CHAPTER 4
Spring Training

Earning a living by doing research is the strangest kind of employment. I'm not sure growing up in a retired immigrant laborer's household in the slums of an industrial town provided the best image as to how to act, work, and behave in such a position. Although the University taught nothing but research and even seemed to ridicule the routine jobs like daily weather forecasting or pilot briefing, it likewise failed at training me for pure research as a way of life on my own. At school all graduate students simply followed their professor. Each of us was trained to believe our school, our professor, and our methods were the very best in the world at the time. Suddenly my task was to learn about everybody else besides Lettau and Schwerdtfeger. If I had been hired to give a non-eastern view coming from a Midwestern university, the tables were reversed on me. I had an immense amount of reading to do about the research of the people from these eastern schools in order to reach the starting datum plane from which to begin my research.

Going to work every day to read and to argue was not physical, and my first pay check, incredibly too large, gave me a guilty conscience. I was paid to do what I loved - learning about the Antarctic and the weather over its icecap. I was paid for reading. So strange! Many days I went home with a headache, with confusion over someone's theory, and a stack of more journals to pore over the remaining night and into the dawning morning. But these were not heavy lifting, nor back breaking labor, nor boring pursuit of the pace of a machine. The pay was so much more than the hard workers, who earned their wages, who I knew from Second and Wright Street in Milwaukee.

A measure of the difficulty I had with accepting my new way of life as a research meteorologist was my inability to explain to relatives and friends what my new job was all about. To some extent that inability remains even to this day as I write this story to my children nearly thirty years after the story. The research story I, in fact, learned. The many people with whom I mingle and serve today have that same inability to listen and understand the awesome councils of science into which I drifted into and out of. The single icon understandable to all is the fact that this research job took me to ANTARCTICA. The great white continent indeed was the ever present lure, the nearly insurmountable threat to achievement, the inspiration to new thoughts, and the fulfilled dream of my childhood.

In all three documents - my job description, the grant proposal of OMR, and the grant proposal of NLABS - the overwhelming purpose was to make a human appearance on top of the unexplored ice dome of Antarctica. This was not of the same scope as the expedition of Columbus. For Antarctica, Capt. James Cook's circumnavigation of the Antarctic region or Capt. Charles Wilkes' continent confirming expedition would be equivalent to such an exploring adventure. The exploration of the high plateau of Antarctica was perhaps equivalent to Francisco Vasquez De Coronado's expedition that penetrated onto the prairie of central Kansas in 1541.

Exploration and all the adventure that goes with it is a lure just about anyone with a touch of historical interest can understand. But that shallow understanding of what I attempted to do, over the years has become almost hurtful. I cringe in mental anguish when someone introduces me as one who has gone to Antarctica. It is the discovery of the inversion winds that took more than five years of research that is the achievement of my life. This lure of scientific discovery is a bit more difficult to grasp. And so many of my friends and colleagues over the years never did. To seek out those special conditions that form the Great Temperature Inversion and discover new insight as to its structure and formation was my assignment. This was what the high monetary reward was all about. Discovery demanded the development of special equipment or the finding of new ways to use standard equipment. Finally mathematical models needed development with computer animations for the best human interpretation of these very complex structures.

The grant proposals followed quite exactly the requests of Prof. Lettau and Prof. Schwerdtfeger. A temperature inversion occurs routinely over cold ground on a clear night just about anywhere in the world. Antarctica, as a test tube for meteorological theories, provided a twenty-four hour night, uniform terrain, and extremes of the inversion. Scientific exploration works best with extremes. Extremes give views scientists' otherwise do not see. This is why going to a place so far away was so important in spite of the great cost of travel and the cost of exposure to the severe elements of the polar winter. The biggest problem was going to be the severity of the cold on the equipment more than on the observer who had the opportunity to seek shelter in the camp.

I was surprised at the planned involvement for me with Paul Dalrymple's research from the Natick Army Labs. I never expected to be involved in this beginning work on the most elaborate, comprehensive, and precise polar study of solar radiation and micrometeorology since the IGY. I would be at the critical starting point of the development of this program. As soon as my preliminary studies were complete I needed to hit the road to coordinate the three principle research offices, OMR, NLABS, and UW-Madison.

By pondering all this, my thoughts drew to fear. These programs no longer were about getting an "A" or a "B". These programs had no answer to look up in a book. If successful, these programs would give pictures of polar weather and maybe even pictures of weather extending to many other places. That thought was so exciting that for many weeks I went nearly sleepless, reading journals, eating waffles at an all night restaurant on Wisconsin Ave. in Georgetown, and reading more.

There seemed to be no end to the required reading. Readings about inversions. Readings about solar radiation exchanges with many surfaces. Readings about instruments, errors of instruments, modification of instruments and designs of innovative instruments. Readings about the Arctic, the Antarctic, and even about the polar cap of Mars. Some of the authors on whom I took notes included:

Aamot, Angell, Anisimov, Armendariz, Arnold, Artemyev, Astapenko, Bardin, Barkov, Barnett, Becker, Bellamy, Bilello, Blackadar, Bonn, Brockamp, Bryson, Budyko, Bull, Businger, Butler, Buttenberg, Cadle, Camnitz, Campbell, Carmein, Cartwright, Cave, Chelchowski, Clarkson, Colson, Cox, Crozaz, Dalrymple, Darkow, Davidson, Davitaia, DeBreuck, DeJong, Deland, Dergach, Deryapa, Dines, Dobosi, Dobryshman, Dolganov, Dolgin, Drummond, Dubovskoy, Dubrovin, Dukhanin, Duncan, Dyer, Emmanuel, Engler, Federer, Federov, Fichtl, Fiore, Fischer, Fletcher, Flowers, Fogle, Frank, Frankenberger, Franssila, Frost, Frostman, Galtsov, Garnier, Gentry, Gilchrist, Giles, Giovinetto, Goltsberg, Gotaas, Gow, Grigoryev, Gutman, Gwendolyn, Hanson, Haraguchi, Harrison, Henry, Hickey, Hirling, Hoeber, Hogue, Hoinkes, Holdsworth, Islitzer, Ivanov, James, Jex, Johnson, Kangos, Kapitsa, Knapp, Koblents, Korotkevich, Koshelenko, Kotlyakov, Kovrova, Kreem, Kruchinin, Ku, Kucherov, M. Kuhn, P. Kuhn, Kupetskiy, Kutzbach, Laikhtman, Landon, Lansberg, Laptev, Law, Lazarev, Lea, Ledenev, Lenschow, Leontyev, B. Lettau, H. Lettau, Lhermitte, Lipovka, Lodge, Loewe, Losev, Ludlam, Ludwig, Luers, MacCready, Mahrt, Maksimov, Martin, Mather, McCloskey, McFadden, McVehil, Meroney, Miller, Morachevskii, Moreland, Morris, Müller, Murayama, Murrow, Nagata, Nagel, Newstein, Nikandrov, Nitzschke, Obukhov, Ohmura, Orlenko, Orvig, Ostapoff, Paddock, Panofsky, Pavlov, Peters, Petrov, Picciotto, Popham, Predoehl, Radok, Rakipova, Raynor, Reifferscheid, Rider, Roberts, Robin, Robinson, Rogers, Rubin, Rubinstein, Rush, Rusin, Sanders, Sauer, Savonichev, Schaefer, Scholes, Schwerdtfeger, Semenov, Sergeyeva, Shamis, Shen, Sherr, Shmidt, Shnaidman, Singer, Slade, Smirnov, Smith, Sokolov, Spano, Sparkman, Spohn, Staley, Stearns, Steiger, Sterns, Sternzat, Stonehouse, Stremikis, Stroschein, Suomi, Taft, Taljaard, Tanner, Tarakanov, Tauber, Thyer, Timofeev, Tkachenko, Treshnikov, Tseltin, VanLoon, Venkateswaran, Viebrock, Villmann, Vinje, Vittozzi, Vonderhaar, Vorontsov, Vowinckel, Wahl, Wait, Walford, Wark, Weertman, Weller, Weyant, White, Wollaston, Yeh, Zabrodskii, Zhdanov, Zigrossi, and Zverenka.

Many journals specific to my research topic had to be digested. Among them were:

Antarctic Journal of the United States
Antarctic Record
Arctic Scientific Research Institute Publications
Bulletin of the American Meteorological Society
Canadian Geophysical Bulletin
Climatological Data for Antarctic Stations
Climatological Data for Arctic Stations
Japan Meteorological Agency Reports
Journal of Applied Meteorology
Journal of Meteorology
Journal of the Atmospheric Sciences
Polar News-Japan Polar Research Association
Polar Record, Journal of the Scott Polar Research Institute
Reports of the National Center for Air Pollution Control
Soviet Antarctic Expedition Reports
U. S. Antarctic Research Program

And maps had to be nearly committed to memory. My old reliables included:

Australian Map of Antarctica
USAF Antarctic Navigation and Planning Chart
USSR Map of Antarctica.

I quickly learned that by working for Polar Met I was one of only six professional meteorologists directly responsible for polar studies for and under the control of the Weather Bureau and later ESSA. That meant serving in an informational capacity where I was expected to provide detailed information to any and all who might ask. Even though my research dealt primarily with the temperature inversion, requests came for information and I was expected to provide advice for all aspects of the polar regions and weather information from energies penetrating deep into the ice core or the frozen ocean to observations of polar noctilucent clouds high above the stratosphere. The U. S. Navy would request emergency information about the capability to land on the sea ice of an uncharted region of the polar ocean. NASA was vitally interested in our view of how we believed ice crystals on Mars might be forming. Many of these minor research projects, unfunded, regularly took a person away from the assigned task. Yet, when we answered these questions, a payoff came in unprecedented cooperation between the many agencies for years to come. I also discovered that during the interruptions the most tantalizing and unexpected explanations of nature were found through the interdisciplinary exchange.

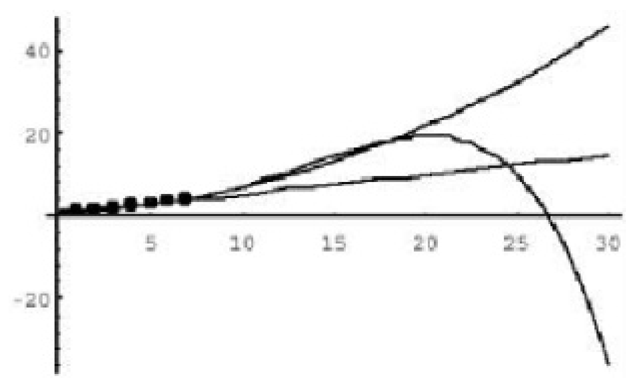
Through this literature survey I noted the large number of Soviet scientists active in the Antarctic. The works of Astapenko and Rusin had to be digested. Mort Rubin continually maintained communication with the Soviet scientists and frequently brought significant publications to our attention at Polar Met. My college Russian came in handy. Even though I considered the study of this difficult language a real burden, to be able to read the technical jargon and interpret formulas and graphs was easier having had those eight credits of Russian.

By reading and translating on my own I could beat the Israeli translating teams serving our government by as much as a year from the publication date. In the publish or perish world that I was now a part of that year was important lest a competitive researcher achieved publication before I did. Not being very aggressive, I was beaten to publication more than once. It's like losing a championship game. Indeed to play at the championship levels is an honor, but . . . ! If you're beaten to publication all that is left for you to do is to write that you also can confirm what someone else has discovered.

After six years of facing college classroom demands and using textbooks, this immersion in the journals and written reports I found to be a very disconnected way to learn. Articles on the inversion that I read in preparation to establish this program of research on the high plateau of the Antarctic included articles about inversions in the tropics as well as in the polar regions. Wind under the canopy of trees in the rain forests was of value. Some reading came from in-house publications of the many government agencies, military publications such as a report from the White Sands Missile Range, reports of an unclassified nature as well as secret documents. Some material could be acquired only by interviewing a military scientist with information given out only on a need to know basis.

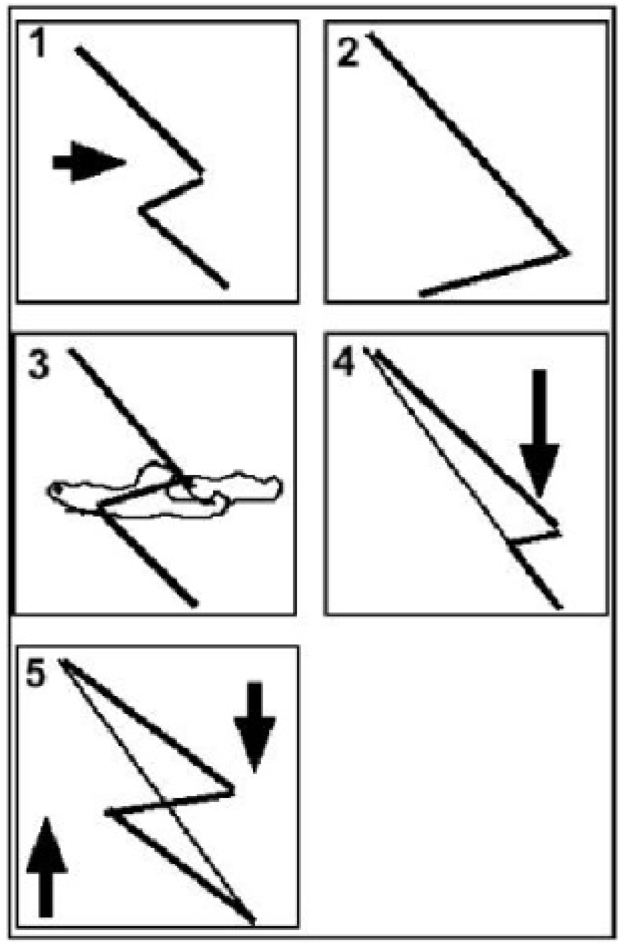
What were simple or at least straight forward equations now all had to be revised for computer applications. Again the military led the way. Where the Weather Bureau scientists used, as an example, the Smithsonian Meteorological Tables, all such tables had to be reduced to computer usable formulas. At the forefront of discovery and research, where there were no answers, many of the tables were strictly empirical and derived equations based on theory had not yet been discovered. This fudging by forcing equations to fit, after being so carefully trained by Prof. Lettau, who cautioned about mere empirical connections, I found to be most unsettling. It turned out that Prof. Lettau used such empirical tricks as well, but as a student of his I never knew it.

His warning still has a powerful imprint. "Ten new theories about any given subject are generated every year. Only the one that is connected to nature theoretically derived from first principles of nature such as the laws of conservation of energy has the potential of correctly describing nature." In a briefing I had at Langley by a science expert from the CIA on Soviet science, I learned Soviet science was primarily little more than this empirical curve fitting. In the cold war race to lead in any and everything the Soviets believed correlations emerged most rapidly by these empirical methods. The little graph displayed here shows this problem. The dots display real data. Three different "laws" in equation form as graphed exactly fit. Nevertheless if a scientist followed either one he or she would arrive at very different predictions.



Reading several hundred authors, many of whom wrote multiple papers on the same topics, all disconnected as each scientist sought in academic freedom what alone interested him, was simply overwhelming. Concerning temperature inversions, I learned that there were at least five different kinds. I had to begin with the normal atmosphere in its simplified form. Yet nothing about the atmosphere was simple. It was a gas, a mixture of gases. Some of these gases were interacting in very complex chemical ways that included energy exchanges. I would ignore these. One of the gases, being water vapor, underwent several phase changes frequently, giving very large energy changes that simply could not be ignored. Once on the Ice, I realized these energies had to be ignored anyway.

As a child I learned that gases filled their container. The atmosphere had no cover to its container but is held by gravity on a spinning globe. As a result very complex relationships developed between height and the density of air as well as its pressure and temperature as the air thinned to nearly nothing at the top of the atmosphere. In fact, all the undergraduate laws for gases learned in chemistry were inadequate in this open beaker of air without laboratory thermostats. Instead, in its simplest adiabatic (non-heating) state, which is not simple in the least, the interrelationships between temperature, pressure, volume, and the amount of molecular mixture present yield a lapse rate (change of temperature with height) of about -5.4 Fahrenheit degrees per one thousand feet (-9.8 C°/Km). This means at normal mid-latitudes near sea level the temperature is 5.4 F ° cooler one thousand feet above the observer on the ground. This cooling with height above the ground is normal.



An inversion is the opposite. In a temperature inversion the air becomes warmer with height for a myriad of reasons. I will list five here. Each square represents a graph showing the relationship of temperature with height where temperature is the horizontal axis and height is the vertical axis. The first square shows an inversion aloft caused by the horizontal advection of warm air. This might occur when a faster moving warm air mass moves from behind a cold air mass and rides over it slightly.

The second square shows an inversion on the surface of the ground. The ground and the air immediately above the ground loses much heat through radiative cooling as happens most frequently during clear cold nights. The infrared radiation or heat at and near the surface escapes the atmosphere without being reabsorbed by clouds or moisture. The result is that the surface layer is losing heat faster than it is gaining it and it gets colder than the air above it. One would be tempted to say that the warm air above it is hotter and since hot air rises it stays there but what is correct in a beaker in the closed laboratory is not true in the open atmosphere. The warmer air through the complex interplay between temperature, density, and pressure is at equilibrium and without heat loss so it stays put.

The third square shows an inversion caused by clouds. Most often the heat given up by the condensation of water vapor to form the cloud is considerably large and warms the air above the cloud. The interface between the two masses of air, air below and air above the cloud, shows the discontinuity as an inversion.

When air aloft is forced to sink, such as air moving over a mountain range sinks on the leeward side, it warms adiabatically. In square number four, the thin line shows how the temperature changed with height before such a sinking. As the air sinks, if its temperature, pressure, and density changes without heat being added, the temperature naturally rises. The very lowest level of air may not experience the sinking that the upper layer of air did, thus its temperatures remain cooler. An inversion between the stagnant surface air and the sinking air aloft occurs and marks the discontinuity.

In the last square, number five, I show a thin line that might represent a temperature profile before the formation of another inversion aloft. In the case of very turbulent mixing, where the air on the ground is carried aloft and the air aloft is rapidly pulled down, an inversion frequently forms. Again the explanation without additional heating occurring from another source is that air forced to rise adiabatically will get cooler according to the interplay between pressure, temperature, and density. Air aloft forced to sink without heating from another source will rise in temperature by the same interplay of temperature, pressure, and density. An inversion then forms in between.

The inversion of my pursuit in the Antarctic, which Lettau and Schwerdtfeger labeled the Great Inversion, was of the radiational cooling type in square number two. At the small station planned as Plateau Station many balloon launchings were not possible. I had been told and I planned to take measure of the Great Inversion by selecting the time of ideal inversions and concentrating on the changes by launching a series of balloons each carrying a radiometersonde aloft as close in time as possible for the periods of changing and ideal conditions. I planned several series of balloon ascents after the final sunset to observe the growth of the inversion with the final absence of the sun. Maximum radiational losses probably would occur then.

Serial launches near the first sunrise would be another good fixed time easily identified as a time for the Great Inversion to be monitored as the solar heating would break it up. The high polar plateau would also offer many times of extreme cold when the Great Inversion would be at its greatest.

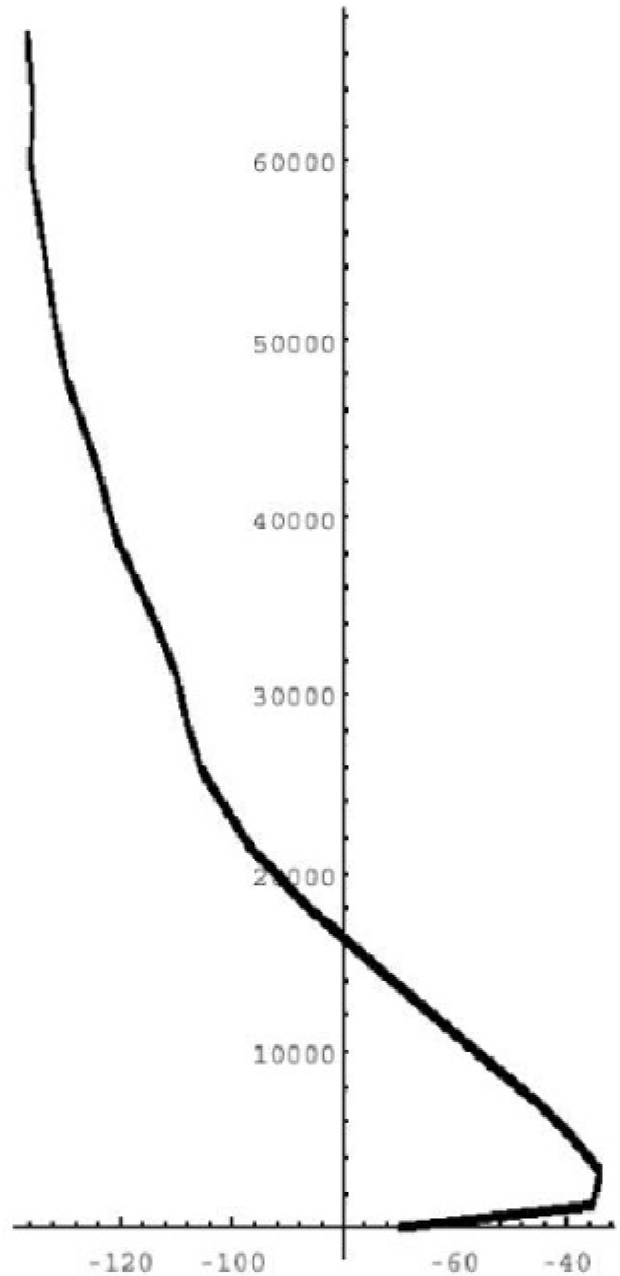
But these other types of inversions should also be present in Antarctica. Weather systems have been known to penetrate across the extremely high plateau. With these invasions of warm air, inversions from horizontal advection and from violent mixing should occur. These times also needed to be reserved for serial balloon launches of radiometersondes. Clouds were known to radiate considerable heat as they migrated across the severe cold icecap. These cloud invasions with storms or without storms were also to be monitored by multiple balloon launchings.

Still other times for such serial balloon ascents were times of increased surface winds or sudden deceased surface winds. These changing weather patterns hopefully would be predicted by men at the International Antarctic Analysis Center (IAAC) in Melbourne, Australia. Mort Rubin had promised to seek out that help and learn how I might be informed such a great distance away. These were my plans. My crash course of disconnected journal readings was beginning to take on meaning with these plans.

Ed Flowers was the best of the professional staff for my personal development. Reviewing my criterion for launching balloons to monitor temperature inversions, Ed told me that unless I had confirmed cooperation in writing from IAAC, he doubted if they would be concerned with my research interests. What about the U. S. Navy at McMurdo? Ed again doubted cooperation. Besides, Plateau Station's position planned on the high plateau in East Antarctica was right under the most intense aurora that usually disrupted communication. Ed Flowers felt that all my balloons should be reserved for the radiational cooling formation of inversions. These indeed were at the center of interest of the proposals from Lettau and Schwerdtfeger.

Kirby Hanson did an intensive study of the inversion at Amundsen-Scott Station (South Pole) using standard equipment in the winter of 1958, the last part of the IGY. Kirby launched a Rawinsonde twice each day, once at 0000 Z and once at 1200 Z. (At the South Pole there exists no meaningful local standard time. Time, Zulu, is the time of Greenwich Mean Time.) A Rawinsonde was a standard radiosonde that monitored temperature, pressure, and humidity. The RAWIN part of the name stood for RAdar WINd since a radar tracking antenna followed the balloon ascent and its signal then was computerized to read out the wind pushing the balloon and altitude of the balloon's instrument package. At Plateau I would not have the luxury of RAWIN. It demanded too much electricity. I would be tracking balloons manually using theodolites and would need the assistance of a second meteorologist. He would be my senior and take responsibility of the radiation programs from NLABS. That person probably would be Dalrymple as the lure of the Antarctic was in his blood.

The latest standard Rawinsonde data was from the year 1964. By 1964, big federal dollars of IGY were no longer were available and only one rawinsonde launch was made per day. The inversion in its strongest form occurs during the polar night so that the time becomes less important with midnight having the same darkness as noon. An average composite of all rawinsondes launched during the month of July in 1964 is given in a graph here. But the inversion, though a dominant feature, is hardly seen with detail.

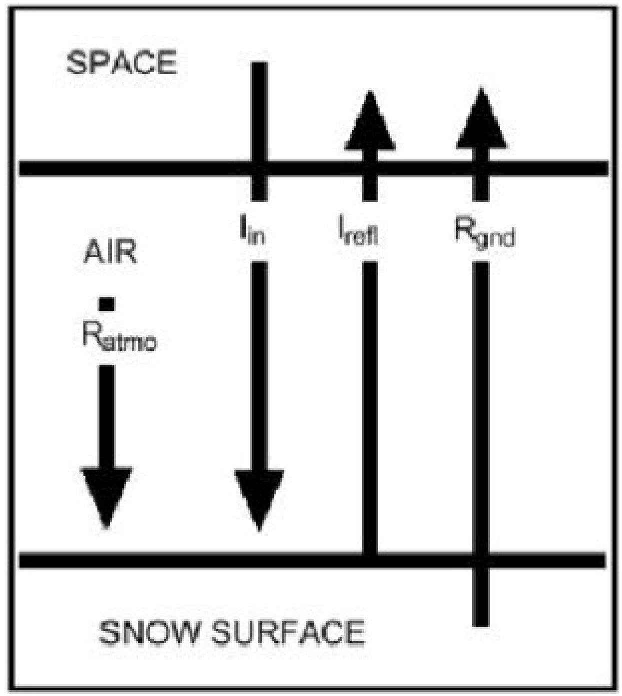


Amundsen-Scott Station (South Pole)
Latitude 90º South
Altitude 9,800 feet (2800 metres)
Average for July, 1964
Abscissa: Temperature (ºF)
Ordinate: Height above the snow (feet)

The support staff of Polar Met (I think Gertrude Sohns) found Kirby's data. Kirby approached the problem from its fundamental causing principle - radiational cooling. Kirby recorded the radiation balance day-by-day for his year of the IGY.

The sole source of energy for the earth is ultimately the sun. Thus a radiation balance study must begin with solar radiation. Meteorologists of IGY vintage and many years following considered such radiation to be in the form of waves. This wave concept has become outdated. Quantum physics, which rejects waves, today can no longer be ignored as it was. However the process of circumventing quantum physics, particle theory of radiation, could be justified along the same track of thinking that one can learn a lot about the effects of water waves even though most of us know those waves on the beach are really a composite of combined action of billions and billions of H_2O molecules. Thus an outdated system of thought remains very much alive even as I write. As waves carry energy from the sun, that energy comes as short waves, some of which our eyes detect as visible light. The first form of this energy Kirby identified and measured as incoming solar (sun + sky) radiation, I_{in}, incident on a horizontal surface near the snow surface. We see the brilliant sun light but it looks yellow because some of the energy is scattered as it comes through the air. The scattered energy we see as blue coinciding with those waves of energy that are kicked around by the air molecules. On a planet with different gases the sky would appear a different color.

In the Antarctic on fresh snow very much of the short wave radiation is reflected right back into space. The blue and other scattered energies are also proportionately reflected back into space. Kirby identified these energies as solar (sun + sky) radiation, I_{refl}, reflected from the snow surface. Now, for the South Pole in July neither of these two forms of short wave radiation is present.



Once the earth receives the short wave radiation, the energy is absorbed and reradiated to space as long wave radiation. As energy, these long waves must balance exactly as equal amounts of energy to the energy from the short waves or else the planet would literally burn up. The air over Antarctica holds considerable heat in this long wave radiation form and that is radiated downward to the snow surface. Kirby called it atmospheric (terrestrial) radiation, R_{atmo}, incident on a horizontal surface near the snow surface. During the polar winter night this is the chief source of heat energy. It obviously comes as secondary energy initially received from the sun at non-polar latitudes, absorbed and converted to convective energies before it invaded the Antarctic.

The last form of long wave radiation that Kirby identified was black body radiation, R_{gnd}, from the snow surface. Everything, including very cold snow, has received some heat energy in the form of short and/or long wave radiation. So it does not burn up, it also radiates long wave radiation. This the cold snow continually does in winter or summer.

For July in 1958, Kirby measured these four forms of radiation, really only the effective last two, both long wave forms of radiation. Over a very long time over all seasons the four forms of radiation would algebraically add to zero. The short wave radiation forms are not present in the polar night. On clear nights (24 hours long) however, more black body radiation is radiating out of the snow than is coming in from the long wave radiation of the atmosphere. Kirby Hanson measured that loss as -56 Langley's per day (calories per square centimetre per day) or -27 watts per square metre, the radiation balance (net flux) through a horizontal surface near the snow surface.

It is this steady net loss of energy, where the air layer near the snow surface loses energy without gaining more, that causes the formation of the inversion. Over the long polar night only storms bring in new quantities of energy. Without them the inversion builds and builds. Taking the average results of all of Kirby's semi-daily rawinsonde launches during the month of July at South Pole in 1958 presented an inversion with a surface temperature of -67 ° F (-55 ° C) and a maximum temperature of -38 ° F (-39 ° C) at an altitude of 1986 feet (605 metres) above the surface.

That is an inversion of nearly thirty Fahrenheit degrees in two thousand feet. Incredible! A typical inversion over the Midwest farm fields rarely measures more than five or six Fahrenheit degrees. On the high plateau of Antarctica the Great Inversion was expected to be even stronger. Kirby measured an extreme inversion on 31 August 1958 at 1200 Z (still the polar night without the sun at the South Pole) with a surface temperature of -96.9 ° F (-71.6 ° C) and a maximum temperature of -42.3 ° F (-41.3 ° C) at an altitude of 2490 feet (760 metres) above the surface, an inversion of nearly 55 F ° of temperature change.

The height of inversions in the Antarctic of 2000 to 3000 feet suddenly struck me with a little fear. A typical radiosonde launch flew through the inversion at about a thousand feet per minute. Schwerdtfeger's hope of slow rising balloons remaining in the inversion for thirty to sixty minutes suddenly was an uncertainty. Before my involvement with this project, nothing had been done as far as designing a new system for carrying the instrument package more slowly. My only hope was to under inflate the standard balloon so that a lack of helium would give the balloon a very weak buoyancy. Time would tell this outcome. I started to appreciate Dalrymple's view of being second at a new research station. In all of Kirby's launches the top of the inversion was the first data point of the record above the snow surface. There was no chance for viewing the details of the inversion with such ascents. A way to slow the balloons had to be found.

One morning, still in July, Bill Weyant came to my research cubical and announced that I had to go with him right away. He had neglected to introduce me to people in another office and it was very political. I should smile a lot, but let him do all the introductions as well as all the talking. We still were located in the old Mexican Embassy, in the central house of that Embassy. Bill took me over to offices located in the outer buildings surrounding the central house so we did not have to go very far.

On the quick walk across the court yard I learned about this other polar office, but did not piece together all the ramifications of the politics until several months later. We were walking over to the Overseas Operations Division (OOD) of the Weather Bureau. During the IGY all the chief research scientists together with administrative personnel worked together to achieve the impossible in distant and drastically different climates. While projects were new and their inventors were active with the projects, the excitement of going to the field was infested with the lure to all involved - researcher, clerk, technician, administrator - all alike.

This was the kind of bonding that occurred between men like Bill Weyant, Harry Wexler, Mort Rubin, Paul Dalrymple, Kirby Hanson, Burt Crary, and Ed Flowers. I suspect it was this bonding around a life threatening frosty existence in pursuit of discovery that kept a man like Fred Fopay loyal to his task as well as the loyalty of his polar friends protecting his government position. I was beginning to notice all these men wore only penguin tie clips on their ties.

After the IGY, the United States had many political reasons to maintain a presence in the Antarctic and that need permitted continual scientific studies in the region. Those men whose careers accelerated with the explosion of new ideas that emerged for the IGY data suddenly preferred the comfort of the research labs or the international service. Being part of the IGY was a union card for many an administrator who was suddenly promoted on the basis of his scientific discoveries and not necessarily on his administrative skills. Indeed, that is exactly what my office was - a research office headed by a Chief who had served several times in the Antarctic but probably no longer desired to get cold. Others, like Herb Viebrock, Bob Becker, and Martin Predoehl, preferred to do the research in Washington D. C. and let others get cold getting the data for them. In fairness to these men, I noted that they did not wear penguin tie clips. They were outstanding researchers. Some men were gifted for discovery from behind a desk. Others were gifted for discovery in the field. There also were many pressures to remain at home and publish. As for me, at the age of twenty-five and single, gifted or not, I could not wait for the adventure. Indeed, for me just flying to Washington D. C. was an adventure.

Bureaucratically then, the research part of the IGY scientists of the Weather Bureau formed the Polar Meteorology Research Group of OMR, which then was transferred to the Research Laboratories of ESSA. Their former colleagues, who became experts at transporting the research staffs and material to any part of the world was made part of the Weather Bureau's Administration. Jealousies arose on both sides. OOD was closer on the top of the power chart. Vaughn Rockney, Chief of OOD and of equivalent rank to Bill Weyant, was only two offices away from the Director of the entire Weather Bureau. As Chief of Polar Met, Bill Weyant served the Director of the Atmospheric Analysis Lab (AAL). He in turn served the Director of The Institute for Atmospheric Studies (IAS). He in turn served the Director of the Institute for Environmental Research (IER) and he in turn finally served the Head of the Weather Bureau who made a much larger government organization called ESSA.

The short of it was that at one time Bill Weyant of the research world served side by side with Charlie Roberts, now Polar Specialist of Overseas Operations - Polar Section (OOPS). Before all the political shifting, both men did original research but now they were bureaucratically held apart, each forced to redefine their turf, which rarely led to overlapping cooperation. Suddenly I was in the middle. All of my travel and the placement of my equipment - that really turned out to be their equipment - would be placed into the field by OOPS. Yet the "glory" of going to the high plateau and being "first" into this unexplored region would be given to a researcher who, now, normally would be spending his time behind a desk.

For the sake of the success of the Great Inversion study we needed the full cooperation of OOPS, and yet the study demanded a greater theoretical understanding of the problems in the field that faced the observer. OOPS since the IGY had filled the many observer positions in both the Arctic and the Antarctic with well-trained technicians, not men of highly theoretical schooling. Yet it was their feeling that most theoreticians lacked the nuts and bolts understanding of the equipment and the experience with the severity of the polar environment. In my case that was exactly true. I had no experience in the polar regions. From Madison, Wisconsin, I had been as far north as Minneapolis several times. I did serve the Weather Bureau as far south as Chicago.

Bill introduced me to Vaughn Rockney. All that Vaughn said to me initially was nothing. In front of me, I believe to embrace Bill and to make me squirm, he angrily protested saying that all Bill needed to do was to give him the program of research he needed and Vaughn could teach any of his "polar rats" even the most complex observations. And they all had been to both poles several times. "Son, are your fillings going to stay in your mouth? Where you're going it gets cold!" Bill Weyant promised he would arrange for my visit to a dentist familiar with Vaughn's people's needs.

We really came to see Charlie Roberts. He was busy. Both Bill and I had a mountain of work to do. I was learning all the needed bureaucratizes. I had much equipment to line up. Various licenses were required to move United States equipment across international boundaries and I needed to be applying for them. I suggested to Bill that we could leave and I could periodically check back. The office of Charlie Roberts was less than a city block away from my own. Bill knew the situation and knew this was bureaucratic chew time. Each office had its turf and its bird calls to claim it. We needed to hear it.

In about an hour, Charlie Roberts, a very pleasant person with the rugged look of a polar regular welcomed us to his office. After a few short pleasantries, he took us to another building and a comfortable lounge to have a cup of coffee, and I listened to Bill and Charlie chat over their many experiences in the Antarctic. I think Charlie wintered over twice and visited Antarctica at least six times. He had been Station Leader at South Pole and at Hallett. He wore a penguin tie clip. He was very glad to meet me. He seemed excited to involve a young man like me early enough to get a researcher

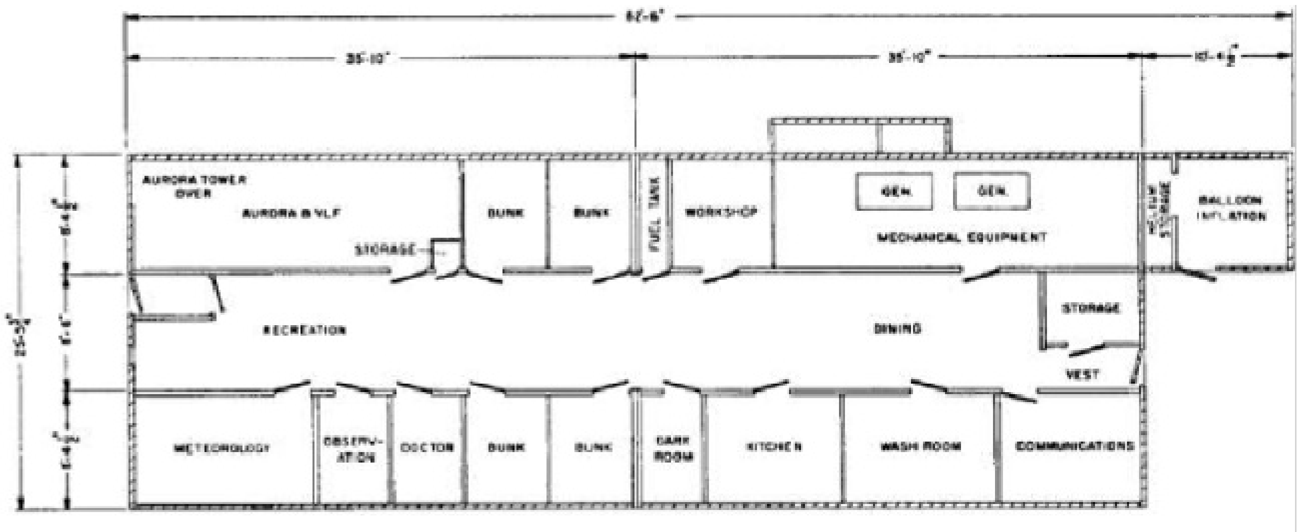


Fig. 1. Plateau Station Floor Plan

into the field before I got comfortable with a warm research life. It was better for both the research and the observer's camps. I knew I had a friend. At Charlie Roberts' urging, I knew I should bring all my requests to him. He would take care of all the exporting licenses and shipping needs. He spared me several years of work over the next several months.

What about Vaughn? Why the bitterness? "Oh don't worry about Vaughn. He's just a seal who lost his breathing hole and has to chew a new one. Before spring training is over, he won't know you're with Weyant and he'll count you on his team."

The expected time of departure for the Ice was shortly after Thanksgiving Day. With less than four months before all material and personnel would begin their respective departures from the states Bill Weyant frantically interrupted my library studies and told me to hustle over to the National Science Foundation (NSF) and see Bill Austin - something about a balloon shelter.

It was a brisk eight block walk from 24th and M Street, down to Pennsylvania Avenue, around the Washington Circle and continuing along Pennsylvania Avenue to 19th and G Street North West. These circles in Washington D. C. were originally designed for military control of the federal city back in the beginning of the nineteenth century. These automobile grid lock days they were a real nightmare. Imagine eight roads coming into the circle at one time. Indeed, my walk to NSF was considerably faster than if I drove a car.

I met Bill Austin with his assistant Jerry Huffman and we went to work immediately. Bill Austin was an engineer and a Coordinator with the Office of Antarctic Programs of NSF. He and Jerry Huffman were involved with the design and the construction of the buildings for Plateau Station from the beginning of this project and were waiting for some long overdue information about balloons. I immediately received the standard chew out which I was beginning to understand as the turf marking practice. They of course were protecting their decisions on the construction of the buildings, especially as those decisions impacted on my research project. Someone should have given them dimensions of the weather balloons a long time ago. Some rough plans for the balloon shelter already were at the building site in Calgary, Alberta, Canada.

The balloon shelter planned at this point was to be a square room of eight feet four inches by eight feet four inches. How big were my balloons? Deliberately placed on the defensive, as if the delay for this information was my fault, I had no choice but to accept their suggestions unless the balloons were considerably larger. What were the dimensions of a weather balloon that carries aloft a radio-sonde? That was not a statistic I learned in graduate school. May I use a phone? I must have called more than a dozen people in the Weather Bureau and no one would give me the exact dimensions of such a weather balloon. In fact, most of the research men I talked to had not used one in a very long time or did not ever use one. From photographs found at NSF we agreed the original dimensions were adequate. In any case, I would be under inflating these balloons so that they would not need quite as much room as any standard balloon.

We agreed that the roof could be ten feet above the floor or less. An accordion folding rooftop door could be installed instead of a side door. Both Bill and Jerry thought, and I agreed, such a door would permit a launching direct from the balloon shelter sparing the intense chilling of the men most likely assisting with the launch. The door could be quickly opened at the last minute allowing the person in the balloon shelter to remain warm and then a rapid closing of the door after launch would fit the necessity of conservation of heat for the entire camp. These were some last minute changes. They couldn't promise anything at this late time but they would try.

I left NSF worried that my entire inversion study was in jeopardy. In time I learned that this was standard operating procedure for the government. It also protected everyone from most probable blame for failure. Scare the other guy. Make him think it's his fault. In time I also began to mistrust my Chief. Either Bill Weyant was resting on his most accomplished successful past or he was becoming too involved with running bridge tournaments on the East Coast. I think things such as the dimensions of weather balloons and plans for balloon shelters should have been worked out many months before.

I learned from Bill Austin that the plans for the Plateau Station buildings began in January of 1965. NSF plans were given over to the U. S. Naval Support Force, Antarctica, where air officers performed the first axing of the program. The first plans from NSF included room for six civilian scientists and six military support personnel. However, there simply were not enough flight hours by LC-130F cargo airplanes of the Air Development Squadron Six to airlift such a planned station as far inland and to such an extreme altitude as the high plateau of Antarctica during the window safely afforded during the summer. The Naval Facilities Engineering Command (U. S. Navy bureau of Yards and Docks) trimmed the station building to accommodate only eight men.

These airlift requirements or limits of the Navy were the driving force behind every decision and the decisions were coldly carried out without much input from the scientific side, which was the chief reason for building the station in the first place. You can imagine the difficult role Austin and Huffman played trying to defend original plans of idealistic scientists who pared back their programs from the start over the eternal problems of inadequate funds. You could understand their effort to put you on the defensive a lot easier when you saw the bold, and at times seemingly not well thought out decisions of the support forces changing the programs and asking the NSF coordinators to smooth everything over.

Only eight men for Plateau Station meant only one meteorologist. Did Weyant know this? Did Dalrymple? With only one meteorologist, with the uncertainty of a balloon shelter, was there going to be an inversion study? Would it be worth anything if inadequately supported?

Delays in designs of buildings always occur. The single largest delay occurred as Lt. Commander Paul Tyler demanded studies on the potential need for pressurized bunk rooms. The anticipated altitude for the placement of the buildings was 12,000 feet above sea level. Making the decision more complex was the spin of the earth that made polar atmospheric pressure even lower and exaggerated extremes. The oxygen content over the polar plateau could be equivalent to altitudes anywhere between 14,000 and 18,000 feet - only about half the oxygen at sea level. Paul Tyler decided to provide oxygen if needed for the builders of the camp on station and, following the lead of the Soviet Union at Vostok, Antarctica, demand high physical and psychological standards and high lung capacity for the winter personnel. Also acclimatization in stages of these men who would winterover would be necessary by having them train at the South Pole before being taken to Plateau Station.

Here is where the U. S. Navy Support Force, Operation Deep Freeze, rightfully knew its business and I'm here to tell this story because of their skill in spite of all the complaints I, as a scientist, had. Saving and maintaining life is what the Navy took very seriously and did not let over enthusiastic scientists' desires over their personal research projects get in the way. I did not come to this view until twenty-five years after leaving the Antarctic.

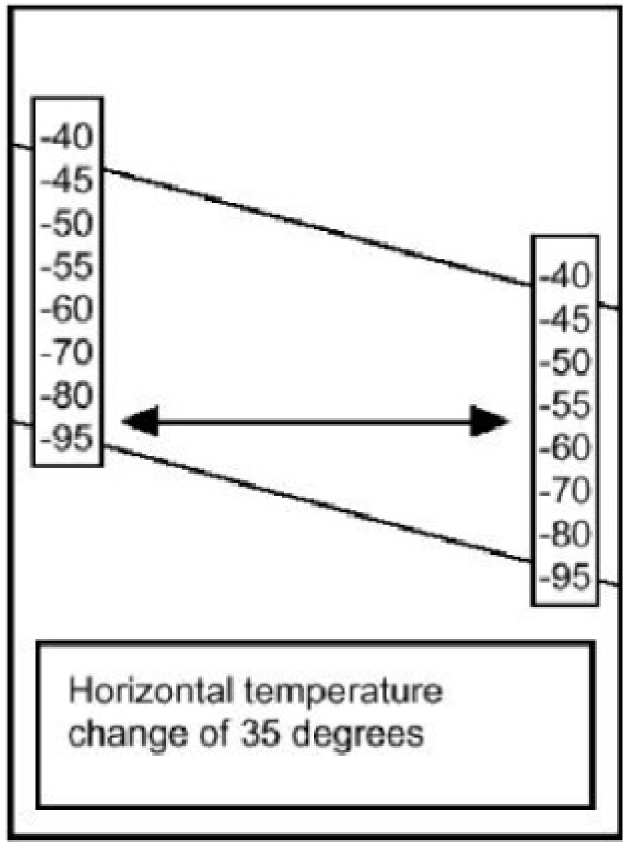
By May of 1965 Ensign David Ramsey, CEC, USNR, received his orders to be the Officer-in-Charge of construction and pulled together his team of ten men of the Mobile Construction Battalion Six. Their headquarters was the naval Construction Battalion Center in Davisville, Rhode Island. They would train as much as possible in theory first and then be taken to Calgary, Alberta, Canada, to practice building the prefabricated buildings manufactured by the Alberta Trailer Company. Obviously, here in Washington D. C. in August, redesigned balloon shelters seemed a hopeless idea. Hope for effective changes emerged once I realized that the main building of Plateau Station was made of four fully outfitted vans linked as two vans on each side of the building and separated by a permawalk in the middle and roofed over. The central permawalk area housed the recreational and dining areas. Science labs, bunks, kitchen, generator rooms, and a medical office were preoutfitted in the vans. The balloon shelter was an add-on at the end of one of the vans. Some adjustment emerged as possible. In my mind balloons began to fly again through the inversion.

Nothing happened in Washington D. C. without pain and frustration. Power and favors were the only things that seemed to work and I was a pathetic rookie. Yet, the cult that wore penguin tie clips had an unwritten bond of support among themselves. The mention that I was working for Weyant or doing work for Dalrymple opened a lot of doors. It was a different world than academia. Names like Lettau and Schwerdtfeger meant little here. Kirby Hanson was a name and the name of Mort Rubin would get a little bit of attention. "Is this Weyant's boy?" With these words, Burt Crary, at the time Deputy Division Director of The Division of Environmental Sciences of the NSF and Chief Scientist of the Office of Antarctic Programs, summoned me into his office at NSF before Austin let me go.

"Are Lettau's and Schwerdtfeger's ideas worth anything?" He's asking me? These men were my professors. I was no judge of my professors. I was a disciple! I had nothing to say. Crary filled the silence, "Well, Dalrymple trusts them." He talked quite a bit about his exploits establishing Byrd Station for the IGY and then in that low quiet voice where he let you think he was letting you in on a secret Burt Crary told a story where he suddenly found himself stranded on a small ice floe moving out to sea at Little America with a killer whale in pursuit looking at him as though he were a tasty seal. After a heroic rescue mission mounted by the Navy, the dried out and warmed Crary mused with the Navy chaplain about prayer. No, Crary uttered no prayer. All he could say as he watched his ice floe move farther and farther away from his rescuers was "Shit. Oh dear. Shit. Oh dear."

With sins of omission I said nothing. I even laughed. Inwardly I was troubled. I stood before one of the chief scientists in the world who was accepting me as a friend, or at least as a young man recommended by very close friends. Why this unneeded put down of religious belief? I was in nearly continual prayer to be accepted on this assignment. I would be in fervent prayer over our safety and life in less than a year. Where was the joke? Much of the research over the Antarctic dealt with evolution and very few of the scientists I met professed any religion at all. Most had this kind of disdain. I was taught in my religious background at parochial schools that the best scientists all believed in Creation. I was taught very wrong.

My inversion wind study was central to the research as I saw it. I intended to work out much of the mathematical modeling before going to the Ice. I never anticipated the overwhelming logistics taking the needed observational equipment to the field. Also unanticipated was the politics and turf defense within the government. The only greater turf defenders more troublesome were those later in my life that practiced their odd trade in the church. But the greatest shock slowing my youthful enthusiasm was the difference in schools of meteorology between the East (schools like Penn State, NYU, Boston U, and MIT) and the Midwest (UW-Madison and U of Chicago).



As a rookie, I was encouraged by and wanted to have my theoretical work looked over by more seasoned colleagues. Trained to develop formulas founded on theoretical principles of standard acceptance I started with functions expressing radiational losses within inversions and horizontal temperature differences caused by regional inversions father down slope. This horizontal temperature gradient would cause a force changing both the wind speed and direction with height. This change I desired to be the first person to measure. I desired also to develop the mathematical model for this phenomenon first proposed by Lettau and Schwerdtfeger.

Most of my colleges at Polar Met and especially down the hallway at the Air Resources Lab led by Lester Machta, particularly Atmospheric Radioactivity Branch and the Atmospheric Trajectory Branch, most were trained at schools out here in the East. They all asked why I was approaching the wind structure over a glacier in such weird ways. Everyone preferred to examine these winds as katabatic winds. Most of them, while having heard of Prof. Lettau, didn't think much of the Midwest. "What could Wisconsin or Chicago offer besides milk and beer?"

Let me try to explain katabatic winds and their placement in the grand scheme of wind classification. Five chief forces are recognized for their strong influence on creating the wind structure. The first is gravity. The earth's gravitational force on the air as a substance with weight is a dominant force compacted by the severe cold and laying on a slippery ice slope.

Hydrostatic force, primarily differences of air pressure over different but nearby regions, is a major cause of all wind and easily recognized in mid-latitudes as causing winds around high and low pressure regions.

Frictional forces of the atmosphere are divided into two kinds and Lettau was an expert on both. The first kind of friction is identified as an internal force meaning a force governing the interplay between the various molecules of the mixture that is called air. Realistically, among meteorologists, this kind of force seemed to govern the small "eddies" of air that still moved in smooth flow lines.

External frictional force is visible in the wild churning turbulent eddies, most visible among the patterns of leaves in air motion or felt as strong buffeting rapid changing currents or air against objects.

The fifth and last force is the Coriolis force, a very misunderstood force. It is caused by the earth's spin about its axis. The Coriolis force is strongest at the poles and weakest in the tropics because the earth's spin is strongest at the poles and weakest in the tropics. It is hard to say with a straight face that this is because there is no spin at the equator. Really, more qualified, the spin around a flag pole pointing up while stuck into the ground on the earth's surface is what I am talking about. Try it. Hold a pencil, as a model flag pole, on a spinning globe on the equator and see that the pencil is not spinning but tumbling. That is exactly what air does or does not do. It definitely does not spin near the equator in the tropics.

The same pencil held at the North Pole does not tumble but, in fact, spins as the earth does. Air, not attached very strongly to the ground as a flag pole, is free to spin on its own and thus spins the opposite way. Thus a high pressure region sitting over the North Pole spins clockwise, opposite the way of the earth's spin.

By moving your pencil to the South Pole but keeping the globe spinning in the same direction, you will notice the pencil and your globe spin the opposite way from the way they spun when the North Pole pointed up. This time an unattached high pressure dome of air over the South Pole will spin opposite the clockwise spin of the earth and spins counter clockwise. This opposite spin in the air over against the spin of the planet is called the Coriolis force.

Contrary to many nonsensical textbooks written for the liberal arts student, who shuns the scientific knowledge of the world around him or her or is only interested in fifteen second explanations, the coriolis force cannot be detected in a toilet bowl or bathtub. The Coriolis force is a force of a very large scale of the order of the planet. It also requires a very long time to set up a balance of forces in the air flow over the earth. Time frames of twelve to twenty-four hours are required. Thus, wind systems of short time duration, such as tornadoes, spin both clockwise or counterclockwise in both hemispheres.

In a sense mathematical modeling is simple. All that is required is to set up equations with all the forces listed as terms. Complete equations for atmospheric motion, easy to string out, are just impossible to solve. They involve too many terms with too many derivatives at too high of powers. What can be comprehended in the mind cannot be rigorously carried out with the best of mathematics then, today, or any time soon. The role of the theoretical scientist is then to reduce the terms and simplify the equations with meaningful and realistic assumptions in order to arrive at equations that can be solved or at least approximated with iterative systems on high speed digital computers.

The most common meteorological equation is the geostrophic wind equation that assumes all terms to be negligible except the pressure gradient force and the coriolis force. These two forces are then set equal to each other and they effectively describe the high speed steady winds between a high pressure region and a low pressure region such that if a high exists in South Dakota and a low exists in Wisconsin, strong winds from the north pour over Minnesota.

$$f\,\vec{k}\times\vec{V}_g = -g\vec{\nabla}_p z$$

This vector equation really describes all three dimensions. Let me reduce it to the horizontal equation for the Minnesota scene just described where v is the wind from the North and the pressure gradient slopes upward toward the West.

$$f\,\vec{k}\times\vec{V}_g = -g\left(\frac{\partial z}{\partial x}\right)_p$$

I believed katabatic winds were of a short time duration and only a local wind structure. In that case coriolis forces would be negligible. Local winds without coriolis forces were identified as Eulerian winds. However, equations for Eulerian winds, which were mathematically less complicated than the general wind equations, still had too many terms.

Wind systems with one force term balanced against only one other term were potentially solvable with integral calculus. These two term equations were sought after with great intensity. Obviously the first scientist to find a solution that also matched the observable in nature, no matter how trivial, would be a success with publication. Wind systems governed only by the pressure gradient and frictional forces were classified as antitriptic winds. Some antitriptic winds were known as foehn winds, chinook winds, bora winds and the zonda. Most of these winds, well understood, were due to sinking air on the leeward side of mountains and were abnormally warm dry winds resulting from the adiabatic warming of the air as it sank and changed its density, pressure, and temperature.

Similarly, katabatic winds were considered to be a balance between the force of gravity on the air made heavier by the extreme cold as well as the addition of snow crystals entrained into the air stream by frictional erosion from the glacial surface and the frictional force on the air.

Following first principles, as Lettau taught, a system of three differential equations could be hypothesized for katabatic winds.

(1) Continuity equation

$$\vec{\nabla}\bullet\vec{V} = 0$$

(2) Momentum equation

$$r_0\frac{d\vec{V}}{dt} + \vec{\nabla}P + \vec{k}\left(gr\right) = \vec{F}$$

(3) Energy equation

$$\frac{dq}{dt} = \frac{\partial q}{\partial t} + \vec{V}\bullet\vec{\nabla}q = \frac{H}{C_p r_0}$$

Continuity required that all nearby changes in any field of variables were all interrelated. In particular, there could be no increase or decrease of mass. There were no abrupt changes of the coordinate system used.

The momentum equation said simply that all momentum changes were balanced and that the only loss of momentum in any and all transfers of momentum to different parcels of air was lost to friction only.

Likewise the energy equation claimed that all energy changes were in balance. This is the time honored law of conservation of energy.

A list of parameters:
H = rate of heat per unit volume by the process of heat of diffusion.
V = vector of motion.
P = atmospheric pressure.
k = vertical unit vector.
F = a vector of frictional force per unit volume.
ρ = density of air.
R = gas constant for dry air.
C_p = specific heat of dry air at constant pressure.
θ = potential temperature, where .

$$\theta = T\left(\frac{1000}{P}\right)^{\frac{R}{C_P}}.$$

In words, potential temperature is the temperature a parcel of dry air would have if brought adiabatically from its initial state to the standard pressure of 1000 millibars.

These equations, continuity, momentum, and energy could then be expanded into their respective component forms.

$$\frac{\partial u}{\partial x}+\frac{\partial v}{\partial y}+\frac{\partial w}{\partial z}=0$$

$$\rho_0\left[\frac{\partial u}{\partial t}+\frac{u\partial u}{\partial x}+\frac{v\partial u}{\partial y}+\frac{w\partial u}{\partial z}\right]+\frac{\partial P}{\partial x}=F_x$$

$$\rho_0\left[\frac{\partial v}{\partial t}+\frac{u\partial v}{\partial x}+\frac{v\partial v}{\partial y}+\frac{w\partial v}{\partial z}\right]+\frac{\partial P}{\partial y}=F_y$$

$$\rho_0\left[\frac{\partial w}{\partial t}+\frac{u\partial w}{\partial x}+\frac{v\partial w}{\partial y}+\frac{w\partial w}{\partial z}\right]+\frac{\partial P}{\partial z}+g\rho = F_z$$

$$\frac{\partial \theta}{\partial t}+\frac{u\partial \theta}{\partial x}+\frac{v\partial \theta}{\partial y}+\frac{w\partial \theta}{\partial z}=\frac{H}{C_v\rho_0}$$

Usually a system of equations as this needs to be reduced considerably before integration is possible. For katabatic winds, one would limit the development of this system of equations only to the fall-line or down slope line, dropping all other components. This procedure would constrain the solution to only two dimensions, the vertical and in the direction of the fall-line. These simplifying constraints were a major reason for going to the polar region with its long polar sunless night. The mathematical demand for steady state conditions,

$$\left[\frac{\partial f}{\partial t}=0\right],$$

were met when solar heating over short periods of time did not exist during the long polar night of many months and thereby greatly simplifying the mathematical model.

I'm not trying to solve things by writing this. I only wish to outline the attack of research. It certainly was more than just reading books in the library. Many times a system of equations as I've just given takes several weeks, even months, to develop. Before solutions are attained, many dead ends are followed. One such dead end for me took me down a path for nearly nine months and almost a hundred hours of computer time, time that my grant simply did not have, time that was stolen during the midnight hours when normal people slept and computers stood idle. Then came the shock that equation six is identical to equation 42 and I had followed a long but continuous circle. No solution possible! What do grown, intelligent, and stable scientists do when this happens? This scientist got drunk!

The large scale pressure changes due to traditionally forming storms and cold clear regions were ignored. Katabatic winds were too small a scale and too rapidly expended as an out rush of cold air down the glacial slope to involve the Coriolis force. These winds were not the winds I was looking for in the Antarctic. I was trained by Lettau and Schwerdtfeger and as their disciple, I was committed to temperature gradients and thermal winds to explain the wind profiles of the great Antarctic temperature inversion.

Yet most of my new colleagues strongly advised that the problem of winds over a glacier had to be dominated by gravity alone and balanced against friction as the heavy cold air accelerated down the glacial slope. Herb Viebrock and Ed Flowers, trusted leaders at Polar Met, encouraged me to study hard and fast the possibilities of katabatic winds as a model for the winds formed by the temperature inversion. Real coastal data from the Antarctic and Greenland pointed to a katabatic solution - gravity winds and not thermal winds. Although Herb had not been in the Antarctic himself (no penguin tie clasp), he was well read in much of the polar literature. To young me he became another Lettau, always ahead of me in the reading race.

("Picking Ice for Domestic Purposes in Hurricane Wind [showing the high angle at which E.N. Webb leans into the wind]." Adelie Land. Photograph by J. F. Hurley, Photographer of the Australasian Antarctic Expedition, in *Home of the Blizzard* by Sir Douglas Mawson, 1915, Vol. I, p. 114.)

With his reading knowledge, Herb alerted me to meteorological details of which I was not familiar published in the descriptive volumes of an Australian expedition. While the world watched the English and the Norwegians race to the South Pole in 1911, the Australians were exploring more than a thousand miles of Antarctic coastline south of Australia and had three separate land parties exploring and mapping details from the south magnetic pole, then at about 150 ° East Longitude, to 90 ° East Longitude. Led by Sir Douglas Mawson from his base camp at Cape Denison on Commonwealth Bay, the Australians were confronted with the most severe surface winds ever recorded. These surface winds were heavy down sloping winds, somewhat warmed by adiabatic sinking of air but held cool by the large quantities of ice crystals carried along and eventually blown out to sea.

"The equinox arrived, and the only indication of settled weather was a more marked regularity in the winds. Nothing like it had been reported from any part of the world. Any trace of elation we may have felt at this meteorological discovery could not compensate for the ever-present discomforts of life. Day after day the wind fluctuated between a gale and a hurricane. Overcast skies of heavy nimbus cloud were the rule and the air was continually charged with drifting snow." (Mawson, *Home of the*

Blizzard, Vol. I, p. 111.) The highest steady wind speed ever recorded on the surface of the earth to this very day was recorded at this same Commonwealth Bay, George V Coast, where gales reached 200 miles per hour (*GUINNESS BOOK OF RECORDS*, 1992, P. 47)

Many of the east coast scientists believed that all down sloping winds on the Antarctic ice dome were katabatic, and stretching the definition of katabatic winds, they wanted me to include long time intervals for Coriolis effects on the katabatic winds. Lettau and Schwerdtfeger always spoke against simple katabatic theory and pointed to the unlikelihood of down sloping outward moving winds, which warm and dry out the air, to be able to sustain a polar icecap for the many thousands of years in such apparent unalterable stability. The transport of large quantities of snow outward from the icecap seemed contradictory. That is why they insisted on a new thermal wind approach where the inversion wind would make the air flow around the icecap and preserve it.

Confronting me was the very real gale and hurricane force winds down slope across the Australian camp. Further study also showed similar winds at the French base at Dumont d'Urville and at two other Australian stations, Wilkes Station and Mawson Station.

The library of Congress has everything. What other katabatic studies were available? The Russian experiences? Too late to wait for Israeli translations. Phone calls to the Russian Embassy. Yes, they had many researches on Antarctica and its weather. I could have them in several months. I wanted them yesterday, not soon or even now. Contacts with Russian scientists through Burt Crary were great helps. Being able to translate a little Russian also was a great help even if I needed a large Russian to English dictionary. I hated studying the Russian language back at school, but that knowledge was now paying off.

Astapenko's volume, *Atmosfernye protsessy v vysokikh shirotakh yuzhnogo polushariya*, was overwhelming. It covered everything about the weather in Antarctica. The Russians called the katabatic winds gravity winds and used a formula developed by L. V. Dolganov: .

$$V \cong 2\sqrt{SK\frac{\Delta T}{T_I} g \sin a}$$

S = the extent of the slope.
K = a coefficient of proportionality.
ΔT = the difference in temperature between air in gravity wind and the immobile state.
T_I = the absolute temperature of immobile air.
g = the acceleration of gravity.
α = the angle of inclination of the slope.

Although this was a strictly empirical formula, the Russians used it to forecast the severity of the gravitational katabatic winds between their several stations on the icecap going down slope from Vostok (inland as our proposed Plateau Station) to the coastal station Mirny. The Russians saw the snow surface winds increasing as the ice surface slope increased. Wind speeds were expected to be three times faster than the wind speed with normal pressure patterns. Although they did not experience the extremes that the Australians and French experienced, the Russians did indeed experience very strong winds, sometimes gale force, at Mirny, Molodezhnaya, and Novolazarevskaya. The countries of Belgium, Japan, and South Africa also maintained coastal stations in East Antarctica but as time ran out prior to my own departure to the Ice, I was overwhelmed and the meaning of their data was lost to me.

Questions of self doubt filled my mind. It was obvious that katabatic winds were a major wind feature for the coastal regions of the high plateau of East Antarctica. Was my college training at good old Wisconsin-Madison offbeat? How could I discover, and worse, publish, views different from "my" professors? Viebrock's encouragement always included statements about maturing scientifically. One needs to grow beyond his or her teachers. Often he spoke of PhD friends who still promoted the same ideas of their professors long after they had been awarded their degrees. How could that be? Loyalty had played no part in experimental science. And science is not static. If, after several years of research, one was still narrow to his or her teacher's views, it was doubtful that such a person was a scientist. More than likely even the research professor had changed views. Nonetheless, what was I to do about inversions and the corresponding wind profiles? I needed to observe them first.

I grew up in a home where there was a dining room chandelier with six light bulbs. Five were always loosened so that they would not come on when the light switch on the wall was turned on. A calendar was placed at the telephone and every call recorded to monitor the number of calls even long after the restricted number of sixty calls per month was dropped from the billing methods of the telephone company. We also had a three minute egg timer by the phone to limit calls to the three minute minimum charge even though no one ever called long distance. Suddenly having access to the government's WATS lines was difficult for me personally to adjust to. When talking to scientists from different parts of the country, I had to conscientiously think about not suddenly hanging up at the point of three minutes.

One long call was with a research scientist working for the Jet Propulsion Laboratory in California. We discussed at great length (hours) problems related to measuring moisture in an extremely dry climate. He was working with instrumentation to detect water vapor on Mars. Antarctica, as the world's driest desert, was a good natural laboratory for testing such equipment. The more I studied the limits of the humidity collecting carbon hygristor, a plastic resistor with a coating of ionized carbon on the surface that would collect molecules of water vapor from the air as a balloon carried it aloft, the more I was skeptical about its ability to measure the humidity in the inversion layer. At temperatures as low as -100 ° F, even if saturated, the air would be drier than the driest desert on earth. At the late date, now only four months from date of departure for the Ice, pursuance of newly designed instruments was hopeless.

We also discussed ways of using subtle light and polarized light to detect the presence of "dry-ice" crystals of solid carbon dioxide in the air or in a vial of snow. At the expected extremes of cold temperature the presence of "carbon dioxide snowflakes" was a strong possibility.

Over and over again I was amazed at how frequently the major problems confronting the scientists at the forefront of their disciplines came down to careful redefining of the fundamental first principles learned in grade school and high school. It was not the unsolvable differential equations that stopped research progress. Just what was the shape of dry ice crystals and how did they reflect light when compared and contrasted with the ice crystals of snow? Large scale models of the molecules that a teacher might use to show a child were needed by the research scientist to visualize what might be happening. These were the questions and this is where the solutions lie.

Another item to cause panic suddenly arose. A multichannel recorder for the radiosondes was not available. I learned the game of government logistics, that all research stops to play the government game of swapping. Weyant sent me to the U. S. Naval Observatory at Massachusetts Avenue and Observatory Circle. It was a huge circular area of wooded park land within the District of Columbia in which several old astronomical observatories were maintained, I believe mostly for their historical value. At one time these observatories served as the standard fixed point for all determinations of exact time for the U. S. Navy. Since then all of that standardization for the government and military was transferred to Boulder Colorado and buried deep within the mountains so that the perfect recording of time might be continued even during a nuclear war.

At the U. S. Naval Observatory several research offices for several government agencies existed so that their close physical proximity to each other could aid in the continual and smooth exchange of data and ideas. Feodor Ostapoff, head of the Sea Air Interface Laboratory (SAIL) maintained an office there. He had several men out at sea on various research ships. One ship, the *USNS Eltanin*, sailed the Antarctic water for many years surveying and exploring long term sea problems. At Weyant's directive I went searching for a recorder that I might borrow with the promise to pay for a new one from our grant money.

To me this seemed a fair request and this kind of swapping was done all the time. What was also done all the time was the initial denials by everyone. I think I went back and forth between my office and the Naval Observatory more than a half dozen times until I got Weyant and Ostapoff face to face at Blacky's House of Beef at a noon lunch and pushed the issue. About three years later I was pulling the same delaying stunts when different scientists needed equipment from me. Some of this cat and mouse game was related to the process of counting favors. Some of it was related to a strong desire to cooperate, but sometimes there was a true need to stall while the items were located. The federal government was a big place.

My entire experience in those first several months was filled with confusion, misdirection, many mistakes, and no linear orderly advance toward Plateau Station. Forgotten instruments had to be ordered. Learn this new theory but maybe the theory I did learn had no relevancy at the South Pole. Will the hygristor on radiosondes work in Antarctica? Did I have a license to ship helium out of the country? I needed special glasses to filter out strong ultraviolet rays before they reached my eyes while standing in the twenty-four hour sun in the Antarctica. Some of the instruments that I was required to order I had never used or even seen before. Even common thermometers caused trouble. Viebrock, who had not been to Antarctica, saved me from one of my more embarrassing orders. "Marty, what's the freezing point of mercury?" Forty below. "How do solid crystals rise in a thermometer when it's a hundred below outside?" From Runge-Kutta methods for solving or at least approximating solutions to systems of differential equations to questions of just how does one measure the temperature of the air or measure that amount of snow that falls on old snow fallen the thousands of years before were questions that arose without end?

All these questions were set in a panicky state with time rapidly running out. Weyant kept asking if I had ordered enough string. No stores in Antarctica! All routine supplies had to be on the Naval manifest for the *USNS Towle* before mid-October. All supply ships were departing from the U. S. at that time in order to penetrate the coastal ice sheets as they began to break up during the austral late spring in December. Some slow cargo ships had already left the States. The *SS Dorset* left Davisville, Rhode Island for Port Lyttelton, New Zealand on 22 August 1965. The USN Cargo Handling Battalion ONE (CHB-1) was already in Christchurch and Operation Deep Freeze 66 was well on it way getting all of its support forces in place.

Finally, at what Paul Dalrymple called "spring training," Bill Weyant had arranged with Charlie Roberts and Vaughn Rockney that I attend their school for observers for the polar regions. At this Polar Operations school held for six weeks starting at the end of August and lasting through most of September at the Test and Evaluation Laboratory of the Weather Bureau at the Sterling Research and Development Center in Sterling, Virginia, about thirty miles west of D. C., there existed a lab housed with all the current instrumentation used for the routine research observational programs conducted by the Weather Bureau in Antarctica and the Arctic.

At this school different groups were brought together for final training before their departure for either the Arctic or the Antarctic. Some of the personnel were known as the "polar rats" because of their employment habit. They began by accepting an assignment in the far north for the Northern Hemisphere summer or an entire year on an ice floe starting in summer. They then would come out of the polar north in fall, and sign up for the Antarctic. Departing the states during the Antarctic summer, they would again return in the northern spring to register for the North again. Many would earn tremendous quantities of money, live high for half a year, and head back to the poles.

These "polar rats" were truly experts at their polar trade while I, in my polar naiveté, wanted to measure the temperature with liquid mercury thermometers that would be frozen. I had more anxiety for attending this school than giving a seminar on the difference between Gaussian and Stefan-Boltzmann distributions and the need for non-centered chi-squared tests. (And I never was comfortable with seminars until I became a teacher.) I was aware that I might know more theory than these men, but the practical use of equipment and under severe conditions? No contest. These men were way ahead of me!

At this school I finally got the background to put my expedition together. I saw my first radiosonde. I launched my first balloon. I took apart and put back together again electrical functioning thermometers. I was laughed at for believing I could measure the humidity in the Antarctic and after testing the humidity device in an air chamber in the presence of dry ice at -109 ° F, I could laugh too. It was impossible. A major source of heat and energy exchange, ice to gas back to ice again, would be denied in the Antarctic air.

The polar school was run by Wesley R. Morris who spent most of the IGY supply season on the icebreaker *USS Atka*. He also winteredover at Eight Station and received a partial disability for severe snow blindness, which still bothered his eye sight in the sunlight. He was a short rough and tumble man not prone to swear publicly, but I think if a person crossed him, Wes would not lose.

My first day at polar school I could tell I was the odd man out. These polar men were all rough and rugged polar heroes. They were for the most part upset that they didn't have an opportunity to go the unexplored high plateau of the Antarctic and be first at Plateau Station. When they saw naive me, they knew they were right. Experience always triumphs over knowledge.

I gave in immediately. Of course they knew more about polar life and observations. I learned quickly. I had to. Every day at the polar school I was losing ground on my theoretical studies as well as follow up time on equipment checks and shipments to the cargo ships leaving from Davis, Rhode Island. Yet, every day at the polar school I became more knowledgeable of my challenge at Plateau Station.

The four students bound for Amundsen-Scott Station were Ronald Stephen, Jerry Hollingsworth, Harold Preston, and Charlie Mabe. Gary Davy, Phillip Gale, William Galkin, Norbert Novocin, Karl Staack, and Brent Scudder were on their way to Byrd Station. Paul Carlson, a very tall and powerful man with a deep booming voice was also part of the school, but on a day when our pay checks were due, while training exercises were just starting, he demanded his pay check. Of course Wes did not have it. When we worked out at the Sterling Labs, we were away from our duty station in Washington D. C. Wes called Vaughn to see if someone from OOPS could deliver Paul's check. Paul, near violent, spent most of the morning on the phone and when his check did not show up at 10 A. M., a specified time to be paid by according to the Civil Service Code, he quit.

We all joked teasing Wes that Paul may have shown us all a way to escape polar service. It was sad at the same time. None of us seemed to get close to each other. Competition kept us all struggling, wondering if we were going to still get selected out of the United States Antarctic Research Program

(USARP). On the Ice we would become intensely close even when separated by thousands of miles of glaciers.

At Sterling Labs we memorized the weather codes, took tests and memorized some more codes. It was a different school. At the University, tests seemed always way over your head. I remember getting a 23% once and still receiving an A. The professors wanted to spread the grades out to identify the brilliant, the smart and the just average. Here at polar school it was impressed on us that this was no longer just school work. We would be sending the weather encoded to ships in the frozen sea and to pilots flying over snow their radar could not detect. A single error could lead to the loss of life far quicker than one cared to think about. The training was intense, repetitious, and intolerant of error. Written tests were few and not as complex as the mathematically oriented tests that I was used to, but any screw up was unacceptable. No one wanted B or C students informing the support operations in the Antarctic of weather. Only 95% or better was acceptable. Scrutiny of your performance with equipment and the speed and exactness of obtaining weather data was even more intense than taking a written test.

QUIZ-ANTARCTIC (August 26, 1965)

Synoptic Code

1 Unscramble the following symbolic elements of the Antarctic code and place them in proper sequence as they would appear for the 0000Z obsn.

$9S_pS_ps_ps_p$ ANT Nddff $3T_xT_xT_nT_n$ 02600 IIiii $T_dT_d$app VVwwW PPPTT $8N_sCh_sh_s$ $4d_xd_xf_xf_x$ $N_hC_LhC_MC_H$ $2D_mD_LD_MD_H$ SYNOP PLAIN LANGUAGE $8N_sCh_sh_s$

2 Briefly define the following symbols:

dd	h	h_sh_s
W	C_L	9
W	7	pp
N	RR	2
S_n	a	ww
N_h	8	D_m
D_M	R_g	D_H

3. Encode the following:

a. Temperature -2 ° C, 12 ° F, -103 ° F, -16 ° C, 14 ° C
b. Wind direction: missing, 155 °, 010 °, 275 °, 180 °
c. Wind speed: calm, missing, 8 knots, 88 knots, 109 knots
d. Prevailing wind past 6 hours South, low clouds moving from North, low overcast sky.
e. (cloud group, mandatory section only) Sky obscured by 10/10 blowing snow and ice fog, vertical visibility estimated 1000 ft.

4. What fraction of the celestial dome is an octa?

a. 1/16
b. 1/8
c. 1/2
d. 1/4
e. 1/10

5. The Maximum Wind Speed is determined from:

   a. The highest 1 minute average speed equals or exceeds 33 knots during the past six hours.
   b. The highest instantaneous speed 33 knots or above during the past 6 hours.
   c. The highest 1 minute average speed equals or exceeds 33 knots during the period covered by W.
   d. The highest instantaneous speed 33 knots or above during the period covered by W.

6. Encircle the incorrect:

   a. Code figure 4, barometric pressure now the same as or higher than 3 hrs ago.
   b. Code figure 8, sky cover overcast, but with openings (binovc).
   c. Code figure 24, visibility variable zero to 2 miles.
   d. Code figure 55, maximum wind during past 3 hrs. 55 knots.
   d. Code figure 01, net 3 hr. change in station pressure .010 inches.

7. Encode the following:

   a. Clouds moving from the NE, direction coded ___.
   b. At the Pole Station the surface wind is blowing from the 090 West meridian, direction is coded___.
   c. The maximum wind is 117 knots from the West, coded group would be___.
   d. Light continuous snow is falling (71), visibility is reduced to 1/2 mile by ice fog (46) and slight blowing snow, generally high (38). Present weather would be coded ___.
   e. In the max min code group, the highest temp for the past ___ hours is missing and the min temp is 13 ° C. The coded group would read ___.

8. Encode the following weather information using the Antarctic code:

   Byrd Station, Longitude 120 ° W, Latitude 80 ° S, Time 0400 LST. Visibility unlimited. Past weather clear. Wind speed calm. 850 mb. Height 1499 meters msl. Net 3 hr pressure change .000 inches. Sky cloudless. Peak gust during past 6 hours calm. Temperature of the dew point missing. Pressure tendency past three hours rising then falling, pressure same as 3 hrs ago. Air temp 28.6 ° F. Ice crystals began 1130Z. Maximum temperature during the past 12 hours 35 ° F. Minimum temperature past 12 hrs -22.5 ° C.

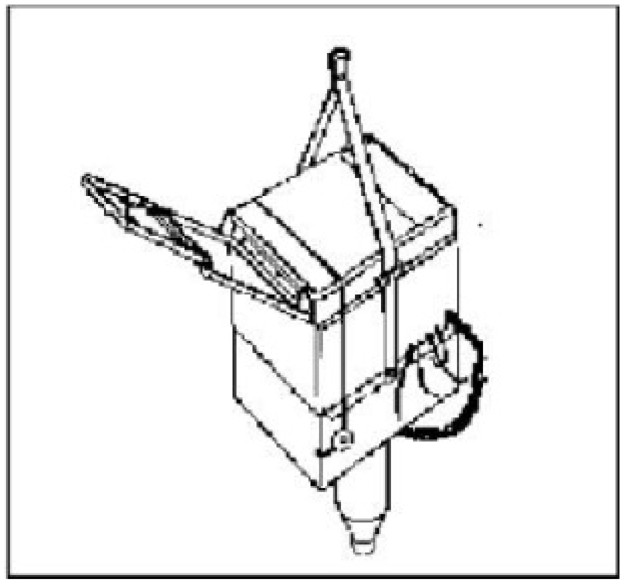
9. A whiteout is considered an obstruction to vision and must be so coded. T or F.

10. Ice crystals, ice fog, and ice crystal haze are classified as precipitation even though they may occur from a clear sky. T or F.

Trick questions simply did not exist. The desire of Wes Morris was to help you never to make an error while on the job and in a position serving others. Test questions were never a secret. You always knew the test would be straight forward with no trick questions. You never had to ask, “What is going to be on the test?” Wes taught me more on how to teach than any work shop of educationese.

At the Sterling Labs I touched my first radiosonde, the chief instrument that was sent aloft by balloon and sent back to a radio receiver data of pressure, humidity, temperature, and by tracing the balloon, the wind speed and direction. The radiosonde was an inexpensive, even for the government, package of cardboard and plastic. The bottom half was a small empty box about five by five by three inches. Into this space the observer placed a water activated battery and connected all the parts that would send signals.

Since the water activated battery's power would drift off its standard voltage level, the first task was to check a high pitched tone and a lower pitched tone which on the recorder in the office gave fixed positions. As the instrument package gave slightly different readings for these standard tones, the recorder could continually be recalibrated in flight, thus giving a continual accurate monitoring of the radiosonde in flight. Important to me, who planned to launch these radiosondes as rapidly one after another as possible, there was a radio dial on each radio transmitter hanging below the radiosonde and underneath the battery that permitted broadcasting of the signals at slightly different frequencies in order to listen to a new launch before the previous one had finished its flight.



In the chamber just above the battery was an aneroid barometer with a pointer like that on the barometer in your den that would show the pressure. On this flying instrument package that pointer was a switch moving across electrical connections that gave tones associated with the changing temperatures recorded by the thermistor. As the balloon rose the associated lower atmospheric pressure would cause the pressure pointer switch arm to cross over to the next connection to give the tone of the hygristor for the humidity. Thus the recorder on the ground simultaneously recorded the pressure by knowing which connection of the baroswitch was on and the temperature or humidity. After sequencing through the temperature and humidity five times, the standard high tone and low tone would be touched by the baroswitch enabling the observer on the ground to recalibrate the entire electrical system.

Critical to an inversion study was the heat budget. Major factors in a heat budget are the heat changes associated with the formation of ice crystals from the gaseous state in air as well as the reverse of ice crystals evaporating back into a gaseous state. A measure of this moisture was achieved by the carbon hygristor on the radiosonde. To a man, everyone at Sterling Labs laughed at my interest in this polar use of the carbon hygristor. Every radiosonde that Wes Morris' polar rats launched at the polar school would ascend to the stratosphere where the temperature cooled to -70 ° F and the humidity sensor would sing back "putt, putt, putt, putt, ..." This toneless signal, too low to interpret, was identified as the tone of motor boating. Without fail the entire school chanted, "Marty's fishing at Plateau again."

At Wes Morris' suggestion I spent some time at the Sterling Labs consulting with an electrical engineer with the Weather Bureau who was introduced to me as Saxy. Saxy showed me the hard facts. At -100.0 ° F, an expected common temperature during the polar night during the most intense inversion conditions, the saturated vapor pressure is 4.664x10-5 inches of Hg. At 56.9 ° F, an arbitrary warm temperature (when compared with polar weather) for Sterling, Virginia, the saturated vapor pressure is 0.4667 in Hg, 10,000 times more moist than what I had to expect on the plateau. This means in the Antarctic, at Plateau Station, even if it was at 100% relative humidity, the hygristor, the humidity measuring device, had to be 10,000 times more sensitive. The sensitivity of the standard hygristor simply was inadequate for polar work, especially on the Antarctic high plateau. It also was too late and too complex to design a new one. My enthusiasm to make all things work was dashed and I had to face reality that this instrument simply could not do the job. Giving up on humidity, Saxy and I proceeded to redesign the radiosondes that I would use at Plateau Station to eliminate the humidity and maximize the temperature data.

A surprise appearance, at least for me, was put in at our polar school by an old friend from the University of Wisconsin. Dr. Pete Kuhn was driven out to the Test and Evaluation Labs at Sterling by Charlie Roberts and introduced to us. He monitored the net-radiometers used on the radiosondes in the Antarctic. I knew Pete Kuhn as the inventor of the airborne net-radiometer. They measured heat radiation from the sky and the ground simultaneously. This measurement was fundamental to all meteorological studies because it measured directly either the heat energy entering or leaving a given place. Net-radiometers were quite expensive. But expensive instruments could not be put on a balloon to be sent aloft as a throwaway. Pete Kuhn together with Professor Verner Suomi invented a very simple but amazing device that remained inexpensive and thus opened the way to attach their instruments to radiosondes and be launched everywhere.

Dr. Peter Kuhn of the Weather Bureau Office at the University of Wisconsin (center) examines a radiometer which was used in the intercomparison tests. Standing with him are Professor Whal of the University of Wisconsin (right) and Edward Jurasinski, Director of Special Products for the Johnson Service Company in Milwaukee.

Pete's net-radiometer had two polystyrene plates painted dull black. Both plates were horizontal, one looking at the sky and the other looking at the ground. Glued in the middle of each plate was a very small bead thermistor. Trapped air space over each bead thermistor was held in by circular polyethylene disks, which permitted long wave radiation to penetrate and be absorbed by the bead thermistors. Incoming radiation minus out going radiation gave the desired net radiation usually measured in calories per square centimeter per minute or converted to degrees of heating or loss per day.

Next to the invention of the digital computer and satellite, no single invention changed meteorology more than Pete's net-radiometer. It measured directly one of the most fundamental components in weather. It measured energy gains or losses at every level. In fact, satellite data might not have been anywhere near as valuable without the net-radiometers they carried. And computers for meteorology may never have been able to begin the basic research without these fundamental measurements of energy.

In 1965 these measurements, especially polar measurements, were far from routine. Although Pete wanted all of us at the polar school to send out some radiation data, he still maintained personal control over every number his instruments measured. All the data collected from these net-radiometers, carried aloft by the Weather Bureau's radiosondes, which were held on the side of the instrument package by masking tape (the entire government is held together by masking tape), were sent to Pete at his office in Boulder, Colorado, where he served the government as a research scientist with the Atmospheric Physics and Chemistry Lab.

The government previously had set Pete up near Prof. Suomi at Madison. By the time of my expedition both Prof. Suomi and Dr. Pete Kuhn had become world leaders on fundamental measurement theory for global problems working with remote sensing devices on satellites, on rockets, and on balloons. Pete's single mindedness for meteorological research sent him to every research center in the world. And now he was giving us considerable time and seemingly enjoying himself while doing it.

I learned how to launch balloons, run the radio receiving recorders, calculate the data as rapidly as possible as it came off the charts while the balloon was ascending, and chart the profile heights from the pressure changes and elevation, range and azimuth of the tracking radar. Although I never could keep up in speed with some of the men who had done this work before, I was capable of holding my own. I had a fundamentally different purpose. While all of my polar classmates learned to reduce their data to only the significant changes as the balloon ascended, I was interested in every change, no matter how small. In fact, my balloons would be sent aloft at the slowest possible speed to record every little change in the inversion and the corresponding radiation and wind profiles.

I would not have at my disposal a tracking radar. At about the halfway mark of the time allotted for me at the polar school, I made contact with the Coast and Geodetic Survey. Their geophysicist selected for Plateau Station, Robert H. Geissel, promised to assist me if a second meteorologist would not be able to go to Plateau Station. Lettau's suggestion, that the detailed wind speeds and heights could be calculated from elevation and azimuth readings from two fixed theodolites a fixed distance apart, was going to be the only method on the high plateau.

Robert H. Geissel. C&GS geophysicist wintered at the Plateau Station.

Two theodolites were acquired through who knows what shenanigans of either Bill Weyant or Charlie Roberts and were delivered out to the Sterling Labs. Since Bob and I both had apartments in D. C., when it was time for him to join us at the polar met school, he would pick me up in his Austin Healey and cruise at more than 90 mph to Dulles and around the airport to the experimental labs to launch our balloons. Bob had been an expert surveyor and served several years surveying with the Peace Corps in India and Nepal. I was glad of that since again I had no experience with theodolites.

Oddly enough this time my inexperience served me best. Bob carefully showed me how to level the theodolite as a careful surveyor. He also established for me some very valuable methods for shooting a base line and gathering error measurements for corrective purposes with the two theodolites. These small tasks, needed to be repeated every time we took the theodolites out of their cases, took considerable time and our classmates had to delay their balloon launch until we both were ready.

Suddenly the balloon was off as a shot rising one thousand feet per minute and out of sight in about fifteen minutes. Inside the experimental lab, the school mates would follow the balloon for several hours as it ascended to more than twenty miles high. On the ground outside I, in panic, wrote down angular elevations and azimuths missing the first two time stations 30 seconds apart. I lost the first balloon altogether in about three minutes. Looking over to Bob about a thousand feet down wind, I saw him diligently turning the keys of the theodolite so I assumed he at least got good results. When

I walked out to talk to him to confess sheepishly that I lost the balloon, I discovered he was still getting ready for launch. Objects that surveyors look at through their transits don't fly away. We both had a lot of training to do.

Accepting the hazing of the polar rats for our failure to track the radiosonde on the balloon, at Wes' suggestion we appropriated a large number of PIBALs, small constant-rate-of-ascent balloons used by a weather observer to determine the base of a cloud ceiling. We gave up trying to write numbers down while we tracked the balloons— that is, once we in fact could move our theodolites fast enough to track them. With two tape recorders next to us we tracked the balloons and by hearing a buzzer through a walkie-talkie we simultaneously marked and barked out the angles of elevation and azimuth. These then were transcribed at a later point and then used to calculate the heights and wind speeds and directions. I learned to capture the balloons. Not knowing that you couldn't track moving objects with a theodolite, my inexperience permitted me to capture launched balloons by tracking them before the experienced surveyor could. In time Bob did too.

QUIZ-ANTARCTIC September 4, 1965

1. Using the strip of Aerovane wind recorder chart furnished complete the following:

   a. Assume the end (time wise) of the wind record is the end of the Chart roll and it is time to replace it with a complete new chart roll. Make the proper entries and identification features thereon. Use the current date.
   b. Assume the beginning of the wind record is a newly installed chart roll. Make the proper entries and identification features thereon.
   c. Record proper time-check entries on the entire chart. assume LST:GCT.
   d. Pick off gust data for each 3 and 6 hrly observation as appropriate and encode gust groups by identifying time indicator.
   e. Pick off any maximum one minute wind and encode using appropriate time indicator.
   f. On WBAN 10B make appropriate entries for peak gust and maximum one minute wind for the day.
   g. On WB 733-1 make appropriate entries for peak gust and maximum one minute wind for the day.
   h. Assume low clouds were observed moving from the NW throughout the entire day; middle clouds were moving from the NE during the first 12 hours of the day and moving from the SW the remainder of the day; high clouds were moving from the SE during the first 6 hours, from the N the second 6 hours, from the W the third six hours, and from the E the last six hours, encode the prevailing wind - cloud direction group for each 3 and 6 hrly observation and identify by appropriate time indicator.
   i. On WBAN 10A, in column 13, record any frontal passage as indicated by significant wind shift.
   j. From the wind chart, list any and all occasions (independent of cloud direction observed) when inclusion of the wind - cloud direction group would have been a mandatory group in the 3 or 6 hrly report due to wind shift alone.
   k. Using the wind chart, enter all of the hourly wind direction and speed values for the day on WBAN 10A.

2. Briefly describe noctilucent clouds, when they would NOT be observable, and how you would distinguish them from nacreousclouds.

3. You are shipping a defective Aerovane Wind Recorder from your station to Polar Operations. List the number and distribution of shipping documents involved in this operation.

4. List the procedures for mailing data records to POP at the close of the summer season.

5. Give an example of a RAWIN message for your particular station, identifying significant and mandatory levels. Balloon burst, last plotted level of wind at 31,162 meters above surface.

6. List the synoptic observations when the following groups would NEVER be sent:

   a. precipitation group
   b. maximum one minute wind group
   c. gust groups
   d. maximum and minimum temperature group
   e. blowing snow group
   f. special phenomena group 93000

7. List the synoptic observations in which the following groups are ALWAYS sent:

   a. maximum and minimum temperature group
   b. maximum one minute wind group

8. In the synoptic message the following are given in ___?

   a.TT;b.RR;c.pp;d.s_n; e. dd; f. VV; g. ff; h. T_xT_x

9. Encode the following weather information using the Antarctic code.

   Byrd Station, Latitude 80 ° S Longitude 120 ° W, 0400LST. Low clouds moving over the station from the north. Middle clouds, direction indeterminate. Peak gust during the past 6 hrs 68 knots from 355 °. Visibility 1/2 mile. Present weather blowing snow, overcast, ice fog, continuous light snow. Maximum temperature past 6 hrs -32.4 ° F. Surface wind direction 015 °. 2/10 sky obscured by blowing snow. Pressure tendency rising, then falling. Wind velocity 38 knots with gusts during past 15 minutes up to 45 knots. Dew point temperature missing. Total sky cover 8 octas. Minimum temp past 6 hrs -40.6 ° F. Weather during past 5 hrs ending 1 hour ago ice fog, blowing snow, overcast, light snow. Light snow began 0115 LST. Height of 850 mb level 1480 meters msl. Temp of air -25.8 ° F. Net 3 hr change in station pressure 0.215 inches. Snowfall since 0115 LST estimated .05 inch. Maximum 1 minute wind speed during past 6 hrs 41 knots from 020 °. 5/10 stratocumulus clouds estimated base 2,000 ft.

The last days of the polar school Hugh M. Muir, a scientist from the United Kingdom with the Arctic Institute of North America, planning to study the aurora from Plateau Station, also visited our polar school to learn how to assist me with the balloon project. Hugh was the kind of bloke we'd all die for, but there was no way he was going to be able to follow the balloons with a theodolite. After every launch when the balloon was miles away, Hugh would get around to ask, "Now you want me to see if I can telescope that balloon, do ya?" The balloon was gone! It was settled. Bob and I were the team.

For research work the pressures of time to meet operation demands was fortunately not present. The data would be collected and interpreted much later. A danger of collecting much useless data was a serious potential problem but physical constraints of our isolated remote station gave us no choice. I knew the problem, in fact, would become much worse in the polar field.

Some other chaps need mention. Charlie Mabe was from the National Weather Records Center in Asheville, North Carolina and had a secure position with the Weather Bureau. I could not understand this thin blooded southern fellow with a wife and six children leaving them for a year to go to the South Pole. He needed money. Before the Antarctic summer was more than half over, Charlie abandoned ship and headed home to North Carolina. I was glad for his family. Oddly, Wes Morris knew he wouldn't make it. He teased him seemingly unmercifully, always calling him Charlie Maybe and made no bones about his most likely failure on the Ice.

Hugh Muir, Aurora Scientist, Arctic Institute of North America, From the United Kingdom, Scotland—actually!

By the end of school out at Sterling, the *USS Calcaterra* arrived at its forlorn ocean station between Dunedin, New Zealand and McMurdo, Antarctica to assist with radio traffic between the States, New Zealand, and all stations in the Antarctic. She also remained at this precarious station in the worst of sea conditions for any emergency needs for Deep Freeze 66. Such was the lonely but vitally important work of a sailor. Also the four LC 130Fs began their overcrowded flying schedule on the Ice.

Somehow, although far from true, after polar school was over, after some very kind words from Wes Morris, I had the confidence of a polar rat and was more than ready to explore. There was much work still to be done. I desired to spend considerable time with Lettau and Schwerdtfeger before departure for the Ice. I had to visit Dalrymple's lab in Massachusetts. His program now was still a big unknown. I still did not have thermocouples for under the snow measurements. Snow accumulation was a puzzle until the day I arrived at the Pole. But ships were leaving with cargo bays full. Time was flying. As the leaves began to fall in Wisconsin, as the temperature began to become comfortable in the District, we were ready and sadly we knew we were ready. Hugh Muir, Bob Geissel, and I, now acquainted, began the ritual of drinking more and more frequently growing in restlessness before the big move.