RESPONSE LETTER

Dear Dr. Matthew Hecht (Editor, Ocean Science)

Please find enclosed a revised manuscript based on submission #OS-2014-12**,** which was originally titled **"ASSESSMENT OF THE ECCO2 REANALYSIS ON THE REPRESENTATION OF ANTARCTIC BOTTOM WATER PROPERTIES"** by M. Azaneu et al. The manuscript has been carefully revised in response to the reviewers' comments and suggestions. The reviewers considered the study an important tool to improve modeling accuracy in the Southern Ocean and of interest to the community. The reviewers indicated that the article presents a careful comparison, being commendably complete. However, their suggestions differ on some important issues. Referee 1 requested to develop the discussion, focusing on the model progression prior to 2004, which preempted the large polynya in the Weddell Sea, and also the effects of this event. In contrast, referee 2 suggested splitting the article and evaluating the reanalysis by considering only the period before the polynya. As previously discussed with the editor, we followed the suggestion of referee 1 and extended the discussion on the polynya event. We sought to accommodate both reviewers' opinions throughout the text. Detailed responses (**R**) to the reviewers' comments are shown in *italics* below. Revised sentences are indicated with blue font. We thank the reviewers for their suggestions, which substantially improved the manuscript. Additionally, the manuscript was carefully revised by a professional English language editing service.

Sincerely,

Marina Azaneu

(Corresponding author)

Authors Responses (R) to Reviewers:

Referee 2

The manuscript aims at providing the science community with an assessment of a new reanalysis product. It focuses on the areas where the lack of observations is dramatic and where previous reanalyses did not perform well. It is important for an accurate modeling of the Southern Ocean that such products are made available, but also that their biases are identified. I agree with Reviewer 1: the manuscript in its current form is too long, quite messy, and contains too much information going in too many directions. It fails at answering the simple questions raised by the abstract, so that in the end we still don't know how good ECCO2 is at simulating AABW. It seems to me that there are two different stories in your manuscript: the assessment of ECCO2 during the reliable period, and the polynya times and their associated issues. It is particularly confusing, as you described the polynya times as "unreliable", yet anyway comment on how close the results after 2004 are to the observations. I would suggest turning the current manuscript into two papers. The first one would be a skimmed version of the present manuscript, dealing only with the representation of AABW in ECCO2 in the "reliable period" (until 2004). The second one would talk about the complex issue of the polynya opening in ECCO2, trying to identify the reasons for this opening (you mention a few in your analysis) as well as the impacts of the polynya on the representation of AABW. You would target a wider audience with two distinct papers, and each paper would be far clearer than the current one. I agree with Reviewer 1 that your figures are too small, but for most of them it does not matter too much. Figures 2 (in particular its inserted panels), 5 and 6 are the only ones that really need to be larger in the revised text. See below for more

comments about all the figures. I also agree that you are too vague in your assessments: you need to give actual values rather than saying that something is “close” or “relatively good”.

R. *We greatly appreciate the detailed review from referee 2. His/Her suggestions were incorporated into the revised manuscript, which clarified some critical points in the discussion and improved the overall quality of the manuscript. However, after a discussion with the editor, it was decided not to split the article and instead to further develop the discussion regarding the conditions that contributed to the polynya event. We agree with the reviewer that the manuscript is long and contains too much information. In the revised manuscript, we sought to exclude non-essential information and maintain only the primary results presented more directly. The vague assessments indicated by the referee were removed from the discussions. The figures were modified for better visualization. In addition, the figures considered to be complementary information were moved to the supplemental material.*

Comments on the science/ideas

1. p1027, last paragraph: you’re going in circles. You’re actually saying that you have no observations, hence you need models, but you can’t trust models so you need to evaluate them against observations...

R. *We argue that observations in the Southern Ocean are summer-biased and restricted to specific sampling campaigns. Therefore, the observations are not capable of providing a complete view of the time-varying aspects and connections between the relevant processes. However, we believe that if the reanalysis product is able to reproduce the mean state of the ocean and to produce estimates that resemble the time discrete ocean samplings, it is possible to fill the time and space gaps of the in situ data with the reanalysis information. Therefore, the reanalysis output would be compared to the individual snapshots provided by the observations so that the product can be used for studies that require more continuous data and high temporal and spatial resolutions.*

2. p1032: you say you are defining AABW with a neutral density criterion. Why do you show the temperature and salinity fields in the results then? It seems like you are only highlighting more biases by doing so.

R. *Recent studies have defined AABW based on the neutral density threshold ($\gamma^n \geq 28.27$ kg m^{-3}) south of the Subantarctic Front (e.g., Whitworth et al., 1998; Orsi et al., 1999; Talley et al., 2011; Kerr et al., 2012; Azaneu et al., 2013; Huhn et al., 2013; Ohshima et al., 2013; van Sebille et al., 2013; Jullion et al., 2013). Besides being more robust, this definition allows for more precise comparisons between different estimates because there was no agreement on the definition of this water mass until recently. However, despite the agreement on neutral density surfaces represented in the reanalysis and in situ data, the temperature and salinity discrepancies compensate each other in some cases, leading to a misleading representation of the water mass. Therefore, we believe that information regarding the hydrographic properties that have a direct effect on the water mass density is important for judging the quality of the water mass representation. We decided to include the temperature and salinity information in the supplemental material when not essential to the main conclusions of the study (e.g., Figs. 9, 10 and 11).*

3. p1032 (again): why don’t you consider the depths 600-3000 m? I am not saying you should study them, but explain why you decided not to show them.

R. *We agree that the criterion for defining the layers is not clear. First, the main goal of the manuscript is to evaluate the bottom layer of the Southern Ocean. Information regarding the rest of the water column is used to better understand the representation of the bottom layer. The* θ–S *diagrams were already used to show distributions of the hydrographic properties for the entire water column. However, we dedicated the averaged layers described in page 1032 to evaluating the geographical distribution of the hydrographic properties in strata that involve the core of water masses that are important for AABW*

formation. The surface layer includes the shelf water masses and the mixture of water masses at the continental slope. The intermediate layer encompasses the core of the WDW in the Weddell Sea (Orsi et al., 1993), including the water mass at intermediate depths that reaches the continental slope and contributes to dense water formation. We added this information to the revised manuscript (Lines 253–259):

"Three depth ranges were selected for determining climatological averages. The surface (SL), intermediate (IL), and bottom (BL) layers result from the average of the ECCO2 levels from 100–150 m, 409–634 m, and 3000 m to the seabed, respectively. The SL includes the shelf water masses and the mixture of water masses at the continental slope. The IL encompasses the WDW core in the Weddell Sea (Orsi et al., 1993), which corresponds to the water mass at intermediate depths that reaches the continental slope and contributes to dense water formation. The BL includes both deep and bottom water masses."

4. p1033: your explanation of Taylor diagrams seems very long and tedious.

R. *We agree. The Taylor diagram explanation was reduced as follow (Lines 277–284):*

"In each sector, the correlation coefficient, centered root-mean-square (CRMS) difference, and standard deviations were computed for the previously defined layers (SL, IL, and BL) from both datasets and considering each hydrographic parameter (θ, S, and γ^n). These statistical parameters are summarized in a Taylor (2001) diagram in which the observational field is considered as a reference (R). Reanalysis fields that are more consistent with observations will be located closer to the 'reference' point. The closer the reanalysis standard deviation is to the observational standard deviation, the better the spatial patterns are represented"

5. p1039: what do you mean by "unusual feature" (line 4)? There is no clear anomaly compared with the observations, so it does not seem unusual. Is it unexpected because normally models struggle to represent it?

R. *We wanted to indicate that the presence of the 28.27 kg m^{-3} γ^n isopycnal in the open ocean regime at intermediate depths is an unusual feature because there are no such dense waters in this level in the real ocean. However, after modifying the article, the layer averages for the entire reanalysis period (1992–2011) are not presented. Therefore, this description was excluded.*

6. p1039: line 24, is it denser because of the temperature or salinity biases? line 29, the density cannot be overestimated because the layer is fresher, it's only because it's colder.

R. *We agree. The sentences were modified as follow:*

"The reanalysis representation of the SL in the Southern Ocean is generally denser than the observations (Fig. 5), which is primarily because the waters are saltier than expected." (Lines 448–450)

"As a result of a fresher intermediate layer, the IL density is underestimated in the B&A and Ross Sea sectors (Fig. 6)." (Lines 450–451)

7. p1040: line 1, why is the density underestimated in BA and circumpolar shelves? P1040, and later in the text: you just spent several pages commenting on the inaccuracies of ECCO2, you can't really say now that it has "a good representation"!

R. *The first sentence highlighted is not included in the revised version of the article because the period (1992–2011) was excluded during the revisions. Regarding the statement "a good representation", we agree with the referee. The article's discussion and conclusions were modified in several ways. Different considerations were included in the reanalysis performance. Please refer to the "Summary and Conclusions" section for details.*

8. p1041: That is too long for something you don't show. As I said at the beginning of the review, I think you should show it, but in a different paper.

R. *In the revised manuscript, we focus on the 1992–2004 results. Therefore, the figure corresponding to the highlighted paragraph is now included in the results.*

9. p1042 and throughout the result section: as each subsection is quite distinct, you should finish them with a brief conclusion, a sort of "bring home message" for what was just tested.

R. *We agree with the referee. A brief conclusion and discussion were added at the end of each section. Please see below for an example from section 3.1:*

"The results based on the climatological ocean state indicate that the hydrographic properties represented by the reanalysis have an average distribution in the θ–S space that is similar to the observations. Following the opening of the polynya in 2004, the Weddell Sea sector of the Southern Ocean became flooded with dense waters, indicating that the relevant physical processes are not correctly represented by the reanalysis during this period. Before 2004, the main oceanographic surface water features are reproduced by the reanalysis; however, these waters are generally denser than suggested in the observations, while deep waters are primarily less dense than expected. The intermediate layer is statistically the closest to the observations, while the deep waters have the worst representation. The misrepresentation of surface waters is possibly due to the lack of skill in reproducing several complex processes that act on the ocean surface in the reanalysis output. This limitation is compounded in coastal waters by the absence of ISW, an important predecessor of AABW (Foldvik et al., 2004). This absence was expected because ice shelves are not considered in the ECCO2 product. Less dense deep waters are related to the lack of the coldest and densest AABW varieties, which possibly result from the absence of newly formed dense waters spilling off the shelf in the reanalysis. Moreover, the coarse vertical grid resolution at greater depths implies that deep waters are represented by the average properties of approximately 400 m of water column, which leads to a poor representation of the approximately 100-m-thick bottom water layer." (Lines 476–493)

10. p1043: line 13, the current colorscale won't allow the reader to see that. I am also puzzled by the subsurface dense bias – is it at the same depth as the warm bias?!

R. *Yes, the density bias is coincident with the warm bias. The referee's question is valid because an underestimation of the density would be expected in this case. However, comparing the absolute salinity profiles from the different datasets, the surface fresh layer is slightly thinner in the reanalysis than in the observations, which causes an overestimate of the subsurface salinity and density. This feature is more evident in the salinity difference profile, which was added to the supplemental material. We modified the passage for clarify and added this information to Lines 515–518:*

"The subsurface density overestimation is coincident with the temperature bias indicated by the θ = 0°C isoline; however, this result is caused by the smaller thickness of the surface fresh water layer that is reproduced in the reanalysis compared with the observations."

11. p1044: end of the page, you do not comment on the process leading to colder WDW (that you mention line 25).

R. *Following the changes in the main discussion topics in the manuscript, this passage was removed.*

12. p1047: the fact that the 0_C isotherm corresponds to your isopycnal seems like a lucky coincidence, at least in the way you phrase it. Maybe rephrase this sentence so that it feels less like you tried any possible diagnostic to get something coherent.

R. *We believe that the correspondence of the Θ = 0°C isotherm and the γ^n = 28.27 kg m^{-3} isopycnal is not a coincidence. As discussed in the manuscript, the AABW upper limit in the open ocean can be defined in different ways, including Θ = 0°C and γ^n = 28.27 kgm^{-3}, which should reasonably coincide.*

Throughout the manuscript, the reanalysis tends to represent AABW that is warmer and lighter than expected. In some regions around the Southern Ocean, bottom waters are slightly saltier than suggested by the observations (see figure 1 below). This deviation can partially compensate the warm bias and bring the γ^n = 28.27 kg m^{-3} isopycnal close to the temperature threshold. In many cases, the reanalysis does not properly represent AABW temperature and salinity properties; however, the reanalysis does maintain a good representation of the expected AABW limit depth around the Southern Ocean when considering the density threshold (Figs. 12 and 18). We modified the sentence for better clarity (Lines 313–318):

"The AABW definition used for the volume transport calculation follows our previously defined threshold (i.e., γ^n = 28.27 kg m^{-3}) for consistency throughout the manuscript. However, in this section, it is important to notice that the AABW produced by the reanalysis is warmer than expected. Consequently, the isotherm limit used by Fukamachi (2010) (i.e., waters colder than 0°C) is not present in the reanalysis section during the comparison period."

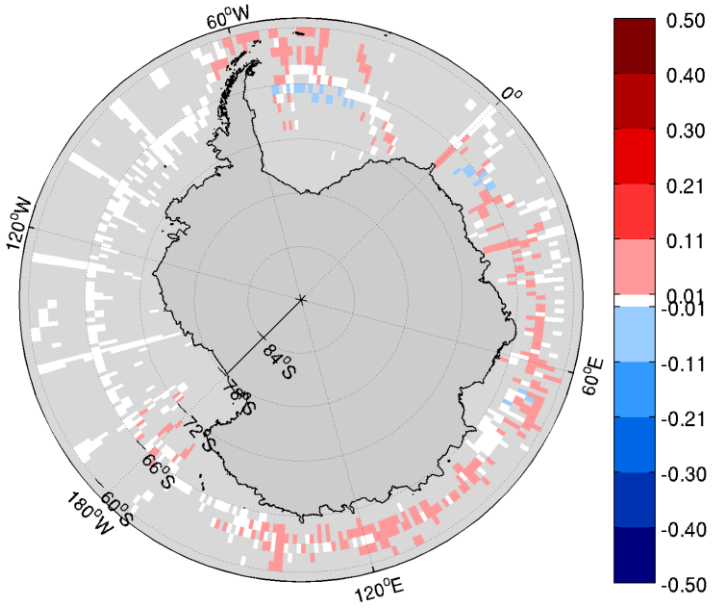

Figure 1: Difference in the salinity fields from the different datasets for the bottom layer (BL).

13. p1048: lines 6-7, the observed signal is actually drowned in its internal variability, you can't expect to have better results unless you get a longer observational timeseries.

R. *We agree. We added this issue to the manuscript on Lines 603–606:*

"The rugged bathymetry and poor model spatial resolution can contribute to the difficulty of the reanalysis output reproducing the deep and bottom volume transport variability. Another important factor that could have contributed to a low correlation in this analysis is the short length of the available time series."

14. p1048: line 24, are your correlations significant? M2 in particular seems too short for correlation tests

R. *We agree with the referee that the M2 time series is too short to allow for strong conclusions despite the statistical significance of the correlations. The M2 current meter sampled from April 1999 to May 2004 has a continuous gap of eighteen months. In the case of M3, which sampled from April 2000 to*

January 2007, with a continuous gap of thirteen months, a more complete monthly time series is proved. We believe that this latter time series is suitable for correlation analysis. The reanalysis and in situ time series exhibit a linear correlation of 0.57, which is found to be statistically significant ($p<<0.05$). We modified the manuscript as follows:

"The monthly velocity time series of the deepest current meters from each mooring are well correlated ($r = 0.58$ for M2 and $r = 0.57$ for M3) with velocities from the ECCO2 reanalysis at approximately the same depths (Fig. 14a). Despite the statistical significance of these correlations, current meter M2 has a short time series and an eighteen-month gap during the four-year sampling period; therefore, these correlation results should be considered with caution." (Lines 614-618).

15. p1051: these findings seem counter-intuitive, the unreliable period should not be a better estimate, unless the representation of processes in ECCO2 is completely wrong. Maybe have a look at Latif et al. (2013) and their hypothesis that polynyas/deep convection are a normal feature of the Southern Ocean that we're simply not observing currently + studies showing that deep convection –at least in models- is the most effective way to form and modify AABW.

R. *We believe that the description of the results was not sufficiently clear and may have caused a misinterpretation of the results. During the reliable period, the reanalysis reproduces very low velocities and transport estimates. With the opening of the polynya (i.e., the unreliable period), there is intense dense water production, leading to an abrupt increase in current velocities and volume transport that exceeds the observationally based estimates in most of the cases. The results indicated by the referee compare the reanalysis-averaged cumulative transport from the K05 study (during the reliable period) with the averaged estimates for the entire period (i.e., 1992–2011). Due to the velocity and transport increases during the final years of the studied period, the average estimate for the entire period is higher than those from the reliable period and is closer to observations. However, this statement can be misleading; this result was removed from the revised manuscript.*

16. -p1053, line 19 onwards: you should average only over the reliable period.

R. *We agree. The result previously presented in Line 19 was removed from the manuscript. However, the remaining estimates were determined only for the period preceding 2004.*

17. p1055: you again conclude that what you looked at is "well represented"... not really! In particular, that is disturbing that twice you actually found a good match between ECCO2 and the observations, but only if you consider the unreliable period.

R. *As discussed before, the averaged estimates were re-determined by considering only the period before the polynya.*

18. p1059: if you decide to keep the paper as a whole rather than separate it, you will need to talk about the polynya far earlier than here. It was quite frustrating seeing all the hints you've dropped through the text and having to wait until the end of the results to have the answer.

R. *Following the referee's suggestion, we moved the discussion about the polynya to the first section of the results (Section 3.1).*

19. p1061: your comments on figure 16 are too short, you don't even mention all the panels.

R. *We decided that this figure does not provide a substantial contribution to this study; therefore, the figure was excluded from the current version.*

Conclusions: you need to re-refer to the figures to help the reader follow, and you do not compare your work enough with the rest of the literature (in particular ECCO and other reanalyses).

R. *We agree. We added figure references in the conclusions and compared the results with other studies, such as Heuzé et al. (2013), Martin et al., 2013 and Dotto et al., (2014). However, the authors are not aware of studies using the ECCO2 global product over the Southern Ocean.*

20. p1061, you need to moderate your assessments or give more precise values: from what you said earlier, no it does not seem to me that ECCO2 "provides a good quality representation" (idem p1064, I would not say "high quality")

R. *As discussed before, the article was modified to account for these considerations and to present more direct comparisons.*

21. p1062, mention that the temperature was better represented than the salinity and density in fig5. You also can't give a single message about the 4 case studies: in the result sections you do not study them looking at the same parameter and do not seem to follow a storyline from one to the other.

R. *We did not find a clear pattern in figure 5 to define a parameter that was best represented in the reanalysis. In contrast, we conclude that the IL is the layer that best resembles the observations. This information was added to Lines 491–493. Regarding the 4 case studies, we simplified this section by decreasing the number of evaluated variables. We restricted the studied variables to the AABW volume transport in case studies I and IV and to the AABW current velocity in case studies II and III. The summary of the results from the case studies is as follows (Lines 694–705):*

"The case studies reveal that the temporal variability of the dense water volume transport (case study IV) and current velocity (case studies II and III) determined from the reanalysis are correlated at a statistically significant level with the observationally based estimates in the regions that are most important for AABW export. Moreover, the intermittent characteristic of AABW production (case study IV) is reproduced by the reanalysis output. However, in Section I (located near the Kerguelen Plateau; case study I), the rugged bathymetry and the relatively low model resolution are possibly important factors that contribute to the difficulty in reproducing the regional variability in AABW volume transport. The absolute AABW current velocity and volume transport in all of the analyzed cases are underestimated by the ECCO2 reanalysis product before 2004. There is no export of WSBW along the northern tip of the Antarctic Peninsula (Section II; case study IV) before 2004, while its transport through the meridional Section III is considerably low, making it indistinguishable in the open ocean."

22. p1064: line 8, which parameterizations? lines 13-15, redo the trends with the "reliable period" only, and I'm afraid the patterns will disappear.

R. *We were referring to the sub-grid physical parameterizations that may affect the representation of dense water, such as vertical mixing, convective processes and density downslope flow (which is important because it is a z-coordinate model) and that include parameterizations linked to small-scale ice mechanics, such as melting rates, brine rejection and the growth of sea ice. This information was added to the text (Lines 884–887):*

"Improvements in the limitations related to model resolution, ice shelf representation, and sub-grid physical parameterizations, e.g., vertical mixing, convective processes, downslope flow and sea ice growth, must be considered for optimal results."

Section 3.4 and table 2 present trends determined based on the periods 1992–2004 and 1992–2011.

23. p1065: ARGO floats and seagliders do not go deep enough (yet) to be relevant for studying AABW.

R. *We agree that Argo floats and seagliders do not reach the deep depths of the AABW in the open ocean regime. However, innumerous questions regarding AABW variability are related to possible changes in the source of AABW dense water production rates in the shelf-slope regime. Argo under ice floats and seagliders are emerging techniques for sampling continental shelf break regions and are possibly capable of providing important information on dense water production (e.g., Heywood et al., 2014).*

2 Comments on the figures

Figure 1 contains a lot of information. Consider not indicating on the map the regions that you don't talk about in the text.

R. *We agree. We reduced the geographic information in the map.*

Figure 2 really is too small. TS diagrams for the whole water column are OK, with the exception of the bottom right one whose legend cannot be read. The inserted panels are far too small. They don't need to be as big as the full depth ones, but as you have plenty of space around your "big" panels you should easily find a way to make the 500m-bottom panels more visible.

R. *We agree with the referee. Considering that the focus is on AABW, we maintained only the* θ–S *diagram for the water column below 500 m. The markers were reduced in size to provide a better visualization of the information. We used only two colors to distinguish between the datasets and added the proximal neutral density surfaces.*

Figure 3: the size of the figures is correct but the fonts would need to be bigger on the colorbar. You should also use a discrete colorscale for all the variables and not just the density. That would be easier to visualize, and would help follow your text in which you anyway mention specific values in temperature and salinity.

R. *For better visualization, we split Fig. 3 into Figs. 5, 6 and 7. We also increased the size of the numbers in the color scale. The color scales were also adjusted to coincide with the text. Complementary figures for the temperature and salinity difference fields were added to the supplemental material.*

Figure 4: the coordinates are all over the place! Maybe you should omit (most of) them.

R. *Following the reviewer's suggestion, we omitted part of the maps' coordinates. We also reduced the number of maps presented and increased their sizes. Fig. 4 is now Fig. 3 in the revised text.*

Figure 5 needs to be bigger. The worst one is the surface layer (a) where several region labels are on top of each others. If you do not want to use more space by turning it into three different figures, I guess you could use numbers for each regions, or even only put the points and draw arrows until the empty areas of the Taylor diagrams where you could put the labels.

R. *We improved the figure by increasing its resolution and also by replacing the sector labels with numbers, as suggested. The time period used for the comparison was also added to the caption of Fig. 5 (which is Fig. 8 in the revised manuscript).*

Figure 6 needs to be bigger as well. The colorscale for the salinity is not really adapted to what you want to show us, too saturated in the red. You should saturate more in the blue, a bit like what you did for the temperature section in SR4 (fig 6b).

R. *For better visualization, we split Fig. 6 into Figs. 9, 10 and 11. Moreover, the panels referring to the observed averages, TS diagrams and the map were removed. The color scale for salinity was modified according to the suggestions. Complementary information regarding temperature and salinity differences was added to the supplemental material.*

Figures 7 to 11 are fine.

R. *We agree. The middle panels were removed from Fig. 7 (which is Fig. 12 in the revised manuscript).*

Figure 12: you comment on c before you comment on b, so they should be reversed. Again, the colorscale for the density section does not seem very appropriate. The use of a discrete colorscale instead of the current continuous one may make the figure clearer.

R. *Panels (a) and (b) were removed in the revised manuscript (Fig. 17).*

Figure 13 is fine to me.

R. *To improve the figures, the longitude information was decreased and the panels referring to the AABW volume anomaly were removed. Fig. 13 is Fig. 18 in the revised manuscript.*

Figure 14 is a bit small, but that may be improved by rearranging into two columns of three lines each (having a next to b instead of on top of it).

R. *Figure 14 was excluded from the article, and its information was added to table 2.*

Figure 15a: the dashed lines on the top panel are not easy to distinguish from the continuous lines. The fonts could be a bit bigger. Figure 15b, same comment about the coordinates as fig.

R. *The resolution of the figure was improved to better distinguish the lines. Panel 15b was modified to exhibit bigger maps. Because the polynya surging is the primary focus of this panel, we maintained only the sea ice averages from the years that include this event.*

Figure 16 is nice.

R. *We decided that this figure does not provide a substantial contribution to the revised manuscript; therefore, this figure was excluded from the current version.*

3 Quick comments on grammar etc.

As the text needs to be rewritten quite a lot I will not point out the individual typos. I anyway have a few general comments.

Avoid repetitions, examples from the introduction: “up to 80 ????

Try sounding surer of what you write (e.g. p1040, too many “might”).

Be consistent:

- call the longitude=0_ meridian either the “Prime Meridian” or the “Greenwich Meridian”
- either say case studies or case studies, but not case studies. Use the same style throughout the text.

- p1038: either separate temperature and salinity for all layers, or put them both in the same paragraph for all the layers

- in the case studies and in table 2, if you say “Kerr et al. (2012), hereafter referred as K12”, then use K12.

Write shorter sentences. Long sentences with four different parts in the middle of a long and complex paragraph (e.g. p1044) are the best way to lose your reader. Try giving one, maximum two, facts per sentence. Also remove useless extra words (e.g. p1039, “regime”)

Avoid mentioning points which are not directly relevant to what you are saying (e.g. p1037, lines 18-19, p1043 lines 1-6) or that you already mentioned previously in the introduction or methods.

Mind the special characters. In the text and in your reference list, accents, umlauts and slashed o are regularly missing. Both LaTeX and Word can easily deal with them.

R. *We acknowledge the reviewer for highlighting these issues. The article was rewritten considering all of these issues.*

References

Karen J. Heywood, Sunke Schmidtko, Céline Heuzé, Jan Kaiser, Timothy D. Jickells, Bastien Y. Queste, David P. Stevens, Martin Wadley, Andrew F. Thompson, Sophie Fielding, Damien Guihen, Elizabeth Creed, JefK. Ridley and Walker Smith, Ocean processes at the Antarctic continental slope. Phil. Trans. R. Soc. A20130047.http://dx.doi.org/10.1098/rsta.2013.0047, 2014.