Ocean Sci. Discuss., 6, C887–C891, 2009
www.ocean-sci-discuss.net/6/C887/2009/

Ocean Science Discussions

Interactive comment on "Modal composition of the central water in the North Atlantic subtropical gyre" *by* A. Cianca et al.

Anonymous Referee #1

Received and published: 9 December 2009

Review of Modal composition of the central water in the North Atlantic

A. Cianca et al.

Summary: This paper aims to identify the mode waters that comprise the North Atlantic Central Water by characterizing their weak vertical density gradients and the low temporal variability of their temperature and salinity signatures. Having identified mode water temperatures, the authors use temperature profiles and satellite sea surface temperatures to infer the outcropping regions of these mode waters.

General comments:

This work is relatively straightforward and makes a reasonable argument about the composition of NACW. However, I have a hard time seeing the contribution of this

work to the greater conversation on mode waters in the subtropical North Atlantic. I think that the presentation would be enhanced by stating a clear motivation to further characterize the composition of NACW, beyond what is available in the (extensive) literature on the subject. For instance, is the work simply meant to be descriptive, or are there implications for dynamics, climate, variability, and/or biogeochemistry? Furthermore, your stated objective, "to show that the eastern NACW in the subtropical gyre is composed by three modes," would be more effective if stated as a hypothesis, especially if there were alternative hypotheses that can be rejected in favor of this one. Finally, some major and minor technical issues that I outline below must be addressed before publication in Ocean Sciences is appropriate.

Major concerns:

One of the major conclusions of your work is that a third, as yet unidentified, mode water comprises a portion of the NACW. However, the evidence presented in support of this seems haphazard. The first piece of evidence is the small "uncertainty error" of the polynomial fit to the T-S curve, which is an unusual metric and should be more carefully justified. Also, you haven't precisely described how these errors are calculated, so it is difficult to evaluate them as an effective indicator of a mode water. My second concern is that the Eighteen Degree Water represents a clear vertical density gradient minimum in Figure 3, but its uncertainty error is not much different from the rest of the water column in Figure 2. In contrast, the "new" mode water is not a vertical density gradient minimum but is a clear minimum in the uncertainty error at ESTOC. Why do these metrics seem to be at odds? By definition, mode waters have a low vertical density gradient – so it is troubling that your new mode water does not seem to meet that simple criterion. This discrepancy must be explained. Third, the entire water column at BATS has a lower uncertainty error than any of the mode waters at ESTOC. Is this a result of having a larger sample size? Or is there actually more variability in the T-S relationship at ESTOC than at BATS? These are issues and questions that must be addressed in the evaluation of your hypothesis.

Finally, I am troubled by several aspects of Figure 4 (and Figure 5, insofar as the content is repeated in both figures). First, it's not clear what exactly is plotted in Figure 4&5. From the text, I think this is the standard deviation of the vertical density gradient, but I'm not sure whether the vertical density gradient is defined in the mixed layer or in a subsurface pycnostad layer. In addition to not knowing exactly what is plotted, I don't understand your choice of the metric to plot. I imagine that the standard deviation shows the temporal stability of the vertical gradient, but it is not clear how that reveals where the mode water is formed. The spreading of the mode water might create a very stable pool of low-gradient water downstream of its formation region, which would be reflected by low standard deviation, but this metric would not reflect the formation region of that mode water. I believe a more intuitive plot would be the average vertical gradient (or potential vorticity) at the densities of the mode waters (3-4 separate maps). The vertical density gradient will at least give an indication of where the mode waters are thickest, which will presumably be closest to their formation regions, before this signature is eroded by mixing.

It also makes me very uneasy that the 18° Water has the weakest signature in Figures 4&5, while it represents the least stratified of the mode waters (Fig. 3). I think this might be an indicator that the standard deviation may not be the best measure of the mode water.

Minor comments:

Page 2489, paragraph 2: It might be nice to mention why NASTMW is called 18° Water. "Gulf Current" extension, should be "Gulf Stream" extension.

Page 2489, paragraph 3: Your parenthetical (deep waters) is unclear. What are the "four" mode waters? I think you are referring to SPNAMW, Deep waters, ENAW, and STMW but the reference is ambiguous.

Page 2490, last paragraph: As mentioned in the general comments above, I think a more effective approach to stating the paper's objective is to offer a hypothesis that will

be tested. Similarly, rather than revealing the result ("one would have a subpolar origin . . ."), you could outline how that hypothesis will be tested and if there are other to be considered.

What is a "resemblance" method? It might be more straightforward to simply state that there is some visual resemblance between some characteristic of your new proposed mode water and the older, recognized mode waters.

Page 2493, last full paragraph: "[A comparison of the T-S at BATS and ESTOC] establishes the significance for both sites as representative of the NACW in the eastern and western regions." I disagree with this statement. Comparing the time series at two distinct sites cannot establish their representativeness for the regions surrounding them.

Page 2495: The reference to Palter (2005) gives credit for understanding the cause of interannual variability in subtropical mode water formation to a study that did no such thing. Perhaps Joyce (1996, Journal of Climate) would be more appropriate. If you want to motivate your study by making the case that understanding nutrient (or other property) budgets at BATS and ESTOC hinges on an understanding of the mode waters that get advected to the time series locations, the Palter reference would be more appropriate.

Page 2494, bottom paragraph: The minimum in the vertical gradient of the potential density anomaly is NOT a conservative tracer, even neglecting mixing and relative vorticity. Changes in the planetary vorticity, f, along the Lagrangian pathway of a fluid parcel will give rise to changes in the vertical density gradient; thus, it is the potential vorticity (the product of planetary vorticity and the density gradient) that is conserved away from the ocean's surface. I understand that in your maps and at the time series stations, f is fixed, but I still think it is worthwhile to be precise here.

You use "potential density anomaly" throughout the paper, which is officially what we call density (in kg m-3) - 1000. Unfortunately, I find the language confusing, since anomaly usually most often refers to the subtraction of a mean. You could simply state at the beginning of the paper that you will refer to potential density -1000 as the potential density and leave it at that.

Figure 4: I think this map should extend west of 65W? Much of the 18° Water recirculates west of there.

References: Missing the Schmitz and McCartney reference from the body of the text.

Interactive comment on Ocean Sci. Discuss., 6, 2487, 2009.