

# ***Interactive comment on “Seasonal variability of the Atlantic Meridional Overturning Circulation at 11° S inferred from bottom pressure measurements” by Josefine Herrford et al.***

**Anonymous Referee #1**

Received and published: 28 July 2020

## General Comments

The study of the seasonal variability of the Atlantic Meridional Overturning Circulation (AMOC) at 11S is very important, however, some aspects of this study need clarification to warrant its publication in Ocean Science.

While any effort to extract as much information about the AMOC variability as possible from temporally and spatially sparse existent data is appreciated, the use of a model simulation (1948-2007) that does not overlap with the observations (2013-2018) is problematic. This makes it harder to pinpoint the reasons for the differences between model and observations and thus to trust the chosen observational strategy.

[Printer-friendly version](#)

[Discussion paper](#)



As a consequence, the seasonal cycle of AMOC transport from the observations is very different from that obtained from the model. Not only the maximum and minimum values occur in different months of the year, but also their amplitudes are statistically different.

In addition, the description of the results using different periods of time is very confusing. For instance, the periodograms of Ekman transport are presented for 2013-2018 from ASCAT dataset and for 2002-2007 from CORE2b dataset. But the minimum and maximum ranges are calculated from ASCAT for 1993-2018 and from CORE2b for 1978-2009. Even though the two datasets overlap for the period of 1993-2009, the authors then show the Hovmöller of zonal wind stress for 2008-2009. It is not clear why this is done. It would be better to compare the wind seasonal cycle obtained from both datasets for the period of 1993-2009.

Finally, the manuscript is long and most of its content is on validating the analysis rather than showing and discussing the main results about the AMOC variability. For instance, the latter is only introduced on page 11. The readability of the manuscript would also improve if information is conveyed in a more clear and straightforward way.

#### Specific Comments

Lines 21-22: "Here, long Rossby waves originating from equatorial forcing are known to be radiated from the Angolan continental slope and propagate westward into the basin interior." Is this shown in this study (here) or concluded from other studies? After reading the manuscript, I could not find any analysis that presents this.

Lines 103-158: Sub-sections 1.1, 1.2, 1.3 and 1.4 should be 2.1, 2.2, 2.3 and 2.4, respectively.

Line 116: I am not sure if it is necessary to describe the software used to calculate the tidal harmonics.

Lines 120-121, 133-134: Fig. 2a and Fig. 2b should be Fig. 1a and Fig. 1b, respec-

[Printer-friendly version](#)

[Discussion paper](#)



tively.

Lines 200-205: Did the authors test other depths to have an estimate of the sensitivity of this choice ( $z=1130m$ )?

Lines 280-283 & Fig. 4: Which of these peaks are statistically significant? Particularly, considering the annual and semi-annual harmonics from 2-year long time series. This is different from the calculated uncertainty shown in shading.

Lines 297-298: Isn't this also related to the fact that the observed time series are very short and cannot capture well the annual harmonic?

Lines 309-311: Why is the periodogram for the CORE2b wind stress calculated for 2002-2007? Could a longer period from the model data be used as well to assess the impact of such observed short time series on the variability?

Lines 310-311: "The CORE2b winds do also show weak semi-annual variability, but only when considering the full time series from 1978-2009", where is this shown?

Lines 311-322: Perhaps, it would help to show an extra panel similar to panels Fig. 7c,d with the climatological evolution obtained from both dataset for 1993-2009. In fact, it is very confusing, the model outputs are for the period of 1978-2007 (Section 3). The Ekman transport periodograms are obtained from ASCAT for 2013-2018 and from CORE2b for 2002-2007. But the minimum and maximum ranges are calculated for the 1993-2018 for ASCAT and 1978-2009 for CORE2b. Why not to show a climatological Hovmöller for the overlapping period 1993-2009, instead of for 2008-2009?

Lines 336-338: The seasonal cycles of TAMOC from the observations and model are not similar. In particular, the maximum observed TAMOC occurs in May and the maximum modeled TAMOC in February, whereas the minimum observed TAMOC occurs in October and minimum modeled TAMOC in August. The amplitudes are also statistically different, comparing the error bars for the observations with the shading for the model.



Lines 359-360: This is not the case for TAMOC (previous comment).

Lines 376-389: It seems that the observation/model comparison is inconclusive.

Lines 395-403: In Fig. 12, why is the slice from 15W to 5W not included in the calculation for the interior transport? The definition of AMOC transport encompasses the whole basin, and if one wants to discuss the contributions of the WBC, interior and EBC to the AMOC variability, the slice from 15W to 5W has to be included in the interior transport. Later in lines 418-420, the authors state that there is a minimum in the annual and semi-annual harmonics in this range. However, this is not a good reason to not include the contribution from 15W-5W in the calculations. If the related transport is also minimum there, including this won't affect the main findings, but it will make the results more consistent.

Line 401-403: This is why the use of a model output that encompass the same period of the observations is so important. And also, a comparison between using shorter versus longer time series from model outputs would permit to evaluate the impact of using observed short time series on the seasonal variability.

Lines 401-427: Fig. 13a is not mentioned in the text but shows that there is not a defined seasonal cycle of the NBUC during the period of 2013-2018.

Lines 428-531: What is the impact of using the combined annual and semi-annual cycles for the eastern boundary after 11/2015 since they explain 44-61% of the variance in the daily BP time series there and for the western boundary before 05/2014 since they explain only 18-24% of the variance in this case (Lines 229-238). This was one of the main reasons to use the model outputs. Doesn't this procedure lead inevitably to the conclusion that the geostrophic transport variations are dominated by seasonal variability (Lines 466-467).

Lines 428-531: This section is too long, and the manuscript readability would benefit if most of this discussion was made in Section 5 when the authors present the results. It

[Printer-friendly version](#)

[Discussion paper](#)



is difficult to go back to figures and description of the results at this point to verify, for instance, that the structure of the meridional geostrophic velocity in the eastern basin is linked to CTW. Is this really shown in the results?

#### Minor Comments

Line 254: "We also test or..." should be "We also test our..."

Line 305: In "Prevailing wind stress along 11S is northwestward...", consider instead: "The prevailing winds along 11S are from southeast..."

Line 325-326: To improve readability, consider "Figure 8 displays the derived time series of TG, TEK, and the sum of both components TAMOC at 11S." instead of "Figure 8 displays the derived time series of TG, TEK, and being the sum of both components, TAMOC at 11S."

Line 412: "... to cancel out each other..." should be "... to cancel each other out..."

Line 460: "und" should be "and".

Line 752: "Hovmoeller" should be "Hovmöller"?

---

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2020-55>, 2020.

[Printer-friendly version](#)

[Discussion paper](#)

