

CHAPTER 11

D.C. Polar Studies

One of the most difficult situations I ever found myself in was a rift between the men at the University of Wisconsin and the leaders of the Office of Meteorological Research. Once I returned from Plateau Station I believe Lettau and Schwerdtfeger expected the Weather Bureau to turn all of the Plateau Station weather data over to them. I cannot say who all might have been involved from the Weather Bureau's side. Mort Rubin? Bill Weyant? It was Herb Viebrock who talked to me very privately. The gist of our conversation was around my responsibility to the Weather Bureau and questions of what might have been promised to the Wisconsin professors. I knew that Lettau and Schwerdtfeger did not want to be bogged down with the logistics of fielding a research team in Antarctica. Yet Lettau, for sure, conducted much research in Peru. The University of Wisconsin through the leadership of Charlie Bentley was the most active university group in the Queen Maud Land Traverse. Other Big Ten Universities conducted major work in Antarctica, most prominently the Richard E. Byrd Polar Institute at Ohio State University in Columbus, Ohio.



Overseas Operations and Polar Met, two offices of the Weather Bureau, provided all the field work for Plateau Station and they expected me to finish the job. The Plateau data was going to stay with the federal government. Perhaps after the initial publications the University of Wisconsin would be welcome to the Plateau data. Hearing this, I got to work.

One of the biggest tasks was collecting all the supporting data to the inversion study. All attempts to receive coordinated weather observations from other stations in Antarctica while we were launching balloons proved futile. Here Fred Fopay, normally non-conversant with anybody, showed his value by knowing where to obtain any weather fact anywhere in the world for any time. Slowly, through his pursuits, I was able to obtain the supporting observations needed.

Thanks to Fred I learned that the Bureau of Meteorology of Australia maintained a history of the synoptic surface and upper air charts for the southern hemisphere. Fred compiled microfilms of these charts and I correlated them with the serial balloon ascents. Shown here are two charts related to the detailed and modeled wind profile discussed in the last chapter.

18 March 1967. "The optimum force required implement the concept of operations and to exploit success is considered 4 and 2/3 divisions or the equivalent; 10 tactical fighter squadrons with one additional base; and the full mobile riverine force. The order of magnitude estimate is 201,250 spaces in addition to the 1967 ceiling of 470,366 for a total of 671,616." (Cablegram from Gen. William C. Westmoreland, commander of United States forces in Vietnam, to Pacific command.)

In the first chart, the surface synoptic chart shows an intense low pressure deepening off Prince Harald Coast just north of Lützow-Holm Bay. (955 millibars = 28.20 inches of mercury) As this storm intensified, its low pressure clockwise circulation extended up into the upper atmosphere's polar vortex of strong west winds [defying every undergraduate geography text]. This deepening surface low pressure pulled and stretched the polar vortex aloft to the coast so that at 500 millibars, as the second weather chart shows, a strong north wind carried moisture deep into the interior of Antarctica

above the polar plateau and above the great temperature inversion.

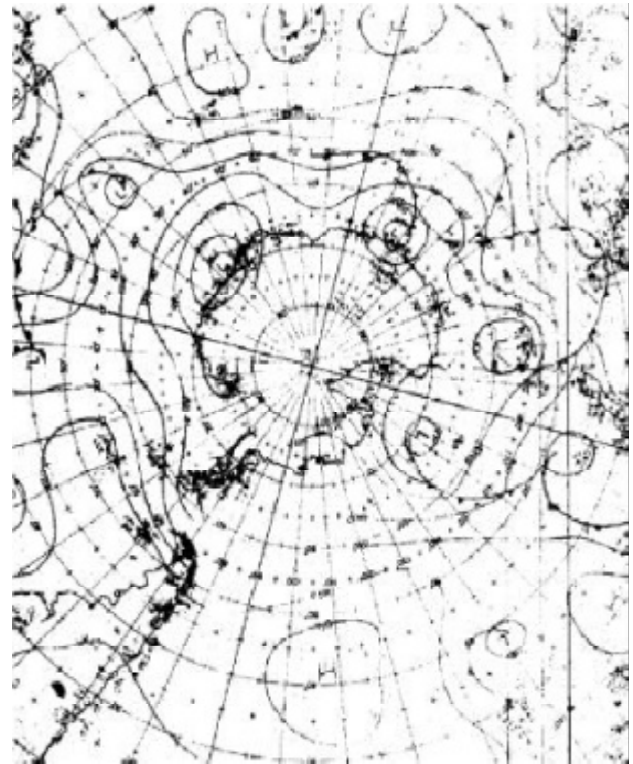
A bigger picture was beginning to take shape with a great deal of personal excitement. This was the source of moisture to the interior. No appreciative weather system such as a frontal system or organized cyclonic system was invading to the extreme altitude of Plateau Station. The isothermal layer of the atmosphere above the inversion probably filled to saturation. The inversion provided the descending air flow bringing saturated air to lower and much colder levels. Some high thin cirrus clouds were visible in the night sky but not so dominant as to be the source of the ice crystals in the air. These were Schwerdtfeger's ice crystals from a clear sky. The net radiation data from Pete Kuhn's radiometersondes (Chapter on Research in Winter), added along side a graph of the temperature inversion for the same date, 7 August 1966, showed a net radiation warming at the lower layers as latent heat from ice crystal formation was released into the inversion.

The synoptic analysis received was so exciting and such an aid in piecing together what was happening on the polar plateau that it stimulated a new proposal. Never achieved at Plateau Station was a communications network between nearby stations to enable scientists to predict weather at their stations in order to carry out effectively timely observational events such as my serial balloon launchings. I wrote in my proposal:

"Preliminary analysis of some of the soundings made at Plateau Station (1966) have shown a low level jet at the top of the inversion. The most dramatic case found so far showed a wind speed of 5 meters per second ten meters above the surface (standard aerovane level). 200 meters above the surface the wind speed had increased to 12 meters per second at the top of the inversion and decreased to 4 meters per second 400 meters above the surface. If such a thin layer jet wind exists on a large scale over the polar plateau, it would not be seen by the normal balloon soundings made at Amundsen-Scott and Vostok due to their rapid rate of ascension."

"Suggestions concerning a study of the Great Antarctic Inversion on a temporal and spatial basis had originally been made by Professor H. H. Lettau and Professor W. Schwerdtfeger, both of the Department of Meteorology at the University of Wisconsin, in a memorandum to NSF November 1964. Their continued interest and guidance in the studies at Plateau Station and the Antarctic inversion would be helpful for working up a coordinated program between the three stations on the polar plateau. Considering the planned termination of Plateau Station after the winter of 1968, special coordinated soundings at Amundsen-Scott and Vostok for that year would take maximum advantage of the remaining lifetime of Plateau Station."

"... The meteorologist who carried out this type of program at Plateau Station during its first year of operation is willing to carry out the field work at Vostok and would also work closely with the meteorologist chosen for Plateau and Amundsen-Scott if such a program were approved. . . . M. P.



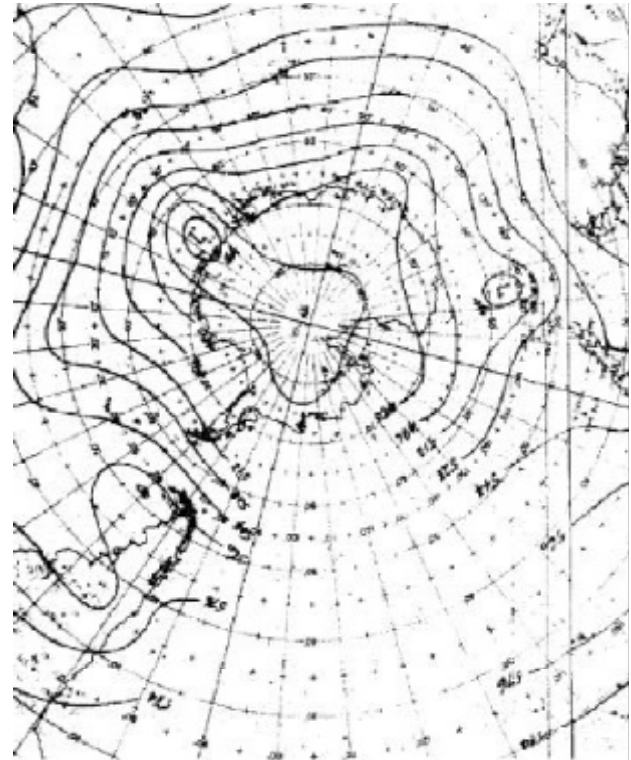
**Bureau of Meteorology, Australia
Surface 00:00 Z, 07 August, 1966**

Sponholz, 5 April, 1967.”

Paul Dalrymple flew down to D. C. and assisted me with this proposal. Likewise Bill Weyant pushed his connections. Harry S. Francis, Program Director, International Cooperation and Information Program at NSF, was key to this proposal. He gave us a hearing and about a year later, too late for the connection with Plateau Station, I did get an invitation to join the Soviet Antarctic Expedition at Vostok. In addition to my college years of studying the Russian language, I filled the Washington evenings with Russian classes from the State Department and practiced translation by keeping Polar Met's notes on Russian Expeditions to Antarctica up-to-date.

In 1967, before a proliferation of personal computers and before global communications via satellite all these interpretations had to be done on the basis of data brought together by staffs of people a long time and a long way away from the point in time and space where the observational project was conducted. How much better all of these interpretations might have been with instant access to all these data and data analysis.

It took several years even with main frame computers, not at all consumer friendly, to work through the millions of calculations. Programming in FORTRAN for the research scientist was a must. It never was enough to simply calculate the position of the balloon and the speed at which the balloon was carried along by the wind, but with the computer I needed always to calculate the most accurate or most probable position taking advantage of every possible measurement and weighting good data over against poor data.



**Bureau of Meteorology, Australia
500 MB 00:00 Z, 07 August, 1966**

Was this proper science? It certainly was normal science, but as I struggled with these methods of data fitting it struck me that a book on scientific revolutions I read while working in the Weather Bureau at Chicago on the University of Chicago campus exactly described the work that I now was doing. “. . . it is hard to make nature fit a paradigm. That is why the puzzles of normal science are so challenging and also why measurements undertaken without a paradigm so seldom lead to any conclusions at all.” (Thomas S. Kuhn, *The Structure of Scientific Revolutions*, Chicago: University of Chicago Press, 1962) I was beating the data into submission following the paradigms outlined by Lettau and Schwerdtfeger. Lettau had defined katabatic wind flow as a short term local phenomenon not involving the coriolis force from the earth's spin. Schwerdtfeger laid down the path to inversion wind based on thermal winds caused by the slope of the icecap.

It was time to go to Madison, Wisconsin for the next step of research. At Madison, Pete Kuhn and his staff were eager to evaluate the net radiation data. There was little problem here. Pete Kuhn was with the Environmental Science Services Administration (ESSA) and its Atmospheric Physics and Chemistry Laboratory so that negotiation for cost and control of the data was easily taken care of by Bill Weyant from Washington D. C. At Madison, by being affiliated with both the federal govern-

ment and the University of Wisconsin, Pete Kuhn had access to an IBM 1620 and a CDC 6600 computer to do a major amount of data analysis. During the many weeks spent on data reduction Pete and I performed several net radiometersonde launches from Picnic Point and monitored the temperature and net radiation over Madison and down wind from Madison up to 10 millibars, well into the stratosphere, to simulate the polar cold with our diesel fuel soaked radiometers. I never was quite sure how bad or how insignificant the diesel fuel treatment was. Pete Kuhn was always eager to publish and when he wasn't pushing me for a joint publication on the radiation part of the inversion study I guessed comparative experiments were not very good. In Antarctica, at -115 ° F, with every instrument rupturing on exposure to that severe cold the only choice was to obtain the data by soaking the balloons and net radiometersondes with diesel fuel.

I met a very pretty girl while in Madison and learned she was attending a geology class given by my favorite teacher, Professor Laudon, in B10 Commerce. Six years earlier, when I took Geology 101 I enjoyed his method of filling six blackboards and running through several trays of slides all in an hour. I learned from this girl that he was still at it and it was OK to come to class with her. I did.

She represented the state of Wisconsin as a queen of the state's agricultural products and later even achieved runner up to Alice in Dairy Land. She was an amazing person to be with and talk to and then Prof. Laudon came on stage and began his lecture.

I could not believe what I heard. Prof. Laudon was explaining with all certainty the continental drift theory. I was so overwhelmed by such a turnabout in the professor's fundamental views that I momentarily forgot the girl, and pursued my inquiries of Laudon. When I took geology about six years earlier he ridiculed the continental drift theory. The continents were fixed and immobile. North America grew in size around the very hard Precambrian rocks of the Canadian Shield. Each new evolutionary age added to the continents' sediments by ocean advances and withdrawals. New continents formed between island arcs as sediments filled in between.

North America emerged between the Appalachian Islands and the newer Rocky Islands. Scientists were looking for new continents to emerge between the Pacific Island arcs. Indonesia was a recent

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260000800009RS2600008096
LOAD DATA
TERMINATION OF DATA
TERMINATION OF DATA
PDQ FORTRAN C2
START
C      TEST PROGRAM AZIMUTH ESTIMATES
C      PLATEAU STATION
C      0.05 DEGREE INCREMENTS
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      EBW, ZBW
5      FORMAT(13X, 216, 4F7.2)
      STEL=ESTRN*.01745329
      ZSTRN=ZSTRN*.01745329-.00872664
      BWEL=EBW*.01745329
      ZBW=ZBW*.01745329-.00872664
      Z1=SIN(BWEL)
      X2=COS(BWEL)
      X3=SIN(STEL)
      X4=COS(STEL)
      RATAN=(X1*X4)/(X2*X3)
      NUM=0
      DO 36 N=1,20
      ZSTRN=ZSTRN+.00087266
      DO 35 J=1,20
      NUM=NUM+1
      ZBW=ZBW+.00087266
      VAL1=SIN(ZBW)
      VAL2=SIN(ZSTRN)
      VAL3=COS(ZBW)
      VAL4=COS(ZSTRN)
      RASIN=VAL1/VAL2
      TEST=RATAN-RASIN
100     ABW=ZBW*57.295779
      ASTRN=ZSTRN*57.295779
      HT1=BASLN*X3*VAL1/((VAL1*VAL4
      -VAL2*VAL3)*X4)
      HT2=BASLN*X1*VAL2/((VAL1*VAL4
      -VAL2*VAL3)*X2)
35     PUNCH 101, LEVEL, ITIM, EBW, ESTRN,
      ABW, ASTRN, HT1, HT2, NUM,
      TEST
101     FORMAT(215, 4F7.2, 2F8.0, I6, F20.8)
36     ZBW=ZBW-.0174532
      GO TO 1
      STOP
      END

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emergence.

Ideas of the super puzzle, that continents once fit together and moved apart, when I had Prof. Laudon were taught as examples of a quack hypothesis. Prof. Laudon in 1961 made mockery and ridicule of such ideas. He taught that if North America moved, the young Rocky Mountains would have to be the older ones and the Appalachians shouldn't even exist. The mountains of Australia would be on the wrong side of the continent, most rivers of Asia would be going the wrong direction, and finally, the puzzle wasn't a very close fit.

Now six short years later whatever happened to these reasons? Certainly the Rocky Mountains did not change these last six years. Australia did not suddenly flip around these past years to give reasons for Prof. Laudon's sudden conversion. Whatever happened to all the reasons he gave that so certainly proved the opposite? At the end of class, completely forgetting the girl I came to class with and was sitting next to, I leaped up on the stage to catch the professor before he left and begged for an answer for his changed views. He ignored me and walked back stage and disappeared. I was intercepted by one of his graduate teaching assistants. In the conversation that followed it became evident that the graduate student didn't even know that Laudon ever taught anything else. The girl was gone.

The University of Wisconsin was always a dynamic campus where any and all ideas were freely debated. That did not change. Frequently during my stay that spring on the Madison campus there were many protest marches and demonstrations against the war in Vietnam. I knew the United States had a few thousand advisers in Vietnam when I left for Antarctica. I now was stunned at the large number of troops our country committed to this Asian country. I was even more taken aback by the lack of interest by the general public. President Johnson increased the American commitment ever so slowly so that few of the public realized it. Returning home from Antarctica where daily news was unavailable now gave me the shocking picture of this sudden and enormous commitment of young drafted men heading off to jungles without much congressional or national debate over how our national security was harmed by communists in Vietnam or how much the Vietnamese really wanted cutthroat capitalism.

I prayed for my friends, the Sea Bees of Plateau Station, and their work in Vietnam. The war was getting so large few young men would escape its scars, but thus far the build up was too gradual to be noticed by the general population. The protest song:

“How many seas must a white dove sail
Before she sleeps in the sand?

How many times must a cannon ball fly
Before they're forever banned?”

brought tears to my eyes and the inactive “silent majority” committed me to activism. Even my church denomination blindly supported this unjust war and was wrongly committed to a blind obedience to government when a democracy expects and demands a knowledgeable voting constituency. Our own Augsburg Confession (Article XVI, paragraph one) and the Apology (Article XVI, paragraph one)



expects a judgment calling for service for “just wars.” My grandfather left Germany to avoid the unjust wars perpetrated by Kaiser Wilhelm. I could understand a similar move to Canada during these times. In the following years, after many thousands of Americans came home in caskets and thousands more came home broken for the rest of their lives with no hope for change and even a seeming rejection of Americans by the Vietnamese themselves, the American voters changed America’s direction.

My visit and exchanges with Werner Schwerdtfeger and Heinz Lettau, my former teachers, were very cold. Conversations stayed primarily on the travelogue and the experience of living in the Antarctic. They only wanted the data from Plateau Station, which Weyant and Viebrock as my immediate superiors did not permit.

Likewise, Schwerdtfeger and Lettau offered no theoretical information. All of us knew too well that we no longer had a student to teacher relationship. I found this very hard. I felt I still was their disciple. In one heated moment I was accused of giving data to K. B. Mather and G. S. Miller of the University of Alaska. The Alaskans had recently published an article, “Wind Drainage Off the High Plateau of Eastern Antarctica.” They must have used government publications of the standard surface observations that are made available daily as it is collected under various international agreements.

I could understand the anger of the Wisconsin men. They were scooped on the wind flow by the easiest and wrong idea that simple katabatic flow with a coriolis force changing it was the cause for wind circulation out of the Antarctic. I too was scooped. The entire research system of the Federal Government was scooped. Even the NSF was a little embarrassed. They had spent all this money on my project to have two guys in Alaska read daily weather reports and publish results before the scientists could get home from the field.

This research world had its entanglements. I was hot with rage before meeting Werner Schwerdtfeger on this visit. On the very same day that I returned from Plateau Station Weyant informed me that Werner Schwerdtfeger’s nephew had convinced the NSF that his Australian University in Melbourne with another lecturer, Dr. Uwe Radok, be allowed to study the snow’s energy balance at Plateau and that all my data of the subsurface temperature be given to them. I never trusted Werner Schwerdtfeger’s innocence in this matter. In my view, Ed Flowers, Herb Viebrock, and I initiated the subsurface temperature and energy program and it was stolen from us. Maybe it was because of insider knowledge at Madison, knowing that I was doing this subsurface study, that the Universities of Alaska and Melbourne redid it all. For sure the international politics had its part. Plateau Station was in the sector claimed by Australia.

Two graduate students patiently waited to see me. Walter Dabbert, a student of Lettau, and Larry Mahrt, a student of Schwerdtfeger, were well into the theory of the Great Inversion over East Antarctica. Their graduate status and degrees were dependent on the findings at Plateau. How could

27 April 1967.

General Westmoreland: “. . . this war is action and counteraction. Anytime we take action we expect a reaction.”

President Johnson: “When we add divisions can’t the enemy add divisions? If so, where does it all end?”

(Notes of a conversation written by John T. McNaughton, Assistant Secretary of Defense for International Security Affairs)

4 May 1967. “As to Vietnam, it seems very doubtful that further intensifications of bombing in the North or major increases in U. S. troops in the South are really a good way of bringing the war to a satisfactory conclusion. As to the United States, it seems clear that uncertainty about the future size of the war is now having destructive effects on the national will.” (Memorandum for President Johnson from McGeorge Bundy)

I hold back? I could have. For my own professional growth I should have. I didn't. I was a young and naive researcher hardening fast.

At church the next Sunday I saw Nancy, the gal from geology class again, asked her to a play at the Student Union, *Medea*, an ugly story about a woman killing her own children, put on by the Wisconsin players. I had to leave shortly and return to Washington D. C. and knew such a romance was impossible. She visited me there and before the summer was over we were engaged to be married.

Returning to Washington D. C., disillusioned about research, I thought theory only followed observations. Lettau and Schwerdtfeger never stopped theorizing and they had every right to do so. That was their generosity. That is why so many of us wanted to follow them. Their minds were never slowed. But it was becoming obvious to me that it didn't matter what my data showed. If my data did not fit their theory it would be ignored. If it did fit their theory - of course, it was expected.

