Comments on "Midwinter Stratospheric Warmings in the Southern Hemisphere: General Remarks and a Case Study"

A. D. BELMONT, G. W. NICHOLAS AND W. C. SHEN

Control Data Corporation, Minneapolis, Minn.

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Our paper entitled "Antarctic Stratospheric Warmings During 1963 Revealed by 15-μ TIROS VII Data" published in this issue was released as a contract report before a recent paper by Julian (1967) was published. As Julian discusses the same warmings, it is necessary to comment on his conclusion that no major stratospheric warming has yet been satisfactorily observed in the Southern Hemisphere. He recognizes several possible explanations for the inconsistencies of radiosonde, rocket and satellite temperatures and winds. Because his conclusion relies so heavily upon the use of the thermal wind equation, it should be emphasized that the data applied to the thermal wind equation must be of consistent space and time scales, especially as observed local winds and temperatures may both vary greatly. This alone is sufficient reason not to consider the estimates as reliable. It is most difficult, if not impossible, to know to what degree one is justified in smoothing the highly critical values of the wind at levels where even the sign of the wind shear varies with height. The rocket wind profiles shown in Julian's figures all show alternating increases and decreases in wind speed with height in the layer from 26 to 30 km upon which he relies heavily. Interpolating a single observation in time or extrapolating one in space may easily contribute unrepresentative values. A remaining cause for the inconsistencies can be found in the unknown accuracy of all the data. Indeed, about the only way we can conclude anything from meteorological observations is to rely upon space or time agreement of the data.

Julian states that "It is difficult to understand how the radiometer could have failed to detect a warming as strong as that suggested by the McMurdo rocket data in July and August and yet so faithfully show the details of the final warming a month or two later." From the analysis done here, the radiometer detected a significant warming (be it major or minor) that occurred from approximately 20 July to 13 August 1963. The warming extended to at least 60S near 170E. There are a number of reasons why Julian's analysis may have failed to reveal this warming. First of all, on the assumption that he investigated the radiometer data around 13 August which should have shown a significant increase over the period 24–30 July, the radiometer data may have failed to reveal the warming if they were not corrected for instrumental degradation, a point he does not mention. During July the floor side of the radiometer was collecting data at the southern extremity of the orbits; by the middle of August, the pattern had changed and the wall side was covering the same area. At this time the wall side was reading about 8C lower than the floor side. An 8C lower temperature at the time of the warming would have completely obliterated it.

Second, if the alternating open mode data were not eliminated from the analysis (a point which is not clarified), the gross mislocation of these data by the computer is certainly sufficient to smear out any change in the radiometer data that might have shown the warming. This mislocation is due to the failure of the logic in the computer programs to adequately distinguish between floor and wall measurements in all cases during the alternating open mode. The result is that part of the wall measurements are located as the floor, and vice versa, and part are correctly located. The effect again is to smear the true thermal pattern when grid-point averages are used.

A third reason why his analysis may have failed to reveal the warming in mid-August concerns the use of large nadir angles. Although he does not state what limiting nadir angle was used in the radiometer data maps, it can be assumed that an angle near the absolute limit of 58° was used to obtain coverage to 70–72S with 10 or more observations per grid-point. There are two points to consider when using these large nadir angles. First, the weighting functions shift upward with increasing nadir angle. Considering Julian's Figs. 1 and 2, for example, and considering that the maximum in the weighting functions shifted upward from 26 km at a 0° nadir angle to 30 km at 58°, the measured radiometer value would increase 7–8C. Then, when the values from large nadir angles are averaged with values of small nadir angles, the thermal field can be significantly changed. Second, the coordinates are determined for only every fifth sampled value from that portion of a satellite revolution that sweeps the earth from horizon to horizon. The four intermediate points are located by interpolation. As very often happens, the points beyond the last anchor (fifth) point are grossly mislocated when the computer attempts to interpolate for them. These points beyond the last anchor point involve only large nadir angles and should not be allowed to enter the mapping program as they, too, are not reliable.

All or any one of the above considerations could have

prevented Julian from recognizing the warming of mid-August that actually was revealed in the radiometer data.

Definitions of stratospheric warmings

We agree with Julian that it is unlikely that any definition of stratospheric warmings will be universally acceptable. However, the suggested classification used by the IQSY Stratospheric Warming Program suffers from the same difficulties as any definition based upon the amount of temperature change or absolute temperature attained. Whether a warm anticyclone remains at high latitudes or is centered precisely on the pole has little significance to us. All degrees of size, duration, trajectory, and degrees of symmetry to the pole exist just as they do for rates of warming. It is not always possible to separate the final or spring warming from the numerous pulses which often precede it—just as in our concurrent paper, the second warming may easily be considered a part of the final warming process. The question is a relative one and can perhaps be answered best only by inspection of time graphs of the temperature at individual stations. The question is not usually whether there is a warming, but rather how intense and how extensive it is. Obviously, temperature changes at one station only, where nearby stations report nothing unusual, are insignificant. The extent, the duration, the rate of rise in temperature, and the total rise are factors which contribute to the relative importance of sudden warmings. Nor, incidentally, should local reversals of the zonal wind be considered necessarily as evidence of warmings.

In Fig. 1 from a still-unpublished contract research report (Engberg and Belmont, 1964), graphs of each radiosonde temperature reported during a 5-yr period at 50 mb for six arctic stations were prepared. These clearly show that the numerous temperature rises and falls each winter at certain North American arctic stations were a common event. It is virtually impossible, as Julian recognizes, to establish arbitrary limits to define which of these changes should be termed warmings and which should not. We must rely upon the general familiarity with the processes to recognize outstanding warming events. Even the use of a fixed attained temperature may lead to the ignoring of the strongest warming one year and to the inclusion of several minor intermittent separate warmings in another year. That the same holds true in the Southern Hemisphere seems most reasonable, although the amount of stratospheric data yet available is too small to permit recognition of the differences in preferred origin, trajectory, frequency, and intensity of antarctic warmings. To do this properly, we look forward to Nimbus C, which will also measure 15-μ radiation because its polar

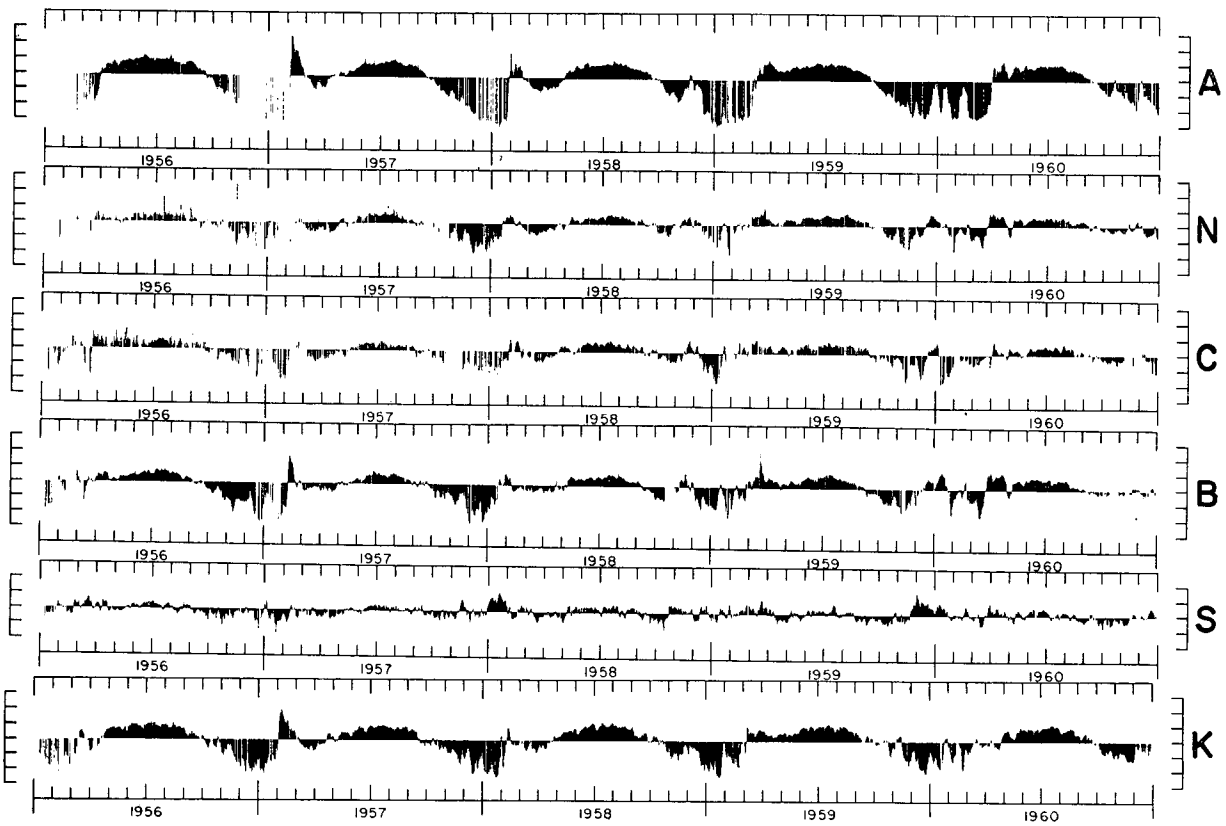

FIG. 1. Observed 12-hr temperatures at 20 km plotted as deviations from the 5-yr mean, 1956–1960. The vertical scale unit is 10C, and warmer values are plotted above the line. A = Alert, N = Norman Wells, C = Churchill, B = Barrow, S = St. Paul Is., K = Keflavik.

orbit will for the first time make possible a truly global analysis of the polar stratospheric temperature field.

The suggested dependence upon reversal of the meridional temperature gradient requires a more precise definition of how this gradient is to be measured. Whenever there is an isolated warm anticyclone at high latitudes there is certainly a reversal of the normal wintertime temperature gradient from a position south of the anticyclone to one within the anticyclone. On the other hand, there may be no change in gradient if one compares temperatures at the equator and the pole. Thus, the scale of the measurements must be comparable to the scale of the disturbance. Are temperature gradients to be measured at individual longitudes or should they be the gradient of the zonally averaged temperature? These questions merely demonstrate again how difficult and probably how unnecessary it is to apply any arbitrary yardstick to the measurement of such a variable phenomenon.

REFERENCES

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