RC1: 'Comment on egusphere-2023-53', Tor Somme, 13 Apr 2023
This paper by Polanco et al investigates the effect of load-induced flexural isostasy and hydro-isostasy on a passive margin system using a numerical model. The authors find that the rate and response times of these processes greatly affect delta stacking pattern, progradation distance as well as extent and duration of unconformities etc. Specifically, the paper concludes that changes in sea level fluctuation during both greenhouse and icehouse times may have resulted in vertical motions that influenced the dynamics of the delta system through isostatic response. The paper is very well written and the figures are generally clear, although there are many figures compared to the length of the manuscript and some can be used as supporting material. Below I have listed some key points that I think should be addressed.

In the introduction, the section covered by lines 35-43 marks the transition from general introduction to the purpose of the study. But in line 44, the authors continue the general introduction comparing glacial margins, sea level fluctuations and isostasy. In line 61, the authors again go into the purpose of the study a second time. I suggest a restructuring of the introduction so that the general introduction is kept separate from the purpose and aim of this study.
We agree with the reviewer and we have restructured the introduction as suggested

In terms of terminology, terms like "Isostatic adjustment", "flexural response", "flexural loading". "flexural subsidence", "flexural adjustment", "flexural isostasy" and other similar terms are just variably throughout the paper, but no definition or clarification of these terms are given. Do they all refer to the same process? I suggest that the authors define these terms early in the paper to avoid confusion.
We now explain the terminology as per the reviewer's suggestion. We have done our best to not use synonymous terms to avoid confusion, for example we now use the term flexural isostatic response throughout the manuscript while before we were using the term flexural isostatic response and flexural adjustment synonymously. In the previous version of the manuscript we did not define those terms as they are commonly used in the geodynamics community, but given the interdisciplinary nature of the manuscript we agree that it is best to define them.

Section 4.1 is called "Flexural isostatic effects on delta morphology and stratigraphic evolution", and also the caption in Figure 8 points to the difference in river morphology in the flexural vs non-flexural model. However, the authors never explain what the effect on delta or river morphology actually is in Section 4.1. Nor is the stratigraphic evolution described in any detail. The plots in Fig 8b shows river mouth locations, but the differences and implications are not discussed. Except for the fact that non compensated models prograde farther into basin, what are these results telling us?
We now describe the model's evolution in more detail (section 4.1) and explain the differences between the non-compensated and compensated simulations (section 4.2). We also extrapolate the results of the non-compensated simulations to deltaic ancient systems formed over a rigid lithosphere, drawing from examples of Triassic deltaic systems (e.g. Martin et al., 2018; Morón et al., 2019; Klausen et al., 2019) and the Early Cretaceous McMurray Formation in the Alberta foreland (section 4.5).

Another relevant issue is the scale and rate of flexural adjustment to sea level fluctuations. In Section 4.2, for example, is stated that "significant bidirectional flexural compensation can take place at high frequencies". Even if it is demonstrated that the

compensation is coeval with deposition, the amount of vertical movement is not discussed. Figures 5 and 7 suggest that the amplitudes are very high, several hundred meters! Figure 10 implies that a 100 m sea level fall will give an isostatic response of about 10 m, however, actual values are not presented or discussed by the authors. It would be useful to discuss how much accommodation is ascribed to flexural loading and how much is described to sea level fluctuations so that the reader can get an impression of the relative contribution of the two processes.
We initially focused on the trends and nature of the response, rather than the scale and rate, because the flexural-isostatic response is dependent on the scale of the load and the rate at which it is imposed, the latter is demonstrated by the power spectra (Fig 7). It explains why a "*100 m sea level fall will give an isostatic response of about 10 m*" only applies to that time snapshot and we present the complex spatio-temporal variation of the hydro-isostasy in Figure 9.

To address the reviewers' comments on the quantification of the results, we now show the normalized results of the flexural deflection (Fig 7) and the elevation/bathymetry (Fig. 10). We consider that by normalizing the results we provide an objective comparison that does not have the bias of the inherent dependence of the flexural-isostatic response to the scale of the sediment and water load. In the new version, we provide a more detailed discussion of the temporal and spatial evolution of the flexural-isostatic response to both the sediment and water loads (section 4.4).

Figure 10 shows how much accommodation is ascribed to flexural loading (simulations with no sea-level change) and how much is attributed to sea-level fluctuations (simulations GH and IH). The complexity arises because the flexural isostasy amplifies changes in sea level. To make this clearer we have plotted the normalized elevation/bathymetry, which shows that the difference in accommodation between the GH and IH is only 10%, but when flexural isostasy is taken into account it reaches approximately 20% (Figure 10).

In the "Results and discussion "section, I also would have liked to see a more concrete discussion on the implication on real systems. In addition to a discussion on the actual rates and amplitudes, as mentioned above, it would be interesting to discuss the interplay between long-term load-induced flexural isostasy vs more short-lived hydro-isostasy. The authors mention that it can enhance valley incision during falling sea level and aggradation during transgression, but are there other consequences of this? For example, will the combined rate of subsidence or uplift be the same across the delta? Can one expect that transgression and increase in accommodation occurs faster in one region compared to another?
We have incorporated a new section that discusses the implications for natural systems (section 4.5). We present subsidence measurements from the Gulf of Mexico, which have a similar spatial and temporal pattern as the ones presented in our simulations. These patterns show uplift in land as well as subsidence in the marine realm. In our models vertical changes are only generated by flexural isostasy, thus we take the similarities in spatio-temporal patterns as a demonstration of the critical role played by flexural isostasy and hypothesize that flexural isostasy is the primary mechanism for generating vertical accommodation in passive margins.

There are multiple aspects to be discussed when it comes to the differences between the long-term load-induced flexural isostasy and the short-lived hydro-isostasy.

1. At the river mouth: The cumulative sediment and water load is monotonic and unidirectional, while when sea-level is also considered we note that fluctuations are bidirectional. We now show the difference in flexural response between constant sea-level and sea-level changes. This is not the hydro-isostasy, but rather the combined effect of the sediment and water load.
2. Along the profile: While it is relatively straightforward to calculate the effect that sea-level change would have on the topography, it becomes more complex when erosion and deposition are both considered. We are illustrating this complex relationship in Figure 9 and discuss it in Section 4.4. The cumulative effect causes a ~20% elevation difference between the simulations with different sea-level changes (Figure 10).

More detailed comments:
Line 72: "thermally mature passive margin", what is a thermally mature margin?
We have removed the term and rewrote the sentence to improve its clarity.

Line 87: Compaction is described together with local controls like growth faults and salt tectonics, but will of course be at the scale of the entire sedimentary wedge.
We agree that the magnitude of the compaction would depend on the scale of the entire sedimentary wedge, but we assume that the effect of compaction would have a local scale (100s to 1000s of km^2).

Line 256: Can you be more specific with the use of accommodation here? Accommodation can be many things. Here I guess you are referring to accommodation created by longer term hydrological and flexural isostasy. You are not referring to rapid changes in sea level, which also can create rapid changes in accommodation that are not filled immediately.
We are referring to the space available to deposit sediments regardless of the mechanism that it is generating the accommodation.

Figure 4. Which map does the scale bar called 'Discharge' refer to?
Discharge is shown in both maps to visualize the paths of the fluvio-deltaic system. The scale bar is the same for both maps. We have added that clarification to the figure caption.

Figure 5, (d) and (e). The amplitude of flexural uplift and subsidence is almost 1 km in (d), and several hundred meters in (e), whereas the amplitude of sea level variations is 100 m. Values of up to 1 km in response to hydro-isostatic loading does not match up. Perhaps I have misunderstood the figure. Otherwise, please check labels and/or scales.
The labels and/or scales were correct. We have edited the figure caption to clarify that the x axis on the second column (d, e, f in the previous version of the figure) shows the flexural deflection caused by the sediment and water load at the river mouth.

Figure 6. Check label, should f5Mkyr be f5Myr? Explain what arrows point to
Thanks, we have corrected the typo and deleted the arrows as they were just showing the peaks in frequency.

Figure 8. Subplots (a), (b) and (c) are not discussed in the text. Black bars on model outputs, are they scale bars as shown to the left? Please explain in the caption. First line of caption, what is (f) referring to? In description of (c), what is a "de-trended river mouth trajectory"? In this bar plot, it would perhaps be easier to just measure the distance to the shelf break?

In the new version of the manuscript we discuss this figure (now figure 5) in section 4.2. We have deleted the black bars on model outputs. (f) was referring to frequency, but we have removed it for clarity. We have also modified panel (c); in the new version we present violin plots (modified kernel density plots) and down-dip shelf-to-slope profiles to show the location of the shelf break.

Figure 10. 'Left', 'middle' and 'right' does not explain the position of the three plots
We have moved this figure to the data repository and added letters to each panel.

Figures 8-15. Consider making some of these figures supplementary material. In many cases, like Figs 11, 12 and 13, they supplement the discussion, but they are not discussed or referred to in any detail in the text.
We have moved figures 9, 10 and 11-13 to supplementary materials. We selected some of the outputs presented in figures 11-13 and combined them into one figure, which is now figure 9. In sections 4.2 and 4.3 we now discuss in more detail figures 7, 8, 9, 10 (previous 8, 14 and 15).