

Interactive comment on “Wind extraction potential from ensemble Kalman filter assimilation of stratospheric ozone using a global shallow water model” by D. R. Allen et al.

A.J. Geer (Referee)

alan.geer@ecmwf.int

Received and published: 19 March 2015

This is a useful study looking into the assimilation of tracer and geopotential observations. As well as the exploration of localisation, initialisation and error inflation aspects of the ensemble Kalman filter, the interaction between tracer and geopotential observations is very interesting. There are only a few minor suggestions for revision:

1) P3958, L22 “..the background error covariance does not include tracer-wind correlations.... This limitation can be overcome by using an ensemble Kalman filter...”. Note that limitations of 4D-Var can also be addressed using hybrid methods and this

C884

is increasingly popular at operational centres (e.g. 4D-Var background errors can be generated using an ensemble method). More generally, though this should definitely not become a “4D-Var versus EnKF” paper, it would be nice if the authors could briefly compare back to their experiences when they performed a very similar study using 4D-Var (perhaps a short discussion or a few sentences in the conclusion?).

2) P3961, L10 Why is the observation error covariance matrix referred to as “R_ens”? This implies, to me at least, there might be something special about how it is created. Would not “R” be sufficient?

3) P3962 L4, “To avoid filter divergence we apply an inflation factor to maintain reasonable spread in the ensemble”. The text needs to say both what the inflation factor is applied to and how it is done.

4) Equations 8 and 9 seem slightly confusing to me. Why is only the latitude dependence made explicit? How was a latitude/longitude/time varying error reduced to a latitude/time varying RMSE? I think I can guess, but please add a bit more explanation. Note also that later, in equation 11 and associated discussions, you are giving a lot more detail about what are probably very similar calculations, but this time explicitly showing how the latitude-dependent RMSE has been computed.

5) P3972, L16-23. Discussion around the chi-squared metric seems to start with a non-normalised version (which is expected to be equal to the number of observations) and moves to a normalised version (expected to equal 1) without explaining properly how the second is obtained, and without changing terminology to distinguish the two. So it comes as a shock in the last sentence when a “chi-squared” of around 1 is considered good.

6) p3976, l22: “It appears that combining height observations and ozone observations acts as a filter to dampen the GW that would otherwise be generated by the ozone observations alone”. I am not sure the “filter” idea is necessary, and it would be good to explain what it means physically. Instead, is it possible that you simply require both

C885

height and ozone observations to properly constrain a shallow water model? Height observations just constrain height and the balanced part of the wind, but leave the unbalanced part of the wind unconstrained. That could allow GWs to develop. But the ozone observations could help constrain the unbalanced part of the wind field, stopping spurious GWs developing.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 3955, 2015.