorology is one in which new arenas open as a result of the new use of weather data. The obvious uses are that people want to know whether to take an umbrella for the next day, and things of that sort. And agriculture was also one of the earliest applications of meteorological data. But then when aviation expanded and became commercial, a big emphasis was put on forecasting for aircraft. After World War II, what was expanding was atomic energy. Although we couldn't talk about it at the time, when Harry Wexler brought me in to be in charge of the unit he called "Special Project Section" (a meaningless title), it was to work in an area involved in atomic energy.

London: Harry Wexler had what kind of position at that time?

Machta: Harry Wexler at that time was director of research for the Weather Bureau. And--I can now talk about it as long as we are doing it--in the late 1940s, or mid-forties, I should say, 1945-47--there was a concern as to whether or not the Soviet Union would or would not be able to develop the atomic bomb, and if they did, how would we know this would be the case? A decision was made to assign this problem to the Air Force, and an Office of Atomic Energy was set up by the Air Force called "AFOAT-1."

Harry Wexler at that time was extremely powerful in military advisory circles. He knew, for example, Colonel Ben Holzman, who was in the Pentagon--they were very close friends. Colonel Holzman was also heavily involved in atomic energy matters, so when AFOAT-1, among its different charges, was going to try to detect radioactivity in the air, they realized that winds would carry that radioactivity and AFOAT-1 ought to have some knowledge of such air transport. So they gave Harry Wexler a relatively small amount of money to set up a unit in the Weather Bureau to study winds primarily coming out of the Soviet Union. Harry Wexler needed people, and it was for that job that he brought me in.

The other people that he brought in were for the most part students based at the University of Chicago who he knew and to some had granted Masters' degrees: Ken Nagler, Les Hubert, Lyle Harris, Bob List.

London: And at that time, can you talk about what your general duties were without getting specific?

Machta: That part of the work from 1948 to about 1950 I can speak of freely. What we were trying to find out was how air would move over the Soviet Union carrying radioactive debris if an atomic detonation took place. One thing we had to figure out was what meteorological data were available there. The Air Force intercepted Russian data on an open basis and the Weather Bureau collected data, but in the course of communications, many errors were created. So we looked very carefully at all of the weather data and decided that the place we were most likely to pick up radioactivity--if there were a test--was on the east coast of Asia, because of the prevailing west to east winds. And the Air Force fortunately at that time had Loon Charlie weather reconnaissance flights, which flew from Alaska to Tokyo, Japan. What they did was put boxes on the aircraft to collect particulate matter on filter papers. I have written this up in a paper published in 1992.

London: Right. Incidentally, do you have any more comments about that paper now? It's all in the **AMS Bulletin**, in 1992. Anything more about that?

Machta: The main thing I've been doing since I published that paper is to find out why a certain person or why the Tracerlab, which is the company that did the radiochemical analysis, alleged that at that time that they knew the proper date of the Russian atomic test whereas AFOAT-1 told us that no one really knew it. My published paper indicates, incidentally, that we were given an improper date and that's why we made a poor forecast. Only many years later we learned what the proper date was, and I looked back to see what our forecast would have been with the correct date of explosion, and it turned out that we would have done quite well.

London: You don't think it was a particular person's mistake for a particular reason?

Machta: It was a mistake, but it was an unavoidable mistake by Tracerlab in the sense that at that time no one knew enough about the way in which radionuclides were formed in the fission process.

London: Now you also did some work in trying to trace through the first or early Chinese explosions.

Machta: What happened was the Atomic Energy Commission realized that radioactivity from testing processes would carry radioactivity through the air. Meteorology was a necessary ingredient to understand where the fallout would occur. If I can backtrack for just a moment, I would like to point out that the reason I think that Harry Wexler set us up as a special project section was an odd reason--not only because Harry Wexler had an in with the military establishment, but the Office of Management and Budget, which, in a sense, controls the government more or less, because of its control over money, had made a decision, perhaps prompted by Wexler and Reichelderfer, who was then the Chief of the Weather Bureau, that the other agencies of the

government ought not set up their own competitive weather services. Instead, they were ordered by OMB to delegate the responsibility and transfer funds to the Weather Bureau to do the research and operations that they thought were necessary. And that's why the Air Force came to Wexler presumably, to ask him to work out AFOAT-1's problem, rather than set up a new weather unit.

The AEC was in the same boat. They had to go to the Weather Bureau and Harry Wexler, realizing that the Bureau was knowledgeable on the transport of radioactivity, then got us involved in work for the Atomic Energy Commission.

London: This brings up two comments I want to make. One, it's not just because of your loyalty to the Weather Bureau but in general, I assume that you're saying that for studies such as these, it would be much better to have a single agency take care of those studies and be the central point rather than setting up a number of other sub-groups. It would be overlapping and studying pretty much the same thing, and it would be a diversified effort.

Machta: I think I would agree that that was one of my motivations, but in truth in those days there were very few people who had access to information about the transport of radioactivity. Everything connected with atomic energy was secret. Nobody could get involved unless they had a clearance. So the number of people involved was pathetically small. It was later on in time, say five or ten years later, we had a lot of competition. But in the early days when we took over in the early 1950s, there weren't many other meteorologists who even knew what a radionuclide was.

London: This leads me now to the second part of the questions I was going to ask. That is, to what extent do you feel that these studies that you were involved in gave a lot of background and basic information on the transport properties in the troposphere and stratosphere by using some types of conservative or at least relatively known types of transient or time-dependent properties?

Machta: I agree that the studies did help understand the atmosphere better. I think that in the early days following radioactive debris contributed significantly and perhaps even more, they stimulated some of the global circulation modeling. For one thing, for example, when the first megaton yield hydrogen bomb test took place which put radioactivity into the stratosphere, each spring thereafter, even with no new tests going on, we found radioactivity being deposited on the ground. And it got to the point where we knew that the stratosphere was emptying itself into the troposphere mainly in the springtime. I must confess that the first time this happened, I was skeptical that the cause of the springtime peak in fallout, I thought there was some peculiarity about it, but it happened so frequently that we now know it really was a true phenomenon. The answer is that tracing radioactivity stimulated a lot of research. One of the several contributions I made was trying to push the use of tracers thereafter; for example, we

used the perfluorocarbons in air pollution studies.

London: You found that the tropopause was not a closed surface that prevented any exchange.

Machta: That's correct. We also found that the exit of air from the stratosphere probably took place either in the high temperate latitudes or at the polar latitudes rather than in the equatorial region, which of course is consistent with what was then known.

London: This goes a long way to explaining a good deal of the ozone budgets and other budgets as well.

Besides your work on transport properties of radioactive debris in the atmosphere, there were many, many other studies that you conducted. Before we get on to these incidentally, is there anything else you want to say about the work that you did on these various studies?

Machta: No...

London: During these times you had a lot of interactions with the AEC. Was that a good cooperative interaction?

Machta: Yes, it was scientifically a good one. In retrospect, I'm not sure if I knew now what I knew then, I would not have been quite so keen to have been a party to the testing of nuclear devices. At that time we were told by Admiral Straus and by others that the Soviet Union was a menace to us--and maybe then they really were--and we had to test our nuclear weapons. Some people suffered on account of it. I just tried to minimize the fallout on populated areas. I went out to many of the tests and I participated in them.

London: These tests were before the explosion.

Machta: No, the tests I'm referring to were the atomic tests we conducted regularly at Nevada and in the Pacific Ocean. I think I was misled, not being a health physicist in underestimating what potential damage might actually have occurred from the fallout from U.S. tests. Although by publicizing the fallout, as I did, I think the world got quite an abhorrence to nuclear testing and contributed, in my opinion, significantly to the nuclear test ban, which took place in 1962, when Kennedy was president. I was in Geneva and participated in the Conference when this agreement was being formulated.

London: That brings up another question and that is, to what extent were you involved in cooperation with the U.N. and participated in U.N. activities in cooperation with WMO, and participation in WMO activities on the international scale?

Machta: At the time, the United States was the world leader in atomic testing and the science connected with it. And I think the leader as well in the consequences of the radioactive fallout from the nuclear tests. I was, at that time, among the few people who had knowledge of most of what was going on. As a result, I was called on quite frequently to attend meetings of the World Meteorological Organization dealing with atomic energy and with the United Nations Scientific Committee on the Effects of Atomic Radiation.

London: So there was a good deal of international activity during this time and subsequently, and you participated in it.

Machta: That's correct, yes.

London: Were you representing the United States in most of these activities?

Machta: In almost all cases I was. With the IUGG, I represented the agency as a scientist, but for most of the international activities, I was representing the United States. That's correct.

London: Aside from these activities on nuclear testing, you were involved over the last 40-50 years in many other scientific activities. Among these are the determination of the global carbon dioxide budget and other trace constituents. Could I ask you first about your work on the CO₂ budget?

Machta: Yes. The modern measurements of CO₂ were started by a young graduate student, C. David Keeling. Harry Wexler, who was very perceptive, realized that Dave Keeling was on to something. He helped him to collect air samples and make analyses in Antarctica, the South Pole. Harry Wexler was in the 1950's heavily involved in the Antarctic program. Harry Wexler set up, with the cooperation of a number of other Weather Bureau people, the Mauna Loa Observatory in 1957-58, to provide a location for Dave Keeling to make his CO₂ measurements. I was associated with Harry a little bit during those early days, but under the direction of Harry Wexler. In 1962, when Wexler died, the Chief of the Weather Bureau wondered who would take over his responsibilities in this area, and indeed he appointed the unit I directed to do so.

So I became in charge of the Weather Bureau's global monitoring. In the beginning, there were really only two locations. One was at Mauna Loa Observatory in Hawaii, and the other was at the South Pole. I realized that this was an inadequate network and for many years thereafter, I tried desperately to get the funds to expand the network. It was only when Bob White and Jack Townsend took over the leadership of the Weather Bureau and then ESSA later on, that they agreed that this was just too important not to do, and they found money out of their own contingency monies and set up two more

stations, one at Point Barrow in Alaska, and one on the windward tip of American Samoa. All four stations are still operating today.

London: You were involved for a long time on the CO₂ budget.

Machta: That's correct. One of the earliest atmospheric constituents that we measured was carbon dioxide. We later expanded to many other constituents. As a result of making these CO₂ measurements, there were two things I wanted to do. One was to get a time trend to see whether the growth that Dave Keeling had established earlier would continue. Of course, we worked closely with Dave Keeling on that.

The second thing was to understand why there was a growth in atmospheric CO₂. The growth in carbon dioxide in the atmosphere--an increase with time of the CO₂ concentration at ground level. The reason for this, we all believed, was the combustion of fossil fuels; when you burn carbon it makes carbon dioxide. Keeling got fossil fuel data from a United Nations statistical unit, which gave the amount of oil, coal, natural gas and so on that was being combusted, and he then converted the fuels to carbon dioxide. It turned out that each year the amount of increase in CO₂ at our stations was only about half, perhaps 60%, of the increase that should have occurred if all the annual carbon dioxide stayed in the atmosphere. And so Dave Keeling formulated the concept of an airborne fraction, which turned out to be about 55-60%.

London: But now in 1971, you gave a paper at the Nobel Symposium in Stockholm. There you discussed a carbon dioxide global budget and in that paper, you suggested that there was a strong oceanic and biospheric component in the budget of CO₂. Is that correct?

Machta: That is correct. First of all, by 1969-70, we had accumulated enough data at Mauna Loa and elsewhere to show that carbon dioxide was truly increasing, as Callender and other people had suggested. We had also accumulated a lot of data on carbon-14. The carbon-14, which was in the atmosphere from hydrogen bomb weapons tests, was almost entirely present as carbon dioxide. Many measurements of ¹⁴CO₂ made were worldwide. I used the ¹⁴CO₂ data and the ordinary carbon dioxide (¹²CO₂) data and formulated a model in which I put a certain amount of the carbon dioxide in the oceans, a certain amount in the biosphere. The model also exchanged CO₂ out of these media. The growth rate of the production of carbon dioxide from fossil fuels was about 4% per year from 1958 to approximately 1969. I assumed it couldn't keep accelerating at that fast rate, so I decreased it to about 3%, and then to 2% per year. It actually slowed down more than that by about 1990. With the carbon cycle model and fossil fuel CO₂ input, I made a prediction as to what the level of carbon dioxide would be year-by-year out to the year 2000. As I talk to you in 1994, it can be verified up to 1992, and it's remarkably accurate within two ppm. Starting at 313 parts per million in 1959, in 1992 it's about 357 parts per million and I predicted about 359 ppm.

London: The important thing about that 1971 paper that I find interesting was the fact that until then it had been commented on but not really realized, I believe, how important the ocean circulation, not just ocean uptake, but the deep sea circulation and the general ocean circulation and the biospheric effects. The ecological effects within the oceans played such an important part in the whole carbon dioxide budget. It seems now from work that has been done in the last 10-15 years and up to the present time that these play an extremely important role in the general CO₂ budget. Is that correct?

Machta: You are correct in that. In fact, after my model, much more sophisticated models have been published by Bert Bolin, other Swedish and German and Swiss scientists, which have models that put mine to shame. I think my paper did initially stimulate the consideration of the biosphere and oceans.

I can tell an interesting anecdote about Dave Keeling. When he first started his work, he was laughed at because everybody knows that the oceans take up all of the carbon dioxide. There is no point in making any measurements because whatever you put in the air will very soon be in the oceans. You're wasting your time monitoring CO₂ trends. But Dave Keeling was a persistent person who did things extremely carefully and you need that care to show that the carbon dioxide was and is increasing in the atmosphere with time.

London: Right. You had mentioned Bert Bolin and you said "And others who were involved at that time and later on in the CO₂ problem." Do you know of other people who were also involved in the CO₂ problem?

Machta: Many, many other people were involved...let me tell a different story on this point. One of the ways in which I and others thought that we could get a handle on whether or not deforestation and the rotting of the wood contributed to increased atmospheric CO₂ was by looking at the decrease of oxygen in the atmosphere. Whenever carbon is burned, oxygen is taken out of the air. The lifetime of an oxygen molecule is very long, about 2000 years.

END OF SIDE ONE

Interview of Lester Machta

SIDE TWO

Machta: I was mentioning the fact that carbon dioxide gets its carbon from the burning of carbon-based fuels and the oxygen from the air. The oxygen in the air has a very long lifetime so that if you were to burn fossil fuels on a timescale of tens of years, you would not make up for the loss of the oxygen coming out of the oceans or elsewhere. The atmospheric oxygen would simply decrease. So with Ernie Hughes and the National Bureau of Standards making some measurements to six significant figures, we made measurements in about 1968 of the atmospheric content of oxygen. We found that people had been doing this in earlier years at the end of the last century--

London: In the early twentieth century.

Machta: The early twentieth century and even before then. As an aside, one of the reasons for an interest in atmospheric oxygen was not meteorological. People thought that if you were confined to a small space and with a lot of people there, they would use up all the oxygen. So measurements were made in all kinds of locations to make sure there was enough oxygen for you when they realized oxygen was necessary for life.

At any rate, Ernie Hughes made measurements in 1968 and we compared them with those in the early part of the twentieth century and found that there was no change over the approximately fifty or sixty years intervening. And knowing the amount of fossil fuel burned, we calculated that indeed you should have seen no change if you could only measure it out to five or six significant figures. Therefore, to see a change, we had to go out to the seventh significant figure. Dave Keeling's son, at Harvard University, developed a technique to measure O_2 out to the seventh significant figure and has actually found a decrease of carbon dioxide which is due, in all likelihood, to the burning of fossil fuels.

London: Now were they able to make measurements from the early twentieth century measurements--those were made on shipboard observations, weren't they?

Machta: No. They were Danish measurements, but they were made in isolated locations.

London: Could they make them to that accurate?

Machta: Yes, by chemical means. And the fact is, if you look at the literature, you'll find that there is not perfect agreement between ours and the Danish numbers. Ernie Hughes, of the National Bureau of Standards, who made our measurements, realized that they

made carbon dioxide per gram of actual air. When he corrected for the moisture in the air, then he got a number which was exactly the same as ours. If you take the number from the literature and don't correct for the moisture, you do not get the same number. This is the same as the difference between humidity mixing/ratio and specific humidity.

London: One of your main contributions at that time were with ESSA and NOAA by then. What other things did you work on during your time at NOAA?

Machta: Well, in the late 1950s, the Public Health Service realized that air pollution problems involved also air transport, although they were not on the same scale as the testing of nuclear devices. But Harry Wexler knew people in the Public Health Service, which is the predecessor to the current Environmental Protection Agency, who were also prohibited by an OMB dictum to set up their own weather service, so they came to Harry Wexler and said, "Would you set up a meteorological unit for us?" And to this day the meteorologists who support the EPA in Raleigh, North Carolina, and elsewhere, are NOAA employees who got their start back in the days when Harry Wexler set up the unit. When Wexler died in 1962, I also inherited that program and it expanded tremendously so that right now there are maybe fifty or sixty people in Raleigh, North Carolina, civil servants who are supplying meteorological analyses for the EPA. We have done a lot of work in many areas of pollution.

London: That work on pollution is still going on now.

Machta: That is correct.

London: Also, one other very important bit of study that was started early on were observations of solar ultraviolet-B radiation and the effects on the biosphere and the measurement of solar UVB radiation. Tell me about that.

Machta: Let me go a little bit into the background. Among the observations which were being made by the Weather Bureau was total ozone with Dobson spectrophotometers on the at about a half-dozen locations in the contiguous United States. When the baseline stations of Mauna Loa and South Pole were set up, I saw to it that we also put Dobson instruments there. I was interested in trends of ozone so that when the fuss about the Supersonic Transport (SST) arose, the Weather Bureau and my group in particular was in a position to provide data on trends and likely future effects of changes in atmospheric constituents as they might affect the stratospheric ozone layer. Money was provided in the SST controversy not only for additional stations to measure total ozone, but also to measure ultraviolet-B radiation.

The people who did the work on the effects of UV-B radiation were Dr. Urbach, a dermatologist at Temple University Medical School, and a biophysicist named Dan

Berger who worked for him. They provided the Weather Bureau an instrument which was built by Don Robertson in Australia--a really very simple device that integrated the solar radiation from about 295 to 320 nanometers, the UVB range which had a response function like human skin sunburning. Dan Berger, working for Dr. Urbach, made some modifications to this instrument so that it is now known as the Berger-Robertson UVB meter.

We implemented a network with funds provided by the Department of Transportation using these meters over the United States. The purpose at the time was to find out whether or not dermatology and skin cancer epidemiological studies would show differences between say, Albuquerque and San Francisco, or Bismarck and Tallahassee. The meters were not intended at that time to provide a trend in the UVB radiation. I think we all realized the crudeness of the instruments. With these crude instruments we collected data and unfortunately, in my opinion, when there was evidence of decreasing total ozone over North America and the United States, there were immediately questions raised as to whether or not the UVB was concurrently increasing and Joe Scotto of the Public Health Service and one statistician in my office started to look at the trends from the Berger-Robertson data. We found contrary to what was expected that the UVB was decreasing. And this has been used to this day to criticize some of the inferences made about the ozone depletion theory. Let me add that a comparison of total ozone and UV-B changes at the same place a few days apart agreed with theory.

London: In these efforts, have you been involved with international efforts to measure the same thing--WMO, for instance?

Machta: We have been. For example, we have stations in Australia. And there's a station in Poland which is making measurements. These generally are done in collaboration with Urbach, who has associations with medical people in different countries.

London: During these times, you've been involved in a number of other problems. I don't want to go into all of them but could you just briefly mention a few?

Machta: Yes. For example, when the SST was coming on-line, not only did we worry about ozone depletion and UVB increases but our office became involved in sonic booms and we had a mathematician, Dr. Albion Taylor, who worked out probably the best calculations of the transmittal of sound from a supersonic airplane down to the ground. We put out instruments in the western part of the United States where military aircraft were going supersonic, were overflying--we did quite a bit of work on sonic booms. That effort ceased when the SST disappeared.

We also got involved heavily with solar radiation and for awhile, we were given extremely large amounts of money by the NSF and then the Department of Energy to

try to rehabilitate the failing Weather Bureau or NOAA ground-level solar radiation monitoring program. We also got involved and are still involved heavily in acidrain research and monitoring.

London: I know that with all these efforts, or as a result of all these efforts, AMS awarded you the Cleveland Abbe award. This is an award for distinguished service to atmospheric sciences by an individual. This was awarded in 1974. Just to get this into the record, if you don't mind, I would like to read the citation for the award:

For outstanding contribution on critical atmospheric problems pertaining to the protection of the environment, especially for his studies of atmospheric constituents and pollutants including oxygen, carbon dioxide and radioactive materials.

I think that that's a great award and an important one indicating the contributions that people in government and outside government make towards the well-being of our society.

Before we conclude the interview, I would like to give you one last opportunity for some comments. How would you, looking back now, over your fifty some-odd years in the field of meteorology, how would you characterize some of the major developments, not just in your own particular field but in general in meteorology over this period?

Machta: It is my judgment, and this would not be agreed upon by everyone, that the single most important reason for the advancement in weather forecasting is the result of the computer revolution. It's my opinion that by being able to solve all our equations of motion, state, etc., which L.F. Richardson tried to do around World War I, we actually have come to the point where our short-term forecasts and even perhaps some longer-term forecasts are now ever so much better than they have been in the past. Some advancement, particularly in hurricane work, is the result of satellites. But in my judgment, the more significant increases in forecasting--whether you should take an umbrella tomorrow--have come from the computer field, rather than almost any other area. For short term or now forecasting radars and rapid communications are more important than computer development benefits. But again, it's in the forecasting field that I think meteorology gets its bread and butter and the computers have been our savior there.

London: Let me interpose a comment on this. This is my comment, which I'm sure you will agree with. I don't know whether when you went to NYU you did a mathematics course with J. J. Stoker or not, or if you knew him. He was a very important mathematician. He commented at that time--this was in the late 1940s--that there were two classes of differential equations, the linear and the non-solvable. During that time and since that time, as you said, the computers have come around. The non-linear

differential equations now can be--many, many of them can be solved. Not, of course, all of them, but many of them. And so the non-linearity of the atmosphere now has had a great deal of advancement in terms of just what you've said by use of these computers.

Machta: Yes, and I must say a very good friend of mine, Ed Lorenz, who graduated from MIT when I did, has really fouled up the matters by showing that you **can't** solve all the non-linear equations. Somehow I was brought up to feel that everything in the world is deterministic, and Ed has done me dirty.

London: I must interpose this because Ed is also a friend of mine, that when Lester Machta says he fouled up everything, Lester was really smiling very broadly.

Could I go on with just a few other generalizations? During the time you've been involved, you've interacted with many people professionally, and you have during this interview mentioned a number of these people: Harry Wexler, Ed, and many many others. Are there any others you can think of?

Machta: Well, I was just before you came looking over some old pictures. One of the pictures I looked at was of the 1962 test ban treaty negotiations. And whose picture did I see next to me but Federov, who was a famous Russian-Soviet meteorologist-geophysicist who was the head of the Russian delegation. In 1962, during the early part of our meetings, the western people were on one side of the table, and the Russians-Soviets were on the other side, glaring at each other.

Reichelderfer knew (the Chief of the Weather Bureau) Federov and told me, "Why don't you give him my best regards? We're good friends; we met in the WMO and elsewhere." So at one of the breaks during the negotiations, I went over to Federov and said, "You know, Reichelderfer would very much love to see you and say hello to you and he sends his very best regards." Immediately thereafter, the ice seemed to have broken. They invited us to their social events, we invited them to our social events, they told us they had their Tellers in Russia just as we did, and really they had many of the same objectives as we did. But they couldn't say a thing, they thought the westerners were just out to do them in. Only when there was some human touch interposed from Reichelderfer did they become friendly and thereafter Federov was especially friendly.

London: So the ice needed to be broken.

Machta: It had to be broken, and we were everlastingly friendly...

London: Were there any other people you can remember?

Machta: A Nobel prizewinner...

London: The only last question I have is whether or not you have any additional comments to make about the general interview.

Machta: Everything's fine, and I hope this does some good for the world in the future.

London: Thanks very much, Lester.

END OF INTERVIEW