Response to Reviewer #1

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˘AˇT 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.

Response to Reviewer #2

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.

Review of manuscript submitted by Meinen et al. to OS titled:
Characteristics and causes of Deep Western Boundary Current transport variability at 34.5°S during 2009-2014

Summary and Recommendation

This study uses 6 years of PIES/CPIES data at 34.5° to describe the variability of the Deep Western Boundary Current (DWBC) transport. The main results are similar to other latitudes, in that the DWBC variability is much larger than the mean. I found at times that there is too much emphasis on the absolute transport when the title of the paper refers to transport variability. Only one model, OFES, is used to estimate absolute transport. Have other models been considered for comparison? I worry that the results of absolute transport are too sensitive to this choice of reference velocity from the model. However, I think that this manuscript is nice contribution to the community and should be published after my concerns below have been addressed.

Major Comments
Line 26-29: This needs to specify that the estimate of absolute transport is from a combination of observations and model output.

Only the time-mean value of the absolute transports is dependent on the model output; the time variability of the absolute transports, which is the main focus of this paper, is completely independent of the model and is based only on actual observations. Furthermore it is only the time-mean of the reference velocity component of the transports that depends on the model; the time-mean vertically-sheared velocity structure is directly measured/estimated using the observations. As such, we feel this is too small a detail to explain in the Abstract, but we have made sure in addressing this and other comments from all of the reviewers that the paper makes clear that it is only the time-mean reference velocity that is dependent on a model.

Line 235: Reference to a 0.2° horizontal grid is not exactly correct. These models have a grid with a resolution of 0.1 x cos (lat). So, less than 0.1 at these latitudes. Give a precise value of the horizontal spacing in km and how this relates to the PIES spacing.

This model is actually run using lon-lat grid points, not x-y (km) grid points, according to the documentation provided by those running the model. So the grid description in the text is correct. However the reviewer makes a good point regarding explicitly

stating how the model resolution compares to the PIES spacing. We have added a sentence to state that the model resolution is 5-15 times finer than the spacing between the PIES moorings, depending on which time period (i.e. before/after the additional Brazilian instruments were added) and which pair of PIES one considers. (New text on Lines 243-244.)

Lines 372-393: There is too much emphasis on absolute transport. Stick with the focus of the manuscript and consider removing this section since it is too dependent on model output.

If we understand the reviewer's point here correctly, they are using a different definition of "absolute" than we intend. We are using "absolute" to refer to the sum of the baroclinic, vertically-sheared, term and the barotropic, non-sheared, term. The time variability of both of these terms are directly measured with the PIES array we are presenting here, as is the time mean of the vertically-sheared term. Neither variability term utilizes the numerical model. The model is only used to provide the time-mean value of the non-sheared term. For clarity purposes, we have added a footnote to the revised manuscript to make explicitly clear what we mean by "absolute". (New footnote #1, Page 5.) We have also added some additional words to a sentence where we explain how the PIES data are used to get absolute velocity. (Lines 178-179, Page 8)

Line 411: Is there any evidence observationally or from the model in this region that the deep reference currents are really constant with depth, especially on the slope where the currents may be bottom intensified, "bottom trapped?"

The PIES-GEM technique provides a fairly robust estimate of the vertical shear of the flow. Unfortunately there are no independent measurements at this same latitude, but the same techniques have been applied at 26.5°N where comparisons to both "dynamic height moorings" and to current meter moorings have demonstrated that the PIES-based estimates of the shear are accurate for large-scale geostrophic flows. The limited comparisons possible with lowered acoustic Doppler current profiler data collected on a few of the ship-sections taken along this line also reproduce fairly similar shear structures to those estimated from the PIES-GEM data in the 34.5°S region. So yes, we think this is robust in this region – but this is something we hope to revisit in the future when additional data becomes available (e.g. there is presently a LADCP section being collected on a German vessel).

Minor Comments

Line 112: I don't like the acronym "SAM" used for the Southwest Atlantic MOC. SAM is commonly referred to as the Southern Annular Mode. Consider defining another acronym to avoid confusion for the reader.

The acronym "SAM" is, as the reviewer notes, used for the Southern Annular Mode as well. However this name has been in use for this project for 7+ years now, and is the name used in previous publications and by the funding agency, so we're reluctant to change it. The acronym is only used a few times within the paper, and is defined the first time it appears, so we are confident readers will not be confused by its use.

Line 227: It unnecessary to use \high quality." This is too subjective. How do you quantify "high quality?" Remove this.

While we agree it is difficult to quantify "high quality", we think it is still a reasonable term to use here. The term "well-validated" is equally hard to quantify. Perhaps one way to quantify "high quality" would be to see how frequently a model run has been used by independent researchers in the science community. This particular model run has been utilized by dozens of researchers in numerous studies and scientific publications in recent years, so we think the phrase "high quality" applies.

Figures

Fig. 2: I don't like the colorbar limits. Consider making it ± 24 like in Fig. 9 with no contours and draw contours emphasizing maximum values.

Unfortunately the ocean is asymmetric with regards to the observed velocities here, particularly in the model. Much stronger negative (southward) velocities are observed as compared to positive (northward) values. So using a symmetric color bar like what is used in Figure 9 is not possible for Figure 2 without washing out the colors used for the positive values. To address this point and a comment of Reviewer 1, we have clarified in the caption that the white contours indicate zero flow – we have also added to the caption a note that the color contours are at 2 cm s^{-1} intervals. We think with these additions it should be straightforward for readers to evaluate the values in the plot.

Fig. 2: It is misleading to us SS topo for model output since the representation of the bathymetry may be significantly different due to smoothing. You should use model topo for model output figures.

While we agree in principle with the reviewer here, by fortune the bottom topography in the model at this latitude agrees quite well with the real ocean depths, so we feel the benefit of having a consistent bottom for comparison between plots outweighs the help that would come from plotting the bottom topography from the model rather than the SS topography.

<u>Response to submitted comment by S. Elipot</u>

We are pleased that the commenter sees value in our manuscript, and we have addressed his comments in a revised draft. Our responses to the specific comments are below interspersed between his 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.

S. Elipot
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This paper is a valuable contribution for the observation and understanding of MOC processes in the South Atlantic. Here, I present some comments with respect to the spectral analyses, and provide some suggestions for improvement (Figures 8 and 13 and spectral analyses starting on line 456):

For comparison to other spectral estimates for large-scale oceanic transports, it would benefit the oceanographic community to use the best methods currently available for conducting the spectral analysis of the DWBC time series presented in this paper (observed and modeled). It has been demonstrated that the Welch's averaged periodogram method is generally outperformed by the multitaper method. In one go, the multitaper provides an estimate of the spectrum from the Nyquist frequency to the Rayleigh frequency corresponding to the longest period of the time series, without the need to divide up the time series and thus to increase the Rayleigh frequency. As the authors have worked very hard to produce this time series of climatological importance, it is a pity not to investigate the transport variability up to the longest period.

While we understand the commenter's point here, we might draw quite a different conclusion. If our goal is to compare with other spectral estimates in earlier studies, it behooves us to utilize a method similar to that used in those previous studies. Otherwise any differences observed would be muddied – i.e. any differences could be due to true ocean differences or they could be due to the differences in methods. The Welch's method has been in use for decades, and the vast majority of previous analyses have used it. So for comparison to previous work, we would argue that it is necessary and/or advantageous to use similar methods to those used in the previous studies, purely to isolate ocean differences from methodological differences.

As a second point, we might be somewhat less convinced than the commenter on the point that the multitaper method "outperforms" the Welch's method. That their results are different is not disputed, but whether "different" is "better" is not as clear to us.

Results using more elaborate techniques such as the multitaper method, and wavelet analysis, are often abused (not necessarily by the commenter!), demonstrating a lack of basic statistical understanding. If a 'fancy technique' claims to provide results at long periods compared to the record length – periods wherein the record itself contains only a small number of samples – then it is mathematically impossible to know whether the resulting spectral estimates, correlations, and/or coherences are robust features of the record or if they are just random chance.

While there is no question that some good researchers in the field have adopted analysis techniques such as multitaper, wavelet, etc., it is the contention of the authors of this manuscript that the Welch's method is well established, commonly used, and is a fairly robust method for analyzing time series while providing solid error estimates. As such we are inclined to stick with the Welch's method used in this paper.

Using the multitaper method would simplify figure 13: a single panel could show the mutitaper estimate for the entire OFES time series in addition to the multitaper estimate for the observations. Depending on what the authors find is the most illustrative, the results could be presented on a x-linear/y-linear scale, or a x-linear/y-log scale, or xlog/ y-log scale.

If presented on a linear-log or log-log scale, the average multitaper has a constant confidence interval (independent of frequency) which could be applicable for both estimates if evaluated with the same spectral parameters. In addition, one could also show spectral analyses of the relative and reference contributions to better understand their dynamics. The choice made by the authors to present their spectra in variance preserving form is likely to lead to misinterpretation of possible outstanding periodicity in the data, so-called peaks. The analyses would benefit from conducting a formal test for periodicity in the data, that is a test on significance of peaks. So far the confidence intervals seem to indicate that there is no such significant peak, despite what is stated in the conclusion of the paper. In addition, there may be something wrong in the calculation or display of the 95\% confidence intervals for the spectra, as these inexplicably sometimes go to zero (clearly visible in Figure 8).

References:

-Percival, D. B., and A. T. Walden, Spectral Analysis for Physical Applications: Multitaper and Conventional Univariate Techniques. Cambridge, UK: Cambridge University Press, 1993.

-Wunsch, C: "Time series analysis. A Heuristic Primer", Classroom notes (January 22, 2010), http://nrs.harvard.edu/urn-3:HUL.InstRepos:15217585

-To calculate multitaper estimates, if using Matlab signal processing toolbox https://www.mathworks.com/help/signal/ref/pmtm.html?searchHighlight=multitaper or JLab free toolbox: http://www.jmlilly.net/doc/mspec.html

Here again, perhaps, using the multitaper method could be an alternative. However, in this case we are not certain that the commenter has understood the key results illustrated in Figure 13 as explained on lines 600ff: a) the distribution of energy in the model, observable as the area under the curve when plotted in variance preserving coordinates as done here, underestimates the energy in the observations at essentially all time scales; and b) that only at very long record lengths (i.e. the nine-year window used in Fig. 13b) do spectral energy estimates at time scales longer than about 100 days become clean and meaningful. We do not think either of these points would be better presented using the multitaper method, the results of which, as noted above, cannot easily be compared to most historical estimates either. We have added a parenthetical note to point out that plotting spectra in variance preserving form is valuable since the area under the curve is proportional to the energy at any given period (Lines 476-477).

Some other comments:
Line 418: how is the statistical significance of correlation assessed? why is the correlation reported if it is not significant? low correlation values are not necessarily not significant, but maybe only not relevant.

We have added a reference for the method used to estimate statistical significance. (Lines 434-435). The low numbers are reported for completeness so that the reader can evaluate the analysis appropriately. We concur completely that low correlation does not imply not relevant – as we note in the text, the relative term is not correlated with the absolute transport, but it is certainly not unimportant.

Lines 437-441: "This observed annual signal is very weak and is highly influenced by other time scales and aliasing." these claims appear here unsubstantiated. The spectral analysis should appear first, then the seasonal cycle estimate second.

We would disagree that the claims are "unsubstantiated" at this point – in fact we think that Figure 7 demonstrates this point quite clearly. The observed variations from one year to the next (i.e. the differences between the gray lines in Figure 7) are much larger than the 'mean' seasonal variations suggested by the average seasonal signal (i.e. the red line in Figure 7). While the spectral analysis does indeed support this conclusion as well, we feel that Figure 7 demonstrates that other time scales are clearly more energetic than the annual cycle in the DWBC transport.

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