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Ocean Science Discussions

Interactive comment on “High frequency variability of the Atlantic meridional overturning circulation” *by* B. Balan Sarojini et al.

Anonymous Referee #2

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The manuscript analyzes the interannual variability in the Atlantic meridional overturning circulation (AMOC) at 26N in the Atlantic. First, the manuscript compares the model results to the 5 year long timeseries of AMOC observations from the RAPID/MOCHA array. In a second topic, the manuscript aims to investigate the relation between AMOC and the meridional heat transport in the North Atlantic.

I think the manuscript covers interesting material, and makes a timely contribution to the current discussion of the AMOC variability. I would like to see the manuscript published, but I do suggest a re-write, and maybe additional analysis. What I am unsure about is the combination of topics in the paper.

My suggestion would be to drop the part on the coherence (and heat transport) in the present manuscript, and focus on the comparison of observed and simulated high

frequency variability in more depth (details suggested below). In summary, I do recommend the manuscript to be published after a major revision.

MAJOR COMMENT:

I get the impression that the authors try to simultaneously cover too many different topics. The introduction provides a general motivation for investigating high-frequency AMOC variability, and ends with a list of topics for the manuscript. However, I don’t find this list particularly coherent – the topics all fit under the broader title, but they don’t seem to build on each other. The topics I see (not exactly the same as listed on p. 222, l. 5f) here are (i) the simulated 26N-AMOC variability and its comparison to observations, and (ii) the coherence of heat and meridional transport throughout the North Atlantic.

I have no fundamental objection of covering these two topics in one article. But it should be clearer motivated, why this is interesting (other than that the same models could be used to study both). Particularly, it should be brought out what is gained by this combination – what is the synthesis? I think it is symptomatic that the introduction is not really deriving the questions but is listing them, and also that both a discussion and conclusions section are missing, but an overall summary is buried in the last section of the manuscript (which still presents results).

At minimum, I suggest

(a) a rewrite of the introduction,

(b) a dedicated discussion section where the different topics are brought together, and

(c) a conclusions section providing a synthesis.

On a further note, I do think that the authors would actually be better off with focusing on one of the two above-mentioned topics. To some extent, I do think that they only scratched the surface for both topics. And I give suggestions for further study below. I do realize that fully covering all these suggestions would beyond the scope of a single paper. However, focusing on one of these topics, and going into somewhat more depth would be not.

My suggestion would therefore be to drop the part on the coherence (and heat transport) in the present manuscript, and focus on the comparison of observed and simulated high frequency variability in more depth (details suggested below).

Specifically, the comparison to the observations should be extended to cover the following topics:

(d) the annual cycle of the AMOC, and its components has been studied in the observations. First, I am surprised that the paper (Kanzow et al., 2010, JClim) is not cited. But second, it would be very interesting to see a comparison of the simulated and observed annual cycle for the AMOC components.

(e) the AMOC decomposition employed for the model ignores the contribution of the western boundary current variability. First, I need to understand what the geostrophic transport in the models is compared to from the observations (to the sum of interior/mid-ocean and FC transport, which would be correct, I think, but is in conflict with what is described at the bottom of page 229). Second, even with the models not resolving the Florida Straits, it should be possible to calculate the strength of the northward western boundary current in all models (even though it's not geographically constrained). Its contribution to the overall variability of the geostrophic transport would be quite interesting to analyze.

Specifically, for the coherence of the heat and mass transport throughout the North Atlantic I have the following suggestions:

(f) For the coherence of the AMOC cell, the existing literature needs to be fully discussed. E.g., Lozier et al (2010) is cited at the end of the manuscript, but it needs to come much earlier (somewhere page 231?). Also, the references in Lozier et al (2010) give a good summary on what has been studied in other models – what is described

here has to be brought in context, and also clearly distinguished from earlier studies. That the 26N AMOC and the 50N AMOC are not immediately connected is by itself no longer a novel conclusion, mechanisms or robustness across different models would be, but the authors are silent on this.

(g) The discussion of the heat transport is to quite short, and mostly consists of the giving the numbers for the different models. Also, figure 5 is not really surprising – isn't this just showing that the overturning contribution to the heat transport gets smaller with increasing latitude? Also, the relation of the 26N to the 50N heat transport would be much more interesting in terms of the individual contributions (overturning/ gyre), but the total. In any case, the results need to be discussed, and not just mentioned.

These are some of the most obvious points where the analysis for both topics stops too soon.

MINOR COMMENTS (in the order as they appear in the manuscript):

1. p. 220, l. 5: 'range of timescales' needs to be defined.

2. p. 220, l.8f.: this statement seems a bit misleading to me, or at least it's not in agreement with what is said on p. 225, l. 21.

3. p. 221, l. 17: again, a bit more details on what timescales are considered would be nice.

4. p. 221, l. 24: mention that the models are a coupled model and a data assimilation product (ocean only model), in case readers don't recognize the abbreviations.

5. p. 221, last line and next page: this is a rather general statement, and would be the starting point for deriving the specific motivation for the present manuscript.

6. p. 222, l. 11: hyphen missing in 'atmosphere-ocean models'.

7. p. 222, last line: I don't understand the sentence starting here.

8. p. 223, l. 21: why are you using control integrations?

9. p. 223, l. 21: why are you using 5 years from observations, but 10 years from the models? How much are the conclusions affected if you used only 5 years in the models? Very simplistic: if you divide the 10 years you have used so far, are the conclusions robust for either using the first or the last 5 years? More sophisticated (maybe not needed, if the simple test indicates robust results): what happens if you bootstrapped the control runs?

10. p. 224, top: I think a subheading would be useful here.

11. p. 224, bottom: I think a figure showing the 5 year AMOC timeseries from observations and models would be illuminating (the standard deviations and mean values in the table are a bit dry).

12. p. 226, l. 11: Cunningham et al. compose (and not decompose) the AMOC from the transport components!

13. p. 226: somewhere here needs to be explained how differently models and observations are handled with respect to the transport components, and to what extent the components are comparable between model and observations. In line with what I mentioned above, I would appreciate to see a close resemble of the observed transports to what is calculated in the models.

14. Section 4: how is the geostrophic transport referenced?

15. p. 228, l. 21: the ‘this’ and the ‘it’ are ambiguous.

16. p. 229: discuss the role of the boundary current variability somewhere here.

17. p. 229/ figure 2 (further extending comment 13.): a visual comparison of observed and modeled transport components would be nice.

18. p. 229, l. 24: I think, Kanzow et al. (2007; same Science issue as the Cunningham et al., 2007 paper) would be the appropriate reference here.

19. p. 229, l. 25f.: as mentioned above, are internal transport in models and observations comparable?

20. p. 230-232: if the topic stays in the manuscript, it needs to be revised (details are above).

21. p. 233: this is where the summary starts?

22. p. 233, l. 4f: I disagree. ‘Common wisdom’ had the mean AMOC at around 18 Sv, and modelers did pay attention to getting a decent mean AMOC strength before there were RAPID/MOCHA observations around. I suggest deleting this statement.

23. p. 233, l. 6f: why is this surprising? Also, references to Marsh et al (2009) and Cunningham and Marsh (2010) are missing.

24. p. 233/234: your main implications seem to be that such decomposition should be done again, and that the AMOC should be also observed further north than 26N? Both conclusions are weak and not novel – I suggest re-writing, since the manuscript has more to offer.

25. p. 224, l. 1: as mentioned before, the Lozier et al. (2010) reference needs to come earlier.

Interactive comment on Ocean Sci. Discuss., 8, 219, 2011.