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Interactive comment on "Reduction of the 59-day error signal in the Mean Sea Level derived from TOPEX/Poseidon, Jason-1 and Jason-2 data with the latest FES and GOT ocean tide models" *by* Lionel Zawadzki et al.

Anonymous Referee #1

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Review: "Reduction of the 59-day error signal in the mean sea level derived from TOPEX/Poseidon, Jason-1 and Jason-2 data with the latest FES and GOT ocean tide models" by Lionel Zawadzki et al.

During the review of this article I noted a number of issues. Clearly the native language of the author is not English, and I recommend that this will be improved prior to publication.

There are also style issues, TOPEX, TOPEX/Poseidon are used intermittently, why not simply one acronym, like T/P.

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The discussion that the results of the authors depend on the used version of the GDRs (see for instance lines 20 to 22 on page 2) makes this article a somewhere technical document and not a science paper. It is a pity that the authors have chosen for this approach.

There are several vague statements in this paper, an example is page 2 line 27-28, and later lines 29 to 31. What does the author exactly mean? It only becomes clear as we read the article completely. On line 25 the authors mention section 0, but there is no section 0, this error is persistent in the manuscript.

Line 1 on page 2 is a repetition from earlier text, this makes the article uninteresting to read. Avoid repetitions.

Line 5 on page 2 says that POD is accurate up to the 1-cm level. Probably this assessment is too optimistic, I think that 15 mm is more reasonable, and for T/P I think that the errors are probably larger. This statement could be better quantified.

Line 10-25 on page 3 indicate that there is a beta prime angle and that it has a periodicity of 59 days in the T/P and Jason-1/2 orbits. This is the aliasing period of the S2 tide. From this point on the paper lists numerous effects that may explain a 59 day cycle in the altimeter data records. On line 24-25 the authors make no attempt to interpret the papers of Lemoine and Cerri which are presentations at a workshop. This sort of referencing is not acceptable in a scientific paper.

At the end of their list, we are now at lines 1 and 2 on page 5, the reader might wonder which of the possible options the authors are going to check in their paper. The statement assumes that tide models may be to blame, but this statement is not necessarily true or accurate.

On lines 4 to 6 on page 7 the difference is made between de GOT and the FES family of ocean tide models and the handling of atmospheric pressure related corrections on the altimeter data record which manifests itself in the dry tropospheric correction, but also the inverse barometer correction and last but not least, air tides. There is no clear explanation why the 151 BPRs are more sensitive.

Table 1 on page 7 is a helpful addition to the manuscript.

Page 8, line 3, it is not clear why we speak about ordinary least squares. Why not simply: least squares as we find it in the textbooks.

Page 8 lines 17-22 : the described procedure is identical to the procedure resulting in the GOT ocean tide models. In other words, I do not really understand why the authors find a residual signal. This could in my opinion only be due to long periodic modulation about the tidal frequencies that was captured in another way between the GOT ocean modeling period and the time period used by the authors. This is not thoroughly explained in this manuscript.

Pages 8 lines 27-29 : I also see that the authors make use of interpolated MSL grids which means that also the sampling differences play a role in their results. If you want to say something about the GOT model accuracy then I believe you should at least take an effort to adopt to the same processing methodology, otherwise you should test the efficiency of sampling and processing which has not been implemented by the authors.

Figure 1 caption page 10: If there are differences between the satellites and the different tide models then the author should try to pinpoint why these differences occur. It may be due to a number of reasons that were mentioned, actually, it may be due to the orbit modeling, but it may also be related to the quality of for instance meteorological information such as the surface pressure which is responsible for the IB correction. Last but not least, the altimeter itself may very well play a role.

Page 10, lines 5 to 22 : this seems to me like a product comparison discussion but I see no scientific analysis to pinpoint the cause of the differences. This remarks relates to what we see in figure 1.

Page 11, figure 2 : It is obvious that there are differences, but, these differences seem

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to be related to mesoscale variability and ENSO phenomena, it is a little bit surprising that this ends up in different ways in the various satellite tide model combinations. Fundamentally however there is no explanation provided by the authors.

Section 5.1 Methods : what is not mentioned is that the FES solutions are based on the assimilation of crossover data because the used representer method is not capable of handling an excessive amount of datapoints for which crossover points are used. The GOT solutions do however assimilate more altimeter data, in principle these are collinear data, and this should be acknowledged when you compare both approaches.

Page 14, lines 16- 18: I do not understand how the authors come to this conclusion, could they please elaborate on what they have tested for J1/J2? This could be rather important information.

Figure 6 on page 15 : It would be good to carefully explain what these differences exactly represent

Page 16, lines 17-19 : I'm not sure what you precisely want to say. A hydrodynamical model for S2 still has to provide radiative forcing to get any accuracy. I find it a little bit surprising that this paper does not continue with the conclusions presented in (Ray 2009) where it is said that 10 to 20% of the S2 tide comes from the radiational forcing,

http://onlinelibrary.wiley.com/doi/10.1029/2009GL040217/full

How large is the variation in the air tide to explain the differences that you get to see in the data here. The most prominent thing however is that the results are satellite and tide model specific.

Page 16 line 23-24 : It may very well be the T/P orbit in this case, but how are the authors going to demonstrate this? Perhaps from overlapping ground tracks between Jason-1 and T/P, this could be a very specific test to assess the quality.The test procedures is not part of this manuscript.

Page 16 line 27-28, the statement that the 59-day error is clearly linked to the inaccurate account of the beta prima angle is a) unclear, the reader does not precisely understand what is meant, also, b) if it is related to orbit modeling then this has not been demonstrated in the paper.

Page 17, references, there are many presentations from what is called grey literature, this is not acceptable for a scientific paper. This part alone is sufficient to ask for major revisions. What we should see is a list of peer reviewed articles and perhaps some technical reports.

I recommend rejection because there are too many issues with this paper, I think the author(s) is(are) better of to resubmit a new paper.

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