Response to Interactive comments by Anonymous Referee 2 on "High frequency variability of the Atlantic meridional overturning circulation" by Balan Sarojini et al.

We thank the reviewer for the constructive suggestions and generally positive attitude towards the paper.

Responses to Major comments

We propose to follow the reviewer's "minimum" path of rewriting the introduction and conclusions, rather than splitting the paper. We agree that there are two foci of the paper, namely the simulation of variability and comparison with observations at 26^oN, and the coherence and relationship between the AMOC and meridional heat transport at various latitudes and longer time-periods. The connection between these, and indeed the motivation for the study, is the dataset from the RAPID monitoring array at 26^oN. In the first topic, we are using this important dataset to evaluate and compare a range of models in regard to high-frequency variability, which is a new kind of observational information. In the second topic, we are using the models to set the high-frequency observations at 26^oN into their climatic context. The main motivation from climate science for the RAPID monitoring array is the climatic influence of the AMOC and how it might change in the future, and we depend on models for information on the climatic influence of the AMOC on multiannual timescales. We will clarify these interests in the introduction and the conclusions.

In rewriting the paper we will include further references to the existing literature and some additional figures, arising from suggestions by the reviewers. On a couple of points, we agree that more detailed analysis of mechanisms would be useful. However, such analysis is unfortunately not practical in the context of an intercomparison of a large number of models such as this. We are at present engaged in further study of HiGEM in particular to follow up points raised by the present paper in more detail.

(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.

The annual cycle of Ekman and geostrophic parts in the models will be included (two more figures and discussion) and Kanzow et al. (2010) will be cited as well.

(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 geo-

graphically constrained). Its contribution to the overall variability of the geostrophic transport would be quite interesting to analyze.

We do not ignore the western boundary current (WBC) variability, but evidently we need to explain our decomposition more clearly. The geostrophic transport in the model, which is a sum of external (related to the sea surface height) and internal (related to the density changes) parts, is compared with the observed geostrophic transport, which is the sum of mid-ocean transport and Florida current transport. This will be explicitly stated in the Table 1 caption and Section 4. In the models, the external and the internal parts are calculated from the western boundary to the eastern boundary (coast-to-coast) at 29^{o}N in all the models. An exact comparison of the WBC between the models and observations, in a way which will treat models consistently, would be difficult, because the models position the currents differently with respect to their various representations of the coastlines. Instead, we are proceeding with this comparison in HiGEM, which does resolve the Florida Straits.

Confusion may have been caused by our comment on anticorrelation between the external and internal components. An external component is not considered in Cunningham et al. (2007) and Kanzow et al. (2010); instead the correction term for the mass-conservation plays this role, in effect. This will be made clearer and Kanzow et al. (2007) will be cited at P.229, l.24.

(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 26^{o}N AMOC and the 50^{o}N 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.

We agree with the reviewer that the citation of Lozier et al. should be earlier in the paper. This will be done, and we will also discuss earlier papers on this topic, such as Baehr et al. (2004), Lozier et al. (2008), Roussenov et al. (2008), Willis (2010). We will point out the robustness of the results; that is, the various models, despite their variety of formulation, lead to similar conclusions.

(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 26^{o}N to the 50^{o}N 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.

We think that Fig 5 is significant, especially regarding the similarity of the slopes. As the reviewer has earlier pointed out, this figure suggests the robustness of the relation of the heat and volume transports in the north Atlantic among models. The results will be discussed. As mentioned above, a further analysis involving decomposition of the heat transport would be valuable but is better done with one or a smaller number of models.

Responses to Minor comments

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

This will be changed to five-daily, seasonal and interannual.

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.

We will clarify this statement in the text. We meant the magnitude of the annual cycle is somewhat similar (Fig 1c), but as in p.225 l.21, the shape of the annual cycle is not well captured and shows a spread in the models.

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

This will be done.

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.

This will be changed as suggested.

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.

We will take this into account in rewriting the introduction.

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

This will be corrected, thanks.

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

The sentence will be changed to, "HiGEM is computationally expensive, but several multi-decadal runs with it have been completed."

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

We are using control integrations because the models are generally started from present-day climatology and it is customary to evaluate control runs with respect to present-day climatology. This avoids the complication of whether radiative forcings of climate change are done the same way in different models and whether trends associated with climate change are realistically simulated; thus it simplifies the comparison of model results.

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, *areconclusions 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?*

We will follow the reviewer's suggestion of testing whether results are affected by using the other five years of model data.

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

Subheadings 'Models' and 'Observations' will be added to the section.

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).

We will add the suggested figure.

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

This will be changed as suggested.

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.

This will be explained as suggested; please see the response to point (e).

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

In our decomposition, the barotropic, external component is calculated using the sea surface height and the internal component is based on the zonal density gradients considering all the grid points (Equation 4.).

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

The sentence will be changed to, "The residual due to the local acceleration is negligibly small and is ignored in all models."

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

This will be done.

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

This will be done.

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.

The reviewer is right; the citation will be changed.

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

No, but the geostrophic transports are; this will be clarified.

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

See above (Responses to Major Comments).

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

We intend to improve the summary and discussion in the revised version.

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.

Yes, the time-mean of the AMOC has generally been considered important during model development. Our point is that the models are unlikely to have been tuned to reproduce the recently observed short-term variability.

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

It is often assumed that if the resolution is increased, variability in all time-scales will be increased. This remark will be expanded and the references will be discussed.

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 26^{o}N? Both conclusions are weak and not novel - I suggest re-writing, since the manuscript has more to offer.

We intend to improve the conclusions.

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

This will be done.

REFERENCES

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