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Interactive comment on "Wind extraction potential from 4D-Var assimilation of O_3, N_2O, and H_2O using a global shallow water model" *by* D. R. Allen et al.

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This is a very interesting paper that investigates the theoretical limits to wind extraction from tracers in the stratosphere. The "perfect model / perfect observation" approach is informative in this context. With the perfect approach it is possible to almost completely reconstruct the wind fields. After adding observation error, the results are not quite so good but they are still good enough to encourage further work on tracer assimilation. However, I think there are fundamental problems in the way background error has been treated in this study. The experiments should be re-run to correct this. It is unlikely the conclusions will be fundamentally different, but it is necessary to check, and for the credibility of the results, the data assimilation setup needs to be as correct as possible.

Major issues

1) The background error has been set up as follows (section 2.2): "Various values of the horizontal correlation lengths and background error standard deviations were tested to maximize tracer-wind extraction". This is the crux of the whole paper and the authors need to provide more detail. I imagine the authors would have been trying to maximise the wind extraction potential (WEP) in the first cycle of experiment 1. In this approach, the background errors would have been appropriate for exactly the combination of initial conditions, observation errors, and observations found in the first cycle of experiment 1. However, they are unlikely to have been appropriate for the system as it evolved during cycling, or for the other experiments where observation errors were larger. After 10 days of cycling, the errors in the forecast fields in experiments 1 and 2 are radically different: Figs. 7 and 9 show wind errors of around 2m and 20m respectively. The background errors used in the data assimilation should reflect this. If not, the data assimilation system is suboptimal and the WEPs calculated after 10 days are incorrect. Alternatively, if the background error tuning has been done to maximise the 10-day WEPs, the single-cycle WEPs are incorrect.

The best way to address this issue is to tune the background errors for each experiment, and to tune them differently for the single cycle results and for the cycled results.

2) It is asserted that (e.g. the abstract) "assimilation of very noisy observations may worsen the wind fields". In an optimal data assimilation system, this should not be possible. In other words, if the basic assumptions of 4D-Var are valid, particularly that observation and background error are correctly modelled, this cannot happen. Hence, it hints at a sub-optimal data assimilation system. The text seems to imply a misconception in the results of experiments 3 and 4 are presented. Here, large observation errors are applied and there is (end of section 4) "a significant worsening from the initial conditions when assimilating noisy data". The observations are not

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actually worsening the analysis.

The two endpoints on the scale of data assimilation quality are (A) when we have sufficient observations to analyse the real world exactly and (B) when we have no observations, and no matter how good the initial conditions, the model will drift away from reality and lose all skill after 10+ days. As you slowly increase the availability and quality of observations, there will come a point somewhere in the middle where the observations are just good enough to stop the model drifting away from reality completely. Even experiment 2 (realistic obs errors) appears to be below this point, since RMS height errors steadily increase through the 10 day period (Fig. 8) - indeed this is acknowledged in the last sentence of the conclusion. Hence, the observations in experiments 3 and 4 (very high obs errors) are totally insufficient to stop the model drifting away from reality.

The crucial point comes back to the background errors: this is a suboptimal system and there might be much greater wind extraction in the high-obs-error case if the background error variances were much larger. These kind of issues have been coming up in the preparations for early-20c reanlyses (e.g. Whitaker, 2009) where it has been shown that 4D-Var can reconstruct the global weather from very sparse observations from the early 1900s, but only if the background errors are sufficiently large. Trying to use background errors appropriate to the present day observing system would extract nearly no information from the sparse observations.

To sum up major issues 1 and 2, the authors need to really carefully think about the way the background errors are constructed in their experiment. Should they be constructed so as to be appropriate to a current NWP system with full observations? Probably not, given this is a "perfect" setup and the paper attempts to see if tracer observations alone can constrain the wind fields. But certainly they need to be separately optimised to maximise wind extraction in all the different cases (e.g. single-cycle, 10-day cycling, and the varying range of observation errors in experiments 1-4).

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Minor issues

1) The paper is focused on stratospheric wind extraction but it needs to be put in context of tropospheric water vapour assimilation, which has been improving wind fields through the tracer-effect in operational systems for at least the last 20 years (e.g. Andersson et al., 1994 , Peubey and McNally, 2009). Perhaps even the title of the paper could be more precisely targeted as: "Wind extraction potential from the assimilation of stratospheric 03 ..."

2) Section 2.2 on the data assimilation system is lacking a few details. See the major points particularly, but also:

a) It would be great to have a few sentences on the "accelerated representer approach" without the reader having to consult the references - i.e., how does it vary from other 4D-Var algorithms?

b) Please discuss the approach of constructing a perturbation model for the TL yet using a line-by-line approach for the adjoint. Why not construct both the TL and the AD with the line-by-line approach? At least then the code can be checked using a standard adjoint test.

c) The BECM (page 25298) could be explained in a little more detail, addressing key questions such as "is it represented explicitly (e.g. full matrix form)?" It would be really great to see the matrix expanded in the text to show the sub-blocks (e.g. wind-wind, wind-height). That would be really helpful in summarising which correlations have been modelled and which have been ignored.

d) The paper needs to provide some basic diagnostics to help the reader see if the assimilation system is optimal or not, for example Chi squared or Jo/n

3) In the discussion (Sec. 5) reference is made to the fact that the ozone-wind adjoint has been cut at ECMWF. It could be mentioned that this was because of biases between the modelled and observed ozone fields in the stratosphere. The only way

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the model could adjust to the observed ozone fields was to make major, erroneous changes to winds and temperatures in the upper stratosphere. Hence, biases are one of the major practical obstacles to overcome before we can extract winds from tracers in the stratosphere.

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