I must first apologize once again to the authors, but this time for having written something wrong in my first review.

I wrote, concerning Fig.9, *the vorticity is a linear function of the wind components, and cannot be as such be less Gaussian than those components*. Well, that would be true if the wind components were Gaussian when considered as global random functions over a spatial domain. Gaussianity of local values does not imply global gaussianity, just as individual gaussianity of two random variables x_1 and x_2 does not imply gaussianity of the couple (x_1, x_2) (there might be dependence between x_1 and x_2 which forbids gaussianity of the couple). More precisely, the wind components must be highly dependent at short spatial range, and the dependence may be such as to forbid gaussianity of the spatial derivatives (of course, that dependence, if it exists, cannot be seen on the standard second order correlation, but may be visible, as seems to be the vase, on skewness and kurtosis).

This is actually totally consistent with the additional results shown by the authors in their response to Referee 1. They show that, if the temperature is close to Gaussian, its spatial derivative is clearly not.

This incidentally can occur with variables that are exactly Gaussian locally, and has nothing to do with a slight deviation from Gaussianity being amplified by differentiation.

I apologize to the authors for the additional concern and work I have given to them. At the same time, I think my erroneous remark will in the end be useful. The authors show that the wind components (and the temperature) are not globally Gaussian. And it is global gaussianity that matters for the standard theory of assimilation. I think that should be stressed in the paper.

And it also shows that checking local Gaussianity is not sufficient in the context of assimilation. Of course, checking global Gaussianity would be extremely costly (if possible at all with the available amount of data).

I have a few other comments, some of which I could have included in my first review, but which escaped my attention, or that I considered then of secondary importance.

1. P. 4, l. 7, *It solves for the most probable state in Eq. (1)* …. Actually, in present NWP algorithms, the minimization is only approximate, and the minimum in the 'Gaussian' cost-function used as an expression for Eq. (1) will not in general be the most probable state in the non-Gaussian state. I would suggest *It approximately solves for what is, in the Gaussian case, the most probable state in Eq. (1)*.

2. P. 12, ll. 8-10. … *the area diagnosed with the highest values of variance, located South of the Balearic islands, is associated with low values of K^2*. Well, that is not what is visible on Fig. 4a (see also Fig. 4c).

3. Figure 6c. It is not clear what the figure represents. It shows two humidity profiles, which I understand must be relative to a specific time. But no time specified, and the text reads (p. 13, l. 13) *According to Fig. 6c that displays the time evolution of the mean cloud contents, …*

And, concerning the other panels of fig. 6, I presume the full black curves, labelled 'analysis' are relative to the analysis at initial time 00:00. Say it explicitly.

4. P. 8, l. 17. I would suggest … *for the Gaussian hypothesis H_0*.

5. P. 11, ll. 1-2, Say in the text that Fig. 3 is relative to a 3-hr forecast.

6. P. 15, ll. 23 and 24, *derivation* → *differentiation*

P. 16, l. 2, *Cevenol event*. All right, but it would be preferable to let the reader know sooner (subsection 3.1) that you are studying a ‘Cevenol event’.