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Ocean Science Discussions

Interactive comment on "Deep Western Boundary Current transport variability in the South Atlantic: preliminary results from a pilot array at 34.5 S" *by* C. S. Meinen et al.

Anonymous Referee #1

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This manuscript describes the absolute transport variability of the western boundary current in the South Atlantic at 34.5S. Absolute transport variability is determined with ∼10.5 months of measurements from an array of pressure-equipped inverted echo sounders (PIES) and from 27 years of output from a high-resolution ocean general circulation model (OFES; Ocean general circulation model for the earth simulator). Main results: Data show that variability is comparable to that found in the North Atlantic deep western boundary current. Model deep western boundary current variability indicates that 38 to 50 months of observations are required to adequately characterize the variability.

My major criticisms of the manuscript break down into two categories, 1) methodology to determine transport from the PIES array and 2) a thin/cursory discussion.

PIES methodology

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?

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. 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?

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

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

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be similar? What dynamics do these two locations share? What about comparisons to the Line W estimates?

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

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.

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.

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.

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

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

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?

Interactive comment on Ocean Sci. Discuss., 9, 977, 2012.