American Meteorological Society
University Corporation for Atmospheric Research

TAPE RECORDED INTERVIEW PROJECT

Interview of Thomas F. Malone
11 February 1988

Interviewer: John S. Perry

Perry: Interview of Dr. Thomas F. Malone held on 11th of February 1988, at the American Academy of Arts and Sciences, Cambridge, Massachusetts. The interviewer is John S. Perry of the National Academy of Sciences and we're going to start now talking with Tom.

Did you want to start with Tom Malone or did you want to start directly with GARP? Gisela [Kutzbach] has suggested that we start by asking such basic questions as who are you, where did you come from, how did you get interested in meteorology, what were the really shaping forces in your career, parents, role models as a child that sort of thing.

Malone: Well I was born in Sioux City, Iowa in 1917. My parents had been born and reared there and my both sets of grandparents lived there. My mother happened to be down there when I was born but I was actually reared in South Dakota on a ranch about half way between Rapid City and the Black Hills in Pierre, we call it "Pier" out there, which is in the center of the state right on the Missouri River. We lived on a ranch, which was about 40 miles north of the railroad station, Philip.

When I was a lad there in the 1920's we had no radio. We had cars. We had lots of horses and cattle. During my boyhood we transformed from a ranch to a farm but my father had two primary interests. One was directed at telephones and he installed the first telephone system in that part of western South Dakota using barbed wire fence instead of telephone line between his home and his cousin's home three miles away. He had a second interest which was my necessity in view of his ranching interest and that is weather because with no radio you had to make your own forecasts and if you had to get your cattle protected from a storm you had to watch the sky. So, his two abiding interests were telephones and meteorology, he called it weather. He had a barometer, aneroid barometer, and had an uncanny ability to look at the sky and anticipate what the weather was going to be. It was forced on him because, as I say,

there were no radios and newspapers were weekly out there. To make a long story short my brother went into the telephone business and spent his professional career with the telephone company and I went into meteorology.

I started out by going up to Rapid City and talking to Harley Johnson who was a meteorologist in charge in Rapid City. As I was finishing high school I was sort of torn between journalism, law and science. He said, "Forget about meteorology until you have gone to engineering school and picked up some background in mathematics and physics." That was good advice so I went to the School of Mines and did my work in math and physics and general engineering. Fortunately I was able to…I used to read the Literary Digest and I read an article about meteorology and the name of the man in the Weather Bureau, I believe D. M. Little was his name, who was a deputy to the Chief of the Weather Bureau, and I wrote him a letter expressing my interest in meteorology and it turned out that they needed what they call an Emergency Assistant in the Rapid City Weather office. At the end of my freshman year at college I had a little job, $1.00 a day, in the hospital as sort of an attendant and I just happened in the Weather Office and they said that they were looking for me and that we have a job for you for $4.00 a day, which was a lot of money in those days.

I spent every summer during my four years of school working in the Weather Bureau as an Emergency Assistant. I used to take observations and run down to the Western Union office and put them in a word code and transmit them. Then we did plotting the maps and the usual routine. There was a small library at the office and the most sophisticated book was Humphrey's textbook on weather. There was another book by Waldo, but it was rather elementary. That was my introduction into meteorology.

When I began to finish my college work my advisor head of physics, Carl Watson, asked me what I wanted to do, and I said I wanted to study meteorology and he said, "Well, there are two places, one is Cal Tech and one is MIT. You pick one but not both and I will support your application." So, my mother had a dear friend, Orpha Haxby, who was the wife of a famous rancher in that part of the world and she said, "If you can get into Boston Tech that is the place to go." MIT was known as Boston Tech. So, I decided on Boston Tech and made my application and got the scholarship and was admitted and came to MIT in 1940.

Perry: 1940.

Malone: Yes. I started college in 1936.

Perry: Who were the main professors in 1940 there in meteorology?

Malone: Carl Rossby was just leaving. He and Hurd Willett had started the school in meteorology there. It was a division in the School of Aeronautical Engineering and Professor Richard Smith, who was head of Aeronautical Engineering, had cognizance over the meteorology division and Sverre Petterssen had just come aboard as chairman of the department. So it was Sverre Petterssen who was chair of the department, Hurd Willett was one of the senior professors and Delbert Keily was there and Charles Stark Draper who was in aeronautical engineering did some teaching on meteorological instruments. But primarily the key figures were Sverre Petterssen, Hurd Willett and Henry Houghton taught physical meteorology, so that they were the senior people. We had people like Neiburger, who taught and supervised the laboratory where we drew the maps and Jim Austin was there; he just finished his first year. He worked on his doctorate when he was a young assistant professor at that time.

Perry: When the war started MIT became one of the training centers for the cadet program in meteorology. You were instructed in that weren't you?

Malone: I entered with the first big class of about 80 or 90 Air Force cadets and a few CAP, Civilian Air Patrol I think they call it, who had taken a pilot training in college and were sent to MIT. So, there was a class of about 100 that started out the same time I did. There were maybe a half dozen civilians and the rest were all cadets and there were a few naval officers at that time so that we all went through that course. Ken Spengler was a member of that class.

At the end of the year they made me instructor and I stayed on at MIT except for a slight sojourn in Africa in the 19th Weather Squadron toward the end of the war, where I served as a consultant to the Air Weather Service. I had been told that I was going to go to the Philippines and participate in the forecast of the invasion of Japan. I became, I think, MIT's greatest expert in Chinese weather. I got some old maps and studied all the Chinese weather maps and immersed myself for several months in Asiatic weather. But when I got down to Asheville, someone whispered in my ear that you're going to Africa. My task was to set up an Upper Air Center in Cairo, Payne Field was the base, for what was called the Red Ball Express, which was to ferry equipment and soldiers from across to the Pacific Theatre. They had an alternate route, which went across the Pacific into Africa and across Africa and onto the Indian into the Philippine Theatre. But before we even got the Upper Air Center which was to forecast for the flight of the aircraft on the Red Ball Express, the bombs dropped and people began coming home.

Perry: So, in that period at MIT with the cadet program and everything, many people who figured prominently in the postwar bloom of meteorology were part of that program, weren't they? Bob White and Ed Lorenz and what other names stick in your mind? That seems to have been a common thread in so many careers in meteorology in the aviation cadet program.

Malone: The genius of the program was resting with the standards they set on the mission of the program. They required solid grounding in physics and mathematics, which meant that you attracted a superior student into the program. It provided an infusion of talent that was simply crucial to the subsequent development of meteorology. Yes, Ed Lorenz was…he followed me by a year or two. I was actually one of his instructors. When I finally left MIT at my tenure position it was Ed Lorenz who took my tenure spot so I sometimes think that my greatest contribution to meteorology was to leave a tenure position open for Ed Lorenz to develop his career at MIT.

Perry: But at any rate, after the war you then joined the faculty at MIT.

Malone: Yes, actually I became an assistant professor during the war. I started out as a research assistant and became an assistant professor.

Perry: How did the Travelers thing evolve? I never got that really clear in my head.

Malone: Ken Spengler was the intermediary there. The Traveler's Insurance Company decided that they wanted a weather service because the weather station in Hartford was being moved to Windsor Locks where Bradley Field was located. There was some dismay in the Hartford community that they were losing their weather station. A community seems to feel that it has no special stake in the weather unless the weather station is right there in the community. Why, because there is somehow the view that a community without a weather station is sequestered from the main current of activity of meteorological activity. They had a very creative head of their public relations, public information, by the name of Harry Barsanty. Travelers owned the radio station at that time, 50,000-watt radio station, WTIC. It was one of the early pioneers, and they had a director of news, Tom Eaton.

Tom Eaton and Harry Barsanty called Ken Spengler and said they would like to set up a weather service of some kind and whom could they get. I had been doing some moonlighting with a station down in Quincy, WJDA, and I used to broadcast every morning from my home, I had a line and went on the air 2-3 times at 6:00, 8:00 before I left for MIT. Ken knew about this so he suggested my name to them. They came up surreptitiously one night and taped my broadcast in the morning

unbeknownst to me and went back and listened to them. They decided that they would like to explore something with me. So, they came up and talked to me and we met over at the MIT faculty club for lunch. They said they wanted to set up a weather service in connection with their radio station and I said that was interesting but my commitment was to research and to fostering the private sector in meteorology. That was quite a thing in those days; there was a lot of ferment in the postwar years about finding opportunities for the returning Air Force and Navy meteorologists and the private sector was an attractive possibility. Carl Rossby was very dedicated to that, he made quite an issue of that when he was president of the American Meteorological Society.

Joe George, who was the chief meteorologist for Eastern Airlines, did an analysis for the Department of Commerce at the Weather Bureau and highlighted the potential role of private meteorology. I said that it was not very attractive to me just to be a forecaster but if they had in mind developing the private sector and doing some research then that would make it worthwhile. So, I went down and talked to the top brass at Travelers, they got about 8-10 of their vice-presidents together, and we had a good talk.

The conclusion was that they would set up a research center as well as a weather service, so we did have the weather service, which provided round the clock weather forecasts for WTIC sponsored by the Travelers and also a research center, which would look at scientific problems in meteorology and also look at issues in meteorology that were germane to the private sector, specifically to a large multiple line casualty, property and life insurance company which had a rather substantial stake in meteorology. That was during 1954-55 and there was a span of hurricanes on the east coast. They had large investments of life insurance premiums in agricultural lands in the Texas panhandle, for example, where they were mining the ground water to produce long staple cotton and it was only a question of time till that ran out. We did analysis of that and showed them the land would have to revert to dry farming and this kind of thing. All kinds of geophysical hazards, the meteorological hazards, earthquake hazards and flood hazards, these were all germane to the business of the multiple line insurance company and so we worked on all those problems. It somewhat broadened my view from strictly meteorological to geophysics because of the interest of the insurance company and then I was asked to serve as president of the American Geophysical Union and that got me into geophysics.

While at Travelers I also was, since we were the only ones that had the research designation when they decided to go into other kinds of research particularly economic research, business research, market research, they asked me to take that on so that I had to acquire some rudimentary

knowledge of economics and market research and re-institute a long range planning at Travelers. Maury Beech, one of my students at MIT and a bright young man, and Sterling Tooker and I asked him to head up the long range planning effort and one thing led to another and he finally ended up as president, chairman of the board, meteorologist made good with the Travelers. All of that led me to sort of an environmental career. Meteorology, geophysics, and economics brought me into contact with social sciences. As chairman of the U.S. National Commission for UNESCO I was very much interested in bringing together the natural scientists and the social scientists. We had committees on social sciences and a committee on natural sciences. We used to have great discussions that went nowhere anywhere because we never really identified problems that they were prepared to work on. Members of the Commission were prepared to talk about them and not to work on them.

Perry: Still true. How did the international thread enter so strongly? You mentioned UNESCO, was that really the first major international thing? Do you think your interest in international affairs stemmed from the days in Cairo?

Malone: I think the international interest stemmed from the intrinsic nature of meteorology. It has to be a global field of study which involves global conditions and traveling to Cairo I did see another part of the world but I became convinced that a global perspective was absolutely essential in meteorology and then I realized that meteorology was a field in which, since you were forced to take a global perspective, was a stepping stone toward a global point of view which was going to be necessary as we completed the 20th century.

Perry: So in a way you were way ahead of the development of later programs and put the word global on them.

Malone: Yes. I started going to international meetings I think it was in 1957 when there was a meeting of the International Union of Geology and Geophysics, maybe it was the International Association of Meteorology in Toronto and I went there. I very early formed a close attachment while at MIT with Canadians and Kenneth Hare, who became a colleague of mine. Matter of fact we were doing some research at MIT using some work that George Wadsworth and Joe Bryan had started during the war and characterizing weather patterns by mathematical tools and we fit what are called Tschebyshev polynomials to the pressure contours. We began to explore statistical methods of forecasting and we really, I at least, didn't know what we were really doing until Ken Hare came down from Canada as a consultant to our little project which was sponsored by the Office of Naval Research and quite quickly perceived what were about and the whole project took on some meaning. So, I became very impressed at an

early age with the importance of international interaction and broadening your perspective and exchanging views and insights on new problems. So, the nature of the field and my early contact in broadening my own perception of what was going on was quite influential in vectoring me to a global sort of perspective.

Perry: Well, this leads naturally to the question of where did GARP come from anyway? If you were to trace the many threads and streams in GARP where would the first trickle start?

Malone: The first trickle started one day when I was seated in my office on the 17th floor of the Travelers building. The telephone rang and it was Jule Charney at MIT. We had become close friends. We used to have debates about statistical vs. dynamical methods of approaching a prediction problems but we became quite close friends working with the committee on meteorology which Francis Reichelderfer, Chief of the U.S. Weather Bureau, set up to examine where the Weather Bureau was going and it was chaired by Carl Rossby and Lloyd Berkner. Rossby died and Berkner became the chairman. So, we became good friends and Jule said that Richard Goody had had a call from Bruno Rossi who was a cosmic ray physicist at MIT, who was working with Jerry Wiesner, who was science advisor to President Kennedy, and they were interested in preparing for a Vienna meeting between Kennedy and Khruschev. Jerry was looking for some scientific topics which would provide some kind of a linkage between the Soviet Union and the United States, this was in 1961, and would I come up and join Richard and Jule in meeting with Bruno Rossi, and exploring with him any possibilities that might exist in this field of meteorology which to any informed scientist, which Jerry was a very well informed scientist, immediately had strong international overtones.

I came up and we sat in one of the old Radiation Lab buildings, which are still there from the World War II days with Bruno Rossi and started a conversation. Then we both, Richard and I, mentioned, and Jule concurred, that one of the aspects of this field was the potential for human interaction in altering the weather. There was a lot of interest at that time in two things. One was the cloud seeding, which was highly speculative and quite controversial and even then, and the papers show it, there was the concern about the human impact on the atmosphere. Particularly, this was 1961, it was quite clear that the increase in carbon dioxide was altering the gradient of balance and Dave Keeling's results were beginning to come out. The famous wiggly line going up, it's probably the best-known meteorological chart in the world, and so we mentioned that and Bruno's ears pricked up and he said, "Hey, this has possibilities." So I stayed over that night and had dinner with Bruno and we explored some of these things and he said, "Yes we would like to look at this very seriously."

I was concerned that we have some kind of a consensus within the small group so I suggested that we convene a half dozen people at the American Meteorological Society, which had just moved to 45 Beacon Street, to talk with Bruno about this so we did. We got Jule, we got Richard Goody, we got Henry Houghton, who even in those days was sort of a very senior statesman and a father figure. Houghton had very good judgment and if he agreed with something it made good sense. Also if he agreed with something, you were around to third base on the homerun because Henry was very influential in the field and his part was very crucial. Harry Wexler came up from Washington and Dave Johnson and Maury Tepper. I don't recall whether we got Sverre Petterssen at that meeting or not, I think we did because he was very anxious to be involved in it. We said yes here was an area, which could be set before the Soviets as an area for cooperation. So we drew up a little paper and it was passed through Rossi to Jerry Wiesner. The paper has become lost now [*paper found according to TEM*], but I was sorry it was lost because it was a very naïve document, but it was the seed out of which a lot of things view. As everyone knows the conversations at Vienna collapsed; Khruschev and Kennedy did not get along at that meeting, and we thought our enthusiastic efforts to propose something, which would at once advance the science and contribute to international understanding between those two super powers, was going to come to naught. But fortunately Kennedy then had to make a speech before the United Nations in September of 1961 and some of the advisors in his office and I don't know whether David Beckler or maybe Jerry himself said, "Hey, let's take a look at that paper that Malone and colleagues prepared.

Interview of Thomas Malone

TAPE 1, SIDE 2

Perry: This is an interview between John Perry and Tom Malone tape 1, side 2.

Let's see, we were in 1961 and they were scouting up topics for Kennedy's speech to the U.N. Let me ask this, what topics were highlighted in the now lost, "think peace" paper for Kennedy and Khruschev? Was that paper any different from the way that GARP finally evolved?

Malone: It was an unfocused, soft of emotional advocacy for international cooperation on the problem, which was intrinsically global and the specifics of the ultimate program had not yet emerged at that time. Probably there was no great loss in those papers because they would not be terribly impressive. If you looked for the seeds of what eventually became GARP you would not find them there.

The real significant meeting was held in Washington on August 31st, in Jerry Wiesner's office and he was there and Harlan Cleveland was there and Rabi was there, Rabi fell asleep during my presentation, Pete Scoville, who was then with the CIA, was there and there was a man who looked like an owl, he had big hornrimmed glasses, and I wondered who he was and it was Arthur Schlesinger. He was sort of a personal aide to Kennedy, and he used to go around and sit at these meetings and sort of absorb what was going on. What he did with his information, I don't know, he was like a sponge--he soaked up information. It never came out to my knowledge. Det Bronk, who was president of the National Academy, and was chairman of a little international committee in the White House was there. Dave Beckler was there; he was a sort of executive officer for Jerry Wiesner. There were others I don't remember. I think I have the minutes or the roster but those were the main players. It was Det Bronk, Jerry Wiesner, Harland Cleveland and Dick Gardner.

Dick Gardner and Jerry had a big argument about what I have forgotten but I was an innocent bystander. The question was, is the meteorological issue one which should be included in President Kennedy's talk. I made an eloquent plea that it be. I am probably glad that that plea was lost because it would not be persuasive in the perspective of today's view. It was more an emotional plea. But it carried the day and I was on my way to Australia to give a keynote talk to an international conference in cloud physics, stopping over in Honolulu at the Pacific Science conference and so I agreed to take a sounding there and see whether this thing made sense or not. I did and Horace Byers who was president of at the International Association of Meteorology and Atmosphere Physics (IAMAP) and I flew

from Honolulu to Sydney and then to Canberra together and I discussed this together with Horace because he was a very senior person in the field and was president of the International Association of Meteorology and Atmosphere Physics. His support would be important for any kind of a global approach. After the meeting, the all day meeting, at the old executive office building I stopped overnight at Sverre Petterrsen's place in Chicago and filled him in. Sverre was very enthusiastic about this type of thing and stayed up late talking about it and ultimately they convened a little group while I was down in Australia. Jule Charney confirmed the point of view, which I had advanced in this small meeting. So anyway, two sentences got into President Kennedy's speech. So, all the running around and speechifying during 1961 did pay off in those two sentences that appeared in his speech.

There was second crucial step after that and that was there was some pulling and hauling about whether this would be an intergovernmental program or scientific program or some coalition of governments and scientists. There were strong views on this issue in Washington. I would say that they were not instigated by Reichelderfer, but by people on his staff to make sure this became a WMO operation. The people I was working with were quite anxious that it be a mixed effort of governments and NGOs. The real issue boiled down to this; under what framework would it be carried on. Would it be a WMO framework or a scientific framework. The thing came to a head because the resolution, which was put into the United Nations, the first resolution in 1961, called for WMO to take this lead and I was concerned about that because I felt that the scientific unions not because they were such a powerful organization but because it was important that the initiative reach a community which was broader than the WMO's community.

It came to a head one day when I was in my office in Hartford and I had a call from Herb Holloman. I was spending some time in Washington as a volunteer assistant to Herb. I had argued successfully with Herb that it should be a mixed program, a scientific non-government and intergovernment. Herb said, "Come down quick because Dick Gardner," who was assistant secretary of state deputy to Harlan Cleveland, "is coming over and we're going to resolve this thing today." It would determine what position we would take in the United Nations. In those days, this was in 1963 or 62, the United States played a dominant role in the United Nations. Positions taken by our representative were often prevailed and Herb said, "Come down as quick as you can." Travelers had a little Twin Beech airplane, and so I jumped in the Twin Beech and hurried down to Washington hoping I would get there in time. When I got there Herb said, "It's all over, we had an Indian wrestling match and I won." Gardner is going to invite both ICSU and WMO to develop this thing. It was a very crucial step because even though I missed the main

fray, my contribution was to persuade Herb Holloman that it should be a mixed program and it was Herb Holloman who Indian wrestled with Dick Gardner out in his office over in Commerce. Dick Gardner was a very articulate bright young man. He was Harlan Cleveland's man and commuted back and forth between the State Department and the United States mission to the U.N. That was a crucial step because the invitation then went to ICSU and WMO to work together on this. It was decisive because all they had to do was make sure that this path was followed up and this effort occupied me for several years.

Perry: As I looked back on the history of GARP when we first came into that job at the Academy I was struck at the way the program for a while seemed to have grown in scope and then have narrowed down to a focus on the predictability idea and the global experiment. That must have been a period of a lot of delicate political scientific negotiations as everyone tried to creep under the GARP blanket and then the blanket was reduced to a single sharp sphere. How did that all come about?

Malone: With this involvement the NSF became very interested in meteorology. At that time Paul Klopsteg had taken over chairmanship from Mike Ference, I think it was, no I guess he followed Berkner. Paul Klopsteg was the associate director of the National Science Foundation. He was appointed chairman of the Committee in Meteorology and he said it should be changed to atmospheric sciences and so it became atmospheric sciences and I thought it was the charitable thing to do.

Perry: This was in the academy?

Malone: In the academy. The Committee of Meteorology became the Committee of Atmospheric Sciences at the urging of Paul Klopsteg who was a physicist and chairman of the Committee of Meteorology at that time. Then atmospheric sciences was then, that term, was adopted in the White House and the committee was set up…panels set up on atmospheric sciences chaired by John Tukey, a very wise man. All the agencies learned that atmospheric sciences was the in-thing in the White House. You're right, everything was transferred from whatever it was to atmospheric sciences. A phenomenon not unlike some things going on here in 1988 that will change.

It was a sensitive time, a difficult time. It was a time of frustration. One of the most embarrassing incidents in my contact with this was when I was working with Herb Holloman and Dan Rex and Ed Epstein from Michigan. Ed was a statistician.

Perry: Epstein.

Malone: Ed Epstein and I were sort of a volunteer staff support to Herb Holloman who took more than a casual interest in meteorology as assistant secretary of commerce for science technology who was brought in by Jerry Wiesner from General Electric. He became enchanted with this field. So, I was working as a sort of special assistant in trying to develop this program. It was so broad that it was not persuasive. We prepared a presentation to the commerce and the state department. Sverre Petterssen was involved in this in some fashion; I have forgotten. Dick Gardner demolished our presentation. It just wasn't very good. It wasn't good because it was not focused. It was simply not credible to a thoughtful, intelligent outsider such as Gardner. I was devastated because we had spent a lot of time and spent all night working up some of these presentations.

I can remember calling Jule Charney on New Year's Eve I spent in Washington, Paul and I had been working in the Commerce Department trying to find words to describe this thing. Herb would call us in and I thought he'd wrestle down because he had a very strong temper and Herb said to Ed and me, "Well, guys, how to we fix this?" That's why all of my life I have been a very strong admirer and attached to Herb Holloman because one time he could have blasted me out of the room, he did not. He had every reason to do it. The answer really was Jule Charney's great contribution when he recognized that there are limits to predictability and that are understanding of the theory of large scale motions, global motions, had outstripped the observations and the program, which would provide sufficient coverage to test his computer runs that he and, I guess it was Joe Smagorinsky made, which indicated you could go out about ten days, would be an exciting, important and provided focus. That is where a rather amorphous concept was transformed into a sharper focus program. It was Jule Charney's great contribution.

Perry: When was that?

Malone: That was in around 1963-64. I have a paper in my briefcase which is the…I think I was chairman of the Committee of Atmospheric Sciences then and we set up a panel on this Charney, there is a report.

Perry: I think that report came out in 1966.

Malone: That's right 1966. Yes.

Perry: The feasibility of a global experiment.

Malone: A rather interesting meeting was held in Moscow on a little barge on the river over there. The International Association of Meteorology and Atmospheric Physics had a symposium over there. Charney was there and

Monin was on that and Obukov; Monin and Obukov are the two great figures in the Soviet Academy of Science. Monin was an oceanographer. There was a deputy to Federov (who was the head of the Soviet Weather Service) and was a very fine man. There were a group of about a dozen of us and the key step came in my opinion when Charney explained what it was all about and, I am simplifying it, Charney explained it all as only Charney can and Monin said, "Hey, this is great, let's do it." That brought the Soviet community and the United States community at quite high levels with all kind of following but Monin had the one side and Charney on the other side.

Another key event in this was, well let me just give a background of this. There were several years when the initiative went backwards two steps for every step forward. It revolved around this question of who was going to carry the ball. It was an organizational problem as well as a focus problem, and I traveled to Vienna, I traveled to Mar del Plata in Argentina. Bob Fleagle was with me in Vienna, in Florence. I tried to get COSPAR involved in this because needed an international mechanism. I was unhappy with the viability of the International Association of Meteorology and Atmospheric Physics. A famous physicist by the name Maurice Roy of France was president of COSPAR--because they always had a Soviet president…a neutral president, a U.S. vice president and a Soviet vice president. Richard Porter was the vice president of the United States and a Russian scientist was the vice president on the Soviet side. Sir Oliver Sutton was head of the British Met Office. We had a big meeting in COSPAR and I took Sir Oliver and his wife out to dinner at the Three Hisars, a very expensive restaurant. I thought I had him convinced at the end of the dinner.

Perry: In Vienna?

Malone: In Vienna. So I laid the thing before him the next morning citing the human mind and spirit driven by the scientific community and Sutton just sat on it so nothing became of that. I went down to Mar del Plata in Argentina. Ken Spengler was with me on that trip and Richard Porter was very much in favor of this but Roy had to be convinced. We met with him and he was not convinced, so COSPAR passed up an opportunity to take the leadership. So, we had to construct something in order to provide a mechanism within the scientific community.

Horace Byers and I went to Toronto again in the Travelers Twin Beech and spent a whole day with Warren Godson, who was secretary general of IAMAP. Warren was adamant that this program would be developed under the umbrella of the IAMAP. Horace and I felt that it should be developed elsewhere. Godson was very gracious and entertained us in his home for lunch and we had a fine day, but he was simply adamant and we

were equally adamant on the other side, so nothing happened. It came to a head at the general assembly of International Union of Geology and Geophysics (IUGG) in Berkley in the fall of 1963 when again I tried to propose a special committee of some kind. Peter Shepherd who was a very senior scientist at the Imperial College of Science Technology. I remember getting a wave of his big finger in front of my nose: "Tom, we will destroy you if you try it and bypass IAMAP."

Again I made a formal presentation to the council of IUGG and…I have forgotten who it was who spoke against it. I can't remember whether it was the delegate from France; anyway my motion didn't pass. I was devastated because I thought we had really missed the boat. The next day when Secretary General Georges Laclavère made his report on the outcome of the meeting, he gave a favorable report. In fact I couldn't believe what George was saying. It was as though they had approved what I had proposed. The key lay in the presidency of IUGG, and that was a bitterly contested election. Belousov, who headed the Soviet delegation, was the president. Our committee, I was chairman of our national committee at that time, had decided to push Joe Kaplan and Belousov was lobbying for Lloyd Berkner or Maurice Ewing and Berkner said nothing doing and I explained the situation to Maurice Ewing and I had a handwritten letter saying he would not went delivered to my room about midnight right before the critical council meeting. Joe got elected by one vote and there was a Dutchman there, a great big tall fellow, he had been ill and I had gone out of my way to get him a doctor and all these things, his wife was very appreciative. He was seated next to me and he said, "Who shall I vote for?" I said, "Kaplan." Because I had been so good to him in his little problem, he said, "Sure." His was the critical vote.

Then Joe Kaplan and George Garland who was elected secretary general to succeed Laclavère took this onto themselves as a project and the executive committee of IUGG met in London and they decided to go ahead and set up a committee and it finally emerged as the ICSU-IUGG Committee in Atmospheric Sciences. Bert Bolin was the chairman and I was the secretary general. Then there was WMO advisory committee and both committees met in Geneva. I can remember our first meeting when Langlo and Oliver Ashford called Burt and me into the office, Davies was out of town at that time, and asked what we were doing there at WMO. Fortunately we had Joe Kaplan and George Garland with us and they established a credibility without which we would have probably would not have been allowed to even meet WMO. So, we met there and the WMO advisory committee met and we got Charney and Ed Johnson and a whole lot of people involved from around the world. Peter Shepherd was one of the key people in this particular group in spite of his threats to me at Berkeley.

The critical incident in the history, we had three to four meetings, wrote little reports which I do have available, but the critical event was the third meeting when we had had a long night down at the elevators in Geneva, eating fondue and drinking all kinds of liquor. Charney was to make a presentation the next morning. You know, Charney liked to sleep late. We had had a rather large night the night before. In fact I remember Peter Shepherd sitting on the steps holding his head in his hands. So, John Sievers was with me so I sent John over to the hotel about 10:30 to roust Jule out. Jule came out and he gave the most beautiful lecture I think I have ever heard. Very critical if you remember that meeting room in the WMO that had a little balcony up there where the Secretary-General would come and watch and he came and listened to Jule. From then on there was a focus on the scientific rationale and the groundwork was laid on that day at our 11:00-12:00 that morning to the kind of cooperation that was required to put the thing into motion. The WMO Advisory Committee had met but they never really accomplished very much, and we finally merged the two. And then we were able to get some money from the Ford Foundation to sponsor a little summer workshop in Skepparholmen, an island up there off Stockholm.

Perry: When was this, 1968?

Malone: 1967, I think. And, a report was prepared. Rolando Garcia was the secretary of the committee.

Perry: Now that was before JOC was born--?

Malone: That's right. JOC was conceived at Skepparholmen. In fact, I can remember that Jule Charney and I spent two hours walking on the island, figuring out how this would be done and who would do it. We decided that neither he nor I would be on this committee, but we would put Bert Bolin on and the United States would put Joe Smagorinsky and Fred Shuman; they brought two complementary talents. We had got out of COSPAR a splendid effort headed by Morris Tepper, who did a marvelous piece of work, Working Group 6 of COSPAR. Took the data specifications which Charney and company had laid down, and converted them into the system design involving satellites—but the critical event in that meeting in Switzerland, at Lucerne, was a presentation to IAMAP about this report by Bert Bolin

Perry: Was this an IUGG meeting?

Malone: It was an IUGG meeting. Because Switzerland is small, it was divided among Zurich, Lucerne and a couple other places. But this was a IAMAP meeting. I can remember Garcia coming over from Geneva with his little

Italian car, a FIAT, just loaded down with these great big reports that came out of Skepparholmen. I was asked to chair a meeting, and Bert Bolin made a beautiful presentation and Sir John Mason got up afterwards and he nipped the thing up from one side to the other. But fortunately he was followed by Reggie Sutcliffe, who had been part of our ICSU-IUGG committee. If you knew Reggie, he was a very quiet but very effective individual. And he knew John was an orator. And John got red in the face and he claimed this was a crazy idea. But Reggie got up afterwards, and "Well, there may be flaws in it, but I think it's worth a try." And my contribution was to have the good sense to call for a vote. It passed. GARP was born and duly reported on in the *New York Times* by Walter Sullivan.

END OF INTERVIEW