Reply

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ABSTRACT

This is a reply to a set of criticisms regarding a previously published work. It briefly addresses the main criticisms. In particular, it explains why some papers identified as having some fundamental flaws were referenced in the original work without detailed exposition of those flaws. It also explains why parts of the conclusions criticized as being contradictory are, in fact, not. It further highlights the need for more publishing of scientific criticisms.

1. Introduction

Vukicevic (2008) has presented a criticism of several sets of statements in Errico et al. (2007, hereafter EBM). These include complaints 1) that EBM identified some referenced papers as being fundamentally flawed without adequate explanation, 2) that, although EBM was a review, it omitted some relevant references, and 3) that it presented contradictory conclusions. We (the authors of EBM) disagree with all these characterizations, for reasons explained in the sections below.

2. Inadequate review of past research

Contrary to assertions in Vukicevic (2008), EBM is not a review of the literature on the assimilation of observations related to clouds and precipitation. In fact, the word "review" does not appear anywhere in the paper until the final section. Rather, as explained in the introduction, it is a presentation of several related but diverse issues that are germane to the problems of assimilating observations particularly related to clouds and precipitation. Even so, 100 papers are cited in EBM.

As with any scientific paper, even if it is not intended as a review, it is appropriate to mention relevant works that predate the present one. In particular, earlier works that guided the present work should be cited for a succinct presentation. In writing EBM, however, we were faced with a great dilemma: There were several peer-reviewed papers that we determined to be fundamentally flawed (i.e., they displayed errors that violated well-established theory or empirical evidence and that, once recognized, removed the foundations supporting the papers' conclusions). Our judgment was thus based on the credibility of those works regarding their physical or mathematical meaningfulness and the reliability of purported knowledge they revealed, which is the criteria required by Vukicevic (2008). We had the choice of either ignoring those works entirely (i.e., simply not citing them) or listing them but with a caveat that those

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papers should be read with utmost care. We chose the latter course. Our dilemma was entirely a consequence of the unfortunate fact that few researchers write critical exposes of fundamentally flawed papers [e.g., see the figure showing the declining rate of published comments in American Meteorological Society (AMS) journals appearing in Errico (2000)].

Several fundamentally flawed papers were identified in EBM and some of those papers had multiple flaws. Since all those flaws passed inspections by reviewers, we cannot assume that they are well understood by all readers of such papers, no matter how apparent they may be to some. We therefore cannot describe them all in this reply that is restricted in both page length and response time. Instead, we therefore expand on only two general flaws that appear in several papers and that are particularly mentioned by Vukicevic (2008). These are the omission of background information and the neglect of spatial correlations of background errors when such information is used.

Background information is generally important because, in any truly adequate data assimilation system (i.e., one intended to produce a real analysis rather than only demonstrate feasibility), it is the best information about the atmospheric state. It is generally more accurate than most observations alone and generally more complete. It is also required to "close" the system of equations defining the "optimal" analysis. In fact, nowhere in Tarantola (1987) is the data assimilation problem discussed in the absence of explicit prior information. Without such closure, the data assimilation problem is usually ill-posed mathematically, since the number of independent observations provided is typically less than the number of state variables to be determined. For the ill-posed problem, rather than the cost function having a vanishing gradient at a discrete set of values of the state vector, or at a single value if the observation operators are linear, it has a continuum of such values. Some published papers ignore these facts about the utility of background fields. Since a continuum of solutions is possible, the particular solution (i.e., analysis) obtained necessarily depends on some unnamed conditions, most likely including the starting point for the iterative minimization scheme employed. Yet, where backgrounds are neglected, those other conditions are not stated. Implications of neglecting backgrounds are sometimes not discussed at all or perhaps only briefly mentioned in passing, without elucidation of any expected consequences. What does appear in places, however, are statements about how fast "convergence" occurs, but with no mention of how such speed is obtained by effectively having a very wide range of solutions (e.g., an infinite set) for the algorithm to find.

Tarantola (1987) mentions that if too little is known about background error correlations (that for the atmospheric problem are spatial as well as dynamical) then it may be appropriate to ignore them. Although these correlations are not yet totally known, especially on the mesoscale, enough is known about them for the atmospheric assimilation problem that that they should not be neglected without ample cause. Furthermore, the history of data assimilation abounds with examples of the dire consequences of ignoring them, especially their approximate geostrophy. That is why at operational centers, the refinement of these correlations is a never-ending task. Yet, the results of experiments conducted without consideration of such error correlations are generally presented without discussion of their otherwise important nature or of the consequences of their neglect. In particular no results are presented and discussed regarding the consequent dynamical imbalances of the analysis produced. Instead the reader is often left with the distinct impression that the great additional effort required to specify and incorporate background error correlations is essentially superfluous.

We agree with the assertion in Vukicevic (2008) that it is possible to learn something from experiments designed using poor assumptions or approximations and nevertheless make scientific progress. Under such design conditions, however, the problem is to interpret the results properly. So for example, applying a four-dimensional variational data assimilation technique to a Lorenz (1963) model using long assimilation windows, a researcher may properly suggest that the presence of chaos can yield multiple local minima of the cost function, thereby requiring global rather than local solvers and clouding claims of any particular solution as "best." Inappropriate, however, would be claims that now the researcher has a useful algorithm for real atmospheric data assimilation. Yet, that has effectively been the claim to the community in some cases where data assimilation systems with either no background or no (implicit or explicit) background error correlations are considered.

Regarding the usefulness of data assimilation research outside operational centers, the problem is not that work performed in nonoperational settings is necessarily inadequate and that only investigators at operational centers should be performing data assimilation studies. In fact, EBM explicitly state the contrary in their summary. Also stated, however, is the necessity of any investigator to understand the data assimilation problem sufficiently well (e.g., the roles of the background and its error correlations) so that any experi-

ments performed have implications for the real problem, as generally claimed. It is just such claims that almost always motivate the work's funding.

3. Incomplete review of past research

Although EBM was not published until December 2007, it was prepared during December 2005. It was "in press" for 18 months. Thus, we do not reference any papers appearing after 2005, except for those known to us earlier in manuscript form.

The interest of using explicit microphysical cloud schemes to model radar reflectivity has been explicitly mentioned in section 4. We have made reference to the recent (at submission time) paper of Laroche et al. (2005) because it raises a number of issues regarding the suitability of various microphysical schemes for data assimilation. Several of those issues are ignored in some of the papers listed in Vukicevic (2008). We assume that there is a confusion of the Greenwald et al. (2002) reference because as far as we can tell it describes a radiative transfer model that is a forward operator and it also discusses forward operator errors to a certain degree. The reference to the latter was our intention in EBM.

4. Discrepancies between recommendations

Vukicevic (2008) states that some of the recommendations in EBM are contradictory. In particular, it is claimed that our calls to both better utilize tangent linear and adjoint models and more thoroughly consider possible effects of nonlinearity contradict each other. We acknowledge that these two statements, given in different sections, could appear confusing to the reader, and we thank Dr. Vukivecic for helping to clarify them. Indeed, these recommendations are not contradictory. It has been shown in carefully designed experiments (reported in some of the literature cited in EBM) that tangent linear and adjoint models, under some relevant circumstances, reveal behaviors that occur in the full nonlinear context that were not, however, apparent prior to application of those linearized tools. Of course, a requirement for obtaining such results is the development of a modeling system for which its linearized approximation has some degree of proven utility.

Even with evidence of the utility of linearized models, the behaviors of precipitation and cloud development remain fundamentally nonlinear. This is particularly true because the current uncertainty of the atmospheric state is far from infinitesimal. Thus, results based on linearizations are incapable of providing all the information we require. Also, the information they provide will be unreliable under some synoptic conditions, thereby requiring more robust, nonlinear considerations in those cases. In fact, some of the carefully conducted experiments cited in EBM reveal such problems. That does not mean linearizations will not remain useful, but rather that they will always yield incomplete information and sometimes even incorrect information. Both avenues of development and consideration therefore should be pursued.

5. Final comments

We hope this reply provides some clarification of the points criticized by Vukicevic (2008). We also hope that it spurs more researchers to publish criticisms of works that they believe to be fundamentally flawed. Without such criticisms, the whole pace of scientific progress is slowed.

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