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

Anonymous Referee #1

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This paper describes and evaluates the high resolution CCMEP analysis which spans the coastline of Canada and the Arctic. The model and assimilation methodology are introduced in relation to the previous regional system. The performance of the system is assessed by an evaluation of background departure statistics against sea level anomaly and in-situ observations, and compared against the Global Ice Ocean Prediction System.

Firstly let me apologize to the authors for the delay to my review and thank the editor for allowing me sufficient time to complete it. I am glad to be able to have thoroughly read the paper and I feel it very well describes the operational system. Overall I am sure this paper is perfectly suited to GMD and should be published subject to some minor corrections/additions.

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Discussion paper

I will first give my more general comments, and subsequently give the itemized comments I had whilst reading through the paper.

General comments:

The paper is very well written and was enjoyable to read. For a description of an operational analysis system the authors have the balance just about right in terms of the detailed description within this paper and referencing previous work. In some areas I was not clear whether those references would suffice or if they were differences between the previous system and RIOPSv2. For example the boundary conditions used for the regional system are not well highlighted (I assume they come from GIOPS but this isn't explicitly stated?). Also when the change to model levels was mentioned it highlighted that the vertical domain was not well described and may lead to confusion so a sentence or two would go a long way to help the reader here.

The section on the online harmonic analysis was far more in depth than the others. I am not familiar with such methods and therefore not sure where the methodology from the literature finishes and the new science (I think related to the sliding window) starts. If this could be highlighted it would be a useful addition.

More generally what is not discussed is the mismatch between a model that contains tides (needed for the coastal applications) and SLA observations which do not contain tidal signals. However the raw altimetry data would necessarily contain tidal signals, so it would benefit the reader to have a short section on SLA observations and why you aren't using lower level SLA observations.

I was hoping to see more about sea ice performance but the final paragraph is sufficient to let the reader know why it is not shown. I wonder if this could be expanded upon to hint at how a future system might look to improve sea ice predictions?

Finally I would ask that the authors look at the colour scales they are using in their plots. I think a lot of the structures we see are artifacts of the jet colour scheme and the use of perceptually uniform colour scales would remove such artifacts and make the paper more accessible to colour blind readers.

Detailed comments:

Line 126: I am unfamiliar with these tidal constituents - is this detail necessary and if so should such parameters be described?

Line 137: Please comment on the vertical domain - "deep layers (from 500m" suggests these are the deepest layers in your domain, but I would expect the domain to go down to $\sim$4000m in the arctic.

Line 170: This has strong similarities to the ECMWF workflow shown in Browne et al. 2019 Figure 3. ::: Browne, P. A., de Rosnay, P., Zuo, H., Bennett, A., & Dawson, A. (2019). Weakly Coupled Ocean-Atmosphere Data Assimilation in the ECMWF NWP System. Remote Sensing, 11(234), 1–24. https://doi.org/10.3390/rs11030234

Lines 209-211: "Another modification required for coupled forecasts was to use 24-h averaged short and long-wave radiation fields to force NEMO-CICE during the analysis cycles such that there is very little diurnal warming present in the ocean analysis". Please can you elaborate on this. Is the system designed to have a strongly damped diurnal cycle in the analysis, or are there reasons for which this was found to be necessary?

Line 215: Smith et al (2015) should be 2016? Or a different paper?

Line 228: Can you clarify - does this mean you apply the increment from the current cycle to the next cycle (in the first 24 hours)?

Figures 3-6: Subplot titles are missing commas, i.e. <SST(r)SST(r+\Delta r)> should be <SST(r),SST(r+\Delta r)>

Figure 3 - star is not visible, please can you add it on top or produce the image at a higher resolution? In fact maybe just change the text to make it clear the point you are

considering is the bottom left corner of the magenta box?

Figure 3 - this is the first time you mention localisation. Please can you elaborate on why you choose the region that you do? Is the localisation a tensor product of horizontal and vertical localisation functions, or is there no localisation in the vertical? Is it a hard cut off or is it smoothly reduced to zero at the boundary of the "bubble" by a Gaussian or Gaspari-Cohn function? (I suspect you will have a reference for this which would be good, but also nice to know if you have had to make any changes here for RIOPSv2).

Figures 3-11,13,14: If possible please replot using a diverging, perceptually uniform colour scale. For instance in Figure 3, <SST(r)T(r+\Delta r)>, the current choice of colourscale appears to show a clear change in correlation around layer 10 whereas I think this is an artifact of the rainbow colour scale. Thyng, Kristen M., et al. "True colors of oceanography: Guidelines for effective and accurate colormap selection." Oceanography 29.3 (2016): 9-13. https://doi.org/10.5670/oceanog.2016.66 Hawkins, Ed. "Scrap rainbow colour scales." Nature 519.7543 (2015): 291-291. https://doi.org/10.1038/519291d

Line 278: "model trial field" is not well defined. I suggest replacing with something like "raw model equivalent field"

Figure 7: Please relabel "CMC" -> "CCMEP" and "Trial" -> "RIOPSv2"

Line 299: ", the use of a rotation" -> ", and the use of a rotation"

Line 306: Are you using Einstein notation here so there is an implicit summation over the repeated index k? This should be noted. Oh I now see it introduced on line 320 - is it used before or only from line 320?

Line 311 - the definition of \omega_k is unclear. please look carefully at the wording you use here.

Line 315: Is this a minimisation over only Xˆk ?

Line 338: Prime symbol is not consistent - MS Word issue?

Line 350: "According to the definition of the sliding window weight". Where is this defined? Eqn 3a? Also have you defined explicitly what "its property of normalisation" is?

Figure 8: Can you clarify, possibly in the caption, that (a) = (b) - (c) - (d)? Again, diverging colour scales would be much better here. In fact the total SSH field (b) is confusing: "Panel (b) shows the instantaneous model SSH field prior to any treatment." but its range is (-1,1). Has this already had the MDT removed? I personally am more familiar with considering the MDT for SLA assimilation when using processed SLA observations that have tidal signals removed, so I would like to see precisely where the MDT is required in your methodology.

Line 477/478. Regarding the Gulf stream errors, would you expect the SEEK filter methodology to be able to effectively constrain a region which such variability given the error covariances are coming solely from a climatology and therefore may be far too smooth there? It is nice to see the errors reduced in RIOPSv2.

Line 485: The difference in errors in the Laptev sea is intriging. Could you produce a comparison plot of the two different MDTs used?

Line 488: We should see errors here in salinity too if that is the case? [Refer to my later comment on Figures 11-13]

Line 489-491: Surely this is entirely due to ice cover - polar orbiting satellites should have much richer coverage in the arctic due to their much more regular return period (if you exclude the polar data gap).

Section 4.2.2/Figure 10. Given the broadly similar structures and different scales between Gulf stream errors and other regions it would be nice to see a difference plot and/or a normalised difference plot of 10(a) and (b). This might be too difficult to produce in a reasonable amount of time/effort, but may prove enlightening.

Figures 11-13: They appear to be missing the polar sector from 45E to 180E. Is this a plotting error? Are there no in-situ obs in the Hudson bay? I was hoping to see salinity departures which would corroborate interpretation of errors around the mouth of the Lena river that you associated to the fresh water fluxes.

figure 11 - Salinity worse in the Baltic sea - are there problems in brackish waters?

Line 522: "upper 50 m of the water column" did you mean 500m as in the plot?

Line 540 - should be Figure 14?

figure 15(b) - are units km?

Line 545: please refer to table 3 for the RMS numbers too.

Table 3: Is this R or Rˆ2 you are listing?

Line 546/546: "likely representing the delay in the analysis system in adjusting to SLA errors". Could it not be due to the non-stationary location of the front/eddies? And looking only at observations with a 10 day return period you are not capturing the evolution of the model between overpasses?

Line 553 - Figure 15 -> Figure 14. Also subsequent references to Fig 16 -> Fig 15.

Figure 15: Caption is for Figure 14

Figure 16 not present but referenced.

Line 622 "OPP" -> "YOPP"?

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