Comments on "Vertical Motion Evaluation of a Colorado Snowstorm from a Synoptician's Perspective"

CHARLES A. DOSWELL III

National Severe Storms Laboratory, Norman, Oklahoma

25 April 1989 and 26 July 1989

In his recent article, Dunn (1988) presents three varying perspectives on the causes of vertical motion: a "conventional" view using pattern recognition and positive vorticity advection (PVA),[1] a Q-vector analysis, and "ageostrophic" processes. Clearly, the object of this effort is to understand what most likely had been a forecasting problem for the Denver forecast office. To attempt to develop a deeper understanding of such events is commendable.

Unfortunately, Dunn's analysis of the situation reveals that he has misunderstood several aspects of the problem. To begin with, in his so-called conventional perspective, Dunn has suggested that the "optimal" position of the surface low is near the Oklahoma panhandle. This "optimal" pattern is supposed to produce upslope flow and "midlevel warm advection precipitation." I believe that the surface flow shown in his Fig. 2 is, indeed, upslope and that Fig. 1a shows rather pronounced warm advection. At least at 1200 UTC on 28 September 1985, low-level upslope flow and midlevel warm advection did not require the surface low to be near the Oklahoma panhandle.

If pattern recognition is to make any scientific sense and to have any chance for success within the context of natural variations, it needs to be keyed to physical processes, rather than being tied literally to specific feature locations. An ideal formula for forecasting failure is for one's pattern recognition to be limited only to those patterns "normally associated with" a particular event.

It is within Dunn's quasi-geostrophic (QG) analysis of the case that he makes his most serious misinterpretations. The first of these is related to the suite of products available to forecasters on AFOS. Given the fields that forecasters have at their disposal, it is understandable that the closest one can come to employing the Sutcliffe–Trenberth approach is to overlay the 500 mb vorticity and the 1000–500 mb thickness fields. Unfortunately, this is not a very good way to diagnose vorticity advection by the thermal wind. Given the choice, one should evaluate several layers in the vertical, each with the vorticity field near the middle of the thickness layer (e.g., 500 mb vorticity advection by the thermal wind between 600 and 400 mb). The fact that Dunn did not do this is not his fault, of course, but it makes his evaluation of the Sutcliffe–Trenberth approach rather unfair.

Dunn says that ". . . thermal forcing frequently appears quite weak during the warm season, especially given the standard operational contour intervals. . . ." While I agree that the standard contour intervals on operational AFOS charts often are inappropriate during the warm season, when it comes to evaluation of QG vertical motion from operational charts, I believe that low-level thermal advection usually is a stronger signal than vorticity advection during the warm season (see, e.g., Maddox and Doswell 1982). As noted by Homan and Uccellini (1987), the vorticity advection component may be present as well, but it may be more difficult to diagnose operationally than thermal advection at low levels.

Of course, it is the *vertical change* of vorticity advection that counts in QG theory, but diagnosing this is somewhat difficult—but not impossible—via the standard AFOS charts. If one makes the textbook assumptions about the QG *height tendency equation* (see Holton 1979, p. 140ff), the magnitude of the height changes at a given level is proportional to the sum of the vorticity advection and the vertical change of thermal advection. The latter is related to system amplification or decay, so for a system that is not intensifying

---

[1] In deference to readers of these journals in the Southern Hemisphere, I would like to advocate the replacement of "positive" and "negative" vorticity advection with "cyclonic" and "anticyclonic" vorticity advection, respectively. This small change (along with a few others, like "poleward" and "equatorward" flow for "southerly" and "northerly" flow) makes the concepts "bihemispheric."

Corresponding author address: Charles A. Doswell III, NOAA/ERL, National Severe Storms Laboratory, 1313 Halley Circle, Norman, OK 73069.

or dissipating rapidly, it may be neglected. Therefore, as a rough approximation, vorticity advection is proportional to height changes, a quantity that can be evaluated on operational charts. This makes it possible to get a qualitative feeling for the vertical variation of vorticity advection.

Next Dunn shows that the fields of **Q**-vector divergence

> . . . certainly do not pinpoint the forcing in the area of heavy snow. This may be due to the spatial resolution of the rawinsonde network, or it may be another sign along with the vorticity advection by the thermal wind analyses that the forcing for the heavy snow was not due to only QG forcing.

This analysis of the situation is the most pronounced interpretation error of the whole article. In the first place, there is no basis for expecting QG analysis to "pinpoint" a mesoscale region of heavy snow, as I have tried to elucidate elsewhere (Doswell 1987). QG process are inherently those associated with synoptic-scale systems; i.e., those characterized by Rossby numbers $\ll$ unity. Scaling arguments do not depend at all on the resolution of the rawinsonde network. Where the Rossby number varies in space (as in real data), QG diagnostics based on high-resolution data may give reasonable results in those regions where the Rossby number fits the QG scaling arguments, even if such regions have scales below that normally considered appropriate for QG scaling to apply. However, Dunn's Fig. 9 does not depict a failure of **Q**-vectors to depict the situation properly.

In fact, if one compares Fig. 9 with Fig. 7 in Dunn's paper, two quite different pictures of the QG "forcing" associated with this system are seen. Dunn has missed an opportunity to comment on this difference, which I do not believe is due *solely* to the geostrophic deformation contribution ignored in the Sutcliffe-Trenberth approach to QG diagnostics. Rather, it is related to the issue I raised earlier about Dunn's evaluation of the Sutcliffe-Trenberth scheme. Having a picture of **Q**-vector divergence at several levels is a reasonable depiction of the right-hand side of the QG equation for vertical motion. One simply does not achieve this sort of *quantitative* evaluation of geostrophic advections associated with QG vertical motion via 500 mb "PVA" arguments, or by trying subjectively to sum the separate (and partially cancelling) contributions from the thermal and differential vorticity advection terms, or by restricting the analysis to a single layer (and that rather crudely) as in Dunn's Fig. 7. For a quantitative evaluation of the QG processes associated with vertical motion, the **Q**-vector approach is probably the best method we now have [see comments by Lai (1988) and response by Durran and Snellman (1988)].

Moreover, one should be careful not to equate **Q**-vector divergence directly with vertical velocity in p-coordinates. In order to depict the latter, one must solve the omega equation. As noted in Barnes (1986), one can scale the **Q**-vector divergence to *look like* vertical motion, but by itself the **Q**-vector divergence field is only the first step toward a diagnosis of vertical motion. However, solving the omega equation can introduce problems, including specification of boundary conditions and time-consuming numerical solution schemes, so it often is convenient to overlook this point.

Finally, Dunn's so-called ageostrophic perspective fails to emphasize an important point, viz., that QG diagnosis implicitly includes an ageostrophic component to the flow. This ageostrophic flow is that required to produce the diagnosed vertical motion field through mass continuity. Although Dunn seems to know this, as evidenced by his observing the distinction in the phrase ". . . ageostrophic circulations at finer scales or *in excess* [emphasis added] of those forced by QG processes . . .", this passes by the reader rather quickly. I have no dispute that significantly non-QG ageostrophic flow exists during this event. In fact, I think it is the primary contributor, which is why QG diagnostics fail to "pinpoint" the mesoscale heavy snow event. Without additional data, it is going to be difficult to diagnose, much less anticipate such mesoscale processes. Nevertheless, I think the **Q**-vector diagnosis presents a very meaningful measure of the *synoptic-scale* contribution to this event.

Furthermore, Dunn's discussion still implies that the distribution of divergence aloft (and, under appropriate assumptions, the associated vertical motion) in the four quadrants of a jet streak is totally distinct from QG processes. As I have just noted, ageostrophic flow is implicit within the QG system; Hoskins et al. (1978) have shown that qualitative agreement with the four-quadrant model of the vertical circulations associated with a jet streak can be obtained with QG diagnostics.

With regard to the viability of Dunn's endorsement of (absolute momentum − equivalent potential temperature) cross sections to diagnose the presence of conditional symmetric instability (CSI), I note that the envelope of low (or negative) CSI depicted in Dunn's Fig. 11 encompasses a region from Rapid City, South Dakota to Denver, Colorado. Of course, the region of CSI may have horizontal extension out of the plane of the cross section, as well. While CSI may, indeed, be a viable candidate mechanism in this case, I dispute the implication that this analysis has succeeded in "pinpointing" the event. Further, if diagnosis of CSI turns out to be important for a significant fraction of heavy snow events, this has implications for the new profiling systems; viz., the new systems should be able to obtain thermodynamic data (temperature and moisture) with reasonably good vertical resolution in order to evaluate CSI.

New techniques and observing technologies are certainly going to be coming to operational meteorology.

However, their impact on forecasting mesoscale processes (which we only dimly understand at present) remains to be seen.

REFERENCES

Barnes, S. L. 1986. The limited-area fine-mesh model and quasi-geostrophic theory: A disturbing case study. *Wea. Forecasting*. **1**: 89–96.

Doswell, C. A. III. 1987. The distinction between large scale and mesoscale contribution to severe convection: A case study example. *Wea. Forecasting*. **2**: 3–16.

Dunn, L. B. 1988. Vertical motion evaluation of a Colorado snowstorm from a synoptician's perspective. *Wea. Forecasting*. **3**: 261–272.

Duran, D. R., and L. W. Snellman. 1988. Reply to comments by Lai. *Wea. Forecasting*. **3**: 348.

Holton, J. R. 1979. An Introduction to Dynamic Meteorology. New York: Academic Press.

Homan, J., and L. W. Uccellini. 1987. Winter forecast problems associated with light to moderate snow events in the Mid-Atlantic states on 14 and 22 February 1986. *Wea. Forecasting*. **2**: 206–228.

Hoskins, B. J., I. Draghici and H. C. Davies. 1978. A new look at the ω-equation. *Quart. J. Roy. Meteor. Soc.* **104**: 31–38.

Lai, C.-C. 1988. Comments on "The diagnosis of synoptic-scale vertical motion in an operational environment." *Wea. Forecasting*. **3**: 343–347.

Maddox, R. A., and C. A. Doswell III. 1982. An examination of jet stream configurations, 500 mb vorticity advection and low-level thermal advection patterns during periods of intense convection. *Mon. Wea. Rev.* **110**: 184–197.