Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/esurf-2021-42-RC1, 2021

Comment on esurf-2021-42

Anonymous Referee #1

Referee comment on "From apex to shoreline: fluvio-deltaic architecture for the Holocene Rhine–Meuse delta, the Netherlands" by Marc J. P. Gouw and Marc P. Hijma, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-42-RC1, 2021

This manuscript evaluates changes in channel body architecture of the Holocene Rhine-Meuse delta. Using perhaps the highest resolution dataset of any field-scale delta stratigraphy, the authors find a remarkably linear decrease in the channel deposit proportion with distance down the delta, a threshold transition in channel connectivity ratio with distance down the delta, and a hooked trend of sand fraction with total distance down the delta that accounts for estuarine and intertidal sands. While these trends are analyzed in the context of reservoir analysis, I think they would also be interesting in a sand availability/sustainability context or the sand fraction required for deltas to aggrade with rising sea levels. These trends are clear and compelling, and I agree that the trends may be characteristic of many modern and ancient deltas.

Unfortunately, I found the context of these results to need significant improvement before it can be considered for publication. By context, I mean that the introduction makes promises that are not met, and the discussion does not yield an improved process-based understanding or challenge existing concepts. I would very much like to see these data published one day, but this context will need to be improved.

The abstract and introduction promise analysis of the Rhine-Meuse dataset in the context of backwater hydrodynamics. I was excited to read about this, because the Rhine-Meuse has rarely been studied in this context. However, this never appears, and I don't think that the backwater length of the system is ever even given. There are many recent studies that give generic predictions about channel dynamics in the backwater zone (Chadwick et al., 2019, 2020; Fernandes et al., 2016; Martin et al., 2018). The narrowing of channel bodies, in particular, appears to be characteristic of backwater zones, including the Rhine-Meuse (Fernandes et al., 2016). As stated above, the trends you show are clear, so it would be valuable to compare them to previous predictions.

In the discussion, the authors attempt to explain the trends primarily as a function of channel belt sand body size, longevity, creation of accommodation, and inhereited floodplain width, and a causal loop diagram. I think this section could also be improved. For example, it is my understanding that the longevity of most channels in the Rhine-Meuse are quantified (Stouthamer and Berendsen, 2001). This suggests that a channel belt width, channel longevity graph could be constructed and empirically analyzed. The paragraph beginning L368 argues that the loss of stream power explains the reduction in mobility. However, backwater studies have shown that shear stress may increase with proximity to the coastline (Lamb et al., 2012; Nittrouer et al., 2012; Smith et al., 2020). The claim that clays and silts are more erodible than peats is an interesting one (L384), but seems contrary to prevailing thoughts that cohesive fine sedimentation is the most resistant material in rivers and deltas (Dunne and Jerolmack, 2018; Edmonds and Slingerland, 2009). In summary, there are concepts out there to flesh out the understanding of controls, but they are not used.

This laundry list of concerns is meant to bring the manuscript in line with the current state of understanding of fluvial deltaic channel dynamics. Some or all of these concepts from the literature may be wrong, or may not apply to the Rhine-Meuse. I would welcome results and a paper that contradict our current understanding, as the field data available to the authors should trump models and push the field forward. However, the qualitative claims made by the authors are too vague to prove the cause of the stratigraphic changes, and are frequently in contradiction with alternate explanations. Another avenue I would recommend is to focus less on a morphodynamic explanation of the trends (as the discussion does) and more on implications. It is of course up the authors, but I think the same data could be used to really advance the growing field of sand resources, with only a brief description of similarities and differences to backwater controls on channel architecture, and I would still consider it a significant advance. I hate it when reviewers talk about the paper they would have written! I just bring it up as an option because the data is fascinating, and I want it out in the world general models of the deltaic clustering of sand bodies and sands or not!

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