

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 #2

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General comments:

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

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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.

Detailed comments:

1. Introduction is too long. Description of the observational evidences of the Weddel Gyre (WG) warming is not relevant (p. 2, lines 17-27)
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.
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.
4. A small discussion on the WG warming (page 2, lines 17-27) is not relevant and should be removed.
5. A long description of the Argo data in the introduction (page 3, lines 724) should be placed in the data description section.

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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.
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.
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.
9. Page 12, suggest to remove lines 15-21.
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 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?
11. Why the maximum number of points for the optimal interpolation is set by 40?
12. What is the noise to signal ratio used in the calculations?
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)
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.
15. Page 16, lines 1-24: this discussion is not relevant to the main issues of the manuscript

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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

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

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.

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.

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