

Ocean Sci. Discuss., referee comment RC2
<https://doi.org/10.5194/os-2021-50-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on os-2021-50

Anonymous Referee #2

Referee comment on "Atmospherically forced sea-level variability in western Hudson Bay, Canada" by Igor A. Dmitrenko et al., Ocean Sci. Discuss.,
<https://doi.org/10.5194/os-2021-50-RC2>, 2021

Review of "Atmospherically-forced sea-level variability in western Hudson Bay, Canada" by Dmitrenko et al.

The authors explore the relative contribution of dynamic atmospheric forcing and river discharge on variability of the sea level next to Churchill River. They also go beyond that and make some conclusions about the Hudson Bay.

The paper will be a great contribution to the field after some minor issues are solved. I find the analysis convincing and the text well written. My comments mainly concerns better description of the datasets, complying with the data policy of the journal and FAIR scientific practice, as well as better representation of information on figures.

Just one general note before the detailed comments. The authors jump from figure to figure in the text that makes it quite difficult to follow. I understand the necessity of coming back to the previous figures from time to time, but forcing the reader to do it so often is a bit cruel, in my opinion. I am not sure about the reasons to stick to the format when figures are inserted to the back of the manuscript. I think the OS format allows you to have them next to the text, which is much easier for reviewers.

Detailed comments:

88-89 "This is the only permanently operating tide gauge in Hudson Bay and the central Canadian Arctic." - I think you already stressed this enough in the introduction, I would remove this repetition.

98-108 There are two major points regarding satellite altimetry data that I miss: (i) There is no discussion of errors associated with the satellite measurements of sea level close to the coast, that is the main area of interest in this paper. It would be nice to at least mention it here, ideally provide the error estimates. (ii) It looks like there was substantial post processing involved in the altimetry data preparation for this paper. In order to comply with OS data policy (https://www.ocean-science.net/policies/data_policy.html) authors should provide the post-processed fields, and ideally the code that was used to generate them. If this is not possible, the explanation should be given in the data availability section. This is actually related to all data presented in the paper.

110-115 Again, please provide the extended time series, or explain why it's not possible.

118 Please indicate where exactly the NCEP reanalysis was downloaded from.

120 You use the ERA5 data after all (Fig.2), but this sentence makes me believe you disregard it. Please describe what ERA5 data was used, and where you have downloaded it from (or that you use it through Smith et al., 2014, as described below).

122-124 Please explain the advantages of manual cyclone tracking over using one of the automatic tracking algorithms that use objective criteria for identifying cyclones. I am not saying it's a bad approach, just a word of justification would be nice.

145 I am not really sure what you are trying to tell with your Figure 2a. First, the correlation of 0.96 is not surprising, as you correlate two time series that have seasonal cycles, it says little on how one is similar to another, just that they have seasons. If you want to do the correlation, I would at least do the 12 month running mean on the time serieses before. But even then, what is the purpose of validating NCEP vs ERA5? What is the reference in this case? If you think ERA5 is better, why not just use it in the rest of the study, as the data are available? Showing that two reanalyses agree or disagree with each other without comparison to observational data does not make sense to me. Authors should better articulate the purpose of the comparison, or maybe just delete Figure 2a and respective text.

Figure 2. Caption says "NCEP and ERA5 (1970-2000)", but it looks like ERA5 data only starts in 1979.

Figure 3. The sentence "91-day running mean of daily atmospheric vorticity index (red, s^{-1}) over Hudson Bay and daily mean sea level measured at the tide gauge in Churchill (blue, m)." is confusing, and reads as if the vorticity index is smoothed and sea level is not. The time series continues to be very noisy, while below you nicely work with 365-day running mean, that filters out the seasonal cycle completely. Please consider showing 365-day running mean for vorticity index and sea level in the upper panel, while keeping

the running correlation in the lower panel. I understand that your motivation for using 91-day running mean is to preserve the seasonal cycle to some extent, but this just does not work visually, we are looking at the noise.

Figure 4. Same comment as for Figure 3. I would still prefer to see a 365-day running mean, currently the upper panel does not convey any useful information to me, except that there is seasonal cycle in both discharge and SLA, but this is not worth a figure. The change after diversion will still be visible on the 365-day running mean.

186-189 Figure 5 is a much better illustration of seasonal cycle changes than Fig. 3 and 4a.

221-224 I was trying to find on the very noisy Figure 3a what you are talking about, but failed desperately. I understand that for the person who looks at these graphs long enough there is no problem to distinguish between "late fall and beginning of winter", but for the mere mortal that just sees these graphs for the first time it's just too much. Please, either highlight the periods you are talking about, or just find some other way to demonstrate them.

227-231 I am sorry, but you can't expect the reader to identify October-November on Figure 3a.

Figure 6 running mean lines are almost invisible, please make them thicker, or use more contrast colors.

314-321 It is unfortunate that the authors decide not to include analysis of thermo- and halosteric effects, as they might show interesting interplay between atmospheric forcing and ocean thermodynamics. I can understand that it might be too much for one paper, but it would be nice if the authors return to it in the future work.

443-445 Can you make any speculation on how possible changes in cyclone activity due to climate change may affect the sea level variability in the Bay? Or maybe 471-475 is a better place for it.