Dear Professor Bass,
We thank you and the two reviewers for their constructive comments. Point-by-point responses are listed in the text below.

Dear authors,
The two reviewers have now returned their reports, apologies for the longer wait due to summer holidays. Both reviewers see a way forward to publication, although they have identified a number of minor issues and edits that need to be addressed in a minor revision. Reviewer #2 still has continuing concerns over the way that hydro-isostacy is working in the model and feels that the revisions have not fully addressed this issue. The third paragraph of their review suggests a way through minor revisions that would avoid this becoming a make-or-break issue, by the manuscript including an explicit statement as to whether hydro-isostasy has been treated the same way as in state-of-the-art GIA models or not, and if not how this may affect or limit some of the findings. The reviewer raises a few other thoughts in response to the revision that the authors should reflect on and address in a minor revision.

Please proceed with a minor revision of the manuscript, including an author-response-to-review-comments, which will then be evaluated at editorial level to determine whether the remaining set of concerns have been adequately addressed.

Andreas Baas
Handling Editor

We clarify that our modeling approach treats hydro-isostasy in the same way as state-of-the-art GIA models, but we do not impose an ice load within our model domain. We also clarify that half of our modeling efforts are focused on an ice-free greenhouse world. Instead, our numerical models build on the work of Sømme et al. (2009), which looked at the effects of climatically-driven sea-level changes on shelf morphology and sediment dispersal, and note that reviewer 1, Tor Sømme, is a well-known expert in basin analysis and modeling, and does not have any comments about our modeling strategy.

Referee one - T. Sømme

38 There is no reference to Fig 2 before this
Thanks, we have now added the reference to figure 2 in line 35.

43 Sømme
Thanks, we have modified it

84 bidirectional
Thanks, we have modified it

205 mean of what? mean of all compensated or non-compensated models?
We refer to sediment accumulation as a proxy for accumulation. The second part of the sentence explains that mean maximum rates of sediment accumulation ‘ are significantly smaller in non-flexurally-compensated simulations when compared to their flexurally-compensated counterparts.’

242 This sentence is a bit detached from the rest of this section and would fit better in the 'Discussion' section
We agree, we have removed this sentence from the manuscript.

253 When discussing the amplitude of isostatic adjustment relative to sea-level change, it would be good to give some actual numbers. From Fig 8 it appears that the amplitude of hydroisostatic adjustment is similar to the amplitude of sea-level variations, is that correct?
The amplitude of the hydro-isostatic adjustment depends on both the amplitude of the sea-level change and the bathymetric changes, which in turn can be controlled by the erosion or deposition patterns. We illustrate this spatio-temporal complexity in Figures 8, 9 and S6. In Figure 8, to be consistent we made the scale of the hydro-isostatic adjustment the same as the amplitude of sea-level variations, but the clarify that the amplitude of hydroisostatic adjustment is not similar to the amplitude of sea-level variations. Figure 9 shows how the amplitude of isostatic adjustment changes with sea-level change.

275 Can you quantify the difference between icehouse and greenhouse systems?
In line 260, we quantified the difference between icehouse and greenhouse systems. “..simulations where the ice-house sea-level curve was imposed display the largest range in hydro-isostatic response (Fig. 9) and the down-dip shelf-to-slope profile has elevations and bathymetries that are 20% lower than simulations with a green-house sea-level curve (Fig 10 c).”

290 Recently we published a paper investigating the stratigraphic response to climate and tectonic uplift in the Paleocene of the Norwegian Sea (https://www.frontiersin.org/articles/10.3389/feart.2023.1082203/full). The situation in the

study area had a complicating factor in that it also was subjected to regional uplift, but the overall basin setting is similar to what you model here. One of the observations we made was that the thicknesses of clinothem wedges was higher than the published amplitude of sea level changes during the Paleogene (from Miller et al). In other words, if high-frequency sea-level rise was around 40 m and the next prograding clinothem unit was around 70 m thick, there is excess accommodation. We attributed this to flexural loading during progradation. Similarly, we observed around 120 m of shelf incision in this greenhouse system, which we largely attributed to regional uplift. But looking at your values of hydro-isostatic loading and unloading, perhaps also some of this could be attributed to this process too (although the Paleocene system is MUCH smaller than what you model). My point is that maybe there are some additional implications here for how we interpret and link thicknesses of stratigraphic units to sea level changes, accommodation and magnitude of uplift. If hydro-isostatic compensation is significant, perhaps we are overestimating other processes like tectonic and/or dynamic uplift and subsidence? You model a very large system, would the effect of hydro-isostatic compensation be similar or even larger in smaller systems? Just some thoughts, but your work certainly got me thinking about these other implications

We agree with the reviewer, as stated in the last sentence of the abstract, one of our primary contributions is to demonstrate that climate-forced sea-level changes result in self-sustaining creation and destruction of accommodation into which sediment is deposited and therefore plays a major role in delta morphology and stratigraphic architecture. We agree that the community sometimes places too much emphasis on tectonic and/or dynamic uplift and subsidence and that was the motivation for adding Section 2 and Figure 1. Figure 1 describes the different components of vertical motions, the time scales over which they operate and their rates of motion. We also recognize that the scale bar in Figure 1 was difficult to understand so we changed the scale bar on the rates of motion to make it more comprehensible.

The effect of the hydro-isostatic compensation would vary depending on the width of the shelf, which scales to the size of the system. Based 1-D flexural models Mike et al., (2013) showed that the surface deflection due to hydroisostatic adjustments is smaller in narrow shelves.

Thanks for bringing your publication to our attention, we have included a discussion about how hydroisostasy can explain the discrepancy between the thicknesses of clinothem wedges and the proposed amplitude of sea level changes in Section 4.5.

295 Some of the examples referred to here are foreland basins and cratonic basins, and would have different lithospheric configuration from what is modeled in this study, so it can be questioned how well the non-flexural scenario is relevant for these cases. Specifically, I don't think the Boreal Ocean/Barents Sea is a good example, since that was an intra catatonic basin in the Triassic. The subsidence was highest in the proximal part of the basin (close to the Urals), quite opposite to the passive margin type basins you model

here. Also, major changes in sediment thickness (Fig 1 in Klausen et al., 2019) suggest that the lithosphere was not very rigid.
We recognize that the examples we present are from foreland basins and cratonic basins. We extrapolate the results of the non-flexural scenario to these settings because the specific cases we mentioned are adjacent to cratonic settings with large elastic thicknesses (see Tesauro et al., 2012) and therefore the amplitude of the flexural response would be equivalent to the cases without flexural compensation.

We politely disagree and we consider that the Boreal Ocean/Barents Sea is a good example. We also consider that major changes in sediment thickness cannot be used as evidence to suggest that the lithosphere was not very rigid. There can be other factors that can explain major changes in sediment thickness.

324 levels of what? sea level fluctuations?
We meant sea levels, we have modified it for clarity.

332 the interplay
We have modified it

336 comma
We have modified it

Figure 5 should not this be 'non-compensated'?
Thanks for catching this. There were several typos in the sentence.
It now reads 'These results show that flexurally compensated models have significantly smaller progradation distances in the river mouth and the shelf break, smaller areal extent and larger coastal elevations (due to flexural uplift) compared to their non-flexurally compensated counterparts'

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

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?)

As we explained in our previous review, our models capture the hydro-isostatic changes observed in both the landward portion of the continental margin and ocean basins. We have changed the associated text and figures to explain how we treat hydro-isostasy. We clarify that figures 1 and 2 are conceptual representations of hydro-isostasy and we were showing where the water load was applied, not the extent of the hydrostatic adjustments. We have modified both figures and figure captions to clarify that hydro-isostasy also affects the landward portion of a deltaic setting.

We also want to make it clear that BADLANDS and gFlex are fully coupled to calculate the flexural isostatic compensation of both the sediment and water load. In our simulations, net vertical changes are the result of the interplay between flexural isostasy (of the sediment and water loads) and erosion and deposition, and we consider it important to isolate the component of vertical changes that are solely associated with the water load. This approach was first suggested in a previous iteration of this paper by the developer of the gFlex code, Andy Wickert, who specializes in numerical modeling of isostatic processes. Following this guidance, we first ran the fully coupled simulations and then used the outputs to isolate hydro-isostatic effects (see lines 245 to 250). These results are presented in Figures 8 and 9 and section 4.4 (lines 245 to 285). In addition, we clarify that the length of the modeling domain in Figure 8 is 4500 km, therefore because of the scale, it looks like the hydro-isostasy is not affecting the areas proximal to the shoreline.

Figure 10 shows the net vertical change resulting from the interplay between flexural isostasy and erosion and deposition. We have expanded the explanation in Figure 10, highlighting the fully coupled-nature of our modeling results, where the flexural isostatic compensation is derived from both the sediment and water loads, the latter referred to as hydro-isostasy.

We agree with the reviewer that the main focus of the paper is aligned with the continental levering effect and we have elucidated this in the caption of Figure 2.

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.

Our modeling approach captures vertical motions associated with water loading in both the landward portion of the continental margin and the ocean basins, thus we treat hydro-isostasy in the same way as state-of-the-art GIA models. However, we do not impose an ice load in our simulations. Moreover, half of our modeling efforts are focused on an ice-free greenhouse world, and as stated in the title of the paper, our manuscript focuses on flexural response to climatically driven sea-level changes, so we do address hydro-isostatic effects.

We believe this is also the first modeling study that focuses on interactions between hydro-isostasy and sediment erosion and deposition. As the reviewer suggested other scholars can focus on the interplay between GIA and surface processes, however this is not the focus our manuscript. Instead, we build on the work of Sømme et al. (2009), which looked at the effects of climatically-driven sea-level changes on shelf morphology and sediment dispersal, and note that reviewer 1, Tor Sømme, is a well-known expert in basin analysis and modeling, and does not have any comments about our modeling strategy. One of the strengths of our approach is that we couple surface processes to flexural isostasy, allowing us to investigate feedbacks between climate-driven sea-level change, erosional and depositional processes, and flexural uplift and subsidence within a dynamic framework.

With regard to my previous comments about Te 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?

In lines 169 - 172, we have added a paragraph that explains "Our models do not include the effect of waves, tides, and shelf currents. BADLANDS is capable of simulating wave-induced longshore drift (see Salles et al., 2018, which implements the equations from Longuet-Higgins 1970). We recognize the importance of waves, tides, and shelf currents in sediment transport but those processes move sediment volumes generally in the vicinity of deltaic depocenters and therefore would not play a crucial role in sediment load redistribution."

We checked the first round of revisions and we assume that the reviewer is asking about lines 130-131 of the first submission. "Details about boundary conditions and input parameters used in modeling simulations can be found in Table 1 and the data repository." As we explained the initial conditions are shown in figure S1 and Table 1 details the boundary conditions. In addition, the numerical model can be found in the following code repository https://github.com/saraemp/egusphere-2023-53 . We have included a figure in the supplementary materials detailing BADLANDS' constitutive equations, we hope it answers any questions the reviewer may have about the algorithms that BADLANDS uses.

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.

We are aware of the shredding concept and we recognize there are many aspects that can be investigated in our models. However, we did not discuss it in our manuscript because, as the reviewer states, the temporal resolution of the boundary conditions (see Table 1) in our numerical models was designed to capture the cycle length that would allow the preservation of the flexural isostasy in the deep-time stratigraphic record, allowing us to address our stated scientific questions. In order to test the cycle lengths that will allow or not (null hypothesis) the preservation of allogeneic signals, a different modeling approach would be required.