Many thanks for the kind remarks about the paper. I have included below only the questions from your report.

– I found the second part of the Introduction (after the author sets out the objective) a bit jumpy and incoherent. References to the Cartwright and Platzman papers alongside re-prints of their figures are used to elucidate the main characteristics of the M1 tide. This section could do with some streamlining, e.g., by gluing the individual paragraphs together. Also, lines 13-14 describe symmetry properties of the degree-2 and degree-3 diurnal tidal potential, which are nicely illustrated in Fig. 2 of Ray (2001). It would make sense to aid a reader's imagination and directly refer him/her to Ray's figure.

Reviewer 2 had a similar comment concerning the Introduction. I have swapped around the final paragraphs in that and reworded things slightly so the Introduction should now read more logically. I have given a mention of Ray's Figure 2 as suggested.

– The M1' tide is introduced without any additional explanation in the abstract.

Jo Williams and Reviewer 2 also commented on this. It is defined now.

– The section on the prior tuning of the numerical model for leading degree-2 constituents (M2, K1) lacks some quantitative measures. Which values of horizontal eddy viscosity and bottom drag coefficient were used? Does the model run with a quadratic term for bottom friction or a simple linear parameterization? Instead of stating that the obtained maps for M2 and K1 are acceptable, include a comparison to data-constrained solutions in terms of RMS values and explained variances.

I should have given more details. Model construction, as a shelf model, is described in Flather (Trieste Summer School, 1988). It was run with a 15-second time step and saved sea levels at each grid point every hour (240 steps). Five days spin-up was followed by 14 days to give the values for tidal analysis. It used quadratic bottom friction, and after some experiments, a coefficient of 0.004 was chosen (0.0025 is more normal in 2-D models, see Heaps, 1978 or Pugh and Woodworth, 2014). The horizontal eddy viscosity (A) was depth-dependent with a coefficient AH of 15.0 m/sec (i.e. A=AH*Depth in metres). Values of A are usually taken to be 100-1000 m^2/sec in shelf models (Heaps, 1978), so the value of A used here is much larger than that in deep water but that provides the required 'glue-like' ocean (but with the wrong physics, see the Supplementary Material). I have added this information to the Supplementary Material. References are:

Flather, R.A. 1988. Storm surge modelling. Lecture notes from Ocean Waves and Tides Course, International Centre for Theoretical Physics, Trieste, Italy, sponsored by Proudman Oceanographic Laboratory, September 26 to October 28, 1988. Unpublished document. A pdf copy may be obtained from the present author.

Heaps, N.S. 1978. Linearized vertically-integrated equations for residual circulation in coastal seas. Deutsche Hydrografische Zeitschrift, 31, 147-169, doi:10.1007/BF02224467.

I assume the last sentence of this comment is suggesting a quantitative comparison of the present model in Supplementary Figure 1(c,d) to a state-of-the-art tide model such as that in Supplementary Figure 1(a,b). I did not do that simply because I knew that any quantitative comparison would not be much use. I am very aware that the model used here is crude by modern standards (and even by the standards of decades ago e.g. Accad and Peckeris, 1978), as I explained in the Supplementary Material, but in this case I considered it adequate for the job in hand.

– Something that can be added to the comparison of the model-based M1 chart to the tide gauge estimates: similar to the author's solution for K1 (SI Fig. 1c), the model appears to overestimate amplitudes, particularly in the Southern Ocean/Indian Ocean. I have the strong suspicion that this results from treating Antarctic ice shelves as fully grounded and excluded sub-shelf cavities from the model domain. Check out the paper by Wilmes and Green (2014): Wilmes, S.-B., and Green, J.A.M. (2014), The evolution of tides and tidal dissipation over the last 21,000 years, JGR Oceans, doi:10.1002/2013JC009605.

There is indeed a likely problem with handling Antarctic ice shelves in the model, together with the general uncertainties to do with Antarctic coastlines and bathymetry. I have inserted a mention of Wilmes and Green (2014) as an example of other authors having had similar difficulties, although I noticed that they had more problems with semidiurnals than diurnals.

– page 5, line 2: "the tidal software" – is this the NOC software introduced in Section 2? The first paragraphs of Section 3 were not specific in this regard.

Reviewer 2 also asked about this. I have made it clear now that I used the NOC software.

– The tidal potential used for forcing the tidal model is specified on line 22, page 10. I wonder if a bit more information on the amplitude is required at this place. Is this the amplitude of the equilibrium tidal potential listed in Table 1 of Ray (2001)? If yes, there might a slight mismatch in numbers (1.27 mm in Ray's paper vs. 1.2 mm here). The text also mentions a factor of 0.80 to account for the effect of elastic body tides (see below), but it is not clear whether or not the quoted formula includes this correction.

No, the two numbers are not the same. The 1.268 mm in Table 1 of Ray (2001) is the M1 equilibrium tide amplitude at Newlyn. If you multiply the 3.99 mm of the potential from Cartwright and Tayler by the Y31 spherical harmonic at Newlyn latitude (approx. 50.1) and then multiply by the degree-3 diminishing factor (approx. 0.8) then you will get to the 1.268 mm.

In my case the 1.2 mm (I apologise that was a mistake and it should have been rounded up to 1.3 mm) is the 3.99 mm of the potential times sqrt(7/(192*pi)) * 3 = 1.3 mm which then has to be multiplied by the geometrical terms as shown in the paper.

The quoted formula (the 1.3 mm) does not include the diminishing factor. I have reworded the sentence to make it clearer.

– Getting picky, yes, but using "Doodson number" in the second column of Table 2 is not fully correct. What is shown are actually the integer multipliers for the 6 Doodson variables that define the argument of the tidal term.

I see your point. I have changed the header of that column to 'Doodson numbers', not least because footnote 1 of the paper refers to the 'first two Doodson numbers'. But all six collectively can also be referred to as a 'Doodson number' (at least by me and see IHO, 2006 for example), or an 'Extended Doodson number' when the phase is included, although to be historically pedantic one should add the 5's as in Doodson's own tabulations. I have added a footnote to the table along these lines. Thanks for pointing this out.

– Wahr (1981) is cited as Wahr (1991) in the both the main text and the supplement. I also recommend a slight re-formulation of the last sentence at the bottom of page 3: "... a special value of 0.74 for K1

to allow for resonant perturbations of body tide Love numbers close to the diurnal eigenfrequency of the Earth's fluid core."

Wahr changed to 1981. I wanted to avoid mentioning unnecessary terms ('Love number' as well as 'diminishing factor'). I have changed the wording about the special value for K1 in order to mention Agnew (2018) which contains many useful references on this topic.

– *Finally, it always brings a smile on my face when a single author of a paper uses "we" in the active voice (such as in the abstract here). There can be different takes on it, but I to wonder with whom did he/she write the paper ...*

Point taken. Reviewer 2 also mentioned this. I made a search on 'we' and reworded things.

Many thanks again for these useful comments.