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Discussions

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 #2

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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.

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.

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).

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?

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.

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?

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.

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

By line number:

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.

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.

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).

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

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

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

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

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

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.

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

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.

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

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).

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?

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.

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

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)

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?

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?

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.

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

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 $\sim$1000 db bottom pressure data.

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.

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

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.

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.

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)

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.

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

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.

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

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.

*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.

*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.

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?

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

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

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

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.

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

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.

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

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)

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).

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 long-term deployments?

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