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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.

G. Candille et al.

gcandi@gmail.com

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We thank Referee #2 for her/his careful reading and for her/his constructive comments which help to clarify the objectives of the paper.

RC: This paper presents interesting diagnostics for ensemble data assimilation methods, known in the weather forecasting community but new to the oceanographic commu- nity. The main weakness of the paper is the contradiction between the objectives an- nounced 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.

AC: We agree with Referee #2, the main objective of this paper is not "to better control the eddy dynamics" in the Gulf Stream, but to provide a tool that can qualify and quantify the uncertainty associated with the description of the eddy dynamics in the Gulf Stream. The abstract sentence (p2648 l7-8) has been modified as follows:

"The assimilation experiment is designed to provide a description of the uncertainty associated with the Gulf Stream circulation ..."

The introduction sentence (p2650 l11) has been modified as follows:

"The main goal of this experiment is to provide a characterization of the uncertainty associated with the eddy-dynamics ..."

Nevertheless, the introduction of new metrics here is not the real point, but only the way to consider the ensemble system as a full probabilistic description of the ocean circulation.

RC: 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 classi- cal tools used in the community to fine-tune an assimilation system, indicating whether the ensemble is over/underdispersed or biased. The authors introduce

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more advanced tools, but do not demonstrate that these provide more powerful diagnostics than the ba- sic 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.

AC: The probabilistic tools introduced here (mainly the CRPS) are not "more powerful" than the "classical tool" (ensemble spread, rank histograms). All these tools are necessary to describe and measure the two main attributes of a probabilistic system: the reliability and the resolution. We need both. In a first step, a probabilistic system must be reliable, but it must also provide useful information (resolution). For instance, the choice of the "semi-arbitrary input parameters" is here all about the reliability of the system what referee #2 calls "fine-tune" process. Improving the resolution mainly deals with the reduction of the model error itself. If we only try to improve the resolution by tuning the ensemble spread, we generally degrade the reliability. We can observe this situation with the T/S-profiles in the paper: the potential gain is better than for the SSH, but the reliability is strongly degraded.

Other changes in the paper according to these two comments:

p2651 l23: "control" replaced by "description".

p2653 l27: "allow an effective control of the mesoscale circulation by " replaced by "well describe the mesoscale circulation with".

p2662 l15: "control" replaced by "well describe".

RC: 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

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IAU for positioning the paper.

AC: The sentence p2648 l11-13 has been modified and IAU mention is removed:

"Then each ensemble member is updated by a square root algorithm based on the SEEK filter (\citet{brasseur_06})."

RC: 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.

AC: Right, we forgot here to mention these precursor works since our study is influenced by these previous ones, especially Brusdal et al. 2003. This reference is added.

RC: 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.

AC: The SEEK form here refers to the mentioned studies (deterministic approach). This is not the SEEK developed in the present full-probabilistic study.

"the SEEK" has been replaced by "a previous SEEK".

RC: The objectives are too vague. Better than what?

AC: New sentence

"The main goal of this experiment is to provide a characterization of the uncertainty associated with the eddy-dynamics ..."

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

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AC: References added: Inglenby and Huddleston 2007, and

"(produced by Ssalto/Duacs and distributed by AVISO, with support from CNES)"

(this is the official way to refer to AVISO data as written on their own website).

RC: 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?

AC: No ensemble here. The mean sea level (MSL) -for the model part- is computed over 7 years with the stochastic version of the model (only one integration). "SLA" for the model refers to that mean sea level. SLA observation refers to its own MSL (computed by AVISO). The sentence has been modified to clarify this point:

"... NEMO_OBS removes the 7-year mean sea surface —averaged from 2002 to 2008— computed using one integration of the stochastic version of the model simulation (see next subsection \ref{sec:ens_description}). In the following, SSH denotes both SLA observations and SSH model outputs which are both anomalies computed with their own MSL."

RC: 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.

AC: Right. This effect and its importance is described in Brankart 2013.

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

AC: It is a scalar product.

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

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AC: It is here a delicate point. We just chose these values in order to get stable numerical scheme, and fast perturbation growing with adequate dispersion (as mention in the text).

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

AC: Yes, this is an interesting possibility that we have tested without much success in this study. These limiting factors are the simplest (and best) solutions we found to avoid numerical instabilities.

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

AC: Right, we purposely chose to show this member. A sentence has been added to point this:

"Note that the introduction of the stochastic perturbations can produce gravity waves as observed on the right bottom panel."

RC: p. 2654, l. 24: Does the SD really “explain” the RMSE?

AC: Yes it does.

"is sufficient to explain" has been replaced by "is of the same order of magnitude as".

RC: p. 2655, l. 1: “approximately equal” is vague. Could you indicate numbers?

AC: We added this example at the end of the paragraph

"For instance, averaged over the Eastern Gulf Stream region (polygon Z on Figure \ref{fig:std_ens_ssh}) and the sixth month of the integration, the model error is equal to 0.17m and the standard deviation to 0,16m."

RC: 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

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in T/S diagrams?

AC: We are not convinced it would be easier to observe these phenomena on T/S diagrams.

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

AC: "ensemble" here means "probabilistic" (word has been replaced)

RC: 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?

AC: The value 10cm takes account of the representativeness error. Of course, this is a mean estimation applied over all the domain. This precision is added in section 4 (p2664 l15)

"(value taking account of both measure and representativeness errors)".

RC: 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?

AC: The observation error is actually introduced in the diagnostics. "can be" is replaced by "is".

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

AC: There are many different diagnostics we could use (Brier score, entropy, contingency table, etc). We chose to only show probabilistic scores (for scalar variables) that summarize the probabilistic properties of an ensemble system.

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

AC: CRPS is not the central point of the paper: it is only a convenient tool used to evaluate both reliability and resolution (probabilistic properties defined in Section 3).

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We think a simple example has no place in this paper. We already refer to the relevant literature for that (Stanski 1989, Hersbach 2000, Candille 2005). However, a sentence has been added in the CRPS presentation p2660 l1

"It can be seen as a generalization of the mean absolute error".

RC: 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?

AC: We think the full description of the Hersbach decomposition is not relevant for this paper (2 references are already cited). The sentence l14-15 has been replaced by

"The term Reli can be seen as the expected value of the absolute error and the term Resol as a correction factor that measures the sharpness of the probabilistic system."

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

AC: Unc is a particular value of the CRPS associated with the climatological function Fc. The integrand Fc(1-Fc) is a variance, but Unc (like the CRPS) has the dimension of the measure dx.

"component" is replaced by "part".

RC: p. 2661, l.2: Related to the former. Unc=0.070, which unit? cm or cmËĘ2?

AC: "Unc=0.07m"

RC: 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.

AC: "(these values take measure and representativeness errors into account)" has been added l19.

RC: p. 2662, l.5: The warning is not very constructive. What would the authors suggest

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as a good compromise for selecting the verification dataset?

AC: To rigorously get the good compromise, many different diagnostics should be performed over different time periods, geographic areas and depths in order to check the statistical stability of the results. In practice we try to aggregate as many as possible data to get stable statistics but trying to conserve the physical/dynamical coherence of the aggregated data. A full article (work still in process) is dedicated to this crucial point. So we cannot develop this here.

RC: 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?

AC: It is equivalent. "like in" is replaced by "equivalent to".

RC: 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.

AC: The matrix gamma is not in the observation space but in the reduced state space spanned by the ensemble. The update necessarily occurs in this reduced state space.

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

AC: There is no truncation in the algorithm. Zero eigenvalue do not cause any problem in the algorithm (see eq. 9 and 10).

"(with no truncation)" added in p2664 l14.

RC: 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 labelled under different acronyms.

AC: Right, but the analyzed step comes from the SEEK method which is anterior to the

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LETKF in the literature.

NoteÂă: missing U^T has been added in eq.(9).

RC: 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).

AC: Right, the mentions about the inflation factor have been removed p2265 l8 and p2666 l16. Also reference Hamill 2001 has been removed.

RC: 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)

AC: This has been added: "The localisation is applied on a local state vector as described in Brankart et al. 2011."

RC: 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.

AC: Some precisions have been added:

"(the cyan curves stop spreading)" p2665 l17.

"(blue dots outside the green curves area)" p2665 l24.

"(asymmetry of the blue dots outliers)" p2665 l25.

The spaghetti plots illustrate our point: we present the probabilistic approach where the ensemble is considered as a whole.

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

AC: "The updated ensembles (blue dots for the forecast ensemble and green curves for

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the IAU ensemble) present a noticeable spread reduction. Also they present a temporal variability ..."

RC: 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.

AC: Yes. "Since the diagnostics are performed against observations" has been removed.

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

AC: The sentence has been removed.

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

AC: Figures 11 (and the related text) have been removed.

RC: 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.

AC: The beginning of the conclusion has been modified.

RC: 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.

AC: Right, the system is underdispersive everywhere except in the Gulf Stream, and our perturbations only explain a part of the model error. The conclusion has been slightly modified to emphasis this point.

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Figures and tables:

RC: 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.

AC: The text has been modified.

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

AC: The velocities have been removed.

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

AC: Corrected

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

AC: Months added.

RC: Figure 8 has no units.

AC: Figure 8 is about SSH tracks: this has been added in the legend.

Typos:

RC: p. 2653, l. 28: strcuture

AC: Corrected.

RC: Fig. and Figure are used alternatively.

AC: Figure.

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

AC: No, we meant "overdispersive" as mentioned p2667 l26.

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