The authors greatly appreciate the thoughtful comments and suggestions of the two anonymous reviewers, and we have sought to address each of their comments in this revised manuscript. Our specific responses to the reviewers are below in italics, interspersed between the original comments/suggestions of the reviewers.

Reviewer 2

The authors report a huge variance (std: 25 Sv, variations of 40 Sv). The location of these instruments is very close to the Brazil-Malvinas confluence (BMC). Indeed, Goni et al. [1996] incorporated an array of PIES at 35S to estimate the dynamics of the BMC. How do we know that it is not variability derived from the highly variable confluence that is being observed rather than the DWBC of the South Atlantic?

The BMC is slightly to the south of the location of our array – in fact the results from the PIES array the reviewer mentions, in addition to satellite and other data sets, helped guide the selection of the array location and should have been mentioned in the text. We have added a few sentences to the text to explain the reasons for the location of the array in the context of historical observations (Data and Methods Section).

The measurements are also very close to the Zapiola anti-cyclone. Could variability from this anticyclone be contributing to the variability seen in these measurements? This to me is the crucial question pertaining to this manuscript: can we be sure the variability measured is representative of the South Atlantic DWBC as part of the South Atlantic MOC and not variability derived from other influences?

The Zapiola eddy itself is far enough from our region (centered near 45°S; e.g. Fu et al., JPO, 31, 506-517, 2001) that it is unlikely to influence our observations, however the reviewer is entirely correct that other circulation features (Rossby waves, etc.) clearly influence our observations. This is true anywhere in the ocean where transport observations are made, not just for the DWBC but also in upper ocean circulation features (e.g. Kuroshio, Gulf Stream, etc.). Teasing out the MOC (thermohaline) components will likely require the full trans-basin array of which our pilot array is just a first step.

Specific Comments

Figure 4 compares the PIES with CTD with the authors saying that the figures compare well. However, I see opposite concavity in the isopycnals particularly around site C. This would imply different currents. Could the authors comment on this.

The deep ocean signals that attracted the reviewer's eye were actually artifacts/contour issues in the plot. To avoid this issue in the revised draft, we have replaced the old Figure 4 with a new version (now Figure 5) that compares the profiles at each site rather *than showing a section. We have also discussed this more in the text (Results Section).*

The authors mention that the variability seen at 34.5S (std: 25 Sv, variations of 40 Sv) is comparable to that at 26.5N (std: 16 Sv, variations of 10 Sv - Meinen et al. [2012]). I would like more elaboration on the comparison as these numbers look quite different to me.

Yes, this wasn't particularly clear in the earlier draft – the numbers are similar but only when one notices that the array at 34.5°S is 7° of longitude wide, while the array at 26.5°N is just under 5° of longitude wide. Much of the difference the reviewer noted scales based on the difference in array sizes; larger horizontal integration domains yield similarly larger transport variability. We have added several additional sentences to the text (Late in the Results Section, and elsewhere) to discuss the variability in the context of observations and the array sizes at other locations.

The array is along a line of latitude rather than orthogonal to the continental slope. A western boundary current would be expected to some extent to follow the topography. Can the authors comment on the choice of arrangement of the instruments and errors that may arise from that choice?

We have added several sentences that explain the selection of the array location (Start of Data and Methods Section). The zonal nature of the array will not introduce any errors since the array is designed primarily for measuring transport rather than velocity. Any obliqueness of the current crossing the array (which will likely vary with time due to meandering, etc.) would cause the current to appear artificially broad and slow if evaluated as velocity, but due to the nature of the transport integration associated with the geostrophic method, the transport will be correct regardless.

The integration domain from 800-4800 dbar is based on work at 26.5N. Why would the authors not choose a domain more representative of the region? Say 1000 (AAIW depth) to 4100 (AABW at the 0C isotherm).

The reviewer is correct that the selection of 800-4800 dbar is somewhat arbitrary and that one key reason for the choice was that it was selected for comparison with other latitudes. However the 800 dbar limit is also consistent with previous separations between the upper layer flows (e.g. Brazil Current transports are generally integrated from the surface down to 800 dbar – such as the Confluence Experiment paper by Garzoli, 1993) and the lower layer flows. Also note that at 34.5 ºS the high dissolved oxygen and low salinity waters characteristic of AAIW are centered at ~ 600-700 m. The lower layer is 4800 dbar but is in some cases the bottom, as it is shallower than 4800 dbar in the inshore spans. But perhaps the key response to this concern is that the character of the transport variability is not sensitive to modest (±100-300 dbar) changes in the integration limits. This is noted in the text (Data and Methods Section).

Further elaboration on the PIES methodology and especially its particular application to the South Atlantic would be of use here.

We have added several sentences to the paper to discuss this and we have added a new figure (Figure 2) illustrating the application of the GEM method to this region in the South Atlantic. The figure also illustrates the estimated 'error bars' around the GEM field by quantifying the scatter between the original hydrographic (CTD and Argo) profiles and the GEM field.

An indication of the location of the BMC and the Zapiola anticyclone would be useful in Figure 1.

Expanding Figure 1 to a large enough region to include the Zapiola anticyclone (which is centered near 45°S – see, for example, Fu et al., JPO, 31, 506-517, 2001) would result in a map that would make looking at the PIES array difficult. However, to address this concern we have added several sentences to discuss the location of the array in the regional context, specifically describing the location relative to the BMC (Data and Methods Section).

The authors decline to comment on the currents from the CPIES at site B. It strikes me that a comparison between these currents and the currents derived from the model would provide validation of that model. It is not clear to me why this has not been investigated.

The current meter on the CPIES measures the velocity at one single point in the vertical (rather than a profile one might obtain from an ADCP), which makes it a fairly stringent test for a comparison to a model. Furthermore, the rest of the velocities and transports discussed in the paper represent horizontal averages over the span between PIES/CPIES sites, i.e. 2-3° of longitude, while the current meter is a single point in a horizontal sense as well. Nevertheless, we have now compared the CPIES velocity to the model velocity at the nearest grid point in the OFES output. The mean meridional velocity from the CPIES at Site B over the 10.5-month time period discussed in the paper is about 1.60 cm/s, while the mean meridional velocity from OFES at the grid point nearest to Site B over the 27-years of the output used in the paper in the model is 1.62 cm/s. We have added a comment to this regard to the paper (Footnote #4), and we have also added some additional basic statistics about the current meter record (Footnote #3).

Technical Corrections
There are no labels on the x-axis in Figure 2.

We have added the x-axis labels – thank you for spotting this typo!

Exhaustive use of "e.g." in the references: 11 times in the first two sections. If there are more references, they could be explicitly listed.

We have attempted to reduce/eliminate any unnecessary use of "e.g.", however in situations where there are numerous (5-10+) references that discuss a particular issue *we have maintained listing only 1-2 representative references. This keeps the bibliography from becoming as long as the paper itself.*