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Comment

Interactive comment on “High Resolution 3-D temperature and salinity fields derived from in situ and satellite observations” by S. Guinehut et al.

Anonymous Referee #1

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The authors provide a comprehensive description of a method that combines satellite sea-level anomalies (SLAs) from altimetry, satellite sea-surface temperature (SST) and Argo temperature and salinity profiles to produce gridded fields of temperature and salinity for the period 1993-2009. The technical description is thorough. The choice of fields shown to demonstrate the strengths and limitations of the method are insightful and will be of value to the broader research community. I suggest a few areas that I think this paper could be improved, but the paper is very good in its current state. I recommend this paper be accepted for publication.

What is the minimum expected RMS error for the reconstructed fields?

The authors evaluate their product by comparing to independent, with-held Argo profiles. This is a suitable strategy and demonstrates well the improvement from the back-

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ground field (climatology) to the synthetic profiles (using SLA and SST projections) and finally the combined fields (adding Argo to the synthetic profiles). See Figure 7 as an example. The RMS (and mean) errors presented in Figures 5-8 assume that the independent profiles are error free. However, the profiles used for this evaluation contain error associated with limitations of the instruments (this is very small – negligible for the purposes here) plus representation error (there are several different names for this component of observation error used in the literature). Representation error is the part of the true signal that cannot be represented on the chosen grid – the sub grid-scale signal. This will be large in some places (e.g., where mesoscale variability is significant) and zero in some places. I suggest that the representation error is the minimum expected RMS error. The authors have apparently generated explicit estimates of the representation error that they use for the second step of the method they describe. I suggest that they consider included these estimates, as an expected lower bound in Figure 5-8. This will help determine whether the products they produce are as good as could be expected – or could be improved if their mapping procedure or the inputs to it (e.g., the regression coefficients) were improved.

I recognize that the estimates of representation error are not well known. But the authors apparently believe them enough to use them for this gridding procedure – so they evidently have some confidence in them.

If the authors were to be more precise, they should not describe the fields showed in Figures 5-8 (and elsewhere) as errors – they are really differences between the gridded products and independent observations. They might more accurately refer to them as differences.

Have the authors attempted to apply error propagation techniques to produce expected analysis errors for the synthetic profiles? Similarly, the OI mapping used in the second step of the method can presumably produce estimates of analysis errors. How do these compare to the RMS error/difference fields they present here?

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How many profiles are used in each 1 degree bin to compute the regression coefficients for the vertical projection? I presume the regression coefficients in the North Atlantic are better defined because of the denser Argo coverage for a longer period – compared to the Southern Ocean for example. Do you see poorer performance of this method where there was less data to “train” the regression coefficients?

Minor comments

There are several places in the paper where the English could be improved (e.g., page 1314, line 2; “that combines efficiently the main. . .” should be “that efficiently combines the main . . .”; and many other places in the paper). The reader can easily understand what is meant, but the authors might consider addressing this.

On page 1321 the authors refer to a “tong” a few times. Do you mean “tongue”?

With reference to Figure 2, showing correlation coefficients between dynamic height and temperature, etc, the authors interpret correlations that are quite small (e.g., - 0.2). What is the significance of these correlations? Given the number of data used to construct these correlations, the authors should be able to estimate the level of correlation, below which, the estimate is indistinguishable from zero. If the amplitude of a correlation is smaller than this significance level, there is no point interpreting even the sign of the correlation.

There is a slight inconsistency in the text on page 1326 with the text on page 1324. On page 1324, the authors say that in the synthetic profiles the “mean error is almost zero” (line 8). On page 1326, they say that “the mean error that was present in the synthetic temperature fields has now disappeared” (line 7).

Where can data from this product be accessed? Is it publicly available?

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