We highly appreciate helpful comments and suggestions by Reviewer #2. In the following, the comments by Reviewer #2 are underlined and our responses to the comments are in normal characters. Modifications to the text are shown in quotation marks with bold characters indicating newly added text, and normal characters indicating text that was already present in the previous version. The line numbering is referenced to the original manuscript version.

On behalf of the authors,

Igor Dmitrenko

Reviewer #2

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.

Overall comments:

1. 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.

We appreciate this favorable evaluation of our manuscript by Reviewer #2. The issues related to description and presentation of the datasets along with other minor points were resolved following specific comments by Reviewer #2. Particularly, we added new section describing data availability.

2. 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.

We appreciate Reviewer #2 for pointing this out. For future submissions, we will move figures directly to the text to make them easier to follow.

Detailed comments:

3. 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.

Omitted as recommended.

4. 98-108 There are two major points regarding satellite altimetry data that I miss:

4.1. 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.

Reviewer #2 is correct saying that errors in satellite altimetry data near the coast are greater than in the open ocean. This is due to land contamination within the radar footprint and to the fact that some geophysical corrections applied to altimetry data are usually optimized for the open ocean and not for the coastal zones. In the revised manuscript (newly introduced 2nd paragraph of Section 2.1: "***The root-mean-square differences between tide gauge records and collocated SLA/ADT data are usually 3-5 cm (e.g., Volkov et al., 2007; Pascual et al., 2009; Volkov et al., 2012) and do not exceed 10 cm globally (CLS-DOS, 2016). When the altimetry data are averaged to produce the seasonal climatology, the measurement error is greatly reduced (at least by an order of magnitude for 28 years of altimetry record). It should be noted that altimetry errors near the coast are greater than in the open ocean. This is due to land contamination within the radar footprint and to the fact that the geophysical corrections applied to altimetry data are usually optimized for the open ocean and not for the coastal zones. In classical altimetry products, however, a large percentage of data within 10–15 km from the coast is deemed invalid and not used for generating SLA/ADT maps (e.g., The Climate Change Initiative Coastal Sea Level Team, 2020). Furthermore, satellite altimetry data was used here only for a qualitative assessment of the basin-scale seasonal sea-level patterns in Hudson Bay. Therefore, the reduced quality of altimetry retrievals near the coast is not expected to impact the conclusions of this study. Sea ice also does not represent a significant problem for computing the climatology, because Hudson Bay is essentially ice free during these months, especially during SON***"), we have mentioned this problem and explained that in classical altimetry products, including the one we are using, a large portion of invalid data within 10–15 km from the coast is not used for generating SLA/ADT maps. We also point out that we use altimetry data only for a qualitative assessment of basin-scale seasonal sea-level patterns. Therefore, the degraded quality of altimetry data within ~15 km from the coast does not represent a problem and does not change the conclusions. As requested by both reviewers, we reference studies that performed quality assessments of satellite altimetry data by comparing it to tide gauge records worldwide (*Volkov et al*., 2007; *Pascual et al*., 2009; *Volkov et al*., 2012). As shown in these studies, the root-mean-square differences between tide gauge records and collocated SLA/ADT data are usually 3-5 cm and less than 10 cm (see also Validation of altimeter data by comparison with tide gauge measurements: yearly report 2016, https://www.aviso.altimetry.fr/fileadmin/documents/calval/validation_report/annual_report_TG_2016.pdf). Furthermore, in the revised manuscript, we also point out that averaging altimetry data over the two selected seasons reduces the measurement error at least by an order of magnitude.

<u>4.2. 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.</u>

No substantial post processing of altimetry data was involved in this study. The daily SLA/ADT maps with all corrections applied are distributed via CMEMS (https://marine.copernicus.eu/). As described in the text, we only (i) computed the global mean sea-level, (ii) subtracted it from each grid point in Hudson Bay, and (iii) computed the seasonal climatology by averaging all available maps for each season. We added this statement in new section ***Data availability.***

<u>5. 110-115 Again, please provide the extended time series, or explain why it's not possible.</u>

The time series of the Churchill River discharge is added to supplementary material. They are referenced in lines 186-188 as follows: "*Churchill River discharge data were obtained from Déry et al. (2016) and extended to 2019; thus, we use a continuous record of daily mean discharge from 1960 to 2019 (Figure 4a* ***and supplementary material****)*". We also added this statement in new section ***Data availability***.

6. 118 Please indicate where exactly the NCEP reanalysis was downloaded from.

We updated text with this information in lines 170-171: *"…were derived from the NCEP atmospheric reanalysis (****https://psl.noaa.gov/data/composites/hour/…***". We also added this statement in new section ***Data availability***.

7. 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).

The reason for involving ERA5 is provided in lines 142-145: "*We also conducted a validation comparing the NCEP-derived vorticity to that derived from the ERA5 SLP utilizing the Web-Based Reanalysis Intercomparison Tools* ***(https://psl.noaa.gov/cgi-bin/data/testdap/timeseries.pl)*** *described by Smith et al. (2014). The comparison showed insignificant differences between the two reanalyses: the NCEP-derived vorticity only slightly exceeds that obtained from ERA5, while the correlation between the NCEP and ERA5-derived vorticities is 0.96 (Figure 2a)*". In this sentence we added the data source for the ERA5 data. We also added information on the ERA5 SLP data following line 122: "***However, we used the ERA5 SLP data to validate atmospheric vorticity derived from NCEP as described below in section 3***".

8. 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.

We used cyclone manual tracking for simplicity. We added this information in section 2.3: "***For simplicity,*** *cyclones over the Hudson Bay area were manually tracked* ***for August-May 1969-1970 and 2003-2004****…*". In this research, we used cyclone tracking only for August-May 1969-1970 and 2003-2004 (Figure 7). For these short time periods, it seems to be unnecessary to involve the automatic tracking algorithms. In the upcoming research focused on climatic forcing of the Hudson Bay circulation, we do this automatically following *Crawford et al*. (2021, doi: 10.1175/mwr-d-20-0417.1).

9. 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.

In general, ERA5 is better due to higher spatial resolution. However, "*We chose the NCEP reanalysis to extend the atmospheric forcing data back to 1950, which covers the tide gauge record from Churchill…*", lines 119-120. In this context, Figure 2a aimed to show that NCEP derived vorticity is very comparable to vorticity obtained using ERA5. We added this motivation following line 122: “***However, we used the ERA5 SLP data to validate atmospheric vorticity derived from NCEP as described below in section 3***”.

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

Yes, Reviewer #2 is correct. We changed last sentence of Figure 2 caption to “…*the monthly mean vorticity derived from NCEP **(1949-2000)** and ERA5 **(1979-2000)** and (b) the monthly mean NCEP vorticity **versus** meridional wind (1949-2020)*”.

11. 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.

The 91-day running mean was applied to both (i) the daily atmospheric vorticity and (ii) daily mean sea level measured at the tide gauge in Churchill. We modified the Figure 3 caption to make this clearer. As for the second part of this comment, we do not agree with Reviewer #2. As pointed out by Reviewer #2, our motivation for using 91-day running mean in Figure 3a was to preserve the seasonal cycle and its interannual variability. Figure 3b shows correlations computed between vorticity and SLA computed for the 365-day moving window. This correlation is mainly controlled by coherence of the seasonal cycles in vorticity and SLA shown in Figure 3a. In this context, gray shading highlighted the periods when correlations between vorticity and SLA was disrupted because there was no coherence between vorticity and SLA in Figure 3a. In contrast, yellow shading highlighted the periods when vorticity and SLA are correlated due to the coherent seasonal cycling in vorticity and SLA revealed in Figure 3a. That is why using 91-day running mean in Figure 3a is important for explaining patterns of correlations shown in Figure 3b.

12. 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.

Similar to comment #11 by Reviewer #2, the 91-day running mean in Figure 4a is intended to show the seasonal cycling and its interannual variability. Figure 4a provides physical background for explaining correlations in Figure 4b.

13. 186-189 Figure 5 is a much better illustration of seasonal cycle changes than Fig. 3 and 4a.

The text referenced by Reviewer #2 is “*The long-term variability of sea level (Figure 3a) and SLA (Figure 4a) shows no abrupt disruption with the introduction of the Churchill River diversion in 1977. However, the seasonal cycle of SLA generated for pre- and post-diversion shows a characteristic difference in the timing and magnitude of SLA (Figure 5a)*”. Here Figures 3a and 4a intended to show no abrupt disruption in sea level associated with the introduction of the Churchill River diversion in 1977. However, Figure 5 definitely shows changes in the seasonal cycle. This is exactly in line with this comment by Reviewer #2.

However, Figure 5 does not show the interannual variability of seasonal cycle. This information can only be derived from Figures 3a and 4a.

14. 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.

We agree with this critics by Reviewer #2. In the revised version of our manuscript, we indicated these periods of diminished and enhanced seasonal vorticity in Figure 3a by black and green triangles, respectively. The corresponding information was added in lines 223 and 225. New sentence was also added to Figure 3 caption: "***Black and green triangles show periods when seasonal vorticity from late fall to early winter was diminished and amplified, respectively***".

15. 227-231 I am sorry, but you can't expect the reader to identify October-November on Figure 3a.

In this context, reference to October-November 1969 and 2003 was associated with Figure 6, not Figure 3a. In Figure 3a, the entire periods from August 1969/2003 to May 1970/2004 were highlighted with yellow shading. In Figure 6, these periods were enlarged.

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

We made these lines in Figure 6 thicker.

17. 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.

We appreciate Reviewer #2 for this comment. Analysis of thermo- and halosteric effects requires numerical simulations. While Baysys project includes NEMO simulations (U of Alberta, group led by Paul Myers), they were not used for this specific research. This would be a separate topic for future research.

18. 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.

As follows from Figure 3a, atmospheric vorticity over Hudson Bay does not show a trend that can be attributed to climate change. Moreover, while working on the manuscript focused on climate forcing of the Hudson Bay circulation, we revealed teleconnections between vorticity and El Niño and La Niña events. That is why the issue pointed out by Reviewer #2 is not clearly understood, and at the moment, we would like to avoid speculations on this subject. Following this comment, we introduced new sentence to finalize our summary and concussions: "***Possible impacts of climate change on cyclone activity in Hudson Bay, and therefore sea-level variability, will be addressed in future research***".