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Interactive comment on "Monitoring Atlantic overturning circulation variability with GRACE-type ocean bottom pressure observations – a sensitivity study" *by* K. Bentel et al.

Anonymous Referee #1

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This paper is well-written, generally well-structured and the methodology and conclusions appears sound to me. Yet, I have mixed feelings about the article; in it's current form, it's an interesting sensitivity study, but nothing more than that. It shows that interannual AMOC variability should be observable by the GRACE satellites with a reasonable error (assuming no noise in the GRACE data), but that's where it ends: results from real GRACE data will be presented in a follow-up paper. As such, it is comparable to a mid-term report: progress has been made, but the exciting results are yet to come. Spreading out theory and actual results across several papers appears to have become increasingly popular over the year, and generally serves little more purpose than to increase the length of the authors' publication list. The theory presented in the

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article is neither novel nor very complicated (which could justify discussing theoretical and real results in seperate papers), the framework to derive the AMOC variations from GRACE-like data has been set up and the GRACE data performing best (JPL CRI mascons) are available at the author's institute. Therefore, including actual results should be feasible (although not trivial, given that the noise in the GRACE data was neglected in the sensitivity study). This would make for a much more exciting paper, with a much higher contribution to the scientific progress. However, as said before, the methods and results look sound, so I will leave it up to the editor to decide if this manuscript should be accepted for publication in its present form.

The authors find that GRACE-like observations can capture AMOC variations with an interannual RMS error of $\sim$ 1 Sv. However, it's unclear how this compares to the interannual variability of the AMOC itself. Is this 1 Sv error small enough to still detect a useful signal? This deserves to be discussed in more detail. Also, the error strongly depends on how one corrects for hydrological leakage. The CRI mascons and optimally placed mascons perform best in most case. In this regard, it should be kept in mind (and mentioned in the manuscript) that the spherical harmonic solutions can be corrected for leakage as well using stand-alone hydrology models (albeit only to a certain extent, since these models aren't perfect). Furthermore, I suggest to include an additional column of figures in figure 7, showing RMS errors for the GRACE simulations without any hydrology included. This way the reader gets a better feel for which part of the error is caused by hydrological leakage, and which part by leakage due to steep bathymetry gradients.

The simulations and results are based on the assumption that the GRACE data are free of error (BTW, this should be mentioned clearly in the abstract). This is justified, but a short discussion should be included on how these errors will affect the results when working with real data. Chambers and Bonin (OS, 2012) found an error of 1.5-2.5 cm water equivalent in the North-Atlanic. How would this translate into AMOC transport error?

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Finally, an important scientific question is whether the AMOC is declining in strength or not. The summary and outlook sections should briefly mention this and discuss how feasible this is with GRACE data (will GIA be a problem? How many years of observations would be required to detect a significant trend, given the ∼1 Sv error in this study?).

- minor comments:

* p 1771, line 6: Why are you using a 15-month running mean and not, for example, 13 (1 yr) months or 19 months (1 1/2 yr). Add motivation.

* p 1771, line 8-21: Many of the readers of Ocean Science are not familiar with the GRACE data and the different products available. I think it would be good to give a short description of the standard products (spherical harmonics) and the mascon products, and how they differ in use and spatial/temporal resolution.

* p. 1772, line 3: 'Aliasing' might not be the best choice here, suggest to change to "contamination" (in the GRACE jargon, 'aliasing' usually refers to high-frequency signals causing spurious long-term signals due to the temporal sampling by the satellites).

* section 2.4: The SH60 data is listed as having a spatial resolution of 3 degrees in table 1, but looking at the detail in figure 3c this appears incorrect. How did you define your grid for the spherical harmonics solutions: a regular lat/lon grid, or did you shift the grid in longitudinal direction (for each latitude) so that the grid points are optimally placed along the coast line? Just as for the mascons solutions, such a variable grid will most likely reduce leakage from hydrology and improve the results.

* Section 3.1: The RMS errors of the mascon solutions show a peculiar feature at ∼29-30 N, extending to ∼ 3000 m depth, which is absent in the spherical harmonics solutions. Any idea what's causing this? Please discuss briefly in the manuscript. Also, would it be possible ot apply the CRI approach to the position optimized mascons to further reduce the RMS error?

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* Section 3.2, first paragraph & figure 6 + 7: Since GRACE cannot observe the mean AMOC, but only its variations. You also should indicate where the maximum (interannual) variabilty occurs in the model domain. In figure 7, you should also include a line showing the RMS of the AMOC transport at the three layers so the reader can get a feel of the signal-to-noise ratio. How the RMS errors of the GRACE simulations compare to the RMS of the model AMOC should also be discussed in the main text, conclusions and abstract.

- Tables & Figures

* Figure 1: I found figure 1 rather confusing and not very illustrative. OBP +/- signs are plotted at intermediate depths, although these are only observable at the bottom (as the name suggests). Futhermore, I'm struggling to understand what the mean and anomalous flow exactly refer to and what the dashed line is supposed to identify. I suggest to re-do this plot and show the mean northward flow from the ECCO2 model, with OBP plotted separately on the X-axis.

* Figure 7: a line indiciating the required level of correlation for significance should be included in the correlation plots.

- Technical comments:

* Define the GRACE acronym on first occurrence

* p. 1767, line 12: to monitor *the* AMOC.

* p. 1772, line 7: CRI, define abbreviation on first occurrence.

* p. 1773, line 4: change "60ˆo" to "degree and order 60"

* formula 1: define 'eta' symbol

* p. 1770, lines 6-13: this paragraph is a partial repeat of lines 19-25 on page 1767. Restructure these two paragraphs to avoid overlap.

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