

CHAPTER 1

The Initial Meeting

“You’d better get your ass down here in a hurry, good buddy. Lettau’s been lookin’ for ya. He’s stopped in twice this morning.”

That was the big, calm, certain voice of Capt. Robert Fox, an Air Force student on leave to the University of Wisconsin at Madison. He was completing his Masters degree on optical RADAR detection of scattering layers in the atmosphere under the guidance of Prof. Stig A. Rossby. Bob and I shared a graduate research office in what we all affectionately called Bull Hall above a dentist’s office next to a flower shop on the corner of Park Street and University Avenue.



For all of my research and teaching years since meeting Bob Fox, I have been grateful for his guidance. In the research world, the most detrimental gift was time. A graduate student was given incredible quantities of time to search out and digest all the available literature on his or her project and then develop a question worthy of being answered. After receiving the degrees, the same life turned into a “publish or perish” world and the same question, “What should be discovered next?” remained, for me, a haunting one. Bob never appeared troubled. With the forward certainty of his military training he suggested putting all of one’s problems in a two-pocket folder marked “Problems Already Solved by Time” and “Problems That Will be Solved by Time.” It works!

Much time vanished struggling with the foreign languages needed to show that your scientific literature search truly covered the global market place of science. My language nemeses were Russian and French. Endless hours passed with data crunchers, the IBM 1620 and the CDC 1604. One could never master enough mathematics, the chief language of science, or the art of numerically approximating the unsolvable partial differential equations.

I was learning what undergraduate science never teaches, the artistry in science: what should be done with data, what shouldn’t be done, what couldn’t be done, and finally what could be gotten away with. All was part of beating the data, taken from nature, into submission to our understanding of science. Such strange pressures led some to drink, others to pursue beautiful women, and still others to sing. The spring of 1965 was for me an excellent time for all three. There was Schlitz and Southern Comfort, Rita and Judy and Marie, and the earnest songs of the civil rights marches.

Crashing through the fog of a deep sleep, the persistent ring of the telephone and the concern of a good buddy had aroused me at 11:00 A. M. What could Prof. Lettau want? He was my thesis adviser, and of course the first draft was long overdue. Some delay was not entirely my fault. I was convinced that my project was exceptionally difficult, and I sought exceedingly complex answers requiring the coordination of many people across great distances.

Some of this was true. The project I started with was supported by the National Institute of Health. It sought to understand atmospheric turbulence over a city using wind and temperature data measured from instruments mounted at many levels on a 720 foot television tower, KSTP-TV, on the boundary of Minneapolis and St. Paul in Minnesota. Dr. Raymond J. Deland of Australia began the

research, but by this spring of 1965, in the mobile world of academia, he had moved to New York University. Dr. Harlord Paulis and Dr. Alton Hollenbeck of the Department of Public Health of the University of Minnesota were also involved. With the departure of Dr. Deland, my master's work took a year setback. I had greatly appreciated the mathematical insights on global circulation that Dr. Deland had given. I truly missed him.

Now Prof. Heinz H. Lettau, a full professor in both the Department of Civil Engineering and the Department of Meteorology, consented to permit my research to continue under his direction. The prestige, the wisdom, the dedication that Lettau gave his students turned out for me a great gain. Lettau was the world's foremost authority on micrometeorology. He was an initiator and co-director of ~~the Great Plains Turbulence Field Program and co-author of~~ *The Atmosphere's First Mile*. He authored over one hundred scientific contributions written in either German or English. He invented the idea of the stability length-scale in boundary layer dynamics that permitted methods of comparisons and groupings of data of the atmosphere made of many interdependent variables continually changing.

His pioneer work in turbulent transfer, climatology, and microscale surface modification gave his students a world-class understanding of the complexities of nature. He showed his students the artistic freedom the scientist had in constructing a mathematical model of nature and insisted on constraints of first principles given generally by nature. I remember Lettau carefully explaining that ten new mathematical models of atmospheric turbulence were generated by scientists every year. Most of them were not worth anything. The mathematical model or the part of it that was worthy of continued study and closest to nature was the algorithm built on the first principles given by nature, such as conservation of energy, turbulent eddies generated from laminar smooth flow and the expected averages and fluctuations from those averages. Methods had to be developed to find the natural varying length-scales within nature as opposed to arbitrary rigid standardized measurements based on human demands.

Why was Lettau so earnestly pursuing me? I knew I was behind submitting the first draft of my Master's thesis. Perhaps he was getting impatient. Maybe he was checking on the utilization of office space by his graduate students, and now I was caught sleeping in. At least this day I had an excuse. I had worked all night from 2:00 A. M. to 6:30 A. M. at the Earth and Space Laboratory Computing Center dividing nearly 300,000 observational items into groups of atmospheric stability and according to anticipated frictional roughness by sectors of wind fetch over the Twin Cities.

I also was guilty of missing a meeting with Lettau on my independent reading study of polar expeditions. Independent reading was a wide open library review on a topic mutually agreed upon by student and faculty adviser. Many times such library searches lead to new research grants for a department and the University of Wisconsin had been a leader in glacial studies since the legendary Prof. Thomas Crowder Chamberlin taught at Madison and established the presence of the Ice Age in the Mississippi River Valley nearly a century ago. It was a natural thing to do - read about Antarctica together with such an expert as Lettau and receive credit besides.

I had delayed setting up such an appointment in an effort to find a book he had not read before I did. I was not very good at independent study; at least I never felt a good grasp of developing a successful literary search of the polar literature. I would outline the works of Nansen and Amundsen, and Lettau would reply, "But did you find an article on Vestspitzbergen or data from the nineteenth century polar years?"

I had always been interested in the exploration of the polar regions and the related contributions to basic research. Many knew Prof. Lettau had just taken an interest in this topic as well. This turned out not to be true for it became obvious that Lettau was interested in polar work a long time but that

grant money and research opportunities just had not materialized. I believe he had read every book in print and in every language on the subject. I would search out several books, digest them, report to him, and if they were popular or of common interest, he would shrug and say he had read them and ask if I had read anything else. The rare books or those loaded with data he took special interest in, especially if an author had identified a problem or expressed some uncertainty. I know of no one else who was as successful at using other data already in print and worked over by others. Lettau was always capable of seeing new and more interesting results.

I planned to dash straight to campus to try to catch Prof. Lettau before he went to lunch. At least the impression I might leave would not show I wasted all the morning. As I walked out of my apartment door down the hall to jump on my forty-eight Schwinn I was interrupted by a second phone call. I hastened back into the room. Kirby Hanson was on the phone.

Kirby was my idol as a graduate student. Actually he was much more than a mere student. He was on leave from the U. S. Weather Bureau pursuing a graduate degree and coordinating several research projects of joint interest between the University of Wisconsin and the Weather Bureau. He had been on the second team to winterover at the South Pole during the International Geophysical Year (1 July 1957 - 31 December 1958). He pioneered work on temperature inversion and thermal radiation studies at the South Pole. His radiosonde data from the South Pole correlated with other international scientific stations gave an initial picture of the unexpected movement of weather systems across the high ice dome of the Antarctic. Now as both a government scientist and a graduate student, he was involved with initial design work of the first weather satellite.

Kirby married a beautiful woman and had two wonderful children. He still had all the ideals of a young person but his family life seemed to temper dreams and hopes into realistic goals and achievements. There was much I learned from the Hanson family. Viewing him as my "big brother" I had gone to him with my crazy interest that I had in Antarctica since I read as a child a grade school *Weekly Reader* about Admiral Byrd and Operation High Jump of the late nineteen-forties. I never was able to talk to friends or relatives about my serious interest in Antarctica without ridicule. Now, as a graduate student, I was studying the findings of IGY and Kirby was a part of that IGY in 1958.



Not only did Kirby listen to my wild ideas and dreams, he also had given me the address of government people with the National Science Foundation. At Kirby's urging I had applied about six months earlier for a position in the U. S. Antarctic Research Program but nothing affirmative had developed. With my own graduate studies bogged down, I had lost concern over that application as an observer. Things like earning a living, doing useful things for society, or even just providing for oneself did not trouble me. My life on a research fellowship was easy after a childhood in the slums.

That there was a life after graduate school had not occurred to me.

Now, on the phone, Kirby informed me that Prof. Lettau, Prof. Schwerdtfeger, and two visitors to the University were planning lunch in the Memorial Union. Kirby relayed that Lettau wanted to invite me to this important lunch meeting but could not find me. Kirby also thought it was important. He could not give me any more details. He would be at the meeting. "One last thing, comb your hair and wear a suit!" Thank you, Kirby. I guess this was no time to be a hippie.

As I showered and shaved, I wondered why Schwerdtfeger was to be at this meeting. My first impressions of Prof. Werner Schwerdtfeger were made in a three credit twelve hour Synoptic Meteorology lab. He was tough and demanding. Two things stood out. The only name from my class that he could remember was that of my good lifelong friend, Donald Panzenhagen. The second was Prof. Schwerdtfeger's absolutely wild excitement while interpreting the processes of weather systems. His English was excellent but with a heavy German accent sprinkled with Spanish.

The student generated rumor mill identified Prof. Schwerdtfeger as a general in the German Luftwaffe during the Second World War. I do know he flew in aircraft a lot during the war and he spoke of collecting correct wind data over the North Sea when the British falsified their weather broadcasts. His wind measuring device often was a machine gun and a stop watch to time the motions of salt spray raised by the bullets. After the war, Prof. Schwerdtfeger served the government of Argentina, and again the rumor mill had this tall colorful teacher in several revolutions in South America. The long hours of laboratory work, weather map analysis, and endless mathematical analysis led us all to understand and appreciate this famous world renowned expert in Southern Hemisphere meteorology.

Being a hemisphere dominated by water, with a polar cap mounted on a plateau high above sea level, the Southern Hemisphere weather was distinctively different from the Northern Hemisphere. Before the routine use of weather satellites that now see all things, I enjoyed his subtle jokes about how much more accurate the theories were in the Southern Hemisphere oceans where observations didn't cloud the imaginative theoretical models. With the University of Wisconsin becoming the best atmospheric physics school in the world, Schwerdtfeger's presence on campus gave us all a global understanding of the weather. I had not had a class with Schwerdtfeger for nearly two years since my undergraduate days. I had no idea why he was involved in this lunch meeting. And more puzzling, why me?

Time was running out! Relearning to tie a tie took more time than expected. Down the elevator in Hasse Towers, a shot along Gorman Street, through a red light at Wisconsin Avenue with a wide skidding turn to slide over to Langdon Street and then mostly down hill needing to slow only a little as the pedestrian traffic clogged the approaches to the Union I raced to lunch with my profs on my trusty Schwinn. Rolling in sweat, dressed in suit and tie, I presented a rather strange view of a scholarly-minded, adventurous graduate student.

18 February 1965. "Policy on Viet-Nam adopted today calls for the following: Joint program with GVN of continuing air and naval action against North Viet-Nam whenever and wherever necessary." (Cablegram from State Department to heads of nine United States diplomatic missions in the Far East.)

Not needing to lock up a forty-eight Schwinn I saved time and arrived at the Memorial Union early to apprehensively pace back and forth in the main lounge. I faked an interest in the art show on

[All boxed in quotes about Vietnam are taken from *The Pentagon Papers* as published by the *New York Times*, 1971, no copyright is claimed in official Government documents quoted]

display by examining paintings and sculpture without seeing them. Then the mob came. Five men arrived, all together, dressed obviously different from the prevailing dress code of the campus in the sixties. In this Union everything and anything was permissible, from the formal Military Ball to the Anti-military Ball.

As they approached, I could see a distinct difference in the men. The professors had coat and tie, but also chalk-dusted sweaters and coat pockets stuffed with things like pipe, chalk, note cards, thermistors, and whatnot. The two government men also had coat and tie, but no sweater, no bulging pockets, only the look of sharp and slick.

Kirby Hanson introduced me to Morton Rubin and Paul Dalrymple. Immediately after a short exchange of pleasantries the very tall and physical Paul Dalrymple in clipped New England speech asked, "Why are you so short? I don't know any short polar heroes." What could I have said?

I still didn't know what this meeting was all about. I smiled, said nothing, and kept listening. We had a fine dinner in the Rose Room, but I don't remember the food. I do remember some attention paid to me. The two government scientists inquired of my experience in the Weather Bureau.

I was a Trainee and Observer at the Weather Bureau Airport Station in General Billy Mitchell Field in Milwaukee the summer of 1962. My duties included the hourly observations and routine pilot briefings. I also was assigned a research project by Bill Harms to analyze and describe the sea breeze phenomenon over Lake Michigan.

After graduating from the University of Wisconsin with a Bachelor of Science degree, I returned to the U. S. Weather Bureau with the rank of Meteorologist and continued this lake breeze research from the Forecast Center in Chicago, then located on the University of Chicago campus. It was this experience and the encouragement of the Chief Research Meteorologist, Larry Hughes, that pointed my life in the direction of basic research and back to graduate school.

I enjoyed the exposure to RADAR analysis and the crisis work of following severe weather such as tornadoes and the storm seiche along the Chicago beaches. There was intense and exacting forecast work developing wind trajectories and temperature profiles for long distance aircraft flights out of O'Hare International Airport. My assignments for these international flights usually covered the air space between Chicago and Newfoundland. While at the Forecast Center in Chicago, I also gained experience and understanding of the mesoscale of meteorology formulated by Dr. Fujita. I enjoyed watching the thermal disc tank experiments of Dr. Fultz that gave insights to global wind circulation. Both these researchers were professors with the University of Chicago.

Awed by the struggle endured daily in the discussions, or rather heated debates at the Chicago Forecast Center, I listened and questioned all things as each of the professional forecasters took turns leading the weather analysis for the day. I took my turn with great trembling and quickly learned how much more rapidly a person gained knowledge by experience rather than from school. One grave error still sticks in my mind. While orally developing my picture of the weather for the northeast quarter of the contiguous states from Illinois to New York for one of these debates, I had used the expression "in my opinion." An instant explosion erupted from John Hovde, the District Forecast Supervisor. "I don't give a damn about your opinion! What is going to happen?" He was the roughest on me. He turned me into a very good forecaster.

Hovde correctly taught more than forecasting. Oracles forecast. They have opinions that some times seem correct and many times are wrong. Science sought the created order in nature. Although the atmosphere contains so many variables that lead to false interpretations and many mistakes, the

science of atmospheric physics is an exact science, as exact as any other aspect of physics, and therefore its models and algorithms give predictions, forecasts without opinions. The degree of certainty leads to the blessings of warning, preparedness, and general progress of understanding. The degree of uncertainty leads to mistakes and a lure to research the unknown. That lure, instilled by Hovde and Hughes, is why I was back at school and probably why I was at this lunch.

Mort Rubin inquired of me what my interest was in the Antarctic. "What did micrometeorology or mesometeorology have to do with the Antarctic?" Was it a bated question? How did these men know of my interest in Antarctica? I really didn't have any reason to go to Antarctica other than just to go - a childhood adventurous dream. The awe inspiring account of *Little America* and *Discovery* by Admiral Richard E. Byrd, the saga of *The Home of the Blizzard* by Sir Douglas Mawson, and the tragic account of Sir Robert Falcon Scott would excite anyone to a passionate desire to see Antarctica without reason.

Quick, I told myself, fabricate a reason. Why were Lettau and Schwerdtfeger suddenly silent? Kirby had returned from the South Pole. Why did he go? I remembered the trouble all of us had with stability in the equations generated from Navier Stokes' starting equations for atmospheric flow and raised issues about the many terms that were time dependent. Time and space changes were not independent in most meteorological phenomena. The Antarctic, with its polar twenty-four hour day in summer and twenty-four hour night during winter would be an ideal laboratory to test some of the mathematical systems otherwise not capable of testing without the added complexity of the sun's daily rising and setting. If we could understand some of the simple ideal systems of change in nature, the persistent hope in all of science is then to graduate to more and more complex systems adding new terms to the equations.

Paul Dalrymple asked if I played any physical contact sport? No! He thought so. Did I play any sport? I used to follow the Milwaukee Braves. He had no sympathy for a Milwaukee boy who stole the Braves from Boston and then he laughed that they were on the road to Atlanta. Paul Dalrymple was a baseball fanatic. He spoke of his own contribution to the Cooperstown Baseball Hall of Fame. His picture and an orange painted baseball were in the Hall of Fame for playing the first baseball game at the South Pole. That story started a long session of reminiscence of the polar exploits of these three government men whom I had no idea were so closely bonded. Rubin, Dalrymple, and Hanson all had penguin tie clips and all wore the frost weathered faces of men who had seen -100 ° F. This was amazing! I had confessed to Kirby many months ago that I had this foolish dream and now at the same dinner table with me three men roared in laughter remembering some of their antics while there. I also sat in respect at the perfect silence of the two professors. I joined them with intent listening.

At the completion of dinner we drifted off to regroup in the Rathskeller, a beer tavern of German decor in the basement of the Student Union. I cornered Kirby to inquire of him what was going on? Who were these men? Why were we all together? Mort Rubin was with the U. S. Weather Bureau in the chief administration office serving on frequent international exchanges. He had been a U. S. Observer at the major Soviet Antarctic station, Mirny, and frequently served for our country on the international Scientific Committee on Antarctic Research (SCAR). Originally, twelve Antarctic Treaty signature nations, and now also many other nations acceding to the treaty, carry out the planning of scientific programs of intensive interest and ensure a prompt and accurate exchange of all discoveries at these SCAR meetings. The statesmanship and the high degree of theoretical concern for basic research were very evident in all that Mort Rubin said.

Paul Dalrymple may have been a poker player but not a statesman. He spoke his mind plainly and bluntly. He spent one austral winter at Little America V, the chief support base for the building of Byrd Station by tractor trains. That first year of the IGY Paul established a micrometeorology program

at Little America. His instruments provided detailed data on temperature and wind profiles over the Ross Ice Shelf as well as basic net radiation measurements of heat flux between the snow and ice interface.

Normally one winter season is all that one man is permitted at a time, but Paul sent two hamgrams from the Antarctic announcing first to his wife that he was going to stay one more winter at the South Pole and second to inform the Governor of Massachusetts he would be back a year later to cheer the Boston Celtics and pay his taxes. His wife went to Bermuda.

Paul repeated his micromet program at the South Pole and became a firm believer in doing things second, letting the unknown hazards be found by the adventurous and developing a better program of study on the basis of some experience. He was with the second team to winterover at the South Pole along with Kirby. He was now head of the Polar and Mountain Laboratory of the U. S. Army Natick Laboratories in Massachusetts.

The experience of these three men, Dalrymple, Hanson, and Rubin, was inspiring to me as a scientific hopeful. They had gone to the source of the unknown for their studies. Paul Siple, a close friend of these three and author of *90 ° South*, in an account of the construction and survival during the first winterover at the South Pole, described science in the spirit of IGY as using to the fullest the modern technology of global communication and high speed transport and modern designed living comforts in order to bring scientists to the edge of adventure and exploration. To remain in the library and laboratory alone was not enough. These men had remained dedicated to that ideal spirit for all this time since IGY.

Then I learned why I was at their meeting. Kirby informed me they had just finished my interview. I would be invited to listen to their conversation after dinner but for the most part Dalrymple and Rubin were finished with questions of me. Gulp. I suddenly felt unprepared. I had no idea. I probably said all the wrong things. I was just a graduate student. I had no experience. It probably was a good thing that I did not know what was going on, but now it all made sense. Lettau had some new interest in polar studies, particularly in Dalrymple's field programs. Schwerdtfeger was the Southern Hemisphere expert. Kirby had been to the Pole. And I wanted to go.

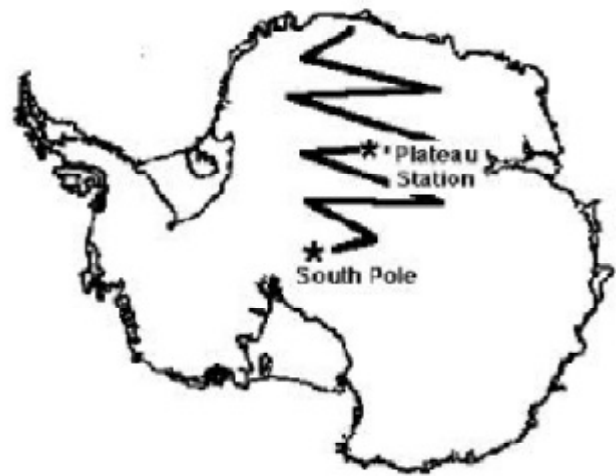
I could see I would never make it. I simply was too nonathletic. The only games I played in my youth were ball tag with a hockey puck and for show I was good at sharpening a switch blade until some police officer confiscated it. Sometimes, being the runt of the neighborhood, I was compelled to sweep the glass out of the alley for the tough big boys so that they could play buckets against a wall using a wire coat hanger for a basketball hoop.

Accompanied by the consumption of several pitchers of beer, the discussion centered on details of proposed polar work. I meditated intently on every detail, missing a lot due to the rapid pace of the conversation. How can you take notes in a tavern? Through these discussions I learned the United States started an extensive traverse program in Antarctica the past austral summer beginning in November of 1964 exploring between the South Pole and Queen Maud Land. This was the last large unexplored area of the Antarctic continent.

The plan for the traverse was to make a zig zag path maximizing travel distance to enhance exploration of the region. The plan contained the objective that this traverse, crawling over the icecap, over the highest and coldest region of the Antarctic, would reach Princess Ragnhild Coast in four years. At several places along the planned route the traverse vehicles would be cached. The scientists would then be airlifted out of the region at the end of each summer and returned to continue their exploration as early each successive summer as the severe cold weather would permit.

A letter dated 16 October 1964, from Albert P. Crary, Chief Scientist, Office of Antarctic Programs, National Science Foundation, calling for research proposals announced, "A plan is also under study to locate an austere wintering-over station at the terminal point of the second year's traverse, 80 ° South, 25 ° East, from the period January 1966 to February 1968. The objectives will be to take advantage of the unique location for research studies that may be of interest in meteorology, micrometeorology, glaciology and upper atmospheric physics. Work will be possible during the winter of 1966, the summer of 1966-67, the winter of 1967 and the summer of 1967-68."

"The purpose of this letter is to bring this research opportunity to your attention. For your information, the elevation will probably be about 3,500 meters above sea level and it is expected that snow accumulation will be very small. The temperatures should be very much like those at Vostok, where the average is about 5 C° colder than at the South Pole station. The station will be modeled after Eight's Station though probably somewhat smaller, with accommodations for four or five scientists."



The super theoreticians, the university men, were full of ideas of what might be done on the high Polar Plateau. However, they both were about ten years from retirement and not interested in learning the problems related with the establishment of a field site in such a hostile environment. The government men had the experience for establishing the station, its supply line and anticipatory knowledge of what to expect at 11,000 feet and -115 ° F. The possibility that I might be one of those scientists to winterover at this extreme camp to gather valuable data began to swell within me.

Lettau and Schwerdtfeger previously submitted suggestions concerning a study of the Great Antarctic Inversion in a letter to the National Science Foundation. It was a letter of suggestions of what might be done, not a proposal. They did not want the logistics responsibility. They might have been over cautious since the University of Wisconsin's Geology and Geophysics Department jointly with the Polar Institute of Ohio State University were already extensively involved with the traverses.

An overview of the Great Antarctic Temperature Inversion was known from work by Kirby Hanson and Paul Dalrymple. First of all, a temperature inversion is a common evening phenomenon, especially during a winter evening, anywhere in the world. The heat radiation cooling of the ground surface sends heat by way of long wave radiation out to space. With not much heat returning to the surface from the moisture, clouds, and air, the net radiation, that heat energy remaining from losses compensated by gains, algebraically adds to a net loss. The ground temperature cools rapidly and the air just above the ground also loses heat and cools but each successive layer above is warmer until the cooling versus warming reaches zero. The resulting temperature inversion is just that, a layer of air where the temperature rises with height above the ground inverse from the normal daytime solar heating pattern.

In the Antarctic on the High Plateau, Lettau and Schwerdtfeger named this layer of air the Great Antarctic Inversion. Kirby and Paul's data showed this inversion over the South Pole to stand some 2000 feet above the snow surface with the temperature at that level to be a warm maximum of -40 ° F and the snow surface measuring a minimum between -80 ° F and -100 ° F.

The Lettau and Schwerdtfeger letter praised the standard radiosonde and RADAR measurement techniques used for normal observation as quite sufficient to arrive at a general picture of the temperature and wind structure over the polar cap at a number of international stations. However, the two professors pointed out several difficulties. The routine aerological radio soundings were launched with large balloons that pass very quickly through the lower layers of the atmosphere. Never seen was the fine structure of the great inversion. The details needed for physical and mathematical analysis were unobtainable in this fashion.

Routine balloon radio soundings were launched every day at twelve hour intervals. That time span "is so large that characteristic temporal variations of the great inversion and their relation to changes in cloudiness, wind shear, and net radiation remain unknown. Almost completely lacking is direct information on the spatial variations of the inversion structure on the Antarctic continent."

Such a thorough study of the changing features of the great inversion and its spatial variation and extent over the high Polar Plateau would certainly improve the general understanding of the behavior of inversions everywhere in the world. In the Antarctic all things were also so well defined and the extreme conditions such as the clear sky, the high altitude, the long night, and the isolation from contributing sociological intrusions would enhance observation of the complicated features not visible to mankind before.

Other features such as mirages, the propagation of sound, light, and other electromagnetic disturbances remained somewhat unexplained in detail and this type of study would have potential toward such an understanding. I remember from Lettau's micrometeorology class stories of World War II accounts that explosions were heard from the war front many miles away causing terror in towns when the fearful immediacy of military action was not so eminent and actually far away. A person standing in a barnyard on cold nights often can hear sounds clearer from farther away. A temperature inversion very definitely is related to this phenomenon.

Lettau and Schwerdtfeger proposed specialized radiosondes monitoring temperature and humidity with fast revolving, mechanical microbarographs and attached Soumi-Kuhn net-radiometers. Special slow rising balloons would carry the instrument package aloft at a rate not to exceed one hundred metres per minute. The normal lift velocities of such balloons were between 350 and 1000 metres per minute. The desire was to keep the inversion monitoring system within the inversion as long as forty minutes before reaching the altitude of 3000 metres. Daily routine observations were to be eliminated in favor of successive serial ascents on specially picked days. The chief meteorologists in the field would be expected to understand the theory thoroughly and know what they were looking for.

"Sound wave recordings of explosions 'ad hoc' released at the surface and at adequately chosen heights" were proposed as an original method of obtaining more information about the structure of the great inversion. A pilot study of the great inversion at South Pole using routine data already in possession at the Department of Meteorology at Wisconsin was suggested. They also suggested that the design and preliminary testing of a radiosonde system to be used could be done at the University. Finally Lettau and Schwerdtfeger wrote, "Needless to say that the two undersigned are willing, and anxious, to take charge of the evaluation and further elaboration of the results of the special inversion soundings, if and when an observational program of the proposed kind becomes reality."

Hard decisions followed. RADAR was out. The small station planned for the terminus point of the second year of the Queen Maud Land traverse could not count on the power necessary to operate

a sensitive RADAR unit. An additional building also would be required. Heat to outlying buildings was not possible. The need for a separate heating plant and additional oil supplies all contributed to the cancellation of RADAR. That was a major loss before a scientist even went into the field.

The sounding system for the inversion study had to keep the aneroid barometer as a check for the height of the balloon and provide necessary data for the triangulation of balloon movement needed for the determination of the wind's speed and direction. An immediate theoretical conflict emerged from these constraints. In an inversion the interrelationships of pressure, temperature, and density of air were to be studied, not used to determine other parameters.

An old fashioned manual system of triangulation suddenly looked pretty effective and independent. Two theodolites manned by the two meteorologists proposed for this assignment could do the task while leaving all other observations for automatic recorders. Each scientist would measure elevation (the angle from the horizon up to the balloon package) and azimuth (the horizontal angle swept clockwise from a preestablished base line to the position of the radiosonde) every thirty seconds. These data with needed trigonometric calculations could give independent heights and distances, the spatial positions, of the balloon carrying the instruments floating in the moving air. The change in the spatial position would yield the wind speed and direction.

All three polar-experienced men expressed concern for both the instruments' abilities to function in the anticipated air temperature of -100 ° F and possibly even colder as well as the ability of the human observers to remain unfrozen long enough to obtain the necessary data. Each balloon flight was expected to last thirty to forty minutes. Preliminary work before a balloon watch was also required. The expected rapid repeat of all outdoor work with little or no warmup time demanded by the study that required serial launches spelled too much frostbite for the men and thermal fatigue of metal parts of the instruments. Metal would become brittle and break like glass at the expected temperatures. Paul Dalrymple, a pioneer of wind chill studies for the U. S. Army, rattled off a considerable number of devastating facts with regard to the exposure of humans and their uncontrollable severe heat losses. Perhaps observation shelters could be developed permitting the observer to remain in warm comfort or at least remain out of the wind. Mort Rubin would look into it.

The base line between the two theodolites should be as far apart as possible. If the balloons were tracked for 3000 metres (nearly 10,000 feet), a base line of nearly two miles was desirable. That distance was too far away to walk back and forth in the severe cold. The greatest fear was for the loss of direction or personal orientation should a sudden change of wind occur and cause visibility in the polar night to suddenly reduce even in the slightest because of ice crystals or snow in the air. The greatest loss of life in the polar regions had occurred when a person lost his way, even a short distance from the door of the main camp building, and froze to death. Kirby, Paul, and Mort, all with polar experience, encouraged the shortest possible distance between theodolites.

Objection! This idea was striking at the very heart of the great inversion experiment. The most exciting and valuable discoveries were anticipated from the wind data. How the wind changed its speed and direction with respect to height and correlated with the temperature at each exact measured altitude demanded a base line of maximum length. The wind displayed the mechanical exchanges of energy with the snow surface. The wind showed the movement of sensible heat across the Polar Plateau. The wind moved the moisture, the ice crystals, the snow. The wind had to be measured as accurately as possible. Without a proper baseline, the University professors pleaded, the study would not be able to formulate worthwhile theory and never would be able to confront old misleading theories.

Paul Dalrymple spoke of his plans to place a one hundred foot micromet tower that would give

the detailed temperature and wind structure to that height. He desired to be the chief meteorologist on the first year of this expedition for the balloon project and remain to establish the tower. All men here believed the Great Antarctic Inversion was considerably higher than Dalrymple's proposed tower. He strongly lobbied for the longest base line possible but not at the expense of the observers.

Struggling to preserve the longest possible base line, Prof. Lettau suggested a small hut with its own heating system such as Admiral Byrd used at Advanced Base on his 1934 expedition. Such an isolated hut could be maintained by one scientist. By living at a mile or more distance, communication could easily be maintained. During good weather, isolation could be broken with visits back to the main camp. I thought to myself, "I can do that." Dalrymple stared at me as if he read my youthful enthusiasm and abruptly interrupted, "Byrd almost died!"

This was the method of science, an endless struggle between the artistic brush strokes of theoretical science, the ideal sought, and the limiting frame, the reality that accepts what cannot be done. In the Second Byrd Antarctic Expedition (1933-1935) this same tension was most prevalent. In its day it was the most elaborate technologically equipped expedition with the highest scientific goals. It was the first expedition to adopt seismic soundings as a method for a large scale systematic mapping of the sub-ice topography. Special techniques maintained sensitive, delicate, complicated instruments that elevated the entire observational scope for all time in the Antarctic. Tracked vehicles, used for the first time, enabled research teams to extend the field of their studies and probe in more remote areas than ever before. Aerial reconnaissance established the boundaries of the Ross Ice Shelf and confirmed that the Ross Sea and Weddell Sea were not connected.

On this Second Byrd Expedition meteorology received major attention. The desire to establish an inland substation, Advanced Base, at the foot of the Queen Maud Mountains some four hundred miles south of Little America II was vital to meteorological research. However, the hardships of establishing Little America II, the struggle of Misery Trail, the severe weather, and frequent equipment failures caused delays and put the entire plan for a substation inland in jeopardy. With it the hopes and dreams of all meteorologists were also jeopardized. A station that distance away would have given significant pressure differences to establish cyclonic and anticyclonic activity as it developed while it slid down or scaled the icecap. Polar dominance of global weather was believed starting with the establishment of the Polar Front theory by the original Norwegian theorists, Vilhelm Bjerknes, Jacob Bjerknes, and Halvor Solberg.

Events forced Admiral Byrd to settle for the Advanced Base only one hundred twenty miles from Little America. This little station could only support one man. Byrd chose himself for the arduous task in isolation and solitary confinement. He wintered alone from March until mid August. A life supporting heater produced excess carbon monoxide gas under poor ventilation and the vital radio needed to correlate meteorological changes between Advanced Base and Little America added to the carbon monoxide poisoning. The meteorological task he bravely set out to achieve by bearing all physical and psychological hardship for the sake of science nearly killed him.

A compromise over the base line for the Great Antarctic Inversion study set the distance at one thousand feet. At all stations one hut was always set aside as emergency quarters with a year's supply of food and fuel, its own electrical and heat generator and room for all party members, albeit cramped. The fear always was the loss of the main camp to fire. At one hundred below zero flexible fire hoses and liquid water supplies would be inadequate to fight a fire. Now proposed was such an emergency camp built one thousand feet from the main camp. It would house the needed observational dome for the second theodolite. A small electric heating system would operate through an electric cable from the main camp. Such a cable would also carry needed communication between the camps. The electric heating system would be independent of the emergency heating system. That

would preserve that system as well as save fuel. Heavy liquid diesel fuel was relatively easy to bring via a ship with an icebreaker escort to McMurdo Base, but to airlift absolutely every item including that fuel and every weather balloon from McMurdo to the high Antarctic Plateau was incredibly costly.

I sat in awe listening to the give and take between the ideal and the real. In a sense, everything about the Antarctic was ideal. No immediate urgent national need demanded a presence in the Antarctic and for the Archie Bunkers, the Great Antarctic Inversion had a low priority. Such is the work of basic research. Industry rarely touches it. Little of it is practical. The discoverers rarely see the practical use of their own work. The inventors of the cathode ray tube searching for the connection between electricity and light had no knowledge that their invention gave the economical fluorescent light. The men who bent that cathode ray in a magnetic field discovered the mass of the electron, and later used it to measure the mass of many atomic particles, but they never dreamed that their invention would be in everyone's home as a television set.

The cost of these proposed projects? I learned much later the price tag to the U. S. Navy was \$200,000.00 per man to keep him alive for one year at Plateau Station. What is the reason for such basic research? Was it worth it? A little of the reason comes out in admiral Byrd's book *Alone*. Byrd wrote: "I am finding that life has become largely a life of the mind. Unhurried reflection is a sort of companion. Yes, solitude is greater than I anticipated. My sense of values is changing, and many things which before were in solution in my mind now seem to be crystallizing. I am better able to tell what in the world is wheat for me and what is chaff. . . . my views about man and his place in the cosmic sphere have begun to run something like this:"

"If I had never seen a watch and should see one for the first time, I should be sure its hands were moving according to some plan and not at random. Nor does it seem any more reasonable for me to conceive that the precision and order of the universe is the product of blind chance. This whole concept is summed up in the word harmony. For those who seek it, there is inexhaustible evidence of an all-pervading intelligence." (Richard E. Byrd, *Alone*, Ace Books, Inc., New York, 1938, p.108) And a hymn he played over and over again as a favorite during his isolation was, "Oh Holy Night, the stars are brightly shinning. 'Tis the night of our dear Savior's birth." (Byrd, *Alone*, p. 139) The reality of risk with the lure of polar research gripped me. Basic research had its value. But things of value came with a price. In paying that price I would grow as a human being. I would also grow as a child of my Lord, for He already paid the ultimate price.

Even with the proper caution, a great risk to human life was real. Real limitations did indeed curtail some of the hopes of Lettau and Schwerdtfeger. Still everyone was enthusiastic for this basic research project. Lettau and Schwerdtfeger hoped for the best available data on temperature inversions this earth could provide. Dalrymple, Rubin and Hanson gave all the practical advice experience could provide to achieve the best possible results safely. Dalrymple and I truly were excited to go get it. Paul's was an enthusiasm tempered with experience and knowledge. Mine was young and foolish. I had already learned in this initial meeting more about science than in any single science class. I also learned that every note taken in the classroom and every idea remembered would be invaluable.

Mort Rubin turned the discussion to inter station communication problems. Part of the Lettau-Schwerdtfeger Proposal called for a continual analysis of weather systems moving into, across, or out of the Antarctic continent. The rates of movement of these weather systems were to be plotted and communicated to the scientists at the new station to provide advisory information to assist them with their serial balloon launchings. Such inter station coordination was anticipated to be difficult.

Such specialized analysis of continental weather was not done except to provide the military

with weather advisories for their supply operations during the summer months. It could be done but many of the stations were established by foreign nations and language would be a barrier. The Great Antarctic Inversion study would go into the field the very next austral summer. The temporary station on the high plateau would be in existence for only two years. Coordination would be difficult to organize in such a short time.

Men at the National Science Foundation didn't believe spatial variation of a microscale phenomenon such as an inversion would be important. The region was too near the geomagnetic pole, the position where the earth's magnetic field lines enter the earth's surface. Near the planned position of the new station, auroral phenomena were very active and interfered strongly with communications rendering them uncertain for the desired immediate data exchange. Scientific news releases would be given to all foreign governments active in Antarctica and perhaps on site extemporaneous exchanges might develop.

The position of the station became an issue. The terminal position of the second year of the Queen Maud Land traverse was not exactly fixed. Schwerdtfeger felt that a perfect laboratory condition for the Great Inversion Study would exist by placing the station on the ridge line of the High Plateau. With the station right on the ridge, which in this case is flat, down sloping cold heavy drainage winds would not be present to affect the study examining the development of the temperature inversion.

Cold drainage winds, called katabatic winds, cascaded down the slippery slopes of icecaps like Greenland or Antarctica. The strong katabatic winds of Antarctica received world wide attention through the famous writing of Douglas Mawson from Australia titled *Home of the Blizzard*.

For the Australian Antarctic Expedition, 1911-1914, Sir Douglas Mawson established a station at Cape Denison on the coast of Adelie Land. It was there, during the months of May and June, frequent hurricane force winds in excess of eighty miles per hour blew steadily. Sometimes these winds blew for several days at a time, always down the slope from the inland High Plateau and seemingly not related to cyclonic activity from the sea. In his book Mawson showed several pictures of men leaning into the steady wind at angles more than forty-five degrees turned down from the vertical with their feet riveted into wind driven snow polished surface ice by Swedish crampons with inch and a half teeth. These winds began suddenly and ended as suddenly as they began.

The textbook explanation of these katabatic winds originated with H. H. Hobbs and was part of anticyclonic circulation formations over polar icecaps. Warm air rose from the surrounding ocean waters. Then it moved upward and inward aloft over the polar cap, became cool, and sunk in the central region of the icecap. For Greenland and Antarctica that region is their respective glacier high plateau. Then this heavy cold sinking air surged outward and accelerated down the icy slopes and reached hurricane force as Mawson observed at Cape Denison. Similar recordings were made at other coastal stations since the Australian Expedition in 1911.

These outbursts of cold air in the Northern Hemisphere were believed to be the trigger that set off the main frontal cyclones of the Atlantic. This theory spawned the interest of meteorologists in the polar regions. Their belief followed reasoning like this: if one can understand polar weather, then weather everywhere might be predictable. The katabatic out flowing winds, based only on coastal observations, became the model for low level wind flow over all polar icecaps.

Prof. Lettau pointed out that although katabatic winds were a dominant feature along the Antarctic coast where ice slopes were steep, his own study recently presented for publication using some of the data collected by Dalrymple at South Pole Station in 1958 showed a wind flow not as

simple as that presented by Hobbs. Katabatic winds were not so all dominating on the entire continent. He identified light katabatic winds existing only in the lowest twenty feet above the snow surface. These winds were directed down slope, steady, stronger than the prevailing winds, but short-lived. Larger scale weather systems easily disturbed these katabatic winds. In the interior of the Antarctic, these winds were weak due to the nearly level but slightly sloped ice dome at the South Pole. According to Lettau's findings, katabatic winds could not account for the general low level wind circulation over the polar ice dome. (Heinz H. Lettau, A Case Study of Katabatic Flow on the South Polar Plateau, pages 1-11 in Volume 9 of *Antarctic Research Series, Studies in Antarctic Meteorology*, Morton J. Rubin, Editor, American Geophysical Union, 1966)

Continuing the discussion in the Rathskeller, interrupted only when a pitcher of beer became empty, Lettau pointed to the contradictions within the Hobbs-katabatic theory for large scale polar air circulation. Sinking air is usually representative of anticyclonic circulation. Anticyclonic flow, the common wind pattern for a high pressure region, inhibited cloud formation and prevented precipitation from occurring. Also strong out flowing katabatic winds would transport surface snow away from the center of the ice dome and outward to melt in the sea. Together, the sinking air and the out flowing air would reduce the polar cap. The Hobbs-katabatic wind theory, accepted as a rule of law in the Compendium of Meteorology and likewise expressed as a law in almost every textbook of weather and geography, simply could not exist as a climatological feature for an icecap with the stability and longevity of either Greenland or Antarctica.

Other results, also generated from Dalrymple's micrometeorology data and taken at Amundsen-Scott Station, hinted at a new approach. Lettau spoke of thermal winds formed by the extremely stable and stratified inversion layer. The inversion phenomenon was widespread over the icecap. These thermal winds, caused by a horizontal temperature gradient, would alter the ambient or normal wind established by the pressure gradient and the earth's spin. Lettau and Schwerdtfeger expected that the wind speed and direction near the snow surface would be forced to turn around the ice dome according to the thermal wind hypothesis. These turning thermal winds of the inversion predicted only a minimum loss of snow and greatly increased the longevity of the icecap when compared with the Hobbs-katabatic simple down sloping and outward winds.

Schwerdtfeger further explained that this inversion wind, if it existed, would be locked to the slope and shape of the ice dome. These winds would create a low level anticyclonic circulation over the ice dome. As a cold core shallow high pressure region, the inversion winds would also establish a cyclonic circulation pattern aloft. A circumpolar cyclonic wind pattern aloft would enhance precipitation, continually adding to the polar cap.

This aspect of the Great Antarctica Temperature Inversion was revolutionary and Mort Rubin, with emotional excitement, recognized its value as a solution to many unsolved problems. On a global scale, it always was a mystery why, with the earth's strong green house heating, there still were the two major icecaps of Greenland and Antarctica.

This phenomenon, in the minds of Lettau and Schwerdtfeger, may have escaped detection because of current data analysis techniques. Instruments were designed to measure expected phenomena and no one ever looked for these inversion winds before. Balloons carried their instruments through the inversion so fast that details needed to identify the proposed wind effects were not visible even if looked for.

This grand finale of theoretical interpretation gave high purpose to future polar exploration at the proposed austere isolated small station in the center of the unexplored High Plateau of Antarctica. Our meeting adjourned with Dalrymple saying to me: "You still want to go South? You're too little." I

suggested my small surface area was easier to keep in thermal equilibrium. I bicycled back to my apartment at Hasse Towers with my head swimming over the awesome project planned for the deep south, over even the remotest possibility that I might personally be involved, and over too much beer!

