CHAPTER 9
Research in Winter

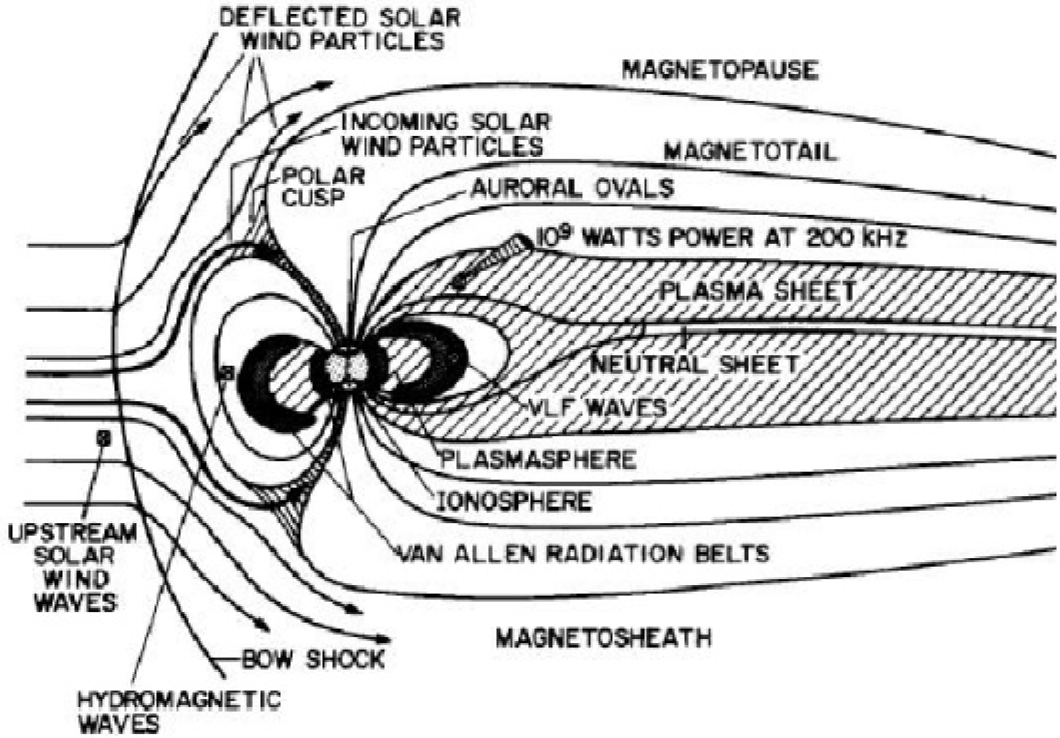
“Nowhere else on earth are external geophysical-meteorological environments as test-tube like or clear-cut and well defined as in the Antarctic.” (H. H. Lettau) This was our mission. This is what all of us had come to Plateau Station for. Exploration of the polar regions contributed most to the discovery and understanding of near space, the very high altitudes of the atmosphere, especially when you consider knowledge growing from zero to the present day understanding.

Specifically, Rob Flint’s research dealt with the studies and mapping of Very Low Frequency (VLF) electromagnetic disturbances occurring in the ionosphere and the high magnetosphere above. One phenomenon Rob monitored was what was called the South Atlantic magnetic anomaly. Changes of phase and frequency of VLF signals seemed to be created with large quantities of electron precipitation from the Van Allen belts and drastically disrupted normal behavior of the D-region of the ionosphere. Little was known of this phenomenon. Global monitoring and synchronous measurements on a global scale were required. Plateau Station was in an excellent position for such studies of the South Atlantic magnetic anomaly being on great circle paths connecting the U. S. Navy VLF transmitter NAA (17.8 kilocycles per second) at Cutler, Maine; U. S. Navy Station NSS (21.4 kilocycles per second) at Annapolis, Maryland; and a VLF recording station at Tucuman, Argentina.

Flint brought to his studies the wintering over experience at Byrd Station where he also performed VLF studies. His expertise on “whistlers” a VLF signal that becomes trapped in the magnetic field and bounces back and forth from one magnetic pole to the other in less than eight seconds, served well the research of the Institute for Telecommunications Sciences and Aeronomy and Stanford University. The study of these whistlers also displayed the instrumentation coordination necessary over the entire globe. A whistler bouncing between Byrd Station, Antarctica, and its magnetic conjugate point at Great Whale River, Canada, demanded perfect clockwork and coordination with precise wave length and frequency measurements for motions near the speed of light.

Almost all of our knowledge about the upper atmosphere and its electrical and magnetic effects rests on such intensive accurate measured observations from the polar regions. The main features like the Van Allen radiation belts were discovered in the IGY and electron precipitation and the flow of plasma independent of the solar wind were discovered in the subsequent years after IGY during which Plateau Station played a major role.

Intricately connected with Rob’s VLF program was the aurora program conducted by the Arctic Institute of North America at the cost of the frost bite of Hugh Muir from Scotland. He maintained continuously running all-sky cameras, which photographed all 360 ° of the sky during all electromagnetic disturbances visible as the southern lights. Hugh also used photometers and an auroral spectrograph which could monitor different wave lengths of the excited electromagnetic spectrum not visible. When great solar bursts erupted on the sun, tons of plasma material was sent out from the sun, and, being charged material carried by the solar wind through space, they intersected the earth’s magnetosphere setting off the aurora displays. This intersection spawned much activity also interpreted by VLF transmissions and monitoring.



(The Magnetosphere of Earth, schematically illustrating a number of important morphological features, "Advances in antarctic geophysical sciences from the IGY to the present," *Antarctic Journal of the United State*s, National Science Foundation, Vol. XXI, No. 2, June, 1986, page 2.)

Although scientists have known the entire earth as a planet with a dipole magnet, it was not until the modern Antarctic exploring expeditions actually came to find the "South Magnetic Pole" that geophysics became a science of the interior of the earth. [The magnetic pole currently in the Antarctic region is really a north pole magnet that attracts all south poles of all other magnets.] The first close-up triangulations for the position of the South Magnetic Pole could be done by comparisons of magnetic observations made by the German Antarctic Expedition (1901-1903) and the Great Britain National Antarctic Expedition (1901-1904).

Led by Prof. Erich von Drygalski, the German expedition's plans were drastically altered when a short summer season caused the freezing of their ship, the *Gauss*, in place near 90 ° East Longitude. This forced them to stay the entire next winter and permitted them to take magnetic observations for more than a year. During the British expedition First Lieut. Albert Armitage, and a year later Robert Scott, led parties up the Ferrar Glacier, penetrated the Transantarctic Mountains, and traversed over a short portion of the high plateau of Victoria Land, enabling them to take some of the most accurate magnetic observations of their day. From these two different places the position of the South Magnetic Pole could be established. The very next British Antarctic Expedition of 1907-1909 led by Ernest Shackleton sent a small party under Prof. T. W. Edgeworth David to the exact spot on the snow surface of the South Magnetic Pole - then at 72 ° 25' S, 155 ° 16' E.

With hindsight today, the major discovery came on the Australasian Antarctic Expedition of

1911-1914. R. Bage, Hurley, and Webb marched to the South Magnetic Pole, then still on the high plateau of Victoria Land. Disappointingly they only measured a magnetic dip of 89 ° 43.5' and had to turn back because of the severity of the wind and cold. Yet, they reached 70 ° 36.6' S and 148 ° 10' E, still 175 miles from the previously measured magnetic pole position reached by Prof. David. With only 16.5 minutes of arc of magnetic dip left to reach a place where they could see a perfectly vertical magnet, the still apparently large horizontal distance left to traverse presented itself as a mystery. The magnetic pole was moving.

During the IGY (1957-1958) the South Magnetic Pole appeared near the coast of the Antarctic at about 68 ° South, 144 ° East, and the French established the station Dumont d'Urville very close to it. A National Geographic Society's map of Antarctica locates the South Magnetic Pole in 1986 at a position nearly one hundred miles off the Adelie Coast at 65 ° 6' South, 139 ° 30' East. These positions show a magnetic pole movement faster than most glaciers which move several miles per year.

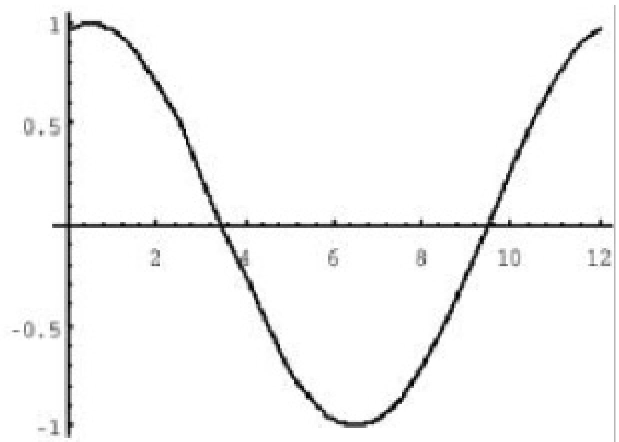
In the comfort of smoke filled warm labs in England and Australia, the world's scientists concluded that the interior of their planet was a very complex layered structure with a large portion of the interior of the earth's core being liquid causing the magnetic poles to move and to move quite rapidly when compared to other geological phenomena.

Bob Geissel came to Plateau Station with the newest designed magnetograph from the Fredericksburg Geomagnetic Center of the Coast and Geodetic Survey. He had both standard speed recorders and rapid run recorders to monitor all magnetic events from the interior of the earth and housed most of this equipment in two small shacks half way between the main camp and the emergency camp. It is to these shacks that Bob faithfully hiked in high wind and in calm every day from our warmest time at -6 ° F to our coldest -121 ° F. With his observations and the observations of many others around the world, maps of the earth's interior and its motions became possible.

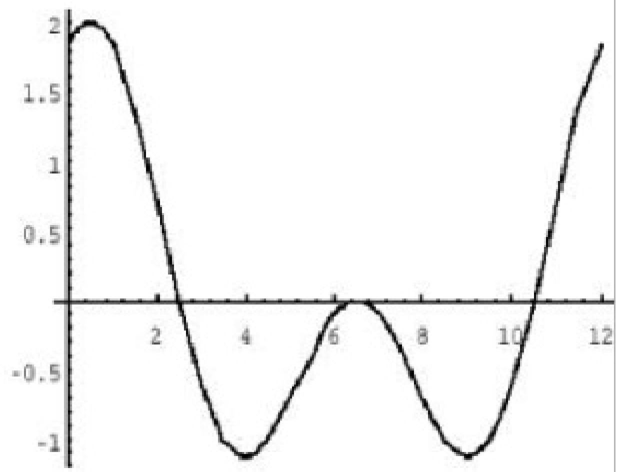
Many medical tests on the eight men wintering over at Plateau Station were planned. Most were carried out the second year. Some blood tests were made by Dr. Gowan that contributed much to revising medical understanding of "normal blood cell counts." A publication shows that we would have been diagnosed as having leukopenia and neutropenia. Our white blood cell counts all fell way below "normal." Our isolation in a germ free environment certainly was the major contributor. Once the resupply flights began routine operations at Plateau Station, our blood counts returned to the expected levels. ("Neutropenia in Healthy Men at the South Polar Plateau," *Archives of Internal Medicine*, American Medical Association, 1970)

Turning now to the meteorological programs for which I had primary responsibility, it cannot be overstated how important Antarctica was for the testing of theories. "There are three important principal factors: the exceptional uniformity of the physical structure of the Antarctic snow surface, the large horizontal scale of the topographical gradients of the continental ice-dome, and the relative paucity of short-time disturbances of the dominant long-period (seasonal) variation of insolation. These factors justify a comparison of atmospheric structure with laboratory results, such as testings in a gigantic wind tunnel, or in a rotating dishpan, or a cold chamber, or the unusual combination of a cold-air wind tunnel which is whirling around. The same three factors stimulate the further development of existing theoretical models. For example, the snow-air interface energy budget invites theoretical deductions and predictions which can be tested most readily by observations from previous and future Antarctic programs." (Heinz H. Lettau, "Antarctic Atmosphere as a Test Tube for Meteorological Theories," *Research in the Antarctic*, American Association for the Advancement of Science, 1971, p. 443.)

By 1971 Lettau fully explained the kernlose winter. He began with the annual cycle witnessed in the seasons all over the earth. This temperature cycle with enough averages shows clearly the changes of the angle of the sun from summer to autumn to winter to spring and back to summer. The calendar year of daily average temperatures for anywhere in the Southern Hemisphere can be approximated by a simple cosine wave.

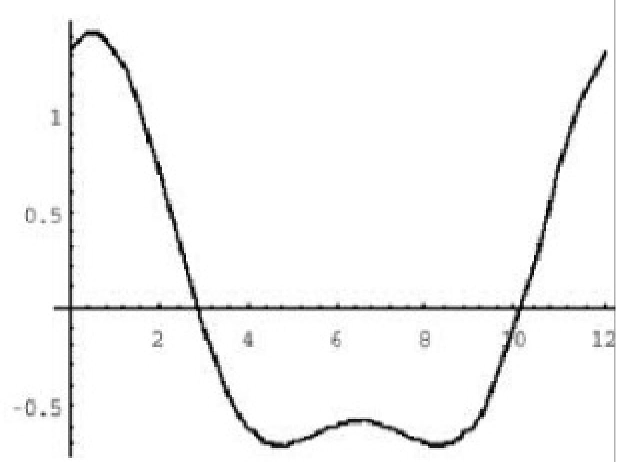


At higher latitudes than the Antarctic Circle where daily sun rises and sun sets for all year do not exist, yet the annual sequence of season continues, harmonic analysis is permissible. In 1971 Lettau explored the addition of a second harmonic, which is another cosine wave added to the annual wave. The second cosine wave had only half the wave length of the first so it would display two maximums and minimums in a year's time. The following graph is the addition of the first two harmonics of exactly the same amplitude.



Now Lettau determined the amplitude of the second harmonic for the South Pole where there is only one sun set and one sun rise per year as 42% of the first harmonic. Lettau's harmonic analysis then for the South Pole gave the following graph.

He claimed the amplitude for the second harmonic decreased proportionately from the maximum at the South Pole to zero at the Antarctic Circle. For Plateau Station the amplitude of the second harmonic must then be 23% of the amplitude of the first harmonic. A comparison of the monthly average temperatures at Plateau Station for my year during 1966 with the two harmonics for Plateau Station in like manner as Lettau's analysis is shown below.



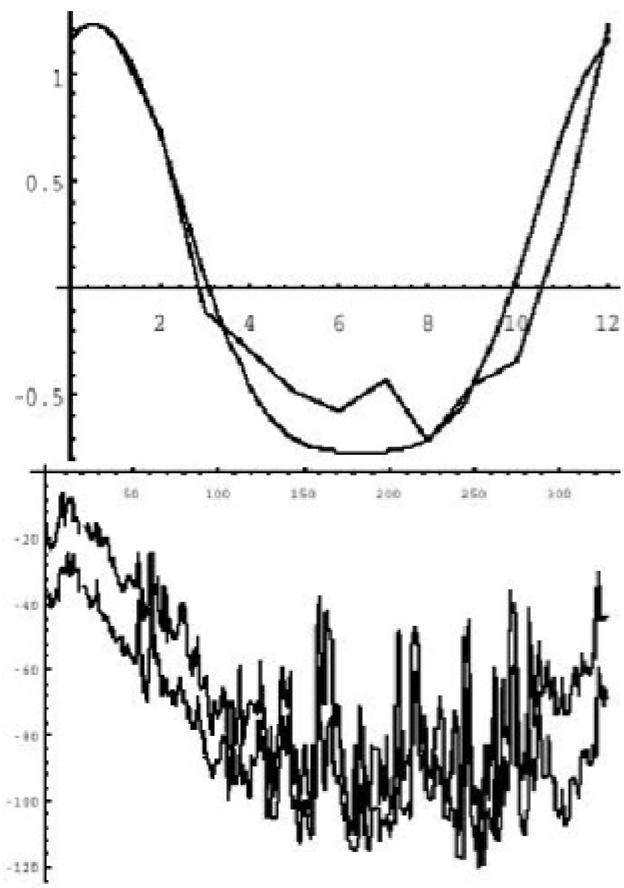
Using this graph Lettau would have predicted -122.7 ° F, as the minimum temperature, had communications worked well in January of 1966. I recorded -121 ° F as the minimum in 1966. Bob Dingle recorded -115 ° F as the minimum in 1967 and Tom Frostman recorded -123 ° F as the minimum in 1968 after which Plateau Station was closed.

What about all the many theories that tried to explain the kernlose winter? Lettau answered: "The difference between symmetry, "rounded at the bottom" [like a single cosine wave], and asymmetry, "flattened at the bottom" or "corelessness," of the annual temperature curve, as between Antarctic stations at moderate and extreme geographic latitudes, is explained by the truncation of the annual cycle of potential insolation. . . . there is no need to search for other and undoubtedly spurious causes of the "coreless" polar winter such as a mixing action of the wind."

Lettau's discovery of the answer for the kernlose winter is an outstanding example for research students to study to learn when to expect that they have an answer and need to look no further. Francis Bacon a long time ago (1620) identified this problem of looking for more order in nature where there was no more as an Idol of the Tribe. Lettau correctly taught us that unless there are first principles from nature herself giving reason for our answers or statistical curve fits and equation models, there will be no value of truth in them. Where statisticians, mathematicians, and scientists always can find different and more reasons to explain things, unless derived from nature, their reasons will remain spurious.

At Plateau Station I stood on top of an ice and snow sheet 11,000 feet thick representing thousands of years of snowfall. Yet the most difficult thing scientifically to pin down was the mechanism by which all that snow fell. Snow gauges in the open wind were untrustworthy. I brought none with me. The most effective way to measure the snowfall was to establish a grid of poles at surveyed positions. By measuring the height above the snow surface that each pole was visible, an average accumulation could be measured by statistically smoothing small localized drifts and effects of foot prints.

Charlie Roberts established the Black Field, which was set off with black flags, about seven hundred feet north of the emergency camp and it was measured once a month. I added the Red Field about four thousand feet northwest of the main camp, an extra long distance away to avoid a larger developing local drift of snow building up around the area because of the presence of the main camp as an intrusion on this region of the snow surface. This distance exceeded routine safe walking distance, but the value of the snow accumulation data I interpreted as worth it.



PLATEAU STATION, 79° 14.8' SOUTH DAILY MAXIMUM AND MINIMUM TEMPERATURES (°F) 15 DEC 1965-7 NOV 1966, 328 DAYS

The Red Field, smaller than the Black Field, was measured after every significant weather event that gave an apparent snow accumulation. I had hoped this Red Field could help settle a small debate between Prof. Werner Schwerdtfeger and me. He was originator of the idea of continuous or at least nearly continuous accumulation of snow by small ice crystals precipitating daily even from a clear sky.

This of course meant that I had to hike out into the extreme cold many times and go this long distance without the aid of a life line. During the polar night I wore a miner's lamp on my forehead powered by batteries inside my parka near my chest. After about an hour, the cold penetrated my parka and made the batteries so cold that they lost power. It was a good clue that I had been outside long enough. When it was colder than -80 ° F, and most often it was colder than -100 ° F, it seemed that breathing the cold air somehow slowed my mental capacity. Thinking was slowed. All physical motions seemed slowed. Of course every activity took extreme lengths of time. To safeguard against poor judgment Rob Flint or Bob Geissel took turns marching out to the Red Field with me and assisted with the many measurements.

Several times I needed to measure the Red Field alone. Walking out that distance unescorted was considered dangerous and I logged in with both Flint and Geissel. They knew exactly how long I expected to be out and exactly where I intended to go. Plateau Station was not quite as dangerous as other interior polar stations since the wind rarely became stronger than twenty knots. The severity of the cold dominated our fears. Since my miner's lamp would be out after an hour and my glasses would be useless due to the extreme buildup of frost from my breath, it was a common practice to turn an outside flood lamp on facing the direction that I would be walking back so I could use it as a beacon. One time I stayed out a little too long but was suddenly aware of my overstay when the main camp's generators stopped as they frequently did after our major shutdown in July. Stars that day were not well defined because of light clouds or ice fog. The wind was near calm and with the momentary dead generator total silence spread over the plateau in a cold deadly fearful manner.

I painfully finished the measurements and started back. My face was well frozen into the parka. My nose was without feeling. My feet and hands were past being numb and pain began creeping up my legs and arms. I had stayed too long. Also, without a doubt, I was overdue at the main camp. Someone normally would be coming out to assist me back had it not been for the generator failure. After what seemed like several hours but was perhaps only thirty minutes, the floodlight at the main camp suddenly came on simultaneously with the comforting roar of the generator. In shock I saw my long shadow ahead of me. The light was behind me. Somehow I was confused in direction or had started walking back correctly and instead drifted in a long leftward circle and was walking northward to the seacoast a thousand miles away.

I turned and corrected my almost fatal error. The longer it took to get back the more thankful to my God I became, realizing the extent of my error. My turn was such that I never would have crossed a life line of rope and bamboo flag poles. Nor would I have crossed the ski way. In the distance, perhaps a full two miles, I could see several men coming out toward the Red Field in search of me and I was most grateful for the arms of Flint and Geissel as my legs became numb and I found them to have difficulty bearing my weight. Hugh Muir followed pulling an unneeded "banana boat." The joy over their concern for me and our quick reunion went a long way toward thawing my legs. I had my data.

Schwerdtfeger, it seemed to me at the time, built a major case on the casual statements of early explorers such as Paul Siple who said that when he aimed a flashlight upward in the polar night he could see ice crystals continually. I tried and indeed you could on some days see ice crystals in the air. My daily observations showed ice crystal precipitation on more than sixty percent of the days, summer or winter. In my view it simply was too easy for ice crystals to be advected by wind from the thousands of miles on the plateau. The polar plateau was already in the "alto" region of cloud layers. The thinnest of clouds could deposit snow or ice crystals, and if only thin clouds, they would go undetected by visual observations in the polar night.

Precipitation from a clear sky when taking into account all other possible sources for ice crystals for me was simply too hard to accept. My arguments with my professor were recorded in his major work "The Climate of the Antarctic," half of Volume 14 of the encyclopedia of climatic studies *World Survey of Climatology*, edited by Helmut E. Landsberg (1970). Later Schwerdtfeger would brilliantly argue how air in an intense temperature inversion would possess sinking qualities of motion. Even unsaturated air sinking in an inversion with a vertical temperature gradient of more than sixty Fahrenheit degrees, most common as confirmed by my own balloon ascents, would easily reach saturation and super saturations and almost certainly precipitate from a clear sky. Thirty years from my year on the Ice I am prepared to concede to my old Professor. He won.

Still, my two snow stake fields showed essentially the same snow accumulation, 6.8 centimetres (2.7 inches) snow as determined by the Black Field. No significant difference could be determined by the Red Field that I measured after each cloud or wind related event. The snow surface, hard in summer and soft in winter, may have given an interpretation in favor of ice crystal precipitation, but not positively from a clear sky. I felt the metamorphosis of the snow surface was primarily due to a fragmenting of the ice crystals on the snow surface because of the extremely cold temperatures. There was a deposition or crystal growth directly on the surface that appeared to be very large. This was a sublimation of water vapor in the air forming ice crystals in supersaturated air at the surface releasing the heat from the phase change to the air. But ice crystal formation in clear air was hard to confirm.

How are theories formulated? Most often they are put together by scientists like Schwerdtfeger who have other theories claiming certain things that might be wrong in one aspect but could be held together by adding still another theory. As the theoretical structure grows, it either elegantly explains more and more and wins the favor of many other scientists, or it becomes top heavy and begins to fragment by self contradiction. It is not often that observation alone settles the matter. Access to frequent publication aids the process. Had I not turned into a high school teacher, my debate with Schwerdtfeger might have developed into a friendly set of observational tests and perhaps more clever students of Prof. Schwerdtfeger would have developed interesting instrumentation to measure what we could not measure in 1966. The shallowness of the Great Antarctic Temperature Inversion, unable to be grasped with even the slowest rising balloons but confirmed by Dalrymple's fixed 30 metre tower constructed the next year, permitted me to give up a good fight, perhaps a little too soon.

Do not let the measurement of a year's snow accumulation, 6.8 centimetres (2.7 inches) as determined by the Black Field in 1966 and 9.11 cm (3.6 inches) in the following year as measured by my successor, Bob Dingle from Australia, go unnoticed. Bob Behling, a very close friend of mine going back to freshman calculus at the University of Wisconsin-Milwaukee and a member of the Queen Maud Land Traverse, shared with me profile data of ice densities. The snow density of the several layers near the surface averaged 0.361 g/cm^3. This density of snow yields 2.45 inches of precipitable water for the entire year. Antarctica is truly the driest desert in the world.

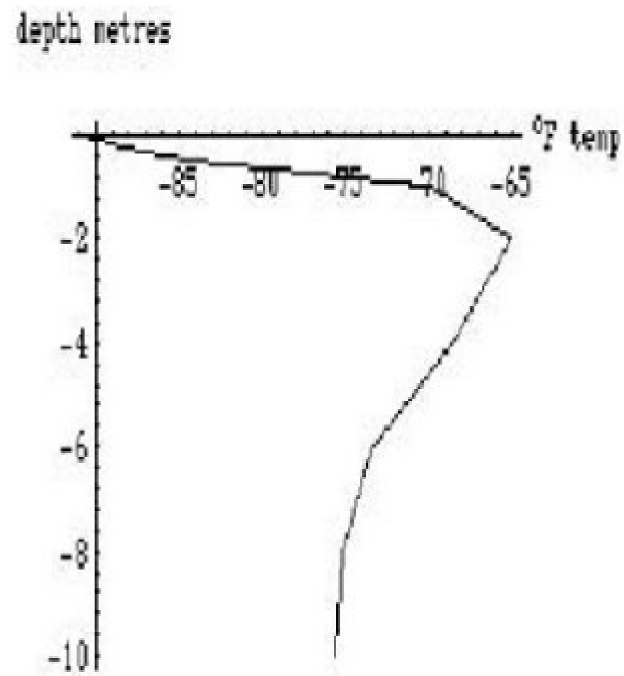
At this rate how did 11,000 feet of snow get here?

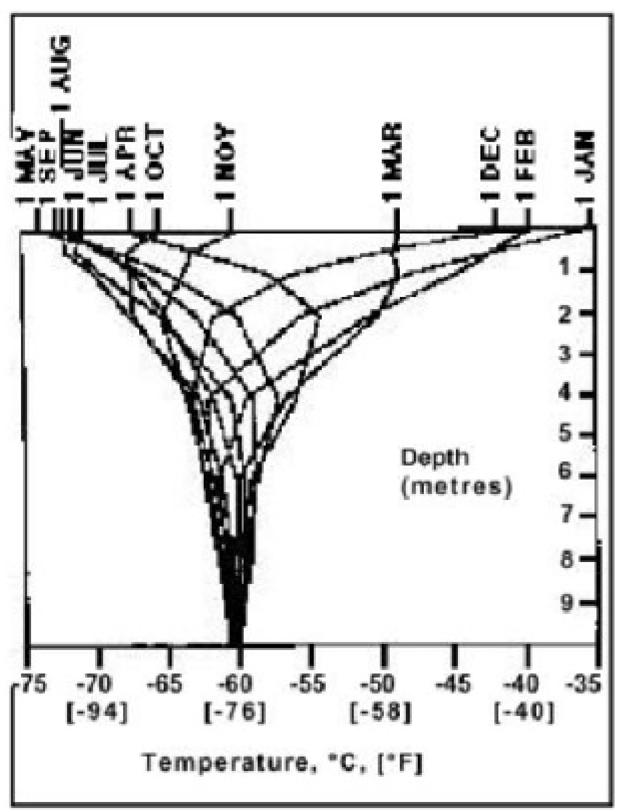
An intriguing study that I developed with much consultation with Lettau, Schwerdtfeger, and Ed Flowers, was how the heat and specifically for Plateau Station how the cold conducted into the icecap. From a pure physics point of view, cold really does not exist just as darkness is not an entity. As darkness is really an absence of light, so cold is the absence of heat. To measure the rate at which heat penetrated the icecap, I buried thirteen copper constantan thermocouples at varying depths in the snow down to ten metres below the snow surface in January 1966. After a considerable number of calibrations that compared each thermocouple with each other as well as with standards, careful measurements were taken every three hours for the remainder of the year.

Starting with the "heat wave" of temperatures like -6 ° F and -20 ° F during January I could follow this heat in later months. But as this heat penetrated the ice its warmest temperatures were damped and were cooler. The next graph shows the summer heat at -65 ° F penetrating downward to a depth of two metres by 1 April 1966.

By drawing lines of averaged temperatures for the first day of each month in the second graph a cone-like figure emerges. If you carefully trace the individual lines for each month you will see the heat wave get cooler and cooler until it becomes a constant near the ten metre depth.

When the cold on the surface reaches its minimum in the darkest winter months of May, June, and July, the cold can also be followed downward but again not as severe. In other words, at each level the maximum and minimum are damped coming closer and closer together with deeper depths until a constant is reached. That constant is near the climatic average of the region. In the air above the snow surface I measured a range of -6 ° F to -121 ° F with the maximum recorded in January and the minimum recorded in August. Two metres below the snow surface this graph shows a temperature range of -58 ° F to -89 ° F with the maximum recorded in March and the minimum recorded in August and October.





The penetrating seasons by conduction become less severe and are delayed. Continuing we see at the four metre depth the maximum temperature occurs in April at -69 ° F and the minimum temperature occurs in November at -83 ° F.

Deeper and deeper into the snow, from the graph it becomes more difficult to distinguish the time. At six metres, the maximum occurs in May and the minimum probably in November with the range of temperature from -72 ° F to -81 ° F. At eight metres the seasons are fully reversed with the summer heat reaching this depth in June and the previous winter coming in December with only an annual temperature change of -6 F ° from -74 ° F to -80 ° F. By ten metres below the surface the temperature no longer varied by more than one degree averaging -76.9 ° F. This was considerably lower than I recorded in the original ice core samples with Commander Pope. Those cores may have been warmed in the drilling process. And our year on the polar plateau may have been a "warm" one with the ten metre depth temperature not responding to the warmer year until many years of a warmer average. This of course would mean that Plateau Station could reach colder temperatures than we experienced.

Note that this cone is asymmetrical. The cold side of the cone is closer to a vertical line drawn through the constant ten metre temperature on the graph. Prof. Lettau identified this as an effect of the kernlose winter of polar regions. Tautochrones, as these graphs are called, in regions not poleward of the Arctic or Antarctic circles are very symmetrical.

(Monthly tautochrones, 1966-1967, for the layer of snow between surface and ten metre depth at Plateau Station. Published by Gunther Weller and Peter Schwerdtfeger, "Thermal properties and heat transfer processes of the snow of the central Antarctic Plateau," Paper No. 15, *Proceedings of the International Symposium on Antarctic Glaciological Exploration* [ISAGE], Dartmouth University, Hanover, New Hampshire, 1968.)

I remember being very angry about the "theft" of this publication from me. Sometime after I was in the field and, in fact, had planted my thermocouples after much labor and frozen face and hands many times over, a group of scientists teamed together from the University of Alaska and the University of Melbourne, wrote a proposal to the National Science Foundation and received a substantial grant to do the same thing at the same place. As the published graph shows, it included the year 1966. That was my data in 1966, published with not even an acknowledgment. I was the only meteorologist at Plateau Station in 1966. It had to have been a complete giveaway by either men in the Weather Bureau at Polar Met or through the Australian observer who replaced me.

When I learned of their proposal, I was determined to publish as quickly as possible but to cover the butt of whoever funded this duplicate project Bill Weyant encouraged me to make the peace and turn over the data to Alaska and Melbourne. Reluctantly I did. It was easy for administration people to give up the hard labors of others, especially when they did not make the sacrifice. As it turned out, Gunther Weller and Peter Schwerdtfeger bore no frostbite either.

In pure research work ideas and who had the idea first is everything. In free and open exchanges, as I always believed was the only purpose for Antarctic research, many ideas were traded and at times no one can say for sure who did the initiating. Men like Heinz Lettau and Werner Schwerdtfeger were always more than generous with me. Their idea of passive measurements of the air using optical views of light rays preserved with photographs of targets gave what I thought would be the most accurate method of determining the most detailed temperature profiles possible.

When you insert a thermometer into an air stream you must interrupt that air stream. In Antarctica it was always worse. The thermometers, always electrical, required heaters or produced heat and it remained an unending nightmare reducing, correcting, or explaining away the errors. If a ray of light from a refracted view of a target could be mathematically traced back to the camera with a very long distance separating the camera from the target perhaps that mathematics could deduce the exact temperature profile of the air through which the rays of light passed.

Jim Sparkman with the Sea Air Interface Laboratory tried to defend his doctoral thesis on this subject about 1961. He was Lettau's student at the time. Then this passive method was fairly new and, the rumor mill claimed, Lettau was fairly silent at the defense of Sparkman's thesis. Noting the silence, a physics professor invited from another department, ate Sparkman alive and the thesis failed.

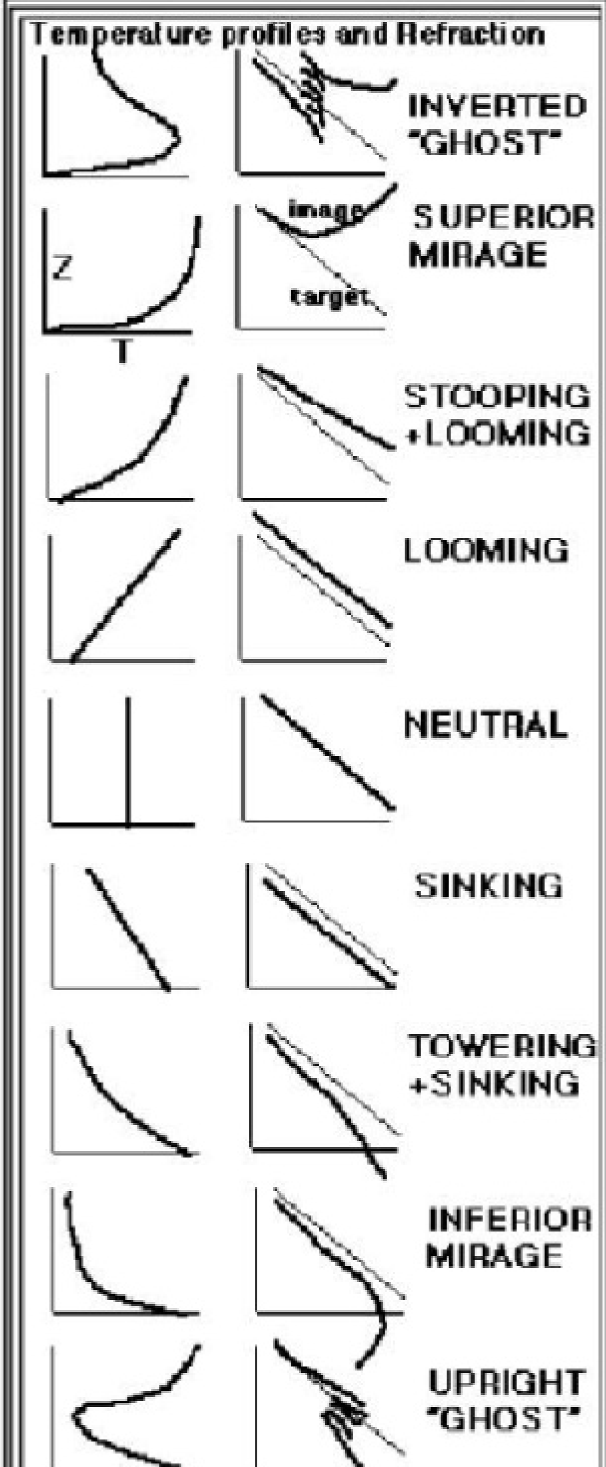
Still, Sparkman's idea was most impressive. He spent much time with me helping me understand this very elaborate mathematical model. Likewise, Lettau encouraged me to try to establish some targets of my own design and try to photograph them for different temperature profiles.

Bob Geissel assisted with the very accurate surveying needed. I placed three triangular targets of bamboo poles each forming a 45 ° right triangle with the snow surface. The tripod of the camera stood exactly one thousand feet from the first target, two thousand feet from the second target, and three thousand feet from the third target. The first year I was limited to photographing these targets during daylight times only. My initial photographs demonstrated the potential of this method and at my request Bill Weyant sent down many strings of Christmas tree lights and very long extension cords. A successor, Mike Kuhn of Austria, followed up with many photographs of the targets draped with colored lights. With pleasure we both endured the pain of the cold and turned our pictures over to Lettau.

When Lettau published his results, (sketches shown here) he was most generous to both Mike Kuhn and me giving us credit in his famous “Antarctic Test Tube” publication.

> “Following my suggestion at the preplanning phase for additional observations at Plateau Station, M. Sponholz set up during the first year of Plateau Station occupancy a horizontal array of three optical targets to document photographically day-to-day changes in the refraction of light in the lower atmosphere. Each target consisted of a rod inclined 45 ° toward the ground, supported by a ten-foot-tall pole, at distances of 1,000, 2,000, and 3,000 feet from the station. The inclined rods were initially marked by colored strips, and later by strings of electric lights brought to Plateau Station by M.Kuhn at the beginning of the second year. Because the index of refraction in air is a function of air density, the vertical temperature profile corresponds to density stratification in the lower atmosphere which causes a curvature of horizontal and near-horizontal optical rays. This in turn will distort the visual or photographic appearance of the target. In a mathematical “inversional” process, the vertical temperature profile may be recovered from measured ray-curvature. Many applications of this principle are reported in the literature. In comparison with previous techniques, the use of the slant-target line has the advantage that simultaneous refraction effects over a height range can be most readily documented photographically, provided that proper distances and target sizes are chosen.”

> “Tentative results of direct temperature measurements along the tower [built the second year] show indeed frequently elevated minimums which explain the appearance of ghosts as indicated . . . and photographed . . . . The systematic matching of photographically documented ray curvature with simultaneous direct thermal probings has not yet been achieved; it is planned for future work. However, the photographic



Schematic illustration showing how a "slant-line target will be optically distorted by atmospheric refraction if the vertical profile of air temperature corresponds to those sketched.

evidence appears to be so convincing that there remains little doubt that measurements of optical refraction can serve as an accurate method of thermal probing." (Heinz H. Lettau, "Antarctic Atmosphere as a Test Tube for Meteorological Theories," *Research in the Antarctic*, American Association for the Advancement of Science, 1971, p. 461-464.)

In private, Lettau praised me for the excellent choice of distance, which I arrived at with some trial and error and a lot of luck, and the shape of the targets, which I stole from Pythagoras. In research sometimes you are involved with the end of a project as a mop up operation. Sometimes you are the initiator. Sometimes you serve others. Always the pursuit of understanding the natural world was the chief excitement, love, and lure. Credit, though important to a young scientist's promotion, was not so important.

The surface radiation study, installed by Leader Stroschein and maintained all year by me was quite successful. Quoting from the literature,

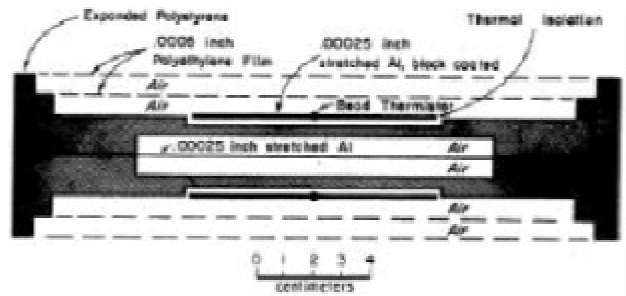
> "This information [previous radiation budget studies from IGY] and the personal field experience of the designers, P. Dalrymple and L. Stroschein at the U. S. Army Natick laboratories, of the radiation program ensured that the field work stood well up to the expectations in the first year of operation, 1966, when M. Sponholz tended to [radiation instrumentation]. . . . At the beginning of the activities it became quite clear that the area of Plateau Station was unrivaled by any other site as far as continuity of fair weather periods, atmospheric transparency, and homogeneity of air and underlying terrain were concerned. This made it promising to study the radiation fluxes beyond the primary scope of furnishing the forcing function in the overall energy budget." (M. Kuhn, L. S. Kundla, and L. A Stroschein, "The Radiation Budget at Plateau Station, Antarctica, 1966-1967," in *Antarctic Research Series*, Vol. 25, American Geophysical Union, 1978, page 42-43)

I no longer can put my finger on our exact results, but in field discussions with Leander Stroschein and Mike Kuhn we gloried in our potential of recording the highest value and therefore the most accurate solar constant ever measured from the surface of the earth through the thin polar atmosphere. Such a number with credit given to us would have appeared in every textbook on earth. Before Dalrymple's laboratory could complete the analysis of all the radiation data, collected in the severe cold by instrumentation maintained with very frozen fingers and hands, from sunny Florida several Nimbus satellites were launched and every accurate reading of the solar constant observed from space above the air rendered any measurements on the earth's surface as only approximations full of error. The current accepted solar constant as determined by a satellite was 1370.6 Wm^{-2} [Watts per square metre] ±0.73 Wm^{-2}, Nimbus-7, 16 Nov. 1978 to 31 March 1986.

Dominating all of our energies was my balloon project or the Great Antarctic Temperature Inversion study. The net-radiometers had to be calibrated, first inside and then after about half an hour of climatizing to the outside temperatures calibrated again. Unexpectedly the transparent covering of the radiometers shattered the instant they were taken into the sub -80 °F air. If left broken, the radiometers would act as if they were turned into elaborate bead thermometers of the air. The covering, keeping the atmospheric air away from the bead thermistors permitted the ability to convert these bead thermistor readings of trapped air into the net-radiation measurements.

In desperation, I added a thin film of diesel fuel to the polyethylene faces of the net-radiometers. This gave them considerable stretching ability and they survived the extreme contractions of the cold air. When I returned to the States, the University of Wisconsin men had horror stricken faces, particularly Pete Kuhn who later moved to Boulder, Colorado. He and I bought several forms of diesel fuel and launched comparative studies from Picnic Point in Madison. Pete Kuhn never would say the addition of the diesel fuel ruined all the observations, and I proceeded to analyze the data as though the diesel fuel had no effect and Pete never denied my efforts. At Plateau Station I did not have a choice.

While each net-radiometersonde cooled the weather balloon had to be prepared. The balloons also became fragile at temperatures -90 ° F and colder. At the South Pole they had similar cold temperatures, but their balloons shot through the inversion layer at a rate of a thousand feet per minute. I wanted the balloon to stay in the inversion layer for as long as thirty to forty-five minutes, a dream I never achieved. This necessitated the treating of the balloons with diesel fuel as well. I would spread the large balloon over the main camp's living room, pour diesel fuel inside the balloon and also soak the balloon in a large bowl of diesel fuel. Only then was I ready to take this sick looking glob out to the balloon inflation shelter.



CROSS SECTION—RADIOMETER

The balloon inflation shelter was a disaster. It was too small (ten feet by ten feet by ten feet) and I had no one to blame except me and my own inexperience providing the designers with dimensions too small. It was way too wide for the under inflated balloons we would launch. When filled to the inflation level I desired, with the least amount of helium that could still lift the instrument package, the balloon was never wider than about six feet but a bubble of helium at the top of the balloon made it stand nearly fifteen feet above the floor. That meant the accordion doors on the roof had to be kept open during the entire inflation period, nearly an hour. Inflating a balloon at essentially outside temperatures while moving around metal helium tanks and connecting and disconnecting metal valves not only was a bitterly cold task but a dangerous task because of moist flesh readily freezing to the metal parts.

A high speed chart recorder monitored the ascent and had to be manually calibrated at the frequent calibration signals sent from the radiosonde. Two theodolites, one under a dome at the main camp and one under a dome at the emergency camp, were used to track the balloon as it rose through the temperature inversion. Readings of elevations and azimuths to a light bulb glowing at the base of the launched balloon were barked into tape recorders.

All of this took many hands, with promises of help assured from all personnel. I would do all the preliminary work of calibration, but then I had to be in under the met dome in the main camp to track the launch of the balloon and net-radiometersonde. Bob Geissel, who trained with me at the research labs in Sterling, Virginia, manned the theodolite out at the emergency camp. Rob Flint assisted during the first several launches with the balloon inflation but quickly took over the entire inflation process freeing me for last minute calibration problems. Hugh Muir and a Navy volunteer, most of the time Jerry Damschroder but also Ed Horton and Bill Lulow and never Jimmy Gowan, would launch the balloon and net radiometersonde.

The launch required considerable physical effort at the extreme cold temperatures of the lowest levels of the inversion. The balloon was allowed to rise out of the balloon shelter, but then had to be recaptured and carried out a short distance from the camp to allow considerable room to run for the person carrying the instrument package which was attached by a long string of fifty feet to the balloon. The long string kept any air interference of the balloon from wrongly interfering with measurements taken by the net-radiometersonde. When the balloon was launched by a Navy man, Hugh Muir would run under the balloon until the ascent of the balloon had sufficient lift to support the instruments without letting them crash into the snow. These were always heroic runs that burned his lungs in the cold.

These launches frequently were done during the normal "day" hours and after a short time, Jimmy announced the Navy simply was too busy to assist during Naval work hours. What the men did on their free time was up to them. In yet another short time it became obvious these launches cut into the movie time and the drinking time. That was the end of the promised support of the Navy.

The genius of Rob Flint gave us a fifth Navy person in the personification of a two-by-four. Rob built a very strong base that he buried deep in the snow as an anchor. Rising out of the snow was this two-by-four with a hook that held the balloon. The hook could be tripped from a long string held by Hugh Muir while he held the radiometersonde. It did not replace the desperately cold run, but it did replace the Navy. We painted a face on the two-by-four and gave it a rank higher than the Officer-in Charge. I saluted this board before every launch.

What made these launches exhausting were the serial requirements. Whereas at other Antarctic stations they made one or two launches every day, at Plateau Station I was committed to study just the temperature inversion. At this small station with limited manpower I followed criterion that looked for exceptionally strong inversions and inversions in a state of rapid change. We would launch balloon after balloon without rest as long as the weather conditions of the event being studied remained. A list of times and launches for a particular series that gave publishable results some time several years later is given below.

BALLOON ASCENT SERIES #8

DATE	TIME (LST)	DEVISE
6 AUG	21:30	NET-RADIOMETERSONDE
6 AUG	23:18	PIBAL
7 AUG	00:25	NET-RADIOMETERSONDE
7 AUG	01:23	PIBAL
7 AUG	05:09	NET-RADIOMETERSONDE
7 AUG	05:50	PIBAL
7 AUG	08:42	NET-RADIOMETERSONDE
7 AUG	09:20	PIBAL
7 AUG	20:43	NET-RADIOMETERSONDE
7 AUG	21:24	PIBAL
8 AUG	01:09	NET-RADIOMETERSONDE
8 AUG	01:54	PIBAL
8 AUG	07:03	NET-RADIOMETERSONDE
8 AUG	07:53	PIBAL

Total Time: 35 hours

Disappointing was the fact that the inversion, though as strong as expected was far shallower than expected, and the longest time I could keep a balloon in the inversion was twenty-three minutes, but Hugh Muir had to run nearly to the Princess Ragnhild Coast before the balloon had enough lift to independently carry the radiometersonde.

Averaging all the data for a balloon series, which monitored a stable strong inversion, an excellent view of the inversion is shown above. While the original estimate of the height of the inversion was 3000 feet, for this series I measured the top of the inversion below 1000 feet and ninety percent of the inversion change occurred under 300 feet.

While I was gathering this data, Prof. Schwerdtfeger and his new student, Larry Mahrt, developed mathematical models for both the temperature profile and the wind profile of the inversion. Their formula as published is:

$$T(z) = T_h - \Delta T e^{-z^*} \text{ , where}$$

$$z^* = \frac{z}{\sqrt{k/f}} \text{ .}$$

T_h is the temperature at the top of the inversion.

ΔT is the temperature difference between the top of the inversion and the snow surface.

z* is the independent variable in the vertical in nondimensional form.

z is the ordinary vertical coordinate with length dimensions for height above the snow surface.

k is the coefficient of eddy diffusivity.

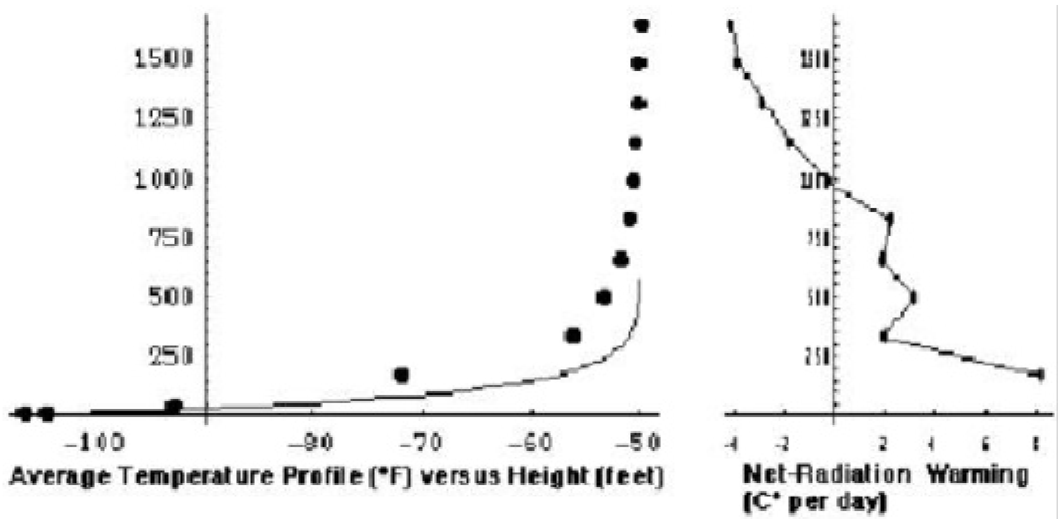
f is the Coriolis parameter.

The Mahrt-Schwerdfeger formula calls for a temperature profile as an exponential function of the height expected in pure nocturnal radiational cooling conditions. Note that this theoretically simulated temperature inversion is even lower than the actual measured inversion. The profile of net-radiation shown with the same vertical ordinate displays an unusual warming in the lowest layers of the inversion.

With a full moon on the northern horizon and clear skies most of the time for this series, visibility varied from unlimited to approximately five miles because of the presence of ice crystals. During some of the thirty-five hours of the duration of this series, high cirrus clouds could be detected against the moon. The surface wind was generally less than five knots from the north and northeast.

As a disciple of Prof. Werner Schwerdtfeger, though occasionally a prodigal on this issue, I believe the heat of sublimation given off when moisture in the saturated air precipitates out as ice crystals is what added heat and raised the expected temperatures of the inversion in the lowest layers. Thus, this warmer layer was giving radiational warming values until the nearly isothermal region starting at a height of 1000 feet. The recorded radiational warming of -4 C ° per day (negative values mean actual radiational cooling) where much of the moisture would have come from is in good agreement with the more routine radiometersonde data at South Pole.

Again Schwerdtfeger won our professional debate, although at the time he did not use this radiometersonde data because of a fear of the added diesel fuel affecting the data. With all balloons continually rising faster than was hoped for, having real inversion temperature measurements at higher levels than Schwerdtfeger's expected theoretical inversion, it was easy at the time to claim the



fast moving balloons influenced the temperature interpretation making it appear that a particular temperature was at a higher level than it really was. Even if it supported Schwerdtfeger's other theories about ice crystals falling from a clear sky, at the time we were all cautious. But ice crystals were all over the place and the changing phase of H_2O from a cold gas to a cold solid does explain all of the peculiarities.

As an active researcher I was most frustrated with the intense task of gathering immense amounts of data without seeing the answers as in some cases I have given the reader here. Some things, like how strong the temperature inversion was, or how far apart mirage targets should be placed to give meaningful results, could be calculated on the spot. The balancing of the radiation and heat budget was a task for computers in the comfort of a research office in civilization. Likewise, the wind structure of the inversion was mathematically too difficult to do anywhere other than with a main frame computer in the comfort of an office in the District of Columbia.

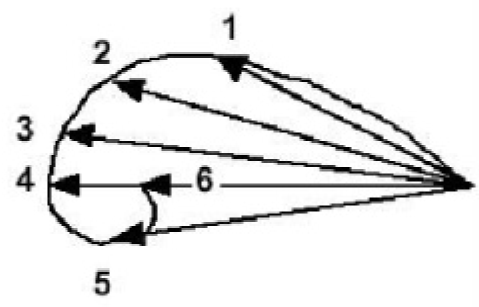
The two readings from each theodolite, elevation and azimuth from each tracking station a thousand feet apart every thirty seconds, actually gave more information than was necessary for a classroom problem on triangulation and positions in three dimensional space. The research approach I took back at Polar Met in Washington D. C. was to use and calculate every conceivable position to establish a volume of probability where the balloon was and with statistical smoothing techniques map out the most probable path the balloon took and from the balloon's changing positions calculate the wind speed at chosen levels. At Plateau Station all I could do was hope I had the right stuff.

The normal undisturbed neutral atmosphere on a spinning earth with friction produced an Ekman wind spiral for a hodograph. Let me explain. In the free hand sketch on the next page, let the wind near the ground in the Southern Hemisphere be mapped as vector #1, an arrow pointing toward the direction the wind is blowing with a length proportional to the wind speed. Then as you would rise to a certain height above the ground where there is less friction, the wind could be mapped again as vector #2 showing its longer length as a faster wind speed. Peculiar to the Southern Hemisphere is the fact that the spin of the earth affects air standing or moving steadily for a long period of time by turning the wind toward the left as you rise in the vertical.

Therefore at level #3 you can see vector #3 a little longer still than the lower level vectors and turned still more leftward.

Permit me now to jump to the top level, #6, where we see vector #6 on top of a longer vector #4 and to the right of vector #5. Each level is higher in altitude than the lower numbered level. When you place the starting point of each vector or arrow at the same point, you can see how these arrows' points sweep out a spiral. For neutral atmospheric conditions, where we face neither an unstable rising and mixing of air nor an overly stable atmosphere where we face sinking air such as in a strong inversion, these spirals are called Ekman wind spirals.

Their most fascinating feature is that these spirals show the wind increasing speed and turning to the left as you go to higher altitudes and as friction decreases but only to a certain level. Then the Ekman wind spiral shows the wind continuing its counter clockwise spiral to slightly slower speeds. In the atmosphere, as you rise higher and higher above the surface, the air is held back less and less because of friction; but in free flowing air, it overshoots and actually moves faster because of friction and must compensate and return to a slightly slower speed. Vector #6 represents the wind where friction of the ground has no effect and this wind is called the geostrophic wind because it is only influenced by the large regional scale pressure gradients in the atmosphere and the earth's spin.



All of us in meteorological research expected that the Great Antarctic Temperature Inversion would drastically alter the shape of the Ekman wind spiral. Disappointing to me was that at no single time or at a single place in 3-D space could one look up into the air and see an exact spiral. Everything had to be averaged seemingly to death. Weather, as a study of the movement of air, was a study of moving molecules, which moved in statistically innumerable ways apparently at random. Like the science of quantum physics, where the small scale events are not related to the large scale witnessable phenomena, micrometeorology never revealed the large scale effects except through averaging techniques.

It was haunting to me not to be able to "see" without averaging. The operational meteorologists who trained me before I became a research meteorologist had a certain disdain for averages. I remember mentors Larry Hughes and John Hovde from Chicago warning me about averages. An average is a collection of non-averages. There is one thing about an average that you can be certain of - few and many times none of the data that form an average are ever equal to the average.

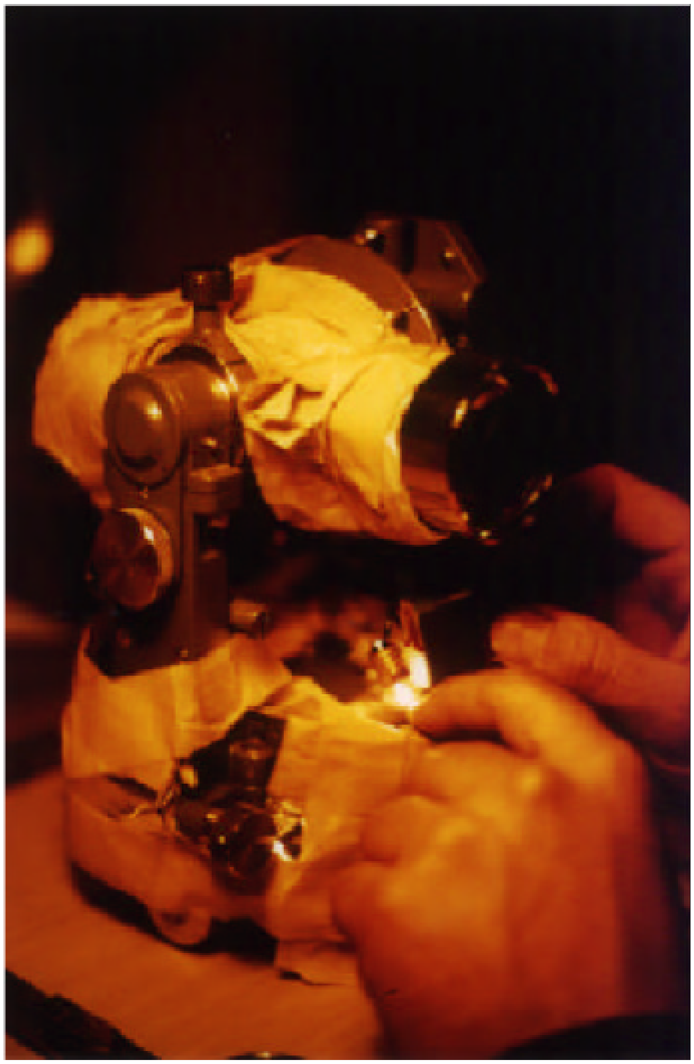
Top: Preparing for a balloon launch

Bottom: Marty Sponholz calibrating net radiometers
(Slide by Rob Flint)

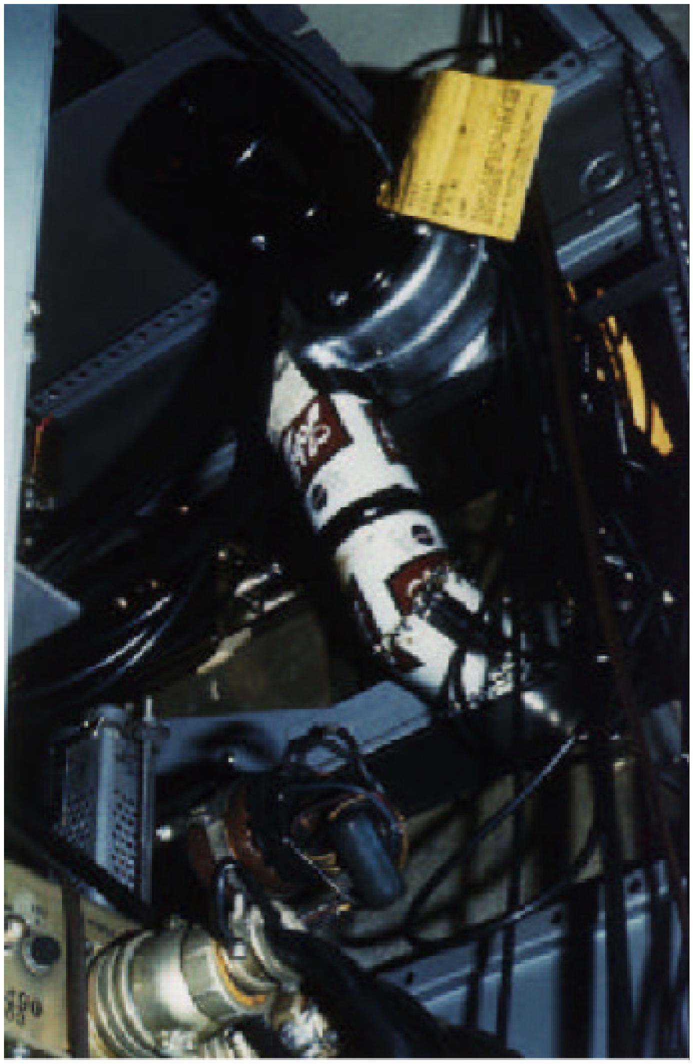
Effect of the extreme cold on the net radiometers

Top: Bob Geissel tracking a balloon in flight in order to compute the wind speed and direction by triangulation

Bottom: A bamboo triangle set up for a photographic study of mirages



Keeping a transit warm by packing it with hot activated pibal light batteries



Using any or everything available for repair of equipment at the isolated Plateau Station