<u>Answer to: Anonymous Referee #1</u>

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.

<u>General comments</u>

First of all, I think the introduction needs re-writing. It is irritating that so much is written about MOC and the different MOC programs in the Atlantic and so little about the problem the study is concerned with. Why do we need to know that there is a MOVE or RAPID when analysing and interpreting TS anomalies in eddies in the Cape Basin? – How does that really is connected to the study here? If it would be I would expect that at the end of the paper I know how this study contributes to the problem. Maybe I overlooked it – but I can't find a contribution of the findings presented here to the MOC. I understand that the complete SAMBA/SAMOC array (full basin width) aims to measure the MOC variability at the respective latitude but really this study is looking into a "local" aspect of hydrography and flow variability in the Cape Basin. It just makes use of the mooring infrastructure/data. I strongly suggest to shorten the AMOC part (to maybe one sentence only) but to expand a lot more on what the study is concerned with (and what is also reflected in the title): A LOCAL study on the structure of eddies (on the mesoscale only) in the Cape Basin. To me this would also mean to expand in the introduction on eddy/eddy interaction (dipole discussion), eddy detecting in mooring data (quite a bit of studies have worked on that), calculation of anomalies (please pay attention to pressure versus density space (see comments below).
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)**.
We added some additional words in the Introduction to explain why we mention this (**l. 44-46**), and at the end of the discussion **(l. 512-516)** we add some additional words explaining the contribution of the mesoscale nonlinear dynamics we are discussing to larger scale processes such as the AMOC.
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)**. Finally, we introduce the different studies observing eddy with mooring data focusing on those using the technique of Lilly and Rhines (2002) **(l. 104-106)**.

I would also suggest a brief introduction to GEMS in this set up – I understand that you used the T/S/pressure at the grid points (moored instrumentation) to set the local sound velocity points – the unknown would then most of the water column upward from the ADCP (upper 500m or so) – and which is were likely a lot of warm water (and thus sound speed) variability lies? Do you use satellite SST to give you one sound speed gridpoint at the surface?
It appears that our initial version of the paper was not clear on this issue, our apologies. The T/S/P data from the moorings are not used in building the GEM fields. Instead, hydrographic profiles from the region (CTD and Argo) are used to build these simple two-dimensional look-up tables. Based on your suggestion, the introduction to the GEMs creation has been rewritten to clarify this point and moved to the Data and Methods section **(l. 131-134)**. We have also added a new figure presenting the GEM fields of temperature and salinity (Figure 2), and we've added discussion about the scatter around the GEM fields (i.e. the estimated accuracy of the GEM values; see the new Figure 2c,d) to the text of the paper **(l. 134-138)**.

<u>Specific comments</u>

l40-50: why do we need to know these details?

l55-60: if you decide to leave in this part it would be useful to introduce HOW the mechanisms are behind MOC and water mass transformation and how precisely does that link to the analysis of eddies in the Cape Basin?
We have considerably shortened this description as suggested, and we have introduced some additional wording to explain the contribution of the mesoscale nonlinear dynamics we discuss in this paper to larger scale processes like the MOC **(l. 34-36).**

l64: are these "anomalies" similar to the anomalies that will be studies here? Or is the first a transport anomaly in the DWBC (western array) and here local anomalies due to mesoscale eddies in the eastern boundary current are studied?
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."

l88: I am missing all references to research on the circulation elements along the eastern boundary and in the Cape Basin. Are there undercurrents? Is there a coastal current? Does it connect to the north poleward or south? I would expect that the South Africans and Namibians have worked quite a bit here and know about the local/boundary circulation? l89: Given the discussion later about eddies, dipoles, filaments etc. could you please introduce a bit more general wisdom on eddies and eddy/eddy interaction and including filamentation and how all that links to water mass transformation (isopycnal heave versus water mass anomalies)?
As suggested, we have added a number of citations to literature linked to the dynamics of the Cape Basin and the processes and the local water mass exchanges associated with eddies, dipoles, and filaments **(l. 64-83)**.

l103: the MicroCat's have no pressure? How did you add pressure information? (it is required for calculating salinity). What will be the uncertainty in Salinity? Please indicate that you did not use the oxygen (or did you?)
We agree, our earlier version did not provide detailed information about which instruments included pressure data, and we did not mention that the oxygen data was not critical for this study. We have modify the text to clarify these issues **(l. 121-122).** We have also added additional information in Table I to distinguish which sensors have a pressure recorder (depths in italics).

l108: why do you know that the PIES was destroyed if it was 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**).

l109: different approaches have been used in the past (e.g. creep function) – why linear? and, does it make a difference using other techniques? or maybe it is not relevant for your specific study (if so – make clear).
Looks like we were not clear on this issue either - thank you for pointing this out. The type of pressure sensors used in the CPIES have been in use for some decades, and they have a well established method for correcting the drift (e.g., Watts and Kontoyiannis, 1990; Donohue et al., 2010; Meinen et al., 2013). Based on this previous work, we know this type of sensor typically exhibits either a linear or an exponential plus linear type of drift. We have followed the traditional technique discussed in the above studies for our pressure records. We have revised these sentences **(l. 128-131)** to better explain our approach.

l120: What is the conclusion on accuracy after the calibration? Is it homogeneous across the data sets? How have the Microcats been quality controlled? And what do you expect for their accuracy?
We have now detailed the calibration process and specify the errors associated to each variables (**l. 167-171**).

l132: Please provide information about ADCP configuration and processing – depth allocation, compass calibration, depth cell length and ensemble lengths, number of bins, burst or spread mode?
This information about the ADCP configurations has been added in Table I. The ADCP was configured in bursts mode and the compass calibration was done in September 2014, before the initial deployment of the tall moorings, with the software *RDI WinSC*. We have not repeated the calibration due to servicing the instruments at sea and the influence the vessel's metal would have on the calibration. When we did the initial calibration all the parameters were within the set ranges and the total error less than 5°.

l134: for my understanding most of the content in this paragraph (3.1.) must be part of the introduction.
l139: is there maybe seasonal or other variability? What drives it? wind, topographic waves? Maybe a connection to the equatorial belt via waves?
Part of the original paragraph 3.1 has been moved in the introduction as suggested. We also added **(l. 80-83)** more information about the forcing mechanism and the variability of this equatorward jet.

l155-156: I would expect that to be part of the following chapter (3.2)
The paragraph concerning the eddy statistics has been moved **l. 227-238.**

l156: why is there such a large std on the propagation? Could you add a few words explaining that. Many stationary eddies? interaction with mean flow?
Thanks for pointing this out – you are correct, some of the 'speeds' that we had estimated were artificially high because we had some spurious features that were being tracked that exaggerated our standard deviation. We have redone this calculation and we have been careful to avoid artificial translation due to the intense merging and splitting in the area. We have now a comparable mean and standard deviation (11 ± 6 km day^{-1}) to that found in previous estimates (**l. 440-442**).

l157: please add time period: "in 14?? months we observed....“
The time period (~14 months) has been added **l. 231.**

l169-170: how do you know that this is "usually below the Ekman layer“? what is your critieria for "Ekman layer"? did you compare ADCP shear with geostrophic shear? Did you calculate the Rossby number for the eddies? how important are the non-linear terms (Rossby number)? and how would that impact a conclusion on Ekman layer and other dynamics?
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 Rossby number for the eddies, we have estimated this number. We have now added the Rossby number estimates from the Lilly and Rhines (2002) techniques to Table IV. A discussion about the importance of the non-linear terms has been added in the discussion (**l. 475-481**).

l170: is only one (the last?) bin used or a mean over a number of bins (which ones)? has the data been corrected for tidal effects via a tidal model or only through the butterworth filter? what is the bin length? (ADCP configuration should be added to Data and Methods section – see comment above)
Only the last bin has been used for the comparison with the surface geostrophic velocity derived from satellite altimetry. The data have been smoothed with a Butterworth filter, but it has been shown that the tidal currents are of relatively small amplitude in this region (Steele et al., 2010). Information about the ADCP configuration has been added to the Data and Methods section in Table I.

Steele, J. H., Thorpe, S. A. and Turekian, K. K. (Eds.). (2010). *Ocean Currents: A derivative of the encyclopedia of Ocean Sciences*. Academic Press.

l189: could you pls provide a depth range here?

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).**

l195: what do you mean by "dipole"? is that just neighbouring eddies or do the centres need to be at a minimal/critical distance? are there only cyclone/anticyclone dipoles or can there by a anticyclone / anticyclone dipole? more to add to Introduction.
We agree with the reviewer, the definition of a dipole was not explicit in the earlier version. We have addressed this point by providing the definition of, and additional information on the dynamics of, a dipole in the Introduction (**l. 69-77**) and in Section 3.1 (**l. 244-246**).

l197: Referenced to a discussion below in this section: It may be helpful to show the nature of the variability discussed here in the context of the 4 "cases" - also in a T/S diagram of the time series - maybe embedded in the background TS. This would reveal immediately if this is a thermohaline anomaly of simply heave.
As suggested, 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). During the period of the anticyclonic eddies, the temperature and salinity, and density values at the shallowest SBE Microcat sensor at M4 show the highest range of values of the all of the time series records. During the time period of the cyclonic eddy, the values are at the opposite end, i.e. the lowest recorded densities. While the signature of these two features is clearly separated, we do not definitively proves if these changes are associated with a thermohaline anomaly or a simple heave. We have added text to explain our assessments in the new version. (**l. 367-380**)

l198: in which respect "consistent" and "complete"? maybe saying "consistent and complete picture of the mesoscale dynamics in the Cape Basin"? Even if the basin would be filled with eddies it is not clear that the basin dynamics as a whole is similar to the dynamics of the individual components.
As suggested we have modified this sentence to make clear that we are speaking about the mesoscale dynamics of the Cape Basin **l. 247**.

l209: you differentiate between propagating (transient?) and "quasi stationary" eddies? what are the thresholds applied for that? or does transient means that the eddy dissolves when crossing the array?
Transient was used for describing the relatively short life duration of this eddy (less than a month) compared to Agulhas rings. We agree that this adjective has a wide application and we chose to delete it, as it was not essential.

l221: what does "associated" mean for you?
This sentence has been modified and “associated” has been deleted as it was potentially confusing. (**l. 272**)

l227: in this summary you leave out all information about the temperature/filaments you discuss before - why? isn’t that the key for the MOC connection?
As suggested, we have added in the summary a sentence to review the number and characteristics of the filaments and intrusions detected. **l. 278-279**

l232: it would be helpful to get an idea about how well the centre of the eddy crossed the mooring. Progressive vector diagram type techniques can help to analyse that (see Lilly, J., and P. Rhines (2002), Coherent eddies in the Labrador Sea observed from a mooring, J. Phys. Oceanogr., 32, 585–598). Very much of your conclusion depends on who well the eddy centre was observed.
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. Using this new method we have evaluated the Rossby number, apparent eddy radius, azimuthal velocity, mean direction, and velocity of the background flow for each of the features we discuss in the paper. Unfortunately, the result were indeterminate with regards to the distance of the eddy center from the mooring position. Nevertheless, the method did allow us to explicitly confirm whether the mooring was inside the core of the eddy or outside at each

time step. More details about this method and its application have been added to the text in several places. **l. 143-157** and **l. 285-308.**

l251: what is the difference between a "core" speed and the maximum velocity magnitude? at which depth is the maximum velo. Found?
We agree with the reviewer that the core of the eddy should not be dissociated from the maximum of the azimuthal velocity. We have deleted the previous definition of the core and applied the resulting estimates from the method of Lilly and Rhines (2002) in the revised manuscript.

l253: How much of this decrease is caused by TS anomalies (relative to ambient water) versus the vertical displacement of isopycnals in the eddy caused by rotation (heave)?
In this section (**l. 309-341**) and the related Figures 7, 8 and 9, we have now added the isopycnal displacements associated with each of the features that we discuss. Furthermore, by shifting our analysis onto neutral density surfaces, we can more explicitly identify which signals are associated with heave. We have revised our discussion in Section 3.4 to explain this.

l257: is this a one-record-only speed? what depth? (this applies to all the numbers presented here (see cases above). with tidal current or not?
All of the moored time series data presented in this study have been smoothed with a 2nd order Butterworth filter with a cut-off period of 72 hours, which will effectively remove all of the major tide constituents aside from the fortnightly tide. We have clarified this point in the Data & method section (**l.125-126**; **l. 141-142**). All of the maximal values listed are a one-record-only speed, but we have now extended the description in the text to include discussion of the vertical shear of the velocity. All the numbers presented in this section have now been more carefully explained, e.g. **l. 314-315**.

l266: This is a bit of a water mass zoo – you provide references but you may say in one sentence the origin of MUW? and OSW?
We agree with the reviewer than the origins of MUW and OSW were not clear in the previous manuscript. We added this information (**l. 347-350**), and to clarify this paragraph we synthesized all the information about the characteristics of the water masses in Table V.

l283: in which respect "stabilizes" the SAMW?
“Stabilizes” was replaced with “is present” (**l. 363**)

l287: besides the changes in the vertical structure due to the dynamical adjustment of the density field - you need to also consider the movement of the mooring - it can be seen that he mooring moved down by several hundreds of meters - for a cyclone that means that sampling is done at very different isopycnals (as uplift and mooring subduction operate into a similar direction)
The different properties of each event from full-depth-moorings and CPIES measurements are now made in density space in the revised manuscript, allowing us to more explicitly distinguish between property changes due to mooring motion from those associated with ocean changes.

l291: is that the bottom? or how many meters above the bottom?
We deleted this sentence as the related Figure 10 was deleted based on our changes due to some of the other reviewer comments.

l294: please give again reference that provides the information why you would expect to find A-AAIW – has it been observed in the Cape Basin before?
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.

l309: is the high correlation maybe because you used the data to estimate the GEMs?

As explained in our reply to the general comments above, it appears that our initial version of the paper was not clear on this issue. The T/S/P data from the moorings are not used in building the GEM fields. Instead, hydrographic profiles from the region (CTD and Argo) are used to build these simple two-dimensional look-up tables, so the GEM fields and the T/S/P data from the moorings are completely independent.

l313: how is this number estimated and what tells us the number?
We agree, in the earlier version we were not clear about the method we used to estimate the number of degrees of freedom, nor did we mention its implications. We have now modify the sentence to explain that the number of degrees of freedom represents a measure of the autocorrelation in a time series, and hence the true number of 'independent' observations, and we added more background for its estimation. **l. 204-210**

l320: This looks strange - 0.38% - I guess that is because you use the salinity and not a salinity anomaly. Would also be low for temperature if you use absolute temperature instead of temperature on the Celsius scale. So my question is what we should take home from a 0.38%? to me it first looks like insignificant... You may think of a better way in showing what you want to show. using anomalies??
We totally agree, the percent of changes on temperature and salinity were not 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 maximal anomalies reach 0.5°C in temperature and 0.15 g kg^{-1} in salinity (**l. 397-425**), which are much more significant for our conclusions.

l326: sometimes you give meters (m) sometimes dbar (not db please). Please use either one or the other (convert m in dbar or vice versa).
We agree, and we now referenced all the vertical levels according to depth (meters) values in the text and the figures.

l330: to me this is very confusing - what is uplift, what is real warming? are these anomalies detectable in GEMS if they are only isopcynal heave?
Concerning the distinction between uplift and real warming, we have now addressed this point by switching to a density vertical coordinate (Figure 11). Concerning this specific anticyclonic eddy, from the SBE37 MicroCat measured data, we have evaluated the changes in temperature and salinity and concluded that they are mostly influenced by the down-shift of the isopycnal layers (**l. 397-404**). From the reconstructed properties (GEM field), the warmer and saltier water mass trapped inside the Agulhas ring is visible in the upper part of the water column.
The GEM field technique will capture whatever is in the original hydrographic data sets (CTD and Argo). So to the extent that the hydrographic data sets capture these anomalies, the GEM field should be able to reproduce them. One limit, of course, is whether the anomalies are smaller than the scatter around the GEM field or not. 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**).

l333: again, how much is heave and how much is a real anomaly? In case of an isopycnal heave the "release" of the anomaly in the event of a dissolution of the eddy would have 0 (zero) impact on the environment.
We agree, and as explained in the comments immediately above, we now addressed this point by discussing the anomalies in density space. For this specific event (cyclonic eddy), we now show that the variability is mostly influenced by the up-lift of the isopycnal layers and water mass transport characteristics of I-AAIW. We have explained this in more detail in the revised text. (**l. 405-412**)

l385: uplift of isopycnals has nothing to do with water masses variability - the local uplift of isopycnals in a vertical homogenously stratified fluid (e.g. through a local geostrophic adjustment of the density field) will create an anomaly when looking in depth/pressure space but no anomaly when looking in density space. This is of fundamental importance when it comes to discussion of transport of water mass anomalies. Water mass anomalies are only "real" when looking in density space. Note, it may be helpful to show the

nature of the variability discussed here in the context of the 4 "cases" - also in a T/S diagram of the time series (Figure 5-8) - maybe embedded in the background TS (see my comment above). This would reveal immediately if this is a thermohaline anomaly of simply a heave.

We agree, and as explained in the comments immediately above we now address this point by evaluating the anomalies in density space (Figure 11). As suggested and detailed in our response to one of the earlier comments n, 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**)

l393: what does "direct" and "indirect" mean - please precise.

Direct or indirect were not properly applied in this sentence. We decided to delete these two adjectives.

l396: Indian Ocean

Fixed as suggested **l. 466**

l407: could you please provide reference for the decorrelation scales

We added this information to the new version of the manuscript (**l. 507**).

Figure 5-8: a TS diagram of the anomaly and a progressive vector plot to determine how close tot he centre the crossing took place would be helpful.

The T/S diagram of each case study is now shown in Figure 10. The hodographs and the progressive vector diagrams are now illustrated in Figure 6.

Figure 11: anomalies versus density would be of more help to show

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.