

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

Anonymous Referee #1

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The study by Sarojini et al.,

uses a set of AOGCMs to answer several questions: (a) whether the models can simulate AMOC variability as measured by RAPID/MOCHA arrays, (b) whether the volume and heat transports relate to each other and (c) provides the spatial correlation lengths of both. The practical application of this work suggests that AMOC at 26°N can not serve as a proxy for heat transport at high latitudes but that the additional monitoring of AMOC at high latitudes would be of a great use to assess the climate variability. This work is certainly relevant and I am happy to recommend acceptance with minor revision.

Line 67: authors might like to cite the COREs by Griffies et al. (2009).

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Chapter 2, Line 90 I would suggest to add 'in the ocean' after 'substantial differences'

In the chapter 3 the authors show that the high frequency amplitude of variability does not depend on resolution and is induced by the winds. In turn, models with UVic atmosphere do not show this variability. Is the wind field prescribed as stationary in UVic? Would it change much if the variable wind is prescribed?

Chapter 4, starting from line 237 authors say that geostrophic transport variability is smaller than the Ekman constitute and is the case for high frequencies and seasonal cycle. This is attributed to the underestimated adiabatic upwelling/downwelling from alongshore windstress.

I would think that the geostrophical seasonal cycle is driven mainly by the surface fluxes. Thus the underestimated variability might also indicate the problems in the vertical mixing scheme (or imperfect fluxes). The deeper look into the eastern/western density profile variability would probably tell more about what happens. Would Figure 2 become better if plotted for HiGEM?

Line 242: should not it be 'variability of pressure anomaly' instead of 'pressure anomalies'. The variability of transport is discussed and not the mean.

Line 253: Isn't the physical mechanism the fact that the pressure at large depths varies less than the dynamic hight and the latter is compensated by the hydrostatic pressure?

Chapter 5: Might Ekman transport anticorrelation for widely spaced latitudes be associated with moving front between subpolar and subtropical gyres?

line 298: remove 'which'

Chapter 5: The time lag between 26° and M_{\max} which is at higher latitudes was mentioned few times in the chapter. At the end it is said that the higher latitude precedes the lower one with the time lag of 4 years. I suggest to say some words about the mechanism behind this lag.

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Line 309: It is claimed that the high latitude AMOC index has its greatest importance for climate variability and the reader is referred to chapter 6. It is however not explained in chapter 6.

Concerning summary: The absence of the systematic relation between the model resolution and the magnitude of transport variability is partially because the whole section was taken into account and was not treated as the constitutes of several subsections as it is often done. Thus no eddy passes over the endpoint of the section. The results point out that the across-section eddy translation is small.

Line 372: 'o' in 26°N is forgotten.

At many places where citations are done there are no points after 'at al.', like in line 59. Something is wrong with bibtex, probably.

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

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