Response to Reviewer 2

Text from reviewers is in black italics with responses in blue.

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.

This is not quite right, the previous version of RIOPS did not have its own data assimilation system but rather used a spectral nudging approach to constrain temperature and salinity fields towards GIOPS analyses. As such, the introduction of the data assimilation system for RIOPS is perhaps the main novelty. The online tidal filter is also novel. The manner in which we assimilate SST data is not new, but has not previously been published.

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.

As this is the first analysis system for RIOPS (and the first pan-Canadian regional analysis system), the main objective of the paper is simply to document the RIOPSv2 system and evaluate the analysis quality as compared to GIOPS (this provides a proxy for comparing to RIOPSv1, since RIOPSv1 was nudged toward GIOPS). Specific "beginning to end" evaluations may build upon this effort.

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?

We agree with the reviewer that the harmonic analysis merits a clear demonstration. A figure has been added to demonstrate the robustness of the online filter as compared to the commonly used "T-tide" filter with a discussion provided in Section 3.4.4.

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.

As noted above, a figure and discussion has been added to focus more on the harmonic analysis. To reduce the "distracting topics", the number of figures included to highlight the covariance structures has been reduced. We do feel that showing these covariance structures and the filtering used in the SST observation operator are essential components of the RIOPSv2 system that warrant inclusion to adequately document how the analysis system is constructed.

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.

Thank you. We agree.

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.

As noted above, only one figure showing covariance structures has been included in the revised manuscript as suggested. The text in this section has also been shortened accordingly. The technical details describing the construction of the error modes has been left in the manuscript as this is an important component of the system.

The description of the assimilation cycle would deserve some clarification and one figure has gone missing.

The description of the assimilation cycle has been improved as you suggest below (specific suggestions) and the issue with the figure captions has been corrected. Our apologies for this oversight.

Otherwise, the paper is very well written and makes an interesting read.

Thank you very much for your kind comments and for taking the time to review the manuscript.

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.
Thank you for the suggestion. We have changed the title to : “The Regional Ice Ocean Prediction System v2: A pan-Canadian ocean analysis system using an online tidal harmonic analysis”

- 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.

Abstract modified as suggested.

- The introduction does not cite any previous attempts to assimilate SLA data in a tidaldriven 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.

A paragraph has been added to the introduction to discuss assimilation of SLA data in ocean models that include tidal variations, including a mention of the Xie et al. (2011) study. We were unable to identify a suitable reference for the GOFSv3.5 system (GOFSv3.1 does not contain tides). We even contacted the scientists responsible for the development of GOFS.

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

No. This reference has been removed from the revised manuscript.

- 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.

It is a reference to indicate how the new system differs from the previous operational version. As such it a useful reference to aid in reproducibility.

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

As described in Section 3.3.4, the gridded SST analysis produced by CCMEP is assimilated into the SAM2 system used by RIOPS. The use of a gridded SST analysis products avoids the need to address various issues associated with the assimilation of SST data (e.g. sensor biases, data density) and provides greater consistency between surface conditions from the ocean model and those used in producing the atmospheric forcing, thereby reducing unphysical surface fluxes (see Smith et al., 2018). This approach is fairly standard and has been used by various studies in the past, e.g. Lellouche et al. (2013, 2018).

- 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".

Additional detail has been provided to the text describing Fig. 2 to clarify. In particular, it is noted that the “R” in RD, RR and RU stands for RIOPS. Also, both RD and RR assimilate all available observations, albeit RD has a longer cutoff and thus includes a greater number of in situ and SLA observations.

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

Done.

- 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?

The idea is that the variance of the innovations should be equal to the variance of the background error plus the variance of the observations error.

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

Line 159 indicates that the inclusion of the bias correction (evaluated in this manuscript) is planned to be implemented operationally in fall 2021. The sentence has been reworded to clarify. The main point is that the version of RIOPS evaluated here (v2.1) is planned for operational implementation in fall 2021.

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

The gridded CCMEP OI SST product is assimilated together with in situ observations of temperature and salinity and SLA anomaly observations using the SEEK approach. As such there is no need to project the SST in the vertical, nor to nudge it at the surface. Multi-variate corrections are produced based on the covariances in the background error modes.

- l277 should refer to Figure 7, not 8.

Corrected.

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

Thanks, we denote C as C_k, then equation (2) becomes as

$$E_k^n = C_k exp(i\omega_k \tau n) \qquad (2)$$

- 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.

Thanks for pointing out this error. Equation (3a) need be corrected as following:

$$J = \frac{1}{2}(A^n - H^n)^* W_{nm}(A^m - H^m) \ , \qquad (3a)$$

- 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?

Yes, we assume tidal residuals are uncorrelated in time and represent W_{nm} as a diagonal matrix that specifies the time weights used in the least-square fit. Ignoring the time correlation allows us to represent W_{nm} as a diagonal matrix rather than a vector in order to use the Einstein Summation Convention.

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

As explained above

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

Yes, it can be cancelled because C_k is time-independent (independent of time step n). This can be shown by denoting $F_j^n=\exp(i\omega_j \tau n)$ and rewriting Eq. (2) as $E_k^n=D_k^j F_j^n$, where D_k^j is a diagonal matrix with C_k as its k^{th} diagonal element. Substituting this into the definition of matrix B and vector Y, we have

$$B_{kj}= D^{*}{}_{k}{}^{k'} F^{*n}{}_{k'} W_{nm} F^{m}{}_{j'} \boldsymbol{D}^{j'}{}_{j} = D^{*}{}_{k}{}^{k'} G_{k'j'} \boldsymbol{D}^{j'}{}_{j}$$

$$Y_k= D^{*}{}_{k}{}^{k'} F^{*n}{}_{k'} W_{nm} H^{m} = D^{*}{}_{k}{}^{k'} U_{k'}$$

By taking the inverse of the B matrix by the formula $(D^{*}GD)^{-1}=D^{-1}G^{-1}D^{*-1}$, and substituting the above equations into Eq. (6a), matrix D (and thus C_k) is cancelled. This means that the final solution of A_n is independent of C_k.

- 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.

Thank you for pointing out this omission. The following text has been added in Section 3.4.4 :
"The other free parameter in the sliding window approach is the time scale used in weighting function. Here a value of 30 days is used. This value must be large enough to permit an accurate fit of the different tidal constituents. Using a longer value reduces the ability of the system to adapt to seasonal changes in tidal variabitity."

- 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.

The use of the prime provides a straightforward way to indicate quantities valid at the previous time step. This is intended to aid in producing a numerical code based on this algorithm.

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

Done.

- 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

As pointed out above, the tidal filter weights are equivalent to an exponential function based on the specified timescale (30 days). These weights have been plotted in the Fig. R1 below. Here, the weighting function has been plotted along with an exponential function to illustrate their equivalence. Note that there is no summer or winter dependence, nor any spatial variation in the value of the filter weights. They are strictly a function of the specified timescale.

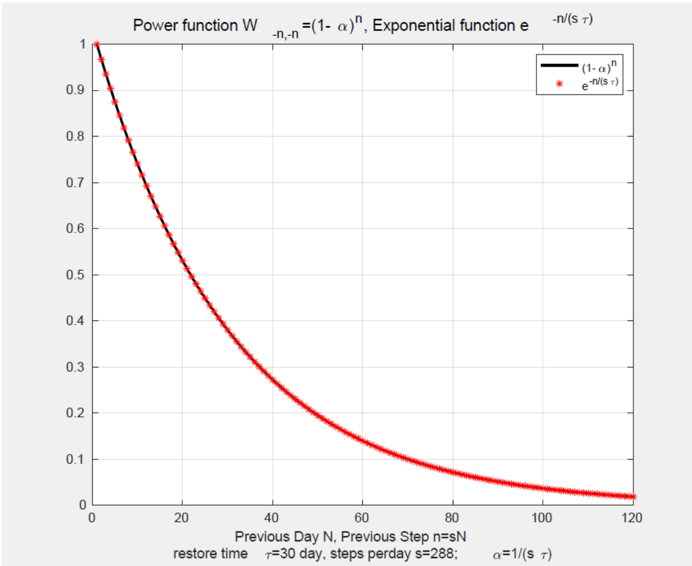

Fig. R1: Comparison of the tidal filter weighting function with an exponential function. Both functions are plotted as a function of days, using the time step from RIOPS (i.e. 288 steps per day) and a restoring timescale of 30 days.

For comparison, we've also plotted a few common tidal filters in Fig. R2. We performed a harmonic analysis at several points using these three filters. While the two-sided functions provide a roughly equivalent result to that of T_tide and the online filter, the one-sided function (required for real-time operational use) results in a significant phase lag and large residuals.

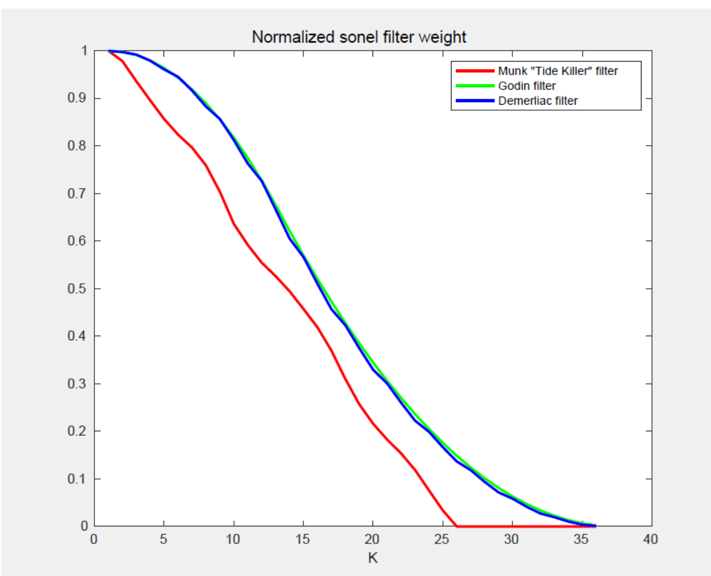

Fig. R2: Normalized tidal filter weights for a few common filters (Munk "tide killer", Godin and Demerliac) with the value of K in hours. Filters were reproduced using information from the Sonel.org website based on information from Bessero (1985).

- l432 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?

The Msf is simply used here as an example of a long-period tidal constituent. The amplitude of the Msf constituent (and other long-period constituents) varies considerably over the domain, but is generally much smaller than the principle semi-diurnal and diurnal constituents. Indeed, they are not worth including in the online harmonic analysis given their amplitude and the associated difficulties in removing them. These sentences have been removed to avoid confusion.

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

The reference here to RIOPSv1 is essential to explain our evaluation approach. One of the main objectives of the paper is to provide an evaluation of the analysis system implemented in RIOPSv2. Since RIOPSv1 was nudged toward GIOPS, we use GIOPS as a proxy for RIOPSv1.

- 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.

Indeed, the number of profiles in the Arctic is smaller than in other regions (Fig. R3). However, in some areas, such as the Beaufort Sea they show a consistent pattern revealing positive salinity biases. Moreover, this number of profiles is greater than is usually present due in part to the additional profiles deployed as part of the Year of Polar Prediction (as discussed in the manuscript).

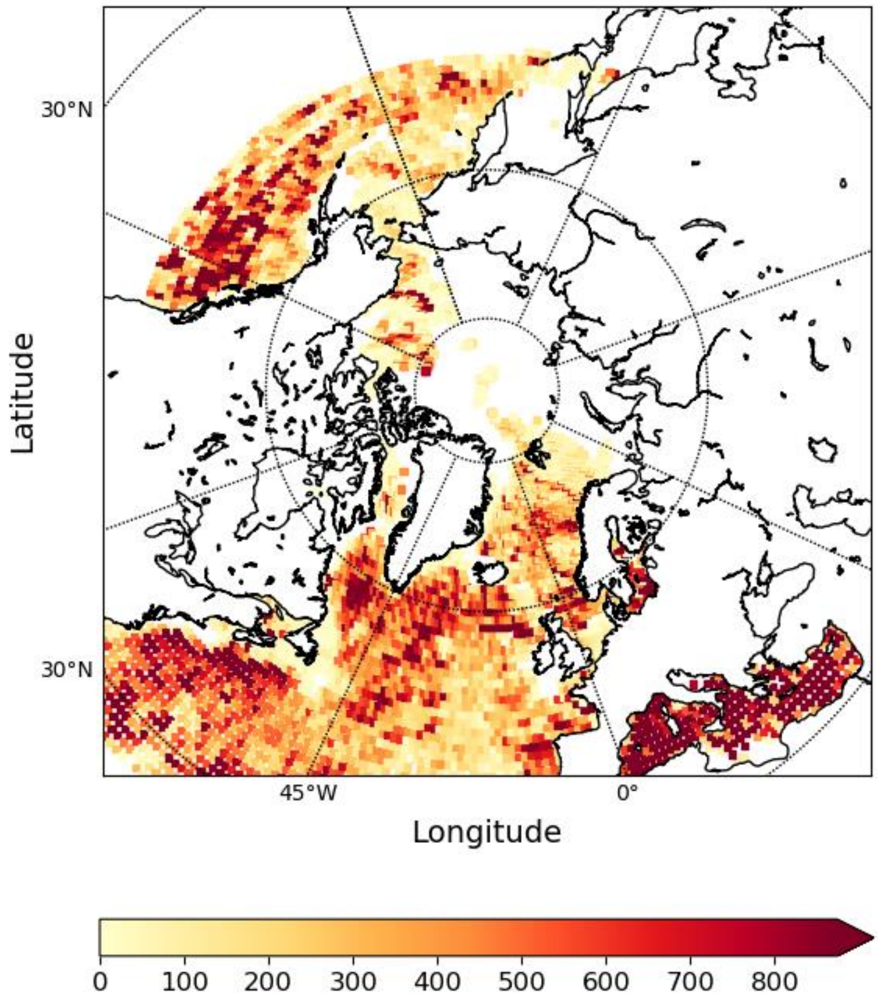

Fig. R3: Number of profiles used in innovation statistics (Fig. 10) of in situ salinity over the upper 500 m for the period 01-Jan-2016 to 31-Dec-2018.

- 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?

As described in Section 3.1, the MDT used is a hybrid product that uses the CNES-CLS 2013 product as a base with modifications based on mean increments from the GLORYS reanalysis (as described by Lellouche et al., 2018). Owing to the small number of data in the Arctic, especially over the earlier period of the GLORYS reanalysis, the modifications to the CNES-CLS 2013 MDT in the Arctic are quite minor (nor do we believe that GLORYS assimilated ITPs). The main changes are found near Indonesia, the Red Sea and Meditarrenean Sea (Lellouche et al., 2018).

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

This paragraph was included as RIOPS is often used for sea ice prediction and so a comment regarding the impact on sea ice related applications is relevant (indeed Reviewer 1 requested that we expand on this!).

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

The sentence has been simplified as follows: "This means that the 2K+1 dimensional real vector space is a complete invariant subspace when operating under S"

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

Yes, the figure is a snapshot valid for 31-Dec-2015. The date used as been indicated in the caption for the revised figure. While the amplitude of the inverse barometer varies based on the synoptic situation, it remains small compared to the amplitude of the tidal signal.

Technical corrections
- l341 Missing "a" before diagonal
Corrected.

- l514 Missing "the" before Arctic Ocean.
Corrected.

- l622 Has the OPP acronym been defined before?
OPP Acronym written out in full in revised manuscript (Ocean Protection Plan).

- l974 the element IS involving THE sine dimension
Corrected.

- Figures 9 to 13 are too small, it is hard to see what happens in the North Labrador Sea
If you zoom in (increase magnification) it is possible to see the details in the Labrador Sea quite clearly. We`ll ensure that the figure quality is sufficient to see these features clearly during the proofing stage of the paper.

- Figure 14 (deep biases) is missing but the caption remained, so the captions are shifted thereafter.
Our apologies for this error and any confusion it caused for the review. Figure captions and references have been corrected in the revised manuscript.

References:

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