**<u>Response to Reviewer #1</u>**

***We are pleased that the reviewer sees value in our manuscript, and we have addressed their comments in a revised draft. Our responses to the reviewer's specific comments are below interspersed between their original comments. All of our responses are in bold italics.***

Interactive comment on "Characteristics and causes of Deep Western Boundary Current transport variability at 34.5°S during 2009–2014"

by Christopher S. Meinen et al.

Anonymous Referee #1

Received and published: 16 November 2016
This paper presents the second set of observations from the 34S array in the Atlantic, measuring the strength of the deep western boundary current. The observations have now been extended to >5 years.

The main new findings that I gleaned from this paper are perhaps unsurprising (given recent developments in monitoring circulation in the North and South Atlantic by this set of authors and others): 1. The strength of the DWBC is highly variable (with a total range of 140 Sv) compared to the mean (expected to be around 15 Sv, but subject to the choice of reference level - see point 3). 2. Variability is particularly strong on sub annual timescales (here in the 90-150 day band, and also the 20-50 day band), and likely associated with eddies or Rossby waves, and 3. It can be complicated to measure mean transport strength using geostrophic methods. In this case, the authors use the velocities at 1500 dbar from a numerical model (OFES) and reference their geostrophic velocities to this depth level.

The paper represents a valuable contribution, particularly given the importance of the South Atlantic transports to ideas of the stability of the MOC (not mentioned in the paper).

I have a couple questions on the methods:

- How sensitive is the mean or transport variability to the choice of reference velocities from OFES? Why did you choose 1500 dbar (L194) if the level of no motion is closer to 800 dbar (L412)?

***The mean value is not hugely sensitive to the choice of reference level or to the choice of model based on our limited testing – for example the mean is fairly similar if output from a run of NEMO is used instead. We have added to Footnote 3 (Page 9) the fact that the 1500 dbar velocity differs by less than 1 cm $s^{-1}$ if NEMO is used rather than OFES. Also, the choice of the reference level for adding the model time-mean has no***

***impact on the time variability, which is derived independently from the bottom pressure observations. Only the time-mean from OFES is used.***

***Regarding the choice of 1500 dbar versus 800 dbar, again the results are not highly sensitive to this decision. Of course as we show, there is no 800 dbar level of no motion in the real ocean anyway, which is why the transport relative to an assumed 800 dbar level of no motion bears no resemblance to the absolute transports shown in Figure 6. In some earlier studies, 800 dbar was used as a level of no motion for geostrophic calculations, as only the relative velocity term was being observed. Because the time variability of the 'barotropic' term is actually measured here, via the bottom pressure differences between pairs of PIES moorings, we can demonstrate that the idea of a level of no motion for the time-varying flow cannot be supported by the data. We have revised-expanded Footnote 3, page 9, to make these points more clear in the revised document.***

- Can you give an indication of how low frequency fluctuations (not measured by PIES) might manifest? Fig 6 shows that the relative velocity contributes less than the absolute velocity to the transport estimates â˘A˘T what portion of the velocity comes from the 1500 m reference vs pressure from the pies? For someone who might like to further interpret the time series of the strength of the DWBC, over what frequency bands is the variability "trustworthy"?

***The 1500 m reference that is added from the model is only a time-mean, there is no time variability associated with the model reference that is added. So all (100%) of the time variability that is observed in the time series in Fig 6 is associated with either acoustic travel time variations between the PIES (relative term) or bottom pressure variations between the PIES (reference term).***

***The reviewer does raise a good point here, though, because like all bottom pressure gauges, the bottom pressure gauges in the PIES are subject to exponential and/or linear drifts which can be difficult to distinguish from variability at longer periods. The exponential drifts are generally only over the first few months of a deployment, and can often be identified easily and removed. The linear, record length, drifts on the other hand are much more difficult to identify as distinct signals compared to long-period variability. One advantage of the PIES is that these instruments can be deployed for longer times, up to 4-5 years, which implies that the linear-drifts observed in the PIES records can only be misconstrued and/or confused with variations with periods much longer than the record length, i.e. decadal and longer in the case of the instruments described here. So we would argue that in a ~4-5 year record, the PIES would do a poor job of capturing variations with periods of a decade and longer, but should be 'trustworthy' for variations with periods up to a few years in length, because a sinusoidal wave of such periods could not be misconstrued as a linear trend in a 4-5 year time series.***

***Unfortunately, at present, there are no in situ measurement systems that we are aware of which can capture the flow variability on pentadal and decadal time scales accurately, aside from the 'old-school' picket fence of current meters, which are generally too expensive to maintain for long-term (5+ year) deployments. So the issue the reviewer raises here remains a long-term problem for scientists working in this field.***

***We have provided a few words on these limitations in the new Footnote 4 on Page 10.***

- The discussion of the pathways of the DWBC seems valuable âˇAˇT that 20% of the DWBC volume transport is taking another pathway, but perhaps is mostly a reference to previous work by Garzoli et al. (2015) and van Sebille et al. (2012). Can the variability of this percentage be deduced from this dataset (or from a dataset that is fully transbasin)? Is the result that the AABW flow is northward subject to any of the reference level or other choices? This also seems like one of the more startling results if you are now identifying that the northward AABW is not in this region.

***Unfortunately, the 20% pathway into the interior occurs north of our array, so we have no way of observing it near the western boundary. The trans-basin array will capture the net meridional flow, and analysis of that data is underway, however details of the flow in the basin interior along 34.5°S will be difficult to tease out even from the full array as it exists today due to the broad longitudinal range between the easternmost site of the western array (44.5°W) and the westernmost site of the eastern array (prime meridian at 0°). This will be a topic for future analyses.***

***Please note that we conclude that the AABW flows <u>southward</u> across the SAM array (see Figure 4). The small differences in time-mean reference flow at 1500 dbar between the models that have been evaluated are insufficient to change the sign of the apparent AABW flow, so this result seems robust within the western array. We have added some words about this in the revised paper to highlight this robust result (Lines 325-328). We concur that this is a surprising result, and we look forward to future data sets, such as the presently ongoing trans-basin CTD/LADCP section being collected along 34.5°S as these revisions are being prepared, which will allow more detailed analyses of these flow characteristics.***

Comment - I find the composite analysis only marginally enlightening. Given the later results on the importance of westward propagating features it is possible that another method of identifying the characteristic patterns of variability would be more suited to this phenomenon. This may be beyond the scope of the present study, as the model results and previous studies in the North Atlantic do support the conclusions of the influence of westward propagating signals on DWBC measurements.

***We acknowledge that the composite analysis is far from perfect, but we feel it does add support to the analysis because as the reviewer notes it is consistent with the model results and the previous analyses. We also agree with the reviewer that a more detailed***

***analysis with additional methods might provide more information, but is beyond the scope of the present work.***

Comment - of the proposed improvements (L685/686), I don't know whether better resolving the westward propagating signals is worthwhile. Investing additional observations on full transbasin measurements would allow a better estimate of the time mean transport, which seems like a worthwhile endeavor. I suppose one reason the higher resolution in the wset could help is if a shorter distance between observations means that eddies are better resolved and so not aliased by the array (L530)?

***We think the reviewer has hit precisely on the value of the enhanced resolution in the western array – improved resolution of eddies (as well as meanders and recirculations) in the western domain. The expansion of the array to be fully trans-basin has already occurred, with international partners from France and South Africa now instrumenting sites along 34.5°S from the prime meridian (0°) to the southern tip of Africa. These instruments have been in place since 2013-2014, so future analyses will allow for inclusion of that data. But we feel the 5 years of data available in the west from 2009-2014 should still be analyzed and used to the fullest extent. And we think future enhancements in the west will yield better results of the DWBC variability as well as its relationship with the MOC variability, independent of the role of the fully trans-basin observations.***

On the figures, I would recommend not using the jet colormap anywhere. In your velocity figures, it can make it hard to visually distinguish between weak northward and weak southward flow, and artificially highlights the "yellow" color which is a mid-range value and otherwise unremarkable. (Fig 2, 5, 9, 11, 14)

***Our mistake here – we had neglected to note in the captions that we had added white contours in these figures to clarify the zero flow lines and to make it easier to see what areas are positive and which are negative. We have now updated the captions to reflect this, and we added white contours to Fig. 14 for the same purpose.***

Minor points:

L24, midpoints BETWEEN three of the existing?

***We see the reviewer's point here that this language was potentially confusing. We have revised this sentence to read "…at the midpoints of the two westernmost pairs of existing sites." (Line 25)***

L46, SOCIETALLY?

***Corrected as suggested. (Line 48)***

L402, Is there a sensible way to choose the offshore limit (rather than a fixed 200 km)?

***The 200 km is dictated by the total longitudinal extent of the 11°S western boundary array. We have added a parenthetical note to clarify this. (Line 417)***

L557-558, Anticorrelated seems expected since both have site B as a boundary. Lack of anticorrelation would be if the transport variability were dominated by variability at sites A and C.

***The reviewer's point would be robust here if Site B was located at some fixed point in the circulation pattern (such as the offshore edge of the DWBC) and the flows were not meandering or propagating. However the location for Site B was selected prior to knowledge of the location of the DWBC flows at this location. And if the deep flows are meandering across Site B over time, both in a 'meander of the DWBC' sense and in the 'westward propagating feature' sense, then it does not necessarily follow that the flow east and west of Site B must be anti-correlated. It is conceivable that the deep flow could all be southward out to Site C, and the reversal could occur offshore of Site C, for example. So we think there are at least some reasons to term this observed anti-correlation as "surprising".***

L567, "more complex and nuanced" - can you be more specific?

***We have added a parenthetical note to provide one example; e.g. the flow might have finer-scale banded structures horizontally that average together to yield the structure shown in the composites, but these bands may exhibit horizontal meanders or zonal translations that result in different impacts on the PIES-to-PIES integrals and yield poor temporal correlations. (Lines 585-586)***

L607-608, I don't see eastward propagation. I see faster westward propagation in the east, and slower westward propagation in the west

***The reviewer is correct that westward propagations do dominate, but if you stare at the figure for a while there are some eastward propagating features that appear. The most obvious feature is a northward (red) event that propagates from around 48°W to say 45°W between about February 1980 to about April 1980. Another eastward propagating northward velocity event can be seen in early 2003 between roughly the same longitudes. These events may be easier to see now that we have added the white contours to address an earlier comment that the reviewer made. So while westward propagating features do dominate, there are some eastward propagating events.***

L641-642, Do these features have a surface expression, as in SSH? Could use SSH to identify the features observed by the PIES (probably beyond the scope of the present study)

***Previous studies have shown that some deep westward propagating events have strong SSH signals, whereas others do not. A detailed analysis of the SSH signals associated***

***with these features is beyond the scope of the present study as the reviewer suggests, but we agree it would be an interesting area for future analyses.***

Interactive comment on Ocean Sci. Discuss., doi:10.5194/os-2016-76, 2016.