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Interactive comment on "Observing and modeling currents on the continental slope: assimilation of high frequency radar currents and hydrography profiles" *by* A. K. Sperrevik et al.

Anonymous Referee #3

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The authors present a data assimilation study where HF radar surface currents and CTD profiles were assimilated. The objective of this study is to assess whether the assimilation of a rapidly deployable HF radar system can improve the regional ocean forecast during e.g. an oil spill event. An initial test is made with an idealized configuration of a frontal system to determine the optimal correlation length and time window. In the second part of the manuscript the results of the assimilation are presented and validated. The present manuscript is an interesting start, but I believe that more work will be necessary before publication.

My major concerns about this manuscript are the following:

1. The authors choose an idealized model configuration of a frontal system to test some parameters of the analysis. However the significance of these results for a realistic system is unclear and this part is not well integrated with the rest of the manuscript. It would have made much more sense to me if the initial test would have been done in the realistic model configuration used later with a classical twin experiment where pseudo-observations from a "truth" run of the model would be assimilated in a perturbed model run.

2. The assimilation windos length of 4DVAR is an important parameter which defines the propagation in time of the information contained in the observations. The authors note in the manuscript that a "slight improvement is obtained when reducing the window length from 72 to 24 h, but there is essentially no difference when the window length is further reduced to 6 h". As this is a central parameter in a 4DVAR scheme, I think that more discussion is necessary. Please show the RMS error (compared to the independent data sets) for different window lengths starting from 72 h and going down to 0 h (which makes the assimilation effectively a 3DVAR scheme). This experiment can be done either with the idealized setup or the realistic case (which would be the most relevant). Authors should also mention the typical time scale of the system. As the manuscript is written now, it seems that a windows length of 6 h is essentially as good as a window length of 24 h (the optimal choice) which leads to the question how appropriate the linearized error propagation under the assumption of a perfect model is (i.e. strong constrained 4DVar).

3. The assimilation of the CTD profiles seems to deteriorate the current forecast. This aspect should be analysed more detail. In general, the CTD observations should be presented. In the idealized experiment only temperature was assimilated. For the realistic experiment, the authors do not mention if also salinity was assimilated. The authors should also include which RMS error variance was used during the assimilation, how it was determined and how sensitive the validation

results with ADCP and drifter are to changes in this parameter.

4. The model simulation for the idealized and realistic configurations is very short (1 day of analysis and 4 day of forecast, with a single assimilation cycle). How statistically robust are the results?

Specific comments

1. section 2: "...correction of the tidal signal before assimilation, as described in Zhang et al. (2010). As our time series is too short to provide a good estimate of the observed tidal signal, no such corrections have been made": Can you be a more specific if tides are included or not in the model and observations?
2. section 3.3: "The 4DVAR schemes implemented in ROMS also has options for multivariate background error correlations, but since the underlying theories are dubious for high latitudes and eddy resolving models, we do not make use of any such options here.": Please provide more information about what approximation are dubious.
3. section 3.3: Instrumental error is one error component. Another one (and in general the largest) is the error of representativity. How has this error been dealt with in idealized experiments? The manuscript explains how the error variance that has been used for the assimilation was derived. However, it is not clear if this error has been actually added to the pseudo-observations (coming from the truth model). And if so, which spatial correlation length of the observation error (generally noted the R matrix) has been used?
4. section 4: How is the model initialized?

5. section 4.1: "For tracers and baroclinic [*the last i is missing in the manuscript*] velocities, boundary conditions as described in Marchesiello et al. (2001) are used. During assimilation, however, clamped boundary conditions with a sponge layer are applied." Why did the authors choose a different boundary condition for the data assimilation?
6. table 3: Why is there a large difference between iSLDMB and iSPHERE drifters during analysis? For the forecast column, does the sum in the skill score also include the time instance from the analysis ? Assume that this is the case, but it could be stated a bit more clearly in the manuscript.
7. section 4.3.1: "Two examples of modelled vs. observed trajectories are shown in Fig. 8.": A bit more description would be useful.
8. section 4.3: What is the RMSE of CTD observation in the analysis?
9. section 4.3.2: "Due to a displacement of an eddy in ALL, this simulation performs poorer for the ADCP location than HF and CTRL during the last days of the forecast." Please show this.
10. Please include in caption of the Figures 7, 9 and 10 to what the model is compared with (drifter or ADCP). It is in the text, but it would be easier for the reader if this is also included in the caption.

Interactive comment on Ocean Sci. Discuss., 11, 1357, 2014.