

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.

Anonymous Referee #2

Received and published: 16 January 2009

General comments —————

The paper of M. Ablain et al gives an overview of the observed changes in the global mean sea level as determined from satellite altimeter data. This issue is known for quite some time, in fact, Steve Nerem was the first in my opinion to work on the TOPEX/Poseidon data, and later other altimeter missions. The information in this paper is therefore at best an update where the main conclusion of the authors is that the recent sea level change (1 mm/yr since 2005) appears to be less than what was observed between 1993 and 2008 where they find 3 mm/yr. Yet it should be remarked that the analysis period is maybe too short, and also, that similar short lived doldrums in the

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MSL trend curve appeared after the ENSO of 97/98. My conclusion is that this paper has no significant new results, also, it lacks references to papers that were published before.

Specific comments _____

Line 1-2 on page 33: You can not conclude that sea level change is accelerating on basis of the differences between the rate observed from tide gauges (1.7 mm/yr) and the rate observed from satellite altimetry which comes to around 3.4 mm/yr. First you should demonstrate that you exclude spatial sampling differences of the sea level signal which are very significant for this problem. Albain et al have not shown this, in fact, nobody has shown this, the MSL accelerating claim is misleading and easily misunderstood.

Line 12-15 on page 33: The activities of Remko Scharroo (which you cite in this article) have indicated that you can combine Envisat and GFO. Perhaps you should better explain what good confidence really means.

Line 16-21 on page 33: It would be very appropriate here to mention that several papers already discuss this issue these tests, where are the references?

Line 2 on page 34: perhaps rectangular should be replaced by spherical?

Line 24 on page 34: CGM02C should read GGM02C.

Line 7 -9 on page 35: This conclusion is rather basic (you find it in many textbooks), but it is perhaps also too basic. GIA is a signal with a spatial structure, you can't just assign a global constant for this correction. Did you re-compute the MSL rate with the help of a GIA model, or did you just assign the 0.3 mm/yr correction to the graph?

Line 24-27 on page 38: What is the reason to exclude the possibility of a geophysical feature affecting the hemispherical difference?

Line 4-6 on page 39: Did you exclude the possibility that the ionospheric correction

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might be to blame for this effect? (Ascending and descending tracks occur at different solar times)

Line 13-14 on page 39: could you please explain how you come to this 0.1 to 0.15 mm/yr

Line 16-17 on page 39: This issue was already studied by Chambers D.P, Hayes S.A., Ries J.C., Urban T.J., New TOPEX sea state bias models and their effect on global mean sea level, JGR Oceans, Volume 108, Issue 10, 15 October 2003.

Line 27+28 in page 39 and line 1+2 on page 40: But what is now new with this conclusion, what is different compared to the work of Chambers et al 2003?

Line 11 page 40: Is the 1.6 m/s/yr correct? are these units right?

Line 20-22 on page 40: your total error is twice as large as the value we see from Steve Nerem's activity.

Line 14 on page 41: replace "Altimeter MSL drift" by "the instrumental drift of the altimeter system"

Line 25-27 on page 42: How is it possible that a difference measurement of about 7cm rms can be used to conclude that an altimeter drift is of the order of a few tenth mm/yr. This issue should be explained. There should be a lot of ocean dynamics in this signal.

Line 1+2 on page 43: You should mention that it is assumed that the use of long time series reduces the impact of the uncertainty on the trend estimation. This is an assumption based on statistics.

Page 47: What load love numbers were used in the ocean tide loading computation?

Figure 5 on page 53: is the offset between the curves deliberate?

Check list:

1) Does the paper address relevant scientific questions within the scope of OS?

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The problem of determining a MSL trend is a relevant activity.

2) Does the paper present novel concepts, ideas, tools, or data?

This paper has some novel ideas but the impact is limited.

3) Are substantial conclusions reached?

Not really.

4) Are the scientific methods and assumptions valid and clearly outlined?

Yes

5) Are the results sufficient to support the interpretations and conclusions?

Yes

6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Yes

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Some references are missing.

8) Does the title clearly reflect the contents of the paper?

Yes

9) Does the abstract provide a concise and complete summary?

Yes

10) Is the overall presentation well structured and clear?

Yes

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11) Is the language fluent and precise?

yes

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

This is a paper without math. One abbreviation was corrected.

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

No I don't think so.

14) Are the number and quality of references appropriate?

Yes

15) Is the amount and quality of supplementary material appropriate?

N/A

While we ask you to take into account all of the above aspects, it is not necessary to explicitly address each of them in the Referee Comments.

Well I just did this.

End of review

Interactive comment on Ocean Sci. Discuss., 6, 31, 2009.

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