

Interactive comment on “A new assessment of global mean sea level from altimeters highlights a reduction of global trend from 2005 to 2008” by M. Ablain et al.

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I was asked to review this paper in response to the comments by Reviewer # 2. I generally agree with his/her comments, and feel that this paper needs some revision especially when it comes to referring to other, older papers. While the main point of this paper (the analysis of errors on MSL trend estimates from altimetry) is important, the presentation of a few of the error sources is confusing, and as Reviewer # 2 pointed out, the final tally is presented in a very confusing way (is it 0.9 mm/yr? Is it 0.5 mm/year? Is it 0.6 mm/yr?) I think some additional work is need to clarify these.

Major Comments

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1. Reviewer # 2 is absolutely correct that some important references are missing. First of all, the authors act as if this observation of interannual anomalies in GMSL associated with ENSO is a novel result, whereas it has been documented before in Nerem et al. [GRL, 1998], Chambers et al. [JGR, 2002], and several papers by Cazenave et al. These references should all be discussed in light of the recent several year change, with some comments that this is not unexpected in the record.

2. On page 38, Lines 25, I agree with Reviewer # 2 that the statement: "This 1mm/yr trend differences cannot be explained by a physical process.: is a little strong. This assumes that global mean sea level rise is uniformly the same over the global ocean, and we know for a fact this is not true. There are long-term changes in the ocean heating and circulation that is different from one ocean basin to another and from one hemisphere to another. We really do not know what the size of these local changes can be, but a difference of ± 1 mm/year is not necessarily unreasonable. It does seem that using a new reference frame makes the trends more uniform, but how well do we know the reference frame?

3. Section 4.4 is very confusing, mainly because relative bias estimates are so dependent on the sea-state bias model used. I think this section needs to be re-written and clarified. I agree with Reviewer # 2 that Chambers et al. [JGR, 2003] should be discussed here. While it is not the SSB model you are using, it does a good job of at least describing the problem and the effect of changing the SSB model on GMSL. Then you can discuss the newer SSB models you use, and the apparent uncertainty in the bias estimate. I do think that your estimates of the bias uncertainty is a little high for the TOPEX-A/B switch. For example, Mitchum has computed this in his tide gauge analysis (after fixing the SSB to the Chambers model) and finds a uncertainty of less than 1 mm because of the long time-spans before and after the switch. I don't really follow your reasoning that it is up to 2 mm. Please explain.

4. Summation of errors. This is where it gets confusing to the reader. Basically, as I follow it, you assume a maximum and minimum error, ignore that some of these may be

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negatively correlated, and sum them up. If this is so, please state this. After this, I do not understand how you reduced this to 0.6 mm/year based on the "mathematic formalism" of Bretherton et al., 1976. Please summarize what assumptions are made about the individual components to reduce the value. Finally, you estimate drift by comparing to tide gauges in the next section. Again, I agree with Reviewer # 2 that there are problems with this assessment and some things need to be clarified. First of all, why are the curves offset in Figure 8 if a relative bias has been applied? Is this artificial, or is there some residual bias in your results. If you have offset them artificially, please state this in the caption; otherwise, please explain the bias. Also, you mention later that the standard deviation of the tide gauge-altimeter global residuals is 7 cm, which is quite large. Mitchum generally gets residuals closer to 1 cm RMS. It also does not look like your residuals in Figure 8 have a 7 cm RMS. Finally, I am a little surprised that your drift in the TOPEX residuals is so high compared to that in Beckley et al. (or Leuliette et al). These both relied on an analysis by Mitchum and the Chambers TOPEX SSB model. They both showed only a little drift in the TOPEX-tide gauge residuals. This paper uses a different SSB model that results in a higher apparent TOPEX-A/B bias (of 11 mm), and then leads to a much larger TOPEX drift. This makes me suspect either the SSB models are wrong, or there is a problem with adding the more coastal tide gauges. Please comment.

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