Ocean Sci. Discuss., 7, C356–C358, 2010
www.ocean-sci-discuss.net/7/C356/2010/

Ocean Science
Discussions

Interactive comment on "A global comparison of Argo and satellite altimetry observations" *by* A.-L. Dhomps et al.

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Received and published: 3 August 2010

This manuscript presents a comparison between sea level anomalies (SLA) deduced from altimetry measurements and dynamic height anomalies (DHA) calculated from temperature and salinity profiles gathered by Argo floats.

The work builds upon a previous paper from the same group (Guinehut et al., 2006, hereafter GLL06) which was comparing altimetry-derived SLA to DHA relative to 700 m depth and computed from hydrographic profiles. As salinity was missing for 75% of the profiles, salinity was inferred from the climatology. Using data from Argo floats unable to use a deeper reference level 1000 m instead of 700m, to use measured salinity and not inferred salinity and to take advantage of a more complete spatial and temporal coverage.

The authors provide a sensitivity study comparing different reference levels: 700, 1000 and 1500 m. They also show that using measured salinity rather than computed salinity improves significantly the correlations between DHA and SLA.

In summary they found that using Argo floats improved the global SLA/DHA consistency.

This first part of the manuscript is straightforward and well written.

In the second part of the manuscript the authors take advantage of the temporal coverage of Argo floats to examine SLA/DHA consistency for the mesoscale time scales ($<$ 200 days) and for longer time scales ($>$ 200 days). They examine/quantify the role of the vertical structure of the ocean on the DHA. Figure 4 and 5 are interesting. Staring with section 4, nice figures are presented with little interpretation and comparison with other works. The writing becomes confusing. Section 6 for example was hastily written and is particularly difficult to follow. What is really new compared to other studies?

A map with the number of Argo profiles used should be shown, globally, per year (necessary for Fig. 7) and per month. The number of floats may be a limiting factor in the interpretation of the DHA/SLA correlation and should be discussed properly.

This work shows potentially interesting results particularly separating intraseasonal from seasonal and interannual , which however need a more careful interpretation. Particularly, the authors should insist on what is new compared to previous works.

For example figure 6 is compared to a similar one in GLL06. What is new is not developed.

Figure 7 is hardly described. Sections 6 and 7 should be rewritten (the abstract as well) and insist on what is new.

The part of the paper devoted to the interpretation and discussion should be expanded.

In summary, I suggest that this manuscript be accepted with revisions.

Interactive comment on Ocean Sci. Discuss., 7, 995, 2010.