Comments on ''Extraction of Geopotential Height and Temperature Structure from Profiler and Rawinsonde Winds''

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In a recent paper, Businger et al. (2001, hereafter B01) demonstrate the application of a technique employing the full divergence equation to derive heights and temperatures from wind information. Their stated goals are ''to demonstrate the utility of the divergence method in weather analysis and to promote future applications and improvements of the method.''

Development of methods using the wind fields alone to derive estimated thermodynamic information can be of considerable importance at a time when the value of rawinsonde observations has been a matter of some debate and concern (e.g., Bosart 1990). In fact, it is suggested in B01 that the cost of asynoptic rawinsonde ascents is ''prohibitive.'' The use of the term prohibitive to describe the costs of asynoptic rawinsonde data is arguably unfortunate. During impending hazardous weather events, it is common for National Weather Service forecasters to authorize the release of special soundings (at selected sites), to obtain thermodynamic variable information not directly observed by the profilers, or to have asynoptic upper-air data in regions not served by the existing profiler network. In spite of the numerous benefits that wind profilers offer, the existing *regional* network of operational wind profilers has never been expanded to national coverage, likely owing to economic decisions. Therefore, it seems that a *national* wind-only profiler network has been ''prohibitively'' expensive, as well.

Thermodynamic retrievals from the profiler observations are valuable beyond doubt, but especially for moisture variables, nothing as yet operational can replace what a few asynoptic special soundings can provide. In view of the limited coverage of the existing profiler network, for some parts of the country, asynoptic rawinsondes are still the only way to obtain crucial upper-air observations for operational forecasting. Although it is clearly not economically feasible to increase the frequency of rawinsondes at all the sites in the existing network, rawinsondes remain an integral part of a diverse upper-air sampling system and occasional use of asynoptic rawinsonde releases at a few sites remains a viable option for some time to come. Moreover, special soundings continue to be an important component of field campaigns in many research projects.

Moving on, I note that B01 considers only a single case study, so this work alone cannot be considered a conclusive test of their proposed technique for thermodynamic retrievals. Rather, their generally positive results are but a first step in what should be a more substantial test than can be offered from any one case study. Although I see some ways in which this work can be improved, to be developed in what follows, I do not consider my concerns to be critical to B01's primary conclusions. Rather, it seems to me that the authors have not pursued several options by which their study could have been made more convincing. In what follows, I have made three primary observations concerning the execution of this study.

First, the authors have chosen not to use the direct vertical velocity measurements derived from the profilers' vertically pointing beam. I believe that inclusion of the profiler measurements of vertical velocity has the potential to improve the diagnosis of divergence. There are straightforward methods to recognize the effects of rainfall contamination in the data (allowing contaminated data to be removed). In fact, in many cases of rainfall contamination, the erroneous vertical winds result in calculated horizontal winds that fail the routine quality control checks. Further, there are many techniques in objective analysis to filter the short-wavelength variation inherent in *any* set of measurements. Since objective analysis schemes generally have low-pass filter characteristics (see Stephens 1967), the scale

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of the analysis can be chosen by an analyst who is familiar with using the methods of objective analysis, thereby minimizing the negative impact of ''subgrid'' scale input in the data.

Thus, I believe there are good reasons to include the observed vertical motions in the analysis. Of course, including them substantially complicates the analysis; the vertical motions must be made consistent with the horizontal divergence via the imposition of the mass continuity equation as a constraint on the analysis. Some considerable additional effort would be required. However, I think it is quite possible that inclusion of observed vertical motions could improve the overall analysis. In so far as the quality of the divergence field analysis is an important component, this is a fruitful line of inquiry for future investigations aimed at optimizing this or other methods based on derivatives of the wind field. I will return to this theme when I make my third point.

Second, the rationale for choosing this particular case apparently is that the Storm-scale Operational and Research Meteorology-Fronts Experiment Systems Test (STORM-FEST) has provided something special by having 3-hourly soundings. These asynoptic soundings provide the opportunity for an interesting validation test, by comparing the results from (a) using all the data, which is what the authors did, and (b) using all the data except for the 3-hourly soundings. The special soundings constitute an independent measurement of the height and temperature structure at the times and locations of the 3-hourly soundings. Since the sounding *thermodynamic* data are not included in the input to the scheme proposed in B01, omitting these special soundings only removes their *wind* observations when the technique is applied. Not using the wind data as input from the few special soundings would almost certainly represent only a slight degradation of the final wind field analysis. This probably would produce an associated small degradation of the retrieved height and temperature fields. However, I believe it to be of considerable interest to compare the retrieved soundings at the sites and times of the special soundings with the actual observed soundings in B01's case.

I want to emphasize that the magnitude of the analysis degradation is *not* the main point of my concern. Rather, the undone comparison I have proposed represents a potentially revealing test of how well the standard data, including the standard rawinsondes and the profilers (but *not* the special soundings), would be able to reproduce the observed thermodynamic profiles *at those special sounding sites*, in the absence of those soundings. Omission of this test represents a missed opportunity to enhance the credibility of their proposed retrieval method.

Finally, the authors have chosen not to use a proven scheme to improve the quality of their divergence estimates based on the wind field. Beginning with Bellamy (1949), through Ceselski and Sapp (1975), Schaefer and Doswell (1979), Doswell and Caracena (1988), and most recently Spencer and Doswell (2001, hereafter SD01), it is becoming clear that the so-called traditional method of estimating derivatives, by first mapping the wind observations onto a regular grid and then using finite differences to estimate the derivative fields, is demonstrably inferior to line integral methods for estimating those derivatives. Recent results presented in SD01 show that the inherent superiority of line integral methods for derivative estimation is substantial even at ''well sampled'' wavelengths, thus correcting an assertion made in Doswell and Caracena (1988). The findings in SD01 demonstrate that the advantages of line integral divergence estimation, in cases where the spatial sampling is not uniform, are *not* limited to short wavelengths, at least when using a single-pass analysis. Moreover, in SD01, it is suggested that multipass objective analysis techniques can exacerbate a problem that *all* distance-dependent weighted averaging techniques have: gradients are unnaturally forced into the gaps *between* observation sites, thereby distorting the patterns of the derivative estimates. The impact of multiple passes on the resulting analysis is a topic outside the scope of SD01 and these comments, but ongoing work suggests that it is not as straightforward as it might seem to be.

I conclude that follow-on evaluations of the proposed B01 retrieval scheme could benefit from considering the issues I have raised. My hope is that in future validation studies of their technique, these suggestions will be helpful in establishing the value of this (or any other) retrieval scheme based on wind field derivative estimates.

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