

***Interactive comment on* “How well can we derive Global Ocean Indicators from Argo data?” by K. von Schuckmann and P.-Y. Le Traon**

Anonymous Referee #2

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Review of the manuscript “How well can we derive Global Ocean Indicators from Argo data?” by Schuckmann and Le Traon

The submitted paper is devoted to an important issue of inferring ocean climate variability from the observations. The unique Argo dataset opens new possibility for a more accurate assessment of several important global ocean climate indicators such as the ocean heat content anomaly, ocean freshwater content anomaly and the global sea level.

However, both the presentation of the results and putting them into the context of other related studies needs a significant re-working. The English text also needs a significant improvement due to numerous errors in wording and style. Some parts of the text are difficult to understand, and there is quite an amount of redundancy throughout the

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whole paper. Though I made several suggestions for possible changes, I strongly recommend the authors to ask for help among colleagues more profound in English.

My assessment: reconsider pending major changes are done

Specific comments:

Abstract:

The second line tells that the floats provide a good coverage down to 2000 m depth. Line 15, however, indicate Argo sampling to extend between 10 to 1500 m.

Line 15: long-term trends can be performed: I think it is the calculation of the trends what is really meant here, since the trends are not performed but rather calculated/estimated.

Line 17: steric rise better change “to ocean level rise (steric component)”.

Line 8: “They include a proper estimation ... better change to “are accompanied by error estimates”

Introduction

Here again (line 19) the depth range down to 2000 m is indicated. Few lines later (page 3, Line 4) the statement is made about Argo floats measuring AT LEAST within the upper 2000 m. Does it mean some floats can measure deeper? Or do you mean possible technology development in the future??

Line 6: the fresh-water content has NOT been neglected, and there are papers (Wijffels et al.??) which must be cited here.

Line 10: “steric component of the levels rise ... is a cause of global SL change. It’s better to say that the warming of the ocean leads to the SL increase due to the respective change in the sea water density.

Lines 12 to 19: the text could be omitted

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Line 20: To my mind the SL rise is (partly) driven through the continental ice melt water input into the global ocean, e.g. the water mass increases leading to the level rise. This additional fresh water dilute the salty ocean water: salinity decreases.

Lines 23-24: bad wording. It's better to say that satellites (which measure the total SL rise signal) help define the SL rise component linked to the input of melt water into the ocean, since this component is NOT measured by argo floats.

Page 4, line 5: “the underlying uncertainties ... are still unclear” – bad style. The uncertainties arise because smth. is unclear!

Lines 9 -11: The sense of the sentence is not clear. The same with the next sentence, starting with “But nevertheless, ...”. Please ask colleagues profound in English for help!!!

Page 4, lines 18-19. Please, rewrite, otherwise the sentence bears an imprint of tauology: “changes ... induce fluctuations..” (the latter are changes as well!)

The overall discussions on the issue why the OFC is important (Page 4 Line 18 to Page 5, line 5) should be rewritten. For instance, the thesis “While impacts are local and regional, the causes and patterns are global” needs more discussion, or should be omitted.

Page 5, line 6: “These discrepancies show..” It is not clear which discrepancies are meant!!

Line 9 change “refined error estimates” to “error estimates” Line 11: change “A careful discussion..” to “a discussion”

Data sets and methods

I am not satisfied with the description of the data and methods.

Line 22: the datasets are processed by processing tool “ISAS-STD” – bad wording. Instead of reporting the name of the software product, it is much more important to

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know how it works, namely: 1) what is a climatological test? 2) how the observed values are interpolated on standard levels?

Line 27: It was already mentioned in the previous paragraph, that the data for 2005-2010 were used – please, avoid such repetitions! Lines 27ff: what does this compromise exactly mean? Why numerous argo profiles are shallower than 2000 m?

Page 6, Line 10: the gridded in situ product ARIVO – here, again, the name (what is not important) is provided, but the essential details are completely absent. I assume “the gridded in situ product” simply means “gridded climatological fields of T and S – is that true? – the description is totally misleading. It is said, the gridded product is used to extract climatology? Does it mean the “gridded product” is a software tool, which allows to read in a climatology??

Further, the argumentation is missing completely WHY the climatology is used to fill the gaps. Using climatology would obviously lead to T or S-anomalies being simply zero? Than what is the motivation here?

Testing the results against different climatologies is justified. However, the description of the results is unsatisfactory. For instance, I disagree that the differences between the two cases (ACLIM and WOD05 climatologies) are larger at the beginning of the time series: it is simply not seen in the fig. 2!!! Moreover, the statement about the differences being largest for GSSL is simply wrong: how could different variables be compared? Is, say, a 10mm SL difference larger than 0.5 degree C difference???

Page 7, line 1: “the variance information to build this criterion . . .” needs more explanation. On the previous page the authors mention that a 3-sigma limits have been selected – why? Or is it a subjective decision?

Page 8, Line 5: “The GOIs are evaluated from the horizontal data distribution” - I do not understand, what distribution is meant here. Line 6 domain involves the effective coverage – bad wording, please, rewrite.

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Line 7: “Mean estimations of physical parameters” – what is meant here? Is it the estimation of the mean value???

Page 8 Line 13ff: the usage of the weighting matrix W is not sufficiently justified. Why the anomalies are (much) larger near the coast? Is it an artifact of the averaging within large boxes, or is the variability higher there? What happens (with respect to the time series) if the coastal boxes are omitted??

Lines 15ff: I suggest simply to omit the two last sentences of this section: they bring no new information at all.

Error estimation

Page 9, Line 1: redundancy again: please, remove two sentences starting with “Using the box-averaged. . .”

Lines 5ff: I was pleased to see the comparison of different argo-based products. However, here again the description of the inter-comparison procedure is unsatisfactory. What is shown in Fig. 3 are three steric height time series. Were these time series obtained by the authors of the submitted manuscript? If yes, how were they calculated?

I disagree that the largest deviations are observed in the beginning of the time series, as the year 2007 exhibits the largest discrepancies.

Page 10, line 8. “... the climatology used to fill vertical gaps and to evaluate the anomaly fields”. To my understanding the climatology provides the reference against which the anomalies are calculated. I do not understand how the climatology is used “to evaluate” the anomalies.

Page 10, lines 11-14. It is not clear to me what residuals are meant which were used to estimate E-clim. Or is it the RMS difference between the two time series which provides a measure for E-clim???

Page 11, line 1. I think, earlier in the text a different year was mentioned for the time

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moment since then the coverage was complete.

Page 11, lines 10ff. I recommend delete from “This approach allows . . .” to the end of Line 17, as all the information here is redundant.

Lines 17-19 “A forecast calculation . . . has been established” – how the calculation can be established??? Line 18: here, the same redundancy again: the reader has already been informed many times on when the argo array has achieved its complete shape.

Page 12, lines 1-4 I do not understand what they want to tell here.

Method validation

Page 12, lines 6-9. Bad formulation: “Altimeter observations . . . correlate with in situ upper ocean observations” Observations do not correlate: the observed parameters may be correlated. “For this purpose, maps of. . .” Which purpose is meant???

Line 11: what kind of “in situ estimations” do the authors mean?? Lines 22-24: I suggest to delete the sentence starting with “However, using . . .”.

Line 25: “The comparison between the two global averages calculated in DIFFER-
EWNT ways. . .” Just a few lines earlier the authors note, that for the calculation of the MSLA from the gridded altimeter fields the SAME method as described in section 2, w3as used – this is an obvious inconsistency.

The following description of the two MSLA time series is unsatisfactory. The authors note differences between the two curves in “high-frequency variability”. The scale of this variability is not defined, but the highest frequency variability as resolved by the curves in Fig. 4 is rather similar (to my mind) for both curves, and it is for the longer-period variability where the two curves differ. Moreover, the Fig. 4 indicate an offset between the ANOMALY curves. After deletion of this offset the agreement would significantly improve.

Page 13, line 2. Here again the year is indicated when the sampling becomes com-

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plete: redundancy!!!

Page 13, Lines 8-10. “The calculation . . . has been chosen to represent. . .” WRONG: the three globally averaged characteristics of the ocean state were selected to represent the so-called GOIs!!!

The description which follows is completely unsatisfactory and should be rewritten.

For instance, they start with the GSSL (Line 11), but then start to discuss the errors, which obviously decrease with the improvement in the argo array not only for the GSSL, but for all indicators under consideration. Moreover, the decrease in the error magnitude with time is barely visible in Fig. 5 (by the way, the footnotes a), b) and c) are absent in the Figure).

The new values of the OHCA should be compared not only with those by Schukmann et al. 2009, but with several other estimates available in the literature.

Line 21: I do NOT agree that the inter-annual variations of GSSL and OHC are smaller in amplitude compared to the long-term variability. Here, again is not clear, what the long-term scale is. Should it be the whole analyzed period 2004-08, than the typical amplitude of the inter-annual variability is not smaller. Moreover, considering just the size of the error bars, the long-term change in GSSL and OHC is also not significant.

Conclusions

This section need substantial re-working. It is not clear for me, why the attention is drawn only to the paper by Lyman et al 2010 (lines 5-7, page 14).

Line 14: there is a significant inter-annual global variability at global scale – oh, what is it!!!

Line 15ff: actually, it is not shown in the paper, that the short-term trends can not be estimated. Again, what dies it mean “short-term”???

Line 17: “uncertainties due to inter-annual fluctuations are not included in our error

estimation” – completely unclear, what do they want to tell here. In the next sentence they say, “this (WHAT???) will change with the growing set of Argo measurements”

Line 22: please, do not pretend to say your method is a proper one

Line 26: “uncertainty estimations due to the data handling”: I guess what they really mean here was something like “ estimation of uncertainties arising due to the method which was used”

Line 27: of course, it is trivial, that only the period 2005-2010 has been analyzed, and that the trends for this period may differ when calculated for the other periods.

Page 15, last sentence before the Appendix should be deleted.

Fig.2, page 23: please, insert labels a), b), c) for the respective panels and introduce changes in the main text. The sense of the last sentence in the figure caption is not clear: “,... for the choice of reference climatology ... two different climatologies are used”. Nowhere in the text is indicated which climatology has been chosen.

Fig. 3 I guess what is really show here is the globally averaged steric height ANOMALY.

Fig. 4 Method validation. . . I guess they wanted to say “Validation method”. I do not agree with this terminology. Usually in situ observations are used to validate the results based on the proxy data (in this case it was the satellite altimetry). It is better to speak about the comparison of the two time series, one of which was derived from the satellite altimetric observations.

Fig.5 “Revise estimation of ...of” I suggest to change to “Estimates of ...” This comment is also relevant to the main text, as it is not clear which estimates have been revised in the submitted manuscript. Please add literals a,b,c to the respective panels.

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