

EC1: ['Associate Editor's Comment on egusphere-2023-53'](#), Andreas Baas, 02 May 2023

Dear Authors,

We have now received two comprehensive reviews on your manuscript, both of which have raised insightful questions and supplied helpful recommendations. The reviews suggest a major revision in order to both improve the explanation and presentation of the work as well as potentially conduct further simulation and analysis to address some of the questions and comments. Your revised manuscript and your author-response document will then be returned to the reviewers for a second round of evaluation. Given the nature and extent of the further work that may be required please feel free to contact the editorial office to request any deadline extensions you may require.

Andreas Baas,

Handling Editor

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Dear Professor Bass,

We thank you and the two reviewers for their constructive comments. In the revised manuscript we have:

- Expanded the discussion about the rates of vertical motions that each process contributes to in sections 2 and 4.5
- Expanded on the numerical methods we use to clarify the range of processes that our simulations capture (section 3)
- Created a new section (4.1) where we present a detailed description of the model results
- Expanded the results and discussion that focused on the effects of the flexural isostatic response on delta evolution (sections 4.2 to 4.4)
- Added a new section that explains the implications of our simulations for natural systems (section 4.5) and compared the rates of motion of our simulations with data from the Mississippi deltaic depocenter (Figure 11).
- Modified the figures to address all the reviewers comments. We have normalized the changes in elevation and bathymetry (Figures 7 and 10) so that the results can be compared more efficiently and calculated the rates of vertical motions (Figure 11).
- Reduced the number of figures from 15 to 11 by combining some and by moving others to the supplementary materials. By doing that we have addressed the imbalance between the text length and the number of figures.

We have answered all the reviewer's comments with point-by-point responses.

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 km²).

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).

RC2: '[Comment on egusphere-2023-53](#)', Torbjörn Törnqvist, 29 Apr 2023

Review of ESurf-manuscript #2023-53 (The flexural isostatic response...)

The isostatic response to deltaic sediment (un)loading is relevant for studies of the stratigraphic record as well as for understanding flooding hazards in modern deltas.

Polanco et al. highlight the need to include flexural responses to both sediment and water loading along continental margins, something that may not yet be the norm in stratigraphic modeling (although I must admit that I haven't followed this literature very closely in recent years). They do so by coupling a landscape evolution model with a flexure model.

Among others, they find that flexural compensation leads to more complete stratigraphies and more limited horizontal shifts of facies belts. As the authors state (lines 116-118) the goal is not to test the model versus observational data, but rather to develop hypotheses that can be examined by means of future empirical studies.

I focus here mainly on some broader issues, along with a few more detailed comments.

However, I must add that I struggled with several of the figures and tables, where information was sometimes hard to decipher if not confusing. By way of example, the caption of Fig. 10 talks about a left, right, and middle panel (but that's not how the figure is arranged; please use a, b, c) and they mention a dashed grey line which I didn't see. I think I know what they mean, but the reader shouldn't have to be guessing. There is also a dark brown layer in the lower panel that is not explained. I would urge the authors to carefully examine all figures and tables for such problems; again, I did not try to identify all these issues (but see my comments below about the tables).

My main comment concerns the role of hydro-isostasy which I believe is more complex than suggested. The authors appear to emphasize the effect in the full marine realm where indeed the result of sea-level fall may be uplift (and vice versa). However, the opposite is typically the case along the landward portion of the continental margin (due to lateral flow in the asthenosphere) as widely shown by the mid- to late-Holocene sea-level change in the Southern Hemisphere. This is why some prefer the term "continental levering" (e.g., Mitrovica and co-workers). The implications of this for the present analysis are potentially significant, because any given sea-level change would affect the sediment-dispersal system differently (in fact, in opposite ways) depending on location. Since this is not discussed by the authors, I'm not sure it is something the model incorporates. Figs. 12 and 13 appear to suggest that there would be no hydro-isostatic effect landward of the shoreline. This needs to be addressed; not least because it may have implications for the self-sustaining feedback mechanisms described in lines 242-244. In short, I suspect the story may be a bit more complicated.

Our models capture hydro-isostatic uplift in the landward portion of the continental margin, and produce effects similar to those in the empirical data of Frederick et al. (2020). Figures 11 to 13 (now Figure 8) show the calculation of hydro-isostasy, which is performed using the water load and therefore in the marine realm, in contrast Figure 15 (now Figure 10) shows how its effects not only occur in the ocean, but extend onshore as well. In Figure 15 (now Figure 10), landward from the shoreline we note an elevation difference of more than 100 m between the green-house and ice-house simulations.

We agree that the role of hydro-isostasy is complex and we believe that because of that complexity, its effects have been understudied and underappreciated. In Figure 2 we aim to communicate the complexity of hydroisostasy and its interactions with the sediment dispersal system and associated spatio-temporal variability of sediment loading and unloading. We consider this to be one of the most important contributions of this paper.

The authors use a fairly thin elastic thickness for the lithosphere (50 km) compared to what is commonly done in GIA models. However, since their focus is on relatively long timescales, they could benefit from the inference by Wolstencroft et al. (2014; their Section 5.3) that the elastic thickness decreases over longer timescales due to flow (i.e., a lower viscosity) in the lower lithosphere. That study found values near 100 km for the Holocene, but closer to 50 km for timescales extending into the last interglacial.

Wolstencroft et al., (2014) states “ The effective elastic thickness of the lithosphere may be >100 km over the Holocene but decreases by at least 50% to ~50 km on the 100 kyr timescale. The latter value is compatible with previous estimates of effective elastic lithospheric thickness for the region (e.g., Bechtel et al., 1990).”

Thus, with the timescales of $>10^7$ yrs we are using an elastic thickness of 50 km is suitable. This value is in agreement with both Wolstencroft et al., (2014) and Bechtel et al., (1990, Nature). In addition, as we explain in lines 143-146 the elastic thickness value we use (50 km) is within the range observed in passive margins (Tesauro et al., 2012) and similar values have been used in previous flexural studies of continental-scale deltaic depocenters (e.g. Driscoll and Karner 1994; Watts et al., 2009).

Below we clarify the points made by the reviewer about (1) the relationship between elastic thickness and time and (2) the use of two elastic thicknesses for different time scales.

- (1) We politely disagree with the reviewer’s statement that the “elastic thickness decreases over longer timescales”. The elastic thickness (T_e) increases with the age of the lithosphere (Δt) due to the time dependence of lithospheric cooling via thermal conduction. That relationship can be expressed using the following equation published in Stein and Stein (1992, Nature)

$$T_e = (2.70 \pm 0.15)\sqrt{\Delta t}$$

- (2) Wolstencroft et al., (2014) use two different elastic thicknesses, which are time dependent because they use a Maxwell viscoelastic, spherically symmetric Earth model. Such models combine the properties of elastic and viscous spheres: they respond initially as an elastic material, but if the load is applied for long enough they behave as a Newtonian viscous fluid (Watts 2001, Farrington et al., 2014). This is why Wolstencroft et al., (2014) state that “the effective elastic thickness of the lithosphere may be <50 km on a 10^5 year timescale, but closer to 100 km over 10^3 to 10^4 year timescales”.

We reiterate that the elastic thickness we use is in accordance with Wolstencroft et al., (2014) and thus the results of our simulations are correct.

Does BADLANDS include waves, tides, and shelf currents? This may not be critical for the present study, but it would be good to know (in lines 130-131 there is mention of further details in a data repository which I couldn't find).

Our models do not include the effect of waves, tides, and shelf currents. Although, 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.

Salles T., Ding, X., Webster, J., Vila-Concejo, A., Brocard, G., Pall, J. A unified framework for modelling sediment fate from source to sink and its interactions with reef systems over geological times. *Nature Scientific Reports*, 8, 5252. doi:10.1038/s41598-018-23519-8

Longuet-Higgins, M. S. Longshore currents generated by obliquely incident sea waves. *J. Geophys. Res.* 75, 1–35 (1970).

The manuscript includes fifteen figures and two tables, which is a lot compared to the length of the text. I believe a portion of this could be moved to supplementary information without any harm. For example, are Figs. 11, 12, and 13 all necessary in the main paper? And how about Table 1?

We have moved figures 9, 11-13 to the supplementary materials and have discussed in more detail figures 8, 10, 14 and 15 (now figures 5, S6, 9, 10). In addition, we simplified Table 1 and removed Table 2 as we now plot the information listed on that table in figure 7.

Line 56: model predictions by Ivins et al. (2007) produced subsidence rates that are far higher than observations due to input sediment loads that were off by an order of magnitude. Instead, I suggest Jurkowski et al. (1984, JGR) who pioneered this type of work in this region and came to better results. Among others, they demonstrated the presence of a peripheral bulge associated with a deltaic depocenter and explained this mechanistically, well before others did (lines 80-81).

Line 56 lists references for papers that have addressed the cyclical nature of sediment- and hydro-isostatic adjustments associated with the response of sediment dispersal systems to climate and sea-level change. There are a number of issues with the Ivins et al. paper, and with other papers cited in that list that can be contested - they were pretty early on in this type of discussion. We only cite it as previous work on this topic and think it is appropriate to keep them in the list of citations with the other papers.

The classic paper to demonstrate peripheral fore-bulges associated with a deltaic depocenter is Driscoll and Karner (1994), but we now cite the Jurkowski et al. (1984) paper as well since it pertains to the Mississippi delta, which is referred to on many occasions in the paper.

Line 92: A better reference than Jankowski et al. (2017) would be Keogh et al. (2021, JGR-ES) which explored this issue in much more detail.

We agree, and use the insights from the Keogh et al. (2021) paper instead.

Line 101: <1-2 mm/yr is a bit ambiguous; here I think they can safely say <1 mm/yr.

The reviewer would be correct if we were discussing the modern subaerial delta plain, but our focus, as stated in the same sentence, is at the scale of the deltaic depocenter. The Mississippi deltaic depocenter extends to the shelf margin. As shown in Frederick et al. (2020) time-averaged deep-seated subsidence rates at that latitude, which is $\sim 28^{\circ}\text{N}$, are up to 3 mm/yr. We have changed the text for clarity and it now reads deep-seated components are generally $\leq 1\text{-}3$ mm/yr (Frederick et al., 2019)

Line 122: Fig. S1?

It refers to figure 1 of the supplementary materials

Lines 154-155: can this be documented in supplementary information?

Agree, it is now included in the supplementary information

Fig. 3: needs work; for example, fonts are too small.

We have modified the figure to improve the readability

Fig. 5: the caption refers to Fig. 1, but presumably this should be Fig. 4?

Thanks, we have fixed the typo.

Tables 1 & 2: in their present form, these tables are a bit cryptic and hard to interpret for the reader. Please avoid acronyms whenever possible (it's hard to relate to "f5Myr A25m") and river mouth lengths of >2000 km sound strange. I assume this is the entire river length? It would be much better to use "river-mouth transit distances" as in the main text. And what is "mean maximum accumulation"? Is it the mean or the maximum? I'm sure there are reasons for terms like these; they just need to be explained.

We have simplified Table 1, we kept the 'acronyms' because they relate to the sea level curve we imposed in the simulations and are used in the accompanying figures. We deleted Table 2 and the information is now displayed in Figure 5.