

## ***Interactive comment on “OSSE for a sustainable marine observing network in the Marmara Sea” by Ali Aydoğdu et al.***

**Anonymous Referee #2**

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The system gave me access to the previously submitted other review. I agree with their judgment that this is an interesting paper and that it is eligible for publication in NPG eventually. I also agree with their major comments 1 and 2 (which are probably linked to each other). I do not agree with their major comments 3 and 4: (3) not testing the impact of all choices on the results does not appear to be a problem to me if those choices are clearly stated; (4) the style is generally good, with some exceptions (some of them given by the other reviewer in "Minor comments"). Please find below comments of my own.

Major comments:

A. Unless this has been published elsewhere, it would be useful to have a brief analysis of the physical situation in the NR at the time of the assimilation experiments, if possible

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with figures.

B. I had an overarching question in my mind throughout my reading of the manuscript: is the combined effect of i.c. perturbations (a mix of short-term time lag and interannual variability) and diffusivity perturbations able to explain at least part of the model-data differences, within observational error? This question can be posed for (B1) simulated data, and (B2) any existing real observations (e.g., SST). The B1 question is a question of consistency of the innovations with Ensemble spread + obs error: it is partially covered in the ms. (e.g. Fig.8), but not exploited. The B2 question is about the realism of errors: it was not covered in the ms (only the conclusion mentions "lack of data"). Coming back to question B1: I have been frustrated that Fig.8 shows RMSE (of the dimension of the variable) and spread (of the dimension of the variable, \*squared\*). It would have been better to show \*MSE\* (not RMSE) and spread (assuming that this is \*prior\* Ensemble spread): then you could have tested whether the (prior) innovation variance was more or less of the same order as the (prior) "Total spread" (= your estimate of prior error + your estimate of obs error). I did the squaring visually, and the orders of both quantities do not match each other, especially for salinity. I believe that this should even briefly be discussed.

C. Why did you limit yourself to 7 days? Some of the error processes, especially those associated with mixing and stratification, could act on longer time scales.

D. The localisation cut-off scales are very short. Can't this trigger fast unphysical responses, for instance via temperature-sea level covariances?

Minor comments:

(some already made by the other Reviewer, but with my own words)

1. Page 4, lines 11-12: How is "prior covariance information" related to "dynamical balances"?

2. Page 4, lines 11 and 13: "the covariances are updated in every assimilation cycle":

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Isn't this contradictory with "it preserves the prior covariance information"?

3. Page 4, line 25: The generally adopted procedure to perturb diffusivity parameters does not use a centered Gaussian pdf.
4. Page 14, line 5: "is similar" -> "behaves similarly"
5. Page 15, lines 8-9: "...correct the subsurface fields": Fig. 9 is in data space (surface), so one cannot see a subsurface effect from that figure.

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Interactive comment on Nonlin. Processes Geophys. Discuss., <https://doi.org/10.5194/npg-2017-74>, 2018.

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