Dear Dr. Halsted,
apologies for my late response. I now have carefully evaluated your responses to the reviewer´s comments. In the light of these, and my own thoughts on this work, I would like you to revise your paper accordingly. I will send it out for review again though. Behind this reasoning is how exactly your work complements and perhaps goes beyond what Wittmann et al. (2011; 2020) have done needs some major work and additions. I am delighted that the statistical analysis confirms what we have suggested previously, but please try to go beyond the statistical analysis. In this regard, I am looking forward to read about your explicit findings regarding hillslopes and floodplain storage in the revised manuscript. Some authors have developed models of characteristic length scales and transport velocities (e.g. Pizzuto et al.; Lauer et al.). Outcomes of these could be of help to you to further understand floodplain mixing and the response of cosmogenic nuclides. Evaluating channel forms and processes a bit deeper could help, too, to go beyond the statistics. Please also try to implement the findings and data of Dr. Jautzy, as his work has some implications here regarding steady-state assumptions. Also, I can see and understand your reasoning behind using the ratio of denudation rates (i.e. erosion rates in the MS in some places, it would be great if you used the term „denudation“ consistently), and not concentrations, for evaluation of complex sediment histories, but clearly state the limitations, too, of that approach. Be and Al do not behave exactly the same with regards to production rate scaling, if I remember correctly (I might be wrong here) so these effects would bias the denudation rate ratio, but not the commonly used concentration ratio. Some of this may be masked by using mean elevation for denudation rate calculation from the Cronus calculator. Could you use a pixel-based approach here instead? Although I like the comment by Dr. Braucher on Section 5.2/ denudation rate discordance, please keep in mind that the 10Be does not give a “true” denudation rate, but is also affected by decay in case the ratio is below the surface production rate ratio. Also, as decay time scales are different between Al and Be, the resulting integration time scales for D are different, which may result in yet another form of bias when comparing the two. Many thanks for your submission to GChron. With best regards, Hella Wittmann.