Ocean Sci. Discuss., 12, C1006–C1014, 2015
www.ocean-sci-discuss.net/12/C1006/2015/

Ocean Science
Discussions
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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.

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Received and published: 23 October 2015

Statement of Revision

We greatly thank both reviewers for their valuable comments which helped to improve our manuscript significantly. We addressed all points in detail. In the following we want to give our statement to Referee #2's comments.

Comments by Anonymous Referee # 2

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- Is there a reason that you cannot combine the mascons pos opt with CRI? It seems like this could provide even better results.

Technically, it is possible to apply the CRI filter to the position optimized mascons. However, opti- mizing the position in the Atlantic Ocean would shift the whole mascon configuration globally. The main point of the simulation here was to show that mascon placement with respect to the topography actually makes a difference. Instead of computing a new global mascon solution with the position optimized mascons, which includes coastline correction, the goal is working towards a new 1 deg mascon solution, which would have even more benefits than a new coastline resolution improvement.

- L225-228. In the methods, the authors use a fixed depth to separate their layers (909 m). However, the depth of maximum overturning may vary with time and with latitude. At a minimum, you should mention this in the paper, as it is a significant and known issue with estimating the overturning with alternative observational strategies (mentioned, I believe, in Send et al (2011) for the MOVE array at 16N).

Indeed, the depth of maximum overturning can vary with latitude and time, this is now added to the text. It may introduce (small) errors when the total overturning transport is computed (added to the text in section 3.2). However, when comparing layer transport to the ECCO2 model reference, the choice of the depth of the layers is not crucial. Therefore, we think it is feasible to choose the maximum overturning to be constant to evaluate the method of deriving transport from OBP observations at a GRACE-like resolution. But we do note now that there is a spatial & temporal variation of the AMOC structure.

- Related to the above comment, more a question for thought than something that needs changing. Suppose that the top 100 m was entirely wind-driven Ekman transport. Would you be able to reconstruct the overturning and perhaps constrain some variability by applying a mass conservation constraint? Or in other words, if you add up your 3 layers, how close to zero do you get? It should differ from zero by the Ekman transport and perhaps by the AABW circulation. If you can reconstruct the overturning, perhaps you can make a GRACE-derived version of Fig. 6.

When the three layers considered here are added up, the remainder (Ekman+ABW) is in the same order of magnitude as the signals which we obtain for the three individual layers. Furthermore, the remaining signal agrees reasonably well with the Ekman + ABW signals from ECCO2 (i.e. in our case transports above 100 m and below 5000 m depth).

Summing up the two layers that contain the southward transport should give a reasonable estimate of the AMOC transport anomalies (assuming mass conservation). Therefore, we added new plots showing the sum of the two deeper layers from a mascon resolution with CRI in comparison to the model truth transport.

GRACE can only be used to observe temporal AMOC anomalies, but not the full signal. Therefore, a temporal mean of the full signal cannot be derived form GRACE.

- Is there a reason that the OBP data were detrended? Can GRACE recover trends in bottom pressure associated with trends in transport?

See answer to Referee #1's question - model drift is also an issue, so we detrend everything to not be affected by that (the other reasons are still valid, of course).

- I am confused by the authors claim of a 1 Sv error estimate of the AMOC time series. The authors note that the AMOC can be estimated w/1 Sv error (L301), but based on the remainder of the conclusion and the absence of a figure showing the AMOC time series (rather than layer transport time series as in Fig. 8) leads me to wonder whether the AMOC time series was constructed or not. It would be worth adding a time series of the AMOC (a new Fig 9), perhaps for the two chosen latitudes? Since Fig. 8 is quite busy with a lot of lines, perhaps the new figure could include only the

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model reference and the best estimate of the AMOC (either or both of the top 0-909m layer and the sum of the lower two layers)? I think this figure is necessary if you want to use "AMOC" in your title, rather than something more like "transbasin transports".
We have added a new Fig. 9 showing the total southward transport, derived from OBP at a GRACE-like mascon resolution and at the original ECCO2 0.25 deg resolution, compared to the ECCO2 model truth. This figure is additionally discussed in the text, error RMS and correlation coefficients are interpreted. Furthermore we updated Fig. 8 and added time series derived from OBP data without hydrology, as suggested by Referee # 1. This should be more clear now. We also decided to skip the negative example for 35 N. The fact that some latitudes are more favorable to the GRACE resolution than other is made clear in Fig. 7 already.

Textual comments:
On the title, you say "GRACE-type" but in the paper, "GRACE-like". I think "GRACE-like" may be more appropriate.
L17-20. Long sentence.
L22, 23. Write out acronyms on first usage. Note "RAPID" is not an acronym, though MOCHA and MOVE are. Elsewhere there are other acronyms not spelled out.
L29. Consider whether a reference to Frajka-Williams (2015) for AMOC variability manifest in sea level changes is appropriate and helpful.
L31 and throughout. Suggest not capitalising Eastern and Western.
L35. "zonal cross section" could be replaced by "latitude"?
L38. The "i.e." is probably not necessary
L40-41. I think the other major difference is the smoothing of OBP fields in a GRACE-like manner.
L65. Odd punctuation within the parentheses.
L70. Clarification. The quantity T(z) is not really a transport (or at least does not have units of Sv), but rather a transport-per-unit-depth.
Eq(3), delta z is not defined.

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L86. inter-annual -> interannual. Not necessary to specify "periods greater than annual"
L92. From a quick skim of Elipot et al. (2013), I don?t see where they used hydrographic data to confirm the dominance of the western boundary. I do see, their section 2a, references to Kanzow metal 2010 and Bingham and Hughes 2008 on the dominance of the western boundary. Possibly you are referring instead to Elipot et al. (2014), their section 3b(1)i?
L105-106. This is also a limitation of in situ pressure.
S2.2. Any more model details? I don?t think you mention that this is a state estimate with data assimilation. Is GRACE data assimilated, though that doesn"t necessarily matter for this analysis.
L116. is -> are. "data" is plural.
L117. "longitude" and "latitude" are unnecessary.
L134. Gulf stream -> Gulf Stream
All changed.

Fig. 3. Fascinating. I suggest a better choice of color scheme to highlight the data, unless your point is that there isn't much structure left in the 3rd panel. Typo in caption. "sh"?
Actually, the two main points are to show the mascon structure and the little remaining signal in the smoothed spherical harmonic data.

Fig. 4 Why no enlargement for the spherical harmonics panel?
The enlargement of the mascons is to show the effect of the coastline resolution improvement. There is no such process for the spherical harmonics, the data is very smooth. However, we added an enlarged plot of the spherical harmonic solution.

L153-154. Awkward. Suggest "Besides signal leakage from continental hydrology,

leakage of the signal within the ocean between different depths must be considered."
L160. optimal -> optimally
Corrected.

Fig. 5 (see also comment on L70), suggest referring to T as transport-per-unit-depth as. Some x-axes appear to have disappeared. TWS in figure should be "hydrology" to match caption. Is there no mascons optimised plus CRI version?
All changed. We did not compute a mascon position optimized plus CRI version, instead there is an effort towards a one degree mascon solution, which will be even more beneficial (see also response to first comment).

L168. I don't understand what "forward-simulated" means here.
Unnecessary, it's removed.

L170-178. I don't understand the discussion of how removing a mean introduces errors.
Removing a mean over all depth would propagate information (and errors!) from very shallow areas to deeper layers. Since continental hydrology is a much larger signal in magnitude than ocean bottom pressure, even after leakage correction the hydrology leakage errors are still significant. By not removing a mean, we avoid the very deep layers being "contaminated" by the hydrology signal (that tends to contaminate the shallower layers more).

L180-185. How much of the error is due to not capturing the variability vs not capturing the magnitude?
The majority of the error is due to not capturing the correct magnitude due to mascon averaging, see also Fig. 8.

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L187. Can you discuss the source of the leakage at 25N in more detail? hydrology? depth? Some influence of the Bahamas? Ok, you say in L196 that it's due to the mascons. Why 25N? Interestingly, the latitudes for which this is a problem appear to correspond to those latitudes where the Willis (2010) method of recovering the AMOC from Argo and altimetry works, presumably due to the steep bathymetry (allowing Argo floats to get close to the boundary).
The steep bathymetry is the reason for leakage in the mascons. At about 25 N the 1000 m and 3000 m depth contour line are almost next to each other (Fig 2). With a three degree mascon, values from above 1000 m to below 3000 m will be averaged out into one OBP value for one mascon. We added this to the text.

S3.1 This is a long paragraph. Suggest breaking somewhere. L192? also L206?
L213. remove comma
L225. Kanzow et al. (2007) also showed something like this for timescales of 10-days.
Corrected.

*L225-228. This is assuming that you know your depth of maximum overturning. This is probably an unavoidable limitation of your study. It is also a limitation of the MOVE array at 16N. At 26N, the depth of maximum overturning varies (McCarthy et al, 2015). If there were to be a trend in the depth of maximum overturning, for instance, but you chose a fixed depth of 909 m, you would not measure the part of the trend in the AMOC associated with the changing depth.
We added a sentence to point out issued with choosing a fixed depth. However, we do not estimate trends from GRACE, since the errors associated with GIA corrections are too large (our answer to third comment).

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*L270. So, errors in the middle layer are high, but in the lower layer are low. Where is the signal of variability dominant? Is it enough to resolve the upper and deep layer to recover the MOC?
There is slightly more signal variability in the middle layer (new Fig. 6) but the deep layer can serve as a proxy for AMOC variability (e.g. RAPID time series). The upper layer misses the Ekman transport (which cannot be observed by GRACE) for the complete AMOC signal. However, we constructed the complete AMOC time series, by summing up all southward transport. See new Fig. 9.

*Fig. 8, why is the green line missing from the left column? Ah, ok, I see from L278 that they are covered. Perhaps make one dashed? Also, can the axes be rescaled to contain all the lines? Is it worth plotting only the best reconstructions in this case, to really see how well they do? Yellow and green lines are very hard to read. For the best reconstructions, can you comment on what part of the variability is well-reconstructed? Does GRACE get the trend if not the interannual variability?
Fig. 8 has been updated according the suggestions. It should be much better to read now. There is still one instance where the scale doe snot fully accommodate the curves, however, we prefer to choose the axes as they are now, because otherwise, other (more important) details would be less readable. The actual value of the points which are not accommodated are not important.
Annual signal and trend are removed, see above (trends from GRACE).

L280. form->from
Corrected.

L301. Is it worth mentioning that this is about as well as RAPID can recover AMOC variability (Mccarthy et al 2015), though that was for full time variability.
This has been added to the conclusions section.

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L305. OPB -> OBP

*Suggest one additional figure. While the medium layer is certainly ruining some of your signal, can you come up with an estimate of the MOC at your two sample latitudes? perhaps the best estimate, and plot those time series with only the best estimate and the model-reference time series? And perhaps discuss the variance explained.

Corrected and a new Fig. 9 is added and discussed in the text.

Interactive comment on Ocean Sci. Discuss., 12, 1765, 2015.