Review of: Towards Earth System Modeling: Coupled Ocean Forecasting

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Manuscript Synopsis

The article is a review of the ongoing trend toward the use of coupled prediction in ocean forecasting. As I am to understand, this is part of a series of reviews in a guide to the operational oceanography value chain. The article is a wonderful view of the need for coupled ocean prediction, the potential and potential benefit of integrating ocean prediction with existing atmospheric and hydrological prediction value chains and infrastructure, and finally some of the challanges to coupled prediction, and in particular coupled prediction including coupled assimilation. I would only *suggest* a few minor revisions to the article. My sole complaint would be while the cited Brassington et al. [2015] laid out the aspirations of the ocean prediction community to embrace coupled forecasting almost a decade ago, this article perhaps does not give enough credit to the various operational centres and systems that have managed to make progress on this front in the intervening period – my *Minor Comment* 2

My recommendation is Minor Revisions

Major Comments

None.

Minor Comments

1. I will begin at the beginning with the abstract's opening words: "The work we do is hard." Okay, I have paraphrased that somewhat for effect. That phrase, for me, evoked an image of a cartoon by Nathan W. Pyle posted on our coffee room bulletin board that "Science is difficult." (`https://www.facebook.com/nathanwpyle2/posts/466709991490794/` ; I apologize if the link is broken, but neither do I want the Journal subject to copyright violations.) More particularly, it eludes to a sediment that what we do is hard – and we should not particularly expect success, or be disappointed in a lack of success. A manuscript's abstract tends to be fairly personal, and I am not going to suggest the authors change this, as I imagine a lot of thought went into starting the abstract in this fashion. I just thought it good to remind that words can sometimes be read in unintended ways, with unintended consequences. Personally, I might have gone down a route that great strides have been made in ocean forecasting, but future advancement of our work, and our earth system prediction colleagues work, will require a coupled approach.

2. Brassington et al. [2015] lay out some of the intentions of the community a somewhat dated decade ago. Although the use of coupled seasonal and climate predictions was then, and is more so now, fairly ubiqious. Coupled short range and Numerical Weather Prediction (NWP) predictions are still a relative rarity, but they do exist, and probably should be given some credence, [e.g. Komaromi et al., 2021, Mogensen et al., 2017, Smith et al., 2018, Peterson et al., 2022], but I am sure the authors' literature review can identify some more (even if one has to resort to technical reports).

3. SST (Sea Surface Temperature) is not defined before its first use.

4. ll. 126-129. It is perhaps worth mentioning the ECMWF approach of integration into a single executable might be detrimental to open source / code sharing requirements, even if just one of the components is propriety code.

5. The authors discuss barriers to coupled data assimilation, particularly with regards to the added complications of cross model covariances in strongly coupled data assimilation. Unmentioned are other barriers, such as the differing time scales inherent in ocean forecasting and atmospheric NWP – likely further exasperated with the inclusion of land surface/hydrological modelling and biogeochemistry. However, Lea et al. [2015] does suggest using the shorter NWP based windows does allow for the retention of the longer oceanic time scales, as long as the memory inherit with cycling the system in time remains intact.

6. Futhermore, the authors do not mention some potential advantanges of coupled data assimilation, beyond the obvious acheivement of a more balanced initial state: Coupled data assimilation allows for coupled observation operators. Data assimilation of remote sensed SST, and more particularly remote sensed radiances, is inherently a coupled problem with the observed radiance a function of the SST and the atmospheric transmission, existing strategies (i.e. using processed SST retrievals) leave open the possibility of introducing external, and potentially contradictory biases from other systems. Similar advantages also exist with ice freeboard measurements (dependent on ice thickness and snow thickness), or even for remote sensing of ocean colour (dependent on ocean colour and atmospheric moisture; personal communications). Further examples likely exist outside my realm of knowledge. Again, this would be an advantage, allowing a fully self-consistent observation, with potential for a better and more self-consistent estimated state, although hardly a trivial exercise.

7. Spread and initial condition uncertainty (ll. 146-149). While I would agree atmospheric spread inflation schemes can often inflate ocean spread (SST) beyond initial condition uncertainty, I would also argue that quite often ocean spread does not adequately represent observed uncertainty. Peterson et al. [2022] showed that sea ice initial conditions failed to adequately represent the uncertainty in the estimation of the sea ice state. While that was in the case of a deterministic ocean and sea ice initial state used for ensemble forecasting, similar underestimation of the uncertainty exists in ensemble initializations: Sea ice perturbations in Zuo et al. [2017] are achieved by randomly sampling high resolution OSTIA sea ice concentrations into the lower resolution ORAS5 ensemble, however Renfrew et al. [2021] suggest the sea ice edge in OSTIA is too wide, owing to the large footprint of the SSMIS retrievals of the OSTIA assimilated OSISAF sea ice analysis. Randomly sampling a high resolution product, whose effective resolution is much coarser, is not going to adequately sample the uncertainty in sea ice concentration. Without any definite example, I would suggest at least for instances when an SST analysis is assimilated (which excludes [Lea et al., 2022]), similar reliance on a single smoothed analysis might lead to an under-representation of the SST observation uncertainty in an initial spread of SST – although here, the inherent smoothing of the SST analysis is not as obvious – the microwave satellite footprints are actually quite high resolution – it is the correction of (due to atmospheric transmission) bias, anchored by more sparse insitu measurements, that likely leads to the smoothing of the analysis.

8. Please do not forget to fill in (or remove) the acknowledgements section.

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