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Ocean Science Discussions

Interactive comment on "Atlantic Transport Variability at 25° N in Six Hydrographic Sections" *by* C. P. Atkinson et al.

Anonymous Referee #1

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General Comments:

I believe this paper makes a interesting and useful contribution to the field and a new perspective on an important dataset. This study examines the deep limb of the Atlantic Meridional Overturning Circulation while many other studies of 25N sections and Rapid-WATCH data focus on the upper ocean. The deseasonalization procedure used to remove the effect of the seasonal cycle from the hydrographic sections is new and possibly improved compared to previous studies. It is valuable to see the decomposition of the overturning systematically broken up into depth and temperature ranges. Therefore, I believe this paper is worthy of publication, but I believe that there are some flaws, below, that will need to be addressed before it is finalized. My overarching critique is that I believe that the authors have laid claims to the significance of changes in

transport that are intriguing and merit publication but do not appear to be statistically significant. This needs to be clearly stated throughout the paper.

Specific Comments:

(1) The authors do a good job projecting the uncertainty due to Ekman and Florida Straits transports onto the transports in each layer and the AMOC. I think the approach of Table 2 is excellent. However, there is no accounting for any ageostrophic velocity variability in the DWBC. The vorticity balance in the DWBC is different from the basin interior, so DWBC transport is not completely represented by geostrophy. Is there any way to use the direct current measurements of Rapid-WATCH in the DWBC to assess the possible additional error due to ageostrophic transport in the DWBC?

(2) In the text, I believe that the authors need to clearly differentiate between an error in the estimate of a mean (e.g. the +/- 2 Sv error for annual mean Ekman and Florida Straits transport) and a variability (e.g. the standard deviation of a time series, std(TEK) = 3.9 Sv and std(TGS) = 3.1 Sv in Kanzow et al., 2007). Using a variability would increase the uncertainties in the transports significantly. Please explain why errors in estimate of the mean are used instead of variabilities.

(3) Table 5: Why is the Ekman flux in the Rapid data different from the Ekman flux applied to the hydrographic sections? It seems to me that they should all be the same if the goal is to filter out interannual variability in the comparison.

(4) In Fig. 8, the authors show how transports in the different water mass ranges from the hydrographic sections compare to the envelope of variability in the Rapid-WATCH time series. The error bars for each transport must be shown since there is also uncertainty in those values.

(5) Page 123, lines 7-9: What is the basis for this conclusion? In the previous paragraph, it was clearly stated that only transports in UNADW (1981 and 2010) were outside the +/- 2 standard deviation envelope, not LNADW transports. How can there

be a “significant” change in LNADW?

(6) Pages 131-132, last paragraph of section 3.3

This whole paragraph was extremely unclear to me.

In the first sentence, how are the red lines in the bottom row of Fig. 13 “deviations in from density compensation”? Fig. 13 shows temperature and salinity separately and we see clearly that the water freshens and cools. However, the extent to which DSOW at 25N is density compensated is not quantified. Also, where is the effect of isopycnal heave on transport quantified?

The authors state that the changes in DSOW transport are not due to changes in water mass properties. Is this statement the same as saying that the DSOW export has not changed because the amount of DSOW has not changed? If this is indeed what the authors are arguing here, this is not surprising given the fact that most of the calculations presented here partition watermasses by depth, thus resulting in (nearly) constant amounts of each watermass in each section. Because the amount of (depth partitioned) DSOW is not changing, then the only way to change the transport is with a change in velocity, which is related to the slope of isopycnals. I think it would help the reader to see a direct comparison of the density structure from each hydrographic section. This comparison would have the added benefit of helping to quantify the effect of isopycnal heave and precisely where the isopycnals are moving the most (with a focus on deep isopycnals, of course). Finally, in order to be consistent with the other temperature class transport calculations, one should quantify changes in the amount of DSOW when watermasses are partitioned by temperature.

Next, the authors state that the temporal changes in DSOW transport are consistent with the cumulative transports in Fig. 9. Please explain this connection for me in more detail. The temporal pattern described in the text (page 132, lines 9-13) is not obvious to me in Fig. 9 (LNADW panel). This is important because I’m having difficulty seeing precisely where on the section the changes in DSOW transport are happening. I do not

see a consistent temporal shift in the lines of Fig. 9 (LNADW panel) at any particular location.

(7) What is the envelope of variability of the Rapid-WATCH data compared to the lines in Fig. 14? Just as it is important to assess the envelope of variability of the transports from Rapid-WATCH, to me, it seems critical to assess the envelope of variability in the DWBC shear. While the lines in Fig. 14 tell a compelling story, their spread may not be significant relative to the observed shorter term variability. Also, the authors should be explicit about the computation of dynamic height anomalies - there must be some assumed, steady, uniform dynamic height profile in order to make this comparison. What is this reference profile and are there any errors in using it?

(8) In Section 5, conclusion #3, I disagree with the use of the word “significant.” The transport values of LNADW do not lie outside the +/- 2 standard deviation envelope, especially when the errors in those transport values are taken into account (my comment #4). Conclusion #4 is correct, but it is important to point out that we don’t know whether or not the observed changes are significant with respect to the shorter term variability. In light of my comment #6, above, I had difficulty understanding the authors’ reasoning resulting in conclusion #5.

Technical Comments:

(1) In this paper, NCEP/NCAR data averaged over 2009 was used to estimate Ekman flux for 2010. It seems that 2010 data has been posted (although I have not used it yet myself). Why not use 2010 data?

(2) The black line in the left column of Fig. 8 is hard to see. Please make this a contrasting color (perhaps a light gray line?) so it is easier to see.

(3) Figure 8: I think it would help to show the +/- 2 standard deviation lines (of the Rapid-WATCH time series) across the whole length of the panels on the right side of the figure.

Interactive comment on Ocean Sci. Discuss., 9, 105, 2012.