Response to submitted comment by S. Elipot

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
selipot@rsmas.miami.edu

Received and published: 5 December 2016

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.