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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.

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This paper examines the reduction of a well known 59-day error signal seen in GMSL from altimetry when using newer ocean tide models. At the moment, I am torn between "reject and encourage resubmission" or "major revisions." I have opted for "major revisions" because I feel most of the issue with this paper could be handled with some small reanalysis and change in the focus of the paper.

I have also read the other review, and agree with many of his/her criticisms, especially regarding the English usage and grammar. Many of the sentences/paragraphs could

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be revised to improve the readability of the document. If the American co-authors (Ray and Zelensky) can't do this, then perhaps an editing service could be utilized?

Regarding the content, my main concern is the main gist of the paper, which is also reflected in the title – that the error can be removed using the newer tide models. While in one respect, this is true, the problem is that the tide models themselves have been corrupted by the error, thus potentially reducing their effectiveness for tidal corrections for a whole host of data. While most of this is most likely due to TOPEX errors (as nicely demonstrated by the GOT experiments) it is not entirely clear to me that Jason errors will also not contaminate the tide models. The latest GOT model (for instance) fits Jason much better at the 59-day period, but does this mean it is better? Not necessarily.

The BPR data might provide insight on this. As is, Table 1 and the discussion looks at the RMS for all frequencies. How does the 59-day period look?

Secondly, the regional analysis is not very convincing, because of the large formal error (due to high SSH variability in many regions, as Reviewer # 1 noted). Most of the amplitudes are below the formal error. What is the SNR? Unless it is > 1 everywhere, I'm not convinced by any of this analysis. Also, why look only at amplitude? Why not phase? That could especially highlight differences between the TOPEX and Jason altimeters.

In short, I think the authors have a paper that can be salvaged with a little better focus and some more analysis:

1. Be clear in the title and throughout the paper, that the tide models are not really removing the error, they have likely absorbed it, and may in fact be biased at that 59-day period.

2. Better quantify the differences between TOPEX and Jason 59-day errors. Some of this may be in the grey literature references, but really should be in this paper, the first

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publication I know of to really look at this to such an extent.

3. Look at the BPR residuals in terms of the 59-day period.

4. Revise the regional analysis to consider the size of the error.

Finally, regarding the Scientific Significance of the paper. Even if all these revisions are made, I don't think the Scientific Significance would increase considerably from my ranking of "fair." This is a small error, and does not adversely affect using altimetry to study climate signals or ocean dynamics. It is interesting from a technical aspect, but this paper still does not fully explain the source of error or why it is different between TOPEX and Jason satellites. IMO, the paper really isn't relevant for Ocean Science, but might be better suited for a more technical/engineering journal – perhaps one of the IEEE journals. But I leave that to the editor.

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