Ocean Sci. Discuss., 11, C1144–C1152, 2014
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Ocean Science
Discussions
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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

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Received and published: 10 December 2014

Response to interactive comment on "A wind-driven nonseasonal barotropic fluctuation of the Canadian Inland Seas" by Anonymous Referee #2

GENERAL COMMENTS

Reviewer Comment (RC): 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. 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.

RC: 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.

Author Response (AR): We appreciate your thoughtful comments on and assessment of our manuscript. The manuscript will be revised based on your comments as well as those of the other reviewer. More immediately, you will find a detailed, point-by-point response to all of your comments herein below.

RC: 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.

AR: This is a warranted concern. Our comparison between nonseasonal GRACE ocean bottom pressure averaged over the Canadian Inland Seas and GRACE terrestrial water storage averaged over the watershed draining into them is motivated by Peralta-Ferriz et al. (2014, "Arctic Ocean Circulation Patterns Revealed by GRACE", JCLI, 27, 1445-1468), who use an analogous technique to test for land leakage in bottom pressure over the Kara and Barents Seas (a point that we will make clearer in the

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manuscript revision). But to probe more deeply and address the reviewer's concern, we computed correlations between the averaged terrestrial water storage signal and pointwise bottom pressure at each GRACE ocean grid cell. The results (not shown) evidence something of a spatial gradient: coefficients are ∼0.1-0.3 over Hudson and James Bays while they are ∼0.3-0.4 over Foxe Basin, suggesting that the GRACE data over the latter basin might be more noisy. However, none of the correlation coefficients were found to be statistically significant at the 95% confidence level, and therefore we conclude that our suggestion that the GRACE ocean bottom pressure fields are not overwhelmed by land leakage is robust. These points will be stated in abbreviated form in the manuscript revision.

RC: 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.

AR: Following the reviewer's suggestion (below), our simple barotropic model is vetted more fully in the manuscript revision by bringing in output from a higher-resolution ocean-sea ice model. Specifically, we consider bottom pressure and sea level* from the Estimating the Circulation and Climate of the Ocean Phase-II (ECCO2) cube92 solution (available at ftp://ecco2.jpl.nasa.gov/data1/cube/cube92/lat_lon/quart_90S_90N/), which boasts higher resolution, inclusion of sea-ice dynamics, as well as forcing by surface heat and freshwater fluxes, among other factors, and which covers through 2012. Similar ECCO2 solutions have been used in recent studies of high-latitude GRACE ocean bottom pressure behavior (Peralta-Ferriz et al. 2014; Volkov and Landerer, "Nonseasonal fluctuations of the Arctic Ocean mass observed by the GRACE satellites", 2013, JGR-O, 118, 6451-6460). (*Note that, in the presence of sea ice, "sea level" is here defined as the physical depression of the sea surface itself along with any

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sea-ice mass loading in equivalent water thickness.)

AR: Overall, for the common period 2003-2012, we find that our simple barotropic model performs as well as (if not better than) the ECCO2 solution in reproducing the observations. For example, we altered Fig. 3 in the original manuscript to now include bottom pressure averaged over the region and sea level closest to Churchill from ECCO2 (see attached file). The barotropic model better approximates the phase and amplitude of the observations than does the ECCO2 solution: while ECCO2 explains 36% of the fractional variance in the averaged GRACE bottom pressure time series, the barotropic model explains 50% of the variance; the barotropic model explains 58% of the variance in the tide gauge time series, whereas ECCO2 explains 54%. This altered figure will appear in the manuscript revision.

AR: Although we prefer not to add addition figures to the paper, other comparisons between ECCO2 and the barotropic model were also carried out. For example, we linearly interpolated the ECCO2 fields onto the barotropic model grid, and then computed correlation coefficients between nonseasonal sea level/bottom pressure from the barotropic simulation and either bottom pressure or sea level from ECCO2 (not shown). (Note that, in contrast to our barotropic model, sea level and bottom pressure are not generally equivalent in ECCO2, due to the inclusion of ocean stratification.) We find statistically significant correlation coefficients between nonseasonal sea level/bottom pressure from the barotropic simulation and both bottom pressure and sea level from ECCO2 over the entirety of the Canadian Inland Seas, demonstrating that, despite different levels of complexity in terms of model physics, the estimates are comparable, signaling the predominance of barotropic dynamics in the region for the time scales of interest. Interestingly, we found that sea level from the barotropic model and ECCO2 were not significantly correlated over much of Hudson Strait, which could be indicative of variations in the important baroclinic boundary current in this region (cf. Straneo and Saucier, "The outflow from Hudson Strait and its contribution to the Labrador Current", 2008, DSR I, 55, 926-946).

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AR: These results give us confidence that our simple model is adequate to the task of explaining the lowest-order observed behavior. More details on the ECCO2 solution and comparison against the data and our barotropic model will appear in condensed form in the manuscript revision.

SPECIFIC COMMENTS

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

AR: Representative values of the layer thickness will be given in the revised manuscript.

RC: 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.

AR: In the revised manuscript, we will attempt to clarify this phrase, and also give mention to the inflow from Baffin Bay.

RC: 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).

AR: This is a very good point that will be mentioned in the revised manuscript.

RC: 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.

AR: We remove the global altimeter signal from the tide gauge series partly to be consistent with the other datasets considered in this study, which have had their respective global means effectively removed. While most of the variance in the global mean sea level curve is indeed explained by the linear trend and seasonal cycle (which are not the focus of study here), there can be important anomalies thereupon, for example, as discussed by Boening et al. (2012, "The 2011 La Nina: So strong, the oceans fell",

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GRL, 39, L19602). For these reasons, we prefer to still remove the global signal from the tide gauge.

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

AR: We compute the inverted barometer response following Equation (1) in Ponte (2006, "Low-Frequency Sea Level Variability and the Inverted Barometer Effect", JTECH, 23, 619-629). We will make this point clear in the manuscript revision.

RC: 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.

AR: See our response above to the general comment.

RC: 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.

AR: The correlation coefficient between the nonseasonal GRACE terrestrial water storage averaged over the Hudson Bay Drainage Basin and the tide gauge sea level time series (0.16) is not statistically significant.

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

AR: This relates to one of the above comments (p2339, lines 24-26). Again, this is a good point that will be mentioned in the revised manuscript.

RC: p2344: As noted in the general comments, I have concerns about the simple model framework. Why not include some comparisons with one of the publicly-available higher-resolution ocean-sea ice models?

AR: See our response above to the general comment.

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

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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.

AR: The suggested word-choice changes will be made in the manuscript revision.

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

AR: Yes. All model runs are performed over the same time period. This point will be made clearer in the revision.

RC: 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.

AR: This is a valid point that will be mentioned in the revision.

RC: 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.

AR: We agree with the reviewer. It will be stated more clearly in the manuscript revision that this suggestion is speculative.

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

AR: The suggested word-choice change will be made in the manuscript revision.

Technical correlations

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

AR: Thank you for pointing out this typo. It will be corrected.

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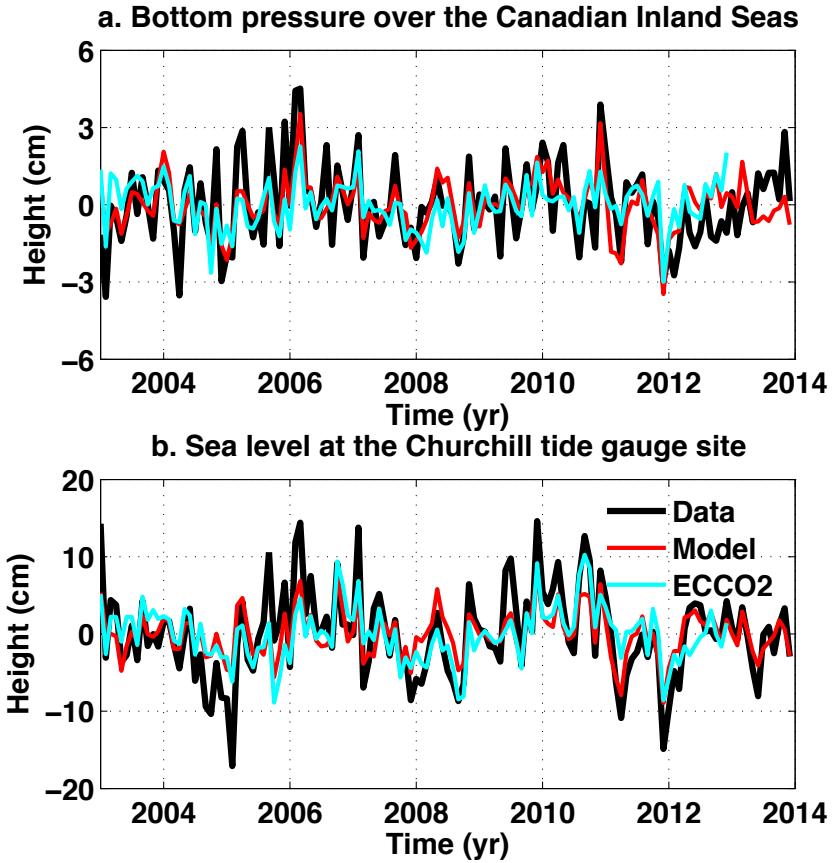

Fig. 1. As in Fig. 3 of the original manuscript submission but with cyan curves indicating the respective quantity derived from the ECCO2 cube92 solution.

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