(bold: authors comments, non-bold: reviewer comments)

To the editor:

Through communications with users and internally, we had noted an inconsistency between our version of the model and that maintained on SEANOE. This was the result of a conversion error in the NETCDF file formatting that was not noticed and resulted in a small offset in the downloadable model. It is important to note, that this has been rectified and the issues have no bearing on the results of this manuscript. The updated files as well as the previous error-version of the files are still available on SEANOE.

Suggestions for revision

Most of the changes made by the authors are good, and I have no problem with most. Let me address several points, based on their replies and changes, in which I have disagreements. These disagreements are not critical, except for the final one, but I want to note them here, because the authors perhaps missed what I was attempting to say.

We would firstly like to thank the reviewer for the additional input and we appreciate the effort being done to improve this manuscript as well as helping with ideas for future iterations of the model.

Ionosphere. My point was that the use of the NIC09 model in the model testing favored the EOT20 model. The authors disagree, saying "a different ionospheric correction will not have an influence on the sea level variance reductions." I disagree. Let us assume that NIC09 model is inferior to the Jason dual-frequency measurement (likely true). That means the errors in NIC09 were partly absorbed into the EOT20 tide solutions, presumably mostly in S2 wave. Yet when the model tests are run, that error in EOT20 then compensates for the error in NIC09, resulting in a reduced sea-level variance. The FES2014 model would not have absorbed the NIC09 error, so it would result in a larger sea level variance. Thus, the test favors EOT20. Nevertheless, the effect may be small, so I won't insist that the test be redone. (But perhaps the effect is not so small?)

We agree with the reviewer on the importance of the ionospheric correction. In order to evaluate the influence of the correction in the sea level variance analysis done in this publication, we did a 'quick' sea level variance analysis for Jason-3 using the dual-frequency measurement correction together with the FES2014 and EOT20 model.

The mean variance reduction of EOT20 compared to FES2014 is 0.82 mm when using the Jason dual-frequency measurement (compared to 0.89 mm when using the NIC09 correction), with the mapped differences showing similar results to what was seen in the previous figure for Jason-3 (for example the higher variance reduction of EOT20 around

62-66 S). Overall, the variances themselves are smaller using this ionospheric correction, hence showing ‘relatively’ larger scaled sea level variance differences.

So the reviewer is in fact correct that there is an effect of the ionospheric correction visible in SLA reduction. However, the effect is small (0.07 mm) and has no impact on the conclusion of the variance reduction analysis and the assessment of EOT20.

Load tides. I do not understand the logic of the authors' reply on this point. The reply seems to say that the load tide over land should equal the FES2014 load tide, which cannot be, since any change in the ocean tide results in a change in the load tide (everywhere in principle). But it doesn't really matter, since the load tides in the EOT20 data archive are undefined on land. (It just means that the GNSS community cannot use these published load tide maps.)

I think there is a misunderstanding here. Of course, we did not question the fact that continental load tides are influenced by ocean tides. Our point was that EOT20 is only containing information over ocean areas (for the ocean as well as for load tides). Thus, we also don’t plot this.

Reference for alias periods. I had noted that the original Smith (1999) reference was incomplete -- e.g., it had no journal name. I also stated that perhaps a review paper would be a better reference, since the alias periods had been reported in earlier studies before Smith (1999). Instead, we now have two obscure departmental reports from Ohio State University! It isn't very important, but I had been thinking of reviews such as C. Le Provost's chapter in the 2001 book "Satellite Altimetry and Earth Sciences". Even Carl Wunsch's recent physical oceanography textbook could be used. But it's the authors' choice.

We have changed it to be Le Provost’s chapter:

Le Provost, C.: Chapter 6 Ocean Tides, in: Satellite Altimetry and Earth Sciences, edited by Fu, L.-L. and Cazenave, A., vol. 69 of International Geophysics, pp. 267–303, Academic Press, https://doi.org/https://doi.org/10.1016/S0074-6142(01)80151-0, 2001.

Tide gauge statistics. This is my one major remaining concern. The tide gauge RMS statistics do not appear correct to me. I'll separate the comment into 3 parts:

(a) Apparently both the EOT20 authors and Stammer et al. (2014) are computing a tidal constituent's RMS difference between model and tide gauge by integrating in time over a tidal cycle. The authors claim they aren't, but their Eq (8) is exactly that. [Note their Reply differs by a factor of sqrt(2)]. (It isn't necessary to cite Piccioni et al. (2018) for Eq (8), as it is a standard result since the integration over the trigonometric functions can be worked out analytically.) But Eq (8) is for a single tide gauge, and perhaps the summation over all gauges is the problem?

The computation done by Stammer et al. isn't clear on this point, because they used a weird notation with an overbar that evidently stands for two different kinds of averaging. Therefore, I checked with the relevant coauthors of that paper, and I am now fairly certain the Stammer RMS calculation (for a single constituent) was as follows (Latex format, and in the authors' notation):

\begin{equation}
\left[(2N)^{-1} \sum_{k=1}^N
\left((A_{M,k}\cos P_{M,k} - A_{T,k}\cos P_{T,k})^2 +
(A_{M,k}\sin P_{M,k} - A_{T,k}\sin P_{T,k})^2 \right) \right]^{1/2}
\end{equation}

for N tide gauges. Is that what is used here? If not, perhaps that would explain why the RMS values here differ. In any event, the new text seems incorrect on these points.

(b) The differences with Stammer et al. are important, and it isn't true (as stated in the revised paper) that the "relative results of the models compared to tide gauges is the same". Referring to Figure 5, we see EOT11a shelf RSS values are inflated by about 50% (about 7 cm in Stammer versus 11 cm here), but GOT4.8 values are inflated by over 100% (about 6.5 cm versus 14 cm). Perhaps there is something else different than the maths. How were the model values interpolated to the tide gauge locations? If this was done inaccurately that could explain some differences. (Interpolation may be more critical for GOT4.8 because the grid interval is larger.)

(c) Figure 5 (top) shows the RSS shelf values are much worse than the coastal values. This also contradicts Stammer et al. In fact, Figure 5 (bottom) seems more realistic in this regard, with coastal stations worse. In the top panel, the EOT20 value for coastal gauges is even lower than the value for the open ocean gauges, which seems very suspect. I think these calculations need to be rechecked.

We would like to thank the reviewer for this comment as well as the clarifications with the authors of Stammer et al regarding the RMS estimation. In fact, we have now implemented this into our RMS calculation in order to be consistent with Stammer et al 2014 and we can clearly see results similar to what was seen in Stammer et al for the EOT11a and GOT tide models.

Furthermore, we reached out to Richard Ray as we were still unable to match the results of the Shelf Sea datasets. The Shelf Sea dataset itself does not contain data for every major tidal constituent at each ocean bottom pressure station, i.e. for some only four major tides are estimated. Ray discussed that the RMS was calculated for individual tides for the observations that were available, with the overall RMS's being based on a different number of tide gauges for each constituent, a problem that does not occur in the open ocean and coastal region. We had only looked at the ocean bottom pressure data that contained data for all 8 major tides. This handling was not discussed in Stammer et al, but thanks to the clarification from Richard Ray we are now following the same approach. However, we only use observations where there is data for at least five constituents, which results in the dropping of five bottom pressure sensors, hence why

the results of GOT and EOT11 look ‘slightly’ better in our comparison compared to Stammer et al in the Shelf Seas.

Based on this analysis and the comments of the reviewer, we have changed this result within the text as well as updated Table 3 with the changed RMS results as well as Figure 5 and Appendix A3.

Changes have been made to the text based on changing the numbers and adding a few more explanations, from lines 207 - 254.