

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

**TAPE RECORDED INTERVIEW PROJECT**

**Interview of Athelstan D. Spilhaus  
June 28, 1991**

**Interviewer: George P. Cressman**

Cressman: This is an interview with Athelstan Spilhaus on May [sic] 28, 1991. Dr. Spilhaus is known to members of the American Meteorological Society as a meteorologist, an oceanographer, an educator and an inventor. He has agreed to be interviewed at his house in Virginia where he is living in retirement.

Dr. Spilhaus, you're a man of very wide interests, with accomplishments in many fields. Can you tell us something of your early background? What influences on your early life led you to such an interesting career?

Spilhaus: I had very good parents. My mother was very much interested in academic life. She was the first woman graduate of the University of Capetown, which her father, Sir Thomas Moore was instrumental in establishing the educational system in South Africa, so the academic kind of background came naturally to me and I lived in that kind of world, and that was of course the most profound influence.

I went to the University of Capetown. By standards of today, it was a financially poor university, but with excellent teachers, even down there on the tip of Africa. My grandfather, a Scotsman, had gotten a Scots' idea of education in fundamentals, and not being able to afford great laboratories and things, they taught classical subjects: mathematics, applied mathematics, physics, applied physics, chemistry to biochemistry. But engineering, which I took--there was really very little engineering, which is the way to learn engineering, in my opinion--learn the basics. So I got that, that started me off. I wanted to go into aviation and after I graduated from the University of Capetown, I went to Germany to work in 1931 at Junkersflug\_\_\_\_\_werke. Junkers were the big builders of excellent planes in those days, and there I became interested in aeronautics. Hitler began to come in around Dessau, which was a working town outside of Berlin, and it was uncomfortable...I can't say there was any prescience about what Hitler would do, but it was really an uncomfortable situation and by then, I'd been interested in studies more about aeronautics...So I decided to come

to MIT and left Junkers and came to MIT and studied aeronautics.

Now I am going to try to say how I got into meteorology. In aeronautics, they always tell us how the Wright Brothers had conquered the air, which sort of irritated me because in those days, 1932, airplanes were falling down all over the place, with icing on their wings, and gusts on takeoff. Man hadn't conquered the air at all. He built a flying machine that engineers with their characteristic disregard for the medium in which they were working were really paying not too much attention to the atmosphere. And of course, this was my great mentor--our great mentor, George--Rossby's--he had the same idea, he was the one who started the aeronautical/meteorological services under Guggenheim in California--and he started the School of Meteorology at MIT. I think his \_\_\_\_\_, at least the application part of it, was for the service to aircraft.

Well, this irritation of man conquering the air--I decided that if I was going to go into the airplane business, I had better understand the medium. So after I graduated, and took one degree in aeronautics, I went upstairs in the Guggenheim Building and there was Rossby and his meteorologists, a wonderful little department with about as many professors and instructors as there were students. But both professors and instructors were excellent and students were excellent. The names of my fellow students, they're well-known in meteorology, great people. You mentioned Horace Byers, later of Chicago, Jerome Namias, R. B. Montgomery, an oceanographer, Chaim Pekeris, a very great theoretician--these were my fellow students, so the whole atmosphere was absolutely stimulating. I always think of Rossby as being a man among those who shaped my life. And the lives of many others, I'm sure, in meteorological business.

Well, that was how I got into meteorology. What was the question, what was the answer?

Cressman: Perhaps now you can tell us how you got to New York University to establish a department of meteorology and oceanography?

Spilhaus: I worked in my field until 1935, and I'd taken my aeronautical degree and had a job at the Sperry Gyroscope Company, in the summer even in the depths of the Depression of those days. And yet, I thought I ought to go back--my plan had always been to go back to South Africa and start the aviation business. So I went back, but the only job I got was assistant director of technical services in the department of defense, which was then the British Army in the Union of South Africa, which was a British colony. A great experience, but pretty boring. And so after a year of that, I wired Rossby and he invited me back to MIT and I was on the payroll at Woods Hole Oceanographic Institution, so that's the way I got back to MIT and the meteorology department, but really working on ocean problems.

Rossby was at that time studying jet streams, to which he made notable contributions later on the jet stream in the atmosphere. And he was also studying the Gulfstream, so he had me doing rotating pan experiments, which were later carried on at Woods Hole by von Arx and by Fultz in Chicago. I don't know whether those rotating dishpan experiments ever contributed much, except from the point of view of showing things, so you could visualize these jet streams. It is really very difficult to relate to the dishpan experiments to theoretical analysis of the flow of the atmosphere and the ocean, because there wasn't a suitable non-dimensional parameter like Reynolds number to fly and \_\_\_\_\_ number, hydraulics didn't fly and there wasn't really a simple non-dimensional parameter that one could use to stimulate the--

During that time, I worked--as a matter of fact, one valuable thing I did during that time was oceanographic, not meteorological. I had done a lot of work in meteorological instruments and when I got into the ocean, Rossby, \_\_\_\_\_ we needed a rapid measuring temperature device in the oceans and that led that year, 1936-37, to my \_\_\_\_\_ the bathythermograph, which was quite a landmark invention in oceanography. It brought oceanography into the synoptic or near-synoptic...and you could catch these eddies by rapid measurements of them, which you could never have done with the old system of lowering water bottles into the sea.

So that's what I did that year. At the end of that year, I was invited--this was toward the end of 1936--I was invited by Dean Seville at New York University to come down to New York and start a meteorology department. Rossby had been again the one who had suggested me to him. And I went down to New York University where they had a geology department with a few meteorology courses which we soon turned into a pure meteorology department. The geologists retired.

I was always interested in the theoretical and dynamical side of meteorology; **never** could get excited about forecasting. I recognized the importance of forecasting that could be done, but I never really thought it could be done, but I was lucky enough to get a then well-known forecaster who was not a theoretically trained man at all, Gardner Emmons. He had been at MIT with the gang, and he was a man who really didn't take to the theoretical side of meteorology at all. But he was devoted to looking out the windows and forecasting and studying maps and had a reputation as a very good forecaster. I was lucky enough to get him as my sidekick, as this little two-man department grew up.

Later on, in 1939 or so, I added Raymond Montgomery, who turned it into a department of meteorology and oceanography, because by then I was so interested in the oceans to realize that we couldn't really--I got interested in the oceans because I found out I couldn't really study the atmosphere without understanding

the oceans. It's a very logical thing: to understand aeronautics, you had to understand the atmosphere; to understand the atmosphere, you had to understand the oceans. I had Ray there to do the ocean link.

The Weather Bureau, with a new head, Reichelderfer, was very cooperative, sent students to us as they did to the other universities, the others being then only MIT and Caltech. UCLA wasn't in existence, Chicago wasn't in existence. So there were just three meteorological schools in the country. At New York University, we started around little things. Interesting in the light of the interest of today was I started the first ozone measurements in the United States. I brought a Dobson ozone spectrometer from England and with the cooperation of the Weather Bureau, got two excellent students, Carstenson and Hall, to do the observations and later the instrument was loaned to the Weather Bureau and they used it as a prototype for the beginnings of an ozone network, which is interesting today.

That's how I got to know you.

Am I digressing too much?

Cressman: Not at all. It's very interesting.

In 1940, meteorology was not a highly regarded branch of science. But in dynamic meteorology, which you taught, that was considered by many students as a course which was difficult and not likely to be helpful in their future work, which was expected to be forecasting the weather. Yet dynamics was to be the key to future of forecasting. I remember during a lecture that you gave a student asked, "What good is this?" You replied, "It isn't any good, but isn't it interesting?" Could you see at that time the future of dynamic meteorology?

Spilhaus: Yes, that was a \_\_\_\_\_ remark and really I believed it. I didn't believe in forecasting. I believed that dynamic meteorology was of far more interest than forecasting. I was intelligent to know that, as your question suggested, that the only interest in meteorology from the public or government point of view or anyone's point of view, was forecasting. So just like putting a man in a space capsule, when a monkey could have done the job just as well, you had to put a man in the space capsule to justify expenditure of public funds. You have to forecast to justify the expenditure of public funds in understanding the atmosphere, and I'm afraid I haven't changed that view today and I'm pleased to see the new theory of chaos that some of the smarter guys than I are questioning whether forecasting beyond a very limited extent is not possible. I did indeed see the validity of dynamic meteorology. After all, it's fundamental fluid dynamics applied to the atmosphere. And the same fundamental fluid dynamics applied to the ocean.

Cressman: At New York University, you made a really big contribution by training so many

meteorologists in the war program and at the same time, maintaining your perspective on science. What happened after the war program was over? Certainly, this must have cut out about 80% of the students or more that you were getting. So what did you do after that?

Spilhaus: The war program was a great contribution. It was in a way a thing that ultimately hurt the department at New York University, which had become a very good little department. What happened was when the training of these thousands of cadets was well underway, I went into uniform myself in early 1943. Actually, late 1942, I went into uniform first in the Signal Corps and then the Army Air Corps \_\_\_\_\_ . And the training just carried on at NYU. I didn't return to the University until 1946 and as you probably surmise, the number of students went way down. However, the people on the staff at NYU were very good. We still had Montgomery, I brought in Bernhard Haurwitz, we had Jerome Spar, Pierson, who did a lot of good work on waves in the ocean. We had a good little cadre for a little school like that. When I left, I was given the responsibility--I left being the head of the meteorological department, but Bernhard Haurwitz should take that on. Because I was going to be director of research for New York University. That was the first time after the war when research grants from government were up for grabs. NYU and all the other universities had their avaricious eyes on Washington, so I did that very well because I knew the military and the government very well by that time from my time in uniform. So I had very little to do with the meteorology department actually until the time in 1949--although I was nominally still a professor in meteorology--the department ran without me, until 1949, when I took on the deanship at the University of Minnesota...

I don't know why I'm being interviewed as a meteorologist because I don't think my contribution to meteorology was very great. But the one thing that might interest you—you may not know about—was the Zworykin-von Neumann-Spilhaus Committee—did you ever hear about it? When I got back, at the end of 1945, I found a letter from Vannevar Bush waiting for me saying, "Would you please get together with Zworykin of RCA? Zworykin has a proposal which most meteorologists won't even give credence to: he wants to control weather."

Zworykin was a brilliant man with wild ideas and Bush wrote, "You're probably the only meteorologist who would consider thinking about this." Well, true. And Zworykin and RCA being near Princeton--Trenton, and von Neumann being at the Institute of Advanced Sciences [Study] in Princeton, they had gotten together and I joined them as a meteorologist. The three of us sat down and von Neumann--he was working on computers then--made the very wise comment to Zworykin and to me: "I had said yes. I think control of weather is feasible. After all, we control it inadvertently in our cities: Paris has its private thunderstorm over the \_\_\_\_\_; in Africa, we burn the lands during the thunderstorm season to blacken the earth and trap the \_\_\_\_\_ of

convection; in France, they build walls to the south so they catch sunshine. All this is weather control. But," von Neumann said, "before we control the weather, we must forecast it. Otherwise, how do we know whether we've controlled it or not?" It seems a very obvious, but yet a very wise statement. So we realized, both of us, of course, immediately realized the validity of this and so the project, instead of being directed toward the control of weather was directed toward the use of computers for prediction of weather.

I believe, and I went with Zworykin and von Neumann to Washington to Reichelderfer to get the grants; the initial grants were von Neumann's work and they were [the] original weather computing prediction. I think von Neumann, you can correct me, was the first one to think of computers for weather prediction. Other than Richardson, L. F. Richardson, who didn't have computers. I have been familiar with Richardson's work and of course von Neumann's computers took away the thing that Richardson ended his book with: that you could forecast the weather by numerical processes but it would take 5,000 people to make the computation. That was an interesting sideline. I don't take any credit for it, but I was there at the creation.

And I believe the Weather Bureau [Service] now still has a facility in Princeton, don't they? Smagorinsky?

Cressman: I don't know about a facility in Princeton. Smagorinsky, I think, up until his retirement was mostly in Princeton in the last decade or so--Princeton University.

Spilhaus: The principle will be carried on with Smagorinsky then...a good one, too.

Cressman: In the early 1950s, I was surprised and delighted to meet you in Nevada at the test grounds of the Atomic Energy Commission. Can you tell us anything at all about your activities there?

Spilhaus: Yes. That really had little to do with meteorology. I was asked actually by the Secretary of the Navy, but that was a cover because by then I was at Minnesota. I was well-known in oceanography, and because of the secrecy attached to the first military atomic tests, they had the Secretary of the Navy invite me to Washington. Everybody would think, oh, oceanography. Actually, my job was to work in the Joint Special Weapons Project, they called it that time which was atomic weapons. They gave me the job of being the civilian director of the first atom bomb conducted by the military. Because according to Truman's dictum, he had that awesome responsibility of deciding on dropping the bomb during World War II, he had put the atomic energy business squarely under the control of civilians, quite rightly. And for a long time, the pendulum swung too far in that direction and the military had really not had any opportunity to test weapons effects. So I directed these first weapons effects tests, which were called "Buster Jangle" in those days, in 1951 in Nevada. In fact, the fact that it was held in Nevada was

very interesting. Truman also because he'd made that decision on Hiroshima and Nagasaki, said there will never be any more tests of atomic weapons in the United States. The test that I was to direct was underground and above ground of the ground test two bombs. But the underground was the most important and it was originally planned to be carried out in Amchitka, the Aleutian Islands. Admiral Settle, who was then a great balloon man--made the first \_\_\_\_\_ of the upper atmosphere in a balloon--Tex Settle was the Naval commander of the task force because they set it up like they set up \_\_\_\_\_ and those others one.

Tex and I flew up to Amchitka, a dreadful island from the point of view of weather. We landed with the wind blowing 100 mph in both directions, made a controlled crash \_\_\_\_\_. We had some geologists along, Dave Griggs and a few wise people. We examined the site and for the underground test the island was so inhomogeneous we worried about the chances of causing a landslide on the steep sides of the island, which would cause a tsunami, and for many reasons and principally from the point of view of the validity of the tests underground and the inhomogeneity of the geology, I said, "Perhaps we can't conduct the tests here." And I really admired this admiral, because he went along with me completely even to wiping out his task force, which is a great thing for a man to do when he's at the end of his career as a Naval admiral, and backed me to the hilt. We went down and faced the Atomic Energy Commission to convince them we had to bring these tests back to Nevada. We would kill scientists taking--there were thousands of scientists involved--taking scientists up to Amchitka, undoubtedly we would have lost troops. Part of the exercise was having for the first time troops as near as was safe to the site of the explosion. One of the objectives was to have troops there. And the whole thing was logistically impossible.

We had quite a tangle with the Atomic Energy Commission and finally I do recall Mr. Straus, I don't think he was an admiral, was the civilian chairman of the Atomic Energy Commission. It was an awesome body, very good men, and I can remember the man who turned the tide in the commission in our favor. He was a great gentleman by the name of Samuel Pike. He'd been a former SEC commissioner, Senator in \_\_\_\_\_ and now a Senator in Washington, and he was on the Atomic Energy Commission. Finally, he said one afternoon after we'd made a presentation, he said to somebody, "Gentlemen, I'm convinced. I'm willing to eat crow, feather end first." So we got the go-ahead to, and Mr. Straus and I went to see Truman and I have a great admiration for Truman, tremendous admiration. The two of us went in and said we wanted to bring the tests back to the United States in spite of his dictum. And he said, "The civilian leader of the AEC and the test director says this to me, it will be done." He didn't wait to kind of--

**END OF TAPE 1, SIDE 1**





## Interview with Athelstan Spilhaus

### TAPE 2, SIDE 1

Cressman: It would be interesting to know what you did following that.

Spilhaus: After the atomic bomb tests, no, before that, I had gone to Korea. The Korean War was on and I had gone to Korea for Secretary \_\_\_\_\_. I was very active on committees for the Defense Department, I was on the Scientific Advisory Board of the Air Force and I was on the Scientific Advisory Board of the Secretary of Defense. They sent us to Korea. The \_\_\_\_\_ of the Army wanted to find out what could be done to improve the instrumentation for the ordinary army on the battlefield and particularly on the front zone of a battlefield. So we went over to Korea. Of course, I had to give up Minnesota, but peripheral activities were interested in a Pacific Science Congress, which I took a great interest in, trying to--I went to the Phillipines several times, we had meetings in New Zealand, Thailand and so forth, and that was up to--oh, I forgot, I was named Ambassador to UNESCO by Eisenhower in 1954. And I served in that capacity until 1958. When I resigned that job, because the International Geophysical Year was coming on and I wanted to pay attention to that, I went to Thailand and the South Pole about 1958. I was active in the IGY in a very general way. I was on the national committee, which was not just meteorology and oceanography but the most general way.

Then, I was really getting into oceanography. President Kennedy appointed me Commissioner of the Seattle World's Fair and I put on a show out there, a U.S. science exhibit, which is now the Pacific Science Center up there. Then I, in 1963, I got pre-occupied about how to get the government interested in the sea, the resources of the sea. I was disturbed about that, that our Merchant Marine was going downhill, our fisheries were going downhill, and that was when I devised "Sea Grant," the Sea Grant colleges. I proposed that in 1963 and I lectured all over the country, drumming up interest in Sea Grant colleges and it worked. In 1966, Johnson signed the Sea Grant College \_\_\_\_\_. So that was what I was really working on in the intervening time. Nothing else really of meteorological consequence, except the ocean in terms of what meteorologists thought about the ocean.

Cressman: I think the interest of meteorologists in the ocean is increasing a good bit, and I think that your early interest in this may prove justified in the long run. Particularly in the questions of longer-range forecasting and climate change.

You discussed earlier the department at New York University which I had the good luck to attend when you were there. But then, in the late 1970s, we were able to co-locate a forecast center of the National Weather Service with the Synoptic Laboratory of the department at New York University. So we had a

good arrangement going there.

Spilhaus: In the seventies?

Cressman: Yes, early seventies.

Spilhaus: I didn't know that. I'd lost touch.

Cressman: The idea that rubbing shoulders with the university staff would improve the scientific content of our own work.

Spilhaus: Absolutely.

Cressman: On the other hand, we felt perhaps it would interest the university staff members in a lot of technical questions.

Spilhaus: I think that's excellent. Did you do that at other universities? I never knew of this. You see, I'd lost touch.

Cressman: I don't think we got very far with that program. I liked the idea myself; we had that going at Miami for some time. But then, a really sad thing with the New York office when NYU closed its uptown campus. Now, I understand this was for economic reasons which also involved deterioration of the neighborhood and so forth. It was a sad event.

Spilhaus: Very sad for me, having started [there].

Cressman: Still, there are many good universities now teaching and doing research in atmospheric sciences. In fact, we can't sit here and enumerate them; there are too many. Do you think that the result of all this academic growth in atmospheric sciences can be \_\_\_\_\_ in the long run?

Spilhaus: I think the success of atmospheric sciences must be maintained--my goodness, if you don't do research on the atmosphere...I mean, there are three things--the land, the ocean and the atmosphere--all research has to be on one of the three. I don't see how you can neglect the atmosphere. I'm not sure that the interests in atmospheric research can be maintained as it has in the past purely on the basis of forecasting. And in a way, while the problems, such as the \_\_\_\_\_ ozone hole, pollution of the atmosphere--which has focused attention perhaps on a thing which I perhaps recognized way back in the ozone days in which Rossby recognized when he went to Uppsala because he was interested in chemical oceanography, I think these things will sustain an interest in atmospheric physics, which is not necessarily directed towards forecasting in the ordinary sense but long-range problems. And the interest will be, instead the modeling for forecasting problems, the modeling, which I believe is going on today, to study

the ozone hole and these kinds of circulation problems. Oh, yes, I think it must be maintained, the atmospheric research.

Cressman: Well, climate studies used to be considered, when it was called climatology, used to be considered by many to be a boring subject. But now, they're here toying with dynamic meteorology and with physical meteorology and with oceanography in a way that few people would ever have thought. Climate problems are now national issues. Do you have any further views on the current excitement about this matter?

Spilhaus: I think it is just sort of late. I know that I was interested in climatology, very much interested in climatology, because climatology was the thing I went into-- [what] the ocean's about. I thought of oceans as heat banks, that absorbs the heat and gives it off--it's the driving heat bank for the atmosphere, which is a trite phrase used by everybody. Therefore, the climatology over the ocean is terribly important and yet in the olden days, when meteorologists drew climatological maps of the world, you would notice that the lines were noticeably absent over the oceans. Partly because of observational scarcity but probably because of lack of interest. Of course, that's the very place we need the climatological data and it's people like Jerome Namias who comes to mind--of course, there must be others, I don't know, who've shown the importance of this. Munk, as an oceanographer, sees this story and wants to use what we call--the reverse of what the "B.T." did--the bending of sound waves to define the profile of heat in the seas, instead of, as I did in the anti-submarine warfare, find the distribution of temperatures in certain layers to find the bending of sound waves. A very simplified thing of what Munk was doing. I think that is extremely interesting, and I remember when I was with NOAA in 1974, saying to Bob White (he and I had this thing of getting the oceanographers and meteorologists together in NOAA): "It hasn't worked; they don't talk to each other."

I said that really the **essence** of climatology--those that have knowledge \_\_\_\_\_ should be pitched in the ocean. "If you're going to do anything about this long-range forecasting, it's the heat back in the ocean that we've got to observe and then understand and then predict from it." Oh, I think you're absolutely right--and I'm delighted in this interest in the climatology of ocean and atmosphere.

Cressman: Well, I have a question on a different matter, almost a final question: about the American Meteorological Society. The growth of the membership, the publications, and activities of the Society since the 1940's and 1950's, is truly astonishing. The national meetings are, in the opinion of some people, too big to observe and too expensive to attend. What should be the future of the American Meteorological Society? Do you have any suggestions?

Spilhaus: I don't think the last comment that the meetings are too big and too expensive is

exclusively the American Meteorological Society. I think that's true for all societies. The one academy that I have an honor to belong to that doesn't fall into that trap is the American Philosophical Society in Philadelphia. It restricts membership very severely, much more severely than the National Academy, and a very, very enjoyable society that doesn't really want to be in the forefront of absolutely current things but is willing to be a truly scientific and of course broad society.

However, that's not the criticism--I don't like to criticize the American Meteorological Society because I think that all these societies do good for their membership. But if I were to criticize the American Meteorological Society, I would say that is, it tried to serve two masters--it tries to serve atmospheric science and the profession of forecasting. Now the profession of forecasting dominates it. And I think that domination--you can tell the difference by looking at the quarterly **Journal of the Royal Meteorological Society** as contrasted to the American Meteorological Society publication--that the professionalism in the AMS is very much stressed compared to the science. I think that should be perhaps changed or, I don't know, there are many good things to say for a professional society. But the question is whether the AMS hasn't gone overboard on that side. That's the only question in my mind.

Cressman: I think that some of the pressure on professionalism, perhaps a good bit of that pressure, may come from the sector of broadcast meteorology. Because there is always the question of who is a meteorologist and who isn't a meteorologist, and who's sailing under false colors and who is pulling the public's leg, and so forth.

Spilhaus: I understand that and of course, I even go so far as to say that in short-range forecasting and by and large, where the weather forecasts on television are the absolutely have the highest \_\_\_\_\_ rating of anything, I'm quite sure--people looking at for picnics and their ordinary affairs. I believe, and I served on a little committee of \_\_\_\_\_ one time (you may remember) where we discussed the privatization of forecasting. It didn't take place, and the government has to be in forecasting, but we go back to where I started: the chief use of meteorology directly is in air navigation...the government really has an obligation to the safety of life and limb. And international treaty obligations in air navigation. Meteorology as far as we talk about these meteorology for contractors, a group of contractors--there's no question \_\_\_\_\_; but whether these people wouldn't do better then diverting to climatology, to planning on their activities on reliable climatological probabilities rather than forecasts, is still a question in my mind.

So I think that the professionalism, and I think (you know this better than I) that professionalism within the great government groups of meteorologists, is very high, a very cohesive union, you might say, of forecasters. Atmospheric scientists don't have such union ; they don't need it. Perhaps they do if they need to get

more support.

And the thing is, it's very much like the practicing medicine people and medical science \_\_\_\_\_ somewhat. In the same way, in meteorology you don't find the strong big cadre of professional forecasters really supporting esoteric atmospheric research.

Cressman: We've been working very hard here today. I have only one final question. You are well known now for a hobby that you've had in retirement: a hobby of surprising complexity and extent. Your construction of a doll culture brought you some local fame.

Spilhaus: It's not dolls, it's mechanical toys.

Cressman: I saw a big expose in the local newspaper about a year ago. It was brought home by Marvin Hunter, who visited you.

Spilhaus: I have a museum which has become quite well-known, but they are mechanical toys. I have some dolls, I have the Edison doll. Very few people know that Edison made a doll with a phonograph in it, a little phonograph, and it's the mechanical ingenuity of the toys that intrigues me as well as the art. \_\_\_\_\_ the museum, it's been a continuing hobby of mine and a very interesting one. I've written books and things about mechanical toys. That's just a hobby.

Cressman: I've worked you very hard this afternoon--

Spilhaus: Not at all. I thank you, George.

Cressman: I thank you very much for your contribution here and we're going to keep your tape on file.

Spilhaus: I hope it's of some interest. It's been a pleasure to talk to you. I have to say this to the tape: you were one of the best students we had in the cadet training and we wanted to keep you on as an instructor, but then I thought unselfishly that really we had given you all we had. And you needed the stimulus of Rossby and Rossby felt the same thing and that's how you would up in Chicago. And we got an equally good man from Rossby...so it's interesting to come back full circle to that, George.

It's a pleasure to see you.

Cressman: Thank you very much.

**END OF INTERVIEW**

