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Interactive comment on “A wind-driven nonseasonal barotropic fluctuation of the Canadian Inland Seas” by C. G. Piecuch and R. M. Ponte

Anonymous Referee #2

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General comments

The authors examine the non-seasonal variability of sea level in Hudson Bay and adjacent coastal seas using ocean bottom pressure estimates derived from GRACE satellite gravity data, sea level records from the Churchill tide gauge, and a simple barotropic ocean circulation model. They show that the model does a reasonable job of reproducing the observed sea level variability, and then use the model to identify the leading mode of non-seasonal variability, and correlations with the NAO, wind forcing in Hudson Strait, and wind forcing in the wider North Atlantic. Using a number of model runs they propose mechanisms accounting for the observed sea level variability.

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[Interactive Discussion](#)

[Discussion Paper](#)



The paper is generally well written, and builds on work presented by the authors in their earlier GRL paper. The study represents a novel examination of shelf sea level using GRACE data.

I have a couple of reservations about the methods employed in the paper. I don't think that the manuscript needs major reworking, but I feel that these points need to be addressed in more detail.

- Because of the coarse resolution of the GRACE data, estimates of ocean bottom pressure are subject to leakage of the terrestrial signal. The authors address this point, but I believe that it warrants further discussion. Chambers & Bonin (2012) suggest that, even with their leakage correction, there is still some leakage in coastal areas. The authors show that, averaged across the domain, there is no correlation between the terrestrial and oceanic signals. However I do not believe that this is conclusive - the terrestrial signal may be dominant in some parts of the oceanic domain, but not others. I feel that more needs to be done to show that this isn't a problem.
- The ocean model that is used is of a relatively coarse horizontal resolution and does not include a sea ice component. By the author's admission, it does not represent all processes. The only validation of the model is a comparison of the OBP/sea level with the GRACE estimates and the tide gauge record. I appreciate that this simple model is useful for the experiments described later in the paper, but I would be more comfortable seeing more extensive validation of the model results.

Specific comments

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11, C1042–C1046, 2014

[Interactive
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[Discussion Paper](#)



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Comment](#)

p2338, lines 20-24: Representative values of the layer thicknesses would be helpful.

p2338, line 26: The phrase “a net flux of water mass across the sea surface...” is rather confusing. I would also like to see mention of the inflow from Baffin Bay in this section.

p2339, lines 24-26: It should be noted that the bathymetry is also poorly sampled, with large uncertainties (especially in the northern parts of the CIS).

p2342, lines 11-12: I note that removing the global MSL and IB reduces variance, but is it really appropriate to remove the global altimeter (i.e. deep ocean) signal? It seems unnecessary, because the linear trend is removed anyway.

p2342, lines 12-13: How is the inverted barometer response calculated?

p2343, lines 1-8: As noted in the general comments, I’m concerned that there is still some leakage of the terrestrial signal, and I would like to see a more detailed justification that leakage isn’t a problem.

p2343, lines 9-11: Is there any correlation/coherence between the terrestrial time series and the tide gauge sea level? One would hope not, but it would be good to know.

p2344, lines 19-23: It is likely that ETOPO5 bathymetry has large uncertainties in this region, because of sparse measurements. This should be noted.

p2344: As noted in the general comments, I have concerns about the simple model

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framework. Why not include some comparisons with one of the publicly-available higher-resolution ocean-sea ice models?

p2346, line 24: I find the phrase “leading” more widely understood than “gravest”.

p2348, line 7: The leading mode is correlated with the NAO, rather than “related” to it. “Related” infers more of a causal linkage. Similarly on line 21.

p2349: Are all of the later model runs performed for the same time period as the baseline run?

p2349, lines 25-29: What we don’t know is which of the regions are important. Wind forcing is coherent across large areas, resulting in significant correlations between CIS sea level and wind forcing for a large part of the North Atlantic. It might be wind forcing in just one small area that is important, even though the correlation is much wider.

p2350, lines 4-8: Whilst these mechanisms are possible, there is nothing in the study to indicate that they are at play here. I think that it should be clearly stated that this is speculation.

p2351, line 4: “Correlated” with the NAO, not “related” to the NAO.

Technical corrections

p2361, figure 4 caption: Penultimate line should be as follows (additional words in bold)
- “...The NAO time series is taken **from the...**”

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