1 Response to editor minor revisions

Thank-you Richard. I've addressed all you comments below,

Topic Editor Decision: Publish subject to minor revisions (review by editor) (15 Aug 2018) by Richard Ray Comments to the Author: Dear Jo & colleagues:

Co-editor Phil Woodworth and I have both gone through the revised paper and compared reviewer comments and replies. Before final acceptance, there are a few items that need to be addressed. Most are very minor.

The paper is now much clearer, with the new notation being especially helpful. In fact, I recommend that you add somewhere the point you make in the Replies, that a tilde denotes a harmonic prediction – otherwise, a reader might fail to notice that helpful guide.

done

Given that I now understand the paper far better than I initially did, I have a couple of more substantial points (or puzzlements).

1) Page 5, line 20 and surrounding: You express mild surprise that tide+surge has large perturbations of Mm and Mf (and also MSf, although you note that could be from M2-S2 interaction). One possible explanation that occurs to me is the following: With atmospheric forcing, the sea level response turns very red, putting lots of non-tidal power into periods that will get soaked up in Mm and Mf estimates. This is why reliable tide-gauge estimates of Mf,Mm almost always require a many-year time series.

I'm actually far more surprised by the large effects at M2.

Yes. I've added a note that: " Another possibility is that non-tidal power in the tide-and-surge model is leaking into $\mathbf{M}_\mathrm{m}$ and $\mathbf{M}_\mathrm{f}$ estimates. Eliminating this would require a many-year model run."

I think the M2 change comes from the consistent bias due to surges slightly altering the tide in the same way in a given location and pushing a bit of energy into one of the shallow water terms. I'm not absolutely sure though. We had some nice model results from another project that might help clear it up - details may appear in another paper!

2) I am puzzled why the tide prediction errors in Fig 1b are so large. This is just $\tilde{M}_t$ being computed from harmonics directly estimated from M_t and yet 62 constituents fail to capture so much of the signal?? And by my eye, for some locations, the error appears to be fortnightly (i.e. spring-neap periodicity) and more or less constant throughout the year.

I'm assuming that the 62 harmonic constants were computed for the 2013 timespan, identical to the timespan used for the figure. True? Or are the harmonics computed from a different timespan, as in Flowerdew et al (2010)? If the latter, I can understand this more readily.

In any event, it would be interesting to understand how these errors arise. For some location showing this large error (perhaps 10 cm?), it may be enlightening to examine in more detail why the 62 harmonics leave so much error.

You may argue that such additional research is beyond the scope of the present paper, and I accept that. In fact, it would likely require more than a single year of data to understand what is going on. However, understanding

this error should be important to your group. So I think further analysis is warranted at some point.

You are correct that the harmonics are derived from the same period, and yes there are sites where 62 do not represent even the tide-only model well. It's better when you go to the 115 list.

Eg around Avonmouth, with 62 and 115 constituents:

This is something I absolutely do want investigate in more detail - to understand better how to select constituents around the world, when it is necessary to use a very large number, and how the errors are affected by the length/density/noise characteristics of the time series. But I think we have to leave it for another paper.

Edited the paragraph describing that figure to include: "In practice $\Delta <$ 5 cm at most UK sites and the monthly cycle has gone, but in the Bristol Channel there is still an error of around 50 cm, indicating that the 62 harmonic constituents are not capturing all of the model tide, and further shallow-water constituents may be required."

3) Page 4, line 1: with the change in notation, W should be G ?

Yes. Corrected.

More minor points: 4) Page 6, with the change in notation, do you still want to use H(t) for this section?

No, that should be $\tilde{G}(t)$. Corrected.

5) Section 3.1: Minor point, but not only does ECMWF contain the S2 air tide, but it's an incorrect representation of it. This is because 6-h sampling is the S2 Nyquist, so ECMWF cannot capture the full S2 signal. The point is made in papers on air tides, although not very germane in the present context, so you can mention or not as you see fit.

Added:

" The 6-hour sampling prevents ERA-Interim forcing from capturing the $\mathbf{S}_2$ atmospheric tide correctly [Dobslaw2005], but the analysis in this paper is self-consistent with the forcing used."

6) Figure 3, right panels. I cannot distinguish the red lines. Are they BOTH supposed to be dashed? Compare to left panels.

Corrected. I've also tidied up this figure so it'll fit in single column.

7) Page 3, line 2. New text: "we replace the shelf model". WHAT shelf model? None has previously been mentioned up to this point, other than those of other groups.

It was in the previous paragraph. Slightly rephrased to tie better.

8) The figure legibility is mostly better now. I see you enlarged fonts in Fig 6b, but not 6a. Moreover, the color scale is cut off at the high end on both panels.

Edited.

Phil Woodworth also has the following comments. Note especially his point about the 115 constituents, which if he (and I) understand the paper correctly would allow you to reduce somewhat the very large Table B1.

p3, 6 - define ECMWF acronym

done

21 - maybe I have missed something, but she has no real tide gauge data, only the 1 year and 1 month GSTM data sets mentioned in lines 11-12. So what is the '115 for more than one year' relevant for here? I could not see a mention of using this set in the rest of the paper. I had noticed this last time but for some reason it didn't make my list. if this is dropped then it means removing the 115 set from the appendix B table.

I did do some tests that the results detailed at the beginning of section 3 weren't too dependent on the choice of constituents. But in the interests of clarity I've tidied table B1 to only be the 1 year /1 month lists, and added a supplementary spreadsheet with the 115.

p5, 32 - funny that MA2 should change so much and MB2 doesn't. But no matter.

It's only a bit less that the cut-off I chose.

p6, 18 - I would reword this slightly as it is a bit cryptic and I was puzzled what 'this data' meant for a while:

Since the GSTM data used for this was the 1 month set (Jan 2012), this exercise made use of only ...

I don't understand why they used the 1 month set and not the 1 year set for this, all they are doing is deriving some harmonic constants which are then run for 18.6 years

It's because I wanted the full map, and I decided only to save the high-frequency time series every grid point for 1 month because of the amount of data. In hindsight I'd use a sparse grid everywhere for output. There's also the one-year run processed on the coast. Rephrased to clarify.

p14, 3 - GESLA needs referencing, either to http://www.gesla.org and/or the journal reference:

Woodworth, P.L., Hunter, J.R. Marcos, M., Caldwell, P., Menendez, M. and Haigh, I. 2017. Towards a global higher-frequency sea level data set. Geoscience Data Journal, 3, 50-59, doi:10.1002/gdj3.42.

Apologies! Rectified.

p15, figure A1 - this is much better than before and I even understand why the Bering St is where it is in the list now. Could the unphysical 100 and -100 be dropped from the labelling on the y-axis? Doesn't matter much I guess. I thought at first the dashed line at -90 was simply indicating the pole but there isn't one at +90, and there are 3 other dashed lines at Europe latitude. the caption should say what they are.

The dashed lines were left over from an earlier figure explaining the construction but they don't add anything here. Edited.