**Review of revised *ESurf*-manuscript #2023-53 (The flexural isostatic response…)**

The authors have made several revisions, both in the text and the figures, including moving some figures to the supplement. While I think this has improved the manuscript, I remain concerned about how hydro-isostasy is being utilized. Below, I will seek to find a way to avoid this turning into a make-or-break issue.

Hydro-isostasy still appears to be treated as if it only affects the ocean (see e.g., Fig. 1). Fig. 2c exemplifies this further: it mentions "hydrostatic uplift" (which I presume should be hydro-isostatic uplift?) but places this under the landward portion of the domain. Since Fig. 2c illustrates sea-level fall, the hydro-isostatic uplift would occur under the ocean, whereas the adjacent continental margin would experience subsidence. This is the continental levering effect that I alluded to before. The authors claim in their response that my concerns have been addressed, but Fig. 8 suggests otherwise. It is essentially unchanged from the figures in the previous version of the manuscript – hydro-isostasy landward of the shoreline is shown to be zero. If the hydro-isostatic signal on land has been masked in these maps, it should be explicitly stated (although I wouldn't know what the benefit of this could be). Fig. 9 suggests that hydro-isostatic change only occurs within the model domain that sees water column change. The authors refer to Fig. 10 as an example of inclusion of hydro-isostasy but I see no mention of this anywhere in the figure or the caption. Are they saying that this is included in the calculated long profiles? (On a related note, I struggled a bit with this figure anyway; for example, what exactly does the grey shaded background represent? Conditions at t=0 perhaps?)

In summary, I am still rather confused about what exactly has been done. In the event hydro-isostasy has not been treated in the same way as state-of-the-art GIA models would do (i.e., a process that affects ocean basins and continental margins alike) this must be fully acknowledged. This would be a limitation that could potentially affect some of the conclusions, and the authors could highlight this as a target for future research. As a final note, GIA, as generally used in the recent literature, includes hydro-isostasy (GIA is a rather comprehensive term that also incorporates such things as changes in the Earth's rotation, time-dependent shoreline change, and so on). One possible solution would be to adopt the more narrowly defined glacio-isostasy and hydro-isostasy as used by the Lambeck group.

With regard to my previous comments about $T_e$ and the role of waves/tides/currents, I appreciate the detailed responses (even though I don't necessarily agree with all of them) but note that these issues need to be addressed in the paper. I doubt that I would be the only one with these questions. For example, why not provide the specifics about BADLANDS that I asked about?

Finally, and looking at Figs. 6 and 7 in particular, I would add a suggestion beyond my previous review. Since the authors examine the imprint of allogenic cycles in the stratigraphic record, it is important to address the issue of timescale and whether it is likely that these cycles will be recorded. I am alluding here to the signal shredding concept pioneered by Jerolmack & Paola (2010, *GRL*) that has been refined by a host of subsequent studies. For example, the work by Li et al. (2016 & 2018, *Geology*) which considers both sediment delivery from the hinterland and basin deposition is particularly instructive in the present context. The good news is that the cycle length used in the present study likely satisfies the theoretically predicted minimum periodicities for preservation in the stratigraphic record. It would be useful to briefly address this issue.

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