RESPONSE LETTER

Dear Editor,

Please find enclosed the response to reviewer #2 comments based on the OSD submission *doi:10.5194/osd-11-497-2014* entitled "ASSESSMENT OF THE STRUCTURE AND VARIABILITY OF WEDDELL SEA WATER MASSES IN DISTINCT OCEAN REANALYSIS PRODUCTS" by T. S. Dotto et al. The manuscript has been carefully revised in response to the reviewers' comments and suggestions; detailed responses to their comments are below. Our reply is in *italic* font, whereas the reviewers' specific comments are not italicized. We thank the reviewers for their suggestions, which have significantly improved this article.

Authors Responses (AR) to Reviewers:

Referee #2:

General comments:

In this article the authors assess the accuracy of five global ocean reanalysis products (ECMWF, CFSR, MyOcean, ECCO2 and SODA) in representing the water mass structure of the Weddell Sea over the past few decades. The focus on the Weddell Sea is warranted by its importance for production of Antarctic Bottom Water, which ventilates the deep ocean and may substantially impact climate over timescales of hundreds to thousands of years. The Weddell Sea is a particularly challenging region for ocean reanalyses due to its small dynamical length scales and the presence of dense overflows from the Antarctic continental shelf.

The evaluation consists of a direct comparison between observed and reanalysis temperature, salinity and neutral density along the WOCE SR4 and A12 sections. All of the reanalysis products examined capture the qualitative structure of the water masses in both sections. The largest discrepancies are typically found in the Antarctic Surface Water (AASW), where the models struggle to represent the interaction between the ocean, sea ice and the atmosphere. Large discrepancies are also found in the boundary current at the Antarctic shelf break, and in the deepest Weddell Sea Bottom Water (WSBW) layer, which, for example, is largely absent in the ECCO2 model. The reanalyses exhibit multi-decadal trends in the properties of AASW, Warm Deep Water (WDW), Weddell Sea Seep Water (WSDW) and WSBW, though the various products disagree on the signs of these trends, and the magnitudes of the trends are often much smaller than the discrepancy between the observations and the reanalyses.

The comparison between the reanalysis products and observations is thorough, aided by eleven multipanel figures comparing the mean water properties and their variability, and presenting statistical measures of the error. In general it seems that all of the reanalysis products require considerable development to improve their representation of the Weddell Sea water masses and their trends, particularly in their representation of dense water production and overflows. I am therefore not sure that I agree with the authors' conclusion that "ocean reanalysis products are a valuable option for studying the climatological states of the deep layers of the Weddell Sea" (L600–601), but this may be a matter of opinion. Either way, this article allows interested physical oceanographers and climate scientists to judge the relative strengths and weaknesses of the reanalysis products at a glance, and is therefore of practical use to the scientific community.

AR: We greatly appreciate the detailed review. The reviewer suggestions have been incorporated, thus clarifying certain critical points of discussion and improving the overall quality of the manuscript. Regarding the reviewer statement concerning the manuscript conclusion, we try to clarify this point by rephrasing the sentence. In fact, we believe that the ocean reanalysis products assessed in this manuscript may serve as a valuable tool for the study of states in the deep ocean, considering that their results produce hydrographic values close to the observations and capture the main regional features, as was shown in our manuscript. Moreover, the reanalysis products can fill the temporal gaps in the studies of the deep layers of the Weddell Sea. Nonetheless, studies that compare and evaluate these products are essential to avoid misleading interpretations due to spurious trends; therefore, caution is needed in their use.

L. 600. "Overall, our results suggest that the MyOcean and SODA products are valuable tools for studying the states of deep layers in the Weddell Sea because these tools closely reproduce the hydrographic absolute values of the observational data and capture the main regional features."

Comments/questions

• L129–189 and Table 1: Looking over the observational datasets, it appears that the WOCE sections used as the basis of the authors' evaluation is itself assimilated into all of the reanalysis products except CFSR. The WOCE dataset cannot therefore be regarded an independent test of the reanalysis products, and it is unsurprising that CFSR deviates from the WOCE observations much more than any of the other products. I don't think that this invalidates the authors' approach: it is still helpful to evaluate the agreement between the reanalyses and observations. In the absence of any independent observations, it is sensible to base such an evaluation on the WOCE data because they are representative of the Weddell Sea water mass structure and have been obtained consistently over a long period. However, the authors should include some discussion of these points prominently in the manuscript.

AR: We agree with the reviewer that the WOCE datasets are not a completely independent test. Following the reviewer's suggestion, we added the following statements:

L. 325. "However, the mean RMSE could be influenced by the CFSR results, given that CFSR does not assimilate the WOCE deep-ocean data."

In Section 5, we added a new second paragraph (L. 448). "Moreover, it is important to highlight that the WOCE dataset cannot be considered a fully independent test of the reanalysis products because this dataset is assimilated during the simulation phase by most of the models. In this sense, it is reasonable that CFSR deviates from the WOCE observations much more than any of the other products (Figs. 2–5). However, in the absence of any independent observations, the WOCE dataset remains the best choice for such evaluation because of its comprehensive nature, high resolution and the representativeness of the Weddell Sea water mass structure and spatial distribution."

• 178–181: I would like to see a more detailed discussion surrounding the authors' decision to exclude the later ECCO2 data. These years are presumably some of the most important, being the most recent, and if the purpose of the evaluation is to allow other scientists to judge the quality of these reanalysis, then why obfuscate anomalous behaviour in the models?

AR: The ECCO2 data are thoroughly discussed in Azaneu et al. (2014) "doi:10.5194/osd-11-1023-2014", and the authors conclude that the data of the latter period are not useful for hydrographic studies in the Weddell Sea. Azaneu et al. (2014) show that after the opening of the oceanic polynya, the thermohaline values of Weddell Sea water masses became unrealistic. The entire water column became cooler and denser because of the unrealistic ocean deep convection. These unrealistic values can be seen in the figures of the time series (Figs. 7-12). We added a sentence regarding those points raised by the reviewer.

L. 181 "These authors showed that after the opening of the oceanic polynya in 2004, the thermohaline values of Weddell Sea water masses became unrealistic, most likely because of strong, open-ocean deep convection simulated in the Weddell Sea."

• L260–262 and L269–271: I find it difficult to see the patterns of discrepancy described for the AASW. It is easier to see distinguish the patterns of discrepancy described later for the WDW, WSDW and WSBW with the aid of Figures 7–12 and Table 4. Perhaps the authors could include the mean error in θ, S and γn in AASW layer in the text here to reassure the reader.

AR: We agree with the reviewer that this paragraph is confusing. We intended to call attention to the different representation of the sections in terms of the overestimation/underestimation of values. For this purpose and for clarity, we have rewritten the entire paragraph. Please see the new paragraph below:

L. 255–274. "All the ocean reanalysis products evaluated had difficulty representing the AASW hydrographic values (Figs. 2–5). The mismatch between the data and the surface water representations was most likely a consequence of difficulties the products faced in reproducing several complex processes and fluxes acting on the ocean surface, which are seasonally influenced by physical processes at the air-sea and sea ice-ocean interfaces (Whitworth et al., 1998). The majority of the reanalyses showed the mean differences colder by 0.002–0.16°C than those actually observed in the data recorded in situ along the WOCE A12 line (Fig. 4a). Conversely, for the WOCE SR4 section, all the reanalyses revealed mean differences that were 0.06–0.4°C warmer than the observations (Fig. 5a). For S, most of the reanalyses overestimated this field by a mean difference of 0.01–0.11 in both sections (Figs. 4b and 5b), whereas for the γ^n field, the majority of the reanalyses showed a mean underestimation of 0.004–0.3 kg m^{-3} (Figs. 4c and 5c). For the S and γ^n fields, CFSR showed the greater mean differences (-0.3±0.17 for salinity and -0.3±0.16 kg m^{-3} for neutral density), whereas MyOcean detected minor differences (an overestimation of 0.003±0.09 for S and an underestimation of 0.004±0.08 kg m^{-3} for γ^n). The greatest mean differences were found primarily at the AASW/WDW interface and near the continental boundaries (Figs. 4a–c and 5a–c). Note that along the WOCE sections, depending on the product, the simulated values both underestimated and overestimated the observations (Figs. 4a–c and 5a–c)."

• L310–311: Dense water overflows are critical to the structure of the Weddell Sea, and to the global overturning circulation. How did the authors determine that the overflow of WSBW was absent in the reanalyses? Do none of these reanalysis products include some kind of dense overflow parametrization?

AR: Dense water overflow was considered absent in the sections because there was no clear descending plume of newly formed WSBW cascading down over the Weddell Sea western continental slope of WOCE SR4 during the period analyzed (as can be seen in Fahrbach et al. (2001)) when compared to observations. In addition, none of the reanalyses include any type of dense overflow parameterization that could inject dense water directly into the abyssal basin.

Moreover, regarding those questions, we have performed certain additional investigations of WSBW downslope flow in the eastern part of the Antarctic Peninsula and in the southern part of the Weddell Sea. All of the reanalysis products showed the absence of the WSBW plume in the continental slope of *those areas during the simulated period, as shown in the figure below for the MyOcean product applied to the eastern part of the Antarctic Peninsula, around 70°S. The dense water plume was only present in MyOcean product beyond 2009, however this plume was WSDW and not WSBW.*

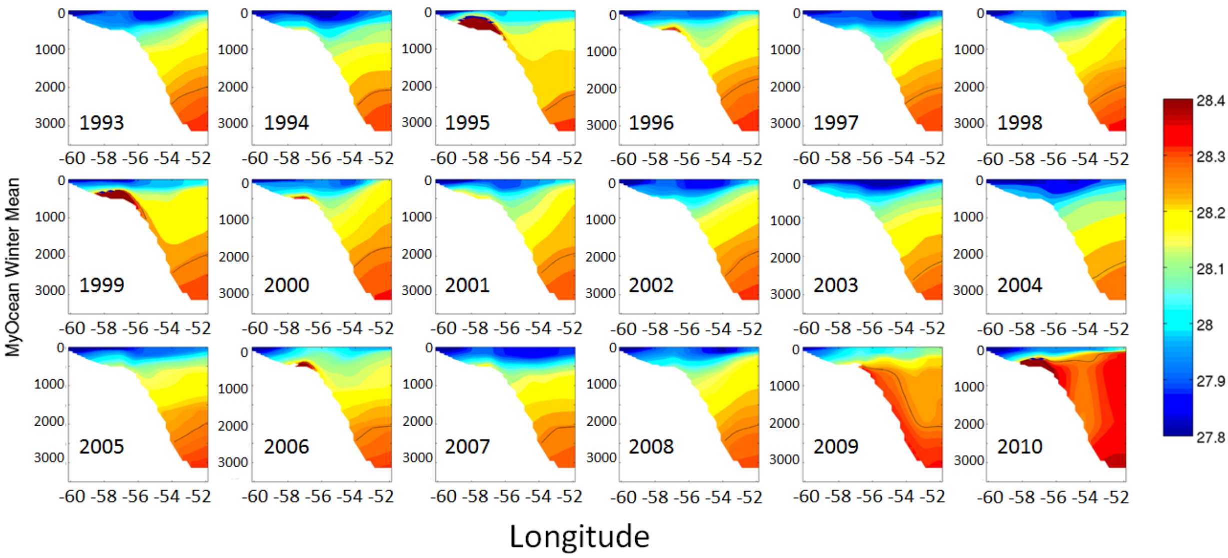

Fig. 1. MyOCean cross shelf neutral density (kg m^{-3}) sections around 70°S in the eastern part of the Antarctic Peninsula during winter months (June–August). The first panel (upper left) refers to 1993, and the last panel (lower right) refers to 2010. The Y axis is depth (m), and the X axis is longitude. The black lines refers to the 28.27 and 28.4 kg m^{-3} isolines.

ECCO2 also showed a plume of WSDW beyond 2001 along the slope of eastern part of the Antarctic Peninsula at 70°S. However, ECCO2 had a density increasing along all water column beyond that year (as can be seen in the figure below), which is associated with a temperature decreasing and a salinity increasing. Beyond 2009, a plume of WSBW is visible descending the continental slope, but in 2010, the area of this plume is overestimated.

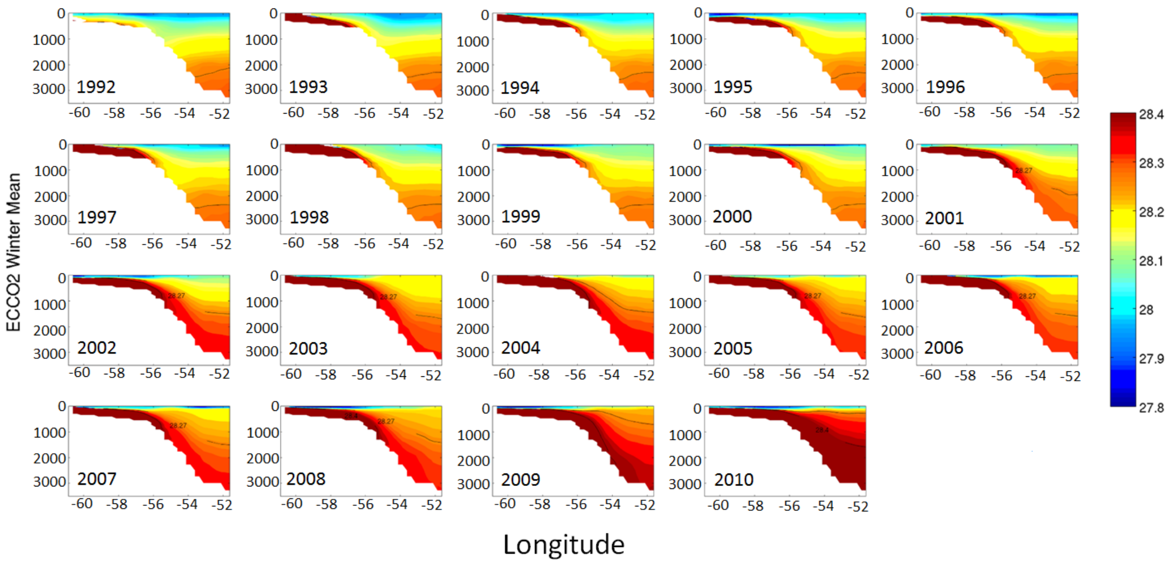

Fig. 2. ECCO2 cross shelf neutral density (kg m^{-3}) sections around 70°S in the eastern part of the Antarctic Peninsula for winter months (June – August). The first panel (upper left) refers to the year 1992 while the last (lower right) refers to 2010. Y axis is depth (m) and X axis is longitude. The black lines refers to the 28.27 and 28.4 kg m^{-3} isolines.

• L323–324: Reanalyses are judged to be accurate if their RMSE in a given property is smaller than the average, including CFSR. Given that CFSR is the only reanalysis product not to assimilate the WOCE sections, would it be fairer to judge the accuracy based on the mean RMSE excluding CFSR?

AR: We performed the proposed sensitivity test based on both criteria, with and without the RMSE of CFSR. When CFSR was included, we considered all the products as an intercomparison analysis. Otherwise, when CFSR was excluded, we only considered the products that assimilate the WOCE data. As a result of this last approach, we saw the bias generated by the CFSR in the mean RMSE. Thus, the accuracy of the reanalyses based on the mean RMSE changed, being only as accurate as MyOcean in representing all the hydrographic properties in both sections. Thus, using RMSE without CFSR is a fairer method of judging the accuracy of the reanalyses because using this approach, the four reanalyses assimilate the WOCE data. Please, see below the sentence included on L. 333:

"It is fairer to judge the accuracy of the reanalysis outputs without CFSR RMSE, because using this approach the four reanalysis compared assimilates WOCE data."

• L326–328: Could the authors elaborate on how the stronger currents in WOCE A12 might enhance turbulent processes and salt diffusivity? Presumably the resolution of the reanalysis products is too low to see any mesoscale turbulence, so I infer that we are talking about small-scale turbulence. Do the authors mean that large vertical shear may be generated, activating a vertical mixing parametrization that depends on the gradient Richardson number? If so, is the vertical shear really large enough at these depths for this to be a plausible explanation?

AR: We rephrased the sentence and added references to clarify the idea. We believe that the effects of dynamic processes associated with the presence of Maud Rise could affect the ocean dynamics and the water mass mixture near section A12 more than in section SR4. We just emphasized the fact that the region around the section A12 is more sensitive to changes in the water column than section SR4, because it suffer more intense hydrodynamics processes (e.g. WOCE A12 region is affected by the inflow of water in the intermediate layer, eddies processes and is a region where the double cell structure of the gyre is splitted). Please see the following rephrased sentence:

L. 329. "This difference could be associated with more intense hydrodynamics processes occurring closest to the WOCE A12 section (e.g., Klatt et al., 2005) than in WOCE SR4, implying that region is more sensitive to changes in the water column structure. The dynamic impacts on the three dimensional oceanic flow field all the way up through the water column is accentuated by the presence of Maud Rise seamount (Holland et al. 2001)."

• L394–397: The authors mention that formation of a large open-ocean polynya in ECCO2 leads to anomalous behaviour beyond 2004. Is this also the cause of the anomalous behaviour in ECMWF and MyOcean?

AR: We do not believe so. We investigated the sea ice cover in the MyOcean product and the sea surface temperatures in the ECMWF because the latter product does not include a sea ice model in the reanalysis but uses sea ice concentration data as a criterion for the parameterization of sea surface temperature. We concluded that polynyas are not the cause of the anomalous periods in either product. Moreover, without further analyses, which are beyond the scope of this study, we cannot argue about the causes of the anomalous period in either product.

• L37–38 and L473–475: I'm not convinced that this statement is justified. In no case is the horizontal resolution "merely" increased: simulations with different resolutions are compared between entirely different reanalysis products, and judged based on hydrographic sections from a very small part of the ocean. If horizontal resolution alone (as implied by "merely") were increased, then in fact I think there almost certainly would be an improvement in the quality of all of these reanalyses.

AR: We completely agree and rephrased the statement to correct this ambiguity and avoid any misunderstanding. In addition, we deleted this sentence from the abstract and rewrote it. Please see the following rephrased sentence:

L. 37. "Improvements in parameterization may have as much impact on the reanalyses assessed as improvements in horizontal resolution primarily because the Southern Ocean lacks in situ data, and the data that are currently available are summer-biased."

L. 473–475. "Although increased horizontal resolution is important to better simulate hydrographic representations, we highlight that improvements in parameterization, such as advection schemes and sub-grid-scale mixing processes, may have as much impact on the reanalyses as modest increases in horizontal resolution (e.g., Renner et al., 2009)."

• L479–480: I find it questionable to say that parametrizations are as fundamental as horizontal resolution. In principle, in the limit of arbitrarily fine horizontal (and vertical) resolution, the ocean fluid dynamics would reduce to the Navier-Stokes equations, and no parametrizations would be required. So grid resolution is certainly more fundamental. I think that perhaps what the authors mean is that improvements in parametrizations may have as much impact on the reanalyses as modest increases in horizontal resolution.

AR: We completely agree and added a new sentence in the manuscript. Please, see the response above.

Minor comments/typos

The standard of the English prose in this manuscript is generally very good, but here and there I found sentences to read a little awkwardly. I won't provide an exhaustive list, but I suggest that the authors give the manuscript a careful proof-read.

AR: The m/s was resubmitted for a new round of native English Speaker review.

• L39–41: I understood what was meant by this sentence, but I think it should be rephrased for clarity, particularly in the abstract.

AR: To avoid misunderstanding, we rewrote the sentence. Please see the revised sentence below:

L. 37–41. "Improvements in parameterization may have as much impact on the reanalyses assessed as improvements in horizontal resolution primarily because the Southern Ocean lacks in situ data, and the data that are currently available are summer-biased."

• L106–108: I think it would be appropriate to add citations in support of this statement.

AR: Done. Please see the additions below:

"However, ocean reanalysis systems can produce spurious trends and inhomogeneity (Carton and Santorelli, 2008) caused by the limited and summer-biased sampling, especially at high southern latitudes (Bromwich et al., 2011)."

• L204–205: I didn't understand this sentence. Please rephrase.

AR: Done. Please see the rephrased sentence below:

"Reanalysis grid points closer to the geographical location of the in situ hydrographic stations (observations) were selected from the monthly mean fields corresponding in time to the period of the in situ measurements."

• L209–211: I suggest a minor rephrasing to emphasize that these numbers refer to the grid spacings for the interpolation.

AR: Done. Please see the following rephrased sentence:

"Horizontally, the reanalysis and the observational datasets were spatially interpolated and gridded with 0.5° latitude and 1° longitude for the WOCE A12 and WOCE SR4 sections, respectively."

• L284–285: I would understand "compensate" to mean that the changes in density due to freshening and cooling exactly cancel one another, whereas I infer the authors mean that the fresh bias and cold bias have opposing effects on the density.

AR: As this passage was unclear to all the reviewers and does not add relevant information, we deleted it from the text.