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The manuscript by Schmid and Majumder uses observational estimates and an ocean model run to evaluate the transport and variability of the Brazil Current (BC). The authors have put together an interesting data set but results are inconclusive. The authors describe the BC in their data, agreeing reasonably well with previous estimates and compare their observational values with an ocean model experiment, which however is poorly described in the manuscript. The interannual variability is also poorly described, with a superficial analysis and correlation with climate indices. The description of these correlations is either very thin on dynamical explanation or simply not informative. For example, the authors correlate the BC with the AMO, noting that correlations are not robust as expected given that the AMO is an index based on North Atlantic SST anomalies.

Response:

Thank you. We expanded the analysis. Detail are provided below

Please note: all page and line numbers are based on the revised version in bold font.

General Remarks 1:

The model run is poorly described. How was the model forced? why are the authors using a reanalysis for the period 1993–2012 and then an analysis (free run?) for 2013–2015? A free run and a data assimilating model are two different models, which often have little in common. The model data, as used in the manuscript, do not add any relevant information or mechanism. I suggest to either thoroughly compare observations and model, and use the model data to perform more in-depth analysis, or simply discard model results and focus on the observational data set and its comparison with previous estimates.

Response:

Thank you. The model was not run by the authors. We revised the text to clarify this and now provide a link to more information (page 8, 1.11–19). None of the model runs are free runs (as specified in the manuscript: "... and uses the Navy Coupled Ocean Data Assimilation (NCODA) system for assimilation." We used the reanalysis version when available. For the more recent years, the reanalysis was not available. Therefore, we used the analysis to get a time series that covers the full time period. No discontinuity is detected at the transition from the reanalysis to the analysis. We expanded the text related to the model throughout the manuscript.

General Remarks 2:

The observational data sets, as presented, are confusing. Temperature and Salinity go from 2000 to 2015. Velocities from 1989 to 2016. Satellite data from 1993 to 2015. Most figures are presented for the period 1993–2015 so you should just present that common period.

Response:

Thank you, the text has been expanded (page 7, 1.6–24; page 8, 1.1–4).

The summary is:

We use hydrography and altimetry from 2000–2015 (the period for which we had Argo and SSH data available) to derive the relationship between dynamic height and SSH (SSH ended in 12/2015 at the time we started the computations). We then use the SSH time series 1993–2015 to derive the synthetic dynamic height for the longer time period based on the derived relationship. For the trajectories, we used as much as possible to get a more robust monthly climatology.

Minor points:

page 2, line 4: The mean transport of the Brazil Current RANGES from 3.8 ...

Response: thank you, revised

page 2, line 13: It would be useful to state in the abstract what is the main result of the EOF analysis.

Response: thank you, done (page 2, 1.13–16)

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page 4 line 10: not sure what you mean here. The Western Boundary Current is not in Sverdrup balance.

Response: Agreed. What we meant is, that the subtropical gyre is largely governed by the Sverdrup Balance. The western boundary current only closes the gyre. We rephrased this (page 4, l.10). What complicates things is that the SEC carries water from the Indian Ocean into the western South Atlantic and bifurcates at the western boundary.

page 8 line 9: what are all these experiments? no information is given on the model, how it was forced, for how long and whether it is reproducing the BC realistically.

Response: Thank you. These experiments are made available online (the model is not run by us). The paragraph was expanded to clarify this and now points to the web page that makes the experiments available to allow readers to obtain more information (page 8, l.11-19).

page 9 line 8-10: some text is missing here or the English should be improved.

Response: Thank you - in the process of changing the way the references are shown (transition to the template for Ocean Sciences) the beginning of the sentence starting in the middle of line 9 got lost.

page 9 line 12: it seems to me from Fig.3 that the model produces a vigorous BC further north, and that 25S would be more appropriate.

Response: thank you, HYCOM shows strengthening of the Brazil Current between 25S and 28S. We revised the text (page 10, l.13-16).

page 9 line 16: I think you should clearly state that the model seems to produce feeding westward currents that are located further north, say at 26S and 21S.

Response:

Thank you, there indeed is a shift with respect to the latitudes of the westward flow. A sentence was added about this (page 10, l.13-16).

page 9 line 20: what is the full period?

Response: Thank you. We added the time period at the end of paragraph 4 in section 2 and added "of 23 years" to the end of the sentence (page 10, l.24).

page 9 line 21: Who is they?

Response: Thank you. "they" = "the layer thicknesses" from the previous sentence. The sentence was revised (page 11, l.1).

page 11 line 16: (Figure 4) It is also true that previous estimates seem to agree better with the model and fall within the simulated standard deviation (red) rather than the observational data set (black). Given that the model has a very fine horizontal resolution, and presumably it is simulating most eddy activity in the region, it could well be that the observational data are giving you too-weak transports.

Response: Yes, the model is eddy-resolving. However, it seems like the other factors are more important in the comparison of quasi-synoptic surveys (from previous studies) with monthly means (from Argo & SSH and HYCOM).

page 12 line 6: TOPEX/POSEIDON

Response: Thank you, corrected (page 13, l.10).

page 12 line 10: You have concluded before that the BC is not well defined north of 25S as it is not fed by the westward current. So why discuss here the "northern region" north of 25S?

Response: Thank you, we rewrote parts of the first paragraph in section 3 (page 10, l.2-11) to make it more clear that the BC is poorly defined in the mean field because of the eddy variability, shifts in its location as well as

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its relative weakness. Even so this is the case, one can estimate the BC transport and study its variability.

 page 13 line 20: showing the EKE from the obs and the model seems very important so I don't understand why the authors have decided not to show this.

Response: thank you. We did not show the EKE fields, in the manuscript because we already had a lot of figures. To show them without making the manuscript much longer we decided to present EKE figures as a supplement.

 page 14 line 7: why have you chosen these three latitudes: 24S, 35S and 38S?

Response: We picked one latitude far north of the confluence in the regime dominated by small transports and two in the vicinity of the confluence (the regime dominated by high transports and high variability). This information has been added to the manuscript (page 15, l. 8-10).

 page 15 line 5: you give an estimate of 7.9 to 26.2 Sv, but these values are not reproduced in Fig.5, which shows a max value of around 21Sv. If it is due to filtering the authors should clearly state that they are talking about unfiltered data.

Response: Thank you for catching this. The numbers in the table for the observations at this latitude were wrong. While revising the table we also redid the computations focused on the interannual variability. In addition to that, we cross-checked all numbers and made corrections wherever necessary.

 page 15 line 19-22: sentence very difficult to read. I suggest rewriting this paragraph.

Response: Thank you, we rephrased this (page 17, l.11-16).

 page 18 line 1-6: This is very superficial and not useful. Maybe a power spectrum could give an indication of the variability and its significance?

Response:
 We expanded the part about the wavelet analysis in the previous section and rewrote large parts of this section (page 19-22) including the first paragraph.

 page 18 line 8: why did you choose these indices: SAM, NINO3.4 and AMO? why not local interannual indices like the Atlantic Meridional Mode or the South Atlantic dipole indices? Later, little dynamical explanation is given for the correlation, or lack of, between these indices and the BC.

Response:
 We choose SAM because it includes observations close enough to the subtropics to potentially have an effect on the gyre strength. We choose NINO3.4 because of studies by others on the teleconnections between the tropical Pacific and the South Atlantic.

We agree that including AMO does not provide much insight and thus removed it.

Thank you for suggesting to look at the more local indexes. We added SASD Mode (SST anomalies averaged within 30-40S, 10-30W minus those averaged within 15-25S, 0-20W) to this study.

We also looked into a potential role of the Atlantic Meridional Mode and detected no significant correlation. This was not added.

(page 19-22)

 page 19 line 9: the AMO is a North Atlantic index. So why did you use it? what were you expecting to find? I suggest removing any discussion on the AMO.

Response: Thank you, as stated above, AMO was removed.

 page 19 line 10-12: I don't find this conclusion convincing. You find the

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largest correlations where the BC is weak and not entirely formed. At 24S the BC has not received most of the westward current flow. You should look at the core of the BC and find significant correlations.

Response:

We revised most of this section and expanded it (page 19-22).

page 21 line 12: what is the mechanism proposed by Lopez et al.(2016)?

Response:

We revised most of this section and expanded it to explain in more detail what is going on (page 22-25).

page 21 line 12: I find it hard to find the 'scope' of this manuscript. What are the main goals and conclusions of this manuscript? they should be stated and clearly presented.

Response:

Thank you. We expanded the analysis, revised large parts of sections 4.4 (page 19-22) and 4.5 (page 22-25) and updated the conclusion and abstract.

page 22 line 3: I would remove the model data as they do not add anything to the conclusions.

Response:

We kept the model, expanded the use and revised the conclusions and the abstract.

page 22 line 22: very vague statement on the variability of the current.

Response:

We added a sentence to this paragraph to make it more clear (page 27, 1.5-6).

page 23 line 8: again, please remove any analysis related with the AMO

Response: Done.