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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.

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We would like to thank Referee for his thorough reading of our manuscript and providing very constructive comments.

One of the major concerns is the application of our advection-diffusion model. The region we are considering here, namely the region that the assumption of the constant ε would be applied, is far away from the direct dense water outflow with vigorous entrainment/mixing by the gravity current. The situation is hence in the geostrophic regime, not the vigorous gravity-current regime, so we basically think the model is valid. This would answer the question concerning the OMP analysis and selection of the flow path.

Another major concern is reduction in volume of the AABW. We consider that effect of decrease in volume of the AABW is negligible because decrease in volume is rather small and its influence to the model results is similarly small (Please refer to the explanation in comment 16 for details).

We noticed in the manuscripts that explanation was not sufficient in some part and there were a few descriptions which may lead to misinterpretation. Thus in the revised manuscript, we appropriately changed the order of paragraphs, modified the descriptions, supplemented the explanation such that possibility for giving misinterpretation to be excluded. We believe that your points are now taken and helped us a lot to improve our manuscript. Our response follows, one by one, the list of the reviewer's comments.

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

Authors: We agree that first three sentences would benefit from citing literature. Thus, we cited Orsi et al. (1999) and Jacobs et al. (2004) for the first, Broecker (1997) for the second and Jacobs et al. (2004) for the third sentences respectively.

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

Authors: Done.

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.

Authors: The area and observations considered by Jacobs (2004:2006) is in the depth range of 2000-3000m and density range of AABW. However, the word "coastal" is misleading indeed and hence we removed it. The "coastal" cooling signal (Jacobs, 2004; 2006) may be subject to aliasing to some extent. However, it is difficult to estimate the effect quantitatively in this data-lacking area. The fact that the region considering here is far away from the variable outflow suggests the high-frequency signals are attenuated, and the limited data suggest the dominance of long-term trend. In this data-lacking region, we believe that it is worth to provide an information with reasonable possibility.

Hence we note the possibility of aliasing and this and related sentences now read as ;

"The AABW warmed over much of the basin between the years 1936-93 and 1994-96 (Whitworth, 2002), but cooled in the region between 140 and 150 °E from the year 1950 to 2001 (Jacobs, 2004; 2006) and at 140°E between the 1970s and 2002 (Aoki et al., 2005), although the magnitude of the trend is partly subjected to the higher-frequency signals. These overall changes in temperature with space and time suggest that possible changes in the relative contribution of different source waters may have also played a role in observed changes in the AABW."

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

Authors: We agree that decrease in export of dense water from the Western Ross Sea reported by Rivalo et al. (2010) may be subject to aliasing of interannual variability. However, though we did not make statement on the manuscript, Rivalo et al. (2010) also estimated lower export of dense water in 2003 (slightly higher than that of 2001) than that of 1997, and it will strengthen the existence of long-term signal. We considered that their estimates are worth citing as the maximum extent of long-term variability. Thus, we added the statement about their estimate in 2003 along with the possibility of aliasing in their estimates.

The revised sentence now reads as;

"Rivalo et al. (2010) estimated that the export of dense water from the western Ross Sea in 2001 and 2003 are 45% and 30% lower than that in 1997 respectively, although these estimates may be subject to aliasing due to higher-frequency variability."

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)".

Authors: Thank you very much. This suggestion is now implemented throughout the manuscript.

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

Authors: We followed schematic view shown in Fig. 1of Rintoul (2007), which is based on work of Orsi et al. (1999, 2002), Gordon and Tchernia (1972), and Mantyla and Reid (1995). We also can find equivalent description in Johnson et al. (2008) which is based on Donohue et al. (1999). Hence we cited reference as "(e.g., Rintoul, 2007; Johnson et al., 2008)".

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 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.

Authors: [We think this comment for P2202 L1-12, not for P2201 L1-12, from the sequence.]

There is a potential for aliasing in tidal cycle. However, we consider that tidal amplitude is not significantly large because extreme properties in this section locate on deeper range (3600-3800m depth, 63-64°S), sufficiently away from the source to the west (over 300 km from the Adélie Sill) and after adjustment to geostrophy.

We added sentences in the first paragraph of Discussion as

"There would be potential errors due to temporal aliasing of higher-frequency signals such as spring-neap tidal cycles (e.g., Whitworth and Orsi, 2006). However, most of the WHP sections used here locate well away from the bottom water source region, and, even near the source region, one-year mooring observation (e.g., Fukamachi et al., 2000,Williams et al, 2010) shows relatively small signal of Fortnum tide in summer. Hence the effect of such aliasing was not considered here, although it needs further quantitative discussions based on continuous direct observations such as moorings."

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?

Authors: The area over which the 0.37 W/m2 would have to be applied to account for the observed heat change is 3.55×1012 m2. This area corresponds to the area of 1000 km (meridional) × 3550 km (zonal) which are the typical meridional and zonal spatial scales of the Australian-Antarctic Basin. The unit TW in the original manuscript was a mistake and now corrected as GW (Referee probably noticed some inconsistency between 0.37 W/m2 and 1322TW). We would like to thank Referee for pointing this out.

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

Authors: Done.

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?

Authors: The error estimate of ±3 Gt yr-1 was now added, as was originally shown in Table 2a. This uncertainty derives from uncertainty of the change in SR3 (shown in

Table 2b).

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

Authors: Yes. Corrected as "small" accordingly.

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.

Authors: We agree that it is worth to mention the importance of discussing the change in density stratification here. We add sentences with parentheses that state about its importance on mixing and about its local changes are concerned to be important factor in later sections as

"(Density stratification is an important parameter in considering mixing (e.g., Gregg, 1989). As discussed later in Section 3, its changes in the downstream region of the Ross Sea are concerned to be important factor, although its local change in this basin is relatively small.) "

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.

Authors: Since the mean flow is westward in this region, there is no effect of inflow of the ALBW in the RSBW, upstream an east of the AGVL region. Thus, changes in bottom property are not necessarily consistent with those of the SR3 and the southern I9S which are strongly affected by inflow of the ALBW. We have added comment on a possible reason of this contrast between warming here and cooling in the SR3 and I9S.

The sentences are modified as ;

"Hence wide-spread warming is observed throughout the flow path of the RSBW except just near the source region (within 300 km from the Drygalski Trough). Thus, in the upstream of the AGVL region, where is not affected the inflow of the ALBW, situation is somewhat different from the downstream region where the cooling signal is obtained (SR3, southern I9S)."

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

Authors: Most of the concerns here and hereafter are come from the settings here are not well conveyed. This is partly because the explanation of the oceanography and available observations from the Ross Sea to Australian-Antarctic Basin was somewhat inadequate (development of the simple model was the result of data availability/limitation but that is not clear as it was). Hence, we changed the structure for Sect. 3.1 and 3.2 to explain the Ross Sea settings and limitation in available observations. Along with the revision above, we reversed the order of Figs 7 and 8, and the region of the modification of the MSW is excluded from Fig. 9 for conciseness.

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?

Authors: The stream tube model that Referee suggested is developed for examining mixing process of descending dense plume, which is in the state of gravity current, and entrainment process involved in the motion is incorporated as the primal mixing process. However, the region we treat is the flow path of the RSBW, equivalent to downstream region beyond 150 km from the mouth of the outflow region, where the slope is moderate. Thus, relatively calm mixing in the geostrophy regime rather than strong entrainment is expected. Hence, for this purpose we developed a simple model of our own.

In addition, we did not analyzed the region of the modification of the MSW (roughly equivalent to the region from the shelf break to end of the slope) for the reason that observed data are extremely less and inhomogeneously distributed over the region in 1970s (we have only 4 data at around 40km downstream from the Drygalski Trough as shown in Fig. 8 of original manuscript).

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.

Authors: We consider that assumption that ε is constant is applicable for following reasons.

The mixing process expected in the flow path of the RSBW, where we examined advection-diffusion process, is mixing driven by vertical shear reside in internal wave field rather than vigorous entrainment driven by gravity current. We simply assumed ε to be constant because it is expected that energy provided to larger spatial scale motion (e.g., tide), which provided energy to turbulence, is constant in decadal or longer time-scale.

Even if assumption that ε is constant is not quantitatively reasonable, the inference that vertical diffusion will increase with decreasing density of the RSBW, which is a key finding of this section, is still consistence with literatures on scaling of ε and/or Kz. For example, Gargett and Holloway (1984) proposed scaling of Kz is proportional to N-1 or Kz is proportional to N-0.5 and furthermore, in the region near topographic boundary, scalings which relate Kz to inverse of N are proposed (e.g., Fer, 2006: Kz is proportional to N-1.4$\pm$0.2, Mackinnon and Gregg, 2003: Kz is proportional to N-1). (Please note that the negative values following after N denote exponent on N all through this Authors comment.)

The sentence that

"ε is expected to be constant in time scale of our interest since dynamical background that drives turbulence remains unchanged."

is slightly modified as

"Here, we simply assume that ε to be constant in decadal or longer time-scale since energy provided to larger spatial scale motion (e.g., tide), which provides energy to turbulence, is constant (validity of this assumption will be discussed later.)."

and, following sentences are added to the fourth paragraph of Sect. 4.

"ε was assumed to be constant (Eq. 3) since it is expected that energy provided to larger spatial scale motion is constant. This assumption is considered to be applicable because, even if it is quantitatively not reasonable, the inference that vertical diffusion will be enhanced with decreasing density of the RSBW is still consistence with literatures on scaling of ε and/or Kz. For example, Gargett and Holloway (1984) proposed scaling of Kz is proportional to N-1 or Kz is proportional to N-0.5 and furthermore, in the region near topographic boundary, scalings which relate Kz to inverse of N are proposed (e.g., Fer, 2006: Kz is proportional to N-1.4$\pm$0.2, Mackinnon and Gregg, 2003: Kz is proportional to N-1)."

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.

Authors: As explained for 14a, in the flow path of the RSBW where we examined advection-diffusion process, descending motion of plume would be no longer vigorous and horizontal velocity along flow path should be dominant. Thus, we consider that fourth term in LHS of equation 1 is negligible. We did not revised manuscript in regard to this comment because explanation on such geographical details precedes this scaling in the revised manuscript.

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

Authors: We have extensively rewritten this section.

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.

Authors: The sentence is modified as ; "Hence, when volume transport uH is large, vertical gradient θz should be large (and/or horizontal gradient θx should be small). If uH is small, θz is small (and/or θx is large)."

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.

Authors: The main source of RSBW is HSSW according to the literatures like Jacobs et al. (1970). As suggested, this situation may be subjected to change when the HSSW property. However, in reality, LSSW is freshening at much higher rate than HSSW (Jacobs and Giuilivi, 2010), and so this is not likely. Jacobs and Guilivi (2010) also explained that the salinity change in the RSBW shown in Fig. 6c of this manuscript can be explained by salinity change of the HSSW (their Fig. 2 and 13). Thus we consider that, at least until the year 2004, there is little need for inclusion of the LSSW end-member in the OMP analysis.

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?

Authors: This question probably comes from misunderstanding of what we have done in our analysis. Our analysis domain here is the well downstream region away from the outflow. To avoid the ambiguity in the data coverage, we have analyzed the region of RSBW flow path where spatial density of the profiles is equivalent between the 1970s and 2000s. Thus we consider that advection-diffusion process is equally evaluated for the both periods. In the region of the modification of the MSW, we recognize big difference in spatial density of observed data between these periods. . However, as pointed out by Referee, we could find some descriptions which may lead to misinterpretation (e.g., P2209, L22-24). Thus, those descriptions are deleted in the revised manuscripts.

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

Authors: In the region near the source (e.g., in the region less than 250 km from the Drygalski Trough), local large spatial difference often result in large noise due to narrow nature of the RSBW flow. In fact, noises, possibly due to meso-scale structures, in temperature in 2000s are large enough to make the large-scale signal unclear. How-

ever, as for salinity, freshening of the RSBW resulted in smaller contrast between the ambient water and thus, smaller noise particularly in 2000s. Hence, the large-scale salinity signal to noise is better than that for temperature which still maintains large contract between the ambient water.

The sentences are modified as ;

"In practice, the relative contribution of the large-scale pattern and small-scale variability along the flow path of the RSBW is different for temperature and salinity near the source region. While the large-scale pattern of temperature and its gradient are subject to significant small-scale perturbations, those of salinity and its gradient are dominant over small-scale spatial variations. This tendency becomes remarkable in the 2000s because salinity contrast between the ambient water is reduced in this period due to the freshening of the RSBW while that in potential temperature remained relatively unchanged."

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

Authors: Please refer to the explanation in comment 14b.

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.

Authors: [We think the term "RSBW" here refer to the AABW in the Australian-Antarctic Basin in our definition from the sentences in general comment and the quotation that V of the RSBW is constant.] We consider that effect of decrease in volume of the AABW is negligible because it is estimated that decrease in volume is rather small and its influence is similarly small as follows.

Decrease in thickness of the AABW layer (γn>28.30 kg m-3) are rather small and estimated from the 3 sections as 11-14% during the year 1995 -2005. By incorporating 14 % of decrease in volume in the model (Eq. 9), its influence is estimated to increases originally estimated influence of the RSBW (Table 4) only by 10%. Hence, we consider that effect of change in volume of the AABW is negligible.

In addition, section and basin wide change (Table 2) are assessed in fixed volume of water below temporal mean isopycnal of γn = 28.30 kg m-3 (shown in Fig. 4 and 5) and thus, change in thickness and/or volume of the AABW is not considered. We consider that this procedure is justified because decreases in thickness of the AABW layer are small.

We added sentences in the first paragraph of Discussion as

"The estimates of the section and basin-wide changes (Table 2) are calculated in the fixed volume of water below mean isopycnal of 28.30 kg m-3. "

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

Authors: Please refer to the explanation in comment 13.

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?

Authors: This is reasonable. This happened because we consider this rate over the long pathway of RSBW from the shelf to 150°E while this type of estimation is usually done for the vigorous outflow process.

Limiting for the downslope process, for example, according to Fig. 2B of Gordon et al. (2009), potential temperature and salinity of the possible end member are less than 1°C, below 34.72 in the upper slope (shallower than 1000m depth). However, the

recirculating AABW of potential temperature -0.5∼0 °C is likely playing important role in the mixing process in more deeper range. If we take the potential temperature of 1°C, salinity of 34.72 (this should be upper boundary) as the end member, mixing ratio of the HSSW and the LCDW would be about 1:3 . By taking contribution of the recirculating AABW into consideration, however, above end member is not realistic and lower value for temperature and salinity should be applied. Thus, we followed definition of Gordon et al. (2004) for LCDW (0°C, 34.69) who estimated generally accepted mixing ratio of 30:70 (or 1:2.3) at mid slope.

However, the purpose here is to estimate the plausible volume transport into the Australian-Antarctic Basin box, and not to estimate the entrainment process itself. We have done this based on observation, and hence we exclude the sentences related to this estimation.

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.

Authors: The region and situation of interest is not around the plumes. We have applied an estimate of Polzin and Firing (1997) to examine heat flux across the isopycnal of γn = 28.30kg m-3, which locate 700-850m above the bottom (e.g., Figs. 4 and 5), but not to near the descending plume (P2204, L15-19).

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

Authors: [We think the term "Ross Sea" here refer to the Australian-Antarctic Basin.] Please refer to the explanation in comment 16.

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

Authors: Please refer to the explanation in comment 16.

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.

Authors: Done.

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