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Interactive comment on "Australian tidal currents – assessment of a barotropic model with an unstructured grid" *by* David A. Griffin et al.

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We thank the referee for their review, but are very disappointed that they could not "find any new or interesting scientific finding". We are quite sure that the principal finding of the study is both new and interesting (at least to some).

Our study shows that tidal currents can be predicted accurately enough to be distributed to the public as high resolution maps, i.e. not just at locations of in situ data (using the observation-based constituents). Is this new? We think so. Why, otherwise, would predictions not already be being published? Is it interesting? Again, we certainly think so. Tides are the dominant component of velocity variance for much of Australia's shelf seas. So anyone either needing to know the current velocity, or in the

growing field of Operational Oceanography, will be interested in our study. And we do not think that only Australians will be interested. Because of the diversity of our tidal conditions, readers will recognize regions similar to theirs, and perhaps be motivated to build similar models.

Responses to specific points follow:

- On p. 3 lines 53-54: Is there any reason to choose a depth-averaged configuration even though COMPAS is 3D model? A: Only for practical reasons. We would not have got to where we have now if we had to wait the extra time to run and analyse a model that is ∼10 or 20 times the cost. The performance statistics would also become even numerous if a depth dimension was added. Proposed edit: " . . .. a depth-averaged configuration is used for this first version of the model, to reduce the computational cost, development time and analysis time."

- On p. 4 lines 72-73: Need to explain why the onsite depth measurements did not use for estimation of the model topography. A: The 95 onsite depth measurements are far too few to make anything but very localised improvements to the model topography. If we were going to start using extra pointwise bathymetry data in addition to existing gridded products it would make more sense to start with the available datasets having many more than 95 points. Proposed edit: "Onsite depth measurements at the locations (Fig. 2) of the tidal currents validation data discussed below were not used for estimating the model topography because they are too few to make other than local corrections".

- Figs. 1-2: Need to describe a space interval in bar graph. A: sorry, we should have been clear. PE: "The colour bar tick labels apply also to the bar graph above."

- On p. 3 lines 61-62: The authors need to clearly explain how to give tidal current speed for a dual weighting function to generate the mesh. A: The tidal current speed was given by an earlier version of the model. PE: ". . . generated using a dual weighting function dependent on bottom depth and a preliminary estimate of tidal current speed"

- On p. 5 line 84: So, was the Herzfeld et al. (2020) scheme used? A: yes. The flux adjustment is optional, as indicated 2 lines earlier by 'provision for'.

- On p. 5 line 89: Need to explain how to determine the value of 0.003 for the bottom drag coefficient. A: This is covered in the following 7 lines. See next Q.

- On p. 5 lines 92-96: I think that these sentences are not so clear. Need to rewrite them. A: Agreed, we will rewrite this paragraph.

Is there any reason to choose the starting date as 24 Feb 2017 for the model running? A: Subject to confirmation: No, there was no particular reason to start on 24 Feb 2017.

Did you predict tides from T_Tide using observation derived harmonic constants? A: Yes. Sorry, we thought this is already clear. E.g. abstract sentence #2: "…. assess it against a validation dataset comprising tidal height and velocity constituents at 615 tide gauge sites and 95 current meter sites"

How long have observation records been used to calculate these harmonic constants? A: The record lengths (and what type of tide gauge) was used, is not available for the tide gauges, but probably greater than 6 months, because K1 amplitudes are listed for all stations. This, however, may be because inference was used.

And which cases did you run 7 or 30 days? A: there were 68 experimental runs. We don't consider these details necessary for the reader .

Is the running period related to observation period? A: there is no observation period. Validation of the final version of the model is based on derived harmonic constants of both the model and the observations – not observed time-series.

How long did you run the model for spin-up time? A: See line 92-93 "including a 1-day ramp period". The ramp is the spinup.

What kind of 'parameters' did you tune and how to adjust the model? A: Parameters: 1) The bottom drag coefficient 2) spatial variations of bottom drag 3) bottom drag scheme

4) coastal depth 5) horizontal viscosity 6) turbulence closure scheme 7) bathymetry smoothing 8) flux adjustment timescale 9) tidal potential forcing on/off 10) bathymetry data source 11) interior relaxation to tpxo on/off . We will add this list to the manuscript.

- On p. 5 line 97: Is there any reason to run the model over 222 days? As you know, at least 183 days are required to separate out the S2 (K1) and K2 (P1). A: yes 183d is the requirement. We aimed for a whole year and failed to achieve that.

- On p. 5 line 106: Is there any reason to use UTide for tidal current analysis rather than T_Tide which was used for tidal analysis? A: Nowhere do we say that T_Tide was used for tidal analysis, so we assume you are referring to line 94 where we say it was used for prediction. So why did we use it rather than UTide for prediction? Because if you only have a table of constituent amplitudes and phases (as is the case for our tide gauges), we find it easier to use T_Tide than Utide. And why use UTide for analysis of current meters? Because it is the newer code, with various improvements.

- On p. 7 line 160: Information on record length is important to determine whether the S2 (K1) are fully resolved or not. I recommend that the authors check this point. A: The record lengths for the tide gauges are unknown. K1 amplitudes are available for all stations but this may be because inference was used. This information, and what type of instrument was used, is not available.

- On p. 7 line 162: Put equation number for a penalty function along with reference for this. A: we can do that, but since we don't refer to this equation in any other sentence we think inline is sufficient. We are happy to take editor advice on this point.

- On p. 7-8 lines 169-170: Put equation number for reM2, rebarM2 and reLF (if possible). Need to be multiplied by 100 in order to match the value of Table 3. From these equations I could not obtain these values listed in Table 3. Need to check equations. A: i) eqn number- in the published version, absolutely ii) mult by 100 – no. The quantity is a ratio. The units of Table 3 is shown as %, therefore implying the factor of 100, iii) You are correct that Table 3 has an error, so thank you for spotting that. The listed sign of reM2 is always positive but indeed it should not be. Taking row 4 as an example: maj_m=128, maj_o=133, phase diff=9deg, sub_o=9: reM2=(128-133)/133 which is -4%, not 4% as listed. The other columns, however, are correct: rebarM2=abs(128*exp(i*pi*9/180) – 133)/133=16% as listed. reLF= (abs(128*exp(i*pi*9/180) – 133)+9)/(133+9)=21% as listed.

- On p. 22 lines 330-332: I do not understand the authors' arguments on tidal potential forcing. On p. 5 lines 90-91, they stated that effect of tidal potential forcing was minor, resulting in no consideration. Need to explain this point clearly. I think, if tidal potential forcing is important in a certain area such as Bass Strait, it should be used.

A: We certainly agree that tidal potential forcing should, in principle, be used. This is why we included it in the model as an option. But we found, as we stated, that it made a negligible difference (and doubled the runtime). With regard to Bass Strait in particular, we stated (lines 330-332) that this result was in conflict with Wijeratne et al for reasons we do not know. We refrained from elaborating, for example by including http://www.marine.csiro.au/∼griffin/ARENA_tides/tides/constits_QC/Bass_h_M2.html (which shows that Bass Strait heights are well modelled) in the paper. The fact is that Bass Strait is not one of the model's problem areas.

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