Ocean Science Discussions

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Comment on os-2021-64

Anonymous Referee #2

Referee comment on "Assimilating realistically simulated wide-swath altimeter observations in a high-resolution shelf-seas forecasting system" by Robert R. King and Matthew J. Martin, Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-64-RC3, 2021

The stated aim is to "assess the impact of assimilating SWOT observations with and without the expected correlated errors and to assess simple methods to reduce problems associated with correlated errors." These objectives have been accomplished admirably, and the outcomes are summarized directly and succinctly in the abstract.

In the introductory preamble, present altimeter capabilities are a bit underestimated. On line 33: "Although along-track observations can have a sampling frequency of ~7 km, various sources of noise limit the feature resolution to 100 km (Xu and Fu, 2013)." This is pessimistic for modern altimeters. The small footprint of AltiKa, and the enhanced along-track resolution of the SAR-Mode Delayed Doppler altimeter on CryoSat, Sentinel-3A/B and Sentinel-6 has brought the resolved wavenumber spectrum down to ~50 km, or possibly less with advanced re-tracking (e.g., ALES – Passaro and Birol papers). Admittedly, this is along-track, and is not realized in 2-D gridded products.

Similarly, it is claimed (line 46) that "data from altimeters could help to constrain processes such as tides and storm surges which are represented in the model … However, the sampling ... by existing nadir altimeters is not currently sufficient to adequately constrain them in the NWS region." I think it's more the case that we don't yet have DA schemes that can make full use of the observations in constraining these dynamical processes. Useful information is there in the data.

There is a nice review of prior work on dealing with correlated errors. I found this very helpful.

A strength of the paper is its approach of evaluating what SWOT might bring to a fully configured, mature and advanced high-resolution operational forecast system, as opposed to a hypothetical analysis. The study is thus highly relevant to the mission of GOOS and OceanPredict, and will be well received by those user communities for the guidance it

offers.

Are the open boundary conditions for Nature Run and OSSEs the same? It's not explicitly stated. One disturbing result that is never really explained is why there should be a slow drift in SSH bias. Is the free run model steadily changing net volume, that assimilation serves to restore by reimposing the MDT along with the observations?

I would be interested to see a map of the regions that are predominantly in the category of top-to-bottom temperature difference less than 2^{o}C where the balance adjustments to temperature and salinity are not applied. This woud add context to Figs. 7 and 11. Unfortunately, we are not offered a map view of the skill for temperature and salinity to complement Figs. 7 and 11, which is an oversight the authors might care to address in revision. I leave it to them to decide how to usefully present this 3-D skill assessment in a 2-D map.

I have some reservations about how appropriate the balance operator approach is for shelf seas, but that’s a can of worms we can’t open here. However, I would not oppose some rampant speculation about how altimeter sea level data might be better exploited in shelf sea DA systems.

The faux observation errors introduced to the pseudo-observations is explained and justified very well, including the simulated geolocation error.

The term RMSE is not defined when it is first used, and it is not spelled out whether this is full Root Mean Squared Error of observation minus model, or what is frequently called Centered RMS Error in geophysics, being the RMS of the difference between observation *anomaly* and model *anomaly* from their respective means. CRMS and bias are independent errors. I suspect here we have CRMS, otherwise we would need to tease out the effect of bias in the RMSE statistics. But, conventionally, RMSE includes bias, so please clarify.

I would welcome some speculation as to why temperature and salinity on the shelf is improved, but velocity and sea level are not. Here, some spatially explicit view of where the balance operator is being applied, and where it is not, might be instructive.

Indeed, I wonder if the results in Fig. 10 would differentiate further if they were conditionally averaged by whether the balance operator was applied, or not. I encourage this addition to the paper. Perhaps the authors already made this calculation and found it of no consequence. If so, a remark to that effect would be useful to readers.

Speed (Fig. 11) is only one measure of current errors. What about direction? The speed

error could be zero but with respective currents pointing in opposite directions.

Overall, this is an excellent paper. A great strength is the consideration of a comprehensive suite of practical correlated error mitigation strategies (half-SWOT, different range averaging, etc.). The well designed and well executed set of OSSEs should motivate other such studies, not just of SWOT but also the numerous emerging new observation networks (HF-radars, autonomous platforms, etc.) which would be valuable information to the GOOS Observations Coordination Group and similar national agencies, and OSEval Task Team of OceanPredict.

Minor comments:

Fig. 2 and 3 captions. Please say whether the bias is Free minus Nature, or Nature minus Free.

The resolution of many figures is poor. It looks to me like these are produced with matplotlib, in which the case the fix is simply to specify dpi resolution in savefig.