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.

<u>Reviewer 1</u>

More discussion of the techniques is required. The authors point to papers describing methods used in the North Atlantic yet the success of the PIES techniques is site specific. How well does the GEM work in the South Atlantic. I'd like to see some quantitative estimates here. The authors point to figure 4 (comparison of potential temperature/ salinity/neutral density sections) as 'excellent'. Yet when I look at these figures, I see opposite slopes in the deep isotherms and neutral density surfaces. On question immediately comes to mind. Do the baroclinic transport estimates agree?

The reviewer is quite correct, the success of PIES analysis in general, and of the GEM technique for analyzing PIES data specifically, varies from region to region. The cited Garzoli and Garraffo (1989) reference demonstrates the ability of the IES to capture upper ocean signals in this region, however the ability of the GEM technique to capture data above and beyond the primary upper ocean signals that were studied by Garzoli and Garraffo (1989) has not been previously demonstrated. We have included an additional figure (Figure 2) to the revised paper that shows the temperature GEM field and the scatter about the GEM field. The scatter field (shown both in absolute units and as a signal-to-noise ratio) illustrates the 'accuracy' of the GEM estimates of temperature as a function of pressure and travel time. For brevity the salinity and specific volume anomaly GEM fields are not shown, but the accuracy is similar to the temperature. The text has been updated to reflect these explicit 'error bars'.

We have also added a footnote (Footnote #1) that discusses the comparison between the travel times calculated from the CTD profiles and the concurrent travel times measured by the PIES, and quantifies the percentage (4.4%) of the observed variability that this accuracy represents. Because the baroclinic streamfunction (i.e. Fofonoff potential) is tightly correlated with the travel time, the baroclinic transport accuracy is similar.

I could not really follow the leveling procedure – more information is required. I disagree that the 'leveling constant' between two pressure gauges at different depths is time independent. That would only be true if there was no time dependent stratification between the two gauges. Is this the case along the sloping topography? I don't think so. They might be able to quantify this contribution between using the GEM/travel time to determine what the stratification is between the gauges.

We have added some additional information as the reviewer suggested (Footnote #2). The reference cited, and additional citations within that reference, have demonstrated that the density variations between instruments at depth are negligibly small as compared *to the signals of interest as long as the instruments are all well below the main thermocline and halocline depth level(s).*

In addition, the authors suggest that Rossby waves might be responsible for the inshore variability – are these Rossby waves or could they be vertically trapped topographic Rossby waves? How would they account for a vertically trapped velocity structure with the PIES methodology?

We have inserted some additional text in the Conclusions section to indicate that the observed velocity variability appears surface trapped, rather than bottom trapped, which would argue for Rossby waves rather than TRWs, although the short observational record and the small number of observed events makes specific attribution quite difficult. We also added some additional text to the Results section discussing a previous study of waves just to the south of our array.

The PIES-GEM technique estimates density profiles explicitly, whereas the velocity structures are determined only as gradients between pairs of PIES. As a result, the GEM technique is capable of observing higher-mode vertical structures such as bottom trapped waves and/or subsurface intensified flows. This has been shown in previous studies of, for example, the subsurface-intensified Antilles Current and the bottom-intensified Deep Western Boundary Current (Meinen et al. 2004).

How well does the model velocity agree with the deep currents measured by the CPIES at location B?

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).

Gaps in Discussion
What does it mean that the variability is similar to that found at 26.5N? If the DWBC reconstitutes itself somewhere between 8S and this location, why would the variability be similar? What dynamics do these two locations share? What about comparisons to the Line W estimates?

We have added several sentences discussing this – including adding comparison to the recent results at Line W – to the text (Results Section).

Can OFES provide some insight into the connectivity of the DWBC in the South Atlantic?

Latitudinal connectivity of the DWBC in the South Atlantic is a bit beyond the scope of the present manuscript. Certainly the model can be used for such analyses (and the *authors are aware of at least one other study ongoing on that topic).*

Why integrate from 800 dbar? From the dissolved oxygen section (Figure 3) it looks like that would cut into the AAIW layer? Why not chose neutral density layers associated with the water masses. It seems they were motivated to use 800 dbar in order to compare to the 26.5N section but in my opinion that is too narrow a focus. They could do both, an estimate to compare with 26.5N and then an estimate that is physically motivated.

Ideally in order to calculate the transport in water mass categories at this location we would need oxygen information continuously at all of the PIES sites, but we do not have that data (see the water mass definitions in the Results Section – The impact of the lack of oxygen on the water mass integrations is noted in the Data and Methods Section). However, the sensitivity of the calculated transport variability to modest ±100-300 dbar changes to the upper and lower integration bounds is fairly weak. This is now noted in the paper in the Data and Methods Section.

The discussion on page 987, near lines 5 brings up the Brazil-Malvinas Confluence suggesting that the retroflection is tighter than suggested by earlier studies but comparable to the Rio and Hernandez mean topography and the OFES model. This discussion could be expanded. They could show those fields at the very least. The manuscript as presented is a bit myopic. Take, for example, Figure 1. What are we to take from the shipboard ADCP vectors? Could this figure be expanded to show the large-scale topography as well as the dynamic sea surface height topography from the satellite altimeters? It would help sort out the western boundary current and the adjacent eddies/recirculations and put the array in a larger context.

The goal of this paper was to focus on the data collected recently in this project. The larger scale patterns collected via satellite, etc., have been discussed in some detail already in previous studies. As such we have sought to address this suggestion by doing a more complete job of explaining the location of the array in the context of those studies. A paragraph has been added to the paper (Data and Methods Section) discussing this issue.

I'm confused by the discussion on page 986, starting line 6. I think they are trying to get at correlation length scales. Might be more interesting to look at the correlations between the deep pressures and the travel time separately.

We believe the reviewer meant page 988? The discussion here is about velocity correlation, which can only be done between the three spans between PIES/CPIES since velocity is calculated as the gradient between pairs of PIES/CPIES. The correlations between bottom pressures and/or travel times would not be the same as the correlations of gradients between pairs of bottom pressure gauges or pairs of IESs. We are quite confident that the correlation length scales of both barotropic and baroclinic velocity are smaller than the horizontal distances between the 2-3° longitudinal spacing between our PIES/CPIES. Our goal here is to look for compensation between flow in neighboring *spans, which is not necessarily related to the correlation length scales when we are using such broadly horizontally-averaged velocities. We have revised this paragraph to make our goal more clear.*

While the authors might disagree with me I'd like more discussion of what might cause the variability? They mention the north/south shifts in the large-scale upper western boundary current system – can they quantify that with an altimeter proxy?

We believe a detailed discussion using altimetry (which has ~20 years of ~10-day resolution data) to try and diagnose the observed variability in these relative short observational records (~10.5 months of daily data) is premature. Certainly the altimeter data will be useful for analyzing the upper ocean circulation when more data have been collected, and that is part of our future plans.

How much of the variability is due to the inclusion of intermediate waters?

As mentioned above, the transport variability is not sensitive to ±100-300 dbar changes in the vertical bounds of the integration layer, and we think we can exclude the inclusion/exclusion of the intermediate water as a significant source of the variability. Also note that at 34.5 ºS the high dissolved oxygen and low salinity waters characteristic of AAIW are centered at ~ 600-700 m. But a detailed discussion of the physical mechanisms producing the observed variability will require a longer record than we have at present.

It's interesting that the DWBC appears to have a full-water column structure – what does that mean?

This has been observed at several of the other locations where the DWBC has been observed using current meter moorings, dynamic height moorings or PIES/CPIES (for example, at 26.5°N and 42°N). Presumably this is related to the recirculation cells that seem to spin-up in virtually every location where the DWBC has been observed. Also note that though in the South Atlantic NADW is "sandwiched" between deep waters originated further south (UCDW and LCDW), the hydrographic data suggests these water masses are recirculating and flowing southward along the western boundary. However addressing this in detail is beyond the scope possible with the relatively short records we have in the South Atlantic to date. One of the longer North Atlantic records would probably be better suited to addressing this question.

The authors mention the dominated time-scale of about 10 days in the abstract. I can't find that discussion in the body of the manuscript. Could this be related to the bottom pressure variability discussed by Hughes et al. 2007 JGR 112, C01011 'Three forms of variability in Argentine Basin ocean bottom pressure' Is there a 10-day signal in the local wind field? Local wind-stress curl? Does the 10 days show up more in the pressure signals or the travel time signal?

The 9-10 day time integral time scale was determined for transports between the pairs of PIES, so it includes both baroclinic (travel time) and barotropic (bottom pressure) signals and is based on horizontal gradients between sites rather than at individual sites. A detailed analysis of the time scales involved in the various components (baroclinic vs. barotropic; onshore vs. offshore; relationship to wind forcing) is part of our future plans, but such an analysis would necessarily be fairly limited with the relatively short (10.5 month) records we are presenting in this paper and we feel it will be better done when we have longer records. As such, the reviewer is correct that this point is not dwelled on enough in our manuscript to merit mention in the Abstract. We have removed that phrase from the abstract.