Title: Atmospherically-forced sea-level variability in western Hudson Bay, Canada
Author(s): Igor Dmitrenko et al.
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Referee comments:

Overall comments:

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.

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.

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).

Specific comments:

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".

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.

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.

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.

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.

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?

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.

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.

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)?

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.

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”?

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.

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.

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. 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?

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)?

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

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.

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