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.

Wolsterncroft 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 Wolsterncroft 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 (Te) 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)

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

(2) Wolsterncroft 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 Wolsterncroft 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 ~28°N, are up to 3 mm/yr. We have changed the text for clarity and it now reads deep-seated components are generally ≤1-3 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.