American Meteorological Society
University Corporation for Atmospheric Research

TAPE RECORDED INTERVIEW PROJECT

Interview of Thomas F. Malone
February 18, 1989

Interviewer: Earl Droessler

Droessler: It is February the 18th, 1989, and this is Earl Droessler. I'm going to be interviewing Tom Malone, and we're meeting at St. Joseph's College in Hartford on this beautiful campus, where Tom is a scholar in residence. And this morning we will be talking about the progress in the atmospheric sciences since World War II. And over the same 40 year period, the development and activities within the American Meteorological Society, and then we'll develop a focus or an emphasis on the activities and the happenings during your term as President of the society, Tom, which were the years 1960 and 1961.

Malone: Yes, I was President in '60 and '61. And I think it was more the seeds that were planted during those years, rather than the spectacular outcomes during those years. One of the important things we did was to take the concept of Paul Klopsteg to use the term "atmospheric sciences" and we had a fierce battle, but we eventually decided to change the name of the respected *Journal of Meteorology* to the *Journal of Atmosphere Sciences*. It caused great pain to Werner Baum when we did that. And I remember very well the council meeting when it happened. And the critical intervention was made by Lou Battan because Lou was a dyed-in-the-wool meteorologist. And when he came in late to the meeting, and when his opinion was asked, he said yes, this was the way to go. And it was an auspicious move and characteristic of the insight of Lou Battan in perceiving the direction in which things were moving. And being proactive, rather than reactive. And I think that that has been the hallmark of the American Meteorological Society over the last 25-30 years. And of course the groundwork was laid for this by Henry Houghton when he assumed the presidency back in 1946, I think it was. And there was divided view in society about whether it should expand and establish a secretary in Boston and the treasurer at that time was very reluctant to – Burl, Ralph Burl, was very reluctant to. And he wrote a strong letter, which was published in the bulletin, but Henry was not deterred. And so we were trying to build on what Henry started. And this seemingly small

step of changing the name of a journal, but anyone who's ever changed the name of a journal knows how very hard it is. And that experience carried over for me because I became President of the American Geophysical Union also in 1961, and for a couple years I carried both presidencies. And there too we followed the initiative set up by Lloyd Berkner, and it was during that period that the whole stable of publications of AGU had their real origin. It was a period when Phil Abelson and Jim Peoples were editors of *JGR*. And they did the most remarkable job of any editors of a scientific or technical journal. They did so well that when they were searching for an editor for *Science* magazine, and when the president of the Carnegie Institution talked to me about this, I was able to wax eloquent on the talents of one Phil Abelson who went on to become probably one of the greatest editors that *Science* has ever had.

One of the hallmarks of the Society had been communications within the scientific community and communications to the outside world. And I would say that the barrier, if you can use that word, between the scientific community and the outside world is eroding. We can no longer view the scientific community as sequestered from the larger society in which it is embedded. And one of the tasks that now is coming front and center is the task of fostering a dynamic and creative interaction among science, technology, and society. And this process had lots of rifts deep in the American Meteorological Society. And I would say the society has been in the forefront in trying to address this. Its statements that came out, its meetings, its publications. All our earnest and effective attempts to establish this kind of dynamic and creative interaction. And I think it's reflected in the growth of the Society over the last 25 years.

Well, the work with the Society and the work with NCAR and UCAR, which deepened my conviction that an essential role for the meteorological community was to foster international collaboration. Since we deal with a medium, which is intrinsically global, it's only natural that the scientists and professionals involved with that medium take a global view. And this has been the hallmark of the Society and is developed over the years with the meetings held in China and other parts of the world. The effective role of Ken Spengler and the WMO in Geneva. And this new window of opportunity opened up on this. One day I was sitting quietly in my office, on the 15th floor of one of the Traveler's buildings—on the 17th floor of one of the Traveler's buildings, and I had a call from Jule Charney. And he said that Bruno Rossey, who was a famous cosmic ray physicist from MIT, who had been working as a sort of consultant to Jerry Wiesner in Washington, had approached Jule and Richard Goody about some thinking going on within Jerry Wiesner's shop (he was a science advisor to President Kennedy at that time) about initiatives that might be mounted internationally. And that one of the areas they thought had some potential was the atmospheric sciences, and really would like to

talk to us. And so he said, can you come up? So I came up and Richard and Jule, and I sat down with Bruno Rossi in his old office in those obsolete World War II buildings that were frame buildings that were constructed as emergency measure to house the first signs of the radiation lab. We sat down in those rickety, old, wooden, barracks type offices. And the conversation was rather desultory. And then we mentioned the possible role of the -- the human role in influencing the atmosphere. At that time weather control, conscious weather control was quite a live topic as a result of the work of Captain Orville, Shorty Orville. And his weather control board. And also we were aware of the unconscious effect on the atmosphere. The increasing global human activity--and as soon as we mentioned the possibility that there could be some kind of human intervention conscious or unconscious in the behavior of the atmosphere, Bruno's face lit up and he said hey, there's something that should be explored. And I'm going into this because this is the seed of GARP, this meeting in Bruno Rossi's office in the old, ramshackle, wooden, barracks type buildings on the campus of MIT and Cambridge. It was the place that first came to my attention.

And so Bruno invited me to dinner at his home that evening. We had a nice conversation and he was very much interested, and asked us to prepare a little think piece on this. And I was very conscious as President of the American Meteorological Society of the need to have a constituency in the community and not come up with some idea of very few people. So we convened over in the Rossby Room of the society and a little group that involved Dave Johnson from Washington, I think Harry Wexler came up, I think that Morrie Tepper was there, and Sverre Petterssen came in from Chicago, and Bob Fleagle came in from Seattle because I had been very much impressed with Bob Fleagle. He had a terrible time getting there. He had to take the train. This was 1961 and he rode the train all night and missed his connection to Chicago, rode the train all night from Chicago to Boston. But he was there and Henry Houghton was there because I felt that with Henry Houghton's concurrence and blessing, whatever we're about would be well received. Not only because he had such a high reputation in the field, but because the judgment. The judgement. It wasn't just his personal charisma, it was the fact that Henry's judgement was generally accepted as being as near infallible as any human judgement can be. And I don't mean to put him on a pedestal.

So we drew up a plan then for international cooperation, and it was in preparation for our meeting that President Kennedy was to have with Khrushchev in Vienna. This was in the spring of 1961. And we prepared a little paper, and I went down and met with people in the executive office building, and Jim Killian was -- no, this is after Jim Killian's time. This was Jerry Wiesner's time. Anyway, a paragraph or two of cooperation bilateral, Soviet-U.S., was the first thought. Well, as everyone knows, the

Vienna meeting between Kennedy and Khrushchev was disastrous. Khrushchev thought that this young man was -- could be put in his place by an experienced, older person. And nothing came out of it. But the seeds were planted in the office of the science advisor of the potential of the atmosphere sciences as an element in international relations. And when Kennedy was in invited to address the United Nations in September of 1961, the idea was picked up again. And I was invited down to Washington to meet with Dr. Wiesner and Dr. Rabi, the famous physicist from Columbia, and Harlan Cleveland, who was Assistant Secretary of State for International Organizations, and a very immaculate person, and Dick Gardner who was Deputy Assistant Secretary to Harlan Cleveland. And some people from the Defense Department and the CIA was represented there by a man who's since passed away. And Det Bronk was there because he chaired a panel of the PSAC, President's Science Advisory Committee, on international programs. And I made an impassioned presentation. I thought it was impassioned, but I noticed that Rabi went to sleep. And there was a terrific argument between Jerry Wiesner and Dick Gardner about something. I can't remember the details about it. It was pressed with the vigor which those two went at each other. But in any case, they decided that something should be done. And that this was a fitting topic to be introduced by President Kennedy at the United Nations.

And I was on my way to Australia at that time to participate in a weather modification conference sponsored by Taffy Bowen, or led by Taffy Bowen, and (inaudible) Sir Frederick Smith of the CSIRO, and Horace Byers and I flew in a plane through Honolulu to Sydney, and I had some papers on my speech down there, which was going to urge some kind of an impassioned effort. And I had told them that I would put up on the little trial balloon at Canberra , and then see what _____ it would be a signal to go ahead. So the response was good at Canberra. And it was -- meanwhile then, I had stopped after my exciting encounter with all of these important people in Washington. I slept overnight at Sverre Petterssen's home in Chicago before I went to Australia. So I shared all of this with Sverre, so in my absence they could meet in a small group. Sverre and Jule Charney, and a few others met down there, and said yes, these ideas were worthwhile pursuing. And the end result was that there were two sentences in President Kennedy's United Nations address. Something about seeking international cooperation, then understanding the atmosphere, and possible control of the atmosphere. And of that, those two sentences then were the leverage upon which the whole _____ thing was constructed. Because there was an attempt later on to focus this program within the World Meteorological Organization. And I had deep convictions that the scientific community should be involved. And at that time I was working as volunteer assistant to Herb Holloman, who was

Assistant Secretary of Commerce. And the question was, basically, should the follow up resolution to his speech address WMO exclusively, or should it be addressed to WMO and the International Council for Scientific Unions. And I argued very strongly with Herb that it should be -- it should involve the inter-governmental organization because they had logistic strength and stability, it should involve the non-governmental organization because they have imagination and freedom to communicate and it's the complimentary characteristics of these two organizations, which taken together produce a better program than any one acting unilaterally. So I was commuting back and forth in our little Beechcraft Traveler's had to Washington, and I had a friend that called from Herb Holloman one day, saying, look, Dick Gardner who's deputy to Harlan Cleveland, is coming over and we're going to talk about this issue. Get on the plane, get down here. So I had went over -- got the plane, I jumped on, and I started off for Washington. When I got there, the meeting was over. But the briefings I had given Herb were persuasive. And he and Dick Gardner was a very strong person and Greg _____ and I'm not putting him in the devil's advocate position at all. They had an Indian wrestling contest of some kind in Herb's office and Herb won. And the decision was to focus the upcoming resolution on both the non-governmental and the inter-governmental organization. And that was terribly important because in my view, it was the combination of the two organizations that undergirded the success of the global atmosphere service program.

So we pursued that over the next several years at meetings -- Vienna meetings and Florence meetings, and meeting Mar del Plata, where COSPAR was meeting. And I was trying desperately to get it implanted somewhere. It was kind of difficult because ICSU is very complicated. I remember a visit with Horace Byers that I made to Toronto. We took the Traveler's plane together, which was my principal vehicle of transportation those days it seems. And Horace and I jumped on the old Beechcraft and flew up to Toronto to talk with Warren Godson, who was Secretary General of the International Association of Meteorology and Atmospheric Physics. And the -- the nub of the conversation was should the role of this program be centered in the International Association of Meteorology and Atmospheric Physics, or should a broad based committee of some kind be put together? And Warren was adamant that it be in the IAMAP, and he gave us a wonderful dinner we've had all day at his home, but we never resolved and ended up going to his. This topic was pursued then, subsequently, at the 13th General Assembly of the International Union of Geology and Geophysics in Berkeley in 1963. And I was president of the American Geophysical Union at that time, so we had to be the host. And it was a very interesting meeting. It was very complex. It almost floundered on the question of China and East Germany. Professor Belousov? of the Soviet Union was President of the

International Union of Geology and Geophysics, so which the Association of Meteorology and Atmospheric Physics is one of the seven associations there. And he's a very dedicated scientist, but he said unless we let the East German representatives come, and unless we excluded the Taiwan representatives in contrast to the People's Republic of China, that we have to postpone the meeting because of the principle of free circulation within ICSU, which is very important. And that created a panic, and I was dispatched to Paris to meet with [Lockelvere?] who was -- and his name is important because he did something at Berkeley that was crucial, he'll never be recognized by. He was the secretary general of the International Union of Geology and Geophysics, and active in the IGY, as you probably know. So I flew over to Paris to talk to Lockelvere, and so he put in a telephone call to Belousev, and Mike Baker, and Lockelvere, and I sat in Lockelvere's apartment for about six hours waiting for the return call. And he had a model of Johnnie Walker Red Label so it was a pleasant, affable evening. I described that in a testimony I wrote to Mike Baker we established a report on that evening pondering the question of the big international meeting of IUGG going to collapse or not. And we go through on the telephone and Belousov ironed out the differences. Worked out a compromise so the GDR people could come and the Taiwan people were there but they didn't have voting rights, and it was compromised when they had the meeting. And it was a very interesting meeting. One of the problems was the election of a new president for IUGG. And as the chairman of the national committee, I had been instructed to get Joe Kaplan elected as president. And Belousov wanted someone else. Some feeling between Belousev and Kaplan, and the series of council meetings was very lively and very tense. I remember when Belousov came into the meeting, we had a big table, and he said gentlemen, fasten your seatbelts. That was his way of announcing the meeting was convened. And he prepared for a rough journey. It was a rough journey, but we -- and all of the little politicking is interesting. One thing that you remember the great oceanographer who was head of Columbia?

Droessler: Maurice Ewing.

Malone: Maurice Ewing was there. And Belousov wanted Maurice Ewing to be the president of IUGG. And Maurice Ewing is a great man. There is just no two ways about it. But I had my instructions from the committee to get that presidency for Joe Kaplan. And so finally we had the -- John Beckman had a party at his home the night before all this election took place. And so I had a chance to talk with Maurice there, and I told him what the position of the delegation was. So Maurice came back to my room that night about midnight with a letter saying unequivocally that he was not a candidate, would not stand, would not serve if elected. And he and his sister, who was Jim Peoples' wife, were -- Jim was there.

Anyway, it was a magnificent gesture on his part. And when I told Belousov about this, then he turned his attention to Lloyd Berkner. But Lloyd had already left, and of course Lloyd had participated in the decision to elect Joe. So that -- the -- not withstanding Maurice Ewing's statement Belousov proposed the name so I -- during the consummating of the election I read his letter of withdrawal, and we had already submitted Joe Kaplan's name. And showing how little things influence developments, there was a great big geophysicist from the Netherlands there, and he had some kind of a -- at the meeting, he had some kind of a warning or heart attack or something. And I had got his doctor for his wife importuning and he was all right. But we developed a very close friendship. And we were seated next to each other at the council meeting and he said, who should I vote for? I said vote for Kaplan. So he did. Kaplan won by one vote. And Kaplan went on to lead IUGG in a very distinguished matter. He presided at the subsequent meeting in Switzerland, he and Katherine presided in Zurich and Lucerne, and Geneva, I think it was. So that is all in the way of sort of background.

But there was a meeting of the International Association of Meteorology and Atmospheric Physics at Berkeley. And so I made the presentation of some kind of a special committee. But _____ and there was a very, very well know, extraordinarily able UK meteorologist at the meeting and his name was Peter Shepherd. And he had been at the Institute of Science and Technology over in UK, he was head of chair. And I think he was professor and brought B.J. Mason. And I could remember to this day when I made my pitch, Peter getting up and waving his big finger in front of me saying Tom Malone, if you pursue this, we will destroy you in this program. Which was a very serious threat coming from such a distinguished person in the field. So we debated the issue in the council. What IUGG should do. Should it take interest in this because they have oceanography, other areas in them -- or -- and we had Jean [Tulong?] on our side. But someone got up and made an impassioned speech saying it should go to IAMAP. And it passed, and my spirits just fell. I thought that it was all -- the aspirations, not just mine but people who had been involved in this were crushed. So it was a rather woeful evening. But the next morning the secretary general made his report to the assembly, and someone thought he pulled the rabbit out of the hat and he said that IUGG was going to mount a program in this. How he did it, I will never know, because that meeting was held late in the afternoon, and there were all kinds of receptions that night. But anyway, _____ wrapped it up. He gave a magnificent secretary general's report. He gave it in English and in French. I heard only the English part, of course. Incidentally, Belousov, who had never known anything except Russian before the IGY, which is only about six years away, gave his presidential address first in Russian so that _____, and he gave it in French; French and English were the two languages of IUGG. And then

because the assembly was hosted by the United States, he gave it in English. And then he gave it in Spanish. And I was told later he was prepared to give it in German, but he had the forbearance not to inflict the same speech. In fact, it was a tour-de-force by an individual who in a short period of time had carried on a vigorous scientific career, but had acquired enough proficiency in language to make a public address in four different languages, with a fifth language in Zurich.

Then what happened is that Joe convened the executive's committee of IUGG in London, and the next -- this was in September, which was early the next year. And they passed a resolution to set up a committee which would look at this thing; and it emerged as IUGG/ICSU committee. And another strong supporter and person who played an important role, and who was agreed as secretary general was George Garland from Canada, a solid-earth geophysicist. I'll come back to him later, because he was the person who focused ICSU's attention on the thing now called global change. In an address he made at the general assembly of ICSU, in 1982 in Cambridge, England, when he and Belousov were invited to -- it was the observance of the anniversary of the IGY. They both gave addresses and George said that we had learned a lot about the interaction of the different parts of _____. And we've learned a lot about the segments. A lot about ocean, atmosphere, solid earth, it was time to put them together, and to bring in the importance of man. It was a -- and I've cited that, and it's quoted in the book "Climate and Change," which emerged, which was the progenitor of the whole climate -- or whole global change program, following a subsequence general assembly of ICSU in Ottawa. You know, you've asked for some names of people important, and these are names that won't show up in the literature on this. But I know that they were important and Joe Kaplan.

Droessler: Was George Garland then president of ICSU at this time?

Malone: No, George Garland was at the Berkeley assembly. He was elected Secretary General to succeed Lockelvere, and his role was important because we had to put together a little committee to pursue this. And we had Bert Bolin as the Chairman. We tried to involve him through COSPAR. A lot of disappointments on that score. And I was the secretary, and we had Jule Charney on it, and we had Peter Shepherd on it. And his contribution was very important. Not only substantively, but his participation, his stature. And I've often reflected wryly on his waving his big finger at me at Berkeley, and then his role in advancing this, which was a very enlightened interpretation of the logic of establishing a diverse base for this program. Narrowly constructing this base. So the meeting was convened in Geneva in 1964. And Bert and I went over, and we were interrogated by the officers of WMO on what we were doing there. And we felt a little lonely, but our presence was bolstered enormously by the

fact that Joe Kaplan was President of IUGG, and George Garland, who was Secretary General of IUGG, attended the meeting. And they brought a credibility to this little committee in the eyes of the management structure at WMO, which was extremely important be cause we were being asked, you know, what are you doing here? But -- and Joe's role in this is not -- has never been recognized, although I have tried to build in some awards that have been under consideration by Joe and George Garland, he's retired now, but these -- their role like that of Bruno Rossi and Det Bronk when he sat down there in the Executive Office building and approved all of this -- I think were critical at the time as I saw them. So we met two or three times in Geneva, this committee did, and finally it produced a report which proposed an international program. And there was a parallel committee established under WMO and the two committees met at the same time and we had joint sessions. And they finally joined us, and there were some very important people on their committee, _____ the Soviet scientist, and Bugaev, who was deputy head of the hyrdometeorological service. Deputy Fedorov, who was a very powerful man not only in the hydromet service, but the political structure of the Soviet Union.

So we had the proposal, but it needed to be developed and fleshed out. And we didn't have very much money because IUGG had been funding our travel, and that was, you know, expensive. What we needed was a sustained summer studying one time or another. So I went to the Ford Foundation to ask them for a grant. Mr. _____ I talked to, and he listened, and nothing much. He didn't seem very responsive. So I sat down and wrote him a letter, and I said that I'm sorry I couldn't be more persuasive as Chairman and all this stuff. Anyway, he got on the phone and he called around the world and found out that this was a good idea and they gave us $75,000 as I recall. With that money, we were able to arrange a summer study in Stockholm at Shepper_____. And we got the people like Jule Charney, and the Soviet scientists, and UK. There were about 30 or 40 people that we got, locked them up there in this island for three weeks, and they produced this report.

I should say that the -- the real intellectual foundation for all of this was in the mind of Jule Charney. And there were enough people interacting with Jule, real substantive, that, you know I talk about meetings but one meeting stands out in my mind. It was a meeting we had in Geneva, and we were going on, and on, and on, and we went down to the silos and had a big party one night. And the next day was the major presentations. And it was a rather late night, and I was -- they had fondue and wine. (laughter) We drank a lot of wine, did a lot of fondue. There were a lot of large heads the next morning. And Jule, who was a notorious owl rather than a lark, he worked best at night and not so good in the morning. He

was supposed to make a major presentation of his basic notion that our theory had advanced far enough that to test the predictive capability, we had to have global observations analysis. That was the rationale raised on debt. But it was -- had to be fleshed out, this is more than just these words. And so John Sievers was on in the staff of the Committee of Atmospheric Sciences at the Academy, went down and rousted Jule out of bed, and he got up and he gave the most brilliant, persuasive address I think I've ever heard in my life. And it had an impact because Davies knew that, he was the secretary general, knew that Jule was going to be addressing this issue. And in that council room with WMO, there's a whole balcony up there where the secretary general can come and observe the proceedings without being conspicuous. And I noticed Dr. Davies, who's an honorary member of the American Meteorological Society was up there. And we had other -- we had the individual from UCAR was involved.

Droessler: Vin Lally?

Malone: Vin Lally. He'd been down in Antarctic with the balloons which should have navigated the southern hemisphere. He flew in and landed about 9:30 that morning and with some latest results of these balloon flights. It turned out that the balloons were not important ultimately, but they suggested the kind of technology which was then within reach. And Dave Johnson was there, and I would say that it was the combined effect of Jule Charney's intellectual trust, and Vin Lally's dramatic entrance landed in a 200 foot ceiling from the Southern hemisphere with these results on the map. And the marvelously affective presentation by Verner Suomi, who -- and this is what really jelled the thing and made it possible to go to the Ford Foundation and ask them for some support. So the conference was held, and then we were fortunate enough to engage to Rolando Garcia as the secretary general, and that's a famous, little story because we needed someone in Geneva to staff this operation. So Davies was in New York, and so was Rolando. And the Traveler's Insurance Company had a suite down at the Pierre -- Pierre Hotel. So I arranged for dinner with Rolando and Davies. And we had a marvelous evening, a long evening, very convivial evening. And at that session, we agreed that Rolando would take up residence in Geneva at WMO on behalf of this emergent committee, this ICSU/IUGG committee. And it would be a joint WMO/ICSU affair, which was important. And so Rolando Garcia was at _____ and put together the big report. And I remember him coming, and we had done a meeting of IAMAP in Lucerne. And Rolando arrived in his little Fiat with fifty of these great, big, thick reports of the _____ meeting, and the car was tilted over, hardly able to navigate. And so I was supposed to chair the sessions and discussions. And Bert was the presenter, he was chairman of the meeting at _____. And he made his typical effective

presentation. I remember very well as soon as he sat down, John Mason got up. And laced into that program, proposal, as only John Mason can. And I thought, oh, heavens, this is going very badly. But fortunately, we also had the director of research for the UK Meteorological Office -- what was his name? -- on our little committee. Oh, dear. And he was a very quiet man, but enormously respected. Published papers in the *Quarterly Journals of* _____ widely cited. So he got up and very quietly said, well, it may not do all that we thought it would. He had been in the—

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Interview of Thomas Malone

TAPE 1, SIDE 2

Malone: Anyway, called to a vote and it probably should have passed. And I -- as I described John Mason's questioning, I should emphasize that this wasn't because John was pigheaded, but it was a typical British tradition of sharply questioning anything that came up. And they do this as a matter of course. And ultimately John Mason's chaired this committee that was established, and he and Bob White were the two key figures in the governmental side. He as head of the UK Meteorological Office and Bob White is head of NOAA that made that thing go. If those two individuals hadn't put their effective shoulder behind this program, it never would have gone. Because if you got Bob White and John Mason in a council room at WMO headquarters, argue for something, it's going to go. And also they were able to put their sources to back up their sessions. I don't mean to paint John, I think it's typical of the UK approach as Peter Shepherd was pushing the idea that we were putting forth at Berkeley. John was questioning ideas at Lucerne. He had not been to _____. So then it went up to the IUGG council, which was meeting in Zurich, and since it was recommended by IAMAP, it passed. And went from there up to the general committee of ICSU. So then the -- finally the world had been traversed, began in '61. And it ended in about -- well, it was back to '67 I believe it was, when that meeting was held in Switzerland. And those six years were marked by retreats. (laughter) I remember one time -- two incidences that show that things don't always move in a nice way. Sometimes you go backward more than you go forward for a whole year. I remember being in Vienna at a meeting of COSPAR. We're trying to get COSPAR involved because the space component was very important, the space element. And Sutton, Sir Graham Sutton, who was head of the meteorological office at that time, he preceded B.J. Mason. He was there, so I thought well, I'll take Sir Graham and his wife out to dinner. Explain all this so that when I make my presentation tomorrow, he will know what it's all about. So I follow Ted Hesberg and he told me about the Street Hussar restaurant, which was right back of the Bristol Hotel where we're staying in Vienna, a very fine old hotel. And I treated Sue Graham, Mrs. --Lady Graham-- to a marvelous dinner. We had a very amiable conversation and I thought that went well. So I made in my presentation the next morning, and the Frenchman who was president of COSPAR said, we'll assess the discussion. Mr. Graham said I don't think we should do this at all. So I think the COSPAR part I think just collapsed completely.

Bob Fleagle and I were in Florence , and we argued this thing with COSPAR again and it was rejected. We finally went to Buenos Aires, and from there because we had some political bonds, we went down to Mar del

Plata, which was a summer resort but they opened up for this -- Rolando Garcia was the host at that time. And I made a presentation. Dick Porter was the U.S. vice president on that, and _____ was the Soviet vice-president of COSPAR. Always had U.S.-Soviet vice presidents of COSPAR; usually a Frenchman was the president. And I got nowhere with _____. So that what I'm saying is that I didn't like this have to be nurtured and you have to sustain setbacks. Which, for several years during that span from '61-'67, it moved backward more than it moved forward. And it finally merged, and we had in Shepperholmen, and this was something which Jule and I took a walk around the island, and we decided that neither one of us should be on this joint organizing committee which what's the proposal was, which involved WMO and ICSU. And Bert Bolin was the obvious choice to -- and we put people like Vern Suomi on it from the United States, Bugaev was on it from the Soviet Union. And we very carefully matched the scientific-technological competence and the political realities.

There was one person whose role is probably never been properly recognized amidst the disappointments in COSPAR. They weren't all disappointments, but one person who had attended the meeting in Boston, at the _____ House, with Bruno Rossi was Morrie Tepper, and Morrie was very committed to a space role in this. And he organized what was called Working Group 6 in COSPAR. And they designed the space element of this global observing system. And he doggedly and effectively carried that forward. And without the work of Morrie Tepper, I don't think the program would have succeeded, in spite of the brilliance of Jule Charney's concept or people like Vin Lally and Verner Suomi. So he's one of the unsung heroes of the GARP experiment. But if you go back and read, the Working Group 6 report is a very important document.

Droessler: And it was critical, too, that the GARP have a space-related element.

Malone: Oh, yes. Absolutely. Absolutely.

Droessler: Of course we had these satellites for the observational program. We could not really have covered the whole world without that.

Malone: That's right. So then the task was to work out an agreement between the inter-governmental and non-governmental committees. And I nearly wore out their railroads between Lucerne and Geneva because a mathematician _____ was secretary general of ICSU, and Davies of course was secretary general of WMO. So we drafted up an agreement, and I got _____'s agreement, and go to Davies--he had to change back and [Chanda?] wouldn't have a change. I must have made six round trips as a courier between those two important people. Finally we settled on a text and it was signed and approved by the ICSU officers

and WMO. And it provided the basis for proceeding with the GARP experiment, which took the course of the first GARP global experiments, the first tropical experiment, and then the FGGE experiment of our global experiment in 1979. So that the emphasis on the long road, but -- and so many people were affected. I simply can't pay high enough tribute to people like Bob White and John Mason, who brought to these exciting scientific plans a very solid foundation of thoughtful and-- thoughtful thinking and matching sources with ideas and getting the resources to carry them out.

So it was a remarkable experience of experiment, and it led to the World Climate Research Program. And again there's some unsung heroes on that. And one of the real heroes was Bill Gibbs of Australia. He chaired a little panel of the WMO executive committee. And ICSU was asked to put someone on that, and Harrison Brown was president of ICSU at that time, and he asked me where should he put us? And I said I'll do it, so I joined that panel and we worked very hard. And we had people like Murray Mitchell on the committee. Murray is a solid pillar of -- scientific thought in the climate field. And we had the German, who was a climatologist, _____, who was very articulate. Anyway, we had a very effective program, and we produced a report, and there are all kinds of IAMAP and within WMO. But basically I would say that it was the initiative of that panel by the WMO executive committee that was the real origin of the world climate conference and they were planning for _____ program. Obviously this had relation to the GARP activities, and so we met in London at the Royal Society to consider where it should go. And Bert Bolin and I were there and we made a -- I think Bert was honest to the panel, the _____. Our report was received, was acted upon, but by the time it emerged from the WMO Proceedings, you'd never know that there was a panel there. WMO took the thing over. It went and they had the big world climate conference and world climate research program. But it had to involve the GARP activities, and that meant having the joint organizing committee. At this time Bert passed the chairmanship on to someone else, so he and I met in London with the joint organizing committee to urge them to add a climate dimension to their activities. And once again, my old friend John Mason, gave an eloquent argument against the startling objectives of GARP. But I don't think there was any dramatic resolution at that meeting, but - _____ JOC emerged into the JSC, the Joint Organizing Committee became the Joint Scientific Committee. And that one had the responsibility for the supplement content of the World Climate Research Program. And John Mason was the chairman of JSC at one time. And I remember meeting with John Mason as chairman in [Guangzhou?], China. And at that time we were beginning to think about the global change program. And so we had quite a discussion there. But John very ably led the Joint Scientific Committee because he is a scientist of very great

intellectual stature and very effective. When he takes hold of something, it goes. But my role has always been kind of look ahead where we're going, and John's role has always been to say I should go this way and I don't think we should do this. And I don't mean to cast John in the role of a _____conservative, it's just healthy to have and I repeat this with the kind of debate that goes on traditionally in the UK and the scientific community. Not within the scientific community but in their Parliament they have these very, very strong confrontations of individuals.

Well, then meanwhile we had become conscious of the environmental issue. And the planning was underway for a great conference on the human environment in 1972 in Stockholm. It became the famous Stockholm conference on the human environment. Maurice Strong was the secretary general of the conference. And he is a man of very great ability, a born leader, a self-starter. And we thought that the scientific community should be involved in this. So Roger Revelle and I worked very closely together to organize within the Academy an international environmental program committee, which ultimately I chaired. And then we persuaded the International Union of Geology and Geophysics, and that was Jean [Coulon?], and the president of the International Union of Biological Sciences, which was Don Farner. Fond memory, passed away last year. That -- the ICSU should organize a scientific committee on problems in an environment called SCOPE. And Roger and I lobbied madly for this in Paris in 1968. It was a general assembly of ICSU, and it was put into place in Madrid in 1970. And Dr. Eric Smith from Plymouth Laboratory in UK was named chairman, and I was named secretary general. And that has produced a series of books on the environment that had laid the groundwork for all of discussions of global change to this day. I had hoped very much that we could get out of the Stockholm conference say an international research center of the UConn kind, but at the international level. And we'd call it the international center for the environment. And we had very powerful allies in the Congress. Senator Warren Magnuson, who was a powerful figure was very committed to this kind of thing. He had a different name for it, but it was the same thing. And we tried very hard to get it. And when I left the graduate deanship at the University of Connecticut to go to Indianapolis to a privately endowed research center, it was my hope that we could provide the seed for this kind of a center, which would look at the adequacy and the knowledge for decision making and interact with the decision making process, which must be at the governments and the private sector. But that idea didn't quite go. And the--Ronald Keay who was the executive secretary of the royal society, very active in SCOPE, was adequately opposed to a settlement of this kind. And it was shot down at a meeting of SCOPE in Paris. And one of the shooters-down was my good friend Mohammad [Musass?] of Egypt, got up and he has very great _____ in developing countries. And anyway, it never materialized but one of the

things that has happened moving in that void is the World Resources Institute in Washington D.C., which Gus Speth is the president of. And that has very substantial funding from the MacArthur Foundation. And _____ from Caltech is one that sits on the board. He has been very much interested in this kind of concept and they look at the policy side, but the other mechanism they don't really want to put into place. Interestingly enough SCOPE's efforts took a very significant change in Stockholm, and they decided to mount a major effort on biogeochemical cycling. And one of the enlightened voices was Sir Frederick Smith of -- not Sir Frederick Smith.

Droessler: The Australian?

Malone: No. No. Yeah, Warner. Sir Frederick Warner, a fellow who grew out in society and very prospective engineer in the UK. He was a treasurer of SCOPE at that time. And but the real thing happened there was that Bert Bolin's interest was stimulated. And it's interesting and Bert played a large role in the serious of book (inaudible). Sulfur, the carbon, nitrogen, phosphorate (inaudible) came out. And the interaction with the (inaudible) it's a treasure trove of knowledge. There's been advanced international effort to mount it to eliminate the carbon cycles. And to go back, my first account -- [Raspy?], he was getting great interest in the nitrogen cycle. And in my naivety, I wondered what in the world a meteorologist is doing worrying about cycles of nitrogen, carbon, sulfur, and phosphorous. But Raspy had a vision which transcended that of all of his contemporaries of the younger generation, which I wasn't a part of. But Raspy with great prescience recognized in the early '40s the fundamental importance of the cycling of chemicals in the atmosphere. And the fact that it is chemistry, which links the atmosphere and the ocean as you have energy and materials. And really it's chemistry, which is a -- and if you treat water as part of that logic chemical cycle, which it really is, then you would have a framework for treating the diverse systems of planet Earth as an integrated hole. And that is the foundation of earth system science, which the genius of France [Benetton?] has brought into hide visibility. This committee is (inaudible). So I underscore the foresight from a man like Raspy who recognized the fundamental parts of these and began to think and write about it. And of course he had a very strong influence on it -- Bert succeeded it. Bert was only a student, and Bert succeeded him in Stockholm when Raspy passed away. So Bert took that on, and the scope moved to (inaudible) in that area. There was another development of the international level, which involved meteorologists very unique way. And that had to do with the environmental impact of the nuclear exchange. And there had been an academy study on this, and they looked at primarily the high atmosphere. And then Paul [Trutson?] from Germany was asked by the editors of Ambio, the scientific journal in Stockholm, to prepare an article on the

lack of impact of the nuclear explosion, particularly in the stratosphere. Because Paul was a very knowledgeable student. Chemistry, chemistry is his strong point. A very imaginative individual. Paul was one of the -- he just was -- awarded the entire prize last year. His outstanding contributions to atmospheric chemistry. I had the pleasure of writing the justification for that award. And Paul. in thinking about this and doing some calculations, became aware that the [particulance?], which would be thrown into the atmosphere as a result of the fires and everything, would obscure the solar energy, the solar radiation, which should have an impact on the surface temperature. And he probably used to write thoughtful paper on this topic, and meanwhile Carl [Sagan?] would come in and used to do this from looking at (inaudible) storms and dust storms. And they held a big meeting in Cambridge, so I did not attend. But then they held a meeting in Washington in September 1983. And the idea of a nuclear winter emerged, coined by a meteorologist who was with the [Rich Turco?], who's now with the department of atmospheric sciences at UCLA with a little research company in California. It was started by Bill Graham who had just left the office of science advisor to president Reagan. It was Turco, who was also an atmospheric chemist and extremely imaginative. And they had a big meeting in Washington and what we did was to establish a satellite link to the (inaudible) group in Moscow who were also interested in this, of course. So we had a space link between Washington and Moscow. And I was a moderator in Washington, and [Belekov?], the vice president of the Soviet Academy was a moderator in Moscow. They had been working on this and the meeting worker over there was [Gulutz?], who was a full member of the Soviet Academy of Science. And I'd worked with [Obokov?] brought up by Obokov. I met Gulutz in Moscow in early 1964 when IUGG convened a little group to look at the structure of IUGG and I landed my first trip to Moscow. And some of that (inaudible) was this young physicist by the name of Gulutz, and we had -- he was just out of school and had been assigned to work in Obokov's atmosphere sciences laboratory. So much chagrin, I remember him saying do you mean the United States that you can pick who you want to work? He says I was told I'm a doctor of physics and I was told to work in the atmospheric sciences. He's become a very distinguished atmospheric scientist and an international leader. In any case, we had a very (inaudible) change and there was a lot of (inaudible). Meanwhile, we had begun focusing the tension of ICSU, the executive committee, on the scientific responsibility for nuclear war. And -- I've got to stop now. OK. The attention of ICSU general committee had been brought to the (inaudible) come up with a resolution called for a study the -- not just the environment (inaudible) but the whole issue with the war. Scope meeting and trial took a very strong position on this and said something should be done. So I convene a small group and we talked about it, and one of the touring figures in the United States was (inaudible) and that's Gilbert White from Colorado. And he encouraged me very much to proceed. So I

talked with Fred -- I talked with Ronald Kay, my friend from the Royal Society by telephone. And it happened that Ned Wearner, Sir Frederick Wearner was in his office. And I asked if Sir Frederick could lead -- look at this issue. And Ronald said he thought he could. And the president of the Royal Society was very supportive and made some money available so that a group was convened in London to examine this issue of environmental consequences in (inaudible) to war. And then it moved around, and the leader was Sir Frederick Wearner. He was the towering figure, but involved people like Richard (inaudible) and involved George [Gitson?] of the Soviet Union. And it involved people like Mike [McCracken?] from (inaudible), very, very extraordinarily able individual. To make a long story short, Earl, over the next two years there was intensive study of meetings around the world. Hiroshima, and New Delhi, and Leningrad, and Paris, and about 300 scientists from 30 countries were involved. And two volumes were produced. There are no consequences with the war. One pressed from the physical impact, one the biological impact. To make a long story short, the controversy over nuclear winter subsided with the issues of that report. Because the report was a detailed examination of this, and the bottom line of the whole matter was that in contrast to a globe dripping with ice, which was the popular perception. The impact of the nuclear change would be to lower the temperature sufficiently to disrupt the food production global. And the real essence of their argument was that catastrophic as would be the damage to the combatant countries where you might have hundreds -- a hundred million people in killed. In estimate physical damage the destruction of society. The impact on the non-involved countries would total in the billions. So that the stake of the world is very large in the arms race of the so-called super powers. And this conclusion was buttressed in two, large volumes. And then a special commission was appointed by the United Nations to examine this. And I was asked to serve on that commission, and so was George Glitzen, and so was Professor Yae from China, a very famous meteorologist from Beijing who also was involved in the Joint Science Committee of the world climate search program, and deeply involved in the global change thing. So that we met in Geneva. We met in New York, and prepared a report which was accepted by the United Nations without dissent. So that the scientific credibility of the impact to the non-involved countries, the changes of [complexion?] in the whole situation. A scientific credibility had been established by the scope study. And the political consensus had been reached when the United Nations accepted the [thought?] they said the United States and UK were skeptical about this and had voted against the study when it was proposed. But when the report was issued, there were no dissenting votes and it was accepted without dissent. So that we now have a situation where that is -- they clarify that it was one of the most interesting things that I've been involved in. Then the culmination of many of these things started in my mind when Richard Goody had a group met over at Wood's Hole and prepared a

report for NASA entitled, "Global Habitability". And it was a discussion of the earth (inaudible), the (inaudible) and the cycles. And it was proposed in major effort to look at the global environment. And large space component because we all know that great advanced in a field of science usually follow the introduction of a new (inaudible) -- what it is is [science?] trying to study. A telescope opened up to study the universe. The microscope and its prodigy opened up the study of living matter and biology. Now we don't have a single instrument for studying the global environment. But we have an array of technologies. We have the remote sensing capabilities, we have the communications facilities to move these data around. We have the computation (inaudible) to construct models of the physical laws and to study the processes and quantify the physical basis. And so that we have within reach a capability of bringing our globe under surveillance. Our larger dimension than we envisioned when we set up NKAR or when we set up [Garber?], we set up a world climate research program. And there's one sentence in that report which has been seminal to the evolution of this program, and it's cited in the booklets of (inaudible) change (inaudible). And I don't know who wrote this but I suspect it was Richard Goody, and it said that the human species lives on a planet characterized by change. And we are now entering a unique era in which one species, humanity, has developed the capability of influencing change on the largest, that is global scale. And to do so within the lifetime of a single member of that species, this is a very profound thought, and it matches the thought that was in 1982. It matches the thought that George Garland gave at this commemoration of the 25th anniversary of the IGY when he said that we now understand the different parts of our earth's system. It's time to look at the interaction and the role of men, which is exactly including man himself. And he said that (inaudible) has to reach outside itself and bring in other domains of knowledge. So those two pregnant thoughts prompted me to propose at the meeting of the executive board of ICSU in Stockholm in January of 1983 that ICSU examine this implications of these two (inaudible). Meanwhile, Herb Friedman had made an address to the annual meeting of the national academy of sciences in 1982, also commemorating the IGY. And he said that it is time to now make bold, holistic venture in organized, international research. To understand the interaction of the geosphere and the biosphere. The flows that (inaudible) international geosphere and biosphere program. And so we put together a little committee and we add up what's whole. And we're all prepared to have a big meeting there in June of 1983 and prepare an input to the follow up to the ICSU executive board meeting in Stockholm of January. They said well, let's have a workshop in this in Warsaw. And this meeting was [whole?]. So a bunch of us gathered there and (inaudible) I had dinner with Herb and his -- Herb is chair. And I'm at home and got up in the morning and called Stockholm, and did various things, then I began to feel kind of (inaudible). And I lost the power of speech. I had what you called a transient (inaudible) attack, TIA, it's a

minor stroke is what it is. And I went down to the desk with John, and (inaudible) was supposed to pick me up to try and get in touch with someone. Francie Johnson and his wife were there. See Frank had already gone, but I talked to Francis and she couldn't understand what I was saying. I just -- you know, you can't communicate when you have those things. So anyway, they knew there was something wrong so they got the ambulance, shipped me off to hospital, did all kind of tests, and Frank (inaudible) visit my hotel room. Rosie jumped on the plane, came back. I was determined to go to Warsaw, but she got our son from Maine, his grandson -- her grandson from (inaudible), shipped him back. I recovered. Anyway, the meeting was hell, and [Juan Roder?] was the (inaudible) who carried the message through Warsaw. And the upshot of the Warsaw meeting was that Juan and I were asked to lead a symposium on this topic in Ottawa in 1984 at the general assembly, September. So we convened a symposium, and all day symposium, and looked at the (inaudible) mounting a major examination of the interacting earth systems and the influence of human activity on the system. And it resulted in a major book published by the Cambridge University Press. Oh no, yeah, Cambridge University Press, and a (inaudible) press called global change. And Sir Frederick and I had done some (inaudible) in the Soviet Union about the [lunar?] issue, and also about this global change issue. And we met with Belousov, my old friend and advisory from Berkeley days with whom I had established a very warm relationship. His wife was an artist, she gave us some nice children's books that she had designed. And he's a marvelous person. Bluff, crusty individual, very warm. So he came to Ottawa with the draft of a resolution suggesting that this (inaudible) program -- international geosphere/biosphere program is studying global change in the amount of -- and so we took it and collaborated on refining the resolution. And we have an all day symposium on this topic, and then we concluded it with a sort of summation, and all of the Canadians made an absolutely dramatic summation of the whole thing. And (inaudible) the community and the ICSU passed a unanimous resolution to put together a study group and to develop a program. And that was done and we presented a program at the next general assembly of ICSU and [Burn Swisserman?] 1986 proposing a program of international geosphere (inaudible) studying global change. And that had as its objective to describe and understand the interaction among the physical, chemical, and biological systems. And their role in determining the unique environment of planet earth for life, the changes that are occurring in that system. And the role of human activity and influencing (inaudible) are very pregnant. And now there's a major program underway along those lines, and it's emerging, and Jim McCarthy from Harvard is chairing that. And there's a secretary established in Stockholm, where the (inaudible) is Tom Rosswell, Canada. It's many a name that should appear in these kind of liberations as the individual in Canada who delivered that absolutely dramatic summation of the whole day of -- there had been technical

summations at the end of the day, but I remember [Molene?] who was drafted and he did a (inaudible) job with us, and Bert was chair of the group, Bert Molene and Jack Eddy from -- Eddy and (inaudible) were two of the prime movers in developing that program. Bill [Fife?] from University of Western Ontario was a Canadian who summed that whole thing up and made this dramatic and almost emotional, but very persuasively (inaudible) about this. This affected the whole future for science and its impact in relation to society. So that's going, and now there's an inter-governmental mechanism, which is called the inter-agency panel on climactic change. And we're seeking now to weave together the rural climate research program and international geosphere/biosphere program. We had a very interesting little -- and one of the milestones in this -- that we're hearing is (inaudible) Washington in 1988 by the subcommittee on international and scientific cooperation chaired by congressman hall. And by some stroke of magic, they were able to get two Soviet scientists to come. [Kapayalkoff?] a geographer and Madame [Trostkaya?], the solar (inaudible) physicist. And we were -- I was testifying and Bernard (inaudible) was there, and so Bernard and I got together and we drafted a little note to the President of the United States, and (inaudible) to pursue. What took place there was that there was clearly a joint commitment of the scientific community to pursue this kind of thing. And it was very influential in establishing the kind of linkings that was a (inaudible) pull them together between these two sides of the communities. And it's all of this beginning to move and very recently as you are sure aware of when the summit meeting was held in Paris, Mr. Bush presented to his summit conferees a research plan to pursue this problem of the global environment. And about 1/3rd of the summit statement was devoted to the environment. And what is happening, Earl, is profound beyond all perception. You will recall a famous book by the -- by Paul Kennedy, Professor Paul Kennedy from Yale -- entitled "The Rise and Fall of Great Powers". And it's a historical treatment of the interaction among nations. The emergence of decay over the last 500 years, beginning with the Ming dynasty, moving to the Ottoman Empire, the European situation, the -- the [pox Britannica?] of the emergence in America, the emergence of Japan. And he points out, vividly and perceptively that the -- it is the economic vigor and the military might of nations and they're interconnecting that have determined the rise and fall of great powers.

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Malone: OK. What is happening today is that the driving force that is emerging, that is overtaking and subsuming the military might and economic strength of the global environmental issues. They are moving front stage, and they arise from the interdependence of this community of nations. And the particular feature is that the possibility that human activity, the number of people and their activity, is growing at a rate would suggest that the global environment itself is going to be changed by human activity. This is the greenhouse gas warning. This is the stratospheric ozone (inaudible). This is the species extinction. This is deforestation and their (inaudible). And what is right on the threshold now of broadening the definition of global change. It's not just climate change. It's not just the physical, biological, chemical changes. But the essence and the challenge of global change is to describe, to understand, to anticipate, and to manage the dynamic interaction among the great interlocking, physical, chemical, biological, and social systems. That regulate earth's unique environment for life, that determine the change in that system. You don't need to add it. If you got social system, then you've got the impact of humanity. On -- this brings our society to a kind of watershed as we near the advent of the third millenium. Homosapiens was emerged, say, half a million years ago give or take. In that half a million years, the number of human beings have grown from a handful to 5 billion. 5 billion in half a million years. And the age distribution and the birth rate, which is hard to change, suggest a very high probability that the number of people will double in the next half century, or 50 years, less than 50 years. So we are going to have to be able to accommodate on planet earth, within 50 years, as many people as we have accommodated in a half million years. And to get urgency to that, Earl, over the next 4 thousand days, because we're down to -- in my opinion -- we're down to urgency dictated by data sequence. In the next 4 thousand days between now and the end of the century, we're going to increase the population by about a million. So an added equivalent of another whole PRC to the world. Now not only is the population growing exponentially, we all know what exponential increased it by. But the capability of transforming natural resources into the goods and services that sustain life and give meaning to sheer existence: food, clothing, housing, shelter, employment, education, recreation. That also is going exponentially, because as a result of the scientific and technological advances over the last couple of centuries, in the understanding of matter of energy, of life processes particularly in the last couple decades, DNA, biotechnology, and of information, and the interaction among those. We know that energy and matter are connected. We know that energy and life processes are connected. We know that information and energy are

connected through the (inaudible). So that it is not only the understanding of these four domains, but their interaction that have put in our hands the capability of converting natural resources, and goods, and services. And (inaudible) growing exponentially, and it can grow at a rate in the developing countries, which would provide a doubling time of a couple of decades. But in the developing countries where 90% of that additional 5 billion will be found, you could add a doubling time of 10 years because you can grow -- as we know from Korea, and Taiwan, and places like that -- you can't do it (inaudible) a year. Large industrialized countries don't need to grow. At the present time, we are using diverting or wasting 40% of the full synthetic productivity, which is what sustains life on planet earth. If we double our population, we're going to begin to stress the carrying capacity of earth. So we are pushing the watershed. And it has harmful implication for the scientific community. And we're at this stage where you look at these things I've been talking about, that it is possible to envision three scenarios. We could virtually destroy civilization as a (inaudible) exchange. Killing millions of people, but destroying civilization. Secondly, we could strangle civilization with the affluence of our productive process, which (inaudible) the environment and exceed the carrying capacity of planet earth. And we would enter an era of chaos, and privation, and starvation. And a third option is to look at the probably futures in society on this planet earth, and then envision what might be possible. And then reflect on the steps that have to be taken to move the improbable over to the possible. And I have enough faith in the human destiny to believe that we are capable of electing that third option. It will be a tragedy if we didn't look down the road. Homosapiens have been here for half a million years. Why can't we think of at least another half a million years? We know the planet earth had been here for about 5 million years. And if you look at the energy in the sun (inaudible), the projections there, under the 5 millions years is so that planet earth is about half way through its life. We know that the dinosaurs survived for about 140 million years. Is it too much to think that the human species can't get by for a million years or half a million years for history, another half a million years? Are we not as imaginative, wise, thoughtful, perceptive, moral as the dinosaurs? I think we are. So the task is a daunting one. And it is a special challenge to the scientific and technological community, because of all of these advances. The utilization of energy to -- tinkering with the biological processes, information, matter. Texas, the big thing that's heading up the (inaudible) of matter. That have brought us to this era of exponential growth, and our task is not to charter course for humanity, but to put in place the knowledge base upon which policy decisions in society make. In our country, the private sector and the public sector. So we need to foster this dynamic, equative interaction among the scientific and technological community in society. And you need to work at the task of cultivating and nurturing. But I'd like to call the [ran?] convergence of the natural sciences, social sciences, engineering

sciences, policy analysis as the structure of the knowledge base. We need to examine our whole array of institutions if we're doing this. Our universities are crippled by their preoccupation and (inaudible) department structure. Just as the notion of a holistic, earth system science is emerging, so we must integrate our body of knowledge. And that's part of the reason that I was -- spend so much time last year reinvigorating [Sigma Psi?], the science research society. From just an honor society to a society (inaudible), cause we have 500 chapters of clubs as an outreach in the community. And we're interested in establishing international dimension in our society. We have indications of desire to establish clubs all around the world, and we're going to establish this international center of research in (inaudible) park, down where you live, which would be a central nervous system reaching out into the community. Growing on their ideas, assimilating knowledge. So that the role of the atmospheric scientist has taken on new dimensions, and just has Alan Waterman advised you many years ago to think bigger than you want to think, we must think bigger than the (inaudible) think. And I think the foundations of these are found in the Meteorological Society. Modest, little organization. Is it 10 thousand members? Something like that? But our whole tradition has been international, our whole tradition has been (inaudible), our whole tradition has been motivation as linkage between society and science. I got into meteorology because I saw the impact of the planets on our family in South Dakota during the (inaudible) -- the Depression years in the '30s. Our whole community was devastated by the weather, crops failed. And the soil in South Dakota was that if you started out with a bushel of wheat and planted your property, you found you've run out of seed. We almost did that in the 1930s cause of the (inaudible) fluctuations.

Droessler: So Tom, as we approach the end of this interview, I would like to return to the -- your work with the American Meteorological Society for some moments now. And I have several questions and themes for you to comment on, if you will. And the first of them -- of these -- is when you were the chairman of the industrial meteorology committee in the American Meteorological Society, I believe it was them that the society began the CCM program and the sealed approval program. And would you recall for us some of the activities surrounding those meetings of your committee?

Malone: Yes. I became chairman of the committee on industrial meteorology in 1956, just about the time that I arrived in Hartford to work for a business cooperation as a meteorologist and researcher. And I was deeply committed to ensuring the stature of the meteorologists with the -- in industrial meteorology, and became familiar with the designation chartered property underwriter for underwriters and potentially property insurance, or chartered life underwriter for the underwriter life insurance. It is a very prestigious designation in the insurance field, and one of the questions we had to ponder was would it be better to opt for licensing like

engineers are licensed by states, or should some indication of credibility and competence be done nationally through the professional society? And those were the two options in order to ensure the kind of competence that we wanted to characterize the contact with the meteorologist. Between the meteorologist and their clients. And we decided as a committee to pursue the root through the American Meteorological Society. And I can remember up on the 17th floor of the Central Bureau of Travelers, a meeting with [Newt Grarentz?], who was on this committee on the behalf of the department of commerce at that time. And we talked about this. It's -- I drew up a specifications for the certification and they call it certified consulting meteorologist which is a variant of the certified public -- chart public underwriter -- charter property underwriter. And so our committee deliberated on this and weighed the two. We left the door open for licensing, but we thought that the certification program might be the precursor step for licensing, if that proved to be desirable. Incidentally, it's been reviewed again by a committee that I just finished chairing and our conclusion was that it was not timely to institute a state certification program because of the lack of evidence that it really serves the function of protecting the public. And it's often used as a status seeking activity of a professional group. So we drew up the plans, and I was sitting on the council, and I was secretary of the MS at that time. And it went through the council, and it was -- has now a widespread -- I think in the amount of 3-5 hundred certified consulting meteorologists. And I think the time has come to reexamine that, and we recommended that the council of the exec committee of the MS now review that there's a tendency for the certification to go on, and on, and on. And without an attempt to renew the competence and it was more than just competence, it was integrity, and all these other areas. Which (inaudible), other attributes with (inaudible) very much from the charter property underwriters specifications. So here is the case where we're able to transfer from an industrial sector into a professional sector some of the best things in the industrial sector.

Droessler: Did this work on the part of your committee also cover the seal of approval?

Malone: Nope.

Droessler: That the MS grants to the radio and TV broadcasting?

Malone: No. That was done under the committee, but it was some of those separate operations. When we came down and organized the traveler's weather service, we had both radio broadcasts and television broadcasting. And at that -- in those days, the network weather was done by a man named Clint Ewell from Chicago. And he was on the big NBC news evening new show and carried on a very interesting conversation with John Cameron Swayze, who was the -- who was the Dan Rather, or Jenkins, or Tom Brokaw of his day. In fact he was really the -- John Cameron Swayze was the network newsman and Clint Ewell. So we thought that it would be desirable to have some similar but not the same kind of certification program for meteorologists communicating weather information to the

public. And we drew up the specification. Then we held the first meeting of the seal of approval group in Hartford, Connecticut in about -- I think it was about 1958, something in there. And we had Clint Ewell from Chicago, nationally and his name was a household term. He was known in every community in the country. He was very interesting. We had invited some people from industry as well as professionally and had more of the senior members of the MS here than has ever been assembled at the MS (inaudible). Henry Hilton was down, Shorty Orville was here, and [Park Merriweather?], who had been president of AMS is here, and we had councilors, and past presidents, and we had a marvelous meeting which we hosted the staff hotel. And our luncheon address was given by [Eleanor?] -- Abraham [Riddencoft?], who was governor of Connecticut in those days and went on to become secretary of HCW, and very prominent, nationally known figure. And he was an excellent governor. And that took place after the severe floods of 1956 which described the kind of responsibilities weather people have to communicate this kind of information. And we'd invited a number of people from industry, and to our surprise a yacht pulled up the Connecticut River. And the president of [Cargle?], disembarked. Cargle is the big, big drain company and I can't recall his name now, but he was the president. And he -- it was the first of some embarrassment to me because the traveler's insurance company insured Cargle. And when they found that one of their professional people down in the ranks was hosting the president of Cargle here, they were aghast that they hadn't sent a limousine or no one had ever knew about it. And I was naïve in the ways of insurance industry, and I didn't realize that when the president large, ensured came to town you rolled out the red carpet. But he came and he participated in the panel discussion, and then we had a fine time. And now I believe that meeting of the seal of approval is every year or two. The last one was in Atlanta, and it was hosted by [Larry Ross?], who's a -- does the weather for CNN and I must say is a very knowledgeable person. And I just enjoy watching him. And I think the seal of approval has had considerable impact. I would say not as much as it should. A number of stations around the country flip the seal of approval on the screen. And I was in Indianapolis a few weeks ago, and the meteorologist on their big TV station out there always shows the seal of approval, and I highly endorse that. So these were steps to insure the caliber of our meteorologists in dealing with the public or with a class. And also intended to raise the stature of our profession, and I looked back with some satisfaction on having been a part of that initiative.

Droessler: Earlier you mentioned just very briefly in passing almost that you did some volunteer work in Herb Holoman's office when he was in the commerce department. And that study that Herb Holoman undertook for the secretary of commerce had some far reaching effects. One of them was that it became time for the change of the leader of our government meteorologist, the chief of the weather bureau. And would you comment on what some of the things that took place there in Herb's office and how

out of these discussions are Bob White who is a protégé of yours became the chief for the weather bureau, and certainly was a very, very fine one.

Malone: There was a profound transformation in the weather bureau initiated by Herb Holoman who was a charismatic type of individual A very impatient man, a brilliant man came into commerce at the behest of Gerry Wiester to do for the industrial sector what the land grant and state extension (inaudible) and state extension agent had done for agriculture and the land grant act -- moral act of the 1860s. And he was interested in establishing an industrial counterpart of the extension service. The one of Herb's problems was he was so bright and so impatient that he tended to alienate the congressional chairman before whom he testified on these issues. And he delighted in correcting them, and did it with a smile, but a triumphant smile and a twinkle in his eyes. But some of the crusty congressional committee chairman didn't take kindly to being lectured to by Dr. Holoman, who had come from a brilliant career with General Electric to Washington at the request of Gerry Wiester. What came out of that was the decision at a little retreat of Herb, and Bob, and the senior people in the weather bureau to transform the weather bureau into the environmental science services administration (inaudible). And [(inaudible) Berkner?] I believe it was, and I, and maybe [Spilhouse?] were invited to be a consulted committee to that transformation. It was an ephemeral phase in the progression from the weather bureau to the national oceanographic and atmospheric agencies. And in a way, it was a little bit ahead of its time because environmental issues were not as prominent in the public mind in those days as they are today. But it was a conscious step toward broadening the involvement of meteorologists in science and in society. And is a tangible manifestation of the entire thing we're been talking about as the expanding horizons of both responsibility and opportunity for our fellow meteorologists. So it was -- I would say that Herb had the foresight to recognize that, and he had the happy circumstance of having Bob White to implement. Herb was a very imaginative individual, and sometimes his imagination leaped out of him of his capacity to follow through. But the combination of Herb Holoman and Herb White was a phenomenal success in expanding horizons of the old weather bureau. It was a step which one can only conjecture might have been favored by Francis [Rafferter?]. Herb came in with the idea that the weather bureau needed a shaking up of some kind. Dr. Rafferter had been there a long time. He was not only a consummate bureaucrat, but a very knowledgeable meteorologist. And he held that bureau together, and interacted with congress by the sheer power of his personality and his persistence. He was an inveterate memo and letter writer. And I have letters in my files from [Rike?] that I know he knocked out on his own personal typewriter. And he kept meticulous of all of his conversations telephone or a meeting in his office. And sometimes perceived to be set in his ways, I think we must recognize that Rike showed a clear capability of adapting to changing circumstances and seizing new opportunities. And I

would sight two examples: one is his unfailing support for the introduction of high-speed computers into the forecasting process. And he understood what (inaudible) and Rospy, and Charney, and Thompson, and Phillips were about at Princeton. And was quick to incorporate that into the procedures of a forecast. More important, even, was his strong support for the research effort. It was spearheaded by Joe [Smagrinsky?], a young protégé of Rospy's, and (inaudible), and Charney's at Princeton. And I remember when they set up the new computing center in downtown Washington, where Joe held forth. And this took a great salesmanship of the highest order in Congress because this kind of (inaudible) was not inexpensive. And it required a confidence in the ultimate pay off, and a patience to wait out the pay off. And I remember very well when John [Tucki?] called me one day and said what did I think of this unit of Joe Spang's that they were interested in something like this at Princeton, and Princeton University or Princeton community. And I said it was a jewel in the crown of the weather bureau, and with the remarkable foresight that John Tucki always demonstrates, soon the (inaudible) laboratory was safely ensconced in Princeton where it's become a national and international resource of great importance. The second illustration was Rike's readiness to adjust to the space age. He was urged, I'm sure, by Harry Wexler who had a keen sense of the potential utility of a capacity to view the atmosphere from outside. But if Rike had not been willing to incorporate this expensive dimension into the activities of the weather bureau, we would have been far behind and lost the position of leadership, which the United States community has traditionally held in the adaptation of the space age to modern needs. The really two big benefits in the space age, so far, have been its use in meteorology and its use in communications. Providing a link through satellites from one continent to another. And now the appearance of the white images flooding across the screen on the television weather forecast in the evening are familiar to all the public. But to incorporate this kind of a new technology into the operations required a capacity for adaptability, which was at variance with the perception that here is a stayed, conservative bureaucrat not willing to adjust to new times. In any case, Herb was very sensitive to the great merits of Rike, and arranged a magnificent dinner to -- for Rike's retirement. I thought that the time that Herb went overboard in his attempts at humor at the dinner, because I didn't think he treated Rike with the kind of respect which he wanted. Obviously there was great affection in the jollies that with which Herb had through the ceremony, but I must confess I was a little embarrassed when they put (inaudible). And for occasionally missed forecast, but in any case it was a meaningful event, and the papers that were represented there are still in fact, and I'm quite happy with the whole submission that I made in making the use of weather forecasters to practical purposes using probability forecast. So in any event, Rike was moved into the retirement domain, and in spite of a little happenstance at the banquet, it was done with dignity and affection, and

the kind of respect that his contributions to science, to government service, to public service created a (inaudible). As you know, he had been over in Scandinavia. Brought back the then new air mass (inaudible) analysis concept. And it -- he even brought in the weather bureau as a fresh of breath air back in the '30s with this new knowledge. And he had come out of the navy and been very involved with this lighter than air craft. So I think that the position of Dr. (inaudible) for meteorological history in this country is secure. And although some of us thought he should do this or that and I remember the great George report on industrial meteorology, which we presented with a flourish at a banquet in the American Meteorological Society in New York. And we were fortunate during those areas -- that era of rapid change to have a individual who could accept, adapt, and develop the (inaudible) of change as affected via (inaudible).

Droessler: Now I'm going to focus a little bit on your term as president of the American Meteorological Society. And one really almost earth shaking event happened during your time on watch, and that is the American Meteorological society took over the [Bulfinch House?], the headquarters, and moved off of our cramped and untidy office space on Joy Street. There must be an interesting story to tell about how the AMS acquired the marvelous Bulfinch House.

Malone: Well, there is an interesting story and I would say, first of all, that the credit for that initially belongs squarely on the shoulders of [Ken Spangler?]. It was he who felt that the third floor attic quarters of Joy Street and the American Meteorological Society were not commiserate -- with the importance of the society. And he kept watching on Beacon Hill for possible sites. And he learned that this house, which was owned by Eleanor Sears and had been used by the Boy Scouts, and treated rather badly, was sort of foot loose and fancy free, and up for grabs. This actually happened here before I became president. I was secretary of the society at the time. And it was during [(inaudible) Patterson's?] presidency, but I must call up my kind to take a look at this dilapidated building, and I -- it took a strong act of confidence in Ken's imagination and his ability to transform a vision into reality to go along. Once again, as I did whenever something major came up, I turned to Henry Hilton, and I just called up -- Henry, I said, would you come over and join us in taking a look at this building? For reasons I mentioned before that his judgement is so good, and it has been so good that the perception of the judgement is even better than the judgement. And he is viewed by members of our society as being richly infallible as he would be the first to say not although I can't ever recall a time when he was fallible. But you never know his flaws, and he (inaudible). So Henry took a look at it and he thought too that it was possible. Then there were trustees who had to make the recommendation to Ms. Sears, and so I was called up from Hartford to meet with them. And so I put all -- on all my dignity as a corporate person and took a car to a beach and flew up to Boston. And I remember the flight very well. We had sort of a (inaudible) pilot who

eventually was killed because he wasn't careful enough. We landed at Logan Airport on a cross wind on an icy surface. And I can remember it going down that runway and the plane -- airplane was about 30 degrees to the runway. And I look out the window and see where we were going, but the airplane was aimed about 30 degrees away from that, and we just slid down that runway, but we landed all right. And I made a little fuss over this coming into Boston an airplane frankly to impress the trustees that this was a very, very -- organization had responsible officers who flew around in airplanes. I must be very candid, I was thankfully seeking to impress the trustees. And so we had our meeting and it seemed to go well. Then I pleaded I had to rush back to Hartford on my plan to carry on some business. So whether that little bit of (inaudible) standing had any impact on their decision or not, I don't know. But it was a valiant attempt to try and support Ken's imagination, recognition of potential. There was another incident with that, which I recall with some mixed feelings. When we got into discussing it with Ms. Sears, she had $30 thousand as I recall which were going to come with the house and were used for refurbishing inside and she's in really, very bad shape of water running down the carpets, broken handrails.

Droessler: Terrible.

Malone: (inaudible) left that in terrible shape. She was a very wealthy woman and eccentric. And it occurred to (inaudible) and to me that possible we should cultivate her more than just by taking this house off your hands at $30 thousand. So we had met her and had amiable conversation. And so (inaudible) and I decided to invite her out to dinner and to treat her out royally, and then see if we can approach her on expanding her -- we had the (inaudible) of the plum was on the tree would you say a half a million dollars instead of a measly $30 thousand, which I might say would have been in the reels of a possibility as far as not being really noticed in her network. So we arranged to come to her residence on Beacon St. in Boston, and she'd invited us in, and she fixed Martini for us with (inaudible) gin, which is apparently very high quality gin. So we started off the evening very amiable, went down to this restaurant between Washington and near Washington St. right around the restrooms and not inexpensive. And we had our wonderful conversation, wonderful dinner, and on the way down she was waving out of the taxi window to keep the people away, and she was an imperious female, I must say. And so come toward the end of the evening, we raised a possibility of about 300. Wouldn't it be a magnificent gesture on his part to endow the American Meteorological Society with a handsome sum of funds so that they could proceed to carve out their destiny on Beacon Hill, and Boston, and Mission. With the kind of resources which would match their ambitions. But I must confess that the price of the dinner was wasting. I seriously sad no. And so we go the house and it's to their lasting credit. Can't spend any of that, who's been transforming from a dilapidated, [bull finch?] to a asset on Beacon Hill. But we fell short, and all we have was a delightful

evening in the company of a very entertaining and very wealthy and very eccentric and very strong minded lady.

Droessler: Another thing that I think happened during your administration was the reorganization of the AMS to the bottom. This was accomplished under a study group headed by Patrick McTiger Khan, who is vice president of the American Meteorological Society, and I believe his recommendation for this complete organization -- reorganization came in right about the time you became president and had to be put into affect. Am I right, and what other comments would you like to make on that?

Malone: Well, you're mostly right. Although, I must say that the decision to do this study was part of the plan that I had for the American Meteorological Society. In other words, I felt that it was time to look through the organization. And I could think of no one better equipped to do this than Patrick McTiger Khan.

Droessler: Was it your vice president?

Malone: He was my vice president.

Droessler: Oh, I --

END OF SIDE

Malone: -- firm determination to make Patrick my successor as president. And I thought that it would be very useful and helpful to him in fulfilling that role if he was able to organize the society in a meaningful way during the time when I was the president. One could be the advocate for his plan. It's always easier to have a president who advocates someone else's plan than a president who advocates his own plan. And so I asked Pat to undertake this scrutiny. And with the (inaudible), which inevitably characterized everything that Pat ever did and does, he went at it in a very serious thoughtful fashion. And he put in place a structure, which I think has withstood successfully the test of time. And it elevated the activities of the MS to a new and higher level. And I must also confess that Pat was more conversant with organizations and international things than was I. When he came in with the term commissions, I wondered what he was really getting at. Because in my limited experience at that time, the function of commissions in an organization was kind of estranged to me. But Pat had been around a long time, and he knew how commissions can operate and pull together related activities and group them in a meaningful fashion. And so Pat worked very hard at it, and he presented the proper homework and the reorganization sail through the council without as I recall any great trauma. And my only regret is that Pat was adamant in not being willing to be nominated to be my successor. But he did leave a lasting monument to his tenure as an officer in the society, and that's the structure which I think exists today with very little modification from the one that we approved in 1962.

Droessler: I believe you're right. I think it exist almost in its entirety with small modifications here and there. During your time as president of American Meteorological Society, you also served as the president of the American

Geophysical Union. And these two sister organizations had a lot in common, and I've often wondered if at that time the beginning of the idea that perhaps the AGU and the AMS might become a single organization down the road sometime. Whether that thought process didn't get underway.

Malone: Yes, and as we go along, we are describing little triumphs and big disappointments. Disappointment in not getting that endowment from Ms. Sears. Little triumphs, maybe someone else's triumph for the reorganization of society. There was a window of opportunity there, and as you might imagine, the individual who recognized it first was Lloyd Gerkner, who had been president of the American Geophysical Union and preceded me. And it was obvious he had hand picked me as his successor. And when he noted that I was president of the American Meteorological Society as well as American Geophysical Union, Lloyd who loved to stimulate and manage scientific organizations said this is a time when we should consider an amalgamation. There had just been, and I think it was under his impetus, a reorganization of the east electrical and electronic engineers -- electrical and electronic engineers, which was accomplished, as I recall, with [Pat Heggerty?], who was president of Texas Instruments, and a very close friend of Lloyd's. Lloyd sat on the board of directors of Texas Instruments, he's one of the founders of the organization. And under Pat Heggerty there was an amalgamation of the large engineering company -- large engineering organizations. And Lloyd, who understood geophysics and meteorology better than most geophysicists or meteorologists felt that there was a discernable synergy, which could be developed my consolidating the two organizations. We arranged a meeting in Florida to consider this. And we invited a small group from the two organizations to meet with us. And Ken [Spangler?] and [Waldo Smith?] were along as guests of the meeting, but not as direct participants. It would have been probably not wise to include them, although they certainly would be consulted in the event that there were some serious steps taken. Both Lloyd and I were disappointed that that meeting did not prove affective. It was a case, I believe, of limited perspective on the potential of these two fine organizations. I don't remember all of the players in that. But one individual whose opposition was crucial was [Roy Kellogg?], who I believe may have been president of the section on meteorology at that time. I forgotten what role he was there. He did not recognize the potential for this consolidation. And without his concurrence, it would have been hazardous to push the proposition much further. The one thing we did not need was to have controversy developed in either organization. And it probably would have unless there were consensus among the principle players. I'm not accusing Will of being shortsighted, I'm just saying maybe we were not persuasive enough in marshalling our arguments. I could have fallen short. I don't think Lloyd ever fell short in being persuasive. But even Lloyd's towering statue and his -- the force of his personality was not enough to carry the day. I guess

I have to have mixed emotions. I think all of us, who had been nurtured within the ranks of the American Meteorological Society have a deep and abiding affection and attachment to the AMS. And to see it lose some of its identity because it's unlikely that the name of the emerging organization would have been meteorological. It would have been much more logical to have it be geophysical because we know that committed as we are to meteorology, that meteorology is a part of the geophysical sciences. And so that there would have been a regrettable loss of identity in the term meteorological society. I always think (inaudible) affect on this so the brilliant commencement address which John Gardner gave at Cornell University. It happened after this, but whenever I turn my mind back to the (inaudible) of that meeting to achieve a consolidation, I was impressed with the sagacity of John Gardner, as you know he'd been foundation president.

Droessler: Right.

Malone: He was secretary of HEW, but under Kennedy and he was a remarkable student of institutions. And he gave an address at the 100th anniversary of Cornell in 1968. And in that address he said he'd come to the campus with a great deal of interest. Found the scientist there who had invented a machine to project in the future, and he wasn't in a position to describe how the machine worked because the inventor was waiting to reap some rewards on the race track with his great invention, which prevented you to move ahead. But he said he'd been permitted to move ahead to the year 3000 and that he was talking with some people who were reflecting on what had happened between 1968 and the year 3000. They relented the fact that there had been a breakdown in the social structure a latter part of the 20th century, and chaos had emerged, and then it gave way to a authoritarian, militaristic type of government, which was around the world in that period. And then in 1968 it was more (inaudible) today. And he said scholars went back -- finally emerged and went back to a more humane, democratic form of government. But scholars began to reflect on what happened. Why the collapse of this civilization? And with great discernment, and this was a (inaudible) story. He said that the conclusion was that society suffered from a failure of institutions and the failure of institutions can be laid at the doors of two kinds of people. And the people were uncritical lovers and unloving critics. And the uncritical lovers smothered the institution with their embrace and sought to preserve it in all its forms and manifestations. And didn't want to change a single thing. And the uncritical lovers were skilled in demolition, but untutored in the arts of nurturing institutions. And the combination of uncritical lovers and unloving critics meant that a kind of rigidity set in, and that the institute -- it was an institutional failure. And this is a extraordinary, perceptive analysis of the way people group together institutions to make things happen. And the hazards in falling into the trap of this dichotomy of uncritical lovers and unloving critics. And Rosie and I just published a letter in the local paper in which we commented there on the role of the

Reverend Richard McBrian who's head of the theology department at Notre Dame, and he publishes a -- has a (inaudible) column in our (inaudible) paper here. And some people are not happy with what he writes because he calls the shots as he sees them. And we recalled John Gardner's comment and we said that what John Gardner implied is that we need loving critics. And we classified Dick McBrian as a loving critic of the church. And we said that the church needs loving criticism, it does not need uncritical lovers, it does not need unloving critics, it needs critical lovers. Those who nourish the institution, but don't try and smother it with their embrace, and are willing to put their shoulder to the task of an institutional renewal. And I think the lack of success in that endeavor was because we didn't have enough voice from the loving critics who would have looked at the complimentary strengths and weaknesses of these two organizations, and perceived that we're moving into an era where the boundary between -- among disciplines are crumbling. And this is a problem confronting every university today. It's a departmental structure in which your advancement, your salary, [tenure?] possibilities are contingent about the pursuit of disciplinary studies and publishing in specialized journals. And the world we're living in today is different. And it requires a sort of a kind of renaissance people. And it requires more than dilettante. Steve Snyder of NKAR had written very perceptively on this issue and issues the policy journal published by the academies. And he said that the distinction is that you have to get in the minds of the other disciplines and learn their vocabulary, their modes of thinking, their protocols. And only when you do that can you really be engaged in inter-disciplinary research. And inter-disciplinary research is a much more intimate interaction in which the representatives are just different disciplines, understand how each other think rather than bring it together, of bits of knowledge from different fields of trying to make a whole out of it. Herb Simon of Carnegie Mellon has said that -- and this is Simon's law that you can't be inter-disciplinary until you've published in the period journals all of the disciplines. That is an extreme, and Steve does not go that far. But -- and I think that this kind of discussion is very important to the world we're living in, and especially should be thought about by meteorologists whose field is intrinsically inter-disciplinary. You cannot separate the physics, the biology, and the atmosphere from each other. And we think that we're naturally and I would be saddened if we weren't leaders in advancing the frontiers of inter-disciplinary study. If you go back and read the blue book of NKAR, there was a cardinal objective there, which was to foster inter-disciplinary research. And I hope that when NKAR observes its 50th anniversary that it will be able to point with pride at some path breaking initiatives in doing just that.

Droessler: Along this exciting note of uncritical lovers in scientific fields, especially our field of atmospheric sciences, I think that we will end our interview with a hope and a prayer that our field will develop more and more outstanding, uncritical lovers like yourself, Tom. Who will help to lead

our common endeavors and that of the people that we serve into better institutional arrangements, and better programs of research, and research application. And I want to -- on behalf of the University Cooperation for Atmospheric Research and the American Meteorological Society, both of which organizations you have served so well and long with your effective leadership and your (inaudible)ship, to thank you warmly for these hours of oral history taping.

Malone: Well, thank you, Earl. If I were to leave a sort of epitaph to the community, it would not be my words by the words of a German philosopher 200 years ago who remarked that in his book, "The Critique of Pure Reason", that the human mind tends to address three inter-related questions. And the first question is what can I know? What's possible for me to know? And the second question is what ought I to do? Given the knowledge, which is possible, that's the (inaudible), what are the moral and ethical imperatives for action? And I ought to do it? What can I know and what ought I to do? And the third question is the question on which I base my confidence in the future. The third question is what may I hope? What can I know? What ought I to do? What may I hope? And it is because human beings have a unique capacity to yearn, to hope, to think better, and to respond to the moral and ethical imperatives. Respond with the question of what I to do? And because we are committed by the science we practice to place emphasis on the question what can I know? And this (inaudible) of questions seems to me to be something that should be indelibly imprinted on our consciousness as we go about the task of bringing the good things of our profession in science to achieving that level possible, which is a much higher state and consonant of the dignity of individuals, and the global (inaudible). Thank you, Earl. It's been wonderful to have this time to chat with you, reminisce, and I must say that from a vantage point which very few of our colleagues have, a vantage point of your own career, perhaps I have a greater awareness of the contributions that you have made in the diverse positions of responsibility that you have held. (inaudible), (inaudible) university, (inaudible), state. And I hope that you take a quiet satisfaction all of the contributions that you have made. Some of the things that we've been talking about were in response to your question I have been (inaudible) by my own participation, really were contingent and independent upon your own work. I mentioned earlier that the blue book of (inaudible) never had been finished. You had picked up the phone and said, damn it Tom, we've got to have that report next week. And that was just a small, very small but symptomatic instance of the kind of quiet but effective influence that you have observed over this field. And it's because you've been there, doing things, challenging all of us, and inspiring us, to leading us that my own professional life has been far richer than I ever could have imagined or ever deserved. Thank you, Earl.

Droessler: Thank you very much, and I do take quiet thanks and enjoyment in these last -- spending these last two days with you. It's part of the enrichment in

my life that you have provided for some more than 40 years. And this is Earl Droessler, ending this interview with Tom Malone on the campus of St. Joseph's College in West Hartford, Connecticut on the 18th of August, 1989.

END OF TRANSCRIPT