Interactive comment on "Modelling the effects of ice transport and sediment sources on the form of detrital thermochronological age probability distributions from glacial settings" by Maxime Bernard et al.

Response to the reviewer.

We thank the anonymous reviewer for his review of the manuscript. We here response to the main concern of the anonymous reviewer and then we provide a point by point response to his comments.

We first point out that we changed the title of the manuscript to be more informative about the approach of our study.

The first concern is about the limited sediment transport processes considered in our study. We considered the transport of sediments on hillslopes and by ice but do not incorporate transport by meltwater. The anonymous reviewer pointed out that the majority of sediments is transported out of the glacier through the subglacial hydrology system. This may have important implications concerning the sediment transfer time and thus on the equilibrium time of the frontal moraine reported in our study (about 1500 years). We understand and share the concern of the anonymous reviewer about the role of subglacial hydrological systems in reducing the transfer time of sediments. We have revised the scope of our conclusions to discuss this important point. However, as we only consider the detrital signature of moraines, which are glacial sediment deposits and not fluvial ones, we do not expect a significant impact of fluvial transport on our results. We also discuss this point further in the revised version of the manuscript in section 4.2.

Our time for equilibrium of the frontal moraine (i.e. 1500 years) is similar to the characteristic time estimated by some authors the anonymous reviewer has cited. However, as mentioned earlier, we expect that the frontal moraine mainly reflects the glacier dynamics as the sediments that participate to build such glacial features mainly come from the ice (e.g. Winkler and Matthews, 2010; Bowman et al., 2018; Ewertowski and Tomczyk, 2020). Moreover, our discussion includes a spatial distribution on sediment transfer times which is not captured by the equation estimating the characteristic time. For these reasons we limited our discussion to sediment transfer times but revised the scope of our conclusion.

Initially, we did not aim to compare our results explicitly to the observations given that our synthetic steady Tiedemann glacier dynamics does not reflect the real Tiedemann glacier which is currently retreating. However, the comments of the two reviewers convinced us to consider such comparison. We now compare our modelled detrital AFT distributions to the detrital AFT distributions from Enkelmann and Ehlers (2015) which are coming from ice-cored terminal moraine (see Section 4.4).

We are aware that the erosion rule of Ugelvig et al. (2016) is mainly based on a mechanical model for bedrock fractures (Iverson, 2012), which has not been validated by a comparison with field data. On the other hand, empirical models that consider power laws between erosion rates and sliding velocity obtained by fitting natural data have little physical support and may lack some important processes. For instance, it is well documented that effective pressure plays a role in the quarrying process (e.g. Cohen et al., 2006). The equation of Ugelvig et al. (2016) is an attempt to incorporate this effect on a large-scale model. As discussed in Ugelvig et al. (2016), the strong dependency of erosion to the effective pressure (the power of 3) may be exaggerated. However, the results presented in Fig. 9, do not contradict the observations. If we consider the mean catchment erosion rate (1 mm yr^{-1}) and the maximum sliding speed of the main glacier (~60 m yr^{-1}, Fig. 3), we are in the range of values presented in Cook et al. (2020). Overall, we chose this model because of its mechanistic basis, despite its lack of a validation by natural data.

We share the concern of the anonymous reviewer about the availability of the source code. Therefore, we now make the code for iSOSIA and the external routine for constructing age distributions publicly available. (https://github.com/davidlundbek/iSOSIA).

Specific comments:

1. *Abstract: How does detrital thermochronology enables to avoid biases better than other methods?*

We did not make a comparison to other methods, but meant that thermochronological analysis on sediments (detrital thermochronology) potentially provides information from beneath the glacier, contrary to in-situ thermochronology for which sampling beneath the ice is generally not possible. (Page 1 – line 10).

2. *Page 1-line 22: SPDF should be spelled out the first time it is used.*

Done.

3. *Page 2 – line 3: The authors should be more precise on the order of timescales.*

This point has been removed from the abstract.

4. *Page3 – lines 5-17: Detrital studies can only be 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.*

We share the concern of the anonymous reviewer. Therefore, we have added a small paragraph in the introduction to mention the need of a priori knowledges on the spatial distribution of ages in the catchment and mineral fertility, to interpret the detrital thermochronology data (Page 3 – line 10-11).

5. *Page 4 – lines 9-10: The code should be made publicly available.*

The iSOSIA version to run models presented in this study, and the Matlab code to compute age distributions, is now publicly available.

6. *Page 6 – line 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.)*

We are aware that other authors considered the same assumption about ignoring the abrasion erosion, however to limit the number of references used in this study we now limit our citation to MacGregor et al. (2009).

7. *Page 6 – line 19: While there are 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 Ugelvig 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.*

We already answered this comment in the responses to the main comments.

8. *Page 6 – line 22: I do not think subglacial fluvial transport should be ignored.*

Indeed, we fully agree that subglacial fluvial transport should be considered in future studies. However, the role of subglacial fluvial transport in the building of frontal moraines may be not predominant. We discuss this point further in the section 4.2 of the new version of this study.

9. *Page 7 – line 22: The flux and erosion rate (i.e., velocity) are the same equations (Eqns. 7 and 8) both scale with constant that have the same unit. That cannot be.*

We thank the reviewer to have pointed out this mistake. This has been corrected in the new version of the manuscript.

10. *Page 7 – line 10: The authors assume that all the transport happens within the ice.*

We have answered this comment in the responses of the general comments.

11. *Page 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 need 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 kilometres, implying that the equilibrium timescale estimated here is only applicable to the Tiedeman glacier.*

To our knowledge, the only information on the velocity of the glacier comes from the ITMIX experiments (Farinotti et al., 2016, 2019). We share the concern of the reviewer about the characteristic timescale; however, we stress that showing the spatial distribution of sediment transfer times brings additional information relative to a single characteristic timescale, that may hide large variability. We also agree that the variability from glacier to glacier (Cook et al., 2020) may restrict the generality of the conclusion about the equilibrium timescale to the Tiedemann glacier, but we think of this conclusion more as an insight about the equilibrium timescale of the frontal moraine, and it relevance to the interpretation of detrital SPDFs. Future studies should explore this issue.

12. *Page 10 – line 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.*

The more recent and precise geological map of the Tiedemann glacier area is from Cui et al. (2017) and is available here. The main lithologies outcropping in the Tiedemann glacier catchments are granodiorite and orthogneiss as mentioned in Ehlers et al. (2015), and suggest low bias in fertility of apatite in the area.

13. *Page 12 - line 4: 'limiting the ability" I do not understand why. Intuitively, more variations, such as kink in an age-elevation profile should provide more information.*

A kink in an age-elevation profile give more information in terms of exhumation history. However, the ability to track the source of sediments depends on the uniqueness of the age-elevation relationship (i.e. one age = one elevation). For the AFT age-elevation profile, this is not the case as the same age can be interpreted as two very different elevation.

14. *Page 12 - lines 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.*

We agree that transport by meltwaters would decrease the transfer time of sediments to the glacier margin and should be integrated in future studies. However, we postulate that the debris composing frontal moraine are mainly the reflect of the ice dynamics, as their formation is mainly done through dumping of sediment from the ice surface, and bulldozing process (see section 4.2).

15. *Page 19 – line 15: There are numerous papers on the glacier response time that could be cited.*

We included references on the glacier response time in the new version of the manuscript, including Johannesson et al. (1989); Oerlemans (2001) Roe and 0'Neal (2009); Herman et al. (2018).

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