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Ocean Science Discussions

Interactive comment on "Influence of Ross Sea Bottom Water changes on the warming and freshening of the Antarctic Bottom Water in the Australian-Antarctic Basin" *by* K. Shimada et al.

Anonymous Referee #1

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Review of "Influence of Ross Sea Bottom Water changes on the warming and freshening of the Antarctic Bottom Water in the Australian-Antarctic Basin", submitted to Ocean Science by Shimada et al.

This manuscript analyzes deep decadal changes in temperature, salinity, and apparent oxygen utilization (AOU) in the Australian-Antarctic Basin and environs (most notably the western Ross Sea, an area that supplies some of the AABW that fills the Australian-Antarctic Basin). Warming, freshening, and increased AOU are all found in recent decades. It makes volume-integrated estimates of heat and freshwater content changes. It also formulates and applies a simple advective-diffusive model as well as

a box model in attempts to diagnose the causes of the observed changes, concluding that increases in mixing (related to reductions in abyssal stratification owing to the freshening and warming) together with reductions in bottom water formation rates can explain most of the observed changes. While the manuscript has much to recommend it, it will require major revision prior to being acceptable to publication. The advective-diffusive model replies on a key assumption that is very weak. The assumption that epsilon (the energy dissipation rate) is locally constant in the face of changed formation rates, changed density contrasts between AABW and ambient waters, and changed velocities is a key, and completely indefensible point, in the argument. Also, the box model appears to require that the volume of AABW is constant, an assumption that is clearly violated in the face of deep warming and freshening, both of which act together to very substantially reduce the volume of AABW with time. Finally, the grammar and usage in the manuscript is spotty. Some sections are pretty well written, but others require substantial work to reduce ambiguity in interpretation, increase conciseness, and improve readability. This reviewer will not comment extensively on the language, as reviews are supposed to be on science, and there is a co-author who can help with that task. Specific comments (major with *'s) indexed by page (P) and line (L) number.

1. P2199, L2-8. The first three sentences each would benefit from at least one supporting reference from the literature for the claims made.

2. P2200, L11. Change "the dens" to "dense".

3. P2200, L20. Is the coastal cooling (Jacobs, 2004; 2006) really for AABW, or a component of AABW? It is probably worth noting that the cooling signal is near the outflow, and may be subject to aliasing.

4. P2201, L3-4. Is the 2001 vs. 1997 reduction also potentially subject to interannual aliasing?

5. *P2201, L16-17. Here and throughout the manuscript, paragraphs of start with excerpts from the figure captions. These are poor topic sentences. In almost every

instance, this first sentence (usually containing “is shown in Fig. x”, “Fig. x shows”, or the like) can be eliminated, and the figure can be referred to parenthetically in the second sentence. Doing this improves the writing by starting the paragraph with a topic sentence that tells the reader the point of the figure, and makes the manuscript more concise by eliminating duplication of the figure caption. Even phrases like “(details are given in Table x)” can be shortened to “(Table x)”.

6. P2201. L22-23. Surely there is a reference in the literature for the flow of bottom water through the AAD?

7. P2201, L1-12. What about the potential by aliasing by the spring-neap tidal cycle (e.g. Whitworth and Orsi, 2006). This should be mentioned, and the fact that it makes interpreting changes within the outflow close the the AABW source (where the plume has not yet reached the bottom of the continental rise) from section to section very difficult to interpret, as they may be the result of quite short-term variability.

8. P2203, L23. What is the area over which the 0.37 W/m2 would have to be applied to account for the observed heat change?

9. P2203, L6, Neutral density anomaly has units of kg/m3. Please use them throughout.

10. *P2203, L24. Is it possible to assign an uncertainty to the change in freshwater storage? Given the likely uncertainty of 2 ppm in salinity from cruise to cruise, how much would instrumental errors contribute to this uncertainty?

11. P2204, L3. By “trivial” do you mean “small”?

12. P2204, L19-24. It is probably worth noting here that later on local changes in stratification are hypothesized to be quite important in accounting for the changes observed in the basin.

13. P2205, L16-17. The warming observed except near the source region does not appear consistent with the SR03 description of bottom cooling. Please comment in the

text on which might be more reliable.

14. *Section 3.2. As noted above, there are some serious weaknesses in this section:

14a. There is an entire literature on modeling descent of dense overflow plumes down slopes such as the Denmark Strait Outflow and the Mediterranean Outflow, such as Smith (1975, Deep-Sea Research), Price and Baringer (1994, Prog. Oceanogr.), and many papers that follow. Why develop a new model when these models, which extensive comparisons to observations suggest include much of the important dynamics, already exist?

14b. These streamtube models show that the amount of entrainment, eventual density of the equilibrated plume, and hence its transport depend importantly on the initial density and transport of the plume, the stratification of the ambient water through which it descends and from which it entrains, and the bottom topography, among other factors. Changes in the outflow plume density or transport WILL change the energy dissipation (epsilon), which governs entrainment, so the assumption that epsilon remains constant seems a huge flaw in the model presented in Section 3.2. The statement that “epsilon is expected to be constant in time scales of our interest since dynamical background that drives turbulence remains unchanged” is simply false. It is only this apparently erroneous assumption of constant epsilon that allows the inference that vertical diffusion will increase with decreasing density of the plume through Equation 7, a key, but seemingly indefensible, finding of this section.

14c. While a & b make this point moot, it is not clear that the fourth term on the LHS of equation 1 is negligible. While it is true that $w < u$, it is also true that w is larger for a descending plume than in many other parts of the ocean, and that $dT/z > dT/dx$, so it is not immediately obvious that $udT/dx > wdT/dz$. A more careful scaling of the LHS would be needed if this model were somehow retained in a revised manuscript.

14d. After P2207, L22 the language of this section becomes rather difficult to understand in places.

14e. P2208, L26 – P2209, L1. Please just write out the two cases. This large (small) shorthand is very awkward to read, and the second case can be made much more compact than the first when they are written out.

15. *The analyses in Section 3.3 and Appendix A also raises some serious questions as follows:

15a. In the OMP analysis, could there also be a LSSW end-member? If HSSW production is reduced, does it simply mean that LCPW is dominant, or could there LSSW replacing the HSSW? I am not sure of the answer to this question, but it did arise when reading the manuscript, since LSSW is (or was) produced somewhat east of HSSW in the Ross Sea.

15b. A big difference between the 1970s and the 2000s is that often there is little sign of HSSW, a change that is consistent with decreased export of HSSW or export of HSSW of less extreme properties. How is justifiable to ignore this fact and only include profiles near the source region where HSSW is present in the 2000s (P2210, L9-13)? Shouldn't these changes be accounted for in any model?

15c. P2210, L14-18. Please speculate as to why the large-scale salinity signal to noise is better than that for temperature.

15d. Again, if the assumption that epsilon is constant is indeed suspect, how much can the conclusions of this section be trusted?

16. *The analysis in Section 3.4 also has a potentially significant flaw. The volume V of RSBW is assumed to be constant. However, since RSBW is both warming and freshening, its volume is decreasing significantly. It would seem that the box model must take into account changes in the volume of the box, as the RSBW is not in steady state.

17. *P2212, L20-22. The results of Fig. 10 do not seem entirely consistent with those for SR03. This inconsistency should be discussed.

18. *P2213, L5-8. An entrainment ratio of 6:1 is surprisingly large with respect to other overflows and plumes. Is the LCDW end-member realistic? It seems very cold, almost within the RSBW. If warmer, saltier LCDW were entrained, would the entrainment ratio be smaller?

19. P2215, L26. Again, is the vertical diffusivity estimated by Polzin and Firing (1997) even appropriate near a descending plume? Streamtube models suggest that energy dissipation DOES change when the characteristics of the plume change, as well as when the ambient waters around the plume changes.

20. Table 2a. The volume of AABW in the Ross Sea varies!

21. *Table 2b. Does the thickness of AABW change along with the temperature and salinity? Is this change accounted for in the estimates?

22. Figures 4 and 5. Please show the units for the temperature changes, the salinity changes. Also, here and elsewhere, neutral density has units of kg/m3.

Interactive comment on Ocean Sci. Discuss., 8, 2197, 2011.