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.