

Answer to: Anonymous Referee #3

Thank you for your detailed, helpful suggestions on our manuscript. As we noted in the letter to the editor, we found the comments of the editor and reviewers to be very constructive, and we feel that the revised manuscript is greatly improved now that we have addressed these suggestions.

Below we detail the changes we have made to the manuscript, addressing point by point all the issues raised. The comments from the reviewers are in bold, while our responses are interspersed between the comments in non-bold text. All line numbers indicated in our responses correspond to the new version of the paper.

General comments

There is little (or no) information about configuration and set up of the instruments and calibration issues, how have the authors performed the quality control of the different types of data, or which are the associated error bars of the different measured variables.

In the Data and Methods section, as suggested we expanded the description of the configuration, set up, and calibration of the instruments used in our analysis (Microcats, ADCP, CRIES, and GEM field). We have now detailed the calibration process and specify the errors associated to each variables (l. 167-171). The information about the ADCP configurations has been added in Table I.

For the CRIES/GEM analysis the robustness of the relation between vertical profiles of temperature, salinity, density and measured tau is not shown, even though it is key to the analysis presented comparing mooring and CRIES data. For instance, it would be good to know how does the GEM perform in the deep ocean, how many deep casts are used to build the GEM, etc.

We agree, given the importance of this point, a figure is perhaps required. We have now added this figure (Figure 2), and we've added discussion about the scatter around the GEM fields (Figure 2c, d) to the text of the paper (l. 134-138). As the reviewer suggests, in the deep ocean, the scatter around the GEM field is a larger fraction of the observed variability, however this is because the signals at depth are smaller rather than because the scatter around the GEM field itself is getting significantly larger.

To illustrate how many casts are available, we've added gray lines to Figure 2c, d.

The manuscript relies on an unpublished method (Laxenaire et al. 2017 submitted) for the eddy analyses. I suggest that the eddy detection method should be carefully explained to provide more guidance to the reader (for example, lines 128 and 154).

It is true than the eddy detection algorithm of Laxenaire et al. (submitted, 2018) is still in review at this time as the paper had been re-submitted to JGR. We believe that method is good, but to address this comment and other comments from the reviewers, we chose a version of the algorithm comparable with the ones of Chaigneau et al. (2008, 2009) and Pegliasco et al. (2015). We have also added, in the Data and method section, a description of the eddy detection method and the main changes compared with the original algorithms (l. 178-188).

[...]Most of the introduction is dedicated to the MOC and MOC arrays, but there is very little explained about the connection between the findings in this study and MOC variability. The main objective of the manuscript should be clearly stated and the introduction must be targeted to provide the necessary background for what will be presented in the following sections. If the focus of the paper will be on the local dynamics (as the title and results suggest) more background on regional aspects is needed.

As suggested, we have considerably revised and shortened the description of the MOC (l. 34-36), focusing mainly introducing the South Atlantic MOC (SAMOC) observing systems used in this study (l. 86-94).

Following your advice, we have also introduced in more detail the dynamical background of the Cape Basin, the processes and the local water masses exchanges associated with eddies, dipoles, and filaments (l. 64-83).

I suggest putting this work in the context of the SAMOC/SAMBA project strategy/objectives making it clear what the eastern part of the SAMBA array will address and how is it likely to contribute to improve the understanding of MOC variability and, specially, on which time scales.

We added some additional words in the Introduction to explain why we mention the MOC variability (l. 45-47), and at the end of the discussion (l. 509-516) we add some additional words explaining the contribution of the mesoscale nonlinear dynamics we are discussing to larger scale processes such as the AMOC.

The abstract needs to be revised in order to reflect the main findings of this work.

It appears that our initial version of the abstract did not reflect the main findings of our work, our apologies. The abstract has been revised in the new version in order to focus on the key results of the paper.

Specific comments

114: change to South Atlantic Meridional Overturning circulation Basin-wide Array (SAMBA).

We changed the text accordingly (l. 14)

Please include previous literature regarding the main circulation patterns/boundary current systems in the Introduction. A schematic of the main currents/circulation patterns on Figure 1 could be useful.

As explained in our reply to the general comments above, we have now introduced the main circulation patterns. With this new introduction, we believe that the readers have enough information about the dynamics, so we decided to keep Figure 1 like this for the sake of simplicity, apart from the addition of the C-line of Laxenaire et al. (submitted, 2018).

157: there are updated ACC transport estimates through Drake Passage

We agree, we've added updated literature linked to transport estimates through Drake Passage. l. 90

164: It is not clear how the western boundary buoyancy anomalies have been looked at. Did these studies look at the mesoscale eddy field?

This sentence has been rewritten to clarify this point (l. 97-99): "The transport and water mass anomalies associated with the deep current and migrating eddies near the western boundary have been well studied (e.g., Meinen et al., 2012, Meinen et al., 2017; Valla et al., submitted, 2018) but the eastern boundary anomalies have not yet been examined along 34.5°S."

185: '...some accurate information' I suggest rephrasing this to try to make a better statement.

We modified this sentence, pointing out to the experimental evidences of advected mesoscale eddies through the mooring line l. 102-104.

186-88: This a quite strong statement. I suggest leaving it for the outlook at the end of the manuscript with some explanation for why do you think that is true.

We followed the reviewer's recommendation and moved this statement at the end of the manuscript with additional explanations. (l. 509-516)

193: The usage of the term 'tall' moorings is not clear. Would 'full depth' mooring suit better?

"Tall" was replaced with "Full-depth". We have carefully gone through the manuscript to make sure that we consistently use this new term.

1108: 'destroyed and lost'. Maybe it was just 'lost'?

The PIES crashed hard against the hull of the vessel before being lost back into the sea, and subsequent attempts to contact it failed, so we are fairly confident that the glass sphere was breached and the instrument was destroyed. Nevertheless, we have simplified this sentence as the ultimate state of that instrument is not essential to this paper (l. 127-128).

1113: replace 'various' with 'five'. Please add information about horizontal resolution of the hydrographic sections.

"Various" was replaced with "Five". We added information about horizontal resolution of the hydrographic sections to the new version of the manuscript (l. 162-165).

I116: Salinity and density are not directly ‘measured’ by the CTD. Please clarify.

We agree, we have rephrased this statement (l. 160-162).

I123: It may be worth clarifying here that satellite will give information of the dynamics at the surface.

This has been done accordingly l. 174.

I135-152: Part of this could go to the Introduction.

Part of the original paragraph 3.1 has been moved in the introduction as suggested.

I153: what is the ‘C line’? Laxenaire et al method should be explained if their paper is still unpublished during the revision of this manuscript.

We agree, this is a great point. We have added the position of the C-line to Figure 1, and we have added some explanations in the text as well l. 226-227: “This line extends from the southernmost tip of Africa (Cape Agulhas) and, after crossing various seamounts, ends at 45°S in the Southern Ocean (Figure 1).”

I169: How do you know this? Which is the approximate depth of the Ekman layer here? Are there any seasonal variations?

CTD observations and shipboard ADCP measurements conducted during the various cruises along the eastern section of the SAMBA transect have been used to estimate the mean depth of the Ekman layer. From these measurements, we have noticed that the mean ADCP shear deviates from the mean geostrophic shear above 36 m depth, indicating the presence of ageostrophic wind-driven Ekman currents. We have used this criterion to define the base of the mean Ekman layer along the section as 36-m depth. The estimated mean value of the Ekman layer depth has been added to the text l. 198.

Concerning the seasonal variations, we compared the values of the Ekman layer depth splitting the casts according to the time of the year (two different seasons: September and December), and we did not find any seasonal variability.

I180-186: Which type of filtering is applied to compare the velocities from altimetry and velocity from ADCPs? How is aliasing by the sampling resolution of the altimetry, addressed? The conclusion about the validation of the altimetry-derived velocities with the ADCP velocities should be approached with more caution, given the values for the correlation coefficients. A correlation coefficient of 0.7 leaves about 50% of the variance unexplained. It could be useful to show a comparison between the time series of the velocities from altimetry and from the ADCP to provide more faith in the validation analysis.

It appears that our initial version of the paper was not clear on this issue, our apologies. For the altimetry data, we chose the closest point from the mooring position to do the comparison. We do not do any additional treatment to compare the data. We agree that our conclusion were too radical according to the different values of the correlation coefficient, and we now addressed them with more caution. We thought that another figure to compare the time series of the velocities from the altimetry and the ADCP would not bring new elements as the comparison is done for each case studies in Figures 7, 8 and 9. Finally, the presence of the structures at the mooring is also supported by the Lilly and Rhines (2002) method, used to detect eddies, filaments and dipole from the mooring measurements.

I189: Does the upper layer include subsurface waters? Please give a depth range

We have modified this sentence to make clear that we are speaking about the upper-layer dynamics between 40 and 60 m depth (l. 221-222).

I229: Given the text structure below, I would replace ‘four’ case studies by ‘three’ case studies

We agree, we consider only three case studies in the new manuscript.

I276-277: Is ARMW a newly identified water mass or has it been identified before in all the studies mentioned there?

This sentence has been rewritten to clarify this point (l. 353-356).

I266-267: ‘. . .to recover the regional water masses for each mooring’. This is not clear, what is shown in Figure 10? Is it the depth range occupied by each water mass? Figure 10 is not very informative. The main information from the figure could go to a Table (see below).

We agree. Figure 10 was deleted based on our changes due to your comment and some of the other reviewer comments.

I270-282: It would be nice to condense the information about water masses on a Table to make it easier for the reader to follow.

We agree with the reviewer and to clarify this paragraph we synthesized all the information about the characteristics of the water masses in Table V.

I294 -295: Please add an interpretation: “Interestingly, no A-AAIW is observed in the mooring records during the whole 450-day deployment.”

We did not expect to find A-AAIW, but the absence of this water mass during the whole record confirms the different varieties of AAIW characterized by Rusciano et al. (2012). Nevertheless, we have deleted this sentence as it is not essential to this paper.

I310: Please add an interpretation about the low correlation coefficients found for the deeper levels.

We have added an interpretation about the low correlation coefficients. **I.390-391**

I311: There are correlation coefficients values before in the text (Section 3.3), the description of the method for estimating their significance should be provided before.

We provided this information with the first estimate of correlation coefficients **I. 206-210.**

I318-322: It is not clear with respect to which means are these percent changes estimated from.

We totally agree, the percent of changes on temperature and salinity were not clear and relevant in the previous version of the manuscript. We followed your suggestion and the ones of the editor in modifying this figure, i.e. we modified this figure to illustrate conservative temperature anomalies in °C and absolute salinity in g kg^{-1} with neutral density as the vertical coordinate instead of depth (Figure 11). The temperature and salinity anomalies due to the presence of the structures are calculated relative to the hydrographic properties in “normal conditions” (just before each event occurred).

I353: I suggest changing ‘high spatial and temporal scales’ with ‘high spatial and temporal resolution’

Text has been corrected as suggested **I. 429-430**

I365: Please add references for the previous estimates

We now provide the previous estimates with references related to this issue (**I. 440-442**)

I366-367: ‘accurate description’. . . this needs more evidence

“Accurate” was replaced by “reasonable” **I. 443**

I396: add ‘Ocean’ after ‘Indian’

Fixed as suggested **I. 466**

I408: what is the decorrelation scale number here?

We added the decorrelation length scale to the new version of the manuscript (**I. 507**).

I410: ‘these first independent observations comparisons’ Do you mean between ADCP and altimetry, and between moorings/CPIES?

We meant the first comparison between full-depth-moorings and CPIES data sets. The sentence has been improved accordingly **I. 509-510.**

I411: ‘some confidence’ Please try to rephrase.

“confidence” was replaced by “evidences” l. 510

In the Discussion, I think that a better case could be made describing which are the essential new findings of this study compared with previous studies, and what have we learned from this new data set.

We agree with the reviewer. The discussion have changed to highlight the main findings of this study.

l411-413: It is not clear how this will be accomplished. How is the eastern boundary current system variability is going to be estimated from the present array configuration? How are these mesoscale features likely to impact MOC strength/variability? For instance, do you expect them to average out (or not) for the basin-wide MOC calculation?

At the end of the discussion (l. 509-516) we add some additional words explaining the contribution of the mesoscale nonlinear dynamics we are discussing to larger scale processes such as the AMOC. We also add information about the array configuration that we will used to analyze the the eastern boundary current system variability.

General comments (2)

In general: To define an acronym first it is necessary to use the full words and then in brackets the acronym should be defined. For instance, Acoustic Doppler Profiler (ADCP).

Done accordingly. We carefully checked the different acronyms throughout the manuscript.

I find it confusing that the authors use the same nomenclature for an instrument's site (for instance C1) and an eddy indentified as a cyclone (also C1).

We agree. We have changed the nomenclature for the instrument's site. The CPIES are identified in the new version of the manuscript by the letters: P1, P2, P3 and P4.

Figure 4: For the caption I suggest adding 'for selected dates between' September 19, 2014 and October 15, 2015 ' . . .

Done accordingly.

Figures 6-8: This is perhaps a matter of style, but I strongly suggest providing complete figure captions.

In the revised version of the manuscript, we provided complete figure captions.

Figure 9: Please specify that this diagram corresponds to the eastern part of the SAMBA array

Fixed as suggested

Figure 11: For identifying water masses it is more appropriate to use neutral density as the vertical coordinate instead of depth.

Thanks for pointing it out. As noted in our response to one of the earlier comments, we have changed this figure to represent the anomalies versus neutral density.