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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 #2

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This is a review of "Monitoring Atlantic overturning circulation variability with GRACE-type ocean bottom pressure observations - a sensitivity study" by Bentel et al.

The authors describe an application of using OBP to infer AMOC changes. To my knowledge, this has never been done before, and represents a substantial and valuable bridge between model-based studies (Bingham and Hughes) and possible observational applications. In particular, while model-based studies using 'perfect' OBP have shown that OBP can be used to infer AMOC changes, the realities of space-based observations of time-variable gravity are far from the 'perfect model' assumption. It is remarkable then that the authors find a correlation between AMOC and OBP variations.

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Interactive Discussion
Discussion Paper

This is a technical piece which is necessary to highlight the challenges involved with using these satellite data products. In contrast to the other reviewer, I do not see this paper as lacking in scientific results. If it were to be combined with the “more interesting” findings, it would result in a paper of unwieldy length.

Overall, the paper demonstrates a method to use improved versions of a GRACE-like OBP to determine transbasin transport variability in the Atlantic. The authors articulate the differing challenges at different latitudes, and conclude that AMOC time series can be estimated with errors on the order of 1 Sv. The deepest layer considered (3000-5000 m) is particularly well-suited to GRACE-like recovery.

I have recommended a few changes to the figures which I believe will improve legibility and impact. The figures with lots of lines, in particular, are difficult to read. With 5 lines, it would be possible to use grayscale, where perhaps shades of gray or line style is varied (though Matlab does not do this elegantly). The lines in Fig. 8 are problematic (see comments below).

More substantial comments:

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

- 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).

- 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

OSD

12, C706–C711, 2015

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.

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

- 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 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".

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.

L86. inter-annual âĂŤ> 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. isâĂŤ>are. ‘data’ is plural.

L117. “longitude” and “latitude” are unnecessary.

L134. Gulf stream-> Gulf Stream

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”?

OSD

12, C706–C711, 2015

Interactive Comment

Fig. 4 Why no enlargement for the spherical harmonics panel?

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

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?

L168. I don't understand what "forward-simulated" means here.

L170-178. I don't understand the discussion of how removing a mean introduces errors.

L180-185. How much of the error is due to not capturing the variability vs not capturing the magnitude?

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).

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.

*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).

OSD

12, C706–C711, 2015

Interactive Comment

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.

*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?

*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?

L280. form->from

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. 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 latitudesâĂŤperhaps 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.

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