Reply to reviewer 2

We would like to thank François Counillon for his very careful reading of this manuscript and constructive criticism. We did our best to take his remarks into account in a revised version of the manuscript (see explanation below).

1. *More details about the smoother should be given in the introduction. A lot of paper has addressed the problem of smoother namely: Hunt et al. 2004 (Phys D) Hunt et al. 2007 (Tellus) Sakov et al. 2009(Tellus) How would the authors place his method among these recent studies? I believe the method used here is an EnKS (or a AEnKF or a LETKF) in terms of the estimation of boundary conditions. Here, the model state is extended with boundary perturbations; the present study consists of 1 assimilation cycle of 40 days that minimizes the surface current innovation and M2 tidal parameter. The optimal boundary perturbation is a combination of the ensemble boundary perturbation. The only difference with these methods is the rerun step. Such procedure is commonly used in petroleum application. They estimate parameters (porosity, permeability, etc, . . .) ; observe pressure and production rate at wells (no observation error), and at the end make a rerun in order to have a physically consistent and continuous simulation. They still claim using an EnKF /EnKS as they consider that the rerun does not make a new approach (See for example Evensen 2006 Data assimilation: The Ensemble Kalman Filter). Therefore, I would refer to this method as new, and merge section 4.2 and 4.3.*

   The 4DEnKF Hunt et al. (2004, 2007) and the AEnKF (Sakov et al., 2010), in particular, are indeed very close to the scheme presented. The localization step of 4D-LETKF (Hunt et al., 2007) is not used here. The common underlying idea is the optimization of model trajectories instead of model states. The original manuscript cited an earlier work of van Leeuwen (2001) containing this idea. The manuscript has been changed to make reference to more recent work:

   > The approach used here is closely related to the Ensemble Smoother (van Leeuwen, 2001), 4DEnKF (Hunt et al., 2004, 2007) and the AEnKF (Sakov et al., 2010) where model trajectories, instead of model states, are optimized. In the smoother scheme of van Leeuwen (2001), observations are perturbed as in the standard Ensemble Kalman Filter (Burgers et al., 1998). In 4DEnKF, the observation operator is modified to relate the observations at the time where they are measured to the time where they are assimilated. The AEnKF extends the observations vector and the matrix containing the ensemble members at the location where they are observed by vertically concatenating those vectors and matrices at different time instances. This approach is also used here because it is easier to implement than the 4DEnKF. But both approaches can be seen as equivalent. For an increasing number of ensemble members, the Ensemble Smoother does also converge to the 4DEnKF and AEnKF. In the present study, these approaches are not applied to directly estimate the model trajectory but to the perturbations of the forcings (or the trajectory of the forcing perturbation). Therefore, after the optimal correction of the forcings is computed, the model needs to be rerun to obtain the final model solution. For a linear model, both approaches provide the same results. However, for a non-linear model both approaches can provide different results. The optimal solution from the Ensemble Smoother, 4DEnKF and AEnKF is not guaranteed to satisfy the model equations, while it is per construction the case in the scheme used here. Since the method used here aims to estimate the optimal perturbations, this approach might be called Ensemble Perturbation Smoother.

   The reviewer questions if this method can be considered as a new method since parameters have been estimated in a similar way. However, the estimation of forcing fields is more complicated than optimizing parameters since it require perturbations with a coherent physical structure. The 4D-EnKF and the AEnKF are also presented as methods to optimize the trajectories of the model state. In some cases, both methods might actually provide very different results. Optimizing perturbations is more difficult, because the relationship between poorly known forcing fields and observations is expected to be less linear than the relationship between the model trajectory and observations. However in some cases it is more advantageous to optimize the forcings since they can be validated independently and used in a different model simulation (in particular for tidal boundary conditions where the parameters are time independent). The entire method (including the perturbation scheme) can be seen as minimization of the cost function:

$$2J(\mathbf{x}) = \mathbf{x}^T \mathbf{W}_E \mathbf{x} + (\mathbf{D}\mathbf{x})^T \mathbf{W}_D (\mathbf{D}\mathbf{x}) + (\mathbf{M}\mathbf{x})^T \mathbf{W}_M (\mathbf{M}\mathbf{x}) + \sum_n (\mathbf{y}_n^o - h(\mathbf{x}))^T \mathbf{R}^{-1} (\mathbf{y}_n^o - h(\mathbf{x})) \quad (1)$$

where the first part contains the constraints on the perturbation $\mathbf{x}$ of the complex tidal parameters and the last part is the data constrains including the dynamics model in the "observation operator" h. Such problems are normally solved using 4D-Var, but here an ensemble approach is used and an adjoint of the model is thus not needed. We realize that every ingredient of the approach is not new individually (constrained perturbation method, AEnKF, optimization of forcing fields), but it is combined in a new way, which to our best knowledge has not been done before.

The purpose of separating section 4.2 and 4.3 was to separate the general procedure (section 4.2) and what is specific to the current implementation (section 4.3). Section 4.3 has also now been expanded to state more clearly the definition of the state vector.

2. *There is little information about the data assimilation problematic (except for the noise) and hypothesis made. I would expect in the introduction or in the data assimilation part, a discussion explaining the problematic, and then explain why the chosen data assimilation method is adapted for the problem. For example, I understood that the "optimal boundary perturbation" is a combination of the different boundary perturbation obtained by minimizing variance of the surface current innovation and M2 tidal parameters innovations. This implies that the surface current depends linearly on the boundary condition? Is this reasonable? Furthermore, making the comparison with other method clear, a lot of sparse comments that can be considered as erroneous depending on the context can be removed. (e.g. "unlike Ensemble Kalman Filter" in page 2435, or "unlike classical Kalman Filters" in page 2433 ...)*

   It is indeed assumed that the relationship between the surface currents and tidal boundary conditions can be described reasonably well by a covariance. For a highly non-linear system, this might not be the case. However, for tidal propagation the non-linear terms are generally small. Reviewer 3 asked to compare the results obtained by optimizing the model trajectory with the results obtained by optimizing the boundary conditions. Both results would be exactly the same if the model would be linear. The difference of the RMS error of those experiments compared to tide gage data is very small (10^{-4} m). Therefore we are confident that the relationship between tidal boundary conditions and surface currents inside the domain is sufficiently linear for the method to work. If this relationship would be strongly non-linear, then the model run with the optimized boundary conditions would be not closer to the observations than the free run.

   Concerning the comparison with the Ensemble and classical Kalman Filter we reference to the algorithms as they are proposed originally and to the fact that observations are assimilated sequentially. The model state is only influenced by past observations. This is an aspect that distinguish filters from smoother schemes. We are aware of variants acting as a smoother on a given short time period and as a filter on longer time periods (*e.g.* Sakov et al., 2010). However, the difference between the standard Kalman filter and Kalman Smoother approach is still useful to distinguish the properties of the data assimilation scheme.

3. *In the introduction, the authors detail to my view too much engineering methods for reducing assimilation noise that is out of the scope of the paper. For the purpose of the study, one must use a method that does not produce assimilation noise. A smoother is an appropriate solution, and this paragraph is to my view not needed.*

   I certainly agree with you that a smoother scheme is the appropriate solution for this problem. But most data assimilation applications use a filter (compared to the application of a smoother). In an initial setup we tried also to assimilate the HF radar using the Ensemble Kalman filter, but without success: the assimilation created spurious gravity waves and at the next assimilation cycle (only 30 minutes later), the innovation vector included these spurious waves and lead to larger corrections than the previous cycle and produced even more spurious gravity waves. Several methods have been proposed to reduce this noise from spurious gravity waves, but we came to the conclusion that they could not be applied to the present case. It would be difficult (if not impossible) to distinguish between spurious gravity waves and tidal waves missing or misrepresented in the model solution. It is from this perspective that we introduced the manuscript.

4. *It is assumed that error in tidal signal is only originating from boundary condition, but a part from it is also originating from inaccuracy in the bathymetry. I believe that this deserve a comment in the text. You could mention as a future perspective that the method can perturb not only boundary condition but also the bathymetry as done in Mourre et al 2004. In addition information about the bathymetry is missing in the model part ?*

   We thank the reviewer for pointing out to the study of Mourre at al. We added in section 4.3 that bathymetry (and also bottom friction) are also factors causing uncertainties in the model. As the reviewer suggested, bathymetry perturbations are added as further perspective. In the model section 3, the following was added:

   The bathymetric data for the different model configurations are prepared using the ETOPO-1 topography (Amante and Eakins, 2009), together with observations made available from the German Hydrographic Service (Bundesamt für Seeschiffahrt und Hydrographie, BSH, Germany Dick et al., 2001). The larger scale model and the nested model include tides.

5. *One of the main problem using HF radar is the presence of the Stoke drift in the data but not in the model. Stoke drift is mentioned in the abstract, but not in the rest of the paper.*

   The Stokes drift was indeed not directly taken into account. For the analysis scheme is it thus seen as a "spurious" variability in the observations. The testing of various observation error variances and comparison of the analyzed solution to the observations provide an indirect safeguard preventing to fit the Stokes drift by adjusting the tidal boundary conditions. However, an explicit approach to remove the Stokes drift from the data (or adding it in the model) would be certainly the better approach. This is included in the conclusions as an additional perspective.

6. *Page 2425 line 3: It is not the frequency of assimilation that creates noise, but the way the assimilation is done. In the case of the EnKF it is the linear approximation (and rank issue) that causes most of problems. One can even think opposite: the more frequent the assimilation, the less strong the assimilation, and thus the less the assimilation noise. It is not clear how the tides are force in the model. Are they forced by the outer Nordic Sea model (u, v eta). Which tidal atlas has been used: EOT08a ?, FES2004 ?*

   Concerning the frequency of the assimilation, we agree that the assimilation at a high frequency should not degrade the results in a perfect assimilation scheme (error covariances are exact, model is linear and no bias). Those conditions are however rarely met in practice and the assimilation generates sometimes spurious variability. These problems are exacerbated when the assimilation is done frequently (see also reply to comment 3.). So we moderated our statement to make clear that the fundamental problem are that the hypothesis of the assimilation scheme are not verified if this problem occurs:

   > In this sequential approach however the model undergoes a sometimes vigorous adjustment process when the model is restarted (*e.g.* Malanotte-Rizzoli et al., 1989) if some assumptions of the underlying assimilation scheme are not verified (e.g. poorly known error covariances, model biases or non-Gaussian pdf). A too frequent assimilation of observations can even lead to the situation where the assimilation degrades the model results due to the high-frequency motions generated by the assimilation (Talagrand, 1972).

   Concerning the tidal atlas: the sea surface elevations of the open boundary of the North Sea-Baltic Sea model are generated using tidal constituents obtained from the TOPEX/POSEIDON data via the OSU Tidal Inversion Software (Egbert and Erofeeva, 2002). The solution from this model is used to provide boundary conditions for the nested German Bight model. The manuscript has been updated.

7. *The model resolution (0.9 km , sigma, time) differs from observation (3km, 0.5 m, 18min). Is this taken into consideration with H ?*

   Yes, the model results are saved at the same time step of the observations and the difference in the horizontal resolution is taken into account in H.

8. *I understood that the values optimized by data assimilation are ζ', u', v', is this correct? Those vary spatially, but are constant in time? Maybe it can be interesting to have a figure of the final optimized boundary M2 perturbation ?*

   Indeed ζ', u' and v' are optimized by the data assimilation. They are constant in time and vary spatially (manuscript is clarified).

   We agree, a figure showing the optimal perturbation of the M2 elevation ζ' is added to the manuscript. For the sake of brevity, the velocity fields are not shown because the structure and amplitude of these fields are closely related to ζ' and because they are not used to drive the GETM model. They are only included in the perturbation vector in order to use the harmonic shallow equations as a dynamical constraint.

9. *In 4.2 it is said that x is an ensemble of forcing field. I understand that this part is theoretical as it intends to introduce a new method. For this application, the forcing fields perturbs are only M2 tidal parameters? I find it confusing to have the theory and the application split, and I would rather explain clearly what is x, y, R , their dimensions for this particular application.*

   See the reply to the next comment.

10. *Page 2435 line 21: "Also for every . . . complex tidal parameter are interpolated at the grid of EOT08a data". Is it correct that you assimilate both model surface current vs HF radar, and model M2 tidal parameter vs EOT08a ? I guess this would become more obvious by defining clearly x,y, R. I would rather pace the definition of observation error in the data assimilation part instead of page 2429.*

    We agree with the reviewer and improved the presentation of the implementation:

    The vector $\mathbf{x}$ is composed in the present implementation by the complex M2 tidal parameter of elevation ζ', zonal u' and meridional velocity v' defined over the whole model domain. Those variables and vary spatially but are constant in time. This vector has in total 52374 elements. In GETM version 1.6, tidal boundary condition are implemented such that only elevation at the boundary is used for the ensemble simulation. In future studies, this vector can be augmented by other unknown parameters such as bathymetry or (space dependent) friction coefficients.

    The observations vector $\mathbf{y}^o$ includes the u and v components of the HF radar observations $\mathbf{y}_n^{HF}$ at all time instances t_n ($n = 1, \ldots, N_t$ with $N_t = 1869$) within the 40 day model integration at a resolution of 30 minutes (at 51 time instances no data is available). It also includes the real and imaginary part of the EOTs elevation M2 tidal parameters $\mathbf{y}^{EOT}$ (related to the amplitude and phase). This later field does not depend on time:

$$\mathbf{y}^o = \left[\mathbf{y}_1^{HF^T}, \ldots, \mathbf{y}_N^{HF^T}, \mathbf{y}^{EOT^T} \right]^T \tag{2}$$

In total $\mathbf{y}^o$ has 2120396 elements. The observation error covariance matrix $\mathbf{R}$ is here a diagonal matrix whose diagonal elements are obtained by a concatenation similar to equation (2).

11. *Figures: It is a policy of the journal to put the variables and units on the axis.*

    We added now label and units on figure expect longitude/latitude maps. We checked recently published papers in OS and most do not have longitude and latitude labels in these cases. However, if maps should also include labels, we will made this change.

12. *Figure 2: I think it would be nice to add the model's boundary.*

13. *Figure 6: I would put a line at the value retained (0.2). (Similar comment could apply to Figure 7)*

    Both modifications are applied.

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