Geoscientific
Model Development
Discussions

Open Access

EGU

Interactive comment on "The Regional Ice Ocean Prediction System v2: a pan-Canadian ocean analysis system" *by* Gregory C. Smith et al.

Anonymous Referee #2

Received and published: 9 November 2020

Review of The Regional Ice Ocean Prediction System v2 by G. Smith et al.

General comments The article presents the new configuration of the RIOPSv2 Arctic forecasting system, which - compared to its predecessor - counts two novelties related to the assimilation of SST data and an advanced tidal filtering for the assimilation of SLA. The new system is presented in many details, including illustrations of the multivariate and anisotropic spatial background covariance from the ensemble, a hardcore mathematical derivation of the harmonic analysis used for the SLA assimilation and a comparison of the system results to its mother system, the global non-tidal, coarse-resolution GIOPS. However, the paper does not evaluate any of the improvements from the beginning to the end, which limits the impact of the paper. This applies particularly to the time-dependent harmonic analysis, which represents a significant effort

but which results are frustratingly terse. Is the filter robust? How does it compare to other tidal filters that are not designed for ice-covered areas? Even though the article does not perform a clean assessment of the advanced harmonic analysis against a more rudimentary filter, the paper is overall of high standards and worthy of publication as a description of an operational system. The main barriers to appreciating it are the length of the paper and the tendency to accumulate distracting topics that do not help those who may be tempted to reproduce the results. I would recommend the paper is published under the condition that the authors focus on the novel topic of the paper - the harmonic analysis - and provide more ample evidence that the approach is worth the effort.

A secondary innovation of the paper is the smoothing of model SST fields before assimilation of high-resolution SST, which visually improves the innovation field. I believe that this smoothing does not interfere with the effect of the harmonic analysis and that it tends to make the comparison of RIOPS to GIOPS more relevant, so this part should remain in the paper.

The multivariate and anisotropic structures of the ensemble covariance matrix, however, are not a specific novelty of the present paper but are common to all applications of SAM and other ensemble-based techniques. Since these are not used to explain the results, I would shorten that part to a few sentences. Figures 3 to 6 are also smashing graphics, but they are of little relevance to the rest of the paper. I would recommend removing them (maybe leave one) and their description to shorten the paper.

The description of the assimilation cycle would deserve some clarification and one figure has gone missing. Otherwise, the paper is very well written and makes an interesting read.

specific comments - The title of the paper is too generic to indicate its actual contents. There is no way that anyone interested in the harmonic analysis in seasonal ice-covered waters would track it back to this paper unless the keywords "tidal" and

"observation operator" are in the title.

- l39: Salinity biases of 0.3 - 0.4 psu sound extremely good, so it should be noted that these are averaged over the top 500 m.

- The introduction does not cite any previous attempts to assimilate SLA data in a tidal-driven model. Discarding the papers dedicated to the estimation of tidal parameters, a reference to the tidal GOFS v3.1 from NRL and the more rudimentary method by Xie et al. (2011) could indicate what is available.

Xie, J., Counillon, F., Zhu, J., and Bertino, L.: An eddy resolving tidal-driven model of the South China Sea assimilating along-track SLA data using the EnOI, Ocean Sci., 7, 609–627, https://doi.org/10.5194/os-7-609-2011, 2011.

- l130 Has Paquin et al. (in prep.) become accessible in the meantime?

- The RIOPS v1.3 has not been used in the whole paper, so the description of the old system (including Table 1) should be removed to shorten the paper.

- l158 if the SST is not cycled with the assimilation, how is it assimilated then?

- l170 it took me a long time to understand the 3 assimilation cycles in Figure 2. What is assimilated in the RR cycle? Are the SST and sea ice concentrations not assimilated in the RD and RR cycles? Then, the authors should specify that the first "R" stands for "Regional".

- l176 Pham et al. 1998 describe the evolutive basis of the SEEK filter, please specify that you use a fixed basis here.

- l184 Talagrand's (1998) adaptivity scheme is not common knowledge. Is it following the criterion that the cost function should remain superior to half the number of observations?

- l189 contradicts l159 where the bias correction is only planned.

- l207 is the SST projected in the vertical or nudged at the surface?

- l277 should refer to Figure 7, not 8.

- l310 if C contains the phase, it should be dependent on k. Please note it Ck then.

- l317 from Eq 3a to 3b, only the left-hand term of the product has been conjugated. My maths are buried too deep in my brain to remember why. Please explain briefly.

- l318 Wnm seems to be a temporal covariance matrix. If it is diagonal, this means that the tidal residuals are assumed to be white noise, can you confirm?

- l318 why bother with two indices nm if the W matrix is diagonal?

- l331 if C is depending on the frequency, can it be cancelled?

- l345 The restoring time length appears discretely in parenthesis. I believe this is the only arbitrary parameter of the method please explain the choice of 30 days.

- l350 Can you shorten this sentence using the (m-1)th, or (m-1)st, time step? I find the use of the prime instead of -1 cumbersome.

- l355 The weights are decreasing exponentially. This should be stated explicitly.

- l365 I am missing an illustration of the tidal filter weights, at one sample point in winter and in summer, which could be compared to a more common tidal filter. See for example a few convolutions below, some one them can work one-sided, using data from the past only: https://www.sonel.org/Filters-for-the-daily-mean-sea.html

- l422 The typical amplitude of the Msf constituent would be useful to know here. Is it worth including it at all if its removal is so problematic?

- l462 Again, information about RIOPSv1 should be removed as the system is not used in the paper.

- l535 Are there sufficient profiles in the Arctic for a robust bias correction? It seems the positive impact is only noticed in the Southern part of the domain.

- l585 The MDT in the central Arctic is coming from a different system, the GLORYS reanalysis, which is prone to inconsistencies. Did GLORYS assimilate ITP profiles for example?

- l635 This paragraph is only loosely related to the rest of the paper, is it necessary?

- l962-963. Too long sentence, I cannot follow the point.

- l1036. Is this figure a snapshot? Is it taken in the summer or winter? This could affect the amplitude of the inverse barometer component.

technical corrections - l341 Missing "a" before diagonal - l514 Missing "the" before Arctic Ocean. - l622 Has the OPP acronym been defined before? - l974 the element IS involving THE sine dimension - Figures 9 to 13 are too small, it is hard to see what happens in the North Labrador Sea - Figure 14 (deep biases) is missing but the caption remained, so the captions are shifted thereafter.