Two reviews of the revised version of your paper have now been submitted. The reviewers are reviewers 1 and 2 of the first version, with the same numbers (in particular, reviewer 1, who has again let his name known, is P. G. Penny).

Reviewer 2 considers the paper can be accepted as is, but nevertheless makes suggestions for minor modifications.

P. G. Penny considers the paper can be accepted subject to minor revisions, and makes a number of suggestions, some of which may require some work from you. I would agree with at least two of Penny's suggestions. That you rescale the estimation errors, whose absolute values cannot be easily compared for the components *X* and *Y*. And that you discuss to some extent the reasons for the occasional failure of the assimilation in the case of observation of both components (after all, in a properly implemented assimilation process, introducing additional observations should, at the worst, not degrade the quality of the output).

And I have as Editor additional comments (some of which I could have made before, but that escaped my attention).

1. Eq. (1) (and elsewhere). The matrix $\mathbf{L}$ and $\mathbf{P}^b$ being symmetric, you might mention that the submatrices $\mathbf{L}_{\mathbf{XY}}$ and $\mathbf{L}_{\mathbf{YX}}$ on the one hand, and $\mathbf{P}^b{}_{\mathbf{XY}}$ and $\mathbf{P}^b{}_{\mathbf{YX}}$ on the other, are necessarily transpose of each other.

2. Table 3 shows RMS errors in the assimilated fields. Is that necessary (the corresponding results are shown on Fig. 3, and the same information is not included in Tables B1 and B2) ?

3. Over how long periods of assimilation (or at which assimilation time) have the results shown in Figures 3 and 4 been obtained (the EnKF being a purely sequential algorithm, which may take some time to reach a stationary regime, that may be of some importance) ?

4. L. 275. The significance of the parameter σ_λ^2 is not clear.

5. The final paragraph, which you have apparently introduced in response to one of my previous comments, is actually inappropriate

Although we tested the localization functions in an EnKF our results should translate to EnVar schemes as well, because 3D-Var and the analysis step of the Kalman Filter are equivalent in the case of a linear observation operator (Daley, 1993). The positive semidefiniteness of the localization matrix is essential to ensure convergence of the numerical optimization methods used to implement EnVar (Bannister, 2008). The localization functions we have presented may be used in variational schemes without the need to numerical verify that the localization matrix is positive semidefinite each time a new set of localization radii is tested.

Firstly, variational schemes (at least in their present form) do not build covariance matrices from discrete ensembles, and therefore do not need localization. Secondly the equivalence between variational and Kalman Filter algorithms requires linearity, not only of the observation operators, but also of the dynamical model. It therefore does not apply in the present case of the Lorenz model. Although that paragraph was introduced following one of my comments, I think it would be preferable that you remove it.

Please revise your paper taking into account all comments and suggestions of S. G. Penny, as well as my own. Give a precise answer to all of these comments and suggestions. In particular, should you disagree with a particular comment, or decide not to follow a particular suggestion, please state clearly your reasons for that.

I thank you for having considered *Nonlinear Processes in Geophysics*, and look forward to receiving a revised version of your paper.