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Interactive comment on "The effects of ice and hillslope erosion and detrital transport on the form of detrital thermochronological age probability distributions from glacial settings" *by* Maxime Bernard et al.

Anonymous Referee #2

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Here is my review of "The effects of ice and hillslope erosion and detrital transport on the form of detrital thermochronological age probability distributions from glacial settings" by Bernard et al. In this paper, a glacial-hillslope erosion model is used to predict distributions of detrital thermochronological data. The model is applied to the Tiedemann glacier in British Columbia, where detrital thermochronological data had been previously collected from glacial moraines and glacial outwash (Ehlers et al., 2015; Enkelmann and Ehlers, 2015). The authors conclude that 1500 years are required to reach an equilibrium for detrital particle age distributions. The modeling exercise is

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interesting and the results nicely presented. However, the paper has one very important limitation that make the overall conclusions rather questionable, as I elaborate on below.

First and foremost, the model assumes that all the sediment transport happens within the ice – englacially or through sliding at the ice bedrock interface – using Eqn. 9 in the current paper. Unfortunately, it is often thought that the majority of sediments is transported out of the glacier through the subglacial hydrology system, which is not part of the model presented here. As explained by the authors, several processes govern sediment dynamics and bedrock erosion in the subglacial environment (e.g., Alley et al., 1997). Erosion obviously creates the sediments through quarrying and plucking as assumed and explained in the paper. Some of this sediment produced by glacial erosion can be accreted to the basal ice in locations such as overdeepenings and carried with the ice flow (e.g., Hambrey et al., 1199; Swift et al., 2018), but the majority of sediment produced by glacial erosion is transported by water flowing along the glacier bed (e.g., Walder and Fowler, 1994; Collins, 1996; Willis et al., 1996; Swift et al., 2005; Riihmiki et al., 2005; Delaney et al., 2018, 2019; Delaney and Adhikari, 2020), and this applies for both suspended and bed loads (Walder and Fowler, 1994). The residence time of sediments through such processes has yet to be determined, but one may expect it to be substantially shorter than the characteristic time estimated here, as sediment exhaustion is typically observed at a seasonal timescale. Instead, the characteristic time that is derived here corresponds to the glacier characteristic time, as the authors simply track sediments within the glacier. Equations have been developed to estimate such a characteristic time (e.g., Johannesson et al., 1989;Oerlemans, 2001, 2008, 2012; Roe & O'Neal, 2009; Roe et al., 2017; Herman et al., 2018), and all reach the conclusions that the glacier response time is proportional to its length divided by a characteristic velocity. If one takes a length of 15 km and a characteristic velocity of about 10 m/yr for the Tiedemann glacier – here the models show maximum velocities about 75 m/y – one obtains a characteristic time of about 1500 years. Therefore, the results obtained here do not tell us much about the time required to reach an equilibrium for detrital age distribution, but instead about the glacier dynamics. This is an important shortcoming of the current paper, but I think it could be addressed in a revised version. One possible option is to simply remove the discussions on time scale and solely focus on the shape of the SPDF. A second, more demanding option would be to include a subglacial hydrology model that accounts for sediment transport (e.g., Collins, 1996; Creyts, et al., 2013; Beaud et al., 2018; Delaney et al., 2018).

Second, I have difficulties understanding why the authors have specifically chosen not to apply the model to the existing data (Ehlers et al., 2015; Enkelmann and Ehlers, 2015), which they keep referring to. I think that the approach developed here would have great value and potential to improve our understanding of glacial erosion processes and/or better estimate the contributions from glacial and hillslope erosion, so why not try to fit the model to actual observations, especially when detrital data collected in moraines and glacial outwash are very similar? Our current knowledge about glacial erosion processes, or an erosion rule, is limited to a relationship between sliding velocity and erosion that has limited amount of observational support (Humphrey and Raymond, 1994; Herman et al., 2015; Koppes et al., 2015; Cook et al., 2020), so why not do it here?

Finally, it is surprising that the codes are not made publicly available. ESurf is an open-review and open-access journal. The data policy of the journal states: “In addition, data sets, model code, video supplements, video abstracts, International Geo Sample Numbers, and other digital assets should be linked to the article through DOIs in the assets tab.” I could not find any link or doi for the codes. The codes that are used here have been developed for more than 10 years, and are still not available in the public domain.

Specific comments: - Abstract: how does detrital thermochronology enables to avoid biases better than other methods? - 1/22: SPDF should be spelled out the first time it is used. - 2/3: the authors should be more precise on the order of timescales. (see main comments about existing work on characteristic timescales.) - 3/5-17: Detrital studies

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can only done appropriately, in my opinion, if the source area is properly described. The fertility or age distribution within the catchment must be characterized as much as possible. It is clearly not the panacea as the problem remains ill-posed, but at the very least the authors could make some references about the importance of having a good knowledge of the source area. - 4/9-10: The code should be made publicly available. See main comments. - 6/11: “We follow MacGregor et al. (2009)..” and everybody else (e.g., Braun et al., 1999; MacGregor et al., 2000; Tomkin and Braun, 2002, etc.) - 6/19: While there is some observations that support the link between sliding and erosion (e.g., Humphrey and Raymond, 1994; Herman et al., 2015; Koppes et al., 2015; Cook et al., 2020), there is no available data specifically for the chosen erosion rule, beyond the models of Uglevig et al. (2018). It would be good at least if the authors could acknowledge some of the observational basis for utilizing of this rule, or the relationship between sliding and erosion. - 6/22: I do not think subglacial fluvial transport should be ignored. See main comment. - 7/22: The flux and erosion rate (i.e., velocity) are the same equations (Eqns. 7 and 8) both scaled with constant that have the same unit. That cannot be. - 7/10: The authors assume that all the transport happens within the ice. See main comment on this assumption. - 8: Is there any information on the actual velocity of the glacier? The authors have chosen a relatively slow glacier, although the glacier is comparable to many alpine glaciers and I appreciate that the authors needed a site where some thermochronological data were available. This has some influence on the final result of the characteristic timescale, as it scales as the ratio between the glacier length and velocity. For example, Cook et al. (2020) showed velocities ranging from a few meters per year to several kilometers, implying that the equilibrium timescale estimated here is only applicable to the Tiedemann glacier. - 10/11: It would be useful to have more information about the geology. Ehlers et al. (2015) refer to Rusmore and Woodsworth, but the geological map is very large. Is the geology under the glacier truly uniform? I could not find this information. - 12/4: “limiting the ability” I do not understand why. Intuitively, more variations, such as kink in an age-elevation profile, should provide more information. - 12/14-16: The time to travel through the glacier is entirely dependent on the ice flow model, and it is likely it would be significantly faster if the subglacial hydrology would be included. - 19/4.1: See main comment about sediment transport. - 19/25: There are numerous papers on the glacier response time that could cited. See main comment.

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