Interactive comment on "Analyses of altimetry errors using Argo and GRACE data" *by* J.-F. Legeais et al.

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While there is nothing particularly wrong with this manuscript, I suggest it be returned to the authors for a major revision. This is due primarily to the structure of the paper, some issues with the descriptions of the methods and data which make it difficult for a reader to follow what is going on, and some minor English grammar issues that should be reviewed.

My major comments are below, followed by some more minor criticisms.

1. Much of section 3 is confusing, as most of the results are shown later in Section 5. In fact, the authors do a far superior job in Section 5 of: 1) discussing the multiple problems of comparing altimetry and Argo data, and 2) quantifying the uncertainties and errors introduced. Also, many parts of Section 3 are repeated. As such, Section

3 is ultimately redundant. Therefore, I suggest it be deleted, and the last paragraph of Section 1 (Introduction) clearly state the authors are going to quantify the issues with comparing altimetry and Argo data, which has not been done previously (or at least in this detail).

2. Much of section 4 is not particularly new, and just repeats analysis already published by the authors in slightly different forms. I believe they include this here as a motivation for the study, but in my opinion, a brief summary in the Introduction with references to the work (which is already done) is sufficient. Including this section distracts from the important new aspects of the study, which is Section 5.

3. Thus, by deleting Section 3 and 4, and moving Section 5 up to Section 3, the authors introduce the most important aspects of the paper as soon as possible. Any portions of the old Section 3 and 4 that are not already discussed in Section 5 can be included here.

4. Section 2.3 GRACE. I found the explanation of why the mass estimates from GRACE are needed was quite muddled, and I'm an expert and know why. I doubt a novice to the problem reading this would understand. Please rephrase this section to make it clearer. Basically: 1) altimetry measures total sea level, 2) Argo measures upper ocean steric. But there is an additional signal, mass redistribution in the ocean. It's smaller than the steric (except globally), but can be important in some regions. There have been lots of papers dealing with this and can be referenced here [those by Chambers et al., or Ponte et al are good ones], so that a reader can get more detail.

"as proposed by the University of South Florida" would be better as "as provided. . ." or "as estimated by. . ." because proposed means "suggested" and doesn't give the impression it's been calculated.

Also, it's not clear here if the GRGS maps include the time-variable global mean mass or not. The mean mass variation does contribute some to reducing regional residuals. Please clarify this.

5. I have Major concerns with Figure 9 and discussion. "The GRGS dataset has been adjusted for a -1.1 mm/yr GIA effect whereas this effect is already taken into account in the global mean ocean mass time series." The 1.1 mm/year number is a global number and is specific to the averaging kernel used (Chambers et al., 2010). More over, the authors state a few sentences later: "GRGS solution collocated to Argo profiles versus global mean difference) but it is not believed to be a first order contribution to the error"

Thus, the GIA correction WILL be different. I also don't believe sampling GRGS OBP to Argo grids will not create first order difference. I suspect this may explain the inter-annual differences, and the trend. It's easy to check – just do the same calculation with the average of the GRGS grids over the ocean (with a 300 km mask). That should be most comparable to the estimate of Johnson and Chambers. If not, it indicates more problems.

6. Please include fit uncertainty estimates on all trends (and not just formal error – make sure to scale covariance matrix by the variance of the residuals), and tell the reader whether it is standard error, 90% confidence, etc. I understand the authors are waiting until the end to quantify more systematic errors, but when they discuss trends and drifts earlier, the reader needs to know what the fit error is to understand if differences are really meaningful or not.

Minor Comments:

1. Problems with using articles properly in English: For example, here "the" is used too many times. [delete] (add) "Since the early 1990s, several satellite missions have been equipped with altimeters allowing the estimation of Sea Level Anomalies (SLA) and the monitoring of [the] Mean Sea Level (MSL). This contributes to understand(ing) the role of the ocean in the Earth system and to assess the link with [the] global climate change".

One example. The whole manuscript needs a thorough copy-edit by native English speaker.

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2. "thermohaline" is misused throughout context. In Oceanography, thermohaline is ONLY used with the vertical circulation related to density differences. While these are caused by temperature and salinity variatiations, it is NOT analogous to expansion/contraction. That's thermosteric/halosteric, or steric for the combined effect. Please change throughout to avoid confusing readers.

3. Section 5.7. I don't quite follow some of this statement. "...the deeper the reference level, the more information from the T/S profiles is taken into account through the water column but the more T/S profiles are not used (those who don't reach the reference level)."

I think what is meant here is that not all Argo floats reach the same level (older ones only went to 1000 dbar). If one selects only the deepest reference floats, one has a reduced number. But using all means a mixture of depths and some loss of sampling of steric variations between 1000 dbar and 2000.

Please reword this so a reader can understand. I think the problem was the authors tried to compress too much info into a single sentence.

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