

Interactive comment on “Back to the Future II: Tidal evolution of four supercontinent scenarios” by Hannah S. Davies et al.

Anonymous Referee #2

Received and published: 18 December 2019

General comments

The paper provides an interesting insight into the changing tidal dissipation regimes of four potential future super-continent configurations. The authors provide the results of tidal modelling at an improved temporal resolution of projected future scenarios over previous studies. The authors have found both multiple occurrences of super-tides and periods of reduced tidal dissipation (compared to the present day) which occur depending on the size of the ocean basin(s) apparent in the modeled time slices. This corroborates previous studies' conclusions that we are currently in a time of particularly energetic tidal activity, and provides insight to those who wish to examine paleoclimate. The authors also show large differences in the spatial distribution of dissipation (deep/shelf) and temporal position of super-tide events (if they occur at all) between

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the different future scenarios. It is a good paper that presents an interesting result and deserves publication after some discussion. I look forward to seeing how the results presented here are built upon.

Specific comments

1) Could the authors provide some comment as to the choice of modelling only the M2 constituent (and not including K1 for example), and on why they retain the Earth-Moon orbit configuration (specifically the 12.42 hr period forcing) throughout the future simulations? Are there projections as to how this may change within 250Ma that may be referenced? It will impact the age/size at which future ocean basin configurations form tidal resonance. This prompts a thought on the validity of the extrapolation of particular values (particularly the buoyancy frequency and ocean volume) from present day climatology for the calculation of tidal dissipation/amplitude in the future scenarios. Can the authors address these simplifications to their simulations?

2) Could the authors state clearly if equilibrium forcing is used at the pole boundaries in this study (as done in Green et al. 2018) or if vertical walls were used – the reading from Line 117 is slightly ambiguous. Since this study provides higher temporal resolution for future continent configurations from Green et al. 2018, does the equilibrium forcing (or vertical wall) at the boundary interfere with any potential tidal resonance in basins/enclosures present in this study but not present in the scenarios Green et al. 2018 considered? It is difficult to tell from the map projection used for the figures in the supplement.

3) Regarding the 4 km deep ocean calculation at Line 209: Does the average depth of any ocean basin change significantly to retain ocean volume between the four future scenarios (e.g. due to differing continent polygon overlap and/or destruction of shelves)? How applicable is this calculation of when resonance occurs for the multiple different basin shapes shown in the different scenarios?

4) The paper professes to support a link between the super-tidal and super-continent

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cycles. Since each continent cycle may be comprised of one or multiple Wilson cycles (to which a super-tidal cycle seems more intrinsically linked to), is there not a lack of a well defined relationship between the period of each?

Technical corrections

line 11 – remove comma after “planet”

line 11 – Perhaps “...oceans *can* move...”

line 47 – “at best” is strange wording, perhaps “at a minimum of”

lines 75-80 – various subscripts are printed as normal sized text

line 129 – I assume “The results” refer to amplitudes. Could this be made clearer what is being compared to TPXO9.

line 152 – is PD defined in the text before its first use here?

line 246 – “...which, when combined, produce...”

I hope these comments aid the authors.

Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2019-61>, 2019.