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Interactive Comment

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.

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Firstly, we would like to thank all three reviewers for their detailed feedback in regard to our manuscript. With the help of their comments we hope to have improved the manuscript. In particular, as suggested by the reviewers, we changed the structure of the paper and incorporated a section on comparison with climatologies, as well as with the well-known section along the Prime Meridian. Also, the mapping errors presented are the square root of the variance, whereas before the error variances were presented (hence why the errors looked so small). We would like to point out that the aim of the paper is to provide a data product, and as a data paper we have tried to strictly abide by the scope of the journal in providing a description of the data product. As such, while scientific interpretation of the Weddell Gyre is an important issue that will be addressed in future research, it is beyond the scope of this paper. The authors have tried to stick with the requirements of the journal by carefully avoiding analysing the data (http://www.earth-system-science-data.net/about/manuscript_types.html). In order to make it clearer that this paper is a data paper, we have modified the title slightly as follows:

"A gridded dataset of upper ocean hydrographic properties in the Weddell Gyre obtained by objective mapping of Argo float measurements"

The new structure of the manuscript is as follows:

1. Introduction

2. Source description: Argo float profiles

3. Methods

3.1. Sub-surface temperature maximum

3.2. Objective Mapping

3.2.1. Approach

3.2.2. Objective Mapping

3.2.3. Choosing appropriate length scales and selecting N surrounding data points to a grid point

4. Objective Mapping performance

4.1. Error sources

4.2. Mapping sub-surface temperature maximum: two approaches compared

4.3. Objective Mapping to float profile locations

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5. Results

5.1. Sub-surface temperature maximum

5.2. Conservative Temperature and Absolute Salinity at 800 dbar

6. Discussion

6.1. Approach to objective mapping: comparison to climatologies

6.2. Water properties of the Weddell Gyre along the Prime Meridian

7. Concluding Remarks

8. Appendix: Data format

The following details our responses and corresponding changes to the manuscript to each reviewer in turn. The reviewers comments are first quoted, which is then followed by the authors response. Please note that as a result of the major changes carried out based on the feedback from reviewers, many of the original figures have changed. The attached supplement provides the updated manuscript with changes highlighted, and a list of the updated figures.

Review #1

General Comments

R1: The manuscript "Objective Mapping of Argo data in the Weddell Gyre: a gridded dataset of upper ocean water properties" by Reeve et al. describes an algorithm to objectively interpolate Argo float data on a grid and its application to data collected in the Weddell Gyre. This is a very technical manuscript trying to resolve the challenges when dealing with the data sparse region of the Weddell Gyre. It is overall very well and clearly written, so that the readers can evaluate the work and can reproduce the algorithm and apply it to their own data. The manuscript is therefore highly suited for this journal. I recommend acceptance after minor changes and addressing my

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concerns.

RESPONSE: The authors would like to thank reviewer one for the positive feedback. We fully agree with the recommendations suggested and have carried out the suggested changes. We would like to emphasise that the structure of the paper has changed significantly, and so the feedback to the following sections may now be referring to sections in a different location of the paper.

Specific Comments

INTRODUCTION R1: You give a good introduction to the Weddell Gyre and the interpolation schemes available. You also highlight the lack of in situ data. However, there are other data sources available in this region: ships, moorings, drifters, animal-borne instruments. You should expand the paragraph on ships and moorings to include all other emerging technologies and make clear why you limit yourself to only use Argo float data.

RESPONSE: We appreciate the recommendation of the reviewer, and have expanded the overview of other existing data. Please note some of the paragraphs have now been moved to various sections (e.g. the Argo data description to its own section (section 2) and the interpolation schemes available to methods section 3.2.1). The following paragraphs have been added in the introduction (p2, lines 27-33 and p3, lines 7-15):

“These data are well-established (there are now 30 years of data collected from RV Polarstern alone). Historical measurements however date back to the early 1900’s; for example, Brennecke (1918) combined the observations from Swedish (1901-03), Scottish (1902-04), French (1908-10) and German (1911-12) expeditions and provided sufficient evidence for proposing the cyclonic circulation of the Weddell Gyre. A review of historical research on the Weddell Gyre is provided by Deacon (1979).”

“In addition to repeat hydrographic sections and moorings throughout the Weddell

Gyre, there are also data from Argo floats, drifters and animal-borne sensors. These all combine to provide a multi-platform approach to observing the Weddell Gyre. Here, the authors focus on the Argo float dataset, in order to provide an independent dataset that can be compared to ship-based observations in the near-future. Moorings are excluded from the analysis as they provide vertically sparse data, while drifters only provide surface data, which is excluded from the analysis due to high surface variability, and animal-borne sensors require special treatment due to salinity and depth sensor issues. Thus, delayed-mode adjusted Argo float data are the sole focus of this study."

METHODS 2.1

R1: While you discuss the linear location interpolation of Argo float profiles later in the discussion, you should highlight this in the method section as this has a big effect on the structures in your mapped fields. Some information is necessary, e.g. how long are surface locations apart. How many of the locations do have an 'interpolated' location. Was the increased uncertainty taken into account as an increase error later in the interpolation scheme?

RESPONSE: The following paragraph has been added to the newly added section 2: "Data description: Argo float profiles", which includes a figure (4) showing the interpolated float profile station positions (p5 lines 10-26):

"Complications regarding position errors arise when the float enters the sea-ice zone. As already mentioned, new technology has allowed the floats to avoid surfacing in these regions. However, when a float profiles the water column under sea-ice, it is not possible for the satellite to determine the float position. Thus, the position of an under-ice float profile is determined by linear interpolation between the last known profile position and the position of the first profile upon exiting the sea-ice zone, using the knowledge that floats perform on a 10-day cycle. Such profiles can be seen in particular in Fig. 4, where profiles with an interpolated location are marked in red. This

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situation will be improved as soon as RAFOS data (Klatt et al., 2007) collected by some of these floats have been analysed. About 13% of all profiles south of 50° S have an interpolated location (about 2600 profiles), although this increases to about 38% south of 60° S (about 2340 profiles). The mean distance between under-ice profiles and their nearest neighbor is about 27 km, although the largest distance is about 265 km. The mean number of days between under-ice profiles and their nearest neighbor is about 24 days while the range of days is between 7 and 270. The influence of this uncertainty on the objective mapping will be discussed in section 4 when assessing the robustness of the results."

R1: 2.3 You say that "the sub-surface Weddell Gyre is relatively invariant". Could this only be a result of the lack of in situ data not resolving the temporal changes?

RESPONSE: This is in comparison to regions such as the North Atlantic, where wind-induced mixing leads to a highly dynamic sub-surface layer. We don't see variability to such an extent in the Weddell Gyre, in part due to the "sheltering" role of sea-ice; indeed float data demonstrates this invariance: in figure 13 (showing the area-weighted mean mapping error, discussed in section 4.1), the mean mapping error decreases sharply below 120 m, and a sub-surface error peak coincides with the bottom boundary of Winter Water. A large part of section 2.3 (now 3.2.1) has been removed, and the two paragraphs in the original section 1 about mapping methods ("The objective mapping method…") have been merged with the original section 2.3. As a result, the description of the sub-surface Weddell Gyre as "relatively invariant" has been removed from the manuscript.

R1: 2.5 You base your length scales on the spatial structure of the dataset and not on the spatial structure of the ocean. You force your scale so high that you have more than 95% of your grid cells covered. I do not think that this is a good idea. The result is that you decide how many grid cells will not have data instead of the interpolation scheme together with the dataset 'telling' you that not enough data are available for this grid point. Please make a point why you choose to use only numbers to define

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your scale instead of using oceanographic knowledge. Your cut-off for profiles is only based on your large length scale. Which means that e.g. if you have 40 profiles 900km due east of your grid point the algorithm would still calculate an estimate. For example, you provide estimate west of 35W and south of 70S. Your interpolation scheme has added the weighting based on the distance between profiles, but when there is a lack of data and/or they are bunched together it treats them as being Gaussian distributed in space (as it does not know direction). I would therefor advise to calculate straight lines between the outermost profile locations as boundaries and remove all estimates outside of this polygon. In other words, each grid point should at least sit on a line between two profile locations.

RESPONSE: Firstly, we apologise for some unfortunate typos (F instead of ÏŢ) in the original text which might have led to misunderstandings. The length scales are defined in section 2.4 (now 3.2.2), which originally stated:

"The distances D and F are scaled by a horizontal length scale L (L(stage 1) = 1000 km and L(stage 2) = 500 km) and a cross-isobath scale ÏŢ (F (stage 1) = 0.5 and F (stage 2) = 0.25) respectively."

This is incorrect; F is a calculated product based on potential vorticity and is scaled by ÏŢ, and so the text has been corrected to state (p10, lines 12-13):

"The distances D and F are scaled by a horizontal length scale L (L(stage 1) = 1000 km and L(stage 2) = 500 km) and a cross-isobath scale ÏŢ (ÏŢ (stage 1) = 0.5 and ÏŢ (stage 2) = 0.25) respectively."

Thus, while the length scales are fixed, the condition for whether a data point is used in the mapping to a grid point is dependent on the decay scale, not the length scales alone. This is stated in section 2.5 (now section 3.2.3; p12, lines 4-6):

"all corresponding data points where the decay scale was larger than 1 were filtered out (i.e. only data within the e-folding scale of the covariance function were selected;

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Dig2/L2 + Fig2/ ÏŢ2 < 1).”

Therefore, the criterion for whether a data point is used is based on a combination of horizontal distance to the grid point and the difference in cross-isobathic separation between the data point and grid point positions, thus taking oceanographic features into account. This is because a parcel of water is more likely to flow along lines of constant potential vorticity. By incorporating F into the criteria, we take this factor into account. To illustrate this, the following paragraph has been added to section 3.2.3 (p12, lines 9-20; originally section 2.5), which provides figures that show the influence of f/H in the decay scale:

“Figure 9 demonstrates the influence of incorporating a cross-isobathic separation factor into the decay scale; the contours show the field of influence about a grid point at 71° S, 15° W (i.e. the e-folding scale); this field is circular when the decay scale is based purely on the horizontal separation between grid point and profile locations (Fig. 9a) and elongates along lines of constant potential vorticity when the cross-isobathic separation factor is incorporated (Fig. 9b). Fig. 10 shows the influence of the cross-isobathic separation factor for a grid point close to the 2000 m contour line. Therefore it is possible to use first principals of physical oceanography (in the fact that a water parcel is more likely to travel along lines of constant potential vorticity) to sensibly extrapolate to regions of sparse data coverage; so long as there is little variation in bottom bathymetry (the resulting mapping error will be large in areas of complex bathymetry and thus a dense contouring of potential vorticity).”

The contours follow very clearly the bottom bathymetry and covers larger areas where the bottom bathymetry is very flat (such as, incidentally, the south west corner of the Weddell Gyre). This is because, as the bottom bathymetry flattens, the cross-isobathic separation factor (F) becomes very small and the radius of influence (i.e. the decay scale) becomes more circular; which reflects the reduced variability of hydrographic properties. This also justifies the reason why using floats with an interpolated position does not greatly affect the mapping results; the floats are assumed to flow along lines of

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constant f/H contours, and the linearly interpolated profile position estimates generally do not cross areas of rapidly changing f/H, primarily due to the fact that the changes in f and H are so small (e.g. in Fig. 4). Thus the linearly interpolated positions do not differ too greatly in context of producing an over-smoothed, large scale estimate of the mean field. This has now been discussed in detail in section 4.1: “Error Sources” (p12-14).

Another way we reduce errors when using such large length scales is in our definition of the northern boundary of the gyre when mapping to pressure surfaces; we define the northern boundary as the position of the Weddell Front, which is determined from our map of the sub-surface temperature maximum; all other profile data are excluded from the mapping, thus ensuring that we do not draw data from outside the Weddell Gyre. This has been made clearer in section 3.1 (p6 line 32 – p7 line 2) with the following sentence:

“This boundary definition removes profiles from outside of the Weddell Gyre when mapping to fixed pressure levels, ensuring that profiles strictly within the gyre itself are selected for each grid point, which provides some security when using large length scales to determine the large scale mean field of a gyre.”

The suggestion was to avoid extrapolating by ensuring one can draw a polygon of profiles encompassing the grid point. The incorporation of the cross-isobathic separation factor enables one to extrapolate within reason by taking into account oceanographic knowledge, as we know in which direction a water body is likely to travel. This is effective within the Weddell Gyre due to its relatively invariant nature; in the open oceans of the north-Atlantic the length scales would indeed need to be reduced considerably.

R1: 2.6 I am not sure why you apply this step. Basically, you remove outliers. Shouldn’t they be included in your interpolation? Their main impact would be to increase the error value of you mapped field and therefore represent the potential spatial and temporal variability in your field, which is the main reason for your second step in the interpola-

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tion.

RESPONSE: The mask is provided in addition to the mapped fields in order to provide a clear indication of which grid points have an individual error larger than the errors of 95% of the grid points. It does not in any way interfere with the interpolation. The mask is used to highlight areas of large error on the maps provided in the results section. However, this section has been removed as it has been deemed unnecessary to provide these fields.

RESULTS

R1: 3.1 Some parts e.g. about the grid size are not results, but based on your method and are already mentioned there. Please remove.

RESPONSE: A revised version of the paragraph has been placed in its own section under Appendix 1: Data Format (p38):

"The time composite data sets of mapped field variables are provided as netCDF files; one file for each available time period. The filenames and corresponding variables provided in each netCDF file are listed in Tables 3 and 4 respectively. Mapped fields of conservative temperature (°C), absolute salinity (g kg-1), potential temperature, practical salinity and potential density (kg m-3) as well as corresponding mapping errors are provided for 41 vertical pressure levels (listed in Table 1). Additionally, conservative temperature, absolute salinity and pressure (dbar) at the level of the subsurface temperature maximum are provided. The coordinates represent the centre of each grid cell. The missing value is defined by NaN. Further details found in the global attributes of the netCDF files are described throughout Sect. 3"

R1: 3.3 This is the only place salinity is mentioned. Please properly present the results of the salinity fields as well including the structure and errors.

RESPONSE: While the results section is largely unchanged, salinity is included in the description of the maps presented in section 5.2 (p20 lines 5-19); note section 3 is now

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section 5), as well as in the discussion section 6.2 (p24-26).

DISCUSSION

R1: 4.1 Please but the results into context. Do the temperature and salinity pairs at each grid point correspond to the known ranges of water masses. This check is important as you interpolate on pressure levels and not along isotherms or isopycnals.

RESPONSE: The discussion section (now section 6; p20-26) has been re-written to present the results in the context of climatologies in 6.1 and observations in the literature in section 6.2. This section includes new figures, where a section of the prime meridian is presented, along with its T/S diagram, in order to show how the gridded fields represent, for example, the Warm Deep Water. The original discussion section 4 has been moved to a new section 4: “Objective Mapping performance”. In particular, the new section 4.1 has been largely reduced in length.

R1: 4.2 A very important and interesting step. As we assume the profile data to be the truth, they should be with the uncertainties of the mapped field as they represent not only the error of the mapping scheme but also the spatial and temporal uncertainties. Latter will not be resolved properly, when using sparse datasets. So, while your figures show only small errors based solely on the interpolation scheme, this test shows that the error in temperature of your gridded field is on the order of 0.2C. Please make this clearer. Did you include this uncertainty in the final error estimates of your gridded fields?

RESPONSE: Note this section and section 4.3 have been moved from the discussion (now section 6) to section 4: “Objective mapping performance” under sub-sections 4.2 (p15, ln17) and 4.3 (p17, lin14) respectively. This is a very good point that has been incorporated into the text. It has not been incorporated into the final error estimates, because it is an estimate of error that is open to interpretation, whereas the error estimates provided are a statistical calculation. This has been included in the text as follows:

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In section 4.2 (p16, ln15-30): "Thus, while the mapping error is largely based on both horizontal length scales and planetary potential vorticity separation, the differences between the methods described above (Fig. 15) could be interpreted as an error estimate of the variability in the vertical range of the Tmax that can be attributed to small-scale processes (e.g. due to internal waves, tides, temporal uncertainties) that are ultimately smoothed out in the mapping process. If this interpretation were to hold true, the mapping error due to large scale smoothing is 0.15 °C or less for the majority of the Weddell Gyre, and increases to as much as 0.4 °C in regions of high variability such as north of the western periphery, or in regions of no data such as in the far west of the gyre; for temperature at the level of the Tmax. We can assume that this estimate would increase for pressure levels above the Tmax and significantly decrease below the Tmax based on Fig. 13. As these error estimates are open to interpretation, they are not included in the final mapped error estimates. Furthermore, these errors also apply to the field variables mapped onto standard pressure levels. However, the errors would be reduced as the input data are interpolated onto standardized pressure levels prior to interpolation which removes small scale instabilities."

Section 4.3 (p18, ln8-14): "These values could be interpreted as an estimation of small-scale noise smoothed out through the mapping process, which is generally less than 0.2 °C and indeed within 0.15 °C for over 80% of the data points; which is also the general temperature difference between the two methods of mapping temperature at the Tmax (section 4.2). Again, as these values are interpreted values, they are excluded from the mapping error, but are discussed here to emphasise caution regarding possible interpolation errors."

R1: REMARKS 5. Could you also please give a recommendation on how to improve the dataset? Would it improve by adding other data sources and data from the shelves?

RESPONSE: The answer depends on the aim of the dataset. By including all available data from numerous sources (e.g. ship bottle and CTD data, moorings, buoys, and so on), one would improve the dataset (as long as suitable correction procedures are followed), but would also automatically extend the time period of the resulting datasets. There are of course numerous datasets (e.g. ship CTD, mooring, animal-borne sensors) within the time period of the datasets provided, which would without doubt improve the gridded fields provided. However, the aim is to provide a dataset independent from ship-based CTD, for the sake of comparisons, as well as to demonstrate the potential Argo float data has in representing the large-scale variability of the Weddell Gyre, despite the extreme (sea-ice) conditions. As more float data (or indeed any data) become available, the gridded fields that could be produced will improve. However, the situation will always remain the same within observational oceanography: we are limited by the spatial and temporal coverage of the data available to us. The following paragraph has been added to the final section of the discussion (6.2; p26, ln16-30):

"The WG datasets perform generally well in the context of current literature on the Weddell Gyre water mass properties. The main way in which the new datasets provided could be further improved is the inclusion of additional data. As more data become available, it may be possible to reduce the length scales used, and to incorporate a temporal separation component into the second-pass decay scale, both of which would lead to resolving the gridded fields to smaller scales (both spatially and temporally). Another key improvement (and indeed priority in the field of Argo floats) of any future gridded datasets is the improvement of the position estimates of under-ice floats (see section 2). However, the aim is to provide an Argo-only dataset, of relatively small time spans, looking at the large scale mean field. In general, the resulting gridded hydrographic fields show good agreement with other climatologies and current knowledge of the water masses drawn from ship data. The outstanding question that remains is whether the differences noted most particularly between climatologies and the dataset presented in this paper are the result of methodology, or suggestive of changes in the Weddell Sea climatic system."

Technical Corrections

R1: p.510 l.24: Is "buffer" the correct term here? The role of a buffer is not to transfer

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properties, but to buffer them. So maybe "pump" is a better term or make it clearer why it acts as a buffer.

RESPONSE: This was in particular reference to the paper by Fahrbach et al (2011). However, to avoid confusion the sentence has been rewritten as follows (p2, ln6-8):

“As such, the Weddell Gyre potentially plays a key role in a changing climate through its role in regulating storage of heat in the deep ocean (Fahrbach et al., 2011).”

R1: p.512 l.23: "There are symbols distributed along straight lines; these represent the linearly interpolated ..." This hoes into the caption of the figure and should not be in the main text.

RESPONSE: This has now been removed from the main text, and a different map is provided which explicitly highlights the profiles that have an interpolated position in order to address your point in section 2.1. Thus the text now reads (p5, ln16-17):

“Such profiles can be seen in particular in Fig 4, where profiles with an interpolated location are marked in red.”

R1: p.512 l.27: You not only provide a gridded field. You also introduce a new interpolation scheme. So maybe change the sentence to "The aim of this paper is to introduce an improved interpolation scheme and provide a spatially gridded dataset ..."

RESPONSE: The authors would like to thank the reviewer for the positive suggestion. However, we have not made the change as the interpolation scheme has been introduced in Boehme and Send (2005) and adapted to suit the purposes of the Weddell Gyre region. The aim of the paper is to provide an Argo-based gridded dataset of the hydrography of the upper water column in the Weddell Gyre, and this paper describes how we achieved this. âĂČ

Review #2

General comments

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R2: The submitted manuscript resembles rather a draft version of a technical than a scientific paper. A considerable part of the manuscript text deals with issues not directly related to the focus of the manuscript – the creation of a new Argo-buoys based gridded climatology for the Weddell Gyre. On the other hand, issues important for the assessment of this new gridded product are not discussed at all: 1) the comparison with several existing gridded climatologies, 2) justification of the new product advantages, 3) description of Weddell Gyre features unknown before this new gridded product has appeared (to name only few). The text is full of repetitions, there are many lengthy descriptions, several figures can be easily omitted, the reference list is too long including many references which have no or only an indirect relation to the main issues presented in the manuscript. Therefore I suggest to reject the manuscript in its current form.

RESPONSE: This general part of this review clearly indicated to us that we had failed to clearly indicate the purpose of the paper. The manuscript is indeed a technical paper describing the decision making process when objectively mapping Argo data within the Weddell Gyre onto a regular grid. The authors have carefully followed the criteria outlined in the scope of the journal, in describing the process of data creation while avoiding any scientific interpretation of the resulting gridded dataset. However, we realise that this very aim of the paper was not made clear enough and the authors have set out to improve the manuscript accordingly. The authors would especially like to acknowledge the suggestion of comparing the resulting dataset to existing climatologies, which has now been included. Future work will involve the scientific analyses of the dataset, which will be in context to current knowledge of Weddell Gyre hydrography, and particularly to long-term trends of ship-based observations. Please note that by following the suggested changes, a rearrangement of the structure of the manuscript was necessary.

Detailed comments:

R2: 1. Introduction is too long. Description of the observational evidences of the

Weddell Gyre (WG) warming is not relevant (p. 2, lines 17-27)

RESPONSE: The irrelevant paragraph has been removed and the introduction shortened.

R2: 2. A vast amount of historical hydrographic data gathered in the WG before the begin of AWI research activities (ca. 1985) is not mentioned in the manuscript (page 2 28-32, page 3 1-6). In spite of the fact that these historical data generally have a lower quality and precision the general hydrographic structure of the WG was already known before ca. 1985 . Further ship-based hydrographic studies and Argo programm results added additional details to that original picture.

RESPONSE: The following paragraph has been added to highlight the historical record of data in the region, as well as to highlight the numerous data resources (p2 ln22 to p3 ln15):

"To date, the literature focusing on Weddell Gyre hydrography has been largely based on observations from repeat hydrographic sections – primarily collected during various cruises (e.g. Fahrbach et al., 2004; Fahrbach et al., 2007; Fahrbach et al., 2011), as well as data from moorings, deployed both along the Prime Meridian and strategically placed locations throughout the gyre (Klatt et al., 2005; Fahrbach and De Baar, 2010; Behrendt et al., 2011).These data are well-established (there are now 30 years of data collected from RV Polarstern alone). Historical measurements however date back to the early 1900's; for example, Brennecke (1918) combined the observations from Swedish (1901-03), Scottish (1902-04), French (1908-10) and German (1911-12) expeditions and provided sufficient evidence for proposing the cyclonic circulation of the Weddell Gyre. A review of historical research on the Weddell Gyre is provided by Deacon (1979). These data have provided us with a picture of the structure of the Weddell Gyre and have provided insight to the role of the Weddell Gyre in a larger climate perspective; Fahrbach et al., (2011) provides an in-depth comprehensive analysis of the variations within the Weddell System. However, much of the analysis of long-term

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changes is based on data along the Prime Meridian only – a region of high variability due to its close proximity to Maud Rise – influencing the relatively high frequency fluctuations of observed WDW properties.

In addition to repeat hydrographic sections and moorings throughout the Weddell Gyre, there are also data from Argo floats, drifters and animal-borne sensors. These all combine to provide a multi-platform approach to observing the Weddell Gyre. Here, the authors focus on the Argo float dataset, in order to provide an independent dataset that can be compared to ship-based observations in the near-future. Moorings are excluded from the analysis as they provide vertically sparse data, while drifters only provide surface data, which is excluded from the analysis due to high surface variability, and animal-borne sensors require special treatment due to salinity and depth sensor issues. Thus, delayed-mode adjusted Argo float data are the sole focus of this study."

R2: 3. Since the compilation of a new gridded product (gridded climatology) is in the focus of the manuscript, a detailed comparison with existing gridded (and, perhaps, also with not-gridded) climatologies is absolutely necessary and is completely missing in the manuscript. Starting with the Gordon-Molinelli Southern Ocean Atlas, the other gridded climatologies should be cited: Olbers et al "Hydrographic Astlas of the Southern Ocean", NODC climatologies, "WOCE Hydrographic Climatology", "WOCE hydrographic Atlases for the Atlantic and for the Southern Ocean". In this list Olbers et al. Climatology and WOCE climatology both used the similar optimum interpolation method.

RESPONSE: We include a new section in the discussion (section 6.1; p20, ln27 to p24, ln24) which focuses on the comparison between the gridded fields presented here and climatologies. In particular, we focus on the WOA13 climatology with the time span of 2005 to 2012, which is the closest matching time span to the time spans presented in this manuscript. The authors would like to thank the reviewer for this constructive criticism.

R2: 4. A small discussion on the WG warming (page 2, lines 17-27) is not relevant and should be removed.

RESPONSE: Modified accordingly.

R2: 5. A long description of the Argo data in the introduction (page 3, lines 724) should be placed in the data description section.

RESPONSE: This has been removed from the introduction, and a new section has been created: section 2: “Source data description: Argo float profiles” (p3 ln28-p6 ln13).

R2: 6. Page 3, lines 27-30: it is not clear for me, what makes Argo data so different from, say, classical hydrographic observations, which are distributed irregularly as well.

RESPONSE: The focus of this paper is to provide an Argo-based gridded dataset independent from ship-based observations. We also wanted to highlight the potential in using Argo floats to present a large scale view of the water properties of the Weddell Gyre, collected within a limited period (few years) of time. Argo float profiles are spatially irregular and cover a large region – this paragraph introduces the concept of objective mapping as a suitable method for processing and gridding Argo float data. The paragraph has been removed from the introduction and placed in the methods section 3.2.1: Approach to objective mapping (p7-9). The clarification of this decision to use only Argo floats is now provided inÂňÂňÂň the introduction (p3 ln7-15).

R2: 7. The description of the optimal interpolation method should be placed in a separate section (page 3, 25ff, page 4. The choice of references to earlier research implying the optimal interpolation method, as I mentioned earlier, is not complete.

RESPONSE: This section has been moved to the methods section 3.2.1: approach to objective mapping, and combined with the text originally in section 2.3. A new paragraph discussing climatologies is included as follows (p8 ln15-30):

“Objective mapping methods are typically used in the production of climatologies. The

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most prominent and recent are the WOA atlases (World Ocean Atlas; Locarcini et al., 2006, 2010, 2013; Antonov et al., 2006, 2010; Zweng et al., 2013), which use a 3-pass successive correction method (with the exception of WOA98 which applies a one-pass successive correction; Cressman, 1959; Barnes, 1964; Barnes, 1994), and the WOCE atlases (World Ocean Circulation Experiment; Gouretski & Koltermann, 2004; Orsi et al; 2005), which follows the optimum interpolation technique described above. The successive correction method is used in WOA in order to avoid the use of second-order statistics due to the paucity of data (Locarcini et al., 2013), while WOCE justify the use of the Gauss-Markov technique by acknowledging that the Gaussian correlation function used is highly arbitrary and results in over-smoothing at small scales (McIntosh, 1990; Gouretski & Koltermann, 2004), yet that the successive correction method may yield less consistent results (Sterl, 2001; Gouretski & Koltermann, 2004). Here, the optimum interpolation (or "Gauss-Markov technique") is used. The most notable differences between the mapping method described in the following section and the approach used in the climatologies above are discussed in section 6.1."

An in-depth discussion of the differences between the approach presented here and other approaches is provided in section 6.1 (p20-24).

R2: 8. Section 2.6 should be removed. I do not understand the necessity of error masks. As soon as the OI provides error estimates, the users of the new gridded product can make decision of their own which gridded points should be masked.

RESPONSE: Thank you for the feedback. Section 2.6 has been removed.

R2: 9. Page 12, suggest to remove lines 15-21.

RESPONSE: Modified accordingly.

R2: 10. The description of the results should be re-written completely. In its present form this description simply states that previously known general features of the WG are also captured by the new gridded product. Much more important and interesting

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would be to answer the following questions: -what new previously unknown features of the WG thermohaline structure appear in the new gridded product? -how good is the agreement with the existing climatologies?

RESPONSE: The aim of the paper is to provide a dataset. It is beyond the intention of the journal to interpret the results in a scientific context, and therefore the authors have carefully avoided doing so. However, while hence the results section remains largely unchanged, in the fact that it describes the resulting fields of data, we provide an in-depth discussion of the results in the context of what is already known about the Weddell Gyre hydrography, as well as in the context of existing climatologies. Please see the discussion section 6 (p20-26). Within this section, we note a particular feature present in the datasets provided that is not clearly visible in either climatologies investigated in section 6: the presence of a Taylor Column over Maud Rise (e.g. p23 ln27 to p24 ln4).

R2: 11. Why the maximum number of points for the optimal interpolation is set by 40?

RESPONSE: The following paragraph has been added to section 3.2.3 (p12 ln2): “The number of data points (N) used in the calculation of the field estimate was set to 40, a necessary limitation to cope with constraints in computational power.” Similar constraints have been used by existing climatologies, for example, in WOCE (Gouretski and Koltermann, 2004), where the input data are a combination of original data points, and 55 km2 box averages, and the closest 150 input data points within the radius of influence (750 km) are selected for the optimal interpolation (for more information, please refer to section 6.1)

R2: 12. What is the noise to signal ratio used in the calculations?

RESPONSE: As an example, for conservative temperature at 800 dbar, the Signal-to-Noise ratio (the ratio of signal variance in Eq. 6 to noise variance in Eq. 7; section 3.2.2) ranges from about 0.5 in some regions to an excess 5 in other regions, although the majority of the grid ranges from 1 to about 3 (shown below). Note that the estimate

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of noise is probably pessimistic because it averages the distance between all station pairs where signal variance might contribute to the average (*Fukumori and Wunsch, 1991). Please refer to fig.1 of this document. * Fukumori, I., & Wunsch, C. (1991). Efficient representation of the North Atlantic hydrographic and chemical distributions. Progress in Oceanography, 27(1), 111-195.

R2: 13. Page 15, lines 2-4: should the piece of text to remain, some references to previous works describing the nature of the Eastern WG could be added here (e.g. Gouretski&Danilov, 1993,1994)

RESPONSE: This paragraph has been removed. The individual time periods are no longer discussed to make space for the comparison with climatologies.

R2: 14. Page 15, line 25: the term “scatter-grams” is unknown for me. Moreover, the authors refer to figure 13a, which simply shows positions of profiles colored according to the respective temperature.

RESPONSE: this section has been strongly edited and moved to section 4.1. The term “scatter-gram” is no longer used.

R2: 15. Page 16, lines 1-24: this discussion is not relevant to the main issues of the manuscript

RESPONSE: This paragraph has been removed as a description of the pertinent features of the gyre are provided in the results section 5.

R2: 16. Page 16, lines 25-33, page 17, line 1 ff.: this discussion is a trivial one: the less data we have, the larger the interpolation error

RESPONSE: This discussion has been largely edited and moved to section 4.1. The focus of this section is on the error maps provided in the datasets, and the different sources of error. This discussion is necessary due to the impact of using large length scales, which is discussed in detail.

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R2: 17. Page 19, line 9: this is the third (!) citation regarding the “sea-ice-sensing” floats.

RESPONSE: The paper has now been edited to avoid this repetition. The citation is mentioned once in section 2 (p4 ln3-9). The interpolated float locations are mentioned in section 2 when describing the data (p5 ln10-26) and discussed in section 4.1, in terms of how the interpolated positions influence error (p14 ln4-18).

R2: 18. Concluding remarks section: page 22, Line 7: I am not persuaded at all that the new product gives a more detailed view of the WG.

RESPONSE: In comparison to WOA13, for the closest matching time period available, the data provided here does provide a more detailed view. Although, a higher resolution grid is available which may provide those details. The data of the $\frac{1}{4}^{\circ}$ grid has not been looked into due as the aim of the comparison was to select the closest matching climatology to the dataset provided here. However, the sentence has been altered as follows (p27 ln11):

“The resulting mapped fields provide a complete, detailed view of the pertinent features of the Weddell Gyre. . .”

R2: 19. Figs. 04A-c provide no new information regarding the data distribution compared to the position plots in Figs. 02A-c. Suggest to remove them.

RESPONSE: Figures have now been removed.

Review #3

Overall impression

R3: 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,

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the manuscript reads more like an initial draft.

RESPONSE: The authors would like to thank the reviewer for the detailed suggestions and corrections to the manuscript. In particular, we are grateful for the suggestions regarding the re-structuring of the paper. We have re-organised the script and found this to be extremely helpful in the improvement of the script. The new structure is listed on the first page of this document.

R3: 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.

RESPONSE: While we stick to using conservative temperature and absolute salinity, gridded fields of potential temperature and salinity are now also provided. Indeed these variables have been used in section 6.2 to assess the gridded data in the context of literature. The following sentence has been added to section 2 (p5 ln29 to p6 ln1):

"Conservative temperature is more representative of the "heat content" of seawater than potential temperature (McDougall and Barker, 2011); however, because conservative temperature and absolute salinity have been introduced to oceanography rather recently, limiting comparison with historical climatologies and other hydrographic datasets, potential temperature and practical salinity are also provided."

R3: 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.

RESPONSE: In the original manuscript, the mapped error variances were presented rather than the square-root. This has been corrected; the mapping errors are indeed

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larger. Apologies for this confusion. The profile locations are now overlaid onto the mapped error (Figs. 11-12, 14, 21-22). Note the sub-sampled time period maps are no longer included in the revised manuscript in order to make room for a climatology comparison in section 6.1, as requested by reviewer 2.

Details

Introduction section

R3: 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.

RESPONSE: This paragraph has been removed as it has been suggested that it is irrelevant to the topic of the paper, but thank you for the reference suggestion (Zenk, W., Siedler, G., Lenz, B. and Hogg, N. G.: Antarctic Bottom Water Flow through the Hunter Channel*. Journal of Physical Oceanography, 29, 2785–2801, 1999).

R3: 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).

RESPONSE: Modified accordingly.

R3: "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.

RESPONSE: Modified accordingly.

R3: 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.

RESPONSE: This paragraph has been moved to the methods section under 3.2.1 (p7

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ln23 to p8 ln2). It remains (though shortened in length) as it provides context into the method used and highlights the justification for the decisions made in creating the data product, i.e. the studies cited incorporate strict temporal correlation length scales into the decay scale calculations due to having larger datasets available to them, and also because the study regions are highly dynamic. However, in doing so, the higher latitude regions south of 50° S become poorly represented, due to the failure of the available data in passing the e-folding decay scale.

R3: page 6: "excluding regions beyond the Weddell Gyre boundaries" - please define these here (not in 'methods').

RESPONSE: Thank you; this has been included as follows (p3 ln17-22): "We describe the method followed in order to objectively map the irregular Argo float profile data onto regularly gridded fields (on both pressure surfaces and onto the level of the sub-surface temperature maximum), excluding regions beyond the Weddell Gyre boundaries (50 to 80° S; 70° W to 40° E; although the northern boundary is based on the position of the Weddell Front for the pressure surface maps: section 3.1)."

Methods section (Please note: this first section of feedback in methods is now in section 2: source data...)

R3: page 6: how many profiles are left after removal of duplicates?

RESPONSE: There were only three duplicate profiles from two different floats. This has been included in the description as follows (p4 ln18):

"The profiles are checked for duplicates which are subsequently removed (there were only three duplicate profiles from 2 floats overall)."

R3: "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

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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.

RESPONSE: Modified accordingly.

R3: "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.

RESPONSE: For mapping at the level of the sub-surface temperature maximum, the northern boundary is 50° S. The resulting temperature map is used to determine the position of the Weddell Front, which becomes the northern boundary for the fields mapped on to pressure surfaces. The sentence has been altered as follows (p4 ln24-27):

“(about 75 % of these profiles are actually located north of the Weddell Front; a frontal system which defines the northern boundary when mapping to pressure surfaces; see section 3.1). These “shallow” float profiles most likely occur due to complex bottom bathymetry.”

R3: 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.

RESPONSE: According to the Argo data centre website (http://www.argo.ucsd.edu/Acpres_bias.html), there are reported issues of pressure biases in the Argo data set. While these biases should by now be corrected, there is the recommendation that only profiles with a pressure quality flag of 1 are used and an adjusted pressure error of more than 20 dbar rejected. This was carried out as an extra precaution. The sentence now reads (p4 ln31 to p5 ln1):

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“Additionally, any data points where the corresponding adjusted pressure error exceeds 20 dbar are rejected. This is an extra precaution against pressure biases and is in accordance with the guidelines provided on the Argo website (www.argo.ucsd.edu).”

R3: page 7: "The temperatures in Argo are reported to be accurate to ±0.002 C while pressures are accurate to ±2.4 dbar (Owens and Wong, 2009). For salinity, if there is a small sensor drift, uncorrected salinities are accurate to ±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.

RESPONSE: The above is a standard criterion used by the Argo community (see the Argo quality control manual at http://www.argodatamgt.org). The following sentence has been added to the end of the corresponding paragraph (p5 ln7-9):

“Within the delayed-mode adjusted salinity data (with a quality flag of 1) used in this study, the mean adjusted salinity error is 0.01 psu, while the largest error does not exceed 0.1 psu”

R3: "of seawater in comparison to potential temperature" -> "of seawater than potential temperature"

RESPONSE> Modified accordingly.

R3: "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?

RESPONSE: The sentence has been modified accordingly, and the following has been added as the next sentence in order to explain why the upper 50 dbar have been excluded (p6 ln2-5):

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“The upper 50 dbar are omitted from the dataset due to strong seasonal variability and sea-ice interaction. The 41 levels are spread such that the intervals are smallest at 50 m (10 m) and increase to maximum of 100 m spacing below 800 m. The levels themselves were arbitrarily selected.”

Pressure surfaces have been used at this stage instead of density levels for the sake of future research; the authors are currently using the dataset to calculate the heat content of this layer of the ocean and would like to ensure that the volume is fixed in place.

R3: 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.

RESPONSE: Although the sub-surface temperature maximum is rather stable, the pressure at which this maximum occurs varies considerably. This is because the profiles vary according to location and season when it comes to identifying the sub-surface temperature maximum. When investigating individual profiles, it became apparent that using fixed pressures as conditions for finding the sub-surface temperature maximum would be unreliable. Thus a suitable algorithm was needed to accurately select this level for the ∼19,000 profiles. The maths of this algorithm is actually very simple:

X=sum[zscore(tmp),zscore(pressure)]; i=find(X==min(X)) Tmax=max(tmp(i:end))

This is extremely useful for finding the sub-surface temperature maximum for large datasets regardless of the vertical structure of the individual profiles.

The entire paragraph in section 3.1 (p6 ln16 to p7 ln20) has been rewritten to improve

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the explanation of the method and to clarify the value of such an algorithm when dealing with large datasets.

section on Approach to objective mapping

R3: 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.

RESPONSE: This paragraph has now been removed.

R3: The second paragraph basically describes a method assuming the meridional gradient dominates the field at each depth.

RESPONSE: This paragraph has been reduced in content, but still remains as it summarises the mathematics shown in the following section. The paragraph, which is the last paragraph of section 3.2.1, now reads as (p8 ln31 to p9 ln11):

“In this study, the aim is to provide a broad outlook on the properties across the entire Weddell Gyre. Therefore the objective mapping omits temporal averaging resulting in maps that represent spatially gridded time composites of the field variables for these time periods. The mapping process is implemented in a two-step procedure, allowing for a step-by-step improvement of the mean field estimate. In the first stage, the first guess field is the zonal mean, and the covariance is a function of large scale separation. The resulting field estimate then becomes the first guess field in the second stage of mapping, where the covariance is a function of small-scale separation, which gives extra weight to close-by data in regions where the data are abundant. In regions of sparse data density, the objective estimate reverts back to the mean guess field and the corresponding mapping error is large. This 2-stage method approach reduces the possibility for errors by providing an improved estimate of the first guess field, which leads to a general reduction in the magnitude of the signal variance, <s2>, by which the covariance matrices are scaled by.”

R3: pages 10-15: The description of the mapping technique is detailed, but it is not

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clear how much improvement this technique is over simpler mapping techniques.

RESPONSE: The main improvement this technique provides in comparison to available climatologies is the inclusion of the cross-isobathic separation component of the decay scale, as defined by Boehme and Send (2005). This has allowed for the detailed consideration of bathymetry, and indeed the fact that water bodies have a tendency to flow along lines of constant planetary potential vorticity. This has now been discussed in detail throughout the manuscript, most particularly in the discussion section 6.1 (p20 ln27 to p24 ln24), which provides a comparison to the mapping approach of other climatologies, focusing in particular on the WOCE and WOA climatologies.

R3: 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?

RESPONSE: Regarding the length scales, please refer to the answer to point 2.5 made by reviewer 1. The length scales are indeed large; however, with the resulting fields being time composite datasets, the authors focus on the large scale features of the Weddell Gyre as it is not sensible to focus on the small scale variability without incorporation of a temporal separation factor. The authors have made steps (such as filtering out the data north of the Weddell Front for the pressure surface maps; incorporating differences in potential vorticity into the criterion for selecting N profiles and limiting N to 40) in order to reduce errors as much as possible. Please note the second paragraph in the discussion section 6.1 and table 2, which shows that the length scales of available climatologies for a grid resolution of 1° x 1° are also large (e.g. WOA: 892 km; 669 km and 446 km respectively for each pass, no consideration of bathymetry; WOCE: single pass with 450 km where the grid point is > 500 km from the coast, with a first guess field as the box mean of a 750 km sub domain; or indeed WOCE_SO: single-pass with an elliptical radius of influence of 666 x 333 km in areas where the bottom depth exceeds 4000 m), and do not include the cross-isobathic separation factor which reduces the radius of influence significantly (e.g. figures 9 and 10).

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The following has been included in section 3.2.3 (p12 ln2) regarding N=40:

“The number of data points (N) used in the calculation of the field estimate was set to 40, a necessary limitation to cope with constraints in computational power.”

Results section

R3: 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?

RESPONSE: This statement was based on knowledge drawn from the literature. The sentence now reads (p19 ln4-7):

“A double gyre structure is also suggested, where the secondary gyre occurs in the north-east sector, splitting from the main gyre at about 5° W; this is in agreement with the literature which commonly refers to the Weddell Gyre as a double-cell structure (e.g. Klatt et al, 2005; Beckmann et al, 1999).”

R3: 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?

RESPONSE: This feature is caused by the prevalence of a well-documented Taylor Column over Maud Rise. The following sentence has now been added directly after the sentence quoted above (p19 ln21-25):

“This is in agreement with literature, which shows the presence of trapped water in a Taylor column over Maud Rise; this also results in a localised cooler sub-surface temperature maximum in comparison to surrounding regions (Bersch et al. (1992), Muench et al. (2001) and Leach et al., (2011).”

R3: 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.

RESPONSE: Please note this figure is now Fig. 20. Error bars have now been added which show the combined mapping errors of the corresponding grid cell sections, and grey dots show the original profile data within 1° of the Prime Meridian.

R3: 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.

RESPONSE: The subtitle (now section 5.2; p20 ln4) is as follows:

"5.2 Conservative Temperature and Absolute Salinity at 800 dbar"

R3: 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.

RESPONSE: Thank you; the sub-time periods have now been defined as TP1, TP2 and TP3 in section 2: Data description: Argo float profiles (p6 ln6-9):

"Objective mapping is applied to the entire dataset spanning from December 2001 to March 2013, as well as to 3 sub-sets, where the data are split according to the following time periods: 1) 2002-2005; 2) 2006-2009 and 3) 2010-2013 (hereon TP1, TP2 and TP3 respectively)"

R3: 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.

RESPONSE: This was a mistake due to presenting the squared form of the error (i.e.

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the variance, which has now been corrected. A detailed discussion of the errors and how they are influenced is now provided in section 4.1 (p12 ln31 to p15 ln14).

R3: 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.

RESPONSE: The sub-sampled time periods are no longer presented in the newly revised manuscript in order to provide room for a comparison with climatologies, as requested by reviewer 2. However, the same mistake listed above applies here: the root of the error variances should have been presented.

The scatter plots of station locations are now overlaid onto the error maps as recommended, and the mask is no longer used. Additionally, the following is found in the opening paragraph of section 4.1 to discuss the potential issues regarding mapping errors (p12 ln32 to p13 ln10):

“There are several types of error one should be aware of when looking at interpolated fields of Argo profile data such as conservative temperature and pressure at the level of the Tmax (Fig.11-12) and the maps presented in section 5. The first and most obvious is instrument error (section 2); the second is the relative error of the objective interpolation (i.e. mapping error), which is the square root of the mapped error variance provided (ãĂŰεg=σg2ãĂŮ; Eq. 8). The mapped error takes into account the spatial distribution of the input data as well as its signal variance. The mapping error (e.g.

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Fig. 11c) is the quantitative error value provided and is representative of these factors, but should only be taken to represent an estimate of error associated with the specific interpolation method. Indeed, this statistical error is sensitive to length scales used in the covariance functions within the mapping process. The error estimate is inaccurate, because the "true" covariance function is unknown."

Discussion section

R3: 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 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.

RESPONSE: As previously mentioned on the first page of this document, the manuscript has now been restructured based on the feedback above. There is now a section on the objective mapping performance (section 4), which discusses the error (section 4.1) before the results are presented in section 5. The results still summarise the main features of the presented maps, but the discussion section 6 now focuses on the results in the context of available climatologies and the literature.

R3: 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.

RESPONSE: This is now discussed in detail in section 4.1. Please refer to response above regarding page 18, and the following text in 4.1 (p13 ln10 to p14 ln3):

"The mapping errors are relatively small within regions of adequate data coverage, with small horizontal gradients of change within the variable to be mapped (i.e. so that the corresponding signal variance across the N data points is small), and where bathymetry is considerably constant (thus small variation in planetary potential vorticity). In the western sector of the Gyre interior the bathymetry is relatively flat, and the horizontal gradients of change are relatively small. Therefore the mapping errors in these regions are also small despite the sparsity of data; with the exception of areas where there are no data points nearby, such as in the far south-west region. In regions with dense data coverage, mapping errors can be high if bottom bathymetry is complex due to the increase in the cross-isobath separation between locations, regardless of horizontal distance. This can be seen along the northern gyre periphery, especially East of about 20° W: the data coverage is large and yet so are the mapping errors (West of 20° W there is a spatial gap over the northern submerged extension of the Antarctic Peninsula, which explains the large errors in this region). The bathymetry is complex due to the presence of submerged ridges and trenches. It is also at the very periphery of the gyre where complex interaction with the Antarctic Circumpolar Current takes place (e.g. Fahrbach et al., 2004; Klatt et al., 2005; Fahrbach et al., 2011; Cisewski et al., 2011). Thus, the objective mapping is poorly representative of these highly variable, complex regions. One way to improve the objective estimate of these regions is to incorporate more suitable correlation length scales as well as a temporal separation factor into the decay scale in Eq. (4) and Eq. (5), such as in Böhme and Send (2005). The correlation length scales would need to match the scale of the true field in order to adequately map these regions. Since these regions typically only occur at the very periphery of the gyre, and due to data sparsity throughout the relatively invariant inner gyre, the correlation length scales are chosen to represent the large scale field of the entire gyre. Thus, mapping error can be very low in regions of sparse data coverage if bathymetry is constant between the grid point and the station locations, and if the difference in water properties between the N neighbouring profiles are also

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small."

R3: Section 4.2 and 4.3 are also more methodological - see my comments about restructuring the paper.

RESPONSE: These sections have now been moved to section 4: "Objective Mapping Performance" (p15-17 and p17-18).

R3: Section 5 is nice and short, which makes one wonder why the previous sections are very long and frequently repetitive.

RESPONSE: By following the recommendations listed above and restructuring the manuscript, repetitions are now avoided. The manuscript is still long, due to the newly included discussion and comparison to climatologies.

Figures:

R3: # 1: nice schematic, but some fonts are too small

RESPONSE: The fonts have now been enlarged.

R3: # 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).

RESPONSE: The font sizes have now been increased. The region of small mapping error has an error of about 0.015 °C, a corrected value as the original estimates were of the squared form. The mask has now been removed, and the profile station locations are plotted over the mapping errors to highlight the gaps in the data, as recommended.

ESSDD

8, C429–C466, 2015

Interactive Comment

Please also note the supplement to this comment:
http://www.earth-syst-sci-data-discuss.net/8/C429/2015/essdd-8-C429-2015-supplement.zip

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Interactive comment on Earth Syst. Sci. Data Discuss., 8, 509, 2015.

ESSDD

8, C429–C466, 2015

Interactive Comment

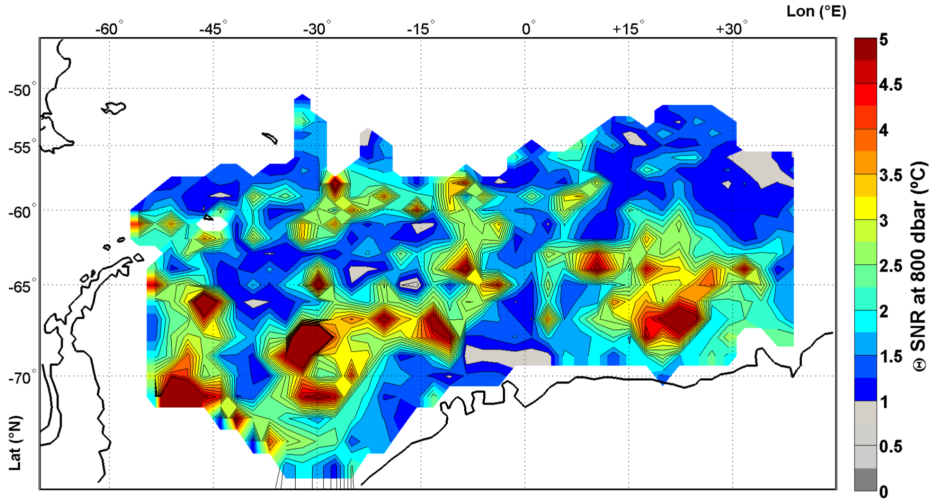

Fig. 1. SNR of conservative temperature at 800 dbar, for the entire time period.

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Interactive Discussion

Discussion Paper