Reply

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It is admittedly dangerous to generalize the results of a single case study. However, in a previous article[1] Dr. Sanders and I listed several cases of the type considered; and recently Sawyer[2], through analysis of data gathered by the British Research Flight, has established the generality of the dry, subsidence-type front. Whether the case of 14 to 15 December 1953 was similar in all respects to the usual dry front may be questioned. Certainly the descent of stratospheric air to 700 to 800 mb must be considered extreme, and therefore a much greater portion of the frontal zone was composed of stratospheric air than is usually the case. However, I have acquired considerable evidence to support the idea that intense frontal zones in the *upper* troposphere are commonly in the nature of stratospheric wedges. Furthermore, there is no reason for assuming that the pattern of vertical motion was different in the case in question than in the normal case of dry frontogenesis, except that it was more intense. Thus it was felt that there were some grounds for generalization. Certainly it is not claimed that the boundaries of dry frontal zones are, in all cases and throughout their entire extent, folded tropopauses.

As regards the quotation from Riehl, LaSeur, *et al*, I feel that there can be no question that the great majority of vigorous cyclones are associated at some stages in their histories with well-developed temperature gradients. It is the precise nature of this association that is open to question, and it is interesting to note in this respect that the latter part of the quoted statement is in direct contradiction to the classical polar-front theory which requires a pre-existing front for the initiation of cyclogenesis. The case of 14 to 15 December 1953 may be regarded as a substantiation of this second part of the statement by Riehl, LaSeur, *et al.*

Since Messrs. Boville and Creswick admit "that fronts are not permanent substantial surfaces and they are continually undergoing three-dimensional frontogenesis and frontolysis", it would seem that there will always be portions of the map where the frontal structure is ill-defined. I have simply recommended that in such areas it is better to represent the baroclinic structure by isotherms than by arbitrary or imaginary lines called fronts.

Messrs. Boville and Creswick have gone to considerable length to show "how a variation in approach can affect an analysis, particularly in areas of scant or uncertain data", and on the basis of their demonstration have claimed that a different interpretation can be placed on the data. They have chosen as their case in point the question of the origin of the air which was located on the 300K isentropic surface in the vicinity of HAT (304) and DCA (405) at 0300 GCT 15 December. In fig. 9f of the article, Washington (405) and Hatteras (304) were analyzed within the kidney-shaped area of stratospheric air which was traced back to the Alberta region of Canada on the earlier charts. Messrs. Boville and Creswick, on the other hand, claim that the air near Hatteras was located near Pendleton, Oregon, at 0300 GCT 13 December, and that the air near Washington was in the vicinity of Denver at 1500 GCT on the 14th.

Even if allowance is made for the uncertainties of trajectory tracing, the differences in these estimates

[1] R. J. Reed, and F. Sanders, "An investigation of the development of a midtropospheric frontal zone and its associated vorticity field," *J. Meteor.*, **10**, 338–349, 1953.
[2] J. S. Sawyer, "The free atmosphere in the vicinity of fronts," *Geophys. Mem.*, **12**, No. 96, 24 pp., 1955.

are extraordinary. Therefore, at my request, five qualified meteorologists, who had no knowledge of the point in issue, constructed trajectories forward in time from Denver and Pendleton, and backward in time from Hatteras. The trajectories were based on streamlines and isotachs analyzed at 12-hr intervals from actual winds. All five forward trajectories missed Washington and Hatteras by wide margins (700 mi or more). All five backward trajectories kept the air within or at the margin of the kidney-shaped area.

These data strongly suggest that Messrs. Boville and Creswick's trajectories are in error. In support of this suggestion, I offer the results of vertical velocity measurements made by independent methods in the area and at the times in question. The methods used were the kinematic method (integration of the continuity equation) and the vorticity method (solution of the Sawyer-Bushby[3] numerical prediction equations). The vertical motions computed by the latter method reveal, at 0300 GCT 14 December, an extensive region of sinking motion centered near the Louisiana coast. Instantaneous vertical motions in excess of 400 mb per 12 hr occurred at the 600-mb level over an area embracing Louisiana, Mississippi, east Texas, and adjacent portions of the Gulf of Mexico. Twelve hours later, at 1500 GCT on the 14th, the intensity and areal extent of the descending motions were little changed, while the point of maximum descent had shifted to southern Virginia. The vertical velocities determined by the kinematic method agreed well with the foregoing results.

Thus, two independent sets of vertical velocity measurements have confirmed the strong subsidence indicated by my trajectories and those of the five disinterested meteorologists. Messrs. Boville and Creswick's trajectories, on the other hand, give a descent of only 110 mb per 48 hr between Pendleton and Hatteras, and 20 mb per 24 hr between Denver and Washington.

The causes for the errors in Messrs. Boville and Creswick's trajectories may be ascribed to a number of questionable procedures on their part:

1. Their estimate of the potential vorticity near Pendleton at 0300 GCT on the 13th is based on a wind observation in which the pilot balloon obviously suffered a leak or some other mishap. Along a line from Boise, Idaho, to Tatoosh Island, Wash., the reported winds at 500 mb varied from 58 kn at Boise to 122 kn at Pendleton to 68 kn at Tatoosh Island. Yet the contours showed little variation in spacing, indicating near constancy of the geostrophic wind.[4] Using the false wind from Pendleton, Messrs. Boville and Creswick measured an *absolute* vorticity of as small as $-f$ (the Coriolis parameter) on the 300K isentropic surface. Had they made the measurement on the 310 K surface, the absolute vorticity would have been $-3.5f$! Riehl *et al*, in their analysis of data from jet-stream flights,[5] have found that the absolute vorticity approaches zero to the south of intense jets but have not reported a single instance of negative absolute vorticity.

2. In order for Messrs. Boville and Creswick's trajectory to indicate approximate conservation of potential vorticity between Pendleton and Hatteras, it is necessary that they made an equally serious error in measuring the vorticity at the Hatteras end of the trajectory. This arises from improper extrapolation of the Hatteras wind sounding, which terminated near the base of the frontal layer. The 300 K isentropic surface was 1000 ft above the lower boundary. The temperature gradient within the frontal zone, as estimated from the cross section in fig. 13, was 10 C per 100 km, corresponding to a thermal wind of 27 kn per 1000 ft. The actual wind shear at Norfolk, the nearest station to Hatteras, was as high as 23 kn per 1000 ft. Since the actual wind speed at the base of the front at Hatteras was 67 kn, I extrapolated a 90-kn wind speed at 300 K (690 mb), as can be seen from fig. 8, the figure used in the vorticity measurements. Messrs. Boville and Creswick, however, preferred to use the last reported wind, which was at 700 mb and which was subject to the uncertainties of a terminating wind. The different procedures lead to quite different values of vorticity in the area in question.

3. Messrs. Boville and Creswick have not disputed the correctness of the 93-kn wind at Norfolk (308) on the 300 K surface, nor the 70-kn wind at Washington (405). Yet they have written "it is plausible that the isentropic wind shear across the three stations (Hatteras-Norfolk-Washington) is effectively zero". Nothing could be less plausible. When the data are deliberately violated, it is inevitable that they may be interpreted differently by different analysts.

4. Another procedure that may be questioned is the use of the mixing ratio as a conservative parameter. For one thing, the humidity element currently in use on the radiosonde has such a large lag that it is difficult to determine the true mixing ratios through a dry inversion. Furthermore, mixing ratios were missing in the upper portions of the frontal zone and in the warm air above due to "motorboating" (see my fig. 1). Thus, a rather substantial layer was present in which mixing ratios were not available. Finally, it appears likely that, in a zone with such a large vertical gradient of mixing ratio, sufficient moisture would be transported upward by eddy diffusion to affect seriously the conservation of this property. Because of these difficulties, I made only qualitative use of the mixing ratio in my investigation.

So, although I will not deny that there may be portions of the study that admit of somewhat different interpretation, I nevertheless feel that the differences noted by Messrs. Boville and Creswick must be attributed to indiscreet handling of the data on their part rather than to the uncertainties that are admittedly present in the data.

[3] J. S. Sawyer, and F. H. Bushby, "A baroclinic model atmosphere suitable for numerical integration," *J. Meteor.*, 10, 54–59, 1953.

[4] *Note added 8 October 1956.* Examination of the original record reveals that, under "Notes," the observer remarked "Data questionable from 16,000 to 21,000 ft."

[5] H. Riehl, M. A. Alaka, C. L. Jordan, and R. J. Renard, "The jet stream," *Meteor. Monogr.*, 2, No. 7, 100 pp., 1954.