Answer to: Anonymous Referee #2

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

As an example, the abstract claims that the data of the SAMBA array are used to assess the "nonlinear, mesoscale dynamics of the Cape Basin". I am having difficulty to find where exactly in the manuscript such nonlinear dynamics are described and studied. Another example found in the abstract is that "future investigations with longer time series ... will ... ultimately improve our understanding of the strength and variability of the Meridional Overturning Circulation". I am really wondering where in this article such a thing is suggested from the results.

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 to order to clarify these different points better focus on the key results of the paper. We have added a bit more discussion of the “non-linear” terms in response to some of the other comments from the reviewers, as we have now evaluate the Rossby number associated to each feature (Table IV) and added in the text (**l. 475-481**) a discussion about its meaning. Also, we have considerably shortened the description of the AMOC in the paper, and so we removed the sentence you mentioned from the abstract. We kept at the end of the discussion a general perspective of this work in the context of Meridional Overturning Circulation future studies.

It is useful to provide some context about the MOC and the SAMBA experiment, but there is too much of it since this is not the topic of the paper. More background should be given about the hydrography and oceanography of the region, as well as the characteristics of eddies in this region.

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)**.

Another aspect of the study that I am very concerned about is the use of the eddy detection algorithm of Laxenaire et 2017, a paper not yet accepted for publication at the time of submission of this paper. An example of puzzling result is found in Figure 7: are we to understand that anticyclonic eddy A19 has grown in size by an order of magnitude from 22-April-2015 to 26-April-2015, and that cyclonic eddy C14 suddenly appeared between these dates? Even if C14 is generated at the Benguela front, it does not appear at all like a coherent structure shed from the Benguela Current. I realize that the Cape Cauldron is a very energetic region where tracking eddies (in the sense of coherent structures) is difficult, but maybe another algorithm could be tested against the one of Laxenaire? Or maybe the atlas of eddies by Chelton et al. could be used?

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, the main changes compared with these original algorithms **(l. 178-188)**. This has led to some small changes in our interpretation (**l. 259-260**; **l. 265-266**) mainly associated with A19 that you mentioned in your comment. In the new version of the algorithm, A19 is not considered as a new structure but is still tracked as A16. This anticyclonic eddy shows a better coherence with time and similar size at the two dates mentioned (Figure 9). On April 26, the cyclonic eddy C14 is already detected with the new version of the algorithm (Figure 9-a). More details about the generation of

this eddy have been added to the text (**l. 265-266**). Finally, the presence of the structures at the mooring is supported by the Lilly and Rhines (2002) method, used to to detect eddies, filaments and dipole from the mooring measurements. More details about this method and its application have been added to the text in several places. **l. 143-157** and **l. 285-308.**

Also, does not it look observations at mooring M4 are more representative of the meandering Benguela current?
We agree with the reviewer, however this fact is not inconsistent with the presence of mesoscale eddies as most of the energy of the Benguela Current is supplied by eddy fluxes (Matano and Beier, 2003). South of 30°S, the observed Benguela Current is characterized by two ‘‘streams’’ separated by a conspicuous cyclonic meander (Veitch et al., 2010). The offshore stream, passing through mooring M4 is situated on the Agulhas eddy corridor (Garzoli and Gordon 1996). Indeed, passing anticyclonic eddies enhance the mean northwestward flow in this region. The meandering nature of the mean flow is therefore a manifestation of the preferential path of transient eddies in this region.

Veitch, J., Penven, P., & Shillington, F. (2010). Modeling equilibrium dynamics of the Benguela Current System. Journal of Physical Oceanography, 40(9), 1942-1964.

Another and final aspect of the study that I find worrying are the conclusions based on the temperature and salinity changes (Figure 11). I understand that the authors attempt to show more clearly the potential changes from mooring and CPIES data by showing percentages of changes (compared to which means?), but are we to be compelled by changes of salinity of the order of less than 0.5%? Or even temperature changes of the order of less than 10%?). Often, when alleged eddies are passing the moorings, I am not seeing much in the time series of Figure 3. How large are these changes compared to, let’s say, the total standard deviation of the records, or again the standard deviations outside of eddy "events"?
We totally agree, the percent of changes on temperature and salinity were not relevant in the previous version of the manuscript. We followed the suggestions of the editor and other reviewers 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 maximal anomalies reach 0.5°C in temperature and 0.15 kg m^{-3} in salinity (**l. 397-425**), which are much more significant for our conclusions. 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).
To have an idea about the changes of T/S compared to the all time series, the T/S diagrams of the time series for each case study have been embedded/added to the plot of the background T/S in the new version of the manuscript (Figure 10). We have added text to explain our assessments in the new version. (**l. 367-380**)

There have been methods developed to detect eddies in mooring data, the paper Lilly and Rhines (2002).
We thank the reviewer for suggesting this additional eddy detection technique. We now applied the technique of Lilly and Rhines (2002) to detect eddies, filaments and dipole from the mooring measurements. More details about this method and its application have been added to the text in several places. **l. 143-157** and **l. 285-308.**

The CPIES data should also be able to tell you a lot more about the processes taking place such as transport, even if, as you state, the decorrelation length state is smaller (what is it?)
We added the decorrelation length state to the new version of the manuscript (**l. 505**). We totally agree with the reviewer that the CPIES data can be a lot more exploited for other processes as transport estimation. Nevertheless, this analysis is beyond the scope of the present study. We actually working on the variability of the Eastern Boundary currents, where the full data set from the 8 CPIES will be analyzed in this way.

Specific comments

l15: nonlinear: what is demonstrated to be non linear here?
As explained in our reply to the general comments above, we have added a bit more discussion of the “non-linear” terms in response to some of the other comments from the reviewers, as we have now evaluate the

Rossby number associated to each feature (Table IV) and added in the text (**l. 475-481**) a discussion about its meaning.

l18: these mesoscale features: which ones?
This sentence have been rewritten to clarify this point **(l. 18).**
l24: substantial role of these mesoscale features: you have not shown how large are the "eddy"-related changes in T and S compared to their sources of variability.
We agree, we now address this point by evaluating the anomalies in density space (Figure 11), allowing us to more explicitly distinguish which signals are associated with a thermohaline anomaly or a simple heave or down-shift of the isopycnal layers.

l71: lead -> led?
We agree, we deleted this sentence based on our changes due to some of the other reviewer comments.

l101: moorings have a sub-surface depth of 500 m: I understand what you mean but this is an odd way of describing the mooring.
This sentence have been rewritten. (**l. 118-120**)

l115-116: the authors know very well that a CTD does not measure salinity and density, please rephrase.
We agree, we have rephrased this statement (**l. 160-162**).

l140: The Agulhas is traditionally said to retroflect, not reflect
Thanks for pointing this out, we changed accordingly. **l. 40**

l165-66: what are the tolerances given? standard deviations or standard errors of the mean or confidence intervals? please specify.
The sentence has been rephrased to specify the given tolerances **l. 193**

l186: accurate: i would temper this by something like "reasonable". Do not forget that a correlation of 0.72 implies that about only 52% of the variance can be explained.
Thanks for pointing it out. Text has been corrected as suggested **l. 219**.

Figure 5 & 6 and others: please add units close to colorbars
We added units on the top of each colorbar for all the figures.

Figure 10 is introduced before Figure 9? and Figure 10 is really not insightful. Figure 10 should contain the definition of the water mass acronyms.
We totally agree. Figure 10 was deleted based on our changes due to your comment and some of the other reviewer comments.

l283: stabilizes? what do you mean? is present?
“Stabilizes” was replaced with “is present” (**l. 363**)

Figure 11: you should state in the caption that the CPIES estimate for the cyclone is missing
In the new version of the manuscript, we chose another cyclonic eddy as its signature was much clearer in the moorings data with Lilly and Rhines (2002) method. At that time, both the SBE Microcat's and the CPIES were recording.

l331: after its recovery and re-deployment: do you mean before the re-deployment?
We agree. We delete this sentence as it was linked to results not presented in the new version of the manuscript.

l365: what are the previous estimates?
We now provide the previous estimates with references related to this issue (**l. 440-442**)

l408: the decorrelation length scale is smaller than this distance: and what is it?
We added this information to the new version of the manuscript (**l. 507**).

line 411-413: "Future investigations with longer time series at these existing sites will lead to a better understanding of the eastern boundary current variability and Indo-Atlantic exchanges, and ultimately improve our understanding of the strength and variability of the AMOC." Ok, this is for further investigation, but you are not even hinting how this could be done. How are your mooring observations going to inform you on the variability of the AMOC? How are you results on water masses and eddies going to be utilized when constructing the trans-basin array. If you want to talk about the AMOC this is what you need to discuss.
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.