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TAPE RECORDED INTERVIEW PROJECT

Interview of George D. Robinson
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Interviewer: Earl Droessler

Droessler: .. This is Earl Droessler and I'm in West Hartford, CT, on June 27, 1994, to conduct an interview with Dr. George D. Robinson at his home in West Hartford.

Good morning, George, awfully nice to be here with you.

Robinson: .. Good morning, Earl, I'm very glad to see you here.

Droessler: .. Thank you for the invitation. Let's begin by my asking a question: how did you get into meteorology from your background in physics?

Robinson: .. Well, it's an odd way into meteorology. I was in the Physics Department at Leeds University, and I got there at an early age and I was granted a PhD degree in 1935, at which time I was 22 years old. The subject of my thesis was slow electron collisions in zinc vapor. Then, for a year, I worked there as a research assistant with a professor of inorganic chemistry, who was a man called Whytlaw-Gray, who claimed to have introduced the word "aerosol" into the scientific literature. He was engaged on a commercial problem connected with the town gas, which was caused by a chemical reaction which was putting out pilot lights. He nailed this down in the end to the formation of what effectively was Los Angeles smog. We had Los Angeles smog in a twenty-liter flask in a cellar in Leeds in 1935 or 1936.

It was the same reaction. We just put into this flask--we broke a little ampule of nitric oxide and we had put in some unsaturated olefinic compound, in fact it was cyclopentadiene. We illuminated this with light from a carbon arc and we got a flaskful of smog. My part of the job was to filter this out and examine it microscopically and the like. The trouble was that the ultraviolet light from the central blue portion of the pilot light was causing this reaction. And effectively, the smog was bunging up the supply to the pilot light and putting the pilot light out, so the cure was out: keep unsaturated hydrocarbons or nitric oxide or both out of there, and I don't know how the chemists did this, but I think they found some way of cutting out the nitric oxide part of the town gas. This cured the pilot light and my professor lost interest in the project, having satisfied the customer, so some of the curious things that I'd found out about Los Angeles smog were just not further investigated.

This has nothing whatever to do with meteorology except that somehow or another it got me the reputation of knowing something about atmospheric pollution by particulates formed in normal reactions in dirty air.

Let's get back to meteorology. This was 1935-36. In 1936, you may recall that the Abyssinian [Ethiopian] war began and there was, in British circles, a certain amount of excitement because the war was in the Mediterranean and Mussolini called the Mediterranean "Mare Nostrum" and the British admiralty was inclined to think that it was their sea, not Mussolini's. The immediate repercussions, as far as I was concerned, was that a man called F. J. Scrace, who had been at Kew Observatory for a long time, was promoted and posted to Gibraltar to look after meteorology in the Mediterranean actually.

Then the real increase in recruitment for the Meteorological Service began. This was nationally advertised; I applied for the job and was interviewed and then told that I wasn't really suitable and thankyou very much. About two weeks after I had this letter, there was a follow-up saying that although they couldn't offer me a permanent job, they would offer me a temporary job which might keep me employed for three to five years.

That seemed a long time to me, but I accepted what had been proposed and was posted to Kew Observatory to take the place of Dr. Scrace, who had gone to Gibraltar.

Droessler: .. How old were you at that time?

Robinson: .. I was 23. I went to Kew and found the job there. Kew Observatory was a meteorological and seismological observatory and the meteorological side was mainly at that time concerned with atmospheric electricity. And the job Scrace was doing was effectively a research assistant to Sir George Simpson, who was the director of the Meteorological Office and whose hobby for a long time had been atmospheric electricity--the job I got into was effectively [that of] a research assistant to Sir George Simpson, who sat in London at the head office, and had research done for him at Kew Observatory. This was really, apart from Porton, this was the only place in the meteorological office at that time which was devoted to research of some kind or another. So in that respect, it was unusual.

And there are now are two things I would like to [discuss]: the immediate one, what did I do for Sir George Simpson, and what did he do to and for me? The other thing is concerned with the recruiting expansion of the Meteorological Office and I just--with your indulgence, put in a minute or two on this.

I said they advertised for--they were called "Technical Officers" in those days. I responded to one advertisement and didn't get the job, [but] got the temporary job. They continued to do this advertising two or three times a year. They got in a new batch of technical officers and whilst this bunch was collecting--they were looking for six to eight at a time--they posted these people to Kew Observatory just as a holding tank until they got six or eight people. Then when they'd done that, they sent these people to a school of meteorology so that at Kew I saw these people come and hang around for a week or two and do odd jobs and generally talk to each other, and then just go off to school. The normal entrant at that time had, I think, three to six months in the meteorological training school, and then went out to his first posting, mainly to RAF stations and mainly on forecasting work.

I never got this. I never had any real training as a meteorologist.

Between early 1936 and the outbreak of war, I would say 20 or 25 technical officers were recruited and this pretty well doubled the number of technical officers in the Meteorological Office. This was started off by the Abyssinian war and continued.

Droessler: .. What sort of research did you personally get into?

Robinson: .. The main research job at Kew was concerned with the electrical structure of a thundercloud. The research consisted of putting up balloons with a semi-quantitative electrometer on them. It gave a firm reading on the polarity of the field at the balloon and an indication of the strength. This meant running around in a thunderstorm, sending up these balloons. They were not radio-reporting. To get results from them, they had to be collected and returned by the general public. There was a little notice on them saying if they would return these, they would get a reward, I think, of ten shillings. A surprising number of them were returned. I think we got about 75% returns, which is unusual. Some of them after trips of 200-300 miles. Simpson ran this from afar and looked at the results and seemed satisfied that I was making a reasonable job, my part of the job being to determine when to put these up. They had to go up in a thunderstorm, but I had to make sure there was a reasonable chance they would be returned.

The actual mechanism of getting them up was amusing. You had to make decisions as to whether you would do this job in a bathing costume or have heavy protective oilskins. Both were uncomfortable, but if you thought there was going to be hail, you used the oilskins. If you thought you would get away with simply a cold shower, you did this because sweating inside the oilskins was extremely uncomfortable. No one thought anything about being struck by lightning whilst doing this although there was a strike to a tree 100 yards away from the point in one of the storms. We went on doing this until the summer of 1939. We managed to get some work on the discussion and writing up done. It was published, I think, in 1941 or 1942, well after the beginning of the war.

As far as my subsequent work was concerned, Stagg was the superintendent at Kew. When I first went there, it was F. J. W. Whipple and he retired, I think, in 1938 and Stagg came to Kew as superintendent. On the outbreak of war, Stagg was pulled out of Kew immediately and given a post at Meteorological Office headquarters and

Simpson, who had retired, came back as a volunteer. He decided that he would like to go as superintendent of Kew Observatory for the duration of the war. He came back. He asked them to keep me there, which they did until about the end of 1939.

To go back a bit, soon after I got to Kew (this would be sometime in 1937) and the balloon barrage was beginning to form and barrage balloons were beginning to be struck by lightning, a committee to discover if anything could be done about this was set up. Sir George Simpson took charge of this committee. To my surprise, [he] appointed me, a raw recruit with no meteorological training, to be the secretary of this committee. I said to my surprise, I would have thought he would have a more experienced character as secretary. I can quote what he said to me: "Don't worry too much about taking notes, Robinson. I know what I'm going to put in the final report." This was typical of Sir George Simpson. And he seemed satisfied with the notes I took. This was really my first job which implied that I had some knowledge of meteorology. It was as a result of this that they decided they had to take me away from Kew Observatory and send me to the headquarters of Balloon Command. This would have been presumably in January, 1940, about then.

To be meteorological officer at the Balloon Barrage Headquarters, this turned out an interesting job because all sorts of problems turned up. One of them, which was very secret at the time and didn't receive any publicity, was a thing called the "Free Balloon Barrage," which involved sending up small hydrogen-filled balloons trailing 2,000 feet of wire with a small bomb on the end and the idea was to get this up in the path of the German bombers coming over, night bombers, and this sort of aerial minefield. The forecasting problem, of course, was these things were floating at about 20,000 feet. You had to forecast a position from which these could be released to go into the path of the bombers. Once the target was known or guessed--and it could be guessed from the Germans' own navigational radio work--you had a sort of four-dimensional forecasting problem. You had to get the wind at the right height in the right position at the correct time and this, of course, was well, shall we say impossible?

The decision to operate it was made at a very high level indeed, and there was no way that anyone, including the poor people who had to do the operation, could stop it. So it went into action and we did three or four releases and I personally went out with the release team on two or three occasions. There was no indication that any one of the aircraft ever did collide with any bit of the free balloon barrage, although there were two or three occasions where it was concluded that some of them had seen it because [of] curious maneuvers, but it was really one of the failures in the so-called air defense of Great Britain, but it was tried and it was this stupid forecasting job which really was my first meteorological problem.

They sent me for two weeks to the Central Forecasting Unit at Dunstable to learn how to forecast wind at 20,000 feet in these circumstances. This was my first formal training and really my only formal training in synoptic meteorology.

Droessler: .. Did you have any ability to track these balloons with their trailing wire nets?

Robinson: .. The normal defense radar would do this. But we meteorologists never saw--after the event, yes, but you couldn't do this at the time of release. So the project was given up.

Droessler: .. It was an extremely difficult task, to decide where to launch the balloons, to get them to 20,000 feet, to put them in the right place, to meet the bombers.

Robinson: .. If meteorologists had been consulted before the event, I'm quite sure they would have said that this was impossible. But the decision came down from on high in the name of Lindemann, who was effectively Churchill's scientific advisor at the time. He was all for it.

Droessler: .. One of the positive notes is that you were able to go to school for two weeks to learn weather forecasting.

Robinson: .. Yes.

I think I said that Simpson went to Kew as a superintendent, as a volunteer wartime job and made it a condition that I should remain at Kew Observatory for the duration. The duration lasted until about the end of 1939, from the point of view of my stay at Kew. And I was then posted as a meteorological officer to Balloon Command. It was during this time that the free balloon barrage work was done, and after a few months as meteorological officer in the Balloon Command, I began to initiate some changes which suggested that Balloon Command didn't really need a meteorological officer. I couldn't get agreement to this from headquarters, but the immediate impact of this was that I was given additional duties as deputy to the senior meteorological officer Fighter Command. Again, this wasn't much of a forecasting job. The forecasting for Fighter Command during 1940 and the next year was just controlled by the requirement of the possibilities. It was that the fighters had to respond to German activity and there wasn't a great deal of time for careful thought about any meteorological requirements.

In this job as Fighter Command's deputy to the senior meteorological officer, by reason I think, mainly, of my background performance in the Balloon Command job, I began to get all sorts of inquiries about curiosities. Stagg, from his headquarters job, on several occasions sent me down problems which the defense radar was beginning to get on cloud reflections--what at that time were described by the radar people as "angels--" and also queries about the distance at which the navigational beams could cover. This was effectively nailed down later to surface layer and questions concerned with the ducting in the lower atmosphere of radio signals, so that you effectively had low-level refraction of the radio waves and this made differences of sometimes as much as hundreds of miles in the distance to which the navigational beams could be used. This was a problem which certainly was not solved by meteorologists, but it did become possible to do something at least about the possibility of predicting it--it was a matter of the temperature/humidity structure at the lowest levels. So it did turn out in a sense to be a forecasting job.

This was just the sort of problem that was referred to me because I was a meteorological officer at Fighter Command. It wasn't again a forecasting job; it wasn't a job that I could solve, but I was sort of a first line of consultation by Stagg, to whom the questions would go at headquarters because he was-- effectively one of his jobs was responsibility for forecasts for the Air Defense organization.

I didn't really do anything about this except that I was probably the first person in the meteorological office who was asked what is the problem meteorologically, and what can be done about it. I can't say that I solved anything, but I was just the first channel for this sort of unexpected problem, which had perhaps some connection with meteorology. The Balloon--Fighter Command job wasn't really a forecasting job.

My next move was curious and unexpected. I was posted to a bomber group as again deputy to the senior meteorological officer. To my own great good fortune, the bomber group turned out to be the #3 group and the senior meteorological officer turned out to be R. C. Sutcliffe. Sutcliffe's response to having posted to him as his deputy a man whose entire training in meteorology was two weeks looking over the shoulders of a forecaster at Dunstable--his response to this was, "Why do they always send me people who don't know a damn thing?" I don't know, he must have taken some liking to me because he taught me a lot. What I know about weather forecasting was taught to me on the job by R. C. Sutcliffe.

It was quite a remarkable operational station. His job was Bomber Group; it was concerned with forecasting for the night bombing and it was part of a daily conference between all the bomber groups in which there was an agreement by telephone on the forecast, which all the bomber groups could at least use as a background for their own job. You had to know the target and it was effectively a conference about an 18 to 24-hour forecast. But the interesting thing, the unusual thing about #3 Group, was that Sutcliffe had in effect turned it into a little research group. And mainly concerned with the development of wind forecasting; they were concerned with winds up to about 20,000 feet for the bombers which were then operating. Again, one lucky thing was that one of the people who had been posted to Sutcliffe, who, like myself had no previous meteorological experience, was a young Belgian, Odon Godart, whose training was effectively as an astrophysicist, being a student and I think a collaborator of De Sitter. He was a top-class mathematician, had been doing research in cosmology in Louvain until the war. And he got out of Belgium. He joined the Belgian Air Force--in what capacity--his only explanation to me was that in being described in the Belgian records [as] "homme de lettres"--you know, a high-class university professor (Homme de Lettres). He said: "So they made me the group postman."

Anyway, he got to #3 Group and Sutcliffe immediately noticed that one of his problems was getting the normal use of pressure coordinates for the vertical part of his work. We began to use this at Three Group. We were doing upper air maps of pressure levels, and before the Meteorological Office of Central Forecasting was using them. Sutcliffe actually put into effect at #3 Group techniques for upper level forecasting which were quite new in the Meteorological Office. He did this before the Central Forecasting Office got onto it, so it was a very interesting place to be.

In addition to Godart, there was Zobel, a technical officer who, on his own, using his own manufactured--he called it a "fluking iron." It was really a chart used for the production of isentropic charts, which would not have been in general use in England, although they were very much a feature of American technique at that time. So we had Zobel on his own producing isentropic charts; we had Sutcliffe and Godart on the pressure level and particularly the thermal wind charts. And we were using the thermal wind effectively translating the upper air pattern. We advected them with the thermal wind and so on. It was an unusual technique, and the really interesting thing was that Sutcliffe was doing this on his own. And the Central Forecasting Office was slowly following him.

When the Central Forecasting Office went on in a big way to using the upper air charts for prediction, not only of the upper air, but of the lower atmosphere, they pretty well had Sutcliffe to follow. I don't think Petterssen would say he was following Sutcliffe, but I think Sutcliffe would say that Petterssen was following him. But so far as I was concerned, this was really my meteorological training--it was where I got interested in weather, and the first forecasting I did.

Droessler: .. You were very fortunate to have been posted to Sutcliffe. He was certainly one of the finest meteorologists. And a good scientist.

Robinson: .. As far as I was concerned, a very pleasant individual to work with. Some people didn't find him so, but I did and certainly everyone in the Three Group.

Droessler: .. You mentioned Petterssen. Where was he at the time?

Robinson: .. I'm not sure. By the middle of my period at Three Group--let's see, I left in 1943--so where he was at the end of 1942, I don't know. By the middle of 1943, he was at Dunstable, installed as the head of the Upper Air section at Dunstable. He was building it up, not quite with the same techniques as Sutcliffe but similar. [There was] a certain amount of originality in it, building up an Upper Air Forecasting section. This must have been sometime about the beginning of 1943, when they began to get this going.

My next move, after #3 Group, was indeed a posting to the Central Forecasting Office. I was posted there as a forecaster, not just any run-of-the-mill forecaster, but as an assistant and potential understudy to the senior forecasters. There were four of them on the roster...Well, we called ourselves "stooges," but we were effectively understudies: "Do anything that turns up, but hold yourself in readiness to be put on the forecasting roster, if any of them went seriously ill or anything of this kind." It was sort of acknowledged as an indication that you were next in line to a senior forecasting job. I think I was the only man who ever had any sort of position like this at the Central Forecasting Unit who hadn't had the Meteorological Office's own training class, which may not have been a bad thing because it allowed the use of a little unorthodoxy in the Central Forecasting Unit, which again, was no bad thing so long as there was someone like Douglas, the senior Senior Forecaster, to prevent you from doing something stupid.

It was an odd thing for a man who had never had any formal training in meteorology to find himself in that position after limited experience after only about a year at #3 Group. The previous work at Balloon and Fighter Command was not serious forecasting work.

Droessler: .. But you were learning fast.

Robinson: .. We learned, yes.

Droessler: .. And under some good leadership.

Robinson: .. It couldn't have been better leadership to have a year with Sutcliffe followed by a year as understudy to Douglas--I'm sure this was the best on-the-job training that anyone could ever have got anywhere. That's how it happened, that's how I happened to be at the Central Forecasting Office in 1944, the beginning of 1944. I can't think of anything really unusual that happened to me [there] except one thing from my past caught up with me, and that was the forecasting of the useful range extent of the navigational beam aids, and this was handed down to Central Forecasting Office as a routine job. You had to do this everyday and someone noticed that in the early mention of this in some meteorological headquarters thing, it was noted that I had given Stagg certain advice two years before, so I got the job of doing the routine forecasting.

I remember it because, Gold, who was effectively running all the forecasting aspects with his own very firm hand, had this stupid way of picking out silly names. The code name for this job was "navi-prop." (Navigational propagation). But one of my daily jobs was to "do navi-prop." Which I did.

END OF TAPE 1, SIDE 1

Interview with George D. Robinson

TAPE 1, SIDE 2

Robinson: .. Professor Booker was then at Cambridge University. He came to Dunstable for a couple of days to talk to me about the "how and why" of the forecasting methods which he had pretty well worked out without really realizing how the necessary information was obtained, and how reliable it was. He hadn't been concerned with the meteorological side, but his contribution was if you happen to know this particular refractive index profile, you can do this job, and this is the sort of reliability you can get out of it. He'd worked out the theory of the propagation, the ground ducting and so on.

So, I say, he spent two days telling me this. He didn't tell me how to do the forecasting, but he told me but why, if it could be done, it had to be done this way. After that, it was just a job of the meteorological team to do this every day at the CFO. If I was around, then I did it. If I wasn't around, one of the other chaps did it.

That's just an indication of the sort of thing that turned up.

I told you about Gold's proclivity for introducing stupid names. One of the requirements which were handed down to our service center forecasting was: forecast, or at least describe the snow cover over most of Europe, right up to Moscow, including Russia. We had to forecast the extent to which the ground was snow-covered. And Gold said, "You produce this forecast or this statement and the code name for this is "SNOLLY." It was snow lying, but I can remember when one of the senior forecasters named Dodds picking up this instruction, holding it like that and saying, "SNOLLY!" and flinging it to the ground.

This was the sort of thing, the way Gold did his job: "You will do this and don't ask any questions!" He was really effectively acting for N. K. Johnson as director, as far as the operational forecasting was concerned. We never really heard of N. K. Johnson, in the forecasting section. All the technical instructions came down through Gold. As far as that was concerned, he was running the office. I'm quite sure N. K. Johnson was having a hard time with the, particularly with the Air Ministry financiers in keeping the office going, but he didn't appear to be handling day-to-day problems which affected the forecasters. This was actually during the war.

But so far as my own work was concerned, this is what I was doing in April, 1944, when my next assignment came down. This involved my being mobilized--I'd been a civilian all this time, even when working at an RAF Bomber Group headquarters and Fighter Command headquarters. I had been a civilian--practically all meteorologists were at that time. I was then mobilized in the Royal Air Force Volunteer Reserve and learned for the first time that I had been on the books of the

Royal Air Force Volunteer Reserve for two or three years. I was mobilized at short notice and sent to SHAEF--Supreme Headquarters Expeditionary Force--and my instructions were to report to Group Captain Stagg, wearing the uniform of a squadron leader. I wasn't a squadron leader, I was a flight lieutenant, and you can't do that sort of thing. This was how the civilians at meteorological headquarters did things.

So I reported to Captain Stagg wearing the uniform of a flight lieutenant, which was the only uniform I was entitled to wear. The promotion followed at some time, but this was--you know, the only instructions I had in writing were to report to Group Captain Stagg and do what he told me to do. But what he ordered me to do, if you're now in the Air Force.

The date I did this reporting was about the 6th of May, 1944. I knew there were plans for the invasion, I knew that roughly it would have to be sometime in June. You picked this up at the Central Forecasting Unit anyway, apart from all the questions of secrecy, because the Central Forecasting Unit was going to be concerned in the forecasting for whenever the so-called D-Day decisions were to be made. I was sent to Stagg. I say, I don't know why they picked on me but I knew it to the extent that I was doing the job, a job which gave me the training and the knowledge to be able to help Stagg. I do not understand why me, and why not the other "stooge"--the reserve senior forecaster. Why they picked on me and not the other one, I don't know. It was probably something to do with seniority, the seniority of a month or two more...than the other man.

That's how I got onto the job. Again, what the job meant, I had about three weeks to find out before the real D-Day forecasting problem began. I don't know who decided to send me there; it certainly wasn't Stagg, although he'd known me at Kew and he might very well have asked for me if he had been given a list to choose from. But he certainly wasn't given a list to choose from. I was sent there, whether he wanted me or not. He wanted someone. I don't know who picked me, whether it was--it could really only have been Gold. And what I had to do I learned when I got there.

The first thing I did was to examine the record of the forecasting process, which was effectively a telephone conference between the main United States forecasting unit at Widewing and Dunstable headquarters, Central Forecasting Office, the British Admiralty Forecasting Office, SHAEF itself and two units--the headquarters of the Air Forces and the British Admiralty headquarters. The final two were effectively advisory listening jobs. The main forecasting units were Widewing, the American one, and CFO-Dunstable, the British one and the British Admiralty Unit, which was then chiefly concerned with state of the sea and conditions on the beaches.

The first job Stagg gave me was to examine the performance of the few weeks of trial runs of this, to the extent that just how good were the forecasts they'd produced, which had to be four to five days. In that month, they'd had some very quiet weather. Really, the only criteria I had was that there had been laid down certain minimum conditions in which an assault on the beaches would be possible, and all I could do was to look through this forecasts and see whether--how the results concerned really approached or missed the possibility of these minimum conditions. Though a five-day forecast was simple, the criterion was--

On day one, did they forecast that the minimum conditions would be met or not. And was the forecast right, or was it wrong? Same for day two. Just look at the forecast and then determine was it right, or was it wrong? Day three, etc. The result of that was that the first day was pretty good and the second day was acceptable, and the third, fourth and fifth were just toss up for it. This didn't strike me as being anything unusual. It did strike Stagg--Stagg was really, you know, "Well, look, these things are wrong. You can't do it...the criterion that I'm using is hardly likely to be met in any circumstances because if you look at the climatology, it just never happens." The minimum conditions practically never happen anyway, and the people who had set up the minimum conditions presumably knew this. The minimum conditions were too strict. I think everyone--I think all the operational commanders knew this, but they weren't willing to go on record as saying, "Well, we set these minimum conditions, but we can operate if--these aren't quite met."

Anyway, that was my first job and Stagg took this to mean that five-day forecasts were useless. They weren't, of course, but this really did surprise him. It certainly didn't surprise me. I don't think it would have surprised any meteorologist with forecasting experience in Western Europe at that time with the data, the observations available. Anyway, that was my first job there.

After this was done, I just joined in the normal routine. I listened in on the weather conferences...part of the nuts-and-bolts of the job in fact was to set up the conference, which nowadays would be a matter of pressing two buttons. But in those days, to get a conference call on secret lines to six or seven centers was quite a feat. Well, it wasn't a feat, but it could get annoying at times. Stagg [said], "You've got to get this conference call set up on time." This was amusing and at times frustrating, but I usually managed it. And there was no skill involved, it was just a matter of patience and luck, but this was Job #1, and when I got the conference call, I said, "Yes, yes, yes...." to six or seven people and they said, "Yes, yes, yes..." to me and then pressed a button and the conference was on. But that sometimes took about half an hour just working the telephone system.

When I first got onto this, it was in a good weather spell, and the conferences went off without too much trouble. I think we learned the proposed date of the 5th of June, was it, around about the 20th of May, about two weeks before. So, two weeks before, we knew what days we were aiming for and since we were aiming for five days, so that this gave us a little more than a week before the first of the so-called operational forecasts, the first day on which June 5th would appear for the first time, we had about a week before that. And during this week, the weather was very kind, cloudless skies and no trouble at all. When the real operation period began, which was I think about the 27th or 28th of May, Stagg and Yates moved down to the Advance Headquarters and left me at SHAEF to work the telephones, collect the people together. They weren't in the room but they were, as far as I was concerned, just another set on the conference call. But this removed me from Stagg and Yates personal contact, and the weather began to get a little difficult. The major difficulties, from the forecasting point of view, were concerned with the simple lack of information over big stretches of the Atlantic. The first job of the conference call was to agree on an actual map of current conditions. It was very rare that the three main stations--Dunstable, Widewing, and the Admiralty--turned up with the same actual map to begin the forecast with, let alone any forecasting. The first part of discussion was saying, "Just what is the current analysis?" which involved interpolating over 2,000 miles of the Atlantic Ocean. Interpolating. So there was the first difficulty, getting these people to--there was usually a compromise that we began with the first day. I can't go into a sort of detail...but I have notes of it. I have notes of the individual contributions of the forecasting units. I've got them here behind me in an old decaying manuscript; certainly no time to talk about them here, but the point is, getting an agreement in those circumstances was very difficult. The conferences sometimes went on for two hours--they were never less than one hour and you had to come up with something. Usually Stagg, but in his absence, Yates, would have to take these in to a meeting of the commanders. Usually five or six of them. And put over the forecast. The so-called "agreed" forecast: sometimes it was hardly an agreement. At least one of the forecasting stations would have called it a compromise, which they'd been talked into. There wasn't really much firm agreement on them.

To get down to my own part in this, it was just collecting the conference calls, but I could comment on anything. On one or two occasions, I did and I think my ideas were certainly listened to with the other forecasters. But my own major contribution to this came in personal telephone conversations with Stagg down at the Advance Headquarters and myself. To cut a long, complicated story short, there were two occasions in which I think I had some influence in what Stagg had in mind, and his own contribution to the final answer. One of these was the--as you know, there was a postponement. The forecasts convinced the Supreme Commander that he had to postpone the original date from, was it the 5th? The first postponement was indefinite, but everyone knew it couldn't be postponed indefinitely. It had to be either one or two days, or two or three weeks. Almost immediately after the conference at which this decision had been made, that forecast was very much of a compromise and we certainly got some readings which almost cleared up the situation--a cold front came over Ireland--I can remember one of the conversations with Stagg. I used these words: "A cold front has turned up from somewhere, and it's halfway through Ireland." This was the thing which confirmed that the decision to postpone was indeed a correct decision.

And then there were further developments, which suggested that there might indeed be an opportunity, with conditions quite far removed from the minima, which had been set months before, but in which experience had shown that it just might be possible to get in. That slot of a day or two, conditions which at least were not impossible for the operation. And this was the decision about putting the job on after the one day, till the 6th of June.

And I really think I had some influence with Stagg on this. One of the stations, particularly Dunstable--Petterssen, Douglas--were very unhappy about this. They didn't think that the forecast was sound enough to commit anything to. Widewing and the British Admiralty did. I think I had considerable influence on persuading Stagg that it was indeed. Well, you need more than a reasonable possibility. You need the nearest thing you can get to a certainty, in any case. I really think, in personal telephone conversations with Stagg, I inclined him to the forecast on which--well, according to Stagg's book, it was Montgomery who even persuaded Eisenhower and said, "Let's go!"

But that particular decision, just how to word that forecast, was the key to--well, starting the D-Day process. I do think I made a contribution on that particular occasion. It was not in the conferences, it was just in personal conversations with Stagg who called me at his initiative and said that he'd heard me muttering on the conference at times and he hadn't been quite able to hear what I was saying. But I'm pretty sure I did turn his thinking round to the "O.K., Montgomery, O.K., let's go" attitude, as against the Petterssen attitude: "This really isn't safe to go on this forecast." But it was a coin-spinning activity, really, in the end.

As I say, I think I made a contribution to it, apart from getting these telephone conferences together physically. I lasted for another two weeks after the real D-Day, and I was then posted to the headquarters of the Second Tactical Air Force, waiting for the headquarters of Second Tactical Air Force to move into Normandy. And really, so far as I was concerned, meant doing nothing useful for a few months, because this meant going to France as a forecaster...and all the communications were such that all the real work was done way back in England, at Fighter Command and so on--all the real forecasting work. There were times in Normandy and on the way to Brussels, where we spent the winter, where we had a completely blank chart. We got no information whatever. And I can remember going in to the commander of the Second Tactical Air Force with a day's weather briefing with a completely blank chart. And he said, "I don't suppose there's a blank chart over there in England." I said, "No, sir, they have all the information." You know, "OK, get out, play golf if you find a golfcourse." So I can't say that I did anything useful in the few months after D-Day other than occupy a tent and move across Normandy and move into Belgium.

I think nominally I did make a contribution in that they appointed me as Commander of the convoy, which moved from Amiens to Brussels. The whole Second Tactical Air Force. They needed, really, a squadron leader rank and I think they did it for fun, to choose a meteorologist as commander. It meant nothing, it was, but--I was in command of the Second Tactical Air Force, which was a unit moving on the road from Amiens to Brussels. Technically, it was seeing that they all arrived in Brussels and I have no idea whether they did or not, because a lot of them moved on ahead to get first into Brussels and see what the place was like. I commanded the rear guard. But that was the only "useful" thing I remember doing as deputy to the senior meteorological officer of the Second Tactical Air Force between, well, up to the end of 1944.

Now arrived in Brussels and thinking back to things of some interest, I recalled one thing of more interest to an American listener to this than to British meteorologists. And it was a thing which happened and soon after I went to Three Group, to Sutcliffe's group, to Three Group, I don't know the exact dates, but sometime during that time, America entered the war. My own personal first realization of this came when three American meteorologists, three of the first group of the United States Air Force to come to Great Britain were meteorologists. Someone turned them over to Bomber Command, to get an idea of how the meteorological arrangements for Bomber Command worked. Someone at Bomber Command sent them down to Three Group, and there I was and here Sutcliffe was. They sent them down because Sutcliffe was there, and Sutcliffe was doing a better job as a senior meteorological officer of the group (in presumably in Gold's opinion) than others were doing, so these three came to Three Group at Exning to see how the meteorological arrangements for a British bomber group were carried on.

To my great surprise, Sutcliffe simply turned these three people over to me. Now, you'll recall, this time my forecasting instruction was two weeks at Dunstable. And I was learning, probably learning pretty rapidly on the subject, but I was learning, and he said, "Well, you can take these on because you'll learn something from it as well as them." I remember the name of the officer-in-charge of this----, and I think he has since had quite a distinguished career. It was Chauncey Touart, Lieutenant Touart, and he had two master sergeants with him.

They sat with me, and amongst other things, at least, I could instruct them in something that--I was one of the few meteorologists who realized what was going on in that I knew Sutcliffe's methods of upper air analysis. I knew how he used them in forecasting surface charts and the like, so I was able to give at least Touart an indication of possibly the most advanced thinking in forecasting, and in particular, forecasting for bomber aircraft flying over Germany at around 20,000 feet. I don't know, I hope they enjoyed it. I don't know where they went after that, but they were part of the first American Air Force personnel group to come into England, and I had the pleasure of talking to them about our methods. As I say, I don't know what they thought about our methods. I don't know how much of them they put into practice but at least they seemed interested whilst they were at Three Group.

I can still remember being amazed and horrified that the plans of the U.S. Air Force included daylight bombing of Germany without protective fighter escort, which of course just wasn't available in those days until the Mustangs turned up later. This just seemed to me like a death wish. And it pretty nearly was. This had been instilled into them, and presumably into the whole of the-- was it the Eighth Air

Force that the Flying Fortress Wing Group were coming to England to do daylight bombing of Germany. I just didn't believe that they considered this possible, as an ongoing operation without--with the sort of losses they could possibly take. I don't really know what happened. I mean, I know what happened in the long run, that they got their fighter escorts going and won the bombing war, I'm not sure whether they won the war but [they] sure won the bombing war in the end, the American Air Force.

Anyway, the point is these were the first three American meteorologists and they were sent to Sutcliffe to receive instruction, and Sutcliffe gave them to me to give them instruction, and receive it from them. I think Touart spent a lot of time at the Cambridge Air Force Research Group afterwards. I've seen his name around.

Droessler: .. Yes. You mentioned him, you also mentioned Don Yates. Were there other Americans you came in contact with at about the time of the D-Day invasion and the forecasting--

Robinson: .. Oh, yes, I didn't say much about Don Yates. I have written things about Don Yates, and he was a very remarkable man. He was a patient man. There were times I sat in the same room with Yates and Stagg, and they were having minor disagreements about things, and during these arguments, if I had been Yates, I would have landed across that desk and belted Stagg over the head with a round ruler. He just sat there quietly. He usually got his own way in the end. He was a very good--as a handler of men, I'm sure (he died recently, too) he'd have few equals in this world as a handler of men. Eisenhower was a good handler of men, too, but Don Yates would have been just as good in his position.

Technically, he really wasn't a practicing meteorologist in the matter of forecasting for five days for Western Europe. Neither was Stagg. Stagg's only real operational forecasting experience had been out in Iraq, which isn't Western Europe at all. No, Yates was very impressive. I don't know how much he contributed to the real handing of the forecasts over to the commanders, but he was always in with Stagg. My only insight on this is Stagg's own book. I suspect that Yates had more influence on the presentation than you would imagine, from reading Stagg's book. Stagg, he wasn't the sort of personally--self-important character. Which wasn't true of Krick, and it wasn't true of Petterssen or the other people concerned. They would always assert, "When I was doing this, I did that." Stagg never put "I" as the first word in any sentence he used. He was much more willing to take advice and compromise than either Krick or Petterssen, or the other people concerned, in the pre-D-Day discussions.

Droessler: .. Were the two meteorologists who briefed the Supreme Headquarters, General Eisenhower, was that Stagg and Yates? They'd be there at all times?

Robinson: .. Yes. And they would really be the only meteorologists in those briefings sessions.

Droessler: .. So they had to carry the day for the meteorological community.

Robinson: .. Yes, I don't know how--all the group listening to them, they included Tedder, who was nominally Eisenhower's deputy-commander of the whole thing. Now, Tedder had been director of research and development for the Royal Air Force. I had in fact sort of given evidence before committees with Tedder. He knew the problems, he wasn't a meteorologist, but he knew what the troubles were and he knew what the problems were, and he had a very good idea of the reliability of a meteorological forecast. Again, I don't know what contribution he had to it, but he would understand the implications of what Stagg and Yates were saying. The others, I don't know. I mean, they'd all been commanders, they'd all had meteorologists showing them maps, [but] Tedder knew something about what had gone behind those maps.

Droessler: .. You were in a very pivotal position, George, weren't you, because you were privy to that conference and what came from the Admiralty, what came from the American group, what came from Dunstable, and so forth. You were in a position to sort of synthesize that and give the Captain as good advice as anybody could.

Robinson: .. Yes, I think he believed this, too. He did occasionally ask--I think on one important occasion, I influenced what his thinking was. But that was it.

I was now deputy to the Senior Meteorological Officer of Second Tactical Air Force, whose name was Farquharson. And this indeed was again not much of a forecasting job because there was, after all, a senior forecaster meteorologist. It's really an amusing thing, sort of thing that can happen to a meteorologist in wartime. Farquharson was the sort of man who got odd ideas. In the course of war, Flushing was attacked and, as we used to say, liberated. Flushing had been a station from which the Germans were launching V-2s, the rocket, not V-1, the flying bomb, but they'd launch V-2s from Flushing. Ferguson got the idea that there must be valuable meteorological data, or even indications as to how the Germans were operating the V-2, so he just sort of walked up to me and said, "Robinson, go to Flushing, and see if you can find out anything about this." This was all right but it was not too easy to get into Flushing. And on the timetable that did get me there, two days after the actual fighting. But I got to Flushing and the only things I remember about Flushing is meeting up with a British commando-major who'd lost his unit. Walking through dead streets--getting into Flushing down a canal on a very much overloaded barge. Finding a home in Flushing, in the house of an elderly Dutch woman who really didn't know what was going on except that her house had avoided being completely destroyed. And sharing a meal there because the British commando major had two packs of emergency rations and I had a liter bottle of cherry brandy, which I'd picked up somewhere in the wreckage going into Flushing. And the Dutch woman cooked the rations, fried the triangular sausages and someone consumed most of the cherry brandy, and the next morning I set out to find out where these rockets had been launched from and what was left. I got into a room with several tough-looking, large but quite pleasant Dutchmen who told me that the headquarters had been in the

Linke Reduit, and there was no way I was going to get into the Linke Reduit until they had gone through it themselves. They were interested in whether any Germans were still running around Flushing, of course. But there was no way I was going to get any information out of it, so I sort of got back home, told Farkie I'd got nothing, and he said, "Why did you get nothing?" And I said, "There was nothing to be got." But that was an interesting two or three days for me, what happens to a meteorologist when he has no meteorology to do.

And the other thing that happened in Brussels, of course, was on New Year's Day. The Army and Air Force headquarters were attacked from the air by Messerschmitts. This was at the time of the Battle of the Bulge. You know, spent bullets hit the walls of the meteorological office.

END OF TAPE 1, SIDE 2

Interview with George Robinson

TAPE 2, SIDE 1

Droessler: .. This is tape 2 of the interview with Dr. George Robinson, in West Hartford, Connecticut, on the 27th of June, 1994.

As we begin tape 2, George, why don't we finish up your World War II experiences and then move you back to England?

Robinson: .. We were in Brussels, weren't we? It was in about March, 1945, I think, when we, Second Tactical Air Force Headquarters moved out of Brussels. We went into Germany, still on the west of the Rhine, to a place called Suchteln, which the Germans had last used as a typhus hospital. So, before going there, we all rolled in DDT, literally rolled in DDT, and were immunized for typhus. This was a short stop because after the Battle of the Bulge, the American forces crossed the Rhine at Remagen, and the move really into Germany began. The Second Tactical Air Force and the British Second Army moved to a very pleasant situation, called Bad-Eilsen, near Hamlin, not too far from Hanover.

The most memorable thing from the point of view of the meteorological office was that this brought Sutcliffe back in charge of us, because Sutcliffe had been with the Second Army and had had various jobs in Paris, again at SHAEF headquarters. And he came back to head the meteorological section of what then became the British Air Forces of Occupation--BAFO, situated at Bad-Eilsen.

What we did there was the usual routine of peacetime forecasting and attempt in some way to rebuild a meteorological service in the area, in the British Zone of Occupation. Because all our assistants and many of our forecasters were being de-mobilized, and if there was to be a meteorological service, we had to have Germans helping us. We had quite an interesting time finding suitable Germans, and Sutcliffe gave me the job of touring part of the zone and digging out German scientists and asking them for suitable names of suitable people. I'll just give one rather amusing detail of this. There was at Göttingen a remarkable collection of German scientists. I think there were at least three Nobel prizewinners there. And in charge of that operation, was one Flight Lieutenant Richard Goody. He was in charge of that, and I went to Göttingen in the hope of meeting someone there who could tell me what had happened to certain German meteorologists I had on the list, and I remember driving there, knocking at the gate, and being met by a forbidding-looking German woman. I said that I wished to see Flight Lieutenant Goody, and I flashed the squadron leader rings on my cuff and said I wanted to see Flight Lieutenant Goody.

"Flight Lieutenant Goody is far too busy."

I said, "Don't talk nonsense." If I'd had better German, I would have made the point better.

"Flight Lieutenant Goody is far too busy to see anyone this afternoon. You could see Professor Prandtl."

Professor Prandtl: the "king of turbulence." So I said, "I would very much like to see Professor Prandtl, not Flight Lieutenant Goody." And I had a pleasant afternoon with Prandtl, but he was getting on in years at the time, and he talked more about how he had missed butter in the war than about meteorology. Nevertheless, he gave me signed copies of some of his better papers and told me that if I really wanted to find German meteorologists, I had better go to Hamburg.

Now we had a meteorological officer in Hamburg, one John Bell, wing commander. Not a scientist by any means, but one of the best extemporary administrators that I have ever known. And he took over this organization of the German service. He had his problems. He had been left behind by the German Air Force about 100 Luftwaffehilferinen. And he had to feed and clothe these Luftwaffehilferinen, and the only way he could do this was to storm into the mayor of Hamburg and almost tell him that the war would begin again if his Luftwaffehilferinen were not properly looked after. He was that sort of chap, he just got hold of the mayor of Hamburg. And the mayor of Hamburg was a busy and a very troubled man at that time, I can assure you. But he looked after John Bell's Luftwaffehilferinen for him.

That was just the sort of thing that was going on. We got some pretty competent German meteorologists who did a very good job in difficult circumstances. But that just gives an idea of what the meteorologists in Germany at that time were most concerned with, not science, not even weather forecasting, just sorting out the possible future of a meteorological service in that part of Germany.

I myself was promoted to be a wing commander and the Senior Meteorological Officer of an RAF group. It happened to be the number two group of ground attack aircraft, and all I remember is that it got me the occasional flight in a Mosquito. I think that's all I did for some months until de-mobilization in June, 1946, yes. In the meantime, I'd been promoted not only in the RAF VR, but in the Civil Service, and they had made me a Principal Scientific Officer. I must admit that most of the wasted time in France and Belgium and Germany--wasted from my point of view; I had been considering trying another career and possibly writing to a friend of mine who was now in the radiochemistry department at Harwell and the like. But this promotion to Principal Scientific Officer at my young age was a bit unusual, so I thought, well, better stick to meteorology, Robinson. I didn't make any attempt to change this meteorological career.

If I could go back quite a long way, there was one other thing I haven't mentioned. When I went to Kew Observatory, I was told that they had long-term ideas about my life and they thought I would be a good man to go on a non-magnetic ship on a world magnetic survey for three or four years. I say "they": these were the personnel department of the meteorological office. It wasn't their ship, but they were so sure I should be on it they were planning my career. At Kew, I spent some time supervising the construction of magnetic instruments for this ship. And there was a non-magnetic ship so its use was sweeping magnetic mines, which it did very well until it was hit by an incendiary bomb and burnt to the water in the Thames Estuary, and that was the end of the non-magnetic ship and the end of my career as an ocean explorer. It was just as well, because I would have been seasick, and the magnetic instruments that I had planned were, by the end of the war, quite antediluvian. There was a most incredible change in magnetic instruments in that period. All the modern instrumental work was done during the war and the need for a non-magnetic ship was obviously going to be non-existent. With airborne instruments...

So, for a while, I was doomed to travel the world in a non-magnetic ship making observations. It affected me in some way at the time but it's probably best forgotten now because I would have been a lousy sailor. That's going back.

We're now--I'd been de-mobilized and gone back to England as a Principal Scientific Officer, and Stagg had gone back to his prewar job, as Superintendent of Kew Observatory. And he asked for me to be posted back to Kew Observatory, and indeed I was. And Stagg had started work on just simply measuring solar radiation before the war, and he continued that way, expanded that. I began to do some work on terrestrial radiation, principally because there was a remarkable instrument at Kew, which had been designed and built by L. F. Richardson and W. H. Dines, no less. It was a pretty precise instrument for measuring terrestrial radiation. The one thing that terrestrial radiation was missing in those days was pretty precise measurements. It could do pretty precise calculations. So I began to use this and that really set the line of work I was engaged in for some years after that. No theoretical work, but purely practical observing work, the first really precise set of measurements--of what was then and still usually is called atmospheric radiation--available then. The Dines-Richardson instrument had been used by the observers at Kew Observatory on a fairly regular basis for a long time, but they didn't realize how carefully that instrument must be treated, which I did. So I got some good measurements and began to work in the area of radiation and Stagg, who was, I think he was the secretary of IUGG or the treasurer, and he got me on one of the IUGG commissions, then it was the IAM (International Association of Meteorology). He put me on the radiation commission and this got me about 1950 into the international conference, almost a circuit, you could call it. I began to have some involvement in the planning of the IGY (International Geophysical Year), mainly because of being on that so-called radiation committee of the Meteorological Commission of IUGG.

Actual involvement in IGY--again I was on the British National Committee of Meteorology and the British National Committee of Seismology--because Kew was after all a seismological observatory--and mainly the meteorology got me involved in IGY work to some extent. We did a field experiment at Kew. We planned a surface radiation station for the IGY, and we actually set one up at Kew Observatory and ran it for a year before the IGY began, but it was set up as a possible surface radiation station. Many of the surface radiation stations were based on this, but we did run the thing to see that it worked and we got a year's results at Kew and they were published in an IGY number of the Proceedings of the Royal Society, not the Royal Meteorological Society, at an international meeting of the IGY as an example of what could be done by the IGY. I don't know that any of the other people who were concerned with instrumentation of stations actually tried out what they were proposing in the kind of circumstances they would be used during the IGY, we just ran the thing at Kew with no one agreeing to the plan at the time. But when they saw what we got, several other countries put in the same sort of station. But the really interesting thing about the IGY was that it resuscitated, I think, after about a 50-year period, serious international geophysical observations, coordinated, done at the same time, done as far as you could get to, all over the world, and with the same objectives and trying to get the same sort of precision and accuracy in the instrumentation. That was the real importance of IGY. It never stopped. The doggerel at the time was: "Old geophysicists never die, they just prolong the IGY." Indeed, you could follow a continuous record of international cooperation in geophysics and meteorology from the beginnings of the IGY.

I was, if you like to use the word "proud" to have been mixed up with it in the beginning in a useful way, but at times, it did get out of hand. Cooperation for the sake of cooperation, for the sake of keeping cooperation going, I think that happened at times.

Well, back to Kew Observatory. I proposed to Sutcliffe, who proposed it to Sutton, who agreed that we should start some really high atmosphere work in the Meteorological Office with rockets almost certainly in mind and satellites a very good possibility. We began to do this at Kew Observatory, which was really a ridiculous place to have that kind of physical laboratory, which require clean rooms and since we were using ozone to a great extent, you had to have very careful ventilation and the like, and Kew was, I think, built in 1760-something for George III to look through his telescope, and looked as though it had been built--it was a beautiful place on the outside, but it was just hopeless on the inside, of course.

This was a time, then, when Sutton in particular began to push for centralization of all the headquarters activities of the Meteorological Office, and get out of Central London. There were people who didn't want to get out of Central London, but there were more who did and in fact the government's plan was to get as much of the Civil Service out of Central London as was possible. There were new towns, so-called, new town being built, one of which was Bracknell, near Reading, and suitable sites were available and I think it was mainly Sutton's pressure within the Air Ministry that got the agreement for the Meteorological Office to have this central structure and to go there. As far as I was concerned, the central structure gave the possibility of getting decent laboratory accommodations.

In the end, we got it and the rocket work moved from Kew, the satellite work moved from Kew and in the end, everything moved from Kew, and I think Kew Observatory is now the offices of some large corporation. When I say it's their offices, it's their showpiece office, where the top executives meet in the middle of the golf course, in a 250 year-old building, beautiful on the exterior. And since they--it's probably beautiful on the interior now, but it was never going to be a physical laboratory or indeed a geophysical observatory with all the electrical and aircraft activity and the lot going on around it...

Directly below the main approach, one of the main runways of Heathrow Airport, for example, so you don't get a lot of peace and quiet there. If you're trying to do anything with shortwave radio waves, they bounce off the airplanes. You've got no noise-free atmosphere in the biggest possible implication of the word "noise": there's noise in everything. So we had to move. We got these laboratories in Bracknell; they were quite well-built. I get a little worried, because this thing was built in 1960 and the contractor who built it said it was a "25-year building." It's got up to 30 years and it's going to have to go on a lot longer than that. So in time, it will become a slum, but I won't be around at that time.

As I say, the rocket experiments were successful; the satellite experiment was partially successful. They could have done a lot worse on a first attempt, but it didn't produce any really startling results and none of the early satellite experiments did. They were just not precise enough, and there just hadn't been enough experience in making them to last for more than--well, in our case, it was a day or two before the mirrors began to deteriorate. But, OK, next time someone put those mirrors up, they weren't the same mirrors.

The one curious result of the fact that I was engaged in an experiment on a satellite was that I was appointed to the World Meteorological Organization's panel of experts on artificial earth satellites. The history of that has some interest. The initial interest of the World Meteorological Organization as such in satellite experiments was of course pressure and very prescient reports of the American meteorological community. The startling culmination was, I think, in 1961, when President Kennedy addressed the General Assembly of the United Nations. One of his points was that we will encourage cooperation in meteorology and atmospheric science effectively because of the new possibilities opened and opening by the use of satellites. The meteorological satellites were mentioned in that address.

So the World Meteorological Organization as such had to take notice of a thing like that. So the first thing--I'm not quite sure how this happened, but the first report they had was produced by Harry Wexler and Victor Bugayev, a very good Russian meteorologist. I don't know what official standing they had, who commissioned it, but it was a report which was considered by the World Meteorological Organization [not at] the big convention, but one of the bigger meetings with all the representatives and I don't know what committee within the WMO had considered this and what they thought about it and indeed, who commissioned it, but it was this two-man report and some committee in the WMO said, "Now the Commissions should see this..."--in particular, the Aerology Commission and I think it was the Synoptic Meteorology Commission. Sutcliffe was head of the Aerology

Commission and Bill Gibbs of Australia was head of the Synoptic Commission at that time.

Sutcliffe showed this document to me and said, "Just another wish list." And I said, "Hold on, it's a wish list, but there might be something that could be done with it." Sutcliffe talked to Gibbs in Australia and they agreed that there should be more consideration of this from their point of view. They told this to the WMO commissions: "Each of you appoint another member who can join Wexler and Bugayev and consider revisions of this document." Bill Gibbs took it upon himself. Sutcliffe appointed me. That's how I became an expert on artificial earth satellites.

But it was very interesting work. It was indeed a wish list, I think, mainly because of Gibbs, who had been in the synoptic business and knew the ramifications and knew where the major missing data were and the qualifications of various people in various parts of the world. I think he took a lead in it but in the end, we all got answers. And we produced a very detailed plan from the wish list. We had major centers in Melbourne--Bill Gibbs; Moscow--Bugayev; Washington--Wexler. I didn't get one, but I didn't particularly try to get--

But those were the main centers. Then we had subsidiary centers and we worked a lot of detail on the communications. We began to say, "How much is this going to cost?" We put in an amended report. As far as Sutcliffe was concerned, he had taken no notice whatever of what we were doing, but when he got back, he said, "Now this is good. We've got to do something about this." The only other comment I remember was [that] Wexler said it was "a pity we'd taken all the poetry out of it." Those were his very words. But he didn't disagree with what we'd done, but we did no poetry.

I had been appointed chairman of this panel by my fellow members because I was the last one to appear for the first meeting and they told me that they'd appointed me in absentia. So I was chairman of the first meeting and the second, "[You] didn't do too badly, Robinson, we're not going to change now." So I was chairman of all the meetings. The last one was in my view a very important meeting. It was the first meeting of the WMO and ICSU to discuss an international project. Wexler had named our project the "World Weather Watch." And somewhere else, another big document came from America..."GARP"--the Global Atmospheric Research Program was being discussed in ICSU circles. And WMO governmental circles and ICSU--if you like, scientific circles--we were talking about the World Weather Watch and they were talking about GARP, so someone said, "We ought to have a joint meeting, at least, of the present ICSU committee, and the present WMO committee, which was a panel of experts. And I was chairman of that meeting, which resulted in the detailing of the GARP program, detailing of the World Weather Watch program, the formation of what they called the JOC, the Joint Organizing Committee, WMO and ICSU together on GARP.

I can't say what I contributed to that meeting, but at least I was chairman. You know, if you're proud of anything like that, I'm proud of the fact that I was chairman of the meeting. I was immediately removed by Sutcliffe from the Joint Organizing Committee because, he said, "I'm going to do this myself, Robinson." I wasn't on JOC, I don't think Gibbs was. Wexler, of course, was on it, Bugayev was. Anyway, I don't where JOC went, but it's still going.

I really think that brings me to the time when I left the British service. I left the British service when I did because I had been in the same job for almost ten years; it was Deputy Director, Physical Research. Mason, who had taken over from Sutton, had told me that I was "unlikely" to get any further promotion within the Meteorological Office. Which means I was unlikely to get another job because there were only two above me and they'd been filled. I made an application for another job in the British Civil Service and the newly-formed Natural Environment Research Council, secretary of. But the amateur chairman--well, not amateur, but part-time "expenses only" chairman of the [Council] was Sutton, who had retired as Director-General of the Meteorological Office and taken on this part-time, remunerated but part-time, job as chairman of the Natural Environment Research Council. He, Sutton, told me to apply for the paid post of Secretary of the [Council], which I did, and indeed was interviewed and was not chosen. There was quite a list of interviewees and Sutton, afterwards, who sat on the Committee of course told me that the Committee decided that there were only two of us who could stand up to the Treasury. And the other one was a marine biologist. And Sutton was a meteorologist, and I was a meteorologist, so we can't have two meteorologists. Everybody will get very jealous of meteorologists!

So I didn't get that job and I wasn't going to get another one in the Office so I was 55 years old and at age 55, the pension arrangements become much more sensible. You lost most of your pension if you retired before 55. If you retired at 55, you'd get it pro rata for the service you'd done. So I could move, and I just thought I'd like to try another job of some kind, somewhere.

I came to the USA at somebody's invitation and then I was interviewed at two or three universities, and I went to see Bob McCormack, an old friend of mine who had spent a year at Kew, and he was in Cincinnati at the time, the offices were [there]. He was the chief meteorologist with the then Air Pollution Control Organization (APCO). I told him I was there and he said, "Look, I can't give you a job, but Glenn Hilst could. I'll talk to him." Glenn Hilst was then, I suppose, he wasn't the boss of the Traveler's Research Center, but he was of one section, of the section that inherited all of the meteorological work. And I got a letter from Glenn Hilst which offered me what, to an English civil servant, seemed to be a very respectable remuneration and terms. And so, I guess I had considerable difficulty in persuading my wife, but again, our children were of the age when--the boy, Malcolm, was just going to University and the girl, Jenny, had just matriculated and was going to change schools anyway. She was 16 at the time.

Droessler: .. Had you known Glenn Hilst before?

Robinson: .. At odd meetings, but not closely. I knew him.

Droessler: .. He was one of the principal leaders within that Traveler's Research Group.

Robinson: .. Yes. Bob McCormack, who got the idea, had said, "I have no doubt that he'll give you a job. It could be a well-paid job." That was in Bob McCormack's own words. Whether Glenn thought it was well-paid or not, I wouldn't know, but I did, with British Civil Service salaries in mind.

It so happens that the head of the Traveler's thing at the time was Doug Brooks. And Doug Brooks was a man to whom, many years before, I had sent my then, the most precise measurements of atmospheric radiation available. I just sent them to him without any comments at all. And he used them in some theoretical radiative transfer work. He thanked me effusively. The next time I heard of him, I was going to take a job at the organization of which he was a part.

Droessler: .. When was this?

Robinson: .. The year I came over was 1968, August. I think that was probably the last month in which the Traveler's Research Corporation turned a profit. They didn't blame it on me, but I think that is a fact. They went downhill very quickly, mainly because they lost support of the Traveler's corporate body, because the head of the corporate body died unexpectedly, from smoking.

Droessler: .. Yes, he was a very close friend of Tom Malone's. Tom and he were close together, and Tom, of course, organized this Traveler's Research Group. When he died, Tom really lost the corporate interest.

Robinson: .. He did his very best to look after the Traveler's Research Corporation, and particularly the meteorological-physical side as distinct from some of the education and health delivery and all that sort of thing that they were getting into. But I think he lost interest in the Traveler's Corporation.

Tom Malone went to [the University of Connecticut] and was a dean at the University for a time. He was very interested in getting us a clear affiliation with the University of Connecticut. That seemed to be progressing pretty well for a time, but the story we had was that the junior faculty was protesting because of these people coming in from outside at what were effectively tenured positions, and they wouldn't have it. I think it was the junior faculty which negated the possibility of some sort of connection there, but I doubt whether it would have lasted.

Droessler: .. Also, I think when they would look at you--I think, if I were one of the junior faculty members at the University of Connecticut, and I would look at this group of very high-powered scientists, meteorologists and atmospheric scientists, I'd get a little worried about them coming to my university. So in terms of what it was going to mean to me...the University of Connecticut at that time was not a very potent research university.

Robinson: .. I never really thought they were going to take us on. I think Tom did for a time. I think he really thought that he could persuade them to do it.

Droessler: .. He's very persuasive.

Robinson: .. If anyone could have done it, he could have, but he didn't in the end.

Droessler: .. Bob White was the head of your group at this time?

Robinson: .. No, Bob White was with the Weather Service. He left before I arrived.

Droessler: .. I see. What happened to your group then?

Robinson: .. Well, we split into two. Glenn Hilst had been looking pretty carefully into the possibility of going off the not-for-profit and starting a for-profit research group in atmospheric pollution. And he and Art Bostick, who was also there at the time, had done a lot of work and they'd run around, financiers, and so on, and they'd made a pretty good plan for running as a for-profit group. The Traveler's would have none of it and the for-profit group that formed at the same time with the same objectives in Boston--what did they call it? Mahoney and someone else started it there as a for-profit group. It was an immediate success.

Droessler: .. The Environmental Research Corporation, I believe it was called.

Robinson: .. ...It could be that. [Narrator's note: However, the Traveler's Corporation was not interested and Bostick and Hilst sought and found an outside source of venture capital and left CEM, together with other members concerned with air pollution monitoring and modeling, to form their own commercial enterprise.]

END OF TAPE 2, SIDE 1

Interview with George D. Robinson

TAPE 2, SIDE 2

Robinson: .. Their successors--I mean, none of the original people remain, but they still are in business as a pretty substantial corporation, and environmental consultants. I forget what they call themselves. That changes from year to year. They made a profit for many years. The rest of us--I'm not quite sure what Hilst did. He didn't join in any executive position, into what we called "Bostick Group," and later than that he went off to Princeton, to a university group there. I don't think he had much faith in the possible success [of] again what we called "Bostick's Group." They were reasonably successful. They didn't have anything like the enormous success of the Boston company, but they lived. The rest of us were sort of left on our own, just responding to requests for proposals and the like and the social sciences slowly disappeared, the health delivery organization slowly disappeared, the education group slowly disappeared, but what you could call the meteorological group just hung on, mainly through NSF work, but to some other things. And I think I can give a better idea if I talk about the work I personally did with Traveler's.

I said that Bob McCormack effectively got me there. He and John Ludwig, I think he was Chief Engineer of the Air Pollution Control Organization, awarded me my first job: it was to produce a summary of the possible long-term effects of air pollution and to recommend a research program. I did this, I've still got it, it's one of the things that I think I did well. The research program, though, by modern standards, was rather comic because John Ludwig said, "I might get a million dollars a year for this, but if you say anymore than that, we're going to be in real trouble." So I put in priorities and costed it out in time spent and so on, this million dollars.

My first priority was acid rain. My second priority was trace gases in the stratosphere. This was 1969. I didn't put carbon dioxide and global warming in because: a) I didn't think it was possible in the circumstances of the time, and b) I thought it was going to be so enormously expensive, but see I thought it was a coming thing, so my recommendation was that they don't attempt to start any research program of their own on this, but keep it in mind as probably the biggest disturbance that human beings have made to the atmosphere and it did have potentially considerable consequences.

I rather liked those first two priorities because you could do something about both of them. In fact, something has been done about both of them.

Droessler: .. Very much so.

So really, you delivered a landmark report at that time.

Robinson: .. It wasn't noticed at the time because just about the time that the detailed costing and time analysis was delivered, the Air Pollution Control Organization was dissolved and the Environmental Protection Administration was formed. John Ludwig lost his job, and a discreet inquiry which I made some months later to the man who'd taken it--effectively, that John Ludwig at the time, the chief scientist, chief engineer on the program side--was Stanley Greenfield, an old friend of mine, and the brother of one of the props of the Traveler's Research Center and so on. I said to him, "Have you seen this?" He said, "Yes, it's wonderful." I said, "Are you going to do anything about it?" He said, "At present, I have funding only for projects of immediate domestic importance." So he was not going to do anything about long-term effects at all...this isn't Greenfield, this is what he was told by the lawyers and accountants in charge, that "you've got these funds and you better spend them on projects of immediate domestic importance," presumably with an eye on Congress.

Droessler: .. George, were you encouraged at that time to publish a report or synopsis of that report in the **Bulletin of the AMS** or some other journal?

Robinson: .. No. I've got a copy of the survey. I looked for a copy of the actual program with the costings and the time spent and the like, and I haven't found that. I've still got the survey. The survey has twice turned up at meetings I've been to, one of them, at least 20 years after it was written, and held up as a sort of "this could be true now, chaps, and what did we do about it? Nothing!"

Droessler: .. That's what happens sometimes to a really solid report that's delivered to one of the federal agencies. It kind of sits there on the shelf, and the author or authors are not encouraged to take the essence of that report and publish it.

Robinson: .. Then the organization that commissioned this report and paid me to do it was non-existent. Two days after I put the final report in the mail, the organization disappeared. And Ludwig, who was the sponsor, didn't have a job in the new organization or at least he may have been offered one, but he didn't take it.

That was my first job with Traveler's Research Center. By the time we finished the University of Connecticut negotiations, we had become the Center for the Environment and Man. This had been registered as a non-profit corporation in Connecticut for several years, long before I came, just as a name. So we were now the Center for the Environment and Man.

Droessler: .. This was the early seventies?

Robinson: .. Yes, yes.

The one thing that did happen to me probably as a result of that was that I was called to attend a big conference in Williamstown--"SCEP"--Study of Critical Environmental Problems. This was organized by a committee which included Tom Malone and the leading participant was Carroll Wilson, a professor in

Massachusetts, who had been some high official at least in the Atomic Energy Organization, if not even the chief of it. There were about forty people there, concerned with it; I don't know who picked me to go, but I was sort of asked to go and told I would be paid the usual pittance that you get in these enormous working groups. And I got there, and to my surprise, two days after that, Carroll Wilson said: "We want you to take charge as chairman of the monitoring group." So from being an unknown recruit from CEM, I became chairman of the monitoring group. I suspect it was because the monitoring group had been stacked with the satellite people--NASA people. Someone mentioned to Carroll--it may even have been Tom Malone--"If you're not careful, NASA is going to take this over, and what can we do about it?" Again, it may have been Tom saying, "Well, you know...Robinson's not oversold on satellites, try him." I suspect that's what--

Droessler: .. That probably was Tom.

Robinson: .. It was a remarkably well-run conference. It did things. Amongst other things, it produced this book which--this was produced within a few months of the conference of forty people dissolving. And it's well-written--

Droessler: .. What is the name of the report?

Robinson: .. "Man's Impact on the Global Environment"...it's a report of the Study of Critical Environmental Problems (SCEP), sponsored by Massachusetts Institute of Technology, published by MIT Press and--

Droessler: .. What year was that published?

Robinson: .. "Copyright 1970." So most of the work--if this was late 1970, then--it was summer, it must have been the summer of 1970 that the workshop took place and this was out before the end of 1970. It's a 200-odd page text, 318 pages. Carefully written, names of everyone concerned by working group.

Droessler: .. What do you think was the impact of that report? As I would look back to the 1970's; 1970-71, that would be kind of a visionary report of man's impact on the environment.

Robinson: .. It wasn't visionary in the sense that Wexler and Bugayev's first report to WMO was visionary. The poetry had been taken out of it. It was--everything that it suggests was at that time possible and is still possible. It was visionary in the sense are you ever going to get anyone to pay for all this, but it wasn't visionary in the sense that it didn't tell you how much it would cost. It does tell you how much it would cost. And priorities and so on. The immediate--

Droessler: .. So it was more of a blueprint report for the future.

Robinson: .. Yes. Well, what little was being done on the subject is set out there, but it mainly is what should be done in the future. And don't run away and think this is going to cost you nothing; it IS going to cost you something, and it's going to cost at least that and that.

The immediate result was the call for another similar conference with foreign, much more foreign, participation. This was held in the next year near Stockholm. My own position in this was that I was one of the members of the organizing group, which again included Tom Malone and Carroll Wilson, William Kellogg and myself. And William Kellogg and myself were appointed as joint secretaries. I remember the discussion about what they should call it and I said, "In British civil service organization, the boss is a secretary." So they called us "joint secretaries." This again was a very good conference and it has a good number of world-famous scientists, foreigners, but again I think the most remarkable thing was the fact that this book was produced in a few months.

Droessler: .. What is the name of that book and who sponsored it?

Robinson: .. The name is "Inadvertent Climate Modification: Report of the Study of Man's Impact on the Climate, Sponsored by Massachusetts Institute of Technology, Hosted by Royal Swedish Academy of Sciences and Royal Swedish Academy of Engineering Sciences." This really did happen. This got into the United Nations and there was a big conference, again, in Stockholm, about a year after this, with this as its real documentation. It's a good book; Phil Thompson wrote quite a bit of it. He was one of the section chairmen, and it was written in the main by Kellogg, myself, Phil Thompson and Steve Schneider. We did the editing and writing within a few days, certainly in less than two weeks, in Stockholm and handed the text to be taken home to the MIT Press. But we worked! We did a lot of work on it.

It was used as one of the major papers at an United Nations conference on just this matter--global pollution problems, of all kinds, water, air and dirt, climate modification. So as far as I was concerned, that was one of my main jobs in the 1970-72 era.

The next one I was caught up in was CIAP. Remember CIAP?

Droessler: .. What does that mean, CIAP?

Robinson: .. I really don't know. It was a result of the great Supersonic Transport scare. Following the conference in Stockholm, my next big involvement was effectively concerned with stratospheric pollution by the proposed fleet of supersonic transports. I said the "proposed fleet" of supersonic transports--at the time the Concorde was in prototype, but there were plans to--I think this is almost a quotation: "20 years from now ['now' being 1973, so we're now 20 years from 'now'], it is expected that there will be a fleet of several hundred supersonic transports flying on worldwide routes." How many are there? These were big planes; these were 250-passenger planes.

They were planned, they were buildable. Boeing could have done it. They had them on the drawing board. It was feasible. Whether it was economically feasible, I wouldn't know; I mean, experience with the Concorde suggested it wasn't but then nobody ever thought that that was going to be an economic success, just a trial supersonic transport effort. But the Boeing one was well thought-out. That was the prognostication in 1973, that there would be 200-300 of these things flying 20 years from now.

The great worry of course was what would this do to the upper atmosphere. My great worry was always what was it going to do to noise--how are you going to stand the noise of 200 supersonic planes flying over land, over populated areas? That was always to me the big worry.

But the real scare was the effect on the upper atmosphere. Not only on ozone, but on particulate matter and so on. They were scientifically respectable problems, they really could be damaging to a lot of things, could--no one really knew, no one had worked through the details at the time; there were just these possibilities, mainly in ozone depletion and its biological effects. And possibly with the sort of particulate haze in the stratosphere, with effects on solar radiation, and indeed precipitation physics that hadn't been worked out. There were a lot of unanswered questions. It was attacked almost on the scale of getting people on the moon. The program was funded by the Department of Transportation. It produced, I think, five documents of which the only one I have here is extremely heavy and I can't tell you immediately the number of pages, but the tenth chapter has a hundred pages in it. It's almost a 1000-page document.

Droessler: .. What's the name of that document?

Robinson: .. This is CIAP, Monograph 3, "Climatic Impact Assessment Program Monograph 3." In fact this particular one is number three in the CIAP monograph series--**The Stratosphere Perturbed by Propulsion Effluents**. It's a final report and it's prepared by the Panel on the Perturbed Stratosphere, G. D. Robinson, General Chairman, H. Hidalgo, Executive Secretary, R. Greenstone, CIAP Coordinator, Editor-in-Chief, A. J. Grobeker. Those names will be known to everyone who was concerned with this very large program. The date of this particular one is September, 1975.

So I think you can say that CIAP was a three-year program. It did a lot of good work. It didn't really solve the problem, particularly of ozone depletion because--if I may just mention some words I had with a very distinguished chemist during the course of this. We were talking about possible errors and the trouble was always that this was concerned with meteorology and chemistry. And I remember saying to Harold Johnson, the great chemist--he was one of the people who had started it all, it was mainly his estimates of possible disaster, which had come into it, but I said, "The meteorology is wrong, all meteorology is wrong, but it's unlikely to make more than a 25% difference in the final answer of ozone depletion. It was wrong as it could be. If the chemistry was wrong, you might even get the wrong answer for the sign." And Johnson said, "The chemistry is NOT wrong."

The chemistry WAS wrong; the reactions were all there and they were all right, but the rate coefficients that had been measured were all over the place. Because of the proceedings, the effect on ozone changed from positive amount to negative amount, as the chemistry reaction co-efficient was changed. I think in the end, when the reaction coefficient had become reasonably certain and there was a very, very small effect, very small effect on ozone. By that time it was becoming pretty obvious that 300 supersonic transports would be an economic disaster. People weren't going to use them if it cost twice as much as going in a 747. But the scientific result was very small, very small effects, and we're still not quite sure in which direction. I got, as a result of this, as did many of the other participants, a nice little certificate from the Department of Transportation saying that I had earned the gratitude and respect of all who fly. I brought this back to CEM, and I put it on my desk and looking over my shoulder was one Hans Jochs, a railway merchant, who said, "It's nice to know the pigeons have been pleased."

But, as far as the Center for the Environment and Man was concerned, that produced reasonably remunerative work for almost three years. As far as the country is concerned, I don't know whether anyone ever added up the cost, but it was very considerable. It's not a cheap thing, even to measure the reaction rate of a radical interaction, over a wide range of temperatures and pressures. Chemists, I'm sure, had a very, very interesting time out of it, and didn't lose money on it. But, again, when you look at it in the end, and say, well, it really did nothing from the practical point of view, deciding what real damage the supersonic transports were going to do to the stratosphere. It came out with probably very little.

But for a time I was the chairman of a large and often-changing committee, which was concerned with the actual effects of propulsion effluents. I don't know why--I knew a lot less about what was going on than most of the people there.

Droessler: .. Before we leave the supersonic transport problem or issue, let me ask, have you ever flown on the Concorde?

Robinson: .. No. I used to fly across the Atlantic occasionally but often on Royal Air Force transport command planes, including the ill-fated Comet. But nowadays when I pay for myself, I take the cheapest possible flight across the Atlantic, and that is certainly not the Concorde.

Droessler: .. I thought, maybe since you were involved in this study at a high level that they offered you an opportunity to travel at a reasonable rate across the Atlantic on the British Concorde.

They're still operating today, I think, quite successfully. I wonder if they're doing all right from an economic point of view.

Robinson: .. Well, as far as I can tell, they're doing a lot of charter flying simply because they're the Concorde, and they'll take it. They can take you by roundabout route to Australia and you're not interested in saving time getting to Australia, just interested in flying at twice the speed of sound, or whatever it is. And sitting in--I don't know, it's probably a big seat, but it's a very restricted place. Before I came here, in England I was on a committee considering the meteorology of flying the Concorde and at the end of one session, the chairman with a broad smile on his face said, "You'd like to kill this project, wouldn't you?" And I had been talking about the difficulties of flying into Singapore with thunderstorms around--you know, Singapore was one of the places that made stops on the Australian flight. If you wanted to avoid the top of a cumulus cloud on that damn thing, you had to start taking action 70 miles away, just to get round it. And that with all the frequency of thunderstorms and he just grinned at me and said, "You'd like to kill this project, wouldn't you?" And I said, "Yes." And he said, "But you won't."

Droessler: .. How high does the Concorde fly?

Robinson: .. I really don't know. It's not too high. I think it's in the 65,000 region. Compared with the U-2, it's low.

Droessler: .. Between 50-65,000 [feet].

Robinson: .. It may be more like 60,000 on its normal--

Droessler: .. So that would be well over most of the thunderstorms.

Robinson: .. Yes, sure, yes. But, you know, the times of the year in Singapore when you have a thunderstorm every afternoon, and they're not all that size, but a lot of them are.

In the course of the Concorde preliminaries, there was a test over London, of public reaction to supersonic aircraft. Done with RAF supersonic planes, of course. A number of people were asked to report in writing on them, and my wife, Eileen, was one of them. Her considered opinion was that if they could give her five minutes notice of when it was going to happen, it wouldn't worry her a bit. Otherwise, she might drop the dinner.

After publication of the so-called CIAP report or reports, the work on the program didn't end. FAA took it over, they called it the High-Altitude Pollution Program, and no longer specifically referred to the supersonic transport--it was "pollution of the stratosphere" of any kind. It was funded by the FAA. There was a scientific advisory committee on which I sat from 1978 to 1982. I can't tell you anything that committee really did, except clean up the chemistry of the CIAP report. It didn't make a great deal of difference to the answers, but it did a much more thorough job with the chemical transport models. But I really don't think it did anything that would have affected any FAA policy on stratospheric flight. It cleaned up the science quite a bit.

That led to a next piece of employment, which was a NASA program, not an FAA program. It was known as the "Stratosphere-Trophosphere Exchange Program" and that was in existence from 1982-1988. I was a member of a committee with the curious name of the "Executive Committee." This was mainly concerned with high altitude flights, flights by U-2 and the ER-2 aircraft. My own personal involvement was that I produced a series of papers on water vapor as a tracer of troposphere-stratosphere exchange. I've been interested in this, and one of my responsibilities in England was the technical work of the meteorological research flight. We didn't have an aircraft there that could get to anything like the height of the U-2, but we were able to get well into the stratosphere at times so I got a certain amount of real involvement in that work so that I don't think they put me on that particular committee for fun, because I was supposed to know something about it.

Again, I had a contract with NASA to examine the results of all the high-altitude flights and produce reports--and I produced reports. I had a certain amount of work done by my colleagues at CEM simply examining enormous numbers of radiosondes. Some of them went back to the IGY program, where ascents were made in unusual places. A lot of it went back to the GARP program. (I get mixed up with--there was one thing we should call the "first" GARP, something using GARP not as a series of initials but as a word.) They did special flights on a number of days, a lot of radiosondes. And I was particularly concerned with those over the Pacific Ocean and so on.
One of the things that strikes me looking at that is just how much information is thrown away--your radiosonde goes into a computer and it comes out as a thickness of 14-15 layers. It has got all sorts of small wiggles, if you like, in it, which mean something. OK, you can't ask a computer to look for something that you can't even define yourself, that you've never even seen yourself. I had a young woman who did it, and having told her this sort of thing, she went through, I've got the number somewhere but it's over 10,000 individual radiosonde ascents in some detail. Plotting, on an old-fashioned piece of graph paper, a lot of them when she really--so she got her name on two papers in the **Proceedings of the Royal Meteorological Society**, which I doubt anyone ever read, but they are there, if you're interested in the results of about 16,000 radiosonde ascents, looked at from the point of view of water vapor and possibilities of transport and then just how dry can it get. It can get very dry. And how that is formed is an as-yet unsolved problem: the extreme dryness part of the--...a fair percentage of the air in the lower tropical stratosphere is so dry that we still really haven't got a good explanation on how it's formed. The only possible one is freeze-drying. But freeze-drying, you have to get rid of the ice, and that gets a bit difficult. Imagine a situation which will set up the freeze-drying conditions of temperature and pressure, and still allow any ice which is formed to drop out. The direction of the air currents are all wrong.

Anyway, that program is still in existence, and being followed up by many of my successors. They know they haven't got the final answers and they're interested in the final answer, so--but again, it's very expensive to fly a U-2 or an ER-2 out into the tropical Pacific in the Indonesia area. You can't fly from Indonesia, you can fly from Australia, which the last expedition did.

Personally, I think I have now managed to retire from the Executive Committee of that program. You have to be very persistent to retire from a NASA scientific committee. Tell them, they take no notice. Tell them again, they take no notice. Tell them a third time, they send you an invitation to the next meeting. Anyway, I think I've somehow got off their mailing list. I am therefore finally retired from scientific work. I don't really think I have the ability to start it again.

Droessler: .. They probably didn't want you to retire until you had completed this project and had made some determinations in understanding of the water as a tracer in the atmosphere.

Robinson: .. I had better men than me, people like Ed Danielson had tried and also failed. Ed kept saying that he had got the answer now, but the next expedition showed him that he hadn't got the answer.

So there is one other aspect of what I've done that I've really left out because it doesn't fit in with the exchange between countries and indeed any of their work and that is the matter which has come to be known as "predictability." The area with which my name has been coupled, which has really nothing to do with the other scientific areas I've talked about, is the matter of predictability, and which the coupling of my name with it arises from a presidential address I gave to the Royal Meteorological Society--

END OF TAPE 2, SIDE 2

Interview with George Robinson

TAPE 3, SIDE 1

Droessler: .. This is Earl Droessler, and we're now beginning Tape 3 of the interview with Dr. George David Robinson. It is Tuesday, the 28th of June, 1994, and we'll continue the interview at his home in West Hartford, CT.

Good morning, George.

Robinson: .. Good morning, Earl.

Droessler: .. Nice to see you up and around this morning. It's a little bit cloudy but a rather pleasant day.

I thought we would first have you talk to us about predictability. We had just barely gotten into that yesterday so why don't you begin with that subject?

Robinson: .. Yes, it got to the stage at one time when my name was usually associated with the subject of medium-long range predictability, and was really a by-product of my involvement in the early, early stages of the World Weather Watch and the GARP setup. I said in previous talk just how I got into this and was in the beginnings of the World Weather Watch. I was, at the time when my involvement became obvious, I was President of the Royal Meteorological Society, and the President gives a Presidential address, which is published in the Quarterly Journal and, on the President's own initiative can be of any kind and even if it concentrates on scientific matters, it is published as an un-refereed document. Not as the official attitude of the Royal Meteorological Society, but as the personal views of the President who is delivering the address.

I gave an address, and the title I gave it was an innocuous one. It said, "Some Current Projects for Global Meteorological Observations and Experiments." In it, I just detailed, from my own point of view and my own involvement how the ideas of the World Weather Watch and GARP were developed. Then I went on to give some of my own personal impressions of this and what I said was that in the course of setting up of this, I had noticed that some of my professional colleagues, particularly in America, were concerned with actual funding for the project, and there was a tendency to say that this project is going to improve the possibilities of weather forecasting. And there were documents saying that this will make it possible to forecast with reasonable accuracy, and even up to as much as two weeks or even a month ahead. And my own feeling was that there was really no sound basis for such a claim. All I said was that "...this worries me, and I don't think this is really a suitable way of ensuring the funding of this project to give the possibility of long-range weather forecasts as a probable outcome of the work on GARP and the World Weather Watch."

I went on to say why I thought this. The reason was rather a fundamental one; it was my own view of the development of the equations, which were being used in models which were being claimed as giving the possibility of these longer term forecasts and the claims were made in pretty positive terms: "Yes, this will happen, if we do. if we set up this World Weather Watch and do the GARP experiments. We can be reasonably confident that this will happen."

My own view was: "Well, the basis of this is the value of these meteorological models which are fundamentally based on certain equations, which are really the Reynolds equations for prediction of a turbulent flow, and I went onto a very naive explanation of why, in my opinion, the Reynolds equations used in this way were not prediction equations. The basis of these models was effectively an empirical equation."

I actually put some numbers into this, some figures into this. They were based on the dissipation term of the Reynolds equations. This was known, again, to be based on certain postulates that Reynolds made, that became known as the Reynolds axioms. One of these Reynolds axioms, to me, limited the use of the Reynolds equation to an unchanging type of turbulence. In fact, the use of the Reynolds axiom seemed to me to say that we're not dealing with a prediction equation. The way it was developed meant that it's only applicable to a statistically stationary field, velocity field. And so it isn't a prediction equation. As the Reynolds equation was then normally used, it had the dissipation term and the dissipation term was expressed by, shall we say the K and gradient expression for dissipation--"K" being the eddy viscosity. I put forward a way in which you could use this to estimate the length of time for which the Reynolds equations was a reasonable approximation of the development. This worked out to be again dependent on the dissipation term, matter of a few days, and was entirely dependent on the scale of diffusion to which the "K" diffusivity term applied. So that the larger the scale, the longer time this was a reasonable method of forecasting. Mine worked out, and it came out that with the known dissipation rates, one could forecast on a scale of shall we say the size of a thunderstorm, for an hour or two. One could forecast on the scale of a mid-latitude cyclone for a day or two, up to say five days. This was in reasonable agreement with the then-known performance of forecasting models, but my point was that this was a fundamental limitation, that it was caused by the fact that you were trying to forecast the development of turbulent motion which was in fact turbulent on all scales from the shortest time scale of an hour or two to the time scale of a few days, a space scale of the major cyclones. There was no reason to expect that this performance could be improved using the equations which were the basis of all the models. That is the Reynolds equation for turbulent flow.

This presidential address was given at an open meeting and there just happened to be some scientific journalists present. One of them happened to be a correspondent of the **Washington Post**, and he gave some quite reasonable report of my presidential address, which appeared in the **Washington Post** roundabout the time that the

Congressional examination of the Weather Bureau's predicted financial requirements for the next year were being considered in Congress. This got a lot of attention, much more than I expected. I thought I was just addressing a meeting of the Royal Meteorological Society. Its first result to me was a long telephone conversation with the representative of the head of the Weather Service. The representative happened to be Hallgren, who was at that time working for the head of the Weather Service, who was at that time Bob White. I had about half an hour's conversation on this matter and I explained what I had said. I said that this was a reasonable report of what I said, but this was just what I said and I had no doubt whatever that if a better meteorologist than me were consulted, they would probably come up with a different answer. But that I think satisfied Bob White, and I don't think there were any difficulties with the funding process, just that there was a temporary scare by the American Weather Bureau leadership.

It sort of stuck to my name and I still believe that there is this fundamental trouble with the equations on which the computerized model forecasting is based, that it still is there, and there's no way around it. There has of course since then been a tremendous increase in the computerized forecasting industry, a tremendous increase in the available observations for this, and it is now really the basis of worldwide meteorological forecasting. The European Centre for Medium-Range Forecasting issues regular forecasts of pressure fields, velocity fields and the like at the high levels of the atmosphere for x days ahead--"x" varies from four or five to ten to fifteen, according to the initial situation. But this indeed was the limitation which I put on it. And I'm saying, I did this at the same time other people better qualified than me were doing it. The most interesting of the papers was a paper by Ed Lorenz on the predictability of a situation with many scales of motion. This, I think, is still the crux; the handling of the problem of the scales of motion. It is really still not satisfactory. I cannot think the problem has been solved, and I still think it's insoluble. The equation is--according to the way it's treated--but it does in the end of contain a sort of infinite regression in the interaction of the scales of motion. You have to have a simplification, you have to have a truncation if you're dealing with Fourier components in the field. If you still go to the old K and gradient method, you have to make--well, it's effectively the same as the truncation, you have to have some empirical relation between the scale and the dissipation term. It's still there. What really worries me is that nobody seems to worry about the fact that the basic equation can only be solved on the assumption that the--you're dealing with an equation for a statistically stationary field. This is implied in the way that the equation is developed. This effectively means that you have no way of forecasting a time development except by making empirical simplifications.

After the presidential address, I did nothing further until I was in the States working with the Traveler's Center for the Environment and Man, as I have explained that things developed. And we got an NSF grant to look further into this predictability problem. In the course of this, I produced two papers following up my ideas. One of them I wrote what I described as a "didactic" paper, with no original science in it at all, but in which I went back and wrote down my own beliefs on the way the equations in use in global scale circulation modeling were developed effectively from the Navier-Stokes equation.

This was indeed a didactic experience. In it, I suggested in fact that students of meteorology should be taught how these equations were developed. One sees an equation, for example, in which there is a velocity component and what is this a velocity of? You never see a reasonable examination of what this velocity is. It's an average velocity, average of what? From this average velocity, you got into the way of doing it, you reduce it to a vorticity field, from a velocity field to a vorticity field. But it itself is still an average over some time or space scale. This paper, which I think was the second of the papers entitled, "Weather and Climate Forecasting as Problems in Hydrodynamics," and I submitted it to the **Bulletin of the AMS**. I chose the **Bulletin** because as far as I was concerned, there was no original science in there; the implication was how this kind of meteorology should be taught. My suggestion was that people should go back to the origin, go back to the Navier-Stokes equation, develop the Reynolds equations, see the effect of different forms of averaging on the Reynolds equations from space, space-time, time, and even to ensemble averages, and look at what the implications these different averaging methods was to the possibility of long-range forecasting because all of them in fact included a simplification, a truncation or something. The basis for this simplification always went back to some of the Reynolds axioms, which imply that it would only be true for a statistically stationary field. So to me the implication was that the only forecast you could get out of these equations and taking into account all the implications of the Reynolds method, the only forecast you could get out was a forecast of no change. Because the Reynolds method implied you were dealing with a statistically stationary field. And to me, that meant it implied that you could not produce a forecast. It wasn't a time-dependent equation you were dealing with. Whatever the velocity component "u" was, it implied that the statistics of "u" didn't change. And this to me meant that you couldn't produce a forecast other than this thing isn't going to change.

Now of course if you use the equations, you do produce a change. My argument was that, OK, this shows that the equation is not reliable because the method of development says that you can't change the statistics and you use the equation on a given initial field, and it does produce a change. In fact, the change it does produce bears a considerable relation to what really happens. But there's no question of going forward with this process forever. The scale dependency in the equations implies a time limit of predictability.

Droessler: .. Did the **Bulletin** publish this journal article?

Robinson: .. The **Bulletin** did not; the editorial committee decided that this was a bit too advanced for the **Bulletin**, and it appeared in the **Monthly Weather Review**. Since then, the **Bulletin** has changed. I can recall one article in the **Bulletin** by [George] Platzman which was far more difficult mathematically. And again, on the subject, a real scholarly approach by Platzman, which was published in the **Bulletin**.

Droessler: .. Has that been recently?

Robinson: .. That was probably three or four years ago.

Droessler: .. The **Bulletin** editor has changed from Ken Spengler to Dick Hallgren, and that might have accounted for that.

Robinson: .. I think there has been a change of policy about the **Bulletin**...

Droessler: .. Did you get much response from your fellow colleagues?

Robinson: .. No, this is what worries me: I got no response at all, no response at all. No one has questioned it. No one has used it. And it was refereed. That's what worries me about this. I still believe it's correct. The only response--and I won't give you the name of the man who said it--but at a meeting on another subject, a quite eminent American meteorologist, one of the few at the time who was indeed a member of the National Academy, and there weren't many meteorological academicians at that time--and at a meeting, which included the then-editor of the **Monthly Weather Review**, who had published this, and myself and this gentleman were talking about it. He said--I can't precisely quote, but he said and I'm sure if you got him in this room, he would agree that he said it--that he didn't care whether it was right or wrong, it shouldn't have been published. And the only reason I can think of what he meant by that was that if this was right, and it would seriously interfere with the funding process of long-range numerical weather forecasting. I mean I myself recognized this possibility, and did say I think it's fairly evident that this wasn't my intention; what we were doing was perhaps not the only thing we could do, but we were doing the best thing we could do and we better not stop doing it because the only current way of improving was to go on doing it.

But realize that there was this fundamental difficulty. I didn't say, "Stop, stop supporting long-range forecasting." I said, "As I see it, you're off-base with this fundamental difficulty, which you can't in any way remove." As I say, it was refereed, I have seen no refutation of it in any publication; I have not received any personal letters from my colleagues refuting it. I have not noticed that there it has had any influence whatsoever on meteorological practice.

Droessler: .. It's quite clear, George, that you feel very, very deeply about this matter and you've given it an immense amount of thought and concern. And sometimes, you know, it will take awhile: history will show that someone will go back and read that paper and bring it forward and it can become an important paper in the future for the discussion of meeting the long-range forecasting. Whereas right now, it's pretty obvious that your colleagues are not going to give it very much discussion or concern. I think you just have to wait for time to catch up with it. This has happened in the past, as you know.

Robinson: .. Yes, but I have now retired from any position in which I have any influence whatever, so I can just continue to watch the evolution of the results of long-range forecasting, which is--it's certainly an understatement to say that this is now in good hands. It's in the best hands we have, and they're producing some very impressive results. But I think there is a fundamental limitation on what they can do. I suspect that a lot of them would agree with this. None of them would agree that this is a good excuse for stopping what we're doing and I certainly wouldn't say it's a good excuse for stopping what we're doing. It's just that I'm not sure that the kids are being taught what is the real basis of what we're doing.

Droessler: .. One suggestion I would make is that you keep reading the literature on long-range weather forecasts and as you said, you are going to keep monitoring how it's progressing, and at some time, you simply write a letter or a comment about a paper that's current and bring up your papers again, bring them up on the table for discussion and who knows what would happen at that time? It's one course that's open to you, you know to write a letter or write a comment on a current paper, and to reference your very elegant papers there. Bring them back to the fore, and hopefully some of your colleagues will give it more thought, and more consideration.

Robinson: .. I consider myself as retired from the profession now and I'm in really no state to get back into the profession as a useful commentator. It's just that I am personally not quite satisfied with the current state of the reasons--the real reason for the mathematical basis of the current models and--well, it's not so much the mathematical basis, it's the physical basis and my main trouble is that being no mathematician, I can't really comment on the state that the mathematics have gone to.

Droessler: .. You certainly have given this an immense amount of study and I think it's courageous on your part to move forward and publish these papers. At the moment, they have not resulted in a response or rebuttal, and I think that's too bad. But there just isn't very much that anyone can do about that at the present time except to leave the literature as it is and hope that sometime in the future, the papers will be picked up and become a very useful part of the scientific discussion at that time. You certainly gave it a good shot, George.

Robinson: .. I don't think they will be a useful part of any discussion. I think that the outcome which these papers imply--that there is a time limit of forward integration of equations which reasonably represent the evolution of atmospheric velocity, pressure, what-have-you fields and there is a limitation. I think that what I and others have done suggests that we're very close to that already and apart from that, my own opinion of the present state of long-range forecasting compared with the state of long-range forecasting thirty years ago, shall we say, is to a large extent due to the enormous increase in observation of the atmosphere. Tremendous increase in the detail and indeed areal coverage and precision of the initial values, which are a matter of observation of the atmosphere. I think--I'm fairly convinced that long-range forecasts have improved considerably in the last thirty years. I personally believe this is due more to increased observations, better observations, more observations rather than to the development of the numerical models and the big computers which can handle the big numerical models. But go on doing what we're doing and see what happens. Look carefully at what happens. Don't say we're getting better because we think we're getting better. Do some pretty careful check experiments, validations and the like, and don't just look at the validations of pressure fields at 500 millibars and so on when the equations themselves are in the validation process. Again, the present input is initialized to the extent you're putting into the computer initial conditions which fit, which are solutions of the equations you're using them on and you're not putting in raw observations of the atmosphere. It's obvious that the actual errors of observation are such that if you put in the raw observations of the atmosphere, you're giving your model a lot of trouble to sort itself out to begin with. So you present information which you know equations that you're using in the model can handle without running wild. This is one of the validation troubles.

END OF TAPE 3, SIDE 1

Interview with George Robinson

TAPE 3, SIDE 2

Droessler: .. George, we've just celebrated the 50th anniversary of D-Day with a great deal of worldwide interest and immense coverage by the T.V. You mentioned in our earlier conversation that the meteorologists, many of them who were active during the D-Day forecast preparations and activities, were gathered for the 40th anniversary celebration of this important event some ten years ago. Would you like to comment on what happened at that meeting, and who came together and how it all worked out?

Robinson: .. Yes, this meeting was conceived and convened by the Monterey Peninsula, Northern California, Sacramento, and San Jose State University chapters of the American Meteorological Society. It wasn't convened by the AMS as a body. It was the idea of these California chapters, and they chose the 40th anniversary and made no secret of it because they felt that this would have been quite appropriate to the 50th anniversary, but they were looking at the age of the people who were concerned and they concluded (and unfortunately it has proven that their conclusion was very reasonable) that many of the people they would like to talk to just wouldn't survive long enough to do this on the 50th. They made no secret of this at all. So they began by sending out letters to all the people they knew who were alive and had anything to do with the forecasting for D-Day. They got a remarkable response. They arranged this symposium--it was held at Fort Ord--and it really covered the 40th anniversary, it was around the 5-6 of June, 1984.

It was not very formally structured. It got all the people that it could, given the funding and the possibility of people traveling to California and gave just a fairly unstructured chance for these people to talk to each other. I can't say much about what it did except bring all the survivors who would go together to talk. I was one of the invited ones, and someone paid my fare to Monterey. I'm not quite sure who. But the record of this is a very interesting document and it's entitled **Some Meteorological Aspects of the D-Day Invasion of Europe** [Proceedings of a Symposium, 19 May 1984, Fort Ord California; American Meteorological Society, Boston]. I think I better read something on the title page of this:

"Printing arrangements for this volume by the American Meteorological Society were carried out as a service to the California chapters who sponsored the meeting...This is not a publication of the Society in the sense of the papers having been reviewed or of the Society agreeing or disagreeing with any of the material included."

It isn't a formal publication of the Society. I gather that it can be obtained, and I don't know at what expense, from the Society, but not as one of their publications. The copy that I have in my hand was distributed by the organizers of the meeting, and I know that all participants at the meeting received a copy of this. I think it still can be obtained, and the best thing I can say to anyone who is really interested in the forecasting arrangements for D-Day should have two documents. One of them is J. M. Stagg's book, **Forecast for Overlord**. The other is this document, because one of the things the organizers did was to ask people who couldn't come to the meeting but who had been involved, to write letters and quite a number of very interesting people responded. And the letters they sent in are part of this and it also includes the text of Stagg's report to the Supreme Commander, which is dated the 22nd of June, 1944. I had something to do with the production of this report; it is Stagg's report, but I fed him with a lot of the details of what had been said during the conferences before the meeting, which I had taken at the time. These are all in the report, and to some extent you can follow with this report what the different forecasting units involved in the D-Day weather discussions were in fact forecasting on the six days before D-Day; you can follow the details of some of the discussions in Stagg's report.

If you really are interested, you ought to have that, you ought to have Stagg's book, and you ought to have the letters from other people. I would suggest in particular you look at a letter from Sutcliffe [p. 98]. I think there was a letter from Bounds [p. 102], who was the short-range forecaster at Widewing at the time, who didn't personally take part in the discussions, but who was there and in the Widewing unit and doing forecasts at the time and who knew what was going on there. And there's an interesting letter from Professor Flohn [p. 95], who at the time was at the headquarters of the German Air Force. That's a particularly interesting letter from the point-of-view of what the Germans were thinking of in the possibility of forecasting. And Sutcliffe is quite good; he says some nice things about me, but he also gives a little pen sketch of all the forecasters involved and he is very complimentary to Yates. He obviously thought an awful lot about Yates and his contribution to this and regrettably, as I'm talking to you now, I've just seen in the **Bulletin**, the necrology of Don Yates.

I've got a very high opinion of Yates, and I'm glad to see that Sutcliffe is saying the same thing here.

Droessler: .. As I remember, one of the principal forecasters for Don Yates was Ben Holzman. I'm sure you came into much contact with him during that period.

Robinson: .. Of the forecasters involved, I worked with Douglas, I knew Douglas very well and he was the best, good old-fashioned forecaster around, and he knew Western European weather. He was, as someone says, he didn't believe that it was possible to forecast the weather five days ahead. He'd been told to do it and everyone was doing it, so he made his best effort to contribute three, four and five-day forecasting, but he didn't believe in it. Nevertheless, he did know the sort of thing that could happen, and if the other people really did believe in three and four, and five-day forecasting--and there weren't many of them left by the time this D-Day exercise was over--if they had produced something which Douglas thought was impossible, from his enormous memory and knowledge, he would have said so.

The other forecaster who really impressed me was Ben Holzman of Widewing. He was really a similar forecaster to Douglas. He was--someone described Douglas as a "seat-of-the-pants" forecaster; well, Holzman was similar. Very sound, very careful. As I say, just listening to the things. He and Douglas were the ones who impressed me with my very limited experience of forecasting. I was doing forecasts in the way I personally thought they ought to be done, and I suggest that anyone interested should read Sutcliffe's letter to the 1984 conference, and he comments on all the forecasters concerned. I don't have time to do the quoting, but he is also very complimentary to Ben Holzman. Well, Holzman was the only man who got one of the major developments, because Holzman was the only one who really did forecast it, according to my notes. As I think I said earlier, this was a time when the Widewing forecasters changed their minds and didn't continue with the developments which Holzman's forecasts would have carried through and which actually happened, but the one correct forecast of what was going to happen to one of the major depressions concerned was made by Ben Holzman, two or three days ahead. It did happen, and the actual D-Day forecast was based on the fact that something else was going to happen to that depression. The depression came down in the North Sea and it was supposed to go straight ahead to the Norwegian coast in the final forecast. Ben had said it would go into the North Sea and it did go into the North Sea. It was very important from the point of view of forecasting the wind on the coast on D-Day. This was again one thing I put into my presidential address about weather forecasting: what is a successful five-day forecast. I put in the actual 1st of June forecast for 1944, the actual five days ahead, and a map which would have been the weather map if the center of this North Sea depression was fifty miles closer to the British coast. And that would have produced pretty well catastrophic winds, so that my point was that if anyone had from the 1st of June map had forecast the actual 6th of June map, they would have said they would have made a very, very successful forecast. If it had been fifty miles out, the effect on the user would have been completely different. And anyone forecasting on the 1st of June who'd got within fifty miles would have said, "Am I good!" But from the point of view of General Eisenhower, they would have been very wrong. All I was saying was, "Well, be careful what you mean when you say you're going to forecast the weather for five days ahead."

Droessler: .. Thankyou very much, George, for bringing this important document to the fore and as part of your comments.

George, if we're through with the 40th anniversary comments, let's move on to another subject, and let us discover who is George David Robinson. Let me first ask, where were you born, and what was your early education and upbringing?

Robinson: .. I was born in Leeds, England, in 1913, and incidentally my mother called me "David," not "George," I think simply because my father's name was George and my mother was in the habit of giving instructions: "George, do this," or "David, do this." This distinguished between my father and myself, the use of the term, David.

I was born in 1913. My first few years were quite seriously affected by World War I. My father was a conscript and infantryman in France from, I think, late 1915, until after the 1918, November armistice. And my mother simply had to work first of all because women were supposed to work, and secondly and more importantly, because she needed the money. So I spent between two and three years of my first five not in the company of either or both of my parents, but in the families of relatives of my father. When I got back to Leeds and my father was back from France and my mother was running the home again, I was pretty well five years old and ready to go to school. Someone had taught me to read. I don't know who. So I didn't have the normal home life of a young Englishman in the years during the First World War.

My schooling was in the Leeds City Education Department schools. I have to be careful because they were known in England as "council schools." Public schools in England had an entirely different meaning; they were pay-as-you-go boarding schools, mostly. So as far as Americans were concerned, I was educated in public schools in Leeds, but the local term was "council schools." By a series of scholarships, I went from the elementary school to this so-called secondary school which was Cockburn High School, which was a rather unusual school in the fact that it was one of the--certainly in Leeds, it was the only, co-educational secondary council school, so in a sense it was unusual. There were girls around in the school I was educated in; this was unusual for those days, I don't think it made really any difference, but to that extent, it was more like an American public secondary school than most British and certainly most European schools at that time. I went through city council scholarships and got a university scholarship and again, from the Leeds City Council, to Leeds University. I might have been able to sit for a Cambridge or Oxford scholarship, but there were two reasons which made it, well, impossible to do this: the first was simply that however big the scholarship, my parents simply couldn't have afforded to send me to either of these universities. The other reason was that to get matriculation in those days to either of those universities, you had to pass a Latin examination. My teachers advised me against taking the Latin examination. I think if there had been any possibility of taking the scholarship, they would have given me coaching in Latin and that would be that. That was my excuse at the time for not taking the open scholarship examination for Oxford or Cambridge. I didn't have Latin. I did have two years of Latin, I could have just about got the examination. I had an amazing matriculation result: I got distinctions in five of the six subjects which I took. A distinction was not unusual, but a lot of people who took the examination didn't get one. The one in which I got only a credit was curiously enough, English language and literature.

So I was a bit of a phenomenon at school and I went to Leeds University and everyone expected me to read chemistry. Just at the last moment, I thought, "It would be interesting to read physics, wouldn't it, rather than chemistry?" So I took an honors degree in physics and as a result of that, I was awarded a government grant--we called them "DSIR" grants--to do research in experimental physics under

Professor Whiddington. This I did in odd circumstances because the job he wanted me to do was to look into a curious type of collision in slow electrons, and he wanted me to do this in something with a two-electron outer shell, as we used to put it in those days before quantum mechanics was firmly established as the only way you could talk about these collisions. The only things I could find that satisfied these conditions were zinc vapor and cadmium vapor. All his experiments had been done with helium and hydrogen, simple gases. I had to adapt this equipment to form and localize and work in an atmosphere of metal vapor.

This really was extremely difficult experimentally, certainly with the equipment I had. It was being done--I didn't know at the time--it was being done simultaneously at Cambridge, using metal vapor and electrons. They had far more sophisticated equipment, but as far as I could tell, they had just as much trouble as I did. It was simply running the apparatus and having to dismantle it and clean it down every second time you ran it and if you got an accidental short inside the collision chamber, then this was three weeks work to clean the thing and clear it up again and get back to the vacuum. It was theoretically extremely interesting from the quantum mechanical point of view because, in the way we looked at it, it was looking for a situation in which one electron colliding with the atom, displaced two electrons in the atom and this was a great change. The professors and these other people had found this happening in helium. So I had to do it in zinc vapor.

I got a lot of collisions in zinc and cadmium vapor, but they were the ordinary good old-fashioned one-on-one collisions, but didn't find what we were looking for, but I suppose they considered I had done a good enough job as an experimental physicist, and deserved doctor of philosophy. Anyway, I was awarded it, but effectively I didn't find what I was looking for. But I got a lot of instruction in patience.

Droessler: .. Would there be a paper published on your work?

Robinson: .. No, there was no paper apart from my thesis because there was no really interesting result from this. Well, the only paper was published in the Leeds Literary and Philosophical Society, and it concerned the experimental arrangements about the actual little vacuum vapor furnace that I made and used. It was interesting from the point of view of emission through an aperture.

Droessler: .. You accomplished your purpose and you became more educated and better trained in research and activities and were awarded your Ph.D.

Robinson: .. At an abnormally early age. Leeds University didn't actually impose an age limit. So long as you had satisfied the examiners, you could have been--well, I did get a relaxation of something because I was a month younger than the only age limitation on getting the Ph.D degree, so--I was effectively 22 years old when I got the Ph.D., because I'd gone to the University a year before the normal age for going. So here was I, a 22 year-old doctor of philosophy, and looking for a job. The professor found me a job as a research assistant to Professor Whytlaw Gray in the inorganic chemistry department. He was the man, and I think I mentioned this earlier, who claimed to have invented the word "aerosol." But I think I mentioned the work I did there because it was really what got me into something remotely connected in some way with meteorology.

Droessler: .. When did you meet your present wife? Where did you meet her and what were the circumstances?

Robinson: .. This was after the war, after I'd gone back to Kew. I went back there in 1946, it must have been in 1947, and I met her at a meeting of the local Gramophone Society, which met weekly and played records. She was a secretary to the head of a very large British concern, which was running plantations all over the world, so she was a very superior executive secretary from the point of view of the position of the man she was working for. During the war, she had joined the WRNS, Women's Royal Naval Service, and reached officer status and then given a job which didn't involve much more than patient classification of documentation and a certain amount of typing and a certain amount of running around, but she was working in the main Admiralty building in the office of the Director of Naval Intelligence. She was doing an important job there, but effectively secretarially-trained and very interested in music and very interested in living in London. We married, I guess, in the middle of 1948. We had in due course two children, a boy born in 1950 and a girl born in 1952. We lived in Kew, near Kew Observatory, and didn't do anything out of the ordinary. Anyway, we're still living together.

Droessler: .. Well, thank you very much, George, for these two days of most, most interesting and attractive conversation. I hope that you enjoyed the interview; I certainly did, and I certainly learned a lot as I do in all these interviews that I take part in, and I want to express the appreciation of the American Meteorological Society for your interview and also my own appreciation for your invitation to come here and to sit with you and to be a guest in your home and talk to you about the life and the professional activities of George David Robinson. So thank you very much, George.

Robinson: .. I've enjoyed it, Earl. It's been a great pleasure doing it. What I regret is that it has demonstrated to me just once more my mental deterioration. I used to be able to go through this sort of thing with no hesitation at all. I now--I don't so much forget what I'm going to say next as change my mind about what I'm going to say next. This stops me--it must be horrible at editing a tape in which your subject...

END OF INTERVIEW

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