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Interactive comment on "Objective mapping of Argo data in the Weddell Gyre: a gridded dataset of upper ocean water properties" *by* K. A. Reeve et al.

Anonymous Referee #3

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Overall impression: The idea of the manuscript is a good one - generating mapped fields of the hydrography in the Weddell Gyre.

The organization of the manuscript leaves room for improvement. Methods are interspersed between discussions of oceanographic features which leads to repetitions and a relatively poor flow of the story. In some ways, the manuscript reads more like an initial draft.

Conservative temperature and absolute salinity have advantages, as stated on page 13, but they also have a disadvantage: comparisons with earlier studies become less straightforward. A brief discussion on how large this impact is within the study region

would be good.

The main weakness is that the errors of the mapped fields are too small in some regions without data or very few data. One such area is then interpreted as revealing signs of temporal variability that is not convincing. All mapped fields should be presented in a way that a reader can know where the profiles going into them are located.

Mainly because of this last concern, I think this manuscript needs major revisions.

Details

Introduction section

page 3: "as well as in AABW within the Atlantic Ocean (Purkey and Johnson, 2013; Couldrey et al., 2013; Azaneu et al., 2013)." I believe there also was a paper on temperature changes in the bottom water in the Hunter Channel in the 1990's.

page 4: "throughout the Weddell Gyre, Argo floats have been deployed in the region since 2000." Argo deployments in that region did not start in 2000. The first high-latitude floats in the south are from 12/2001 (which agrees with Figure 2, and the last paragraph on page 4).

"and may subsequently temporarily abort mission to surface" I'm not sure "temporarily abort mission" is the right wording "abort attempt" seems to better describe what the floats are doing.

page 5: I'm not sure why the introduction describes objective mapping intensively. If such a description is needed, it could be in 'Methods' or in an appendix.

page 6: "excluding regions beyond the Weddell Gyre boundaries" - please define these here (not in 'methods').

Methods section

page 6: how many profiles are left after removal of duplicates?

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"While there is a clear seasonal bias in the number of profiles in the first half of the time series, this bias reduces after 2007. This is due to the introduction of an ice-sensing algorithm that allows floats to abort the present mission to surface if the presence of sea-ice is predicted at the surface (Klatt et al., 2007; Fig. 3)."

The second one of these sentences is repetitive (a similar sentence is in the introduction) -> it would be sufficient to say "due to improved float technology as mentioned above". Reference to figure 3 would be better off at the end of the first sentence.

"are actually located north of the gyre boundary" - this sounds odd, because the introduction states "excluding regions beyond the Weddell Gyre boundaries". So, if these profiles are out of region, then why are they still part of the data set. An alternative is to change the introduction.

page 6-7: "Additionally, any data points where the corresponding adjusted pressure error exceeds 20 dbar are rejected." Seems to me that these will not have a flag of 1.

page 7: "The temperatures in Argo are reported to be accurate to $\pm$0.002 C while pressures are accurate to $\pm$2.4 dbar (Owens and Wong, 2009). For salinity, if there is a small sensor drift, uncorrected salinities are accurate to $\pm$0.1 psu, although this value can increase with increasing sensor drift." Why is the accuracy of uncorrected data of interest if only corrected salinities are used? What would be interesting is to be informed how good the corrected salinity is.

"of seawater in comparison to potential temperature" -> "of seawater than potential temperature"

"The profile data are linearly interpolated onto 41 dbar levels, ranging from 50 to 2000 dbar. The pressure levels used are shown in Table 1." -> "The profile data are linearly interpolated onto 41 pressure levels, ranging from 50 to 2000 dbar (Table 1)." Also: - why choose those levels and why exclude pressures shallower than 50 dbar? - what was the reason for using pressure instead of density levels?

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page 8: the description of the Weddell front can be improved. In fact the whole paragraph is not very clear.

"here we seek the warmest temperature at the deepest depth, in order to ensure the sub-surface temperature maximum is selected rather than, for example, the summer surface water." Seems like this can be achieved by looking for the maximum below about 150 dbar. If that is not an option in some regions, then the maximum below the minimum in the upper 300 dbar could be what the algorithm has to look for.

section on Approach to objective mapping

page 8-9: Not sure why this paragraph is needed, as the data set does not allow applying any of the approaches described in it, as the subsequent paragraph explains.

The second paragraph basically describes a method assuming the meridional gradient dominates the field at each depth.

pages 10-15: The description of the mapping technique is detailed, but it is not clear how much improvement this technique is over simpler mapping techniques.

What makes me wonder: - why was the number of points (N) limited to 40? - why are the length scales for the first and second stages of the interpolation so large?

Results section

page 16: "A double gyre structure is also suggested, where the secondary gyre occurs in the north-east sector, splitting from the main gyre at about 5W." Could this be caused by uneven sampling, i.e., reflect temporal variability in the temperature rather than a double-gyre structure?

page 17: "There is also a considerable deepening of the sub-surface temperature maximum at about 65S, just east of the Prime Meridian, from about 200 m in the surrounding region to roughly 400 m, which occurs directly over Maud Rise (note the mapping error is relatively small in this region)." What might be the case?

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Discussion of Fig. 12: it would be very interesting to see error bars as well as dots from the actual profiles within a given distance from the Prime Meridian in the left panel.

It's not my favourite thing to use symbols in subtitles (e.g. for 3.3). Also, why start with 'An example:' in 3.3 and 3.2? Seems like both are examples.

page 17-18: "for the entire time period from 2002 to 2013." Somehow the 'entire time period' keeps changing. Are data from 12/2001 excluded? On page 16, it was 2002 to March 2013. I would guess providing what is meant by entire time period once is sufficient. Same thing is valid for the 'sub-periods'. That way inconsistencies (and 'confusion') can be avoided.

Basically, the same critique as for the previous plots (Fig. 11_ applies. In the southwest the data are seriously extrapolated and the mapping error remains mostly extremely small. This does not make sense to me.

page 18: "The warmest signal that extends furthest into the gyre (about 1 C) occurs in 2006-2009" This signal is in an area (near 62S, 23E, Fig. 15b) where there are basically no profiles, so this difference must be an artifact of the method rather than a real signal. In fact it is stated in the next sentence that the error is relatively large in this area (Fig. 15e). So, the question is: why can that signal not be shaded as insignificant? Seems to me the method has to be tweaked to allow proper identification of areas with insufficient data coverage. And rather than use the shading technique, I suggest to completely mask them by showing them as white areas. Also, scatter plots of where the profiles are in each time period would be very helpful. These can be overlaid on the maps showing the error estimates.

Discussion section

Section 4.1 is partially a repetition of the Results section (Up to line 24) and it's title seems more appropriate for the Results section.

If my concerns from above are used to revise the maps, then the discussion will be

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easier, because method-caused artifacts can not be confused with actual differences between the three time periods.

After this the discussion goes back to error analysis and potential ways to improve the techniques. While this discussion is important, I think this needs to happen before looking at oceanographic features. In some way, the results section needs to become a 'discussion of methodology' or 'performance of objective mapping' section (maybe as part of the methods section).

The 'Results' and 'Discussion' section could then be merged and focus on what one might learn about the Weddell Gyre from the generated products.

Page 20-21: here it is discussed that some areas with good data coverage have large errors. What I'm missing is (again) a discussion of small errors in areas with no data coverage.

Section 4.2 and 4.3 are also more methodological - see my comments about restructuring the paper.

Section 5 is nice and short, which makes one wonder why the previous sections are very long and frequently repetitive.

Figures:

1: nice schematic, but some fonts are too small

2-4, 10, 11, 13-20: font sizes too small

10 (similar for 11, 13-16): The criteria used for masking in 10b seems odd. Isn't there a way to use an absolute number as criteria? That absolute number can be found when overlaying the contours of the error with the profile positions. A prime candidate region for masking is the white area in 10a around 72S, 45W that is much larger than the masked area in 10b to the west of that region. What is also odd is the very small mapping error around 65S, 22E (almost no data with an error of about 0.001).

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