American Meteorological Society
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TAPE RECORDED INTERVIEW PROJECT

Interview of William W. Kellogg

August 18, 1988

Interviewer: Earl Droessler

DROESSLER: I am interviewing Will Kellogg at his home in Boulder, Colorado on Thursday, the 18th of August, 1988 and the area that we are going to is the AMS, the American Meteorological Society and its activity, with special emphasis on the activities of the American Meteorological Society during the time that Will was the president of the society. Also, we will go into atmospheric sciences development, generally, and ask Will to comment on the part and the role that he played with his colleagues as this significant area of scientific activity advanced through the last several decades.

Will, you were telling us that when you were elected president of the American Meteorological Society and share with us some of the things that were happening in the field of meteorology and the society at that time.

KELLOGG: My years as president elect, president, and past president were from 19072 to 1974, as I recall, and I was looking at some of my notebooks back in the 1970s to remind myself of the things that are happening, that were where happening in those days and I was amazed to see how much was going on, generally, in the world of meteorology and in the world of geophysics. Some of these trends that were national and international trends were reflected, of course, in the evolution of the society, itself and the AGU to reflect what was happening in the world.

DROESSLER: As I remember, at the time that you served as president, the AMS [president] also served as the president of the AGU section of meteorology?

KELLOGG: Yes. This was quite by coincidence that I was president of the two organizations at the same time and it came also at a time when people were wondering how the AGU, which, of course has a meteorology section, and the AMS could work more closely together. Since I was president of both parties for awhile, I was able to bring them together and I think it did result in some steps

being taken to ensure that the AMS and the AGU work together.

The main area in which they cooperated, of course, in the meetings. The American Meteorological Society always cosponsors the Meteorology section meetings of the AGU at the annual meeting. The AGU sometimes cosponsors AMS meetings, also. So, the two do work together. It's become a kind of accepted idea that the two cosponsor their meetings when they overall ion the subject. So, this was one of the things that happened during my tenure.

Looking back through my notes, I see several things that were happening at that time, in the early 1970s. One of them which I was connected with very closely was the study of critical environmental problems in the summer of 1970 in Williamstown; the study of critical environmental problems was SCEP, for short. The results came out in a book published by the MIT Press. The weather and climate section of that book was written by a working group that I was chairman of and it pointed out that in fact, mankind did have the ability to change the climate, although it wasn't clear at that time just what direction we were going to change it.

It was also pointed out in our report that we could change the stratospheric ozone and this, of course, has become something of a _____, but in those days, the idea that supersonic transports could change the ozone layer and cause more ultraviolet to get through came as something of a surprise. When the SCEP study wound up its deliberations in Williamstown and the newspaper reporters came, it was the effect of supersonic transport that attracted the most attention it was at that time that the US was considering funding to new prototype supersonic transports by Boeing or Lockheed. So, this really hit the fan, you might say. I will have more to say about that particular issue because the AMS played a rather important part in resolving some of that controversy.

The other thing that was happening in the world of science or geophysics was that we were beginning to realize that you couldn't study the world without taking into account not the planet, as a whole, which we had become used to, as a result of having meteorological satellites and good communications, but that you had to take into account the oceans; you had to take into account the biosphere, living things, and you couldn't study the world, its climate, and its weather without taking into account these other disciplines. You could no longer stay in your pigeon holes, in other words, if you were really going to make progress. So, this became a theme for the summer study, the next summer, the one called *Study of Man's Impact on Climate*, just outside Stockholm, which also resulted in a book by the MIT Press, and also organized by Carol Wilson, a distinguished professor at MIT. There, we laid it on the line how complicated the climate system was and we knew about it. It became a kind of benchmark for where we stood studying the climate and studying the impact of man; we referred to man's impact, now and now, we would say mankind's impact, or humanity's impact.

DROESSLER: At that meeting outside of Stockholm, in addition to meteorology, were there other scientific areas represented?

KELLOGG: There were atmospheric chemists very much in evidence, and at least one oceanographer. We did not have ecologists at that point, although the atmospheric chemists talked a lot about the role of the biosphere in balancing the carbon dioxide and methane and so forth in the atmosphere.

DROESSLER: Even at that early day, under the aegis of the climate change, we were bringing together a multidisciplinary approach to the problem.

KELLOGG: We had particle people, people like Weickmann, and on particles, Bob Stewart on oceanography, Chris Huener(?) on atmospheric chemistry, Julie London who was a stratospheric person. So, we had a pretty good mixture of people. Michael Budyko of the Soviet Union, was there. He was one of the Soviet Union's most distinguished climatologists. We had Suki Manabe, who is one of our most distinguished climate modelers. So, we had quite a top-notch bunch there.

It was concluded that there was no question about man's impacting the climate, although we couldn't make up our minds as to whether it would be in the direction of a warming or a cooling because the greenhouse gas school of thought said it was going to warm, but then there was another group that was interested in aerosols or particles in the atmosphere, and in those days, before we had done all our homework on particles, we thought that maybe particles could cool the atmosphere and therefore, counteract the carbon dioxide. Now that we've done more work on that, we know that's not true ad they both work in the same direction. But, in 1971, we couldn't come with a clear statement about which was going to win out.

There was at least one other attempt trend in scientific thinking which the American Meteorological society had to respond to and that was the growing interest in solar-terrestrial physics. It had always been something which we involved the upper atmosphere people, the people in aronomy, and they tended to be separate from the so-called meteorologists, that is, arronomy and meteorology were almost two disciplines, event though they both dealt with the atmosphere.

Under the banner of solar terrestrial physics, it became clear that you needed to combine meteorology with the dynamics of the entire atmosphere, so this was bringing meteorologists and aeronomists together in this area, and it's reflected in the AMS committee structure and certainly in the AGU committee structure or section structure.

DROESSLER: Probably also in our meetings, then?

KELLOGG: Definitely also in our meetings.

I mentioned that the AMS was beginning to play -- always could play, and in these areas that I mentioned, was beginning to play an important role. One evidence of that was the annual meeting of January, 1972, when there was a special session organized by Jim Friend and Paul Cadlic(?), on the role of supersonic transports in modifying the ozone layer. We hear so much about that now that it doesn't come as any great surprise that the AMS was talking about it, but in those days, it was a new idea that we could affect the ozone layer, one that was pointed out in the SCEP study and then during the hearings before Congress on the supersonic transports, and then in the Department of Transportation setting up the SIAP program, which was a four-year program to study the impact of aviation on the stratosphere.

During the hearing, the two prototype supersonic transports, a very distinguished atmospheric chemist named Harold Johnston at the University of California at Berkeley came published, through the New York Times, rather than a scientific journal, the idea that the oxides of nitrogen, which would be put out by the jet engines of the supersonic transport fleet would seriously degrade the ozone layer by something like a factor of 50%. This would allow a great deal more ultraviolet radiation to come through and this was cause for concern.

There were people like Jim McDonald, of the University of Arizona, who pointed out the correlation between ultraviolet radiation and skin cancer. The cancer word—the word cancer always frightens people, so Harold Johnston's, combined with Jim McDonald's published testimony really scared people.

DROESSLER: How did the AMS handle this? Did they have a special session at one of their annual meetings and invite distinguished people to speak on it?

KELLOGG: There were a great many people who were experts on the dynamics of the stratosphere which Johnston was not, who questioned Johnson's calculations. His model was greatly oversimplified, they thought, and at the January, 1972 meeting in New Orleans, a special full-day session was devoted to this question. Harold Johnston, himself, was there, in addition to Jim Friend, who organized the meetings. We had Julie London and a great many others who were involved in studies of the circulation of the stratosphere. The upshot of it was that I think Harold Johnston, himself, agreed, after the discussion, that his was an overestimate and the reduction in ozone would be something much smaller than he had originally said, but nevertheless, that it would be in the direction of a decrease in the ozone.

I think the significance, form the point of view of what we were talking about, is that the AMS was able to provide a public forum for discussion of a very important issue.

DROESSLER: That was about 15 years ago. How does Harold Johnston's ideas

correlate with today's knowledge about depletion of the ozone?

KELLOGG: I think Harold Johnston has been proven correct, however, the whole science of atmospheric photochemistry in the stratosphere has progressed a lot further than Harold Johnston's knowledge in 1972.

DROESSLER: And of course, the SST has never materialized and so it would take another kind of activity in the stratosphere.

KELLOGG: We were talking about an enormous fleet of supersonic transport, 500 supersonic transports each flying seven hours a day. That was sort of the assumed by the people who were all making calculations, this ridiculously-large fleet, and I made a calculation along the line in my testimony to Congress during that period that if we really did have that many supersonic transports all flying, it would take all the petroleum in the Alaska Pipeline to keep that fleet flying. So, it shows how really out of joint this whole idea was.

DROESSLER: But, you can't blame Johnston for using that as a base. There would be a tremendous amount of material dumped into the stratosphere.

KELLOGG: There would be. Of course, what has happened since then is that Sherrie Roland at Irvine, California, and Molina, pointed out that a much more pressing issue was the question of the chlorofluorocarbons in spray cans, sometimes referred to as freons, which was putting chlorine in the stratosphere. Chlorine oxide reacts with ozone in the same kind of way that nitric oxide reacts with ozone; it's a catalytic reaction and chlorine destroys many molecules of ozone and so this, then, became the issue. Supersonic transports went on to the back burner and chlorofluorocarbons as we know, are still an issue.

There was the Montreal Protocol of about year ago, where 21 countries agreed to cut down the production of chlorofluorocarbons. It was a historic international agreement. But, that's something that's much more recent than was I was going to talk about, which was back in the 1970s.

DROESSLER: But they really were exciting times back there when the beginnings of the scientific curiosity and scientific speculation and theory were emerging on some of the most important social issues of today.

KELLOGG: That's true, and it was an exciting time. It was also an exciting time for people like myself who were interested in the atmospheres of the other planets. I remember Carl Sagan and I wrote a monograph in the 1960s on the atmospheres of Mars and Venus and it was one of the few National Academy of Sciences reports that was almost immediately sold out. So, it was a period where really our horizons were expanding very fast.

DROESSLER: Going from scientific problems back to administrative problems

and organizational problems, you mentioned just briefly that AGU and AMS were conducting conversations about the possible consolidation or merger or establishment of some kind of a large umbrella organization. Would you comment on what was taking place at that time?

KELLOGG: Lloyd Berkner, who of course, was one of the great leaders in geophysics. He was president of the Associated Universities, a president of the American Geophysical Union and of the resident one and a very distinguished scientist on the international front, the International Council of Scientific Unions area. It was Lloyd Berkner who had the idea, based on his background in the electronics industry -- he was one of the first, by the way, to invent ionosphere International recorders which use a kind of radar technique to measure the electron distribution in the ionosphere. So, he was not only a geophysicist, but an electronic wizard.

His experience with the Institute for Electronic and Electrical Engineering (IEEE) became an umbrella for a large number of electronic associations and societies and apparently, the IEEE has been a great success, very powerful, very well-organized and saves a lot of money and confusion in this diverse and large industry.

So, Lloyd then thought that it would be good to get the geophysical community under an umbrella in the same way. Of course, the geophysical community includes the little old AMS -- I say little old, because when compared to the American Geophysical Union, it's smaller and compared to the Geological Society of American, it's minute. There are several other geological societies -- can't name them all -- but there is one for Pale climatology...

DROESSLER: The Society for petroleum engineers...

KELLOGG: Yes, thank you. There is a big list and some of them dwarf the AMS, but we did have a meeting on this subject and I think that there was a certain rationale in having an umbrella organization. As I recall -- this was many years ago -- the main rationale would be to save money, save secretarial help and you could have a central publishing organization and a central accounting organization and so forth.

I am not sure of my facts, after all these years, I think the AMS wasn't very enthusiastic. Ken Spengler just was settling in to 45 Beacon Street and we were all happy with that independent organization and we saw no reason for joining up with anything else.

The Geological Society of America, I think, was quite negative on the whole idea, too. They had their empire and they weren't going to change that.

DROESSLER: The American Meteorological Society, I believe it was very evident during your administration, was well on the way to financial success.

KELLOGG: It has almost always been in the black under Ken Spengler, that's right.

Another thing stuck in my mind from that meeting and that was the idea that Lloyd always stood for an organization being able to be quick on its feet to perceive new directions that needed to be taken in science. He referred to these as centers of enthusiasm. It's kind of a neat expression, centers of enthusiasm. He had seen this in the geophysical area.

For instance, the emergence of planetary science as a new science, with a small group of astronomers and atmospheric types getting it together and starting a whole new science, really. The people in the upper atmosphere and magnetosphere were, of course, a new breed. As we got the satellites and rockets up higher and higher and we began to be able to see the radiation belts and it became a new subject for research, a very important subject for research, and interplanetary and solar wind, which nobody had ever heard of before suddenly became a big subject.

This was a lesson that the AGU, I think, has learned; that is, they have been flexible in setting up new sections to reflect new centers of enthusiasm, if you will, and maybe to phase out old ones, although that's harder.

The AMS has reflected it in its committee structure, which you very well know about, Earl, setting up new committees which would not have been needed in the old days but which are now needed to deal with such things as solar terrestrial physics. Weather modification was another one that I think probably came into being, and the question of the stratosphere, which had been relegated to a very small group of specialists, suddenly became a big subject, as airplanes began to move in the stratosphere and as the importance of the ozone layer became obvious.

DROESSLER: Another one which you were very much involved in right from the beginning, and still are very much involved, is climate change and the AMS moved to accommodate that with not only committees to look after the scientific (inaudible) and scientific meetings but also new publications.

KELLOGG: I suppose there has always been the climate committee in the AMS, however, what hasn't been always appreciated is that the subject of climatology changed completely around the late 1960s and 1970s. it went from a subject which was just usually taught in the geography department and had to do with statistics of the weather to a physically-based theory of climate, that is, we now are beginning, in 1967 with Manabe's work, to put together a physical model of the climate system, including the atmosphere, the oceans, clouds, radiation, all the things that cause establishes our world's climate. So, from a relatively-obscure and rather dull subject, climatology became a very exciting field of new research involving some of the best brains in the business and some of the fastest

computers. So, it was a new ball game, in other words. This was certainly reflected in the committee structure of the AMS.

DROESSLER: I would like to ask you when you were president of the AMS, associated with Ken Spengler as the executive director of the American Meteorological Society and other members of the council and the commissioners, and so forth, can you recall conversations with them and associations with them that were particularly interesting from the point of view of establishing where we were at that time, as an AMS and some of the new directions that we might be going in?

KELLOGG: Well, I already mentioned some of the new directions that I felt we should go in and we, in fact did. It became obvious that the AMS should respond to some of these new directions in the scientific area.

Going over my notes, I came across a conversation that I had with Ken Spengler and Evelyn Measure(?) on February 18, 1972. I was an incoming president elect and wet behind the ears on the affairs of the AMS, though I had earlier on been a member of the council, but I was very interested to see what Ken and Evelyn felt were important issues in the AMS, and maybe 16 years later, we are dealing with some of these issues and it will amuse you to see that some of the issues in 1972 were not too different from the issues that we still face.

The annual meeting, of course, is a very important aspect of the AMS; always has been and always will be. Bob White, who had been very influential in the AMS all his life, practically, made the made the statement that he was disappointed over the small attendance at the New Orleans annual meeting, only 250 people, including 75 TV and radio people, so it really wasn't much of a turn out. Including the TV and radio people, that would only leave about 175 people attending this annual meeting in a very attractive place.

DROESSLER: Since the council met at that time, some of them were brought there by the affairs of the council.

KELLOGG: Yeah, so it was sort of a pretty dismal turn out and there was no explanation why it was so small. Anyway -- Ken not reflecting not only Bob White's ideas but quite a few others, too -- felt that what we should do is let the annual meeting be small and let a lot of the scientific specialized topics be taken by our specialized meetings, which, of course, we have always done and I think through the years, the specialized meetings have become more and important, haven't they?

DROESSLER: Absolutely.

KELLOGG: They are in different parts of the world and they are organized by the kind of centers of enthusiasm, if you will, that I referred to earlier.

The annual meeting would be a time for reports from the heads of the services, for example. That would be one of the things that would be useful for the NSF, NOAA, NASA, The Department of Transportation, EPA, etc. and have, perhaps, a full day of discussions of how the government agencies were seen in meteorology.

Education and manpower would be another subject that would be appropriate for the annual meeting. What are we doing in this general area, and perhaps, reports on the specialized meetings, but not have the specialized meetings, themselves.

Dick Craig was going to work with Ken on the next annual meeting and reflect some of these ideas in the call for papers, that he sent me three. The subject that Ken and Evelyn brought up was that the award ceremony was too long at the annual banquet and everybody went to sleep over it, and couldn't we possibly have some of these awards presented at the specialized meetings and not have quite such a long affair. That certainly strikes a note.

By the way, I have to boast that I was, I think, the first president who refused to wear a tuxedo at the annual meeting. If I wasn't the first, I was one of the first. I saw no point in the people who sat at the front table having to rent tuxedos for the annual banquet, although I know that you and probably others probably owned tuxedos that fit. I have long since outgrown mine.

DROESSLER: You certainly set a trend there because there is not a tuxedo in site at the AMS meetings today.

KELLOGG: Thank heavens.

I am looking at my notes, now, just to see what some of the things are that we talked about, and relations with the AGU are something that we discussed at some length at that time.

The point was made that the AMS tends to represent their rank and file meteorologists along with the scientists, rank and file meaning the forecasters, of course, whereas the AGU tends to be more research-oriented. They both have foreign members. They are not strictly national. The National Academy of Sciences has taken over the representation of the United States in IUGG, so in that sense, the AGU and the AMS are in the same league now.

Apparently, Homer Newell(?) who was then president of the AGU was very sympathetic with the AGU and the AMS working more closely together. I didn't know in 1972 that I was going to be elected president of the meteorology section of the AGU, but of course, as president of both organizations, I was able to ensure that there was good communication between them.

The idea was also that since my situation might be only temporary, that we ought to have some permanent arrangement, so the AGU and the AMS would work together, and I think it has, through the years, become a permanent arrangement. Traditionally, the AMS does cosponsor the meteorology section of the AGU at its annual meeting. So, these are some of the things that we talked about.

I noticed another thing -- I won't go over that in this report -- but I am sure that Bob White's report on manpower at the annual meeting in 1972 should become a matter of record because it shows how fast the AMS had expanded and how, to some extent, it had changed its character in the previous ten years. That is, the scientists and the people from the universities had taken over a larger and larger fraction of the membership. At the same time, an enormous increase in the research dollar devoted to meteorology had taken place in that ten-year period. Some of it was (inaudible) but a great deal of it was sponsored by the Department of Defense, DOD, and the EPA getting into it.

DROESSLER: And the federal government national weather service, or weather bureau, as it may have been called at that time, remained relatively plateaued in numbers, and so the numbers of professional, practicing meteorologists in the forecast area remained constant, where the numbers in meteorologists working in research, both in the university community and then in the private sector...

KELLOGG: Private sectors, too...

DROESSLER: (overlapping dialogue; inaudible) expansion of meteorologists being occupied in rather splendid jobs --the whole area of broadcast meteorology and private meteorology service to industry started to blossom at that time.

KELLOGG: These facts and figures about the increase in meteorology were becoming well known as a result of people like Bob White digging into it.

While I was president, or when I was past president and still on the executive committee, we asked ourselves what really is the constitution of the AMS; what is the membership. We really didn't know and we really didn't think we could get very good information by just going through the files.

Last promise to the executive committee was that I would design a questionnaire that could be sent to all the members of the AMS and we would find out something about our membership, which I did, and this was published, I suppose, in 1975 or so, in the bulletin, and it showed how many women we had and how many students we had, and how many minorities we had, and the average income, and so forth. It was an interesting questionnaire. It was a lot of work to design one. I had the hardest time getting agreement on how to ask a question that would smoke out what minorities existed in the AMS and fortunately, at one of the first meetings of the AMS committee on women and minorities -- I was always amused that women were lumped with the minorities -- had its meeting at NCAR

when I was designing the questionnaire, so I went and threw myself on their mercy and said, please help me with this question. How am I going to ask how many Eskimos there are, for instance. Of course, the Alaskan representative, who was an Inuit, said, you can't call them Eskimos.

I think if I had been more sophisticated, I would have known that we had to call them Native-Americans, and blacks, is OK, Hispanics is OK. I was not allowed to ask how many Jewish people there were in the AMS because that was considered to be a religion rather than a race.

I suppose that in interviewing AMS past presidents, you have had the comment made by all of them that it was really a very pleasant and a gratifying role to play. It is, indeed. You get to become deeply involved in the affairs of the AMS and also attend meetings and give talks around the country at specialized meetings and get to know a lot of the people that you wouldn't get to know otherwise.

During my tenure, in addition to the pleasant jobs which were somewhat ceremonial, there were two other things which were quite exciting. One was the 100th anniversary of the WMO held in Vienna. It actually goes back to the International Meteorological Organization, or the IMO, which was formed in 1873 by a group of ships' captains that got together to agree to exchange weather information on a regular basis. I attended the 100th anniversary of the first AMS representing the meteorological community of the United States. This was a very exciting time. I might say that although I had been in and out of the WMO, this was the first real introduction to how it works. Five years later, I spent a year with the WMO, not because I had been there at the 100th anniversary, but it was kind of an extension of my association with it.

Another very exciting event took place and that was the first trip of American meteorologists to China. When Dick Reed was president, he began some negotiations by writing some letters to people that he knew in China, including Chu-KO(?) Chen(?) who was the president of the Academia Sinica(?) and also a distinguished Chinese climatologist, who wrote back that he was very much in favor of having more relations between the American meteorologists and the Chinese meteorologists but nothing came of it immediately.

It wasn't until an American meteorologist of Chinese descent, went back to China in 1973, I think it was, and was able to talk to the leading meteorologists in the Chinese Meteorological Society that we arranged a firm invitation. In those days, it had to go through the Chinese Liaison office in Ottawa. There wasn't one in Washington, however, there became one subsequently.

Finally, we got a green light from the Chinese Meteorological Society to spend exactly two weeks in China at their invitation, and the immediate past presidents and incoming president were all there, plus Ken Spengler. That would be Dick Reed, myself, Dave Johnson, and Dave Atlas, Dave Johnson being the then

president and Ken Spengler, all with our wives, except for Dave Johnson, who had just lost his.

This was a fascinating two weeks that we had, during the time in Chinese history when the cultural revolution was still going strong and people (inaudible) envied s had the chance to talk to people and hear and see what was happening during the cultural revolution. It was a fascinating time in Chinese history and it was a fascinating time in the relationship between the US and China. Of course, since then, there have been a number of exchanges.

DROESSLER: Was it true that your visit, under the auspices of the AMS was the first scientific visit to the Chinese...

KELLOGG: It was one of the very earliest. I can't document exactly. The earthquake people had a group of seismologists and earthquake forecasters, sponsored by the National Academy of Sciences, visiting China. We were one of the very earliest.

When we were there, we kept running into another group of Europeans and Americans who were medical people studying Chinese acupuncture practice, traveling all over China, going from hospital to hospital studying acupuncture. All these things were band new. We never had a chance to find out what was going on in China until 1973-1974. This was in April-May, 1974 that we were there.

This was written up and there was a special issue of the bulletin of the AMS devoted to that. Since I was on sabbatical from NCAR, I had time to devote to writing it up so that was essentially my report, with the help of the other members of the committee and their notes and pictures. It was quite a full report on our visit.

DROESSLER: As I see it, the opportunity to be the president of the AMS reached your life about the middle of your career and you certainly were involved in a lot of exciting things, and s president of the AMS, I would as president of the AMS. I would like to go back, now, to the early days of Will Kellogg and get you to comment on how you became associated with the field of meteorology and what were some of the first things that you were engaged in as a fledgling meteorologist.

KELLOGG: Well, I won't try to give you a complete life history, of course, but you asked how did I get into meteorology. That is kind of interesting because I ask a lot of other people how did you get into your profession and it's sometimes interesting to know.

I was a graduate student in physics at Berkeley, having majored in physics at Yale before that. I had also wanted to fly and had a civilian pilot-training course. I signed up for the civilian pilot-training...

END OF SIDE ONE.

KELLOGG: You may be wondering why they had this civilian pilot-training course where they gave free training in flying an airplane. Well, they foresaw, in 1941 that they were going to need a lot of pilots to fly all these bombers that were coming off the assembly lines in Europe right before we got into the war. So, anyway, Mr. Clover's lectures in meteorology were very exciting to me and I decided that meteorology was really much more fun than (inaudible) mechanics, which I was learning from Robert Oppenheimer (?) and others at Berkeley, who were very bad teachers, I thought though they may have been brilliant, and I saw a sign on a poster saying join the meteorology cadet program. I signed up without telling anybody in the department that I was signing up until it was all signed and sealed. They were furious because they said that they would have frozen me in my job as a physics graduate student if they had known that I had any intention of joining the Army air corps. It was too late and I did join the Army air corps and I went to UCLA and got my goal bars as a meteorology cadet and a second lieutenant.

I then was an instructor in meteorology at UCLA and then at Grand Rapids and finally, after the war was over and I had been a test pilot, I went through flight training so I was not only a pilot but a meteorologist. It was great fun and I almost, and I almost stayed in the Air Force so I could go on flying because I loved it so.

It was probably just as well that I got out and went back to school and after about a year of graduate work, it seemed clear that my financial situation was getting desperate with $90 a month from the GI bill and $90 as a teaching assistant, and so I went to work half-time for the Rand Corporation, Project Rand, initially, in 1947, and that got me through graduate school.

I divided my time between Rand and UCLA for awhile, the meteorology department at UCLA, and was assistant professor there. But, I decided that Rand was more exciting than UCLA and went to work full-time for them.

It really was an exciting place. Rand has been referred to as the think tank, which meant a team of people working on a given problem from different angles, multidisciplinary team. When I first joined Rand, I discovered that the big project was studying what they call an artificial earth satellite. Of course, it was highly-classified in those days because it was devoted to reconnaissance. Being an old weather reconnaissance man in my Air Force days, I saw the advantages of a satellite for weather reconnaissance. With a young colleague, Stan Greenfield, we did the first serious study of how you could use a satellite for weather reconnaissance and that was published in 1971. It was classified secret.

I believe that it was you, Earl, that asked that it be declassified so that Stan

Greenfield and I could get an AMS award for this first report on meteorological satellites.

DROESSLER: I remember how happy you were and how happy Stan was, and how happy Stanley's family was when that award was presented to both of you in, I believe, New York City.

KELLOGG: It was a great honor, indeed.

On the 25th anniversary of TEROS one, Stan and I were given another award from the Department of Commerce, along with several other people like Ryn(?) Sumi(?), and Mori(?) Tempura(?) and I have forgotten who all, for having worked on the original satellite. That was one of the things that I did with Rand.

Another one was to put my little department to work on how to calculate radioactive fallout and we published the first unclassified journal article on predicting radioactive fallout. It was published in the Journal of Meteorology, based on my same testimony to Congress on the same subject. In fact, everything was classified in those days so that when they declassified my report to the joint committee hearings on nuclear testing, I immediately sent it into the Journal of Meteorology. My coauthors were Stan Greenfield and Bob Rabb(?).

Another thing which we cooked up during our Rand days, which again was highly-classified except that when a Soviet colonel called a news conference and put a great big package, marked US Air Force Confidential, and showed it up and showed a great big aerial reconnaissance camera, this kind of blew the lid on our floating balloons over the Soviet Union and this was a very big Air Force project that Stan Greenfield and I worked on together and shepherded it through.

We came up with the idea of how to recover these packages after they had flown across the Soviet Union and were snatched by an aircraft mid air. .

DROESSLER: Were these constant level balloons?

KELLOGG: No. They were the great big plastic balloons that were not constant level and of course, they didn't all make it across. They had a very sophisticated ballast system invented by Vin Lally who was at NCAR still, and most of them made it across.

DROESSLER: They flew at what altitude, 80,000?

KELLOGG: I forgot the exact altitude, but it was well above what any aircraft could reach. I think it was probably more like 50,000 or 60,000. In the 1960s, they couldn't get up that high. They were very visible, but they couldn't do anything about it. So, that was one of the things that we were involved in and it was kind of fun to work on it.

DROESSLER: From the Rand Corporation, then, you moved to NCAR.

KELLOGG: Yeah. I first met Walt Roberts at the wind-up of the IGY in Moscow in August, 1958. We saw quite a bit of each other and enjoyed each other's company a lot. He went off to visit his solar astronomer friends in the Soviet Union at the meeting in Moscow was over and I went down to the Crimea with another group to see something in Russia and it wasn't too long after that, that he began to try and recruit me to work at NCAR.

In 1964, I actually moved from Rand to NCAR and it was a very happy move, as far as I was concerned, because I think by that time, Rand was beginning to lose some of its glitter and it didn't seem as exciting a place as it had been when I first went there. But then, when you have been someplace for 17 years, you do tend to get a little stale. So, I was tired of looking at those same walls for 17 years and moved on. But, it was a good experience.

One of the things which I would have liked to have carried over to NCAR more than I was able to was the idea of scientists working together on a project, a big, multidisciplinary project. You might call it big science into intradisciplinary science.

Rand was very good at organizing projects which cut across disciplinary lines and were devoted to practical problems, and I kept trying to sell this to the scientists at NCAR, that is, that the things we were working on were also multidisciplinary and I made some progress, but it never really caught on and to this day, it hasn't really caught on that NCAR is capable of doing bigger science than it does, that is, more intradisciplinary science.

For instance, one example was that I had a group of excellent atmospheric chemists and they would go out into the field and make measurements of what was in the air, what trace gases and aerosols were in the air, and then they would come back and I would say, where did the air come from? What was the history of that parcel that you were sampling, St. Louis or wherever it was, and they wouldn't be able to tell me. So, finally, we were able to convince one of our top meteorologists to work with the chemists, Ed Danielson, to tell them where the air had come from. He was very good at this kind of thing.

This was just one example of the need that I felt, and was only partially successful in convincing others of to work together across disciplinary lines.

DROESSLER: What was the extent of your fist duties or opportunities at NCAR?

KELLOGG: When I first went there, it was as director of the Laboratory of Atmospheric Science. There were really three divisions: the Laboratory of Atmospheric Science, the High Altitude Observatory, and The Atmospheric

Technology division.

In essence, my work was to direct all the meteorological atmospheric chemical, and atmospheric cloud physics research for NCAR. I did this for nine years.

Then there was the reorganization, which is now well known. The reorganization was prompted by the joint evaluation committee and the Laboratory of Atmospheric Science was reorganized into four or five separate smaller divisions. I went away on sabbatical leave to escape the smoke, as it were. I was grateful that they let me come back as a senior scientist.

DROESSLER: What do you do now as a senior scientist?

KELLOGG: Well, last February, I retired as a senior scientist. Ever since the study of critical environmental problems, when I became really concerned about the theory of climate and began to study it and became involved in modeling the climate system, this has been my field, and I am still working in this area, still publishing off and on and still giving lectures on the subject. The thrust of my work has been to try and take a couple of steps beyond what other climate modelers have done. They have been concerned with just temperature change. Well, it seemed to me that if you changed the temperature, obviously, you are going to change the circulation patterns and the rain/snowfall patterns and the soil moisture patterns. This is really more important than temperature change because it's the soil moisture that determines where things can grow. Fortunately, the climate models calculate soil moisture. So, we get this information about how soil moisture may change as the world gets warmer in the future.

This summer, in the April issue of Climate, an article on this subject came out jointly by me and a Chinese colleague, Don(?), Su(?), Zow(?), and it takes the results of five climate models and compares them to see how well they agree on predicting future changes in soil moisture.

DROESSLER: Is your climate change research, has the thrust of it swept into the international area? Do you work with colleagues from other nations and meet with them occasionally on committees, IUGG committees?

KELLOGG: Yes, very much so. In 1978 and 1979, I was spent a year working with the World Meteorological Organization and my title was Advisor to the Secretary General on the World Climate Program. My job really was to draft legislation for the World Climate Program, which was then approved by the Congress of the World Meteorological Organization in 1979. So, that was very definitely international.

At the moment, downstairs, I have my word processor and through a telephone line which goes from here to New Jersey, to Darmstadt, to Moscow, I am in touch with a group of meteorologists and policy makers in this country who are

interested in the policy implications of climate change and also in the Soviet Union. Professor Raoul Sagdea, who is head of the Soviet Institute for Space Research, is the official chairman from that side. Walt Roberts is the official chairman along with Rusty Swigert, the astronaut, from this side.

We have a year-long dialog on climate change and its implications for not only Soviet-US policy, but also changes in policy worldwide, for example, the changes between the industrialized world and the developing world.

DROESSLER: When you say a year-long dialog, you mean, do you prepare something every week and then enter that into your computer and that's distributed to our colleagues?

KELLOGG: We are supposed to be making our contributions weekly and right now, Michael Bedico(?) and I are co-chairs of sort of a subcommittee which is putting together a climate scenario that we can use as a for-instance for the policy makers to chew. We would say, these are our best guesses about the range of changes that will occur and then the policy makers can use that as a basis for any statements they might want to make.

DROESSLER: Well, it continues to be exciting and you continue upbeat, don't you.

KELLOGG: I hope so.

DROESSLER: I would like to ask who is Will Kellogg and when and where were you born and who are your parents and what areas were they following?

KELLOGG: I was born on February 14, 1917, Valentine's Day in the town of New York, Mills. Nobody has ever heard of New York Mills, except for the people that live in Utica, because it's a suburb of Utica, New York.

I went to the Utica Country Day School and then to Brooks School, a prep school in Massachusetts. I then went to Yale, where I majored in physics. My first year out of Yale, I went back to Brooks as a general science teacher, to take over the general science teacher beaching while the regular teacher went on sort of a sabbatical from Brooks.

Following that, I went out to Berkeley to do graduate work in physics. I always assumed that I wanted to be a physicist and I was inspired by my Brooks teacher and also by some very fine teachers at Yale, including Allen(?) Waterman, who was one of my favorite Yale physics teachers.

I think that I already mentioned that while I was at Berkeley doing graduate work in physics, I became interested in meteorology and signed up for the meteorology course at UCLA and became an officer in the Air Weather Service and also a

pilot.

While I was still at Berkeley, I met a very attractive, blonde girl. Yale, of course, had been sort of a cloistered environment. There weren't very many women around, so my eyes were boggling at the Berkeley campus and all the beautiful women. So, I ended up with one of the most beautiful of all. Her name was Betty Thorason and we became engaged while I was still a cadet. In those days, a meteorology cadet couldn't be married -- why I don't know, but you couldn't be -- and so on February 14, my birthday, we got our commissions and gold bars pinned on our shoulders, and I dashed to the Thorason home and was married that same afternoon.

DROESSLER: She was a student at UCLA?

KELLOGG: She had been a student at UCLA and she was doing graduate work in English at Berkeley, getting her teaching credentials at Berkeley. So, we were both graduate students.

DROESSLER: And your children?

KELLOGG: Betty, or Thor as I called her -- her name was, after all, Betty Thorason and everybody called her Thor -- we have five children and five grandchildren. We have three boys and two girls of our own, and three boys and two girls grandchildren.

They are spread around to some extent. Our older daughter is on the faculty at the University of Hawaii, with her husband.

DROESSLER: What's her first name?

KELLOGG: Judy. Our son, Joe, is out in California. He is a building contractor, but also going to continue his studies and do graduate work in business administration starting next year.

The others are here. Karl, my oldest son, has a Ph.D., in geology and working with geological survey. His wife, who has a Ph.D. in education, is in charge of the science curriculum for the Adams County. They have a charming little son, whom we babysit whenever we get a chance.

Our daughter, Jane, lives right near here, in Lafayette. She has two children. Patrick, the older one, is a junior in college now; Kate is just entering junior high school. So, we see a lot of them.

Tom, our youngest son, is now back here working in Denver. He is one of the new generation of whiz-kids when it comes to computers, and when he gets together with my son-in-laws they start talking computer talk. I feel I am in

another world.

DROESSLER: It certainly is another world, a new world. Do you have brothers and sisters?

KELLOGG: Yeah, but they are all dead, two sisters and a brother.

DROESSLER: What was your father engaged in?

KELLOGG: My father was a third-generation lumber dealer in Utica, New York. He ran the Charles E. Kellogg and Sons lumber company and must have made a pretty good living.

My mother -- her family ran the Walcott-Campbell Spinning Mills in New York Mills. They owned them and ran them, and that, obviously, was a very wealthy operation until the Depression came along and the industry moved south. These big mills are still standing in New York Mills, half collapsed in places, but they are a memory of the past.

DROESSLER: Will, thank you very much for the pleasure of being with you and talking with you this afternoon. This is the 18th of August, 1988, and I have just completed an interview with Will Kellogg at his home in Boulder, Colorado. This is Earl Droessler, giving thanks to Will in appreciate for the time and having long had a deep respect for a person and his professional contributions. Will, I think you have always been among the giants of our profession in meteorology and in your retirement, I wish you many, many more, and new careers.

KELLOGG: Thank you Earl, and before you hang up, let me say that you and I have worked together off and on. I can't remember when we first met. I know you were with the NSF when I remember you most vividly. Our paths have crossed many times in the past.

Now, of course, you are taking on this job of talking to people in order to preserve something of our heritage in meteorology, and as chairman of the NCAR history committee, whose piece of machinery I believe you are visiting today,. I want to say that Nancy Gauss and I are so appreciative of what you are doing, going around and giving these wonderful, warm-hearted interviews with important people. I am not that important, but I am glad to be included among them.

END OF TAPE