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Interactive comment on "Assessment of an ensemble system that assimilates Jason-1/Envisat altimeter data in a probabilistic model of the North Atlantic ocean circulation" *by* G. Candille et al.

Anonymous Referee #2

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This paper presents interesting diagnostics for ensemble data assimilation methods, known in the weather forecasting community but new to the oceanographic community. The main weakness of the paper is the contradiction between the objectives announced and the topics actually discussed in the paper. The authors announce very ambitious objectives: "better control of the eddy dynamics observed in the Gulf Stream region", which are not verified against actual observations of eddies (centres and size of eddies), nor compared to the state of the art in controlling eddies. One idea from an earlier paper (Brankart 2013) is taken further in the present paper but remains a marginal issue in the discussions: The link between the perturbations of the equation of state and the eddy dynamics is not discussed (no mention of the potential vorticity

balance or any mechanism impacting the eddy dynamics). Such a discussion would in effect have no room in this paper. The real objectives as they appeared to me are to introduce novel assimilation metrics to the community of advanced ensemble data assimilation in ocean modelling systems, which I consider a sufficiently valid objective for a paper. I would therefore recommend the authors revise their objectives at the lights of the work actually provided and streamline the whole paper accordingly.

A second weakness of the paper is that the practical utility of the metrics proposed is not fully developed. Ensemble spread, ensemble bias, and rank histograms are classical tools used in the community to fine-tune an assimilation system, indicating whether the ensemble is over/underdispersed or biased. The authors introduce more advanced tools, but do not demonstrate that these provide more powerful diagnostics than the basic tools, nor how they can feedback on the choice of model errors (their variance, their spatial scales, temporal scales). I am therefore not tempted at all to implement them. The authors would increase the impact of the paper by illustrating how the diagnostics allow them to select better values for the semi-arbitrary input parameters.

The explanations are often vague (several examples below) and would deserve a little more precision. In particular, the distinction between the SEEK filter and other methods such as the LETKF and any square root version of the EnKF seems unnecessarily confusing, see further comments below.

Detailed comments:

- Abstract: The IAU is a technical stability fix that can be used with any sequential data assimilation method, but does not erect as a data assimilation method by itself. The authors should mention in the abstract that the SEEK filter is used with a stochastic ensemble (or the EnKF, see further comments). This is a more important precision than IAU for positioning the paper.

- p.2650, l. 1. I do not see the essential difference between the stochastic parameterization in Buizza et al. 1999 or Palmer et al. 2005 and the stochastic model errors used in Evensen 1994 or Brusdal et al. 2003 (note that one of the authors was also a co-author of the latter). So the historical perspective seems biased.

- p.2650 l. 15. There has been many variants of the SEEK filter in previous literature (fixed-basis, evolutive, semi-evolutive, SEIK), so that the authors have to clear away any possible confusion about which is used here. At this stage, the reader still does not know if a fixed basis or an evolutive basis is used, which is an important information to set the scene.

- p. 2650 l.12 The objectives are too vague. Better than what?

- p. 2652 l. 3-5. Please give references or the observations used, not only the providers.

- p.2652, l.7. The passage is confusing, is the mean SSH is the 7-years average of the ensemble average? Are the SLA the anomalies referred to the 7-years mean SSH?

- p. 2652, l. 14. The idea that the equation of state depends on scale is new to me. Do you mean that the eos is so dramatically non-linear that the cell averages obey a different equation than the molecular values? If yes, please indicate ranges of density errors at the model resolution.

- p. 2653, l.1. Eq. (2) indicate scalars equal to vectors of size 3 (Gradients). In which direction?

- p. 2653, l.3. Why use 10 days time scales, 1.4 and 0.7 grid points? Please justify the choice of these values.

- p. 2653, l.8. Could you have avoided instabilities by making ksi horizontally correlated instead of using ad hoc limiters?

- Figure 1: the bottom right plot shows worrying gravity waves. Are they produced by the model perturbations?

- p. 2654, l. 24: Does the SD really "explain" the RMSE?

- p. 2655, l. 1: "approximately equal" is vague. Could you indicate numbers?

- p. 2655, l.7: The lifting or lowering of the water column is hard to see on Figure 3. Could you choose another way of presenting this, like plotting the ensemble members in T/S diagrams?

- p. 2655, l. 14: "directly benefit from an ensemble point of view". Vague sentence, do all members share the same point of view?

- p. 2659, l. 11: Errors from altimeters Jason-1 and Envisat are expected in the range between 3cm and 4cm. Can you explain how the value of 10 cm is obtained?

- p. 2659, l. 17: "observations errors can be introduced". Have they actually been introduced in this paper? Is there any reason not to include them?

- p. 2659: Why not considering the classical quantile-quantile plot (q-q plot)?

- p. 2660, l. 7: The CPRS is a central element of this paper and it would deserve one simple example.

- Eq. (5) CPRS= Reli + Resol is not a useful equation, and "the same principle as the rank histogram construction" does not help much either. Can you repeat how the split is performed by Hersbach?

- p. 2660, l.20: What is Unc, then? a variance or a standard deviation?

- p. 2661, l.2: Related to the former. Unc=0.070, which unit? cm or cmˆ2?

- p. 2661, l.18: errors of 0.9K and 0.17 psu seem rather large, at least compared to instrumental errors. If they include mapping errors the grid scale or temporal averaging should be indicated too.

- p. 2662, l.5: The warning is not very constructive. What would the authors suggest as a good compromise for selecting the verification dataset?

- p. 2663, l.20: How do you define the method as a SEEK filter rather than an LETKF or an EnKF now that all these methods use the same propagation step?

- p. 2664, l.10: The update cannot be computed in the eigenspace of matrix Gamma because the latter is in observation space, not in state space.

- p. 2664, Eq. (8): At which level is the spectrum truncated to avoid zero eigenvalues?

- p. 2664, Eq. (8-10): The update is the same as the local ETKF (Tippett et al. MWR 2004) and the forward step is based on an ensemble propagation with random model errors, as in Evensen (1994), so what is left from the original SEEK filter from Pham et al. (1998). If the methods have become identical, it would be confusing that they should still be labeled under different acronyms.

- p. 2665, l.6: Erroneous assertion. The stochastic ensemble propagation does not exempt from using an inflation. The analysis scheme presented by the authors takes the ensemble covariances for the true covariance and therefore misses the part of uncertainty related to the random sampling of the covariance, which the inflation is meant to compensate. This important point is well explained by Bocquet (NPG, 2011).

- p. 2665, l.10: Is the localisation applied on a local state vector or by a Shur product on the covariance matrix? (Sakov and Bertino, CG 2011, Nerger et al. 2011)

- p. 2665, l. 15 and 24. Neither the saturation of the spread nor the slight bias are visible on Figure 6. The spaghetti plots could be replaced by mean + shades for the standard deviation or any presentation that would make the lines more transparent.

- p. 2665, l.21: sentence too long, please rephrase.

- p. 2666, l.2: Strange justification, I do not see the logic in this sentence. Observations of currents are also available from moorings in the Gulf Stream and surface drifters but are not considered here.

- p. 2668, l.20. Unclear sentence. Which information is reliable, and what is the uncertainty "on the 10 days forecast"?

- p.2668, l. 26. This assertion is again impossible to verify in the spaghetti plots. Please modify Figure 11 to make the point obvious.

- p. 2670, l.2: The objectives stated in the conclusion are different from those in the introduction and not more in line with the paper either: objective 1 is too vague to be verified and objective 2 has apparently already been attained in previous papers.

- p. 2670, l.8: Fine that the effects of unresolved scales in the eos are represented, but do they constitute the only possible model errors? Should not we therefore expect the system to be underdispersive? The discussion of model errors should either be expanded to compare with other approaches in the literature or dropped altogether.

Figures and tables:

- p. 2654, l.17: Longitudes are in degrees W in the text and degrees East in Figure 1. Modify either the text or the figures.

- Figure 3: Profiles of velocities are never commented in the text, what are they here for?

- Time series have "lead time" as abscissa instead of "time", indicating that all the forecasts are initialised in April 2005.

- Table 3 has no top labels, just percentages. Indicate months names.

- Figure 8 has no units.

Typos:

- p. 2653, l. 28: strcuture

- Fig. and Figure are used alternatively.

- p. 2668, l.1: "dispersion degraded down to an overdispersive system". Did you mean "underdispersive"?

Interactive comment on Ocean Sci. Discuss., 11, 2647, 2014.