We highly appreciate helpful comments and suggestions by Reviewer #1. In the following, the comments by Reviewer #1 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 version of our manuscript.

On behalf of the authors,

Igor A. Dmitrenko

**Reviewer #1**

Overall comments:

1. With some clarifications, this is a useful contribution to the scientific body of knowledge surrounding processes affecting water levels in Hudson Bay, and provides new insight that atmospheric conditions play a more significant role in influencing water levels in this water body than previously thought. The piece is generally well-written, although could use a thorough final review to address a few minor grammatical, punctuation and sentence structure issues. I have noted a few examples in my specific comments where improvements could be made.

We appreciate this favorable evaluation of our manuscript by Reviewer #1.

2. My overall impression is that the scientific analysis has been rigorous, although I have asked for a few clarifications on certain aspects in my specific comments, particularly surrounding the approach to inverse barometer correction, temporal averaging and de-trending of tide gauge data, importance of Ekman transport versus "conventional" storm surge generation processes, interpretation of satellite altimetry data and associated uncertainty, and the discussion of how sea ice affects SLA. With respect to the latter (role of sea ice), more nuance is needed, and the paper could benefit from referencing previous studies that have studied the role of sea ice conditions on atmosphere-sea momentum transfer and storm surges.

All these suggestions by Reviewer #1 were implemented in the revised version of manuscript.

3. The quality of figures is generally quite good, with a few needs for additional annotation as identified in specific comments below. In general, the plots are a bit crowded (particularly Figures 3 and 4, which could use more separation between the upper and lower panels).

We implemented more space between the upper and lower panels in Figures 3 and 4.

Specific comments:

4. Lines 16 and 18: suggest to check throughout for consistency in use of "sea-level" versus "sea level". Conventionally, a hyphen is only for use as a compound adjective, as in "sea-level rise". In this context, I would expect a reversal of the hyphen usage on lines 16 and 18, i.e. "variability of sea level" and "sea-level variability".

Modified as recommended.

5. Line 38: Multiple uses of "cyclonic". Suggest to delete the first instance. In fact, I find the use of "cyclonic" is generally excessive throughout the paper, suggest to review and decide if really needed in all instances.

We agree with this comment by Reviewer #1. We modified this sentence (line 38) as follows: "*Prevailing* ***along-shore*** *winds drive cyclonic circulation of water within the Bay*…". We also omitted "*cyclonic*" in several places below.

6. Lines 41-42: "even during the ice covered season strong cyclones can amplify cyclonic water circulation in the Bay". This seems to imply there is cyclonic circulation even in the absence of cyclones? Is it more correct to say that during periods of partial ice cover, the increased surface roughness and form drag imparted by ice floes to the water during strong cyclones can amplify cyclonic circulation compared to similar events coinciding with open-water conditions. Discussion of the role of sea ice regime on atmospheric momentum transfer to the water could benefit from referencing previous research in this area, e.g.:

Lüpkes, C.; Gryanik, V.M.; Hartmann, J.; Andreas, E.L. (2012) A parametrization, based on sea ice morphology, of the neutral atmospheric drag coefficients for weather prediction and climate models. J. Geophys. Res. Space Phys., 117.

Lüpkes, C. et al. (2013) Effect of sea ice morphology during Arctic summer on atmospheric drag coefficients used in climate models, JGR

Tsamados et al. (2014) Impact of Variable Atmospheric and Oceanic Form Drag on Simulations of Arctic Sea Ice, American Meteorological Society

Lupkes, C. et al. (2012) A parametrization, based on sea ice morphology, of the neutral atmospheric drag coefficients for weather prediction and climate models, JGR

Andreas, E. et al. (2010) Parametrizing turbulent exchange over summer sea ice and the marginal ice zone

Lu, P. et al. (2011) A parameterization of the ice-ocean drag coefficient, JGR.

Shirasawa, K., Graham, R. (1991) Characteristics of the turbulent oceanic boundary layer under sea ice. Part 1: A review of the ice-ocean boundary layer, Journal of Marine Systems

Hunke, E., and Dukowicz, J. (2002) The sea ice momentum equation in the free drift regime. Technical Report LA-UR-03-2219, Los Alamos National Laboratory, NM.

Joyce, B.R.; Pringle,W.J.;Wirasaet, D.;Westerink, J.J.; Van DerWesthuysen, A.J.; Grumbine, R.; Feyen, J. High resolution modeling of western Alaskan tides and storm surge under varying sea ice conditions. Ocean Model. 2019, 141, 101421.

Kim, J.; Murphy, E.; Nistor, I.; Ferguson, S.; Provan, M. Numerical Analysis of Storm Surges on Canada's Western Arctic Coastline. J. Mar. Sci. Eng. 2021, 9, 326.

Reviewer #1 correctly interpreted our text "*even during the ice covered season strong cyclones can amplify cyclonic water circulation in the Bay*". The Hudson Bay cyclonic circulation is observed even in the absence of cyclonic atmospheric forcing. ***The mean circulation in Hudson Bay is comprised of the***

***wind-driven and estuarine components, where the estuarine portion is driven by the riverine water input (Prinsenberg, 1986a), and the wind-driven portion is attributed to*** *prevailing along-shore winds (e.g., Ingram and Prinsenberg, 1998; Saucier et al., 2004; St-Laurent et al., 2011; Ridenour et al., 2019a; Dmitrenko et al., 2020)*. We added this text after line 37. Following suggestion by Reviewer #1, we also briefly introduced the role of sea ice on atmospheric momentum transfer to the water adding new text following line 44: "***The efficiency of momentum transmission through the mobile ice strongly depends on sea-ice roughness, which is impacted by ice concentration and characteristic length scales of roughness elements including pressure ridges, melt ponds etc. (e.g., Lüpkes et al., 2012; Tsamados et al., 2014; Joyce et al., 2019). In particular, ice floes in a state of free drift within a partial or weak ice cover, typical of the polynya area in western Hudson Bay, increase the transfer of wind stress into the water column (Schulze and Pickart, 2012)***". Reference list was updated accordingly.

7. Lines 85-86: I believe that water level data is available for the Churchill gauge at sub-daily (even sub-hourly) intervals. It would be helpful to comment on the implications of choosing to use the daily mean water level as the basis for evaluating SLA, given (i) a major focus of the study is on understanding the role of wind in contributing to storm surges, which are likely to manifest on time scales of the order of hours rather than days and (ii) likely disparities in SLA response time scales from wind and river discharge contributions, with the latter being more likely on the order of days to weeks.

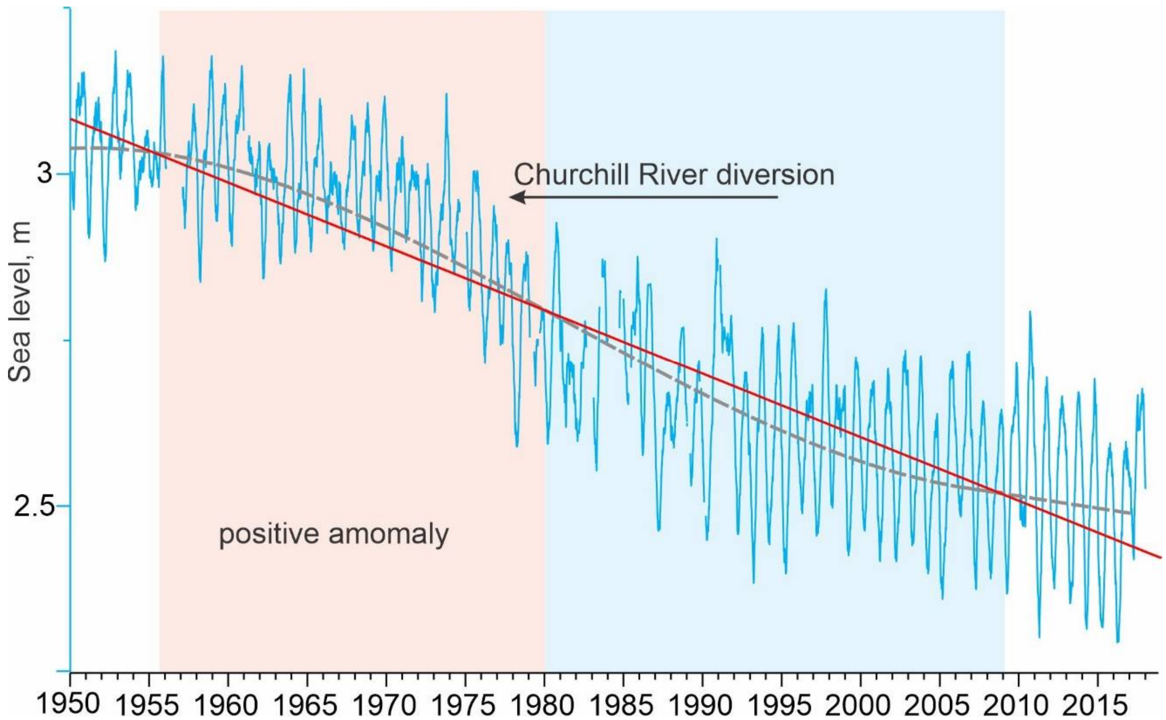
For the majority of tidal gauge data from 1950s, sea level at Churchill was recorded hourly. In contrast, the Churchill River discharge data from gauged observations above Red Head Rapids (station #06FD001) are available at daily basis. The NCEP data on SLP and 10-m wind are available at 6-h intervals. To make these three time series comparable, we decided to analyze them as the daily mean data. We added this statement following line 108 at the end of the data section: "***For the majority of tidal gauge data from 1950s, sea level at Churchill was recorded hourly. In contrast, the Churchill River discharge from gauged observations above Red Head Rapids is available daily. The NCEP data on SLP and 10-m wind are available at 6-h intervals. To make these three time series comparable, we analyzed daily means***".

8. Lines 113-114: The Déry et al. 2016 reference provided here refers the reader to another (2005) reference for details of the approach. I would suggest to directly reference the 2005 work, and provide quantitative values of parameters used in the drainage area correction to give the reader a feel for the proportion of total watershed for which this approach is used.

Reviewer #1 is correct. We changed this reference to *Déry et al*. (2005). However, providing details of the draining area corrections, particularly quantitative values of parameters used for the drainage area correction, seems to be excessive in this context.

9. Line 171: The basis for the selection of polynomial fit to de-trend the data should be more clearly explained or justified, as well as the potential influence on the analysis as a whole. For example, the inflection point in the polynomial fit shown in Figure 3 coincidentally aligns quite closely on the time axis with the Churchill River Diversion. Is it possible that this de-trending method approach might de-emphasize the importance of Churchill River discharge contributions to SLA compared to, say, a linear de-trending approach? Could the selected approach also obscure other inter/intra-decadal influences on water level such as climate variability patterns? Are the correlations shown in Tables 1-3 sensitive to the choice of de-trending method?

We generated figure (below) showing sea level (90-day running mean) and its linear and polynomial approximations. Figure shows that the linier fit slightly underestimates the sea level values before diversion and overestimates them after diversion. In this research, however, we are mainly interested in the sea level seasonal and synoptic variability. Thus, we are not focused on inter/intra-decadal influences on water level such as climate variability patterns. Because the sea-level time series is dominated by seasonal and synoptic variability, the correlations shown in Tables 1-3 are not sensitive to the choice of de-trending method.




For the linear approximation, the coefficient of determination ($R^2$) is 0.39. For the polynomial approximation $R^2$ = 0.41. This confirms that the polynomial approximation provides a better fit for the long-term trend attributed to the post-glacial isostatic adjustment. This is also obvious from the figure enclosed. We added this information following line 171: "***The polynomial fit better explains long-term variability of sea level at Churchill compared to the linear approximation, with respective coefficients of determination (R***$^2$***) of 0.41 and 39***". *Thus, in our study we examined the sea level anomalies (SLA) against the low-frequency trend conditioned by the post-glacial isostatic adjustment* (lines 171-173).

10. Lines 172-173: It is not clear if the inverse barometer correction referred to here is a correction applied to the tide gauge record (in which case it should be clarified that the raw data represented measurements using an unvented tide gauge transducer) or simply a removal of the inverse barometer (IB) contribution to the water level record. If the latter, it would be worth commenting on the magnitude of the contribution of IB to interannual SLA, given studies indicating it is often non-negligible (e.g. Piecuch & Ponte 2015) and the potentially significant correlation with vorticity/wind effects.

We clarified our approach revising text in lines 172-173: "***In addition,*** *the inverse barometer contribution to the water level record was removed* ***using sea-level atmospheric pressure from the NCEP reanalysis***". Following this sentence, we added information on the magnitude of correction attributed to IB: "***The mean correction attributed to inverted barometer effect was –1.19 ± 8.72 cm***". For sure, IB correction is correlated to cyclonic wind and vorticity. Overall, correlation between vorticity and SLA was reduced by ~0.02-0.06 after removing IB. However, this contribution is not directly related to the ocean dynamics, and after discussing this point with co-authors it has been decided to remove the IB contribution.

11. Lines 299-301: There have likely been changes in sea ice cover over the reference period. How is this reflected in the analysis, if at all? Given the dominance of wind effects under ice-free conditions, perhaps there is value in discussing potential future changes if, as expected, ice cover continues to decline.

Thank you for this question. Since the ice-covered and ice-free periods in our analysis fixed to December-May and June-November, we cannot elaborate how changes in sea ice cover impact the results of our correlation analysis. *Andrews al*. (2017) revealed no trend for the open water period at Churchill for 1996-2016. For the entire Hudson Bay, however, the mean shifts toward a longer open water season (1980–1995 vs. 1996–2010) of ∼3 weeks was revealed by *Hochheim and Barber* (2014). For pointing out potential future changes associated with ice cover decline, we added new sentence following line 456: "***In this context, transition towards a longer open water season (e.g., Hochheim and Barber, 2014) is expected to increase the contribution of atmospheric forcing to sea level variability***".

12. Line 308: Is it correct to say that sea level variability at Churchill is primarily impacted by wind forcing, when the authors state that wind forcing only explains ~22% of variability (Line 290)?

We modified this sentence to address concern by Reviewer #1 as follows: "*Our results show that sea level variability at Churchill is* ***rather influenced*** *by wind forcing…*"

13. Lines 310-312: Is there evidence to support the hypothesis that the dominant driver of storm surge on the western shore is Ekman transport, as opposed to a direct response of the water level to balance wind stress acting on the surface, or local bathymetric influences on storm surges? Although Figure 2b shows a strong correlation between the northerly wind component and vorticity, it would be helpful to more clearly explain whether these conditions are typically associated with an onshore (easterly) wind component or not, to confirm the relative importance of Ekman transport versus wind stress.

As pointed out by Reviewer #1, positive atmospheric vorticity over western Hudson Bay is associated with northerly winds (Figure 2b). Taking into account correlation between vorticity and SLA at Churchill, one may conclude that storm surge on the western shore is strongly impacted by Ekman transport. Moreover, there is no correlation between SLA and zonal wind ($R = -0.03$). The local bathymetry along the western shore is rather gentle, with depth gradients of ~2.5 m $km^{-1}$ (Figure 1). Following this comment by Reviewer #1, we added new sentence following line 313: "***A direct response of the water level to balance wind stress acting on the surface does not play a role for generating SLA because there is no correlation between SLA and zonal wind (not shown)***".

14. Lines 318-319: "It seems that these factors can explain the residual fraction of the SLA seasonal variability that is not explained by wind forcing and local river discharge." If I understand correctly, the authors concluded (Line 292) that wind effects and river discharge explain ~28% of SLA variability. The authors are therefore hypothesizing that the remainder, which at ~72% would represent a substantial (even dominant) contribution, is explained by thermosteric and halosteric effects. Is it then appropriate to refer to these as the "residual fraction"?

Following this comment, we changed "*residual fraction*" to "***significant** fraction*".

15. Lines 357-360: The vorticity threshold described appears to imply that sea ice conditions (i.e., concentration, thickness, roughness, mobility as opposed to simply presence/absence) have little or no influence on the role of wind forcing in explaining SLA variability. I would expect ice conditions (e.g., presence or absence of shorefast ice and/or dynamic ice, roughness, concentration, fraction of Hudson Bay covered by ice) to play a role in determining the thresholds at which wind influences (positively or negatively) the SLA.

We agree with this comment. That is why we identify this threshold as "*a very rough estimate of the vorticity threshold attributed to the sea-ice impact*", lines 357-358. To address concern by Reviewer #1, we added new sentence following line 360: "***In fact, extension of the landfast ice as well as sea-ice roughness and concentration can play a role modifying the thresholds at which wind impacts the SLA***".

16. Lines 377-378 and 385: Which hypothesis by Gough and Robinson (2000) and Gough et al. (2005) are the authors referring to in this section? The preceding paragraph identifies multiple distinct hypotheses, and the linkage is not clear. Both paragraphs would benefit from more clearly identifying which of G&R (2000) and G et al (2005) hypotheses are debunked, and which are simply subject to uncertainty.

First, we modified these sentences to clarify which hypothesis we are referring to in this section: "*Overall, the hypothesis by Gough and Robinson (2000) and Gough et al. (2005)* ***about the linkage between the river discharge pulse into James Bay and a positive SLA in Churchill*** *is suggestive of the seasonal disruption of the Hudson Bay cyclonic circulation…*" and following line 385: "*In this context, the hypothesis by Gough and Robinson (2000) and Gough et al. (2005)* ***linking SLA in Churchill linking SLA in Churchill to river discharge in James Bay*** *seems to be inconsistent*".

Second, actually, there is only one hypothesis by *Gough and Robinson* (2000) and *Gough et al*. (2005) explaining a positive SLA observed in Churchill from October-November by the river discharge pulse into the James Bay region with an advective lag of ~4-5 months. This hypothesis was discussed and criticized in lines 377-387 and 400-424. We added new sentence summarizing the second portion of this critics following line 424: "***Overall, our third and fourth points suggest that the hypothesis of Gough and Robinson (2000) and Gough et al. (2005) about a linkage between river discharge into James Bay and SLA in Churchill is inconsistent***".

Third, *Gough and Robinson* (2000) and *Gough et al*. (2005) reported high correlations between Churchill River discharge and SLA in Churchill. We called these correlations into question in lines 388-399. We added new sentences to point this out following line 393: "***This calls into question the correlations between Churchill River discharge and SLA in Churchill reported by Gough and Robinson (2000) and Gough et al. (2005)***" and following line 399: "***This is consistent with a previous concern about significant impact of Churchill River discharge on SLA in Churchill***".

17. 1. Lines 417-418 (and 98-108): Figure 10 shows continuous contours of SSH derived from satellite altimetry data, which are discrete measurements at discrete intervals, usually along widely space satellite tracks.

The pattern shown in Figure 10 is used for qualitative purposes only. While the original altimetry measurements are along the satellite ground tracks, in this manuscript we used a gridded altimetry product. This product is obtained by optimally interpolating measurements from several (at least 2) satellites. The procedure has been widely used since the beginning of 2000s (e.g., *Ducet et al*., 2000, doi: 10.1029/2000JC900063). The details of the methodology can be found in *Pujol et al*. (2016, doi: 0.5194/os-12-1067-2016) and references therein.

<u>17.2. Altimetry data is also notoriously uncertain near the land/water interface. It would be helpful to provide some additional context on the number of observations, spatial and temporal intervals, how the contours were developed, and the uncertainty associated with the SSH measurements (considering absolute differences shown are no more than +/- 5cm). To what extent is the satellite altimetry analysis influenced by spatial and temporal resolution of data and uncertainty in interpreted sea level measurements in proximity to the land/water interface?</u>

Indeed, the quality of satellite altimetry data is compromised within 10-15 km offshore. Therefore, the gridded altimetry products usually discard radar measurements near the coast, for which specific processing is required. Note that disregarding the 10-15 km coastal band in Figure 10 does not change the general pattern and the interpretation of the result.

<u>17.3. It would be helpful to provide some additional context on the number of observations, spatial and temporal intervals, how the contours were developed, and the uncertainty associated with the SSH measurements (considering absolute differences shown are no more than +/- 5cm). To what extent is the satellite altimetry analysis influenced by spatial and temporal resolution of data and uncertainty in interpreted sea level measurements in proximity to the land/water interface?</u>

The gridded satellite altimetry data has been widely used for more than 2 decades. The mapping procedure has been described in many papers (see *Pujol et al*., 2016 and references therein). While additional details on the procedure can be found in references, we do mention in the revised version of the manuscript (2nd paragraph of Section 2.1) that the root-mean-square differences between tide gauge records and collocated SLA/ADT data are 3-5 cm globally (e.g*., Volkov et al*., 2007; *Pascual et al*., 2009; *Volkov et al*., 2012): "***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***". The actual accuracy of the altimetry data is even less than this range of values, because of the uncertainties in tide gauge records. Furthermore, when we compute the seasonal climatology, then the accuracy of a single altimetry measurement improves by at least an order of magnitude (given that there are 28 years and 9 independent measurements in a season). We also mention that sea-ice does not represent a problem for computing the climatology, because Hudson Bay is essentially ice free during the months considered. Therefore, the pattern shown in Figure 10 is robust.

18. Lines 439-440: To what extent might anomalous inflows/outflows be controlled by the spatial distribution of sea ice (also potentially influenced by prevailing wind conditions)?

In this text referred by Reviewer #1 we make a reference to *Piecuch and Ponte* (2015). They found that sea level variability in Hudson Bay is driven by wind stress over Hudson Strait. We assume that in this context, anomalous inflows/outflows will be controlled by interplay between the spatial distribution of sea ice and wind forcing. However, given the lack of an interactive sea-ice model, *Piecuch and Ponte* (2015) did not simulate any role played by sea ice in mediating the transfer of momentum between the atmosphere and the ocean.

19. Lines 450-451: Odd use of "While…" to begin the sentence. Perhaps replace with "On the contrary".

We omitted "*While*" starting this sentence with "*Cyclonic atmospheric forcing…*".

20. Lines 466-470: I'm not sure the argument regarding the link between geostrophic flow and SLA is sufficiently put to rest without some analysis of the shore-perpendicular component of wind.

Following this concern by Reviewer #1, we lessened this statement to "*This seasonal pattern in sea-level variability* ***seems to have*** *implication for geostrophic circulation*".

21. Lines 760-765: Symbols (concentric circles and crosses) require legend or definition within caption.

We added this information in the Figure 11 caption: "***Dotted and crossed circles depict southerly and northerly along-shore surface winds, respectively***".