NOTES AND CORRESPONDENCE

Reply

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In Riishøjgaard et al. (2004), we argued that a Doppler wind lidar system providing observations only along a single direction may be of limited use for substantially reducing errors of meteorological analyses and hence in numerical forecasts. Our reasoning was based on a set of experiments aimed at exploring the information content embedded in different types of hypothetical wind observations: in essence, winds projected on parallel lines of sight (LOS) on one hand, versus true vector wind information on the other hand. In a comment by Stoffelen et al. (2005) on our paper, the authors argue that our experiments do not do justice to a single-LOS system.

Their stated concerns are that 1) "the simple framework poorly represents the characteristics of a state-of-the-art global data assimilation system for numerical weather prediction" and 2) "the DWL scenarios that are discussed have abundant and unrealistic coverage and quality." We agree with the authors on both points. Our aim never was to replicate a state-of-the-art global data assimilation system, nor did we attempt to simulate the sampling pattern of any particular real or proposed observing system. Our paper was addressing a much more basic question, namely, that of the relative information content of wind measurements of a single component versus both components of the horizontal wind vector. The limited value of single-component observations found in our experiments appears to be consistent with the mathematical fact that scalar observations do not contain the full information about the flow, even in the limit in which every grid point is observed.

We understand that the spatial density of the observations varies in time and space. For this very reason, we tested our results over a wide range of observational densities. In addition, although it is true that the discrepancy between the single-perspective and dual-perspective observations is smaller for very sparse observations, it is still large enough to be of concern. The authors' analogy to the usefulness of sparse radiosonde observations is somewhat misleading. One could infer from our results that the radiosonde data are useful precisely because they provide independent information about both wind components. It would be relatively easy to run a test case with a real data assimilation system in which, say, only the zonal wind component would be retained from the radiosonde observations and to compare the impacts. We have not done such an experiment, however, and thus any link between the usefulness of single-perspective wind observations and isolated radiosonde measurements remains speculative.

Stoffelen et al. (2005) make comments in two specific areas. Their first comment is that the domain used in our experiments is of limited size (2000 km $\times$ 3000 km), the number of observations is too large to be realistic, and the assumed observation error is too small for our results to be of value. As already mentioned, our intent was not to assess quantitatively the expected impact on actual data assimilation systems of an actual observing system. It was meant to answer the following general question: To what extent can a perturbation in the atmospheric flow of which the forecast has no prior knowledge be captured by the analysis based on mea-

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surements of only one of the two components of the wind vector? The answer that came out of our experiments appears to be, "Only to a very limited extent." As pointed out in our paper, this result is consistent with what one would expect on purely mathematical grounds, and thus we see no reason to question that conclusion. In regard to the overall number of observations in the experiments, we did, in fact, run cases with varying numbers of observations—from very sparse to very dense. We did so not because we wanted to simulate realistic observation densities from a real-life Doppler wind lidar instrument, but rather to test the validity of our conclusion in the limits of observational coverage. With only fairly minor variations, it holds over the entire range of observational densities that we tested.

Their second comment is that if the background error covariance matrix **B** were correctly specified, then the single-LOS observations would do a better job. There is arguably some truth to this, but our point is that in a realistic situation **B** is almost always locally wrong. The key assumptions built into the **B** matrix that affect the way the wind observations are being captured by the analysis require a priori specification of the partitioning between balanced and unbalanced components of the flow. In the best case, these assumptions accurately represent temporally and spatially averaged properties of the analysis/forecast system. Because the actual partitioning of the atmospheric flow into balanced versus unbalanced components is local in space and time, the **B** matrix is almost by definition misspecified everywhere, especially whenever and wherever "new weather" occurs. It is therefore of interest to test also how well the system performs in the case of a realistic but imperfect **B** matrix. In most of our experiments, the **B** matrix was, in fact, very favorable to the single-LOS observations because it (correctly) specified a nondivergent flow. Stoffelen et al. (2005) are correct in pointing out that the length scale for the correlations is underestimated. However, this length scale is a static quantity in all current data assimilation systems, and yet we still expect those systems to be able to correctly analyze atmospheric waves of different wavelengths. We did not do an experiment in which the scale of the wave to be detected matched up perfectly with the assumed error correlation length, and we do not think that such an experiment would be an honest test of the information content of single-component wind observations.

One might even argue that if **B** could indeed be specified perfectly we would not need any wind observations at all. The entire flow and mass field could then be analyzed perfectly based on temperature observations alone.

Last, Stoffelen et al. (2005) comment that our "study on its own does not provide the required detailed information on which to base a mission design." We believe this point is self-evident, and we have never claimed that our results should be used in stand-alone mode as a basis for mission design. Nor would we ever claim that a single-perspective system is without value for numerical weather prediction. What our results do suggest, however, is that there is a nonlinear relationship between the benefits of having one versus two perspectives. To be more precise, our results show that the information content provided by a dual-perspective system may be vastly superior to that of a single-perspective system for an equal total number of measurements, and we believe that this conclusion is a matter that should be carefully considered in the decision process for future missions. We understand that, despite these results, there may be technical as well as financial reasons for building a single-perspective system rather than a more complicated and expensive scanning or a multiangle system, and we recognize the value of the single-perspective system in terms of its ability to test the overall measurement principle.

REFERENCES

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