

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

TAPE RECORDED INTERVIEW PROJECT

**Interview with Robert H. Simpson
September 6 & 9, 1989**

Edward Zipser, Interviewer

[Diane Rabson, NCAR Archivist, was also present at the interview.]

Rabson: We are doing an interview today, September 6, 1989, with Robert Simpson. Ed Zipser is conducting the interview at NCAR.

Zipser: Bob, I know that you were born in Corpus Christi, I think, in 1912.

Simpson: That's correct.

Zipser: I don't know anything about your family. Can you say a few words about your parents, what they were doing in your early years, their education, etc.?

Simpson: The Simpson family is a pioneering Texas family. They migrated from Scotland, arriving in Texas, in the early 19th century. While my paternal grandmother was the British descendant of Sir Thomas Beauchamp, a 16th century baronet, my paternal grandfather, a Scot turned Wesleyan, was a Methodist minister, a circuit rider traveling by horseback between Austin, Marble Falls, Burnet and Liberty Hill, Texas, a circuit which at times involved interesting encounters not only with his parishioners, but also the Comanche Indians who resented the presence of the white man in their territories.

But my father, who in his early career was a schoolteacher, moved to Corpus Christi in 1920, where he met and married my mother, who had recently arrived from her earlier home in western Tennessee. In Corpus Christi, my father established an agricultural hardware business, much of his trade consisting of heavy farm and ranching equipment sold to the King Ranch in South Texas. I remember well the early days when I helped him deliver big steam tractors to the ranch's Santa Gertrudis headquarters near Kingsville. Always we were invited to the ranch home for refreshments, if not a wild game dinner. I recall how impressed I was, and how approachable, friendly, warm-hearted and concerned the Kleberg family were in welcoming us, even though they were the busy proprietors of that three million acre ranch (largest in the world at that time).

I graduated from Corpus Christi High School with an undistinguished record. In fact, by today's standards, I probably would have had a hard time getting accepted at a good university. I was introverted, spending most of my time as a loner in music. But, with a trumpet and a good teacher, my music activities proved to be the means by which I got to college. After winning first place in the state trumpet solo contest two years in a row, I was eventually invited to attend Southwestern University of Georgetown, Texas, to be first chair trumpet in the orchestra and band with promise of enough work to earn my way through college.

But I am now a little ahead of myself, chronologically.

There was another interest during high school which grew out of my exposure to three fascinating courses in architectural and engineering drawing. These led me to decide that I would pursue a career in architecture. A first step in that direction came after I graduated from high school when I got a job in San Antonio, working as an apprentice architect. However, after I had designed thirty substantial family dwellings and observed their construction, the economic crash of late 1929 put an end of this work and with it, my ambitions to go further in architecture. It was then, almost by default, that my music experience provided the opportunity to enroll at Southwestern, where I ended up majoring in physics and mathematics, rather than music which had brought me there to begin with.

Zipser: Did you have brothers and sisters?

Simpson: Yes, both considerably younger, my brother eight years, and my sister eleven years younger than I. My brother, after a wartime career in the Navy, became a Baptist missionary, first in the West Indies (where incidentally he was also a cooperative observer for the Weather Bureau) and later in Papua, New Guinea. My sister, until her untimely death at age 57, was a very successful music teacher in Corpus Christi public schools.

Zipser: How early did you have any inkling that you might turn out to be in science?

Simpson: I was very much interested, particularly in weather, from the time I entered elementary school in 1919. I was attending the David Hirsh School on North Beach in Corpus Christi when the great 1919 hurricane struck—the worst Corpus Christi has ever experienced. As luck would have it, the hurricane arrived on a Sunday morning. If it had been on a school day, I would probably have been among the several hundred casualties, because the school building, which was sought out by residents as a shelter, was destroyed. In this hurricane we were all less impressed with the wind than with the spectacular rise of water. The storm surge, as viewed from our near-shoreline residence, arrived in two sudden rises. The first put water about two feet over downtown street levels and occurred in a matter of ten to fifteen minutes at most. The second came one to two hours later when, in a matter of minutes, flood levels rose 6-8 feet over street level. This began to flood the interior of our house which was built quite high. The family had to swim—with me on my father's back—three blocks in near hurricane force winds to

safe shelter in the courthouse—the only high building in the downtown area. A lot of what I saw frightened me, but also supplied a fascination that left me with a lifelong interest in hurricanes. Not until years later, after I completed my initial graduate studies at Emory University, however, did a career interest in atmospheric science develop.

Zipser: Your interest in hurricanes is easy to understand. What about science in general? Did you make the connection between the interest in hurricanes and a career in science?

Simpson: My general interest in science, initially stimulated by my exposure to the field of architecture, was brought into focus in my physics studies at Southwestern and extended by the graduate work in physics at Emory University. Unfortunately, when I finished my MS degree in 1935, I found job opportunities for physics graduates were in very short supply. I had done my work in piezo-electricity and wanted to pursue further research in the promising new field of sonar. But I couldn't get a scholarship nor a research position. So once again I had to fall back on music as a livelihood. It had paid my way through all my undergraduate and graduate education up to that point. And the job I finally got was as band director at Crockett, Texas, the first of three such jobs I held during the five years before I got back into science. I ended up in charge of the instrumental music programs in Corpus Christi school system, where my total income was \$4,400 a year—not bad for schoolteachers in Texas at that time. But it was not what I wanted nor what by education I was prepared to do. So in 1939, I started taking Civil Service exams to qualify for positions in the Weather Bureau, Bureau of Standards, and Geological Survey. The Weather Bureau was the first to offer me a job—in fact, the choice of two jobs: at Brownsville or at Abilene. I accepted Brownsville, despite the fact that I was to be employed at the SP-3 level which paid only \$1,440 per year, a stunning reduction in salary. Surprisingly this reduction didn't seem to change our quality of life notably, in part, I am sure, because my fascination with meteorology proved so all-consuming.

Zipser: Did you have any second thoughts about taking a pay cut of \$3,000 a year in Depression dollars?

Simpson: Not at the outset. Before accepting the job, I consulted the longtime meteorologist-in-charge at Corpus Christi, Mr. J. P. McAuliffe, who reassured me, saying, “While this will be a big cut in salary, I'm sure you'll go up fast in the Weather Bureau with your educational background. In a few years, the Bureau will send you back to school on scholarship, you'll get graduate level training in meteorology, then nothing will stop you from rapid career advancement.” While his advice turned out to be fairly close to the mark (probably more fortuitously than his predictive skill), I soon found reason to question the advice.

When I got to Brownsville, I learned that the man in charge, whom I quickly came to respect as a meteorologist, had been with the Weather Bureau for 25 years, had attained a grade of P-1 which paid \$2,000 a year, and he seemed quite satisfied with his lot. Two other people at the station remained in the SP-3 grade after three or more years' service. This kind of outlook I wasn't prepared for. So when Mr. John Riley, Chief of Station Operations in Washington, visited Brownsville a few months later, I shared my concerns

with him. He seemed sympathetic and ended up promising me that if I would accept a six-month assignment at Swan Island in the West Indies, he would see that I got promoted to SP-3 and sent to New Orleans as an apprentice forecaster thereafter. I accepted.

“A lot of water passed under the bridge” before I got back to New Orleans as an apprentice forecaster, but it ultimately proved to be a genuine opportunity to get ahead in meteorology. New Orleans was a most interesting experience, not the least of which was my association with Welby Stevens, the most respected as well as the best educated forecaster at the station. As an intellectual, he succeeded in focusing my interests not on what I needed to know about the atmosphere, but rather the gaps in present knowledge that needed further research.

Zipser: It might be worth saying something about the operations at Swan Island. A lot of people don't know anything about this island and its strategic location.

Simpson: Swan Island, in the west central Caribbean Sea, was one of the more important benchmark stations in the early radiosonde network, with records beginning in the mid-1930's. During World War I, the Island was a strategic site for the Navy's "spark station" for communications relay. After the war, its primary use, until establishment of the weather station in 1935, was as a coconut plantation of the United Fruit Company. In 1941 when I arrived, the weather station had a complement of only four observers to carry out a program of observations and equipment maintenance. This included two radiosonde and four pilot balloon soundings per day, hourly surface observations, and climatic summaries, together with radio communications to transmit these observations. Also we had to maintain the old Cyclo-Ray recorder and other electronic equipment as well as the electric generators, water and sewage systems. What fascinated me most at Swan Island, however, were the tropical disturbances we experienced. One hurricane formed nearby and lingered in the vicinity several days before moving off, giving me a good idea of what a hurricane in the making is like. These experiences were the subject of several technical notes after I returned. The Swan Island experience gave me an opportunity to do a lot of reading and thinking about meteorology. Overall, it was good preparation for the job at New Orleans.

After a little more than a year at New Orleans, Dr. Reichelderfer, then Chief of the Weather Bureau, decided to send me to the University of Chicago on a scholarship assignment.

Zipser: I've heard you speak often of Dr. Reichelderfer.

Simpson: Yes, Dr. Reichelderfer was a remarkable personality, as well as a successful administrator, one who significantly influenced me personally as well as professionally. He was a very approachable person. Those in the field service of the Weather Bureau who visited Washington were always welcomed to his office; he was concerned and enjoyed talking with them, irrespective of their grade or assignment. He was a

stimulating intellectual, and I valued highly the many opportunities I had to talk one-on-one with him.

Zipser: If I could just back you up a minute, I think I heard a story from you about how you first got Reichelderfer's ear, one which had something to do with how you finally got off Swan Island.

Simpson: Yes, that was a memorable episode, possibly of interest to some. While I was still at Swan Island, the war broke out (World War II). Moreover, when my six-month tour there was up, it turned out that John Riley, the man who had promised that I would be sent to New Orleans as apprentice forecaster after six months at Swan, had been transferred to Kansas City as regional director. Joe Lloyd, who had replaced Riley, didn't seem to know anything about that promise. The answer from Lloyd about my inquiry was, "Sorry, you'll have to remain at Swan Island indefinitely." Because of the war it was his view that the Bureau could use me best at Swan; and besides, they didn't have a place for me on the U. S. mainland.

Zipser: How long did your six-month assignment there last?

Simpson: Presumably it would have lasted for the duration of the war. I wasn't very enchanted with that response, having left my wife and daughter in Texas expecting my return momentarily. Moreover, I felt that my background qualified me to support the war effort more effectively than I would by remaining at Swan. So I replied saying that as a native Texan I'd made a bargain on a handshake with a senior official of the Weather Bureau that I would be promoted and sent to New Orleans as apprentice forecaster if I stayed at Swan Island for six months. I expected the commitment to be honored. If not, then I doubted I could continue working for an organization that dealt with its employees that way.

The prompt reply from Lloyd was, "Sorry, you'll have to stay." We continued to exchange messages for several days until I finally radioed that a schooner was arriving at Swan Island the next week, and I intended to be on that schooner en route to another job in Texas since I could no longer work for the Weather Bureau under the circumstances. The very next day I received a radiogram signed by Dr. Reichelderfer saying that I was to be promoted and assigned to New Orleans. A United Fruit Co. boat would pick me up within ten days and transport me there.

Zipser: So it was in February, 1942, something like seven years after your professional degree, that you got your first professional job with the Weather Service.

Simpson: Yes. However, when I got to New Orleans and visited the office where I thought I was to be assigned, they had never heard of me; there were no orders there; they had no vacant positions and didn't expect any openings. Nor were they concerned enough to inquire further about it. I decided then to use (illegally) one of the transportation requests issued me when I left for Swan Island to proceed to the regional office at Ft. Worth to talk with the Director, Earl Hardy, whose reputation ran ahead of him as a hard man, but

fair and square—a demanding man but one who would act swiftly to correct an employee injustice. After I reached the regional office and Hardy heard my story, he sent me out to lunch with his deputy, Russell Grubb. By the time I returned, he had called Dr. Reichelderfer, had confirmed my promotion from SP-4 to P-1 with reassignment to New Orleans as forecaster, and fresh orders transmitted by teletypewriter were waiting for me. Even for those days when personnel administration in government was much more straightforward than now, that was fast action for such an unusual personnel action. But it was characteristic of Dr. Reichelderfer, whose concerns reached to the lowest echelons—and of Earl Hardy.

Zipser: But it didn't hurt you to have gotten Reichelderfer's ear?

Simpson: No, it didn't, and I had numerous but carefully selected opportunities to avail myself of it later. After I had spent nearly a year in New Orleans, during which I had been offered, but not allowed to accept, a commission in the Navy to do forecasting in the Pacific, I contacted Dr. Reichelderfer to inquire what he might envision my career opportunities to be, building on my educational and experience background. I received a very sympathetic hearing without promises, but within several weeks I had orders transferring me to the University of Chicago as a scholarship student.

I arrived at Chicago in the heyday years of the A-course training for military meteorologists [USAAF aviation cadet program—*DR*], a program conceived and sold to the military by Carl Rossby, to create a pool of 10,000 qualified forecasters in the early years of World War II. This was a grand and exciting experience. Rossby and Horace Byers were running the school at Chicago, with Victor Starr, Michael Ference, Oliver Wulf and Helmut Landsberg, among others, as well as such rising young stars as Vern Suomi and John Bellamy.

Zipser: The latter were there as students?

Simpson: No, they were graduates kept on as instructors, concerned primarily with laboratories and with instrument and methodology development. They came, I believe, in 1940 or 1941, while I got there in May of 1943 and stayed until May of 1944. Dr. Byers then proposed that I stay to complete my Ph.D. at Chicago. The War Manpower Commission, however, didn't agree so I was sent to Miami to be a hurricane forecaster under Grady Norton. This consisted of district forecasting and participating in the hurricane forecast and warning program during summers. Two people seemed to be enough for that job, the way the Weather Bureau was staffed at that time. Warren Johnson (of the Fruit Frost Service in Lakeland) had been helping Norton during hurricane season up to that time, but after I arrived, came in only when we had to deal with more than one hurricane at a time. Most of the time, Norton had only one assistant to prepare forecasts, write advisories, and issue warnings.

Zipser: How would you describe the professional conditions of the office at that time? What was the attitude toward forecasting, towards research?

Simpson: Toward research, not very good. Towards forecasting and public service, very constructive. Norton was one of the most curious-minded people I've ever known. He had had a reasonable undergraduate education, not in science or meteorology, but he had an abiding interest and curiosity about weather since his youth, when he had run a cooperative weather station. In the Weather Bureau, he had enjoyed an association with the Bureau's "dean" of forecasters, Charles L. Mitchell, and became recognized as an outstanding forecaster. He was a keen observer and applied his observations systematically.

In that small office, we had four "sub-professionals" to do map plotting and help with communications and some analyses. We analyzed a substantial area from southern Canada to Panama, and the central Atlantic to the eastern Pacific, and constructed prognostic charts as necessary for our forecast responsibilities. I would say there was never a group I worked with more dedicated to serving the public completely than at that office in the penthouse of the Congress building in downtown Miami.

Grady Norton was not only a well-known personality within his constituency, but was also one of the most respected forecasters I've ever known, probably the most effective hurricane forecaster. However, it took me nearly two years of working with that man to realize that he NEVER issued a hurricane forecast if he didn't have to. He advised the public very astutely about what was out there, where the hurricane was, where it was moving, its previous path, how hazardous and how important it might become—and the people listened! Listened with rapt attention, and when the storm got close, and it was time to move, and he said, "Run for it"—they ran! They listened, and they trusted him. He was a most effective public servant, but he never issued a *forecast* until the chips were down, and the warnings had to go up, then he would call his shots clearly. On the other hand, within the office, he was forever making forecasts and very good ones, often written on the margins of the analyzed weather map. During the hurricane of October, 1944, he made a forecast for internal use while the hurricane was moving slowly westward in the Caribbean near Grand Cayman, calling for the center to move across Tampa Bay at midnight two days later. It did just that at 10:00 PM instead of midnight, one of the best forecasts I've ever encountered.

Afterward, I asked Grady, "How did you do it? You must explain the process you go through in making such a forecast."

"Well," he replied, "I just look at the steering currents and reason how they're going to change and make my forecast accordingly."

"But Grady," I continued, "I draw those streamline charts too, and I know you consider the wind at the top of the hurricane steers it. But I don't know how you can come up with the reasoning for changes in the circulations during the 48-hour period." After a little silence, he replied, "Well, if you really want to know, Bob, when I'm in doubt about something important like that, after thinking it over a long time, I go out on the penthouse roof, put my foot on the parapet, look out over the Everglades and say a little prayer. Then, when I return to the office, I know what I'm going to forecast."

Zipser: It wasn't science, in any case.

Simpson: Suffice to say it was systematical, organized, and sincerely done.

Zipser: But not the way we would define it now. Whereas you systematized your observations, your deductions, your physical reasoning, and wrote them down so somebody else could use them. That's not the way we think of science now.

Simpson: Of course not. We've come a long way since then. And I've been privileged to watch the progress of science and technology through the years, including the explosive advances simulated first by the urgencies of World War II and later, motivated competitively following the launch of the Soviet's Sputnik.

Zipser: Well, I know that other opportunities took you away from Miami not long after 1946. But I'm curious. In retrospect, could you have been content to stay on as Grady Norton's right hand for many years?

Simpson: No, indeed. I tried to understand Grady's concept of steering at the top of the hurricane and some other cornerstones of his philosophy concerning forecasting, which I found difficult to reconcile with my own knowledge of physics and dynamic meteorology. I felt there was so much I needed to learn, as well as so many ideas I wanted to pursue, but hardly knew where to start. Realistically, it was clear, however, that Miami, under the circumstances, was not the milieu for realizing those ambitions.

I did write a few papers or notes which got me in trouble with some people. One of the first was with reference to warm rain over the Everglades. I published an article in the Bulletin of the AMS, in 1944, I believe, which promptly got me in a controversy with the great Swedish meteorologist, Tor Bergeron, who didn't consider I was writing from a well-informed viewpoint. We carried on a lively conversation in private correspondence with one exchange in the literature. While I was never able to convince Bergeron there could be such a thing as warm rain, particularly warm showers, I was confident of my observations because I knew where the showers were located from my vantage point atop the Congress Building in Miami, and with a clinometer, it wasn't difficult to compute heights of the cloud tops. Day after day we observed what without question was significant warm rain from cumulus congests over the Everglades. So I thought it ought to be written up, and I did so.

These and a few similar "capers" led to my temporary assignment to Washington Headquarters where I found great stimulation from my associations with the research programs there. In 1947 I was transferred "permanently" to Washington. Meanwhile there was another interruption to my Miami assignment. This was in 1945 when the Air Force asked Dr. Reichelderfer for the loan of Dr. Robert Fletcher and myself to help establish the Air Force School of Tropical Meteorology in Panama. This was a program to provide an educational retreat for A-course graduates who had been taught temperate latitude meteorology but found themselves ill-equipped to deal with the tropical weather

they had to forecast in the western Pacific. This program was similar to that of the Navy's Institute of Tropical Meteorology in Puerto Rico established several years earlier. The Air Force School was at Howard Air Force base which had been carved out of the jungles in Panama. The training at the school, six days a week, was somewhat unique and quite intensive. Courses in dynamic meteorology were taught by Bob Fletcher, in radar meteorology by Myron Ligda, and tropical climatology by Lt. William Odom. I was responsible for a program of synoptic meteorology of the tropics, which combines a series of lectures and laboratory exercises making use of two C-47 transport planes (fondly known as "gooney birds"). These were modified to provide eleven training positions, each with navigation equipment, a free air temperature readout, and a radar repeater scope at each position. The students were awakened three mornings a week at 3AM and brought to the weather office, where they would plot and analyze the latest weather observations, prepare a prognostic chart, and forecast. Then they would board a gooney bird and verify their route forecasts. One plane would fly north to Jamaica, the other fly south to Salinas, Ecuador. When the planes returned, the students returned to the weather office (before dinner) for a postmortem discussion of the reasons for the weather encountered, and of the faulty reasoning which made all too many of the forecasts fail. These laboratory exercises provide the focus for the lecture of the following day. To some students this was a rude awaking to reality, but it proved, nevertheless, an effective learning experience for all of us.

Zipser: What book did you use?

Simpson: We used a set of notes and illustrations prepared by Bob Fletcher and myself.

Zipser: So essentially self taught.

Simpson: Yes.

Zipser: It seems interesting. Looking back on it, the war was nearly over before the Air Force decided they needed this.

Simpson: Yes; however, while only three classes, six weeks each, passed through the school before the war was over, serendipity played a role in our experiences at this school, opening up new ideas and avenues for future valuable research.

Zipser: How did you and Bob Fletcher decide on your concepts; for example, one concept in common use, as I understood it was the frontal concept of the ITCZ or intertropical convergence zone. I wonder how you taught yourself that that was a nonsense concept?

Simpson: It was not exactly a case of the blind leading the blind, although ignorance did abound. My experience at Chicago had brought me in contact with such people as George Cressman and Herbert Riehl who, from their work in Puerto Rico, had set forth some fairly sound and fundamental principals of tropical weather systems including the concepts of an ITCZ and of easterly Waves.

Zipser: Did you have any trouble finding easterly waves in Panama?

Simpson: Yes; frequently they were difficult to identify under the overpowering influence of the intertropical convergence activity in the area. At Panama, of course, the ITCZ received at least as much attention as the easterly wave. In those days we had five radiosonde observations in that area some of which, including that at Clipperton Island (10-degrees-North, 110-degrees-west), were classified and could not be transmitted elsewhere in a timely manner. With these we were able to make very good structural analyses of the ITCZ as it surged northward across Panama, then reformed to the south. Several papers were published based upon this work.

Zipser: I guess the other thing we should talk about is your first hurricane flight.

Simpson: That happened while I was in Panama, a mission undertaken in one of the training C-47s, perhaps with a confidence that was undeserved. In July, 1945, when our gooney birds had proven their “flight-worthiness,” we decided to reconnoiter the first hurricane that came within our flight range. A small hurricane observed approaching the Lesser Antilles later that month turned out to be just what we were looking for. After predicting that it would move to a position in the central Caribbean just south of Hispaniola 48 hours later, we decided to deploy to Curacao, overnight there, and launch our research mission from there the following day – a relatively audacious, if not injudicious undertaking, considering that operational flying in hurricanes had been initiated only a year earlier after Joseph Duckworth, in 1943, had made the first (deliberate) flight into a hurricane eye. However, plans worked out just fine. The hurricane was located only a degree west and a bit north of the predicted position. The plan was to circumnavigate the center keeping the wind on our port quarter, thus spiraling gradually into the eye. While at a flight level of 5,000 feet, we got only infrequent glimpses of the sea surface through the drift meter; the radar kept us in excellent touch with the eye center. The only problem, as it turned out, was that as we found ourselves in heavy rain tucked into a corridor equidistant from the eye and the 8,000 foot mountains of the Haiti southern peninsula, the radar quit on us. However, since we were already pretty well committed, we kept on the projected spiral path and wound up in the eye with little difficulty, entering from the southwest. However, we had used more fuel than expected, and did not have enough for a safe return to Curaçao. So we declared an emergency and landed at Ciudad Trujillo – now known as Santo Domingo, much to the distress of the U.S. ambassador, since our relations with that country were a bit too tenuous at that time for a military plane to be landing there. He chewed out the unfortunate Air Force pilot quite eloquently when we landed. But all ended well as we returned to Panama without further incident, and with a bevy of new questions to explore about hurricane structure.

Zipser: Okay, we’ve taken Bob Simpson to 1945 and his experiences at the Air Force School in Panama. What happened after the war ended and you left Panama and came back to Miami?

Simpson: So many questions had been raised by my experiences in Panama and Miami about hurricanes and tropical meteorology I wanted to get on with research – research that I

didn't feel could be effectively done at Miami. I asked and received, first, a temporary assignment to Washington during the spring and winter of 1946, then ended up with a "permanent" transfer there early in 1947. At the outset my new assignment in Washington was in the training branch, working with Al Carlin to provide an indoctrination of military forecasters transferred to the Weather Bureau after the war. This included brief refresher courses in physics and dynamic meteorology as well as procedures and policies of the Weather Bureau in preparation for assignments as forecasters and in other professional positions. I still had to "bootleg" time to do research and research planning. After several months with the training branch, I was transferred to a position as executive assistant to the Deputy Chief for Operations, Mr. Delbert Little. However, during the summer of 1947, I was able to arrange to spend six weeks at Kindly Air Force Base in Bermuda where I had been given permission to ride "piggy-back" missions with the Air Force 53rd Recon Squadron – the famous "Hurricane Hunters" – during hurricane reconnaissance flights. On these missions, once the operational information was obtained, I was allowed to use the remaining air time to obtain research data. The Air Force soon appreciated the potential value of this research data, and when the great hurricane of September, 1947 came along, I was allowed to mount a number of special flights devoted exclusively to research objectives, using new WB-29s just being delivered to the 53rd for hurricane recon. These were the first aircraft, I believe, to have analogue recorders for the meteorological probe systems. The main objectives of these flights were to find out what really was going on at the top of the hurricane, especially the character of circulation there. Recall that Grady Norton was convinced that the circulation disappeared somewhere below the 2mb level and that the geotropic flow at that level comprised the steering current. In view of the earlier work of Haurwitz, I believe in the mid-30's, I did not see how the vortex could disappear at such a low level, but it was important to learn the distribution of vorticity at upper levels and learn if the circulation near the top of the hurricane did relate discernibly to the steering of the vortex. So I proposed to use one of the new B-29s to get as high as we could and try to describe the circulation near the top of this large hurricane. After it had moved north of Puerto Rico, it passed over the Bahamas, across Florida into the Gulf of Mexico and finally inland near New Orleans, first disabling the Miami Hurricane Office, then briefly the New Orleans Office. On two occasions, one as it approached the Bahamas, and later in the eastern Gulf of Mexico, high altitude research flights were made, circling the center just outside the eye. With the superchargers installed on these B-29s, we were able to reach a pressure altitude of about 36,000 feet, which in the warm core of this hurricane was close to 40,000 feet above sea level. Here we skimmed through the tops of clouds as well as circumnavigated the eye four consecutive times. With five good Loran fixes made during each circuit, it was possible to measure a mean tailwind component of 1000 mph, only a bit less than the maximum wind surface. As far as I know, this was the first observational evidence of the massive vertical transport of momentum by convective clouds in the eyewall - a process which created an imbalance between pressure gradient forces that diminished with height, and the angular momentum which remained almost constant, thus establishing the centrifugal acceleration needed to carry the warm air near the center out into the colder environment and with it a shield of outflow cloudiness.

Zipser: You could navigate by radar?

Simpson: No. Nor could we see the surface, although we could look down into the eye.
However, in those days we had very good Loran coverage in that area and the succession of fixes – five on most circuits of the eye – gave consistent verification of the strong tailwinds we were encountering.

Zipser: Did you ever agitate in those days for good radars on hurricane recon plans?

Simpson: Yes, and no! I have always agitated for better recording systems and navigation facilities. However, in those days radar still had a long way to go, and realistically, no one expected much better radars on aircraft than the old APS-2F and the Navy counterpart for awhile. However, I did push hard for better recording systems at that time. The analogue systems we were using gave apparently useful information on gradients of values, but absolute values of the data obtained, especially of temperature, were not taken too seriously.

Zipser: The Navy got better radars some time after that, didn't they?

Simpson: Yes. Both the Air Force and the Navy were hard at work improving airborne radars; however, I believe it was not until the '50s that the Lockheed Constellation aircraft were equipped with high resolution powerful "early warning" radars. The Air Force B-47 also had a superior radar, although it was an X-band (3 cm) system which was highly attenuated in rain.

Zipser: Did you have the opportunity to write up these research results and establish any kind of research program?

Simpson: Not really. My Washington assignment in those days was primarily in operations, and I still had to "bootleg" the time spent in research. However, since my desk in the central office was just outside Dr. Reichelderfer's office, I had the opportunity to spend many "after-work" hours reviewing with him and Dr. Wexler the data obtained from my research flights, and not only received much encouragement but also many excellent suggestions from both. A number of descriptive papers did come from this effort. Reichelderfer really felt the need for research in tropical meteorology in those days and had asked me to develop a plan and budget to systematically extend the hurricane research effort. The first plan was submitted to Congress in 1948 (and was quickly rejected). My activities in this area were interrupted, however, when I was transferred in May, 1948, to Honolulu in charge of Weather Bureau Pacific Projects, the forerunner to the Pacific regional office established there in the mid 1950s.

Piggyback research flights from Bermuda were not resumed until I returned to Washington in 1952, and not until 1954 when three hurricanes –**Carol**, **Edna**, and **Hazel** - all ravaged the coastlines of New England, did we get budgetary support to establish the National Hurricane Research Project at West Palm Beach, Florida.

Zipser: Let me get this straight. You say you basically started back in 1948 proposing something like the Hurricane Project?

Simpson: Yes. However, in the 1940s we weren't thinking in terms of large, expensive projects, but rather a small well supported program dedicated to better data acquisition, with a few scientists devoting full time to hurricane research. It was only in 1953 and '54 that we began to think in terms of acquisition, with a few scientists devoting full time to hurricane research. It was only in 1953 and '54 that we began to think in terms of acquiring aircraft of our own totally dedicated to research flying.

Meanwhile, I left the Washington scene for my next assignment at Hawaii.

Zipser: What about your assignment in the Pacific?

Simpson: I almost didn't go. My family didn't want me to leave Washington. But Dr. Reichelderfer insisted the assignment would be a key factor in my career development. He wanted someone to consolidate the activities of the four Weather Bureau offices there under one manager. These activities included aviation and agricultural forecasting, research (mainly in support of pineapple and sugar cane harvesting operations), climatology, and the management of the 1000 gauge rainfall network in Hawaii, and the Pacific Projects Office including the supervision of observing and forecast stations across the Pacific, most of them in the process of transfer from military to civilian management. I was reluctant to go not just because my family opposed it, but because I had observed that promising young meteorologists who accepted jobs overseas usually found, when they returned to Washington, their peers had meanwhile scrambled up a career ladder that could not be competitively accessed by those overseas. It took more than a little persuasion for Dr. Reichelderfer to convince me to go. After two weeks passed with no agreement reached, he suggested, "I think you should write yourself a letter for my signature stating the circumstances under which you will agree to take this assignment, which will probably be the biggest career opportunity you will have over the next ten years." So I prepared the letter, a pretty strong one too, enough so that I wondered if he would even consider it. But he did, signing it without a change, specifying: (1) while in Hawaii I would report directly to Reichelderfer for budget matters, personnel actions, research plans and activities, and for counsel and advice; (2) after four years, unless extended by mutual agreement, I would be returned to Washington into a job at or comparable to those of my peers who had been promoted while I was away. This ended up placing me in the strongest position of anyone in the field service, probably far more favorable than that enjoyed by any field service manager before or since. With this flexibility, I was able to do quite a bit of useful research, and at the same time generate research interests among the forecasters and other staff members.

At the same time, my membership on the Joint Meteorological Committee, coordinating the transfer of meteorological responsibilities from military to civilian sponsorship, identified me as a staff member of CINCPAC (Commander in Chief, Pacific)—at that time Admiral Radford, and also on the staff of CINCPAC (Far East) – that time General MacArthur. This involved a great deal of interesting travel, including trips to Tokyo four

times a year. This left me with an interesting array of impressions and baffling questions mainly about Japan during this reconstruction period, and the impact that MacArthur was having on the social and economic systems of that country.

With the Emperor somewhat defrocked as a deity, it was U.S. policy to avoid having him saddled, in the eyes of his people, with responsibility for losing the war. The process, however, left a vacuum among a ritualistic people who could no longer bestow their esteem as before on their emperor. It turned out that many Japanese turned to MacArthur as the subject of their esteem, gathering at his headquarters building in downtown Tokyo each morning, many to prostrate themselves before him on his arrival, a ritual which MacArthur, insofar as was evident, did nothing to discourage. Nevertheless MacArthur did an incredible job of supporting the reconstruction effort, and in setting Japan along the road to democratic, capitalistic government and life style that led them to become a giant among nations.

Still another benefit from membership on Admiral Radford's staff at Pearl Harbor resulted in the establishment of the Mauna Loa summit observatory. Shortly after arriving in Honolulu, I was interviewed by the science reporter of the Honolulu Advertiser, with the result that a front page article the next day proclaimed Mauna Loa to be an incomparable natural laboratory for study of atmospheric processes because of its smooth gentle slopes extending upward through the trade wind easterly to upper tropospheric westerlies where the atmosphere was free of pollutants, the nearest source being thousands of miles away. The article went on to suggest that an observatory near the summit would provide an unparalleled benchmark record for studying the structure of the atmosphere and of weather processes.

To my surprise, only two days later, I received a call from a man by the name of Tom Vance, Director of Institutions in Hawaii, who said, "I think we might be able to do business together in achieving our common interests in Mauna Loa. I have a prison camp at Kulani deep in the rain forest at the 5,000 feet level on Mauna Loa. The inmates there need a challenge to help equip them to be responsible citizens when they leave prison. They can help both in road building and in construction of the observatory building." Not until later did he confide that what he really wanted to do was establish a ski lodge at the summit to be operated by the inmates. This at first seemed a questionable way at best to go about the establishment of the observatory, but after further thought and consultation with Dr. Reichelderfer I decided to pursue the matter further. The big question was how to acquire the road building equipment to construct the access road. To explore the options, I sought out my colleagues on the JMC/Pac at Pearl Harbor. I explained what we wanted to do and told them, "I can get the help of the Geological Survey at the Volcano Observatory to help stake out a roadway likely to be safe from lava flows, and can provide from my regional funds the cash to purchase building materials and equipment for the observatory. The territory (of Hawaii) can supply the labor to construct the building and to build the road. However, we don't have the equipment to build the access road. Can you help us?" They suggested we take the matter to Admiral Radford, which we did. He agreed to loan the project two big road building CATS and ship them to Hilo, providing others would pick up the cost of fuel and spare parts. I quickly agreed the

Weather Bureau would do so. Over my signature as member of the JMC/Pac, the Navy supplied the two road building CATS, and therewith a project was set in motion supported officially only by a handshake or two, to establish an observatory at the summit of Mauna Loa just below the caldera of Makeoweoweo Crater at an altitude of 13,453 feet. It was a small but auspicious beginning.

The real fight came, however, after getting final approval from Dr. Reichelderfer, in persuading the Secretary of the Interior, Oscar Chapman, to allow us to use National Park land to build the road and erect the observatory building. This was a tough job. But with Reichelderfer's help, we finally got approval, after strong objections by Chapman's staff. Plans were finally completed to put a small observatory building (about 10'x10'), with heat and a cooking facility, a bunk for overnight visits, and oxygen to supplement the thin air, in addition to the autographic equipment and probe systems to record wind, temperature, pressure, and rainfall. A support base was set up at the Hilo Weather Office with additional staff so an observer could visit the summit facility once a week to change recorded sheets and check the equipment. Gordon MacDonald, Director of the Volcano Observatory, made a number of trips to the summit with me and on one occasion with Charles Woffinden, Head of the Weather Forecast Office at Honolulu, to stake out the access road. The building itself was constructed in sections at the Kulani Prison site and transported to the summit on a big truck. With the work complete in the fall of 1951, a big inaugural service was held at the summit site attended by some 50 officials, including the Governor of the Territory, Sam King, Washington representatives from the Departments of Commerce, Interior and Navy, as well as a number of scientists from universities and government laboratories. All were transported to the summit in four-wheel drive vehicles. The dedication by Governor King took place during a light snowfall and was followed by an open-pit barbecue around a blazing bonfire.

We were off to a humble, though significant start, which received wide press coverage. But it fell far short of what was ultimately hoped for as regards measurements and ease of access. The road left much to be desired. Despite the fact it had been carefully sealed with cinder cone material, it deteriorated rapidly. My plans were, ultimately, to have more comprehensive measurements requiring monitoring by personnel in residence at the observatory. I had, among other things, hoped to make daily measurements of total ozone.

Why ozone? Because I had been impressed by the potential significance of the cold lows that continually paraded westward from the California area across Hawaii, many of them eventually reaching the western Pacific where they seemed to be associated in some way with the formation of typhoons. Clarence Palmer had proposed an extra-terrestrial source of energy for these lows, a very controversial idea. Nevertheless, it was my idea that we might learn a great deal about these lows and how they might ultimately contribute to typhoon formation if we could study them as they moved over Hawaii, using ozone as a tracer. This and other perhaps more sophisticated equipment would have to await more substantial funding; but we *did* have our foot in the door.

After I left Hawaii, however, it got progressively tougher to maintain the facility at the summit site and finally it was decided, much to my distress, to close it altogether. To me, we had established a foothold at a site with enormous potential for scientific observations of many kinds, and I felt we had been less than perceptive in tossing in the towel so soon.

This, however, was not to be the end of the saga.

A few years later, after I had been named the first director of the National Hurricane Research Project (NHRP), another opportunity turned up quite by accident to re-establish the observatory, albeit at a slightly lower elevation, 11,300 feet.

In May, 1955, I took the longest vacation I ever had in the Weather Bureau, five weeks, in which I took my two daughters to the canyon country in the western U.S. We stopped along the way at Sacramento Peak Observatory, where astronomical observations were being made. There I met a scientist from the Bureau of Standards named Ralph Stair, who was trying to make solar flare observations under very adverse conditions. During the weeks he had been there, he had only had three days of good observing weather. Stair and I soon found we had a number of common interests. I suggested, "Why don't you go someplace where you can conduct observations more than 350 days a year?" "Is there such a place?" he asked. "Oh, yes," I replied. "At Mauna Loa summit. We had an observatory there for awhile which could be re-opened with a little help." "Tell me more" he said. I explained, "The biggest problem is building a permanent access road and a building suitable for continuous occupancy at the site. The Weather Bureau is hamstrung in contracting for such tasks, which must be done through the GSA and usually turn out to be prohibitively expensive. However, I happen to know that the Bureau of Standards has enabling legislation which allows it more freedom to do such things. If we can work together, we may both be able to accomplish our objectives more easily than we could alone. If you, on behalf of the [Bureau of] Standards, would become prime contractor for the project, build the road and observatory building suitable for continuous occupancy, then purchase and install a Dobson ozone instrument, and measure total path ozone at the site for a minimum of two years, I think I can justify diverting funds from the NHRP to cover costs of the road and the building." We both agreed that at first blush it looked feasible and that we would pursue it with our respective bosses.

When I returned to Washington, I had a rougher time with Reichelderfer on this matter than I expected. He was concerned about "congressional intent" for use of NHRP funds, and doubted the connection between ozone measurements and cold lows could be credibly related to tropical cyclone research. At this juncture, however, unexpected support came from Harry Wexler, Director of Research, who said, "This sounds great. And we could measure carbon dioxide there, too. It is a site completely free of upstream pollution and at elevation, which frees it from local pollutants. This could become truly a baseline station for observing and measuring increases of CO₂ in the atmosphere." And with that, the Weather Bureau Central Office was off and running, the money was drawn from NHRP funds, the Bureau of Standards did its thing, and the present observatory and its important record of CO₂ measurements became a reality.

Zipser: I never knew that. I learn something new every day! Well, while we are still in the Hawaii years, do you have something more to add?...

Simpson: I could go on for hours about Hawaii, it was such a stimulating experience. I learned so much meteorology while there, wrote several useful papers, and, most of all, broadened my interest in tropical meteorology beyond my keen interest in hurricanes. But I think we should forge ahead to that period which followed Hawaii.

Zipser: I would like you to tell what happened when you got back to Washington. Back to Little's office. I think that was right before the famous Texas tornadoes in 1952.

Simpson: Yes. I came back right on schedule according to the original letter which sent me to Hawaii. In fact, I left Hawaii on May 18, 1952, four yours to the day from the time I had left Washington. Once again, I was assigned to Mr. Little's office with still another promotion; however, I was used by Dr. Reichelderfer primarily for troubleshooting various problems in the field service of the Weather Bureau. While it was understood that I would return to Bermuda for the whole summer, again riding piggyback with the Air Force on hurricane reconnaissance flights, I would work for the remainder of the year in the operations policy area.

One of the very first assignments I had thrust upon me in this job was to deal with the aftermath of the Waco and San Angelo tornadoes of 1952, in which many lives had been lost. The problem centered around the issuance of tornado forecasts by the Air Force for military operational use. While this was commonly known, the government policy (at the insistence of the Weather Bureau) was that these forecasts would not receive public dissemination. In the absence of an effective PR effort by the Weather Bureau, the public couldn't see why, if the Air Force could make these forecasts, the Weather Bureau couldn't do so to the public's advantage, or at least allow the Air Force to make public dissemination of their product. Scientifically, the Weather Bureau had correctly assessed the problem in deciding that with the gross uncertainties in tornado forecasting and particularly the poor understanding of the physics involved, the most useful public service would be to mount a program of cooperative tornado reporting, with upgraded communications to disseminate warnings promptly. Much had been learned about the use of radar to detect tornado signatures. While few radars were available for this purpose, the policy position was to push plans for a radar network, a policy which had been received very coolly by the Congress because of the expense. Nevertheless, with the death and destruction of Waco and San Angelo that spring, the Texas public was inconsolable – in fact, outraged. As a result, the governor called an open hearing in Austin to decide what the state should do “to protect itself if the federal government couldn't”. The temperament of Texans at this hearing was that as far as meteorology was concerned, Texas should secede from the union.

Dr. Reichelderfer asked Capt. Howard T. (Shorty) Orville, Director of the Navel Weather Service (and Reichelderfer's close friend and colleague), to attend this meeting, representing the three weather services: Air Force, Navy, and Weather Bureau, and sent me with Orville to represent his office – to listen and answer questions, and where

possible to supply help and advice. The meeting turned out to be more intense than either Shorty or I had expected (though probably not more so than did Reichelderfer, who perhaps wisely decided to remain in Washington). However, Shorty, in his usual statesmanlike manner, gave a very fine summary of the events, supporting the Weather Bureau policy on tornado forecasts, and explaining why military forecasts were not in this instance a useful product for general public consumption. He did his very best to pacify the group. However, when the hearing adjourned for lunch, it had been agreed to return in the afternoon to decide what measures the state should take to set up a separate forecast service for tornadoes.

Meanwhile, I called Delbert Little, my immediate boss in Washington, to discuss the status of a project at the Bureau's Instrument Division to modify surplus military airborne radars, inserting new electronics and installing a six-foot antenna to provide resolution sufficient to detect and track severe weather. I knew that the Bureau had just acquired 100 surplus APS-2F radars which someday might be readied for installation across the U.S. I asked Little if he thought we were at a point where we might propose a cooperative plan in which the cost of modifying and installing the APS-2Fs might be borne by individual cities in Texas in exchange for the WB's help in operating the radars and maintaining direct communications links between radars for detecting, and tracking tornadoes. Little agreed we were technically able to do that immediately, but would have to talk to Reichelderfer about the policy. He did, and called me back forty minutes later giving the go-ahead to make the proposal that afternoon. So, when the hearing reconvened in the afternoon, I asked for a moment to make a statement before the agenda of the afternoon were undertaken. After reviewing briefly Orville's earlier statement about the Weather Bureau policy on tornado forecasts, I proceeded to make the proposal approved by Reichelderfer during the noon hour, indicating that for every city willing to subscribe \$10,000 to the cause, the Weather Bureau would modify and install a weather radar capable of detecting and tracking tornadoes, would operate it and provide direct communications between all radars and forecast centers so the public could be kept advised of all existing tornado threats. This proposal turned the mood of the meeting around; it was agreed to pursue this approach to public warnings in Texas; and a delegation was appointed to solicit participation by various cities. In the end about twelve cities signed up for the program. The Weather Bureau contracted with Texas A&M University to modify and install the radars, and within a little more than a year, the Texas radar network was in operation. The program was so popular that cities in Oklahoma, Louisiana, and Arkansas asked to participate as well. Shortly thereafter, senators and congressmen, amazed at this groundswell of interest and concern, decided to support the concept of a National Radar Network, which led to the development of the WSR-57 radar and the present umbrella of weather radars in the U.S.

Zipser: Before we get onto the Hurricane Project history, I just wanted to ask one followup question on the National Radar Network. Presumably a few things had to happen between the time when a consensus to establish the National Radar Network took hold and the time it actually was approved. Did you have some continuing role during this establishment period to make that radar network a reality, and also I am curious as to

whether you might perceive a major difference in the way big government procurements go now compared to the 1950s. Lessons learned and that sort of thing.

Simpson: In answer to the first part of your question, I served primarily as a catalyst. As I said, I served Dr. Reichelderfer and Mr. Little primarily as a troubleshooter, investigating and analyzing operations policy problems and recommending solutions. But by-and-large, once policy problems were ironed out and a modus operandi specified, the operations staff took over and followed through. I think the important thing here is the principle, one I saw work well a number of times, at least in the earlier years of the Weather Bureau. When an important but expensive program is ready for implementation and is essential to the public interest, it is rarely possible to sell the program directly on the promises of the new technology alone. Success, after first establishing feasibility and the peripheral impact on related programs, depends upon the design of a pilot project to be implemented when and where a crucial local need exists, so the enthusiasm and endorsement generated, when a local need is effectively satisfied, attracts broader support for implementation of the program nationally. Government budgetary processes are largely influenced by crises, and a large expensive program almost always has to look to crisis or disaster, local or national, to get the support it needs for implementation. The trick is to be prepared and resourceful enough to strike promptly when the crisis iron is hot, demonstrating from a pilot effort the wisdom of the larger proposal. In the case of the Texas radar network, the Weather Bureau, rebuffed by Congress in attempting to systematically implement a national radar network, seized upon adversity in the Waco and San Angelo disasters to demonstrate the worthiness of radar as a warning tool, and thereby gained the support for implementing a national program.

Zipser: I think we ought to move now to certainly what many of us remember you perhaps the best for, which was your role in the establishment of the Hurricane Research Project (subsequently Laboratory), and we, of course, know that this was stimulated by a series of hurricanes on the East Coast in 1954: **Carol**, **Edna**, and **Hazel**. Just tell us in your own words what happened after that, and your role in it.

Simpson: Earlier we mentioned that virtually every summer of my Washington assignment, I spent some time “riding piggyback” with the Air Force Hurricane Hunters from Bermuda on their hurricane recon flights. From these flights I became convinced that the movement and development of the hurricane was a problem of interaction between the processes occurring in the hurricane vortex and the circulation character of the environment. These convictions grew mainly from the observations and data obtained during the hurricane over flights in 1947 which not only denied Grady Norton’s concept of a geostrophic steering current moving “at the top of the storm” (suspected to be 300 mb), but also demonstrated the role of convection in vertical transport of surface layer momentum to the overflow layer. If we were to understand these processes and to make use of them in forecasting, we needed first a better understanding of the structure of the vortex, and secondly the nature of the interruptions between vortex and environment. This dictated a high priority for research flights through the vortex at a number of levels. Unfortunately, I had a difficult time selling this to my scientific colleagues, including Harry Wexler and Jerome Namias, as well as operational people including Grady Norton,

who remained convinced that hurricane motion, if not development as well, was simply a problem in synoptic scale dynamics. The plans and proposals developed in the early 1950s for hurricane research were aimed mainly at establishing a unit of competent research scientists who would devote themselves to the hurricane movement problem, using data from all sources including those from operational reconnaissance of the kind I had acquired in earlier years.

Zipser: Who made these proposals?

Simpson: I made them. However, they incorporated ideas and suggestions from quite a number of sources. There were frequent exchanges of views among the researchers and the operational people in the central office, mostly due to the efforts of Harry Wexler who, to his credit, sponsored weekly seminars in the old main building of the Weather Bureau Headquarters at 24th and M St., NW, and insisted that operations people as well as researchers attended. This brought together and evoked lively discussions among people with broad and diverse interests, not only from the Weather Bureau, but the Air Force and Navy as well. In the process, the hurricane and the problems it posed received a great deal of attention. The proposals drew considerably upon these discussions. My concerns with the interactions between scales of motion in the hurricane and its environment were fed constructively by these discussions and were reflected, not only in the first proposals, but also in the final plan of NHRP. The first proposal was developed in 1952. It didn't get anywhere in that budget year, or in 1953. But in 1954 something happened that provided enough concern, especially in Congress, to generate support for a national program of hurricane research. That year we had three hurricanes: **Carol**, **Edna** and **Hazel**, each of which affected New England and the middle Atlantic states. All were important, notable hurricanes, and inflicted damage affecting the constituents of more senators and congressmen than in any preceding year of memory. There was, at worst, an open-mindedness, if not a sense of urgency, on the part of Congress to see that hurricane research was accelerated and well supported. In fact, the Weather Bureau, despite rejection of its requests from earlier years, was derided and made fun of for not having been more energetic in pushing hurricane research.

Be that as it may, the net result was a significant overall rise in the Weather Bureau budget in the succeeding years. I remember that in 1953, the annual budget of the Weather Bureau was \$27.5 million for all of its operations worldwide, including observations, forecasting, support services, and research. We got so much money for hurricane research in addition to funds appropriated in the same years for numerical weather prediction that by 1958, a scant five years later, the budget had more than doubled. In 1958, if I remember correctly, the total budget was more than \$58 million.

In late 1954, with Congress insisting on early action, we started re-working the proposals from earlier years, enlarged them in concept and funding requirements by a factor of three or four, and presented Congress what we thought was a proper response to the urgency they were assigning research on hurricanes. It was proposed to set up a three-year project to begin with, then re-evaluated to determine the need for a permanent laboratory in terms of the progress made during those three years.

Tooling up for the data collection program, which I insisted had to be launched in 1956 – that is, while “the iron was still hot” and sentiment in Congress remained favorable. This was a *major* task! But finalization of research plans and objectives was equally demanding. In a national program such as NHRP, it was essential to solicit and carefully consider, if not incorporate, the thinking and the suggestions from a broad spectrum of the scientific community, both U.S. and abroad. While no director had been selected for the project at that time, it fell to my lot to obtain and incorporate as many suggestions as I could from selected “experts” whom Dr. Reichelderfer invited individually to Washington for one-on-one discussion of plans. Included were Horace Byers, Herbert Riehl, Joanne Malkus, Noel LaSeur, Erik Palmen, Jacques Bjerknes, and Tor Bergeron, among others. Then later, after a Project Director had been named and key staff members appointed, visits were made to a number of universities, including MIT, the University of Chicago, and Florida State University, where the draft plans for the project were tried on for size with the faculties and students. I remember in particular the visit to MIT which did not then boast any particular expertise in tropical meteorology. However, Victor Starr, one of my old heroes of Chicago days, showed enormous interest in what we proposed to do and marshaled the full resources of the meteorology department, as well as that of aeronautical engineering from which a great deal of help came in helping decide which of the available aircraft would be most survivable, considering the flight plans and altitudes we proposed to fly, and the hurricane structural characteristics we anticipated encountering. It turned out that discussions at MIT resulted in more provocative questions and discussions than we encountered anywhere else, including the input we had from Weather Bureau scientists. During the visit at the University of Chicago, Herbert Riehl was out of the country, but we had most helpful input from Dr. Yeh (who returned to China shortly thereafter), and from Erik Palmen (visiting from Helsinki).

Gearing up for the data collection program involved a great deal more than deciding on probe and recorder systems for research aircraft. While we had asked Congress for our own research aircraft, the writing was on the wall, almost from the first, that we would have to negotiate with the Air Force and Navy for aircraft to support the Project. The reason was, primarily, that Dr. Reichelderfer, somewhat less than lukewarm about being saddled with his own fleet of research aircraft posing large funding outlays for operation and maintenance, did not push this aspect of the proposal before Congress. (And in retrospect, though I felt differently at the time, he was clearly right. The annual budget for operating three research aircraft, not to mention the capital costs charged to the WB inventory, would have over-powered the remainder of the WB budget and made it vulnerable to cuts that would have damaged both the research and counting operational programs.)

While both the Air Force and Navy recognized the need for intensive research on hurricanes, neither was eager to accept (unfunded) responsibility for supplying the aircraft demanded by the Project plan—except on their own quite restrictive terms. The Navy offered one WC-121 (Super-Constellation) to be operated from their base in Jacksonville, Florida, subject to availability when operational commitments permitted.

From our discussions at MIT, the Super-Connie was ruled out for the flight patterns (cloverleaf at three altitudes) that we had insisted were necessary. Nor was any other Navy aircraft available at that time suitable for our purposes. The Air Force, having just replaced their B-29 hurricane recon fleet with long range, stout B-50s and B-47s, was well equipped to do precisely what the Project needed.

General Thomas Moorman, at that time Commander of the Air Weather Service, and a fine meteorologist, had been an enthusiastic supporter of the “piggyback” research effort we had mounted out of Bermuda in the early 1950s and was personally anxious to see the new National Hurricane Research Project succeed. If it had been his choice, he would have provided the supporting aircraft without argument. However, the policy battle mounted at the Pentagon to resist congressional inferences—i.e., that the military could supply research aircraft support without adjustments in budget—kept him from acting unilaterally. He did, however, appoint his Operations Officer, then-Major Thomas Aldridge, to serve as liaison between the Air Force and Weather Bureau to work out something that would be mutually satisfactory. Tom (who subsequently retired as a Lieutenant General after a distinguished career beyond meteorology) proved to be a particularly adroit tactician in his negotiations, not with the Weather Bureau, but with the Pentagon, mainly through sensing the right time and place, as well as the right persons with whom to discuss options. My role was primarily a political one in “briefing” our more enthusiastic supporters in Congress of the progress made and of potential roadblocks being encountered.

Chief among these supporters was Senator Green of Rhode Island. It is probably fair to say the NHRP would have remained an earthbound program had it not been for the meticulous coordination of efforts between Tom Aldridge and myself, first in determining who could best help the cause within the Pentagon and when to approach them, and in turn, what information should be fed to Senator Green as to when, with whom, and in what way to apply pressures on the Pentagon for the support needed. The first concession made by the Pentagon was that the Air Force would directly support the Project from its base in Bermuda. As Aldridge knew in advance, this would be rejected, first because of the need, collaboration-wise, for maintaining a Project headquarters near the Hurricane Center in Miami, and, more importantly, as I had long since learned from my “piggyback” flights from Bermuda, when aircraft are under the direct overall control of a military base commander, they can be withdrawn from a primary mission, such as research, anytime he considers they are essential for support of his operational responsibilities. Finally, the Pentagon threw in the towel and agreed to supply a detachment of support personnel and operate two B-50s and one B-47 from West Palm Beach, Florida, explicitly dedicated to the support of NHRP. With this agreement, the headquarters of the Project was established at West Palm Beach Airport, and the Air Force promptly delivered the three aircraft for modification and new instrumentation. This included installation of probes and digital recorders, to be done at White Plains, NY, by General Precision Laboratories, a fortunate choice for the task. It was a credit to all concerned that these uniquely equipped research aircraft were ready for flight into the first hurricane of the season early in August, 1956. This may sound like a straightforward if not simple task. But it was not! To meet such a schedule as we had set ourselves – of

necessity a hurried effort to get going while the iron was hot – required not only dedication, but ingenuity and a willingness to cut ruthlessly through normal leisurely bureaucratic procedures and roadblocks, not only in government but among our contractors. But with the momentum generated by this time, project leaders on all hands were well motivated to get the jobs done, and the few who were not, were replaced (there were several!).

The final appointment of a director of NHRP was not made until late spring of 1955. While by default I had had to carry the ball in most of the planning tasks and inter-departmental negotiations, as well as the coordination with collaborating scientists, I was given no indication by either Reichelderfer or Wexler that I would be selected to direct the Project. I was determined to be a part of the Project and felt there was no doubt I would be. However, it was clear that Dr. Reichelderfer wanted, and indeed needed a big name scientist to head the Project, and I didn't qualify on that basis. A number of people interviewed for the job were unwilling to accept it on the terms offered; others, very eager for the appointment, were rejected because of questions, not of scientific competence, but of effectiveness in personal contracts outside of the Project, and personnel management problems from within. When I was finally named director, I felt it was essentially a default selection. I think Reichelderfer's greatest reluctance in accepting me was that he was unsuccessful in getting me to keep my elbows close enough to my sides. Once I was appointed, however, he backed me unstintingly throughout the entire Project.

One of the people quite anxious to head this project was Herbert Riehl, without question the most notable tropical meteorologist of the time. While Dr. Reichelderfer recognized that Dr. Riehl had scientific credentials for the job, he was concerned whether Riehl, as a line manager, would be able to interface effectively with such diverse groups as he would have to deal with persuasively, not only on the scientific front, but with political figures, engineering groups, the military, contract support groups, and others across the nation upon whom the success of the Project would depend. While I didn't know at the time what was happening in the recruitment effort, I learned later that Riehl had been offered a number of options for playing a leading role in the project, but not that of director. He rejected them all. His failure to be appointed director was unquestionably a bitter disappointment—understandably so. Unfortunately, his bitterness led to his refusal to have anything to do with the Project during its formative months, both in Washington and later at West Palm Beach, despite my entreaties for him to join us. It was not until Erik Palmen and Joanne Malkus visited the Project at Palm Beach and became enthusiastic about it, that Riehl was persuaded (by Palmen) to associate himself with the Project. As it turned out, this was probably the most important single event contributing to the scientific success of the project, one which saw Riehl participating in a number of hurricane flights, and ultimately led to the notable Riehl-Malkus collaboration on hurricane energetics and other aspects of the hurricane problem, leading the way for perhaps the most important hurricane research contributions to come out of NHRP. Aside from the scientific significance of this turn of events, for me personally it was the beginning of a lifelong, close friendship with Riehl, and as fate would have it, the beginning of my scientific association and collaboration with Joanne

Malkus, which melded into a personal relationship culminating in our marriage January, 1965, and the beginning of a long, happy and fruitful life together.

Ironically, once we were married and Joanne had moved from UCLA to Washington to form a new research branch for the Weather Service, our direct official collaboration in research was brought to a halt by bureaucratic nepotism regulations. That didn't keep us from working together unofficially, however, or from traveling together to participate in various scientific programs across the world in the years that followed.

I don't know whether you would prefer to talk now about the personnel involved, or pursue further some of the political aspects of the Project. It was indeed politically impacted from its start. In fact, it was almost shot down at the end of the first year—the iron didn't remain hot very long!

Zipser: I remember that first year was a terrible year for hurricanes. That is, there weren't many, were there?

Simpson: No, there weren't. The first year we had one little hurricane that looked like it was going to go right across the Florida peninsula. Indeed, Gordon Dunn, who had been in charge of the Hurricane Center at Miami just one year, had forecast landfall just south of the Palm Beach area; however, as it reached the central Bahamas, it recurved, and the mainland never got more than 25 knots of wind. But we did get our first three reconnaissance flights out of it. None of the probe systems worked on the first, which was my flight, but on the very next one, directed by Noel LaSeur, everything seemed to work except the recording of longitude and IBM card count (we were recording digitally in terms of shaft position for each probe output, and recording values each second on IBM punch cards). When LaSeur's flight returned, there were grins from ear to ear as the crew clamored to the tarmac flushed with success of the mission. But as the punch card operator, with four boxes of punch card records from the flight stepped out of the plane, he missed the last step, dropped the boxes, and the cards flew everywhere under the wash of the probes which were still turning. It took about three years of graduate student sleuthing to restore those cards to proper order and obtain a printout of data suitable for research analysis.

Zipser: Good thing the senators didn't see that.

Simpson: Yes, indeed! Before I got started on that little anecdote, I mentioned something about political impacts on the Project and its survival. As happens during nearly every administration, a year comes along when economic necessity places every agency's budget in jeopardy, and it becomes a cat-and-mouse game as to which line items are to suffer. Since nothing earth-shaking was accomplished the first year of NHRP, a number of people not only in the Weather Bureau but also at the Department of Commerce and the Bureau of the Budget (now the OMB) looked longingly at the large amount of resources devoted to hurricane research as an attractive target for economies, and serve as a cushion for longer term programs which otherwise would suffer. Rumors were rampant that NHRP was on the chopping block to lose the biggest part of its research funding. Dr.

Reichelderfer was concerned about it as were others in the Central Office who saw to it I was kept advised. Others in the Central Office felt entirely too much money was being spent on hurricane research while the rest of the research division was losing ground. So we had both internal and external pressures acting to cut NHRP off at its roots. Something had to be done or we were going to lose the Project, or have it emasculated to the point of ineffectiveness. With this in mind, more than a review of NHRP progress during its first year, I decided to call on short notice the First National Hurricane Research conference, not an AMS-sponsored meeting, but one sponsored by the Weather Bureau at Palm Beach. We invited first the lay users—people who would benefit most from the research results such as the vice president for safety at Dow Chemical in Freeport, Texas, and key figures from offshore oil drilling programs in Texas and Louisiana; and second, those from universities and scientific agencies interested and concerned about HARP's plans and programs. During the three days of meetings at Palm Beach, I was taken aside by the vice president of Dow Chemical, who commented, "I am absolutely delighted to see NHRP got off the ground. However, I know along the way you are going to run into political problems of one kind or another. If I can ever help out, let me know." I replied, "As a matter of fact, we are having problems right now. Our problems, however, are with the White House not with the Congress. The Congress by and large, I think, will back us. But there is no way that I know of to get to the White House." He said, "Well, I do. I happen to be the president of the lobbying group that includes not only Dow Chemical but Westinghouse, Monsanto, and General Electric and a few other large corporations. We don't deal with Congress, we deal with the White House. We are meeting two weeks from today in Washington, and I am going to take this thing up and see what can be done." He made good on his promise, and the Bureau of the Budget made a 180-degree turn; we got continued support, somewhat larger than actually expected.

Dr. Reichelderfer never found out what I'd done. If he had, he probably would have had my scalp, because that kind of political activity was a no-no for employees not in policy positions. But I took the risk and decided that it was worth putting my neck on the line for a program that was doomed without such action. Fortunately, luck has favored me throughout most of my career, and stood by me in this instance from which I believe all have benefited.

Zipser: That's a great story, Bob. Backing up just half a step. I've been in a lot of scientific conferences, although probably never one with this kind of an industry group sitting about with those kinds of stakes; did you have to do any prompting of scientists who were to speak at this conference, or did you basically call everybody together and just let the chips fall where they may?

Simpson: No, I didn't just let the chips fall where they would. I very carefully controlled who was going to speak, and when and in what order they were to speak. People were selected who I knew would make the right point at the right time. In fact, I discussed with each very carefully how we would orchestrate the sessions. Most of the presentations were, of course, by the staff of the National Hurricane Research Project. But we had people from various universities who made comments on the presentations and suggestions and who

spoke about the importance of continuing the Project. So indeed that conference was orchestrated from a political point of view as much as from a scientific point of view, and the press coverage was quite favorable.

Zipser: There are a lot of things I want to follow up on here. The very next year after that crisis was passed, I take it to be late '56, early '57, I'm sure you remember very well the disastrous Hurricane **Audrey** that hit Texas and Louisiana, especially the Louisiana coast: lots of loss of life, lots of operational lessons learned, I'm sure. I wonder what the impact of that disaster was on the Project.

Simpson: That *was* a memorable event—memorable because more than 400 lives were lost, and lost not because of prediction failure, but rather because of failure to communicate properly with coastal residents. The forecast, it turns out, was quite good and timely, as were the issuance of warnings. Unfortunately, the warnings and advisories issued by the Weather Bureau, normally distributed by radio broadcast from a station in Lake Charles, were edited by newly elected city officials who eliminated all but what they considered of importance to Lake Charles, forgetting that coastal residents needed the full text of the advisory to make their evacuation decisions. The result was most people residing on the coast gained the impression they needn't evacuate 'til morning. When they awoke in the middle of the night with floodwaters rising, all evacuation routes had been cut. While this led to a long and protracted lawsuit against the Weather Bureau (finally resolved in favor of the Bureau), it did not influence adversely the NHRP. In fact, it gave it visibility, which was quite beneficial.

Because of the importance of Audrey, I decided on short notice that the Project should be represented on the scene as quickly as possible. So we dispatched our logistics support plane with a small group of scientists who reached Lake Charles about 30 hours after landfall. Once there, I persuaded a military helicopter unit to transport us over the coastal region, survey high water marks and damage, and talk to residents about the reaction to warnings. This not only produced much useful information of scientific value, but also of value in reviewing and revising the structure and semantics of advisories and warnings. Moreover, it proved valuable in obtaining early estimates of the scope and maximum heights of the storm surge. Lee Harris, who had been appointed NHRP project leader for storm surge investigations and predictions, participated in the survey of Audrey and was especially impressed with the value of early surveys in studying storm surges. Harris and his successor, Chester Jelesnianski, went on to develop the excellent SLOSH model for predicting storm surge expectancies, a model still in operational use today. It turned out to be the first of many such helicopter survey trips to the disaster scene made by NHRP and later by NHC. In fact, I participated in surveys of every major hurricane that crossed the U.S. coast from 1957 to 1972, perhaps the most important of all being that of **Camille** where we were able to identify the place of max surge and calculate a record height of 26 feet, only one foot off the final surveyed value of 25 feet.

Zipser: There wasn't any question about your doing this in your position as head of the Research Project? One might have expected, as certainly you made a policy of in later years, for the operational forecasting responsibility to take over that public interface.

Simpson: Whatever the interest of others in these followup surveys after hurricane landfalls, someone from the Miami forecast office was with most of the survey parties. However, the initiative for the surveys was with the Project, later the Laboratory, possibly because of the close informal relationships I retained with the military upon whom we depended for helicopter transportation. When I became director of the National Hurricane Center in 1967, the survey policy was formalized and carried on thereafter by the operations group at Miami, although representatives of the research group were always included in the survey group. The surveys were continued by Neil Frank, my successor at Miami, and got the same support that I had, although I don't think the surveys were attempted as soon after landfall as in earlier years.

Zipser: Okay, we've covered the logistics support part, and how that got started with the post-storm surveys. I'm interested in the processes by which annual meetings with officials got started, and obviously it seemed to me, the forecasters on the hurricane desk actually changed the structure and language of advisories to provide more specific kinds of warnings – when the center would cross the coast, when dangerous winds and tides would start to affect the coast, the whole thing started after Audrey, I think.

Simpson: To a considerable extent that is correct. After that great disaster, a joint effort was launched between the National Hurricane Center under Gordon Dunn, and the National Hurricane Project, to try to get into the areas most seriously affected by a hurricane and talk to the local warning and evacuation officials as well as local residents who responded to the warning and those who didn't. There were also representatives from Washington headquarters who participated, collating the information obtained from various sources, letting contracts with social science research organizations, in some instances, who made recommendations for semantic changes in advisory language. During the decade that followed Audrey, some quite effective changes occurred in warning procedures and advisory language, as well as a common understanding of how the warning and evacuation program was to be organized and conducted in each state—a little different in each state because of variations in assignment of responsibilities for disaster preparedness and relief. During this period the concept of a *Hurricane Watch* came into being and became a major part of the entire severe storm warning system, including tornadoes and winter storms. There followed programs for development of evacuation procedures and readiness programs for all coastal counties, reviewed and amended following the hurricane coordination conferences each year.

Zipser: I may possibly be jumping ahead here, but the obvious issue here is a broadening of the responsibilities of the Hurricane Warning Service to go beyond the meteorologically-correct forecast to initiating a set of actions in cooperation with local officials that would result in evacuations if necessary, whatever the proper civil defense response would be. The question I am asking is, how did all that happen? Was this a personal crusade of yours basically, that you had to fight all the way, and at what point did this become policy rather than Bob Simpson's initiative?

Simpson: The collaboration with authorities responsible for hurricane preparedness, evacuation and relief consisted of informal arrangements initiated by NHRP at the outset, subsequently taken over by NHC, and gradually formalized through annual hurricane review conferences arranged by Washington headquarters, one a technical review usually held in November (the first one at NHRP in West Palm Beach); the other in January attended by all local and federal government agencies concerned with the hurricane problem and its aftermath. At these conferences formal agreements were reached on procedures for communication of information and coordination of hurricane-related sections. I suppose I served as a catalyst for bringing together many different groups initially and for proposing some of the mechanisms that resulted in an effective team effort in the absence of specific regulations and assignments of authority. But the success and agencies convinced me of the need to work in close coordination with each other.

Zipser: The next thing I would like to take up on is your return to the University of Chicago to complete work on your Ph.D. That was a remarkably short time after getting started at NHRP. Anything you want to say in this direction; did you feel comfortable leaving NHRP after only three years? Why this timing? And then I want to get into your years after Henry Wexler's deputy, the start of NSSL, and so forth.

Simpson: It became quite clear to me in conducting the planning for NHRP, something I had often discussed with Dr. Reichelderfer, that anyone involved in leading a research program simply had to have a Ph.D. degree if he were to be effective in his relationships with visiting scientists and collaborators across the world. Aside from the educational advantages, these credentials were essential to his credibility as a leader. With some people it didn't make a lot of difference, but to all too many others, it unquestionably did. From my experiences I convinced myself and later Dr. Reichelderfer that effective collaboration between the university research groups and the government researchers made it necessary that the Weather Bureau mount a program of completing the education of key research and operations leaders. I asked to be the first to be sponsored under the new program and Reichelderfer agreed. Later, when I returned to Washington as Harry Wexler's deputy, I was project leader in implementing this program, the first two returned to the university for this purpose being Cecil Gentry and Harry Hawkins of NHRP.

After the 1958 field program at NHRP, I felt that the Project, its objectives and procedures were sufficiently well-established that little would be lost by turning the day-to-day management over to the very capable assistant director, Cecil Gentry. There was no question where I wanted to complete my education. I wanted to work under Herb Riehl, who had invited me to return to Chicago and let him sponsor my Ph.D. work. Dr. Reichelderfer agreed, and I happily arrived in Chicago the first of January, 1959. I stayed there through June, 1960, completing the course work and getting my prospectus accepted. The writing of the dissertation which was to be done "on the job" at the Weather Bureau, was completed in early 1962. It's possible, though I doubt it, that I might have felt a little different about returning to finish this degree had my alma mater, Southwestern University at Georgetown, Texas, decided a little earlier to award me an Honorary Doctor of Science, which they did in 1963 after I had completed my Ph.D.

When I left Chicago in 1960, rather than being returned to NHRP, I was assigned to Washington as Deputy Directory of what was then the Research Division, headed by Harry Wexler.

Zipser: Let's talk a little now about the establishment of the Severe Storms Laboratory. In our earlier conversations, you indicated that there were some interesting events leading up to the opening of this laboratory at Norman, Oklahoma.

Simpson: There were. My old friend, C.F. VanThullenar (Mr. Van to most of his peers), the Regional Director at Kansas City, an accomplished forecaster and much respected operational meteorologist, had become quite interested and involved in local severe storm investigations, first during his tenure as regional director, and later as first director of a unit created to conduct local severe storm research. It was a small group based in Kansas City, which deployed to Norman, Oklahoma, each spring to carry out reconnaissance of squall lines and severe local storms, hopefully to obtain data from tornado events. VanThullenar's interests extended beyond the setting for data from local severe storms to the physical meteorological aspects of cumulus convection, local severe storms to the physical meteorological aspects of cumulus convection, thunderstorms in particular. He was especially interested in super-cooled water in thunderstorms, and got the Air Force interested in flying highly armored fighter aircraft through thunderstorm tops—an operation known as “Rough-Rider” – to measure total water content in cloud tops, as inferred from infrared hygrometer measurement of mixing ratio from air ingested into the jet engine and bled from the compressor through the hygrometer. VanThullenar also contracted with a fellow by the name of Jim Cook to fly a modified WWII B-25, in search of incipient tornadoes, to chase, and photograph them, and hopefully record some meteorological data from their environment as well as from that of more severe squall lines.

To make a long story short, we were getting a whale of a lot of data from probe systems. The analysis of these data was slow and incomplete, and data were not made available to, nor was there participation or collaboration with, university scientists. From the scientific community at large, there was uneasiness expressed about the objectives and justification, as well as the quality and amount of scientific output from the Project. And, from the policy point of view, the main question was where were we headed. The Project, which had an enthusiastic hard-working staff, a few of whom had excellent educational backgrounds, needed a focal point, someone well-grounded in meso- and convective-scale meteorology, who would bring coherence to the basic research objectives of this program.

It became my unhappy lot to research a policy decision on what to do. The upshot was a decision to establish a comprehensive severe storms research program with year-round headquarters at Norman, Oklahoma, whose activities would be centered more on radar development and data acquisition than aircraft programs, research objectives being divided between analytical studies of radar data and theoretical studies aimed at a better

understanding of the generation and growth of severe squall lines and tornadoes. Clearly, the greatest need of all was strong scientific leadership.

Finally, after a nationwide search and advice from many quarters, I decided the person best suited to bring the vitality and vigor to such a program was Ed Kessler, who at that time was working for Bob White in the Travelers' Weather Center of The Travelers' Insurance Company. Ed agreed to come, although it was not easy for him to convince his wife to move from New England to Oklahoma. But he did, and became first director of the re-organized National Severe Storms Laboratory at Norman. This was a crushing blow to VanThullenar who was given the option of staying with a small group at Kansas City and pursuing things that were nearest to his heart, or moving on to Norman and working as a co-director with Ed Kessler. He chose to stay at Kansas City and shortly thereafter retired. Kessler was given wide latitude and support to refine and enlarge the mission of NSSP and to recruit the best people he could find who would respond to his leadership as needed to produce a laboratory of recognized stature.

Zipser: How did you first come to know Ed Kessler?

Simpson: Ed was a student at Corpus Christi High School when I was band director there in the late 1930s. Those were the days when, as a masters graduate in physics, the only job I could find to support myself and family was in music—the means by which I had supported myself in the college years. While Ed's family and my parents were well known to each other, I rarely had much contact with Ed in those years, mainly because of the difference in our ages.

Zipser: And I had always thought Ed was a New Englander. I assume that Ed has some other talents that came to your attention besides your acquaintance in Corpus Christi.

Simpson: Of course, Ed was well known to me professionally, but long after that early acquaintance, mainly by his outstanding work in radar and cloud micro-physics, but especially for his work in radar analysis of hurricanes. He was doing outstanding work on this sub-synoptic scale motions within the hurricane vortex as viewed by radar, especially on Hurricane Edna, not to mention the other work he did in collaboration with Dave Atlas, and later under Bob White at Travelers' Research. To me, he appeared to be a dynamic, promising, young scientific leader.

Zipser: I knew that, Bob. I just wanted to get it on tape to make sure someone listening to this would not assume that his only qualification was where he came from.

There are so many choices that need to be made in order to set up an institute like that. I wonder what some of the major issues were that crossed your mind as the institutions were being created: relationships with universities, for example, how much in-house research, any university support. The Weather Bureau right on down into NOAA today doesn't have a real great record with university support. What about other policy issues in setting up these institutions?

Simpson: As regards establishment of NSSL, as was the case with NHRP, my concerns first were that the research be done with a specific mission orientation, not applied research per se, but as basic research whose mission is to support over the long haul our service objectives; secondly, that it be done in a manner that cultivates partnerships and encourages participation from the scientific community at large, especially by university scientists, including the awarding of grants for this purpose. I felt that NHRP was a good example of this. Some have questioned whether NSSL was as effective in this regard; but that was the goal we set, and I give Ed Kessler credit for his effort toward this end, although under progressively greater headquarters policy constraints than we had in NHRP. The other goal which I always held out was to keep the research laboratories in close adjacency with related operational units. We succeeded in doing this in 1958 at Miami. Moving NSSL away from Kansas City seemed to be a rejection of that concept, but it wasn't - only a compromise of the moment required first for logistical reasons, and secondly because of a personnel management problem, a compromise mitigated by proximity to, and close working relations with, the University of Oklahoma.

In my earlier years, working as an assistant to Little and Reichelderfer, my travels to forecast centers across the country convinced me that the Weather Service should promote a policy of relocating forecast centers on college campuses every time an opportunity presented itself, not because of the academic atmosphere, but as a means of establishing a mutual awareness of mission-oriented needs for new knowledge, and for early incorporation of research results into operations. The advantages of such a relationship had been well-documented at the University of Chicago during the years Gordon Dunn was in charge of our forecast office on that campus.

The original concept of providing a research scientist at each forecast office to keep the forecasters updated on the cutting edge of new science and to help develop methodology for improving forecasts simply had not worked. One of the reasons was these scientists themselves were isolated with little or no funds to maintain contact with universities and the scientific community, and secondly because of the constant pressures to reduce expenses in operational areas. Regional offices, running short on funds, would insist that vacant forecaster jobs be filled with a research scientist, thus vitiating the entire program concept.

So when I became Deputy Director of the Weather Service, Dr. Cressman and I agreed a better way would be to establish a scientific services branch at each regional office. This worked better in some regions than others, but did not succeed in many instances in cultivating partnerships or active collaboration with university scientists, something I have always thought essential for the robustness of scientific research and its impact on operational missions. Sadly, from my viewpoint, I have lived to see all these concepts rejected, not a single forecast office on a college campus anymore, research in NOAA entirely separated from operations, and less effort to entrain the university community than decades earlier.

Zipser: Let's return to NSSL, how it was set up, and some of the policy questions regarding in-house versus contract research.

Simpson: Initially, the Severe Storm Research Unit was located at Kansas City, and there was much more support, both locally and at headquarters, for keeping it adjacent to the major forecast center there. On the other hand, aside from the personnel management and scientific leadership problem already mentioned, there were a number of other factors arguing for the move. For a succession of years there had been an apparent shift of serious tornado occurrences southward with the perception growing that Oklahoma was the center of Tornado Alley. (The perception in my opinion grew more out of changes in reporting tornadoes, including the development of cooperative reporting systems than to any climatic shift or aberration.) This provided some political pressure to relocate both the forecast center and supporting research in Oklahoma. Of course, there were impelling reasons why the forecast office had to remain in Kansas City. But Oklahoma City was the logical place to headquarter any field programs that we might want to mount.

Zipser: Did it turn out to be a pretty big battle? Who were the main actors that were arguing on both sides?

Simpson: It was **not** a big battle at headquarters in Washington. At the policy level the central office agreed, both from a scientific and political point of view, that Oklahoma was the place for this laboratory. Also, for some there remained the nagging memory of the Texas “secession movement” a decade earlier following the Waco and San Angelo tornadoes: the Air Force was still devoting quite a bit of research on tornado prediction from Tinker Air Force Base in Oklahoma City.

Zipser: Was there a major political or scientific contingent that was arguing strongly for Kansas City? If so, who and why?

Simpson: Yes. Internally, the Kansas City regional office definitely wanted the research to stay in Kansas City. Don House, who was the head of the Severe Storms Forecast Center, understandably felt it was better to keep research in Kansas City. Don, however, was not adamant about it, and ultimately agreed with the location at Norman.

Zipser: After the decision was made, how did you plan to maintain contact between the researchers and the Severe Storms Forecast Center, which stayed in Kansas City?

Simpson: Mainly, I simply instructed Ed Kessler, and strongly exhorted the regional directors at Kansas City and Ft. Worth to see that procedures were set up to assure effective communications both for routine daily exchanges during periods of tornado vulnerability and for periodic technical conferences. Ed Kessler, working with the regional people in the Severe Storms Forecast group set up what I think was a fairly effective means of communicating and exchanging views.

Zipser: The next subject is quite a switch actually. During the years '61 to '64, as I well remember as a graduate student, Joanne Malkus (at the time) was busy seeding cumulus clouds in the Caribbean, and that evolved rather quickly into the hurricane seeding

initiative. Why don't you tell us the history of Project STORMFURY, of your involvement, and Joanne's involvement?

Simpson: Yes, that was an interesting era. However, as a matter of fact, Joanne's cloud seeding program was an outgrowth of the first systematic hurricane seeding mission in Hurricane Esther in 1961. The hurricane modification program became known as STORMFURY. Joanne Malkus was my principal scientific consultant in planning and conducting the STORMFURY program in 1961 to 1963, and became director of STORMFURY herself in 1964, at which time, with the arrival of Bob White, who replaced Reichelderfer, I was reassigned from the research division to operations as Deputy Director of the re-organized Weather Service.

The hurricane cloud seeding era actually began during NHRP when Joanne Malkus, Herbert Riehl, and Roscoe Braham were all involved in that project. Questions were raised at the time we were planning NHRP whether we could include in our objectives the modification of hurricanes by cloud seeding. The decision at the time was that while it was an interesting problem, we had much too little information with which to formulate a hypothesis, and that this should be deferred until we had better information to decide whether such experiments should be given precedence in the ongoing plans. During the 1957 hurricane season, Roscoe Braham of the University of Chicago had contracted with NHRP to conduct the cloud physics data acquisition on research flights as well as the follow-on research. Roscoe had been using an airborne silver iodide generator developed by the Australians (Taffy Bowen and Co. of CSIRO) to seed clouds in Missouri (Project White Top). While it was not a part of his original contract, he agreed, at my request, to install generators on one of our B-50s and determine whether it could be operated under hurricane flight conditions. (It had been used earlier only on light, slow-flying planes.) Ernie Neal was the principal field scientist for the work done by the University of Chicago, although Roscoe himself flew with us on a number of occasions including the ones in which the generator was tested. During Hurricane Daisy, on two of the three days it was reconnoitered, attempts were made to light the silver-iodide generator. We were successful only the last day of reconnaissance. Seeding was done for about 45 minutes mainly in clear air between rainbands of the hurricane, about 50 miles from the small center. As an instrumental test, not an experiment, nothing was published of the event, and because weather modification was such an emotional subject in those years, nothing was reported to the media, or, in fact, to other Project scientists including Riehl and Malkus.

Project STORMFURY was conceived following a flight made by Herbert Riehl in Hurricane Donna in a high-flying Navy jet in 1960. He had wanted to find out something about how the inner core of the hurricane and the eye looked from the outflow level. While the plane was not equipped with the usual meteorological probes, he made excellent sketches of the cloud system on each of two flights as the hurricane approached the Straits of Florida. He described to me afterwards the source of the outflow layer as a single cluster of convection which he referred to as a "chimney" cloud, located in the right front quadrant of the eyewall. This imposing feature I had seen only once before in an immature hurricane reconnoitered with the Air Force out of Bermuda. However, it had

not been observed or recorded in the research flights of NHRP previously, possibly because Project scientists had never flown in the high altitude research plane, a B-47 which had no observer position, to note the character of outflow cloudiness.

Riehl's observations, discussed only as a curiosity by most of the NHRP staff, stabbed my imagination and kept me awake at night. Was it possible the observations included a clue as to how a hurricane might be modified? Was there enough super-cooled liquid water in this chimney cloud? If seeded, would the latent heat of fusion be sufficient to affect the mass balance in surface layers and, if done strategically, cause the eyewall to expend outward? The upshot—much too long a story to relate here—was the research division in Washington agreed it was worth mounting some exploratory experiments. I requested Joanne Malkus to come in from UCLA and consult on plans for such experiments. The result was evolution of plans to make a first seeding attempt in 1961 if a proper hurricane came along, while other plans would be developed by Joanne for seeding individual cumulus clouds in the Caribbean, not only to determine responses to the seeding agents but also to learn whether the expected dynamic growth of the seeded clouds could be documented.

Meanwhile, a review of the time-lapse camera records from the high altitude aircraft of NHRP showed repeated evidence of heavy icing encountered as the aircraft encountered turbulent updrafts passing through the eyewall of hurricanes. Flight after flight showed that the electrically-heated window covering the wing-mounted time-lapse camera would freeze up and require many seconds to melt the ice accumulation after the flow again became laminar. It seemed that certainly there must be an awful lot of super-cooled liquid in the tops of hurricane clouds.

Zipser: Clearly the hypothesis and the scientific arguments are all a matter of record. I think what we want to go into here is the behind-the-scenes discussions, sequence of events, who was on what side, and so forth.

Simpson: One of the more significant sources of contention and argument, as well as important support affecting STORMFURY from its inception, centered around a precocious Navy scientist, Pierre St. Amand, a talented chemist stationed at the Navy's weapons test center at China Lake, California. After learning of the Bureau's plans to carry out experimental seeding of hurricanes in 1961, St. Amand came to Washington and offered to support the Project by supplying "his" newly designed pyrotechnic generators of silver iodide for cloud seeding, huge canisters he had christened "Cyclops," designed to fall slowly in a windmill fashion dispersing silver iodide smoke over a considerable volume surrounding its fall path. He also offered to obtain Navy planes to deliver and release the generators at the top of the hurricane, together with other planes for monitoring the results—almost too good to be true. But after careful inquiry in Navy circles, it turned out that he had the clout and probably the basic ability to deliver what he promised. And so tentatively an informal agreement was reached between his office and mine for this collaboration.

I should mentioned at this point that before the Navy entered the picture, the National Science Foundation, through Earl Droessler, had agreed to support the hurricane seeding

program aimed at reducing hurricane strength and provided a grant to the Weather Bureau, based upon the plans I had developed including the crude hypothesis as basis for the exploratory effort. Our first difficulty with St. Amand occurred when we learned that he had his own idea of where and how to seed, and that his objectives were to steer, not influence the strength of the hurricane. The controversy which followed, with Joanne Malkus supporting my position, reached the highest levels of the Navy. But the Naval Weather Service sided with the Weather Bureau, so that St. Amand was relegated to the task of supplying the pyrotechnics.

He was not, however, to be so easily shackled. He succeeded in throwing monkey wrenches into the works at every turn before the successful mission in Hurricane Esther was brought off. The outcome was a replacement of the informal program with a formal agreement between the Navy and Weather Bureau, and the assignment of Captain Max Eaton to be the co-director with me for a multi-year project named STORMFURY. Max, an old friend of mine, a talented meteorologist and an outstanding leader, was a joy to work with, and the inter-agency cooperation went smoothly thereafter.

Joanne worked with me in developing plans for succeeding year experiments including a more precise formulation of the original hypothesis and the operations plan for the next hurricane mission. She also developed plans for single cloud seeding experiments, which were conducted in the Caribbean in 1962 and in 1963 before the second STORMFURY experiment in Hurricane Beulah in 1963. In the Beulah mission we flew together and jointly directed the seeding. The remainder of this story is fairly well documented in the literature, except to personal aspects of which led to Joanne's and my marriage in 1965.

Zipser: We want to continue to talk about some of the scientific controversy which, even as a graduate student, I remember being hotly involved in informally. I would be most interested if we could proceed with what happened in any order you want, Bob; what happened and the main nature of the scientific and decision-making controversies that surrounded that project those years.

Simpson: Weather modification—always a controversial subject—was, of course, more so where hurricanes were involved. Of course, it was such a poignant story line for the media that the flames of controversy were continually fanned by the press and television. But first I should remark, interestingly enough, that Herbert Riehl, whose observations in Donna had given rise to the thinking which led to STORMFURY, never participated in any aspect of the project, indeed felt quite disaffected with it. In fact, this project probably foreshadowed the end of an era of brilliant collaboration by the Riehl-Malkus duo, though I doubt there was that intent either way.

Zipser: An interesting question to ask here is when Herb made this observation, was this simply a meteorological observation on his part or was Herb Riehl's observation also accompanied by a suggestion that this might be potentially important for seeding? Or was it someone else who followed up with the seeding hypothesis?

Simpson: I had many discussions with Herb Riehl about this that I can't remember exactly who initiated one part or another part of our conversations. But I can say with great confidence that his concern about this observation had nothing to do with weather modification. His concern was for what this portended with regard to the dynamics of hurricanes and their structure.

Zipser: If that's so, would it be fair to say that Herb basically was never a proponent of the seeding program?

Simpson: I think so; certainly, he was never enthusiastic about any aspect of weather modification, and it is my impression that included the classical work Joanne did in dynamic seeding and cloud mergers.

As for scientific controversies, Joanne and I presented dozens of papers at conference and published many others in the literature with few challenges to the basic concept and hypothesis until after her term as director of STORMFURY (1964-66) was over. When Cecil Gentry (somewhat reluctantly) became director, the controversy initially was whether it would be more effective to seed in the rainbands than in the eyewall. This was fed mainly by the modeling work of Stan Rosenthal's group at NHRP. Another concerned doubts that there was enough super-cooled liquid water in hurricanes to make a difference in any event. Unfortunately, that led to several not so pleasant skirmishes at seminars and less formal discussions between NHRP, NHC, and Joanne's cumulus group in the late 60's and early 70's, resulting in regrettable alienations between the organizational units.

Politically, the question of adverse effect of hurricane seeding arose as international issues on a number of occasions, Castro insisting that the U.S. was engaging in strategic warfare with Cuba in changing the course of some hurricanes to send them to Cuba. Mexico bitterly denounced the U.S. for causing a protracted drought "resulting from cloud seeding". The response to these complaints was interagency restrictions of area and of conditions in which seeding would be allowed, restrictions to such degree that little hope remained to demonstrate statistically that hurricanes could be usefully seeded.

Zipser: Max Eaton was also responsible for setting me up in Roosevelt Roads during the summer of '63. I remember him quite well, at least through Noel La Seur at that time.

Let me go back to the STORMFURY hypothesis because I think that is a pretty important part of meteorology history. Surely, there were some grounds for genuine scientific debate on the matter of the super-cooled water. Clearly, you had some evidence in favor of the major actors who disagreed, not terribly quantitative to be sure. Who were some of the major actors who disagreed with you on that, or at least probably would have demanded (I'm guessing now) more data on the distribution of super-cooled water in hurricane clouds?

Simpson: Beyond the internal NOAA disagreements mentioned above, I sensed a considerable amount of uneasiness in the scientific community about the super-cooled water resources.

Both Vincent Schaefer and Bernie Vonnegut of General Electric expressed skepticism about the amount of liquid water available, especially at temperatures colder than -40°C . Others challenged the observations of icing from the B-57. Roland List suggested the plane may well have encountered ice balls, rain drops carried swiftly upward, freezing the drop surface but not the liquid core so that when it struck the plane and splattered the inner core of liquid water on the wings of the plane, it gave the impression of direct icing. Countering this, however, was the analytical paper I published in the Wexler Memorial volume. Also there were independent evidences of liquid water at very cold temperatures, one in which Riehl and I, riding in a Navy jet at 45,000 ft., observed, near sunset, a glory (surrounding the plane's shadow) projected on growing convective cloud tops with temperatures colder than -50°C . And further, there was the evidence from Rough Rider data of mixing ratios far in excess of the adiabatic maximum in the tops of squall line clouds.

Of course, all of the questions raised and the uncertainties present were of concern both to Joanne and me. But in the early days we simply did not have the facilities for obtaining quantitative data needed for proof, other than that from the B-57 and B-47 flights through hurricanes, although I might add that both Joanne and I continued to find that evidence quite persuasive. And by the time we had the instruments to make quantitative measurement, we no longer had the high flying planes available to get the data. We did have the hard observational evidence of ample super-cooled water from single-cloud seeding in Joanne's experiments in Barbados and Puerto Rico where dynamic growth, computed on the assumption of ample super-cooled water, was verified observationally. Finally, to both Joanne and me, the evidence of insufficient super-cooled water during several flights of the P-3 research aircraft in the late '70s was questionable on a number of grounds and demonstrated at best that not all hurricanes along all flight paths have plentiful supplies of this resource. It does pose the interesting question, however, whether one can distinguish hurricanes or processes within hurricanes which restrict the supplies of super-cooled liquid water from which generate copious amounts.

A more recent development from modeling is a point of view advanced by, among others, Rick Anthes, which addresses the chicken-and-egg question of whether changes of strength in a hurricane are initiated by changes in mass circulation, or by changes in momentum transports. It is Rick's (and I think many other modelers') view that changes in hurricane strength occur mainly from environmental changes in momentum transport, that mass distributions change as a consequence and not the other way around, which in itself would pull the rug out from under the hypothesis.

Zipser: I guess we can't leave this subject without asking whether you changed your mind since STORMFURY days on overall philosophy of weather modification, why it fell into disfavor, whether you think it ought to be resurrected; if so, how and when?

Simpson: My view, from the very outset, was that whether the hypothesis was supported or denied, we would learn enough about hurricanes, their structure and thermodynamics from a carefully designed seeding project to be well worth the cost of the project. I still think that's true. Interestingly enough, I got that idea from Soviet Academician Federov

who said of the hail modification project in the Soviet Union, that it mattered not whether they would ever be able to systematically reduce hail. What would be learned, meteorologically, from the seeding project would pay off in other ways. He may have been right.

In any event I still think weather modification, if properly designed and properly carried out whether in hurricanes or individual cumulus clouds, can play an indispensable role in atmospheric research, and I am convinced that weather modification was too summarily dismissed from government sponsorship. Some of these days I am sure we will find ourselves re-designing programs. We will probably get caught designing desirable programs to return to weather modification research in the United States.

Zipser: This is undoubtedly something that goes beyond the purpose of these tapes. Probably we could have almost as many points of view as we've got eminent scientists in here to discuss that, and I'm sure we all recognize that. We should probably leave it at this point and go to possibly the last set of questions, though probably some general ones at the end, to give you a chance in an open-ended way to add whatever you would like.

I would like to ask two sets of questions probably related. One is, what your role has been in the history of the hurricane reconnaissance for research. I'm thinking in particular of how the DC-6s got into the picture in 1960 and how the P-3s got into the picture in the early '70s and then follow that up with whatever you want to say about your years as director of NHC.

Simpson: We have already discussed my role in flying with the Air Force Hurricane Hunters from Bermuda, and the flight into Typhoon Marge from Guam. These were as supernumerary on operational missions. But the objectives were essentially the same, though more restricted than those later carried out by NHRP with aircraft dedicated to research. They were to define and understand the circulation and thermal structure of hurricanes and the scale interactions between vortex and environment, while acquiring as much supplemental data on cloud microphysics as possible. I suppose my role, as such, after the first three years of NHRP at West Palm Beach was to see that the follow-on aircraft were equipped with the most sophisticated instrumentation and recorder systems that were available.

When the Air Force advised it would not be able to support NHRP with aircraft after the 1958 season, the question was whether there was justification scientifically for acquiring another research flight facility? Why not stop and just do research on the data we obtained with the Air Force planes over the first three years. After all, the sense of urgency for hurricane research generated by the 1954 hurricanes had waned considerably, and to procure and instrument new aircraft or re-equip old ones would involve significant additional appropriations at a time when new appropriations were hard to come by. It was perhaps a greater and more difficult challenge by far than the original one in persuading the Air Force to support NHRP. This time we faced the original one in persuading the Air Force to support NHRP. This time we faced not only the external political problem of getting budgetary support, but significant disaffection with the project within the

scientific community. A polarization of opinions had developed between, first, those who perceived that the project as a sink for research funds without evidence in the literature at that juncture of useful results from the “mountains of data” acquired, and second, those enthusiastic supporters who had participated in one way or another in the data acquisition or had been gratified with the data as it applied to their research. Fortunately, the supporters were more articulate at the right times and places than the detractors.

But the latter advantage wasn't enough. We had to demonstrate, through a credible review of progress made, and a statement of vital questions left unanswered, that if the expenditures to date were to pay off, we simply had to continue taking advantage of new technology which had become available in the interim to continue data acquisitions. One of the questions which had reared its head regarded the computations of budgets based upon winds measured by Doppler systems, which suffered during turns of the aircraft and as the result of “the moving ocean platform” from which signals were reflected.

To bolster my arguments in making a case for continuance, I enlisted the strong voice and experience of Verner Suomi in evaluating the performance of earlier instrumentation, and recommending in a summary statement the new technology that should be used on the replacement aircraft. I also returned to MIT and got a re-evaluation of aircraft most suited for our purposes. Finally, I let a contract with General Precision Laboratories—the company which installed our probe and recorder systems on the Air Force planes—to generate specification for the new instrumentation system recommended by Vern to provide the basis for bid invitations.

With this in hand, I pulled out all stops in launching the campaign for continuing the data acquisition program of NHRP. This included recommendations for purchase of three new C-130 aircraft at \$4.5 million apiece, in those days, equipped with inertial navigation systems, a high speed solid state Ampex recorder system, and new meteorological and cloud physics probe systems using the very latest technology. The campaign, of course, began at the central office where opinion was somewhat divided; however, I got the nod to proceed in principle to determine the support from the scientific community. This was done by staging a series of conferences at Palm Beach to which we invited a good mix of scientific expertise from across the nation. The upshot was that sufficient support was generated to present a Weather Bureau supplemental appropriation request through the Department of Commerce far larger than any previous one from the Weather Bureau. I never really expected to get the three C-130s, but felt that when this request was rejected, not by Commerce but the Bureau of the Budget, the alternatives would reduce the budget so much that the remainder of the package would make it through unscathed. That is essentially what happened. We were fortunate to find an airline company, known then as Trans-Caribbean, that had just bought two brand new DC 6As which they had to sell quickly as a result of an opportunity to obtain new jet aircraft they originally thought they couldn't afford. We purchased the DC 6s for less than a half million apiece. The Air Force then came through again, agreeing to bail to the Weather Bureau a B-57 Canberra for high level flights, and a small logistics support aircraft.

All that remained was to let a bid for the modification of the aircraft. We held a bid invitation conference at Palm Beach in which the specs were presented and all options explained. With all this behind me, I shoved off for Chicago to complete my Ph.D., feeling assured that General Precision would be awarded the contract and all would be well. Unfortunately, that wasn't the way it worked out, although I was necessarily out of the loop for the follow-up and unaware of what was happening.

Of the six contractors whose bids were validated, General Precision's bid was competitive with all but one, a Chicago firm with large DOD contracts whose bid was less than half that of the next lowest bidder—an omen in itself that something hokey was going on. And here administration politics struck a blow for mediocrity. The legal staff at Commerce, reviewing the bids and the Weather Bureau's recommendation not to accept the lowest bidder, not only required that the lowest bid be accepted but, without consulting the Bureau, wrote the contract to provide a cost-plus-fixed fee payment schedule. After six months at work, the Chicago company had spent two-thirds of our budget for instrumentation and wasn't nearly half finished.

About that time I received a call in Chicago from Russ Grubb, the head of the Bureau's budget office, asking if I had any suggestions regarding the dilemma. After further discussions with Reichelderfer, I was authorized to assemble an independent team to investigate the problem and recommend action. I then persuaded Horace Byers and Roscoe Braham of the University of Chicago, and Vern Suomi from the University of Wisconsin—all three veterans of successful field programs—to join me in the investigation. The result was a recommendation that the Chicago company be defaulted and a new sole source contract be negotiated with ESS GEE company of Tarrytown, New York to complete the job. This latter firm was staffed by the General Precision engineers who had developed the specs originally and had subsequently split off to form their own company. The recommendation was accepted, and Vern Suomi agreed that he and a colleague, Tom Parsons, would monitor the contract. Fortunately, the overhead of ESS GEE was only a fourth that of the Chicago firm so that job was finished and accepted essentially within the original budget. Incidentally, as an outcome of the default, the Chicago firm was barred from defense contracts for three years, and the company's president and two other senior officers were fired.

When I returned to Washington as Deputy Director of Research under Wexler in 1960, the research aircraft, previously operated and managed as a branch of NHRP, was re-organized under separate management and known as the Research Flight Facility, mainly because of the need for year round use of the aircraft for broader research support than reconnaissance of hurricanes. This unit reported to my office and was quickly drawn into a variety of research programs including local severe storm and cloud physics investigations. Their first international program was in India where, in 1963, they participated in the Indian Ocean Expedition.

Zipser: To what was the decision to go with the P-3s influenced by either Project STORMFURY for the famous visit with Spiro Agnew after Hurricane Camille?

Simpson: I'm not sure I can say. As I recall, we were already on our way toward the procurement of new aircraft in 1967, but the Camille survey incident, after "the feathers put in the air" by the Agnew incident began to settle again, undoubtedly was a catalyst for their procurement, though not as a direct result. The DC-6s by then were on their last legs and had to be replaced. It helped that the Navy was already in process of converting their fleet of Connies and were singing the praises of the P3s. The immediate impact of my report to Agnew during the Camille damage survey was on the Air Force and Navy to upgrade their capabilities. The infamous "interview with Agnew" was, in fact, one of five or six assessments made by those involved in the preparedness and rescue efforts during Camille. Mine was a five-minute statement which briefly reviewed the difficulties determining whether Camille was indeed the threat it turned out to be. I knew all too well the Navy's frustration in trying to replace the old C-121s and P-3s and the Air Force's unsuccessful efforts to obtain recorder systems for their C-130s, so I didn't pass up the opportunity to refer to these needs. But I had no idea how much trouble I was making for Bob White, NOAA's administrator.

Actually the incident was a happenstance. I traveled from NHC in Miami to the Camille damage scene a scant 24 hours after the landfall and had spent two days surveying the damage in the Air Force helicopter and talking with survivors. I found myself at New Orleans Airport boarding a plane to return to Miami when I was handed an urgent message to call Washington. When I did, someone in Bob White's office told me to return to Biloxi and accompany Vice President Agnew on a survey of the damage area. Five minutes later I would have been on the way to Miami, there would have been five, not six reports given Agnew, and goodness only knows what the outcome would have been as regards hurricane reconnaissance facilities. Nevertheless, I joined Agnew on Air Force One and pointed out for him the most significant evidence of damage from flooding and from wind which we had surveyed earlier. Back at Biloxi, Agnew asked for summary statements from the emergency warning people, disaster relief officials, the Red Cross, and the NHC. In my statement I recalled the compounding of uncertainties when Camille was rapidly becoming a record storm when the satellite analysis group in Suitland had insisted that the storm was losing strength, while the NHC forecasters concluded that it was strengthening. And without adequate reconnaissance, we couldn't be certain how loud to ring the bell for evacuation.

Agnew wanted to know what the problem was in obtaining the kind of recon data we needed to verify the strength of the hurricane. My response was what lit the fire which enveloped both NOAA and DOD and, I was told, almost got Bob White fired. I said both the Air Force and the Navy had been trying to get a better instrument and recorder systems aboard their planes, and the Navy had been trying for years to replace the antiquated super Connies whose performance in severe hurricanes was questionable. I continued, stating that every effort in the last three budget cycles was frustrated because the Administration turned down the budget requests for these much needed facilities. This placed the forecasters and the warning systems in great jeopardy on this occasion. Then, on sudden impulse, I lit the fuse, adding that, personally, as a citizen, I felt it was high time somebody got on the ball and saw that this problem got proper attention in the right places in Washington.

Zipser: There wasn't anything wrong with it as far as I'm concerned.

Simpson: Perhaps not; but I knew I was taking a great risk. I had been around long enough to know I was making statements a civil servant had no business making if he valued his career. But I saw the opportunity and decided in a split second to make this risky personal comment. The acutely painful result was that Agnew flew back to Washington, turned right around and flew to the "little White House" at San Clemente and briefed Nixon. Nixon, in turn, called Bob White to California along with the responsible officers in the Air Force and Navy. They all got thoroughly chewed out. Then, in turn, I got promptly called to Washington and was chewed out by Bob White. I believe Bob really thought his job and career were placed in tremendous jeopardy, and it probably was, though not intentionally. But it shows how things work within the bureaucracy. This time, fortunately, it all turned out well. No one got fired or lost a career, the Navy got their P3s, the Air Force got millions for new instrument systems, and from all the momentum generated, NOAA was able to improve its own research aircraft facilities.

While, of course, it wasn't discussed at this briefing, and if recorded in detail anywhere I am not aware of it, we had one of the most grueling experiences of our lives at NHC on this occasion, perhaps worth mentioning here as an epilogue.

The satellite people in Washington had been given quite a bit of responsibility for making independent judgments in issuing hurricane warnings for international areas outside NHC responsibilities. So a procedure had been established for coordination between NHC and Suitland. On this occasion we had had some rather lively arguments on two successive advisory cycles about changes of intensity in Camille as it moved into the Gulf of Mexico. The staff in Miami, both at NHRL and NHC, were of one mind in that the storm had vastly strengthened and that the shrinking of the central dense overcast (CDO) was the result of sinking of air transported up in the eye wall in such abundance it could not be carried away from the core rapidly enough. Finally, I decided to stop arguing and stated categorically in the next advisory that Camille was becoming a very severe hurricane. But we were strapped for recon aircraft to verify our analytical conclusions, while the satellite people continued to snipe at us.

You will recall that on this occasion there were two hurricanes in progress, the other being Debbie, which was the subject of a STORMFURY experiment. Nearly all the Navy aircraft had deployed to Puerto Rico for this exercise, only two remaining at Jacksonville. The first Navy plane sent to the area to penetrate Camille turned back with engine trouble. The second got off several hours later, but in approaching Camille decided it was too severe to penetrate and turned back without getting any quantitative information from the storm core. To complicate matters further, the only Air Force recon planes available at that time were on the West Coast. With a crisis at hand, I called Scott Air Force Base, talked to the Air Weather Service Commander personally, and asked what could be done to get a reconnaissance plane into Camille. He replied that he would have to fly one from the West Coast but would do the best he could. So a plane with two crews was flown to Tampa where it was refueled and dispatched immediately in Camille, using the alternate

crew. When they arrived, they reported a central pressure of 901mb (later corrected to 905). Our worst fears were therewith confirmed, and we pulled out all stops to make it known that the worst storm of record was descending on the central Gulf coast.

Zipser: It makes you kind of ill how fast the Air Force is trying to get out of the reconnaissance now.

Simpson: Yes, that's another story.

Zipser: Another story, you may not want to go into it now, but if there had been another director at NHC besides the one who founded Project STORMFURY, do you suppose those planes would have been sitting ready to go into Debbie instead of sitting there ready to reconnoiter Camille?

Simpson: I guess that doesn't really require an answer.

Zipser: Bob, I think we ought to wrap this up, probably. We could enjoy this for many more hours. But I have a set of short questions which you may want to think about and give quick answers to, without dwelling on them, before we quit here.

Did you every think of going into pure research?

Simpson: No. Not research for research purposes. I never was interested in research except as it applied to and would potentially have an impact on operations or service programs. That is not to say that my interests were purely in applied research. Far from it. I was far more interested in basic research with a mission orientation that I was in applied research.

Zipser: On the other side of the same coin, did you ever get an offer or funding to work directly in industrial meteorology?

Simpson: I never sought or pursued at any time the possibility of getting out of meteorology or out of the government, with one exception. Back in 1952, when the private practice of meteorology was given great support by Dr. Reichelderfer, I had an uncle, a well-known Texas banker, protégé of Jesse Jones, and at that time president of the First National Bank of Commerce in Houston. He called me and said, "Bob, I think the time has come when Houston needs a more specialized weather service than we are getting from the Weather Bureau. If you will come down here and set up a private practice in meteorology, I will underwrite your initial and continuing costs for three years. Your business should be profitable by that time." That was a shocker to me. First, I had never once contemplated such a thing. Second, I was just getting steamed up about the work I was doing for Dr. Reichelderfer and Del Little, all of which was giving me exciting new ideas about how to create a better Weather Bureau. I told my uncle I'd think about it and get back to him within a week to ten days. I talked with a lot of people who were going into private meteorology at that time, one of whom was A.H. Glenn, who was in business in New Orleans at the time. Finally, I decided against the venture. Although I never doubted I could become successful in private meteorology, I had invested 12 years in

government and looked forward to a career there. So at the end of ten days, I called my uncle back and said thanks but no thanks. After that point I never entertained any idea of leaving the Weather Service or meteorology until I retired after 34 years of government service. Later I established Simpson Weather Associated which eventually got into quite a variety of consulting matters including coastal zone management, alternate energy, experiment design and validation and such things as that, in which my university colleagues Mike Garstang, Dave Emmitt, and others helped, first on an ad hoc basis and later took over and ran the company's operations.

Zipser: Was there anything we left out or something of interest that you want to add to wrap this up?

Simpson: We could go on at length with my experiences as director of NHC, but perhaps it is sufficient to summarize a few not well known or recorded events incident to this assignment. It was the only job for which I ever had to apply and campaign hard. It required that I step down a grade from a key policy position in Washington to a line management job in the field, something that is regarded in Washington as "socially" unacceptable. But I felt I had spent my effectiveness in Washington milieu and needed a change.

From my earliest days in the central office I was impressed with how ineffective a project leader in Washington became after spending more than four or five years in any one job in the Bureau. I kept that resolve throughout my career, and I think our bureaucracy would be better off if others did the same.

However, when Gordon Dunn retired and I told George Cressman my desires, he objected; he felt I should remain in Washington. But I went over George's head to Bob White, who was sympathetic, and eventually ended up arranging not only for my transfer there, but for the transfer of Joanne and her entire branch to Miami at the same time. I don't think either of us ever regretted the move. And I can't recall any other reassignment of personnel of such sweeping proportions primarily for the personal advantage of two employees.

Personally, I'm proud of what was accomplished at Miami while I was there, despite the (perhaps inevitable) conflicts, which arose with the regional office toward the end. During my tenure, we did not improve significantly on the mean error in predicting hurricane movement; however, we did reduce substantially the standard error of movement and, along with it, the length of coastline warned during hurricanes. When I went to Miami, it was my primary objective, and in this I believe I succeeded, to bring to bear in hurricane forecasting what had been learned through NHRP of hurricane structure and energetics. This enabled the forecasters to diagnose and upon occasion to challenge the validity of computer model predictions of hurricane movement, and to make intelligent, scientifically informed choices of various prediction options. It was that type of diagnostic reasoning which led to the discussion that Camille was becoming a great hurricane and that critical moment when preparedness actions were most urgently

needed; reasoning which clearly justified our rejection of the opinion and insistence of the satellite analysis group that Camille was weakening.

In winding up this narrative, I sense that I haven't really touched on many more personal matters, and some significant associations with colleagues that I should have at least acknowledged, people who influenced my life and career, and impacted my zest for life as a meteorologist, only a few of whom I've mentioned. On the down side, my first marriage came to an end in 1948 principally because of my self-centered career interests when I finally accepted the assignment to Hawaii. On the other hand, my association with Joanne Malkus, who became Joanne Simpson, has meant more to me both professionally and personally than any other factor in my life. And I would like to think that perhaps it was a reciprocal benefit for us both. There were many things that Joanne did real well that I couldn't touch, and I believe some things that were easy for me and more difficult for her I was able to help with. In any event, our lives, both professionally and personally, and to some extent our respective achievements, have been uniquely buoyed by our decision to meld our two careers through marriage.

Zipser: Thank you very much, Bob. We will now close this interview.

END OF INTERVIEW