Answers to Referee #2

This is a very nice study producing for the first time an estimate of the seasonal cycle of the meridional overturning circulation in the tropical South Atlantic along 11S using a few bottom pressure measurements (BPRs and PIES) on the boundary, satellite winds, sea level from altimetry, as well as information provided from a model (INALT01). I think that this paper reads well and the analysis presented here is important. The authors make innovative use of a few moorings to reconstruct the AMOC volume transport time series.

We would like to sincerely thank this referee for their kind words, the time and effort they put into this review and the helpful suggestions improving our manuscript. Below we address the issues raised in this review by responding to the individual comments from Referee #2 in italic.

General comments:

1. I don't get a sense from the manuscript, how the amplitudes for AMOC seasonal cycle transports documented at 11S compare with those at other latitudes (i.e., 26N and 34.5S) from previous studies. There is recent some evidence from observations that AMOC amplitudes decrease northward of 34.5S (i.e., Dong et al., 2015; Frajka- Williams et al. 2019; Kersale et al., 2020), and it would be nice to know how your results fit into the context of previous studies.

As seasonal AMOC variability is closely related to variations in the regional wind regimes, we do not expect similar amplitudes or phases of the seasonal cycle of the AMOC in the Tropics compared to the Subtropics or Subpolar regions. Therefore, comparing our results directly to the seasonal cycle of the AMOC observed at RAPID or SAMBA does not seem to be extremely useful with regard to the local mechanisms, but indicate the importance of understanding the seasonal cycle when trying to extract longer-term variability from observations. Thus, we added a list of the seasonal cycle amplitudes of the AMOC at RAPID and SAMBA to the discussion: "In the Subtropics, recent estimates of the peak-to-peak amplitude of the mean seasonal cycle of the AMOC range from 4.3 Sv at 26.5°N (2004-2017; Frajka-Williams et al., 2019) to 13 Sv at 34.5°S (2014-2017; Kersale et al., 2020)." Thank you for pointing out the study by Kersale et al. (2020) – we had not been aware of it. The seasonal cycle of the AMOC at 11°S is similar in amplitude compared to SAMBA and much stronger than at RAPID.

2. It is unclear when you report a mean +/- number whether that second number is the standard deviation, the standard error, or the uncertainty. If it is the standard error or the uncertainty, some explanation is needed for how you got to that number (i.e., how many degrees of freedom did you assume).

In the submitted manuscript, all error bars plotted together with mean seasonal cycles (fig.9 (a,c,e) and fig.13 b) were standard errors $\left(=\frac{\sigma}{\sqrt{N}}\right)$ *with σ being the standard deviations and N the number of available years. In the revised version of the manuscript – as a response to other comments - we show the absolute range of possible values as dashed curves instead.*

In the text, the "±"-numbers provided together with estimates of mean transports are also standard errors $(=\frac{\sigma}{\sqrt{M_{eff}}})$ *, but with* $M_{eff}=\frac{M}{n_d}$*, where M is defined as the length of the time series and* n_d *as the decorrelation time scale. To clarify this, we added a sentence to the methods ("In the following, all mean transports are presented together with the standard error* $SE=\sigma\Big/\sqrt{N/n_d}$*, where σ is the standard deviation and nd the decorrelation time scale of the respective time series of length N.") and changed a sentence in section 5.3 to "This is within the uncertainty range of 3 Sv for the AMOC estimate of 16.2 Sv derived from a hydrographic ship section along 11° S in 1994 (Lumpkin & Speer, 2007)."*

3. Assuming those numbers are standard deviation or standard error, that represents the variability in the time series, not the uncertainty associated with your measurement strategy. Have you made a qualitative estimate of the measurement uncertainty for each daily estimate (i.e., examined the sources of error)? If so, what is that error?

Many problems associated with the measurements of the bottom pressure recorders, like the sensor drift or de-tiding, are thoroughly discussed in Kanzow et al. (2006). We followed their procedure and tried to document all the steps and choices we made during the processing of the BPRs. These are the uncertainties associated with the instruments themselves.

The second part of uncertainties arises from the observational strategy: With BP observations only at 2 depths and SLA at the surface, we had to test the uncertainties associated with this observing system and our methods to approximate the vertical structure of the basin-wide geostrophic transport above 1000m. To do this we use the INALT01 model. INALT01 was found to produce comparable variations in the Western Boundary Current transport at 11°S (Hummels et al., 2015) and, therefore, considered to be a good choice for this analysis. Of course, the model is not perfect. However, even if there are differences between the model and the reality, we can use the model "reality" to test our observational strategy meaning that with the help of the model, we were able to quantify uncertainties of the derived transports that are related to different aspects of the observing system.

That's why large parts of our manuscript are already very technical, as pointed out by Reviewer #1. We consider the effect of the uncertainty of each daily estimate on the seasonal variability of geostrophic transports small compared to the uncertainty introduced by the different aspects of the observing system, mainly because of the dominance of the annual and semi-annual variability compared to variability on other time scales.

4. The figures are really nice, however, some of the figure captions are hard for me to parse. I would suggest some streamlining of the figure caption text. Some of the colors used have names that are not familiar to everyone (i.e., petrol in Fig. 8,9 and elsewhere). The colors are fine, just the nomenclature is less common (to me) and may not be familiar to all.

We tried to streamline all of the figure captions as well as to simplify the color scheme and nomenclature.

5. What do you think is the uncertainty in your AMOC transport associated with not having information inshore of the 300m isobath? Did you examine this within the context of the model?

We tested this in the INALT01 model. There, the transport in the western boundary wedge inshore of the 300m isobath is -0.04 ± 0.002 Sv, hence negligible. And from velocity ship sections available along 11°S, we estimate even smaller transports between 0.007 -0.008 Sv for the 300m western boundary wedge.

6. One thing I was curious about is how much of the maximum northward volume transport (i.e., percent variance) does the Ekman vs. geostrophic volume transport account for in the observations and in the model? Do they have a very different breakdown? You talk about the amplitudes of each signal, so the result can possibly be inferred, but it is not explicitly stated in the manuscript.

Considering 5-year subsets of the 30 years of the INALT01 model run, the seasonal variability (repeated mean seasonal cycles) of $T_{EK\ CORE2b}$ can explain between 10-19% and $T_{G\ SIM}$ between 20-28% of the total variance in $T_{AMOC\ SIM}$. In the observations, which cover the years 2013-2018, seasonal variability of $T'_{EK\ ASCAT}$ and T'_{G} (using 5-day averages as in INALT01) can explain 13% and 48% of the total variance in T'_{AMOC}, respectively. This last number is, of course, strongly dependent on our method. Using the *combined annual and semi-annual harmonics whenever a sensor is missing results in over-weighting of seasonal variability while missing variability on other timescales.*

7. Subsections of section 2 are labelled 1.1, 1.2, ... instead of 2.1, 2.2, : : :.

Corrected.

<u>By line number:</u>

21: When you say "long Rossby waves" do you mean annual Rossby waves? If so, the timescale should be mentioned at least once in abstract and in text.

This sentence was rephrased, but we modified this formulation whenever used elsewhere.

33-34: "downward and upward motion: : : Southern Ocean" – I find this part of the sentence hard to parse, I am not sure I understood it.

Changed to "……through water mass transformation, for example, in the subpolar North Atlantic or near the Southern Ocean."

48-55: A good summary paper for all of the international efforts that you may want to include if it is helpful is Frajka-Williams et al. (2019).

Indeed, it is a really good summary paper. We added this reference.

79: The "however" in this sentence doesn't seem needed as you are not making a contrasting statement.

Deleted.

85: Suggest "however they are also" instead of "but also to be"

Changed as suggested.

92: Imbol Koungue et al. (2017) may also be a useful reference here, but you already have several.

Thanks for the suggestion. We added the reference.

99: "can even more straightforward be estimated" is a little hard to parse

Changed to "But, circulation changes in z-coordinates can also be estimated by measuring the pressure differences between the eastern and western boundary at each depth."

120-121 and 133-134: You point to Figure 2 here but I believe you meant to point to Figure 1.

Corrected.

145: It is probably hard to estimate all of the uncertainties in your methodology/ measurement strategy, but the errors due to winds seem possible to estimate given that you are comparing two different wind products in your study. You already do this to some extent in talking about how it affects your results.

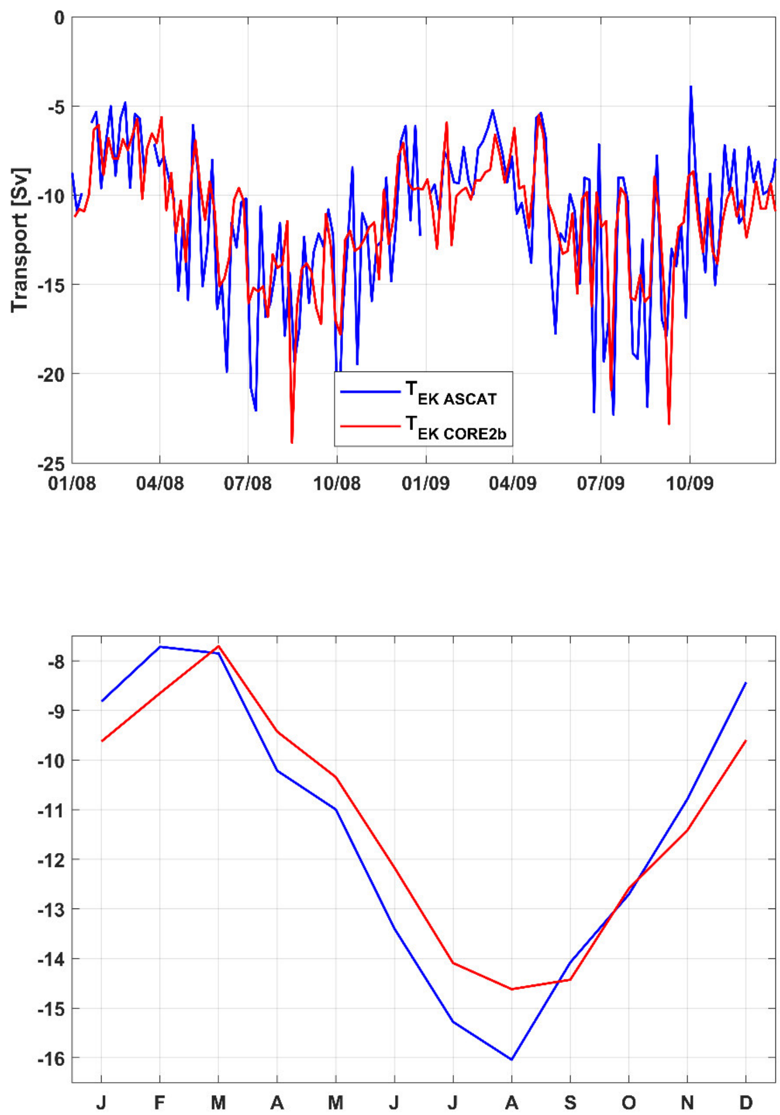

Figure 1 *Ekman transport time series (5-daily; upper panel) and mean seasonal cycles (lower panel) derived from ASCAT (blue) and CORE2b (red) wind stress for the overlapping years 2008-2009.*

In the manuscript we list the mean Ekman transports and standard errors for the two different products, we compare their respective periodograms and mean seasonal cycles together with estimate of interannual variations beyond the observational period. Additionally, Fig. 1 R2 (upper panel) shows the Ekman transport time series derived from 5-daily averages of ASCAT and 5-daily CORE2b wind stress for the overlapping years 2008-2009, which are highly correlated (R=0.82; upper panel). The mean seasonal cycles (Fig.1 R2; lower panel) calculated for the overlapping two years, show differences in amplitude and shifts up to 1 month between extrema. In comparison to the mean seasonal cycle we estimated for the upper-ocean geostrophic contribution to the AMOC seasonal cycle (cf. Fig. 9a in the manuscript), the differences between these two wind products – as estimated for 2008-2009 - are just within the uncertainty of the two methods we use to approximate the vertical structure of the geostrophic transport. We think that further investigations would be beyond the scope of this article.

151: Suggest “To estimate transports on the western boundary, we compute” instead of “We show”

Changed to “…To estimate the western boundary current transport, we compute….”.

157: Suggest “These transports are computed following methodology of Schott et al. (2005) and Hummels et al. (2015) and represent updates from their previous transport time series” or something similar.

We re-arranged the paragraph, but would like to keep the information as the methodology is slightly different in Schott et al. (2005) and Hummels et al. (2015).

177: Shouldn’t it be “from the western to the eastern boundary” in the parentheses?

Yes, correct. Thanks.

191-192: Here a reference to other studies in the South Atlantic may be beneficial (i.e., Meinen et al. 2018; Kersale et al. 2020).

As the methods between this study (using only the BPRs and SLA) are different the methods used to estimate the AMOC at SAMBA (travel time from PIES for "baroclinic" and BP for "barotropic" components), we think, adding these references here does not support the statement. However, we added Kersale et al. (2020) to the introduction when the efforts at SAMBA are introduced.

203: Are your results sensitive to your choice of 1130m as the mean depth of no motion? In the INALT01 model, how much did the depth of maximum overturning vary if you used 900m or 1300m for example?

Yes, they are, but within the range of the other methodological uncertainties. Indeed, at 11°S this depth of no motion is more variable than, for example, at 26.5°N. Most of the time (for 87% of the timesteps) it varies between 800m and 1300m depth, mainly following a seasonal cycle. Varying the level of no motion within this range (see Fig.2 R2; upper panel) changes the mean AMOC transport, which is largest when integrating to 1130m (14.1 Sv), by less than 10%. The peak-to-peak amplitude of the mean seasonal cycle (Fig.2 R2; lower panel) 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.

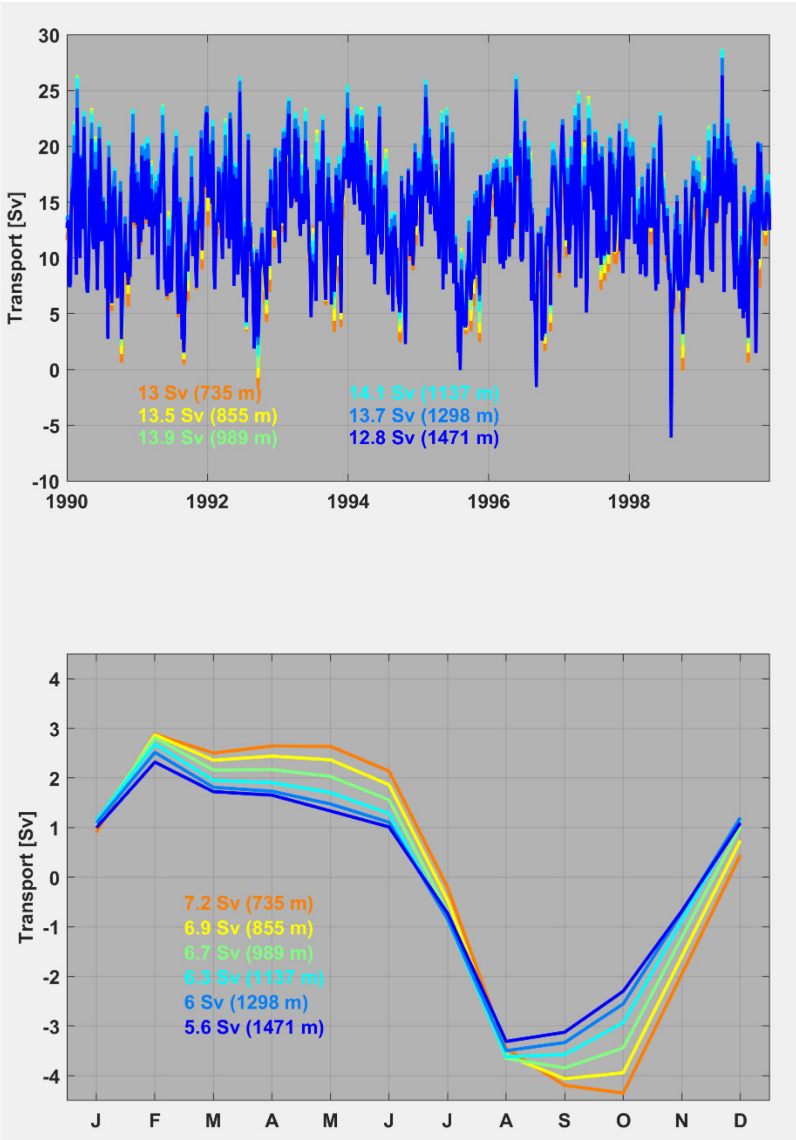

Figure 2 *AMOC transport timeseries (5-daily; upper panel) and mean seasonal cycle (lower panel) at 11°S derived from the INALT01 model velocity fields over the period 1978-2007. Different colors denote different choices of a 'level of no motion' for the integration.*

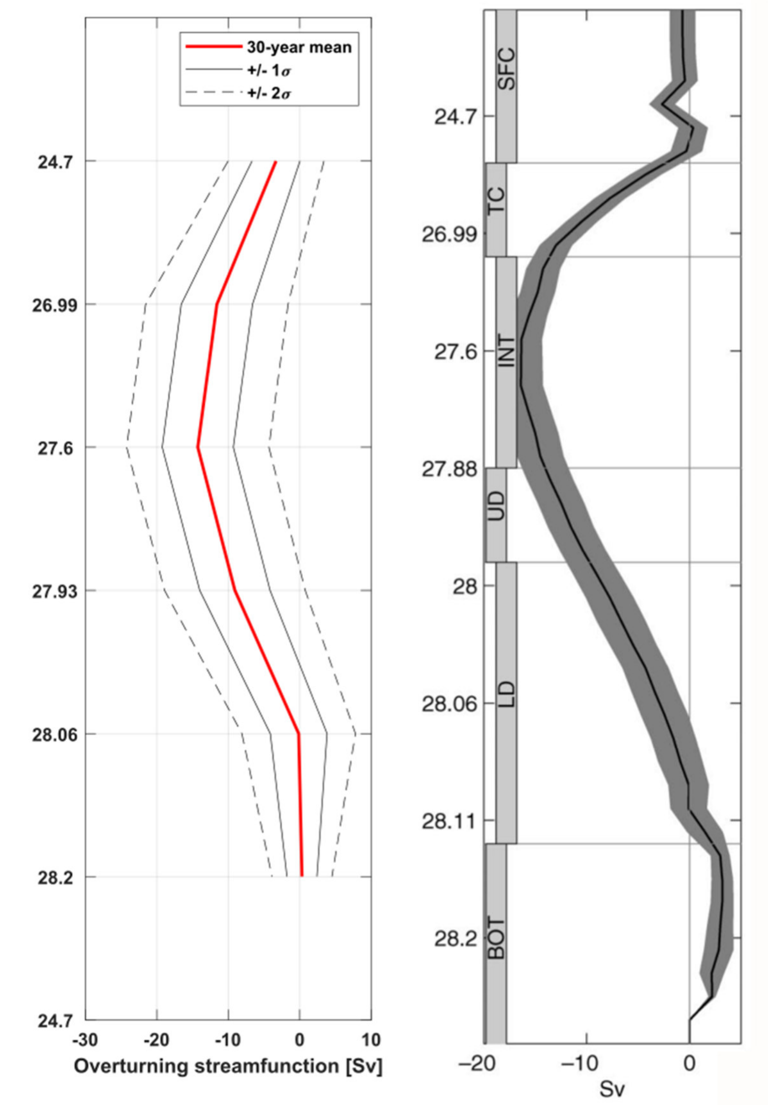

***Figure 3** Overturning streamfunction across 11°S with neutral density classes on the y-axis. Left panel: Derived from the INALT01 model velocity field over the period 1978-2007. Black curves give the one and two standard deviations around the 30-year mean. Right panel: Derived from the hydrographic WOCE sections A8 in 1994, copied directly from Lumpkin & Speer, 2003 (their Fig.8). The shading indicates standard error bars.*

209-213: I think you mention this in the paper, but some models don't have the right volume transport per unit depth structure (i.e., maxima is too shallow/narrow or too broad)? How well does INALT01's structure agree with the few hydrographic estimates of volume transport per unit depth that exist in the region? Maybe something to mention here or in Section 3 when model details are provided.

The only study we are aware of that presents an estimate of the overturning streamfunction at 11°S based on observations is by Lumpkin & Speer (2003, see Fig. 3 R2; right panel). They calculated their overturning streamfunction from the WOCE A8 section along 11°S conducted in 1994 and in defined neutral density classes. The results from INALT01 (Fig.3 R2; left panel) show good agreement regarding the vertical structure and amplitude of the overturning stream function when considering the range of possible variations around the mean over the 30-years of the model run. We added this information to section 3.

230: All of the other dates are month/year in the table, but here you have day/month/year. Suggest just using month/year.

Thanks. Corrected.

233-234: I know you don't have long enough records on eastern boundary to say how robust those%variance estimates are based on 2-years of data, but you could examine whether the % variance estimates on the western boundary are sensitive to using 2- vs. 4- years of data (i.e., look at % variance in the first two years, second two years, full record)

Thanks for the suggestion. We did: The table below lists the % of variances explained by the annual (AC), semi-annual (SAC) and combined harmonics fitted to 2-year subsets of the WB BPR timeseries.

We find the combined annual and semi-annual harmonics to always explain a similar fraction of the overall variance in the time series. But, the fractions related to the annual or semi-annual harmonic changes between the first and second halves of both BP time series (300m & 500m) at the western *boundary. It seems that during the period 2014-2015 the semi-annual cycle was dominant and during the period 2016-2017 the annual cycle. This behavior can also be seen in figure 1. While this shift from one dominant time scale to the other is interesting, it is far beyond the scope of this study.*

Explained variance [%]						
	300m WB (KPO 1134)			*500m WB (KPO 1135)*		
	AC	*SAC*	*A&SAC*	*AC*	*SAC*	*A&SAC*
2014-2015	*9,1*	*18,0*	*27,4*	*6,1*	*24,5*	*31,6*
2016-2017	*22,2*	*2,8*	*26,3*	*20,4*	*8,0*	*30,4*
full	*17,0*	*6,7*	*24,1*	*14,6*	*12,8*	*28,5*

235: Related question: Were the annual cycles from 4 years of data different from the annual cycles of 2-years of data on western boundary?

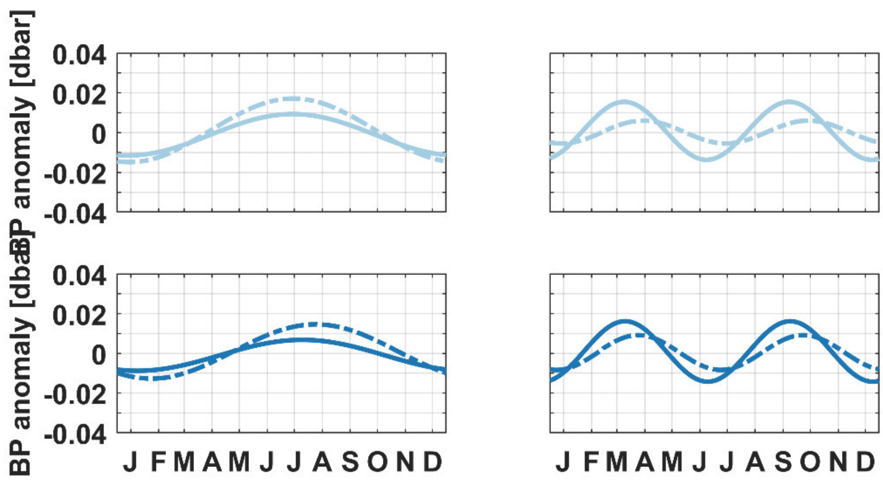

Figure 4 *Annual (left panels) and semi-annual (right panels) harmonics calculated for 2-year subsets - solid for 2014-2015 and dashed for 2016-2017 - of the available western boundary BP time series (similar to Fig. 5c,e in the manuscript) at 300m (upper panels) and 500m (lower panels).*

Shown in Fig.4 R2 are the annual and semi-annual harmonics calculated for 2-year subsets of the available western boundary BP time. Both, the annual and semi-annual harmonics show pronounced differences in their amplitudes (up to 50%) and minor differences in their phases (<1 month) between the two 2-year periods. Interestingly, the ratio of the amplitudes of the annual or semi-annual harmonic change between the two periods – please see the previous comment. However, the combined annual and semi-annual harmonics explain a similar fraction of the overall variance in both subsets of and in the full time series.

236-238: How would you estimate the uncertainty associated with only having seasonal cycle data on the eastern boundary after 11/2015? For example, if you swapped the seasonal cycle for eastern boundary time series data before 11/2015 what error do you make?

Within the fully equipped period 05/2014 - 11/2015 the correlation between the daily T'_G time series derived with 4 BPRs and the daily T'_G time series derived with WB BPRs and the EB combined annual and semi-annual harmonics is high (R=0.85) and statistically significant. The latter can explain ~70% of the total variance in T'_G over fully equipped period. 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).

236-238: You find that the eastern boundary is more important for seasonal cycle AMOC changes, which is consistent with previous studies, but how much confidence do you have in that result given you only have 2 years of data? Confidence can be derived from the analysis of INALT01 and SLA on the eastern boundary that is shown in the paper, but perhaps this is a point to articulate more strongly.

We added this sentence to the manuscript: "We derive confidence in our method from the comparison of the observed BP variations with variations in the simulated BP time series and in the SLA time series off Angola, both covering longer periods."

270: Figure 4 captioning and colors are a little confusing. Please label what is SSH, pressure 300 and 500 db.

We added this information to the y-axis labels in Fig.4 & 5.

274: How do your west coast and east coast bottom pressure findings compare with Meinen et al. (2018) where they also found energy on intraseasonal and interannual time scales in _1000 db bottom pressure data.

In contrast to the findings at 34.5°S, at 11°S, the BP time series at the eastern boundary exhibit more energy on intra-seasonal to seasonal time scales. Although, 90d and 120d peaks are found at 34.5°S as well as at 11°S, any comparison of intraseasonal variability between those two latitudes is difficult considering the differences in local wind regimes, impact or non-impact of equatorial forcing and eddy activity. Interannual variation of the SLA at the eastern boundary are, as discussed in the manuscript, thought to be related to equatorial dynamics and should not compare to findings at 34.5°S. We do also find hints for more energy on interannual timescales at the western boundary, but the time series are just not long enough for reliable estimates.

282: Are the corresponding western boundary percent variances similar in the first two years as the second two years? I know the eastern boundary has more of its variance explained by those harmonics, but this would give us some sense of the stationarity of those four years.

See our answer to some previous comments.

284: "Angola was" instead of "Angola as"

Corrected.

285: It is unclear why there are 3 phase lines in Figure 6b given that you only have annual and semi-annual harmonic. Please clarify.

*We show phase lines for the minimum of the annual harmonic in black and for the two minima of the semi-annual harmonic during a year in grey. The description was changed to "…*phases of the minima of the annual and semi-annual harmonics…*".*

292-293: If I'm not mistaken, you aren't showing the depth dependence of the western boundary phase information (i.e., Figure 6 is only for the eastern boundary) so you could say "not shown" or point to the Figure 5 left panels.

That's correct. As can be seen in Fig.5, amplitudes of the seasonal harmonics are small at the western boundary and phases very uncertain. As suggested, we added references to the corresponding panels in Figure 5 to the text.

294: You could mention here the similarities between the two 500-m deployments on the western boundary and how you get similar results. That builds more confidence in use of 2 years when you only have 2 years. (Similarly, you could break up 300-m western boundary record into two segments and compare first and second segment with full record)

We added a sentence to section 5.1: "This is also consistent between 2-year subsets of the western boundary BP time series."

297-298: Comment: It looks like the model bottom pressure seasonal cycle at 300m and 500m on the western boundary is almost non-existent, but on the eastern boundary the model captures the pressure seasonal cycle quite well.

Exactly. We rephrased this sentenced to "The model tends to overestimate the annual harmonic at the surface and generally underestimate seasonal variability in general at depth - especially at the western boundary the seasonal cycle of the simulated BP at 300 m and 500m depth is almost non-existent."

306: Here and elsewhere you should make clear if the +/-1.9 Sv is a standard deviation/ error/uncertainty.

Done.

307: If it is standard deviation suggest replacing "an Ekman transport of" with "a mean and standard deviation of Ekman transport of." or something like that.

Rephrased.

309: closing parentheses missing after (Fig. (7a,b)

Added, thanks.

314: In Figure 7c,d I would add years 2008 and 2009 on the left y-axis to help the reader easily follow which way time flows.

Done as suggested.

315: I'm confused about the sign of the wind stress. Westward wind anomaly should give you southward Ekman transport anomaly (strengthening) and you say the opposite. I think the sign of the winds is wrong, not the Ekman transport that you state. This is important to sort out.

You are right, the sign of the wind stress anomalies was wrong. Thanks for pointing this out. We corrected it.

316: Likewise, an eastward wind anomaly should give you a northward Ekman transport anomaly (weakening) and you say the opposite. I think the sign of the winds is wrong, not the Ekman transport that you state. This is important to sort out.

See previous comment.

328: You say/show that there is good agreement during the overlapping periods, but you don't give the correlation statistics. Are the correlations high and significant?

Within the period 05/2014-10/2015, when 5 BPRs were in place, we found high and statistically significant (p<0.001) correlations of 0.97 and 0.85 between the geostrophic transport time series estimated from the full set of 4 BPRs and estimated from 3 BPRs (WB 300m missing and replaced with combined annual & semi-annual harmonics) or 2 BPRs (EB 300m & 500m missing and replaced with combined annual & semi-annual harmonics), respectively.

347: "maximum northward transport in June" instead of "maximum in June"

Changed as suggested.

360: at the end of this sentence please indicate the appropriate figure panel to look at (i.e., Fig. 9e,f)

Done.

390: Suggestion "the NBUC (see Section 2.4)" so that readers are reminded how you compute NBUC.

It does not make sense to us to include the cross-reference to section 2.4 in this paragraph. We added it to the next paragraph: "Having a mooring array installed off the coast off Brazil measuring the Western Boundary Current system there (e.g. Hummels et al., 2015; see section 2.4), allows us to directly compare the seasonal variability of the NBUC in INALT01 with observations.".

415: It is hard to see the phase propagation in Figure 14b,c – perhaps add arrows or lines to better convey the sense of propagation.

Figures 14b,c were removed in the process of "streamlining" our manuscript.

419-420: Question: What is the depth of the mid Atlantic ridge in this region, is it deeper than 3000m?

Along 11°S, the top of the MAR is at about 2700m water depth. Actually, in the first submitted version of the manuscript it could be seen as a small white peak in figures 14 (b,c).

435-436: You may want to add something here like "but clearly a longer time series will help us in the future to refine these estimates" or something like that.

Added as suggested.

442: Unclear whether "They confirm" means "Kopte et al. (2018) confirmed" or that your findings in the manuscript confirm.

We changed the sentence to "Kopte et al. (2018) confirm….."

480: You could compare your results to more recent studies like Meinen et al. (2018) and Kersale et al. (2020) where they look at the seasonal cycle of the MOC at 34.5S from PIES moorings which may be relevant for your study.

Interestingly, the seasonal cycle of the geostrophic transport derive from PIES in Meinen et al. (2018) is very different to the estimate derived from ARGO/WOA presented in Dong et al. (2014). However, we deleted this discussion in order to streamline and shorten the manuscript. As we show in our study, seasonal variations in the geostrophic AMOC contribution at 11° S are driven by processes in the tropical Atlantic region. Therefore, a direct comparison between the Tropics and Subtropics on these timescales does not seem to be useful with regard to the mechanisms at work.

488: Here is one place where you can indicate if "long Rossby waves" here means "long, annual Rossby waves" (or if not annual, provide the period)

Changed to "annual".

525-526: You could indicate, that long-term PIES arrays have been deployed for a decade at 34.5S in the South Atlantic (Meinen et al. 2018; Kersale et al. 2020).

At SAMBA, the strategy to estimate the AMOC and its variability relies not only on BP measurement, but also on the acoustic travel times measured by the PIES on both sides of the basin (e.g. Meinen et al., 2017). The same approach was also used before to estimate the North Atlantic Current transport at 47° N (NOAC array; Roessler et al., 2015). This is different to our strategy mainly based on BP measurements. While at the WB we have PIES installed at 300m and 500m depth, the BP time series off Angola are measured with single BPRs.

Here, we wanted to make the point, that there is also a lot of potential in using only the BP measurements. However, it would be interesting to test how the travel times derived from the PIES installed off Brazil can add information to or reduce the uncertainty of our results. We hope to perform the respective analyses in the future.

Question: Some PIES moorings can be deployed with 4-year batteries and that makes it easier to determine pressure drift. Have you thought about doing so for future longterm deployments?

Thank you for the suggestion. We think, the best option would be to have more temporal overlapping observational periods of PIES, which however, requires more instruments. Nevertheless, we try to have the longest possible deployment periods.

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