Answers to Referee #1

We would like to sincerely thank Referee #1 for their thoughtful comments improving our manuscript, as well as, the time and effort they put into this review. Below we address all issues raised in this review by responding to the individual comments, corrections and suggestions in italic.

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. 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.

This is correct. Comparing two different periods does not allow us to analyze the correspondence of interannual variations in observations and simulations. Differences between model and observations are thus partly the result of the different periods analyzed. Unfortunately, at the moment, there is no suitable model run with atmospheric forcing covering the observed period (e.g. JRA55-do) available and sufficiently validated. However, the model is very well suited to analyze general aspects of the seasonal cycle and particularly the uncertainties of our method to derive the seasonal cycle of an AMOC time series from observations: Similar concepts are followed in Observing System Simulation Experiments (e.g. Gasparin et al., 2019; https://www.frontiersin.org/articles/10.3389/fmars.2019.00083/full).

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.

We are sorry, we made a mistake here. ASCAT wind stress is available for the period 03/2007-12/2018, not 1993-2018. These numbers are corrected in the revised manuscript. When we first analyzed the model, the INALT01 model run covered the period 1978-2007. But since the CORE 2b forcing data set covers 2 more years (1978-2009), a direct comparison of ASCAT and CORE 2b wind stress in 2008-2009 (overlapping full years) is possible and performed.

In figure 7a, we show a periodogram of wind stress calculated for 5 years (2013-2018), for which BP measurements are available. As ASCAT covers a longer period, we can also provide an estimate for interannual variations ("minimum and maximum ranges of periodograms calculated for 5-year windows running through the full time series") plotted as an envelope. In figure 7b for CORE 2b wind stress, the 5-year period 2002-2007 was chosen arbitrarily. We understand, that this is confusing. In the revised manuscript, we do only show the envelope representing the interannual range of periodograms calculated for different 5-year subsets of the full time series.

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.

We think that the results of this study benefit from a comprehensive validation of the analysis. In order to improve the readability, we tried to go through the manuscript sentence by sentence, streamline and shorten the text as much as possible.

<u>**Specific Comments**</u>

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.

This is concluded from other studies. In the abstract, we deleted this sentence. In other paragraphs we included Kopte et al. (2018) as the reference in which the westward propagation is discussed as part of an equatorial basin mode (see also Brandt et al., 2016):

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.

Corrected.

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

We would like to keep the reference, but removed the description of the software.

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

Corrected.

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

Yes, we did. In our study, we defined our level of no motion based on the mean depth of the zero-crossing of the meridional velocity along 11°S in the INALT01 model. And, indeed, at 11°S this depth is more variable than, for example, at 26.5°N. Most of the time (for 87% of the timesteps) this depth varies between 800m and 1300m depth, mainly following a seasonal cycle. Varying the level of no motion within this range changes the mean AMOC transport, which is largest when integrating to 1130m (14.1 Sv), by less than 10% (see Fig. 1 R1; upper panel). We added a sentence regarding this sensitivity to section 4.1. The peak-to-peak amplitude of the mean seasonal cycle of the AMOC decreases with depth – from 7.2 Sv at ~730m to 5.6 Sv at ~1470m and its minimum shifts from October to August (see Fig. 1 R1; lower panel). This is probably due to including parts of the southward lower branch when integrating to deeper levels. We added a sentence to section 4.1

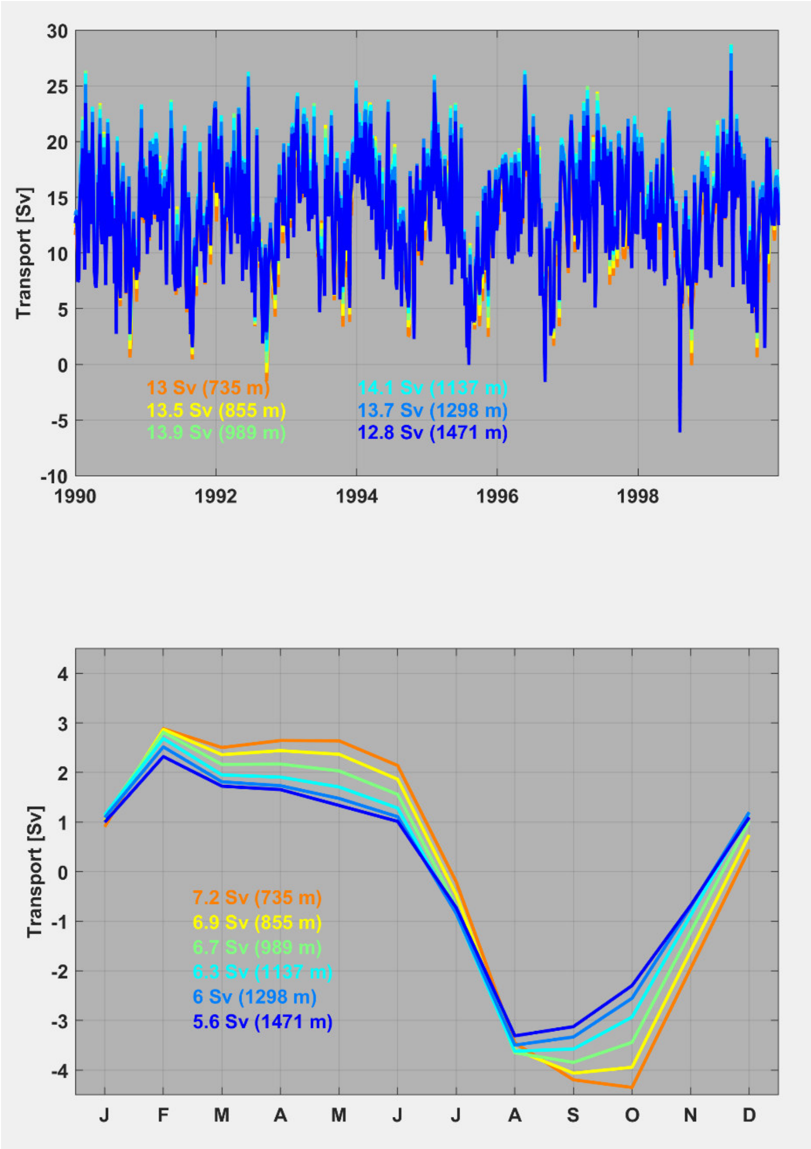

Figure 1 *AMOC transport timeseries (5-daily; upper panel) and mean seasonal cycle (lower panel) at 11°S derived from the INALT01 model velocity fields over the period 1978-2007. Different colors denote different choices of a 'level of no motion' for the integration.*

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.

We tested if the peaks in the periodograms of the BP time series are significant against the red noise background from an AR1 process (see Fig.2 R1; black solid curves are AR1-95%-confidence bounds and black dashed curves AR1-68%-confidence bounds). The annual cycles in the 2-year long BP time series off Angola are the only significant peaks against the 95%-confidence range of an AR1 process. When considering the 68%-confidence range then the peaks of the semi-annual cycles of Angola and at the western boundary at 500m are also statistically significant.

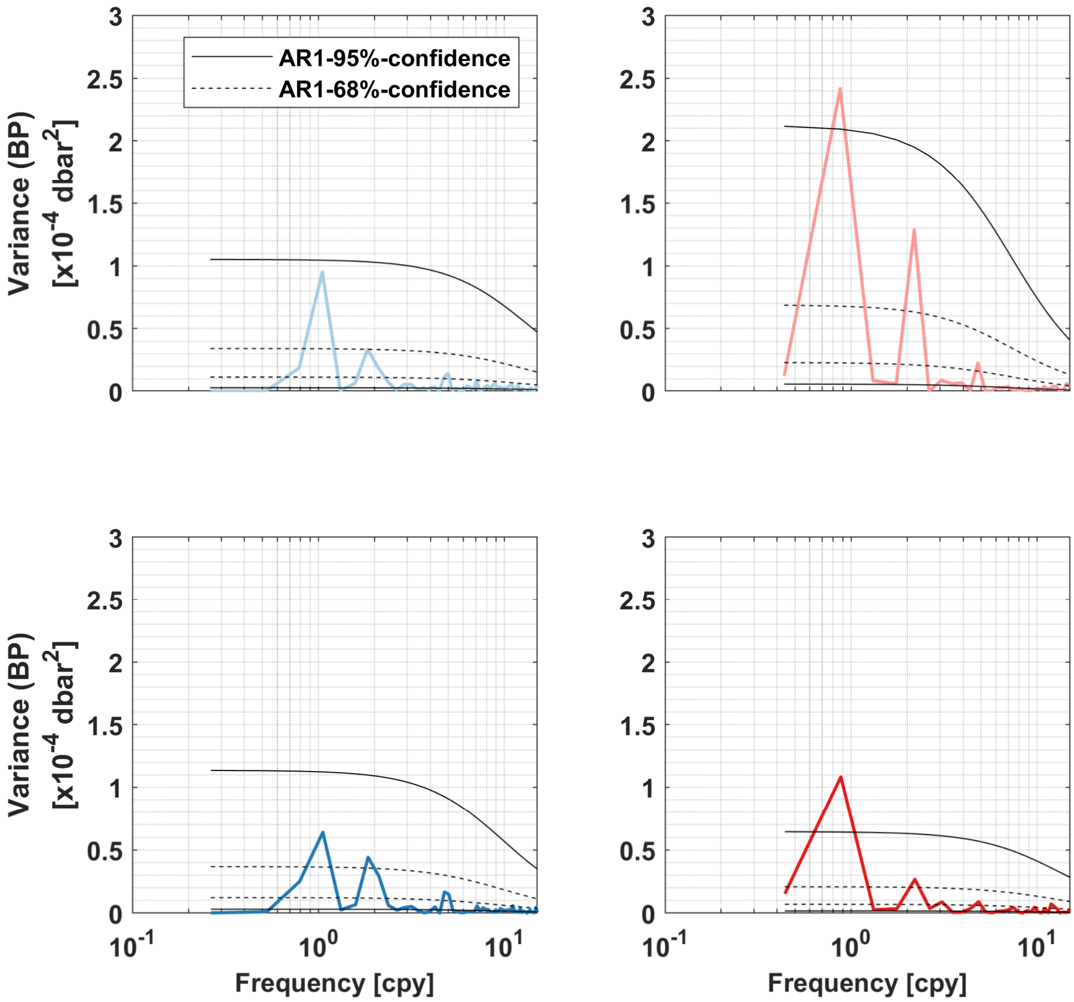

Figure 2 *Similar to Fig.4 in the manuscript. Periodograms of BP at 300 m (upper panels) and BP at 500 m (lower panels) depth at the western (left panels) and eastern (right panels) boundaries at 11°S - calculated over the period 2013-2018. Black curves are the 95%-confidence (solid) & 68%-confidence (68%) ranges of an AR1 process.*

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?

In INALT01, we are able to test how our results are affected by interannual variations and different time series lengths. Typically, we show the possible range of spectra, harmonics or mean seasonal cycles calculated for different 5-year subsets of the 30-year model run, but we also tested how the seasonal harmonics change using for example 1-year subsets, thus the total amount of available seasonal cycles of the model run and the shortest possible time series length. However, we obtain the robust result that, in INALT01, the amplitudes of the annual and semi-annual harmonics at 300m and 500m depth, and especially at the western boundary, are systematically underestimated compared to the observations.

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?

As our observational period covers about 5-years, we wanted to also show the periodogram for an arbitrary 5-year period of the CORE2b timeseries. However, the transparent envelopes, which are supposed to represent interannual variations of the results, already show the minimum and maximum ranges of periodograms calculated for 5-year windows running through the full available time series of

CORE2b (1978-2009) - including the subset 2002-2007. From the comments we understand, that this was rather confusing than helpful. We deleted the solid curve in Fig. 7b and tried to modify the related text and figure caption in the revised version accordingly.

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?

In Fig. 7b, the transparent envelope representing interannual variations shows a second, smaller peak at a frequency of 180 days. As this peak is not statistically significant we deleted this sentence in the revised manuscript.

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?

We are sorry for the confusion, which, we think, is mainly caused by a mistake we made in the caption of Fig. 7. ASCAT wind stress is available for the period 03/2007-12/2018, not 1993-2018. The INALT01 model run covers the period 1978-2007. But since the CORE 2b forcing data set covers 2 more years (1978-2009), a direct comparison of ASCAT and CORE 2b wind stress in 2008-2009 (overlapping full years) was possible and performed.

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.

This is correct. In the revised manuscript, in addition to the minimum and maximum ranges of mean seasonal cycles calculated for running 5-year windows running through the respective available periods we do also show the total range of possible values per month. Even when considering the total range, the mean seasonal cycles of T'_G and T'_{AMOC} are just outside the total range of possible results in INALT01. We rephrased several related sentences and tried to be more specific on where we find good agreement and where we find differences. However, we think that the model is very well suited to analyze the relevant mechanisms and test our method to derive the seasonal cycle of an AMOC time series from observations.

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

Please see the previous comment.

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

We now more clearly state where we find good agreement and where we find differences between model simulations and observations (see above).

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.

This is absolutely right. We extended the slices of the Western basin Interior and eastern basin to 10°W, which corresponds approximately with the location of the Mid-Atlantic Ridge crest. The results change very little. The minimum of the seasonal cycle of the eastern basin contribution, however, is reduced by half its amplitude and shifted from September to August. A small maximum in February is gone. Extending the eastern basin slice to 10°W results in an even greater similarity between its seasonal cycle and the seasonal cycle of the basin-wide upper-ocean geostrophic transport.

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.

As stated above, at the moment, there is no suitable model run with atmospheric forcing covering the observed period (e.g. JRA55-do) available and sufficiently validated. We hope for this to happen in the near future. In order to give an estimate for possible interannual variations, all mean seasonal cycles are shown together with the total range of possible values (single years) and, for the 30-year model run, the range calculated for 5-year subsets. We tried to highlight this more clearly in the text.

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.

Fig 13a is now mentioned in section 2.4. Despite the large year-to-year variability in the seasonal cycle, a mean seasonal cycle can be obtained (shown in Fig. 13b) that can be compared to a mean seasonal cycle and its variability as obtained from the model simulations.

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).

We tested replacing certain BPRs with the corresponding combined annual and semi-annual harmonics in the fully equipped period 05/2014 - 11/2015 (cf. Fig.8a). In those 18 months, the correlation between the daily T'_G time series derived with 4 BPRs ("$T'_{G\ EOFs\ 4\ BPRs}$") and the daily T'_G time series with the WB 300m BPR replaced ("$T'_{G\ EOFs\ 3\ BPRs}$") is high R=0.97 and the correlation with T'_G derived with 2 WB BPRs and the EB combined annual and semi-annual harmonics ("$T'_{G\ EOFs\ 2\ BPRs}$") is R=0.85. As the latter can explain ~70% of the total variance in T'_G over the fully equipped period, we are still confident to capture most of the variability in T'_G after 11/2015. As this period is only 18 months, we assume the 30% of the variance that we seem to miss with our method to be related to the intra-seasonal signals we see in the spectra for EB BP (Fig.4 d,f). However, it is correct, that with our methods we give too much weight to seasonal variability compared to other timescales, but on the other hand we believe it is the best we can make out of the currently available time series.

Having measurements only at 2 depths and the surface, the main reason to use the model, was to understand and approximate the vertical structure of V'_G (z). We do not use the model to fill data gaps or replace missing sensors.

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 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?

We tried to shorten this section as much as possible and shifted parts of the discussion to section 5. In the manuscript, we also tried to clarify, that only the vertical structure and variability of the pressure at the eastern boundary can be related to CTWs – not the vertical structure of the meridional geostrophic velocity integrated over the whole eastern basin.

Minor Comments

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

Corrected. Thanks.

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

We changed the sentences accordingly.

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."

Changed.

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

Corrected.

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

Corrected.

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

Corrected.

References

Brandt, P., Claus, M., Greatbatch, R. J., Kopte, R., Toole, J. M., Johns, W. E., and Böning, C. W.: Annual and semiannual cycle of equatorial Atlantic circulation associated with basinmode resonance. J. Phys. Oceanogr., 46, 3011–3029, https://doi.org/10.1175/JPO-D-15-0248.1, 2016.

Gasparin, F., Guinehut, S., Mao, C., Mirouze, I., Rémy, E., King, RR., Hamon, M., Reid, R., Storto, A., Le Traon, P-Y., Martin, M.J. and Masina, S.: Requirements for an integrated in situ Atlantic Ocean observing system from coordinated observing system simulation experiments. Front. Mar. Sci., 6, 83, https://doi.org/10.3389/fmars.2019.00083, 2019.