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 first wish to particularly thanks the Pr. Ehlers for his review of the paper. His comments are very relevant and have been very appreciated by the authors.

We took into consideration all the comments and we present our reply point by point.

First, we wish to point out that we changed the title of the manuscript to be more informative about the approach of our study.

GENERAL COMMENTS:

G1. *The general validity (beyond the simulations presented) of the authors results and interpretations is difficult to assess. If I understand the text and Table 1 correctly, the authors present a set of simulations with only one set of parameters. These results are nicely presented and described, but the primary interpretations of the paper (e.g. 1,500 yr equilibrium time for observed age-distributions; sediment trapping in tributary glaciers, hill slope vs. Glacial contributions to observed age distributions) are based on this single (?) set of chosen parameters. The results would be generalisable and more broadly applicable if a small set of additional simulations with a sensitivity analysis were included. For example, picking the least well constrained climate, ice, and hill slope parameters and picking reasonable values above and below what is shown in Table 1 should be considered to evaluate how robust the interpretations are in the text. I suggest these additional figures be shown in the supplementary material and then referred to in the main text, or highlighted in a new discussion section with a new figure that compare results.*

In the initial version of the study we presented the models with only one set of parameters for the simulated glacier dynamics. We share the concern of the Pr. Ehlers, so we performed a set of additional simulations with a set of parameter values that lead to a glacier size ~1000 m longer and shorter than the modelled glacier presented in the main study. We chose this range of glacier size difference because greater discrepancies would lead to very different glaciers dynamics and glacier thickness compared to the ITMIX experiments results, which is our calibration for the Tiedemann glacier. The additional models are now presented in a dedicated section in the supplementary materials, where we compare the results with the reference model (i.e. presented in the main text). Overall, the resulting detrital age distributions and equilibrium time for the frontal moraine of these additional models do not significantly vary from the reference model.

G2. *The results of Enkelmann and Ehlers (2015, Chem Geo) compared AFT ages across the ice cored moraine, outwash, and older moraines in front of the modern glacial terminus. The ice cored moraine and outwash samples were statistical identical (Fig. 6E), meaning that multiple ice cored samples when combined produce the same grain-age distribution as outwash. "slight differences" were observed between the individual ice cored moraine material in this study (Fig. 4 of Enkelmann and Ehlers). While the authors of this study (Bernard et al) explicitly say they are not trying to match observations in their study, in the discussion section they end making statements that do compare / evaluate the observations. There are suggested revisions related to this:*

G2-a. In the start of the paper when describing the previous observational studies in the area (and the bedrock data you use), please add text that makes it clearer how these data were collected and which data sets are or are not relevant to how the model is setup and why. For example, the Enkelmann and Ehlers 2015 data are well suited for comparison to the model results (they come from ice cored moraine material at the end of the glacier). In contrast - the Ehlers et al. 2015 data are from

glacial outwash and are not suitable for comparison to how the model is setup. The modelling approach does not track particles through the subglacial hydrologic system. No comparisons to this later study should be made in this manuscript.
G2-b. The concluding discussion section (4.4) and Fig. 10 make model predictions that are more or less comparable to the observations of Enkelmann and Ehlers, 2015. Although some qualitative comparisons are made in the text, the manuscript would be much stronger if the data from Enkelmann and Ehlers, 2015 were also plotted in Fig. 10c and similarities / discrepancies are discussed.

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 a comparison. As mentioned by the Pr. Ehlers, as our sediment particle sampling is focused on the frontal moraine, we only compare our results with the detrital age distributions from Enkelmann and Ehlers (2015) in which the samples come from the ice-cored terminal moraine. Such a comparison, with statistical tests, is presented in the revised version of the manuscript.
At the start of the paper we clarify the provenance of the samples from Enkelmann and Ehlers (2015) and Ehlers et al. (2015). We have also specified that we produced our bedrock ages according to the age-elevation from Enkelmann and Ehlers (2015) for the AFT data and from Ehlers et al. (2015) for the AHe data. In the revised version of the manuscript, the comparison of our detrital age distributions with the AFT data of the ice-cored terminal moraine samples from Enkelmann and Ehlers (2015) is made in section 4.4 (i.e. Implications for detrital sampling strategies) with the figure and the results from statistical tests presented in the Appendices. In that section we specified the provenance of the samples from Enkelmann and Ehlers (2015) (i.e. ice-cored terminal moraine).

G3. The model setup and description needs to be clearer. It is not clear how erosion is done for hill slopes and how ages from hill slopes are mixed with the glacial sourced ages. It would greatly improve a readers understanding of this study if the coupling (and particle tracking) between hill slopes and glaciers was better explained, and also the sensitivity of their results (e.g. Fig. 7) to some of these parameters (e.g. see also comment G1 above). I also don't understand exactly how the 'uniform' erosion model was calculated with the ice model (see detailed comment below). The methods sections needs to explain the model setup for all this better. Section 2.1 (end) explains a non-linear diffusion model is used for hill slopes, but it's not described in enough detail how the sediment transport is done in this part of the landscape. The paper later on nicely explores how hill slope vs. Glacial sediment sources impact detrital cooling ages. Thus, some additional text in the methods section would make it much easier to understand the model results. As it's written now, I could not reproduce your results if I wanted to.

We considered the comment of the Pr. Ehlers and have made the model setup and description clearer having entirely rewritten section 3. In the case of uniform erosion, we specified the production of particles in each cell of the model for both hillslope and glacial sources. In the case of non-uniform erosion (section 3.5), the erosion is determined by the erosion laws presented in section 2.1. The thermochronological age of a particle is based on the source location elevation of that particle and the appropriate age-elevation profile. Thus, each particle carries an age which is not modified during the particle transport. The mixing of particles (i.e. ages) is the result of the transport pattern. However, during the sampling process we can choose to identify and sample particles according to their source location (hillslopes or glaciers). The sensitivity of parameters for figure 7 are now presented in the supplementary materials.
A particle is formed once the erosion products in a cell reach a thickness threshold (H_s = 0.01m). Then, each particle is transported away according to the transport laws for hillslopes (Eq. 8) and for ice (Eq. 9). This is now explained in the start of section 2.2.

G4. Comparison of distributions should include statistical tests. Section 4.4 (see also detailed comment 26 below) presents an analysis of how representative point samples across a moraine compare to the mean of all samples and the bedrock distribution. This section is very useful for understanding how and where people could sample. However, the analysis does not statistically *evaluate if the different synthetically sampled distributions are the same or not. Visual / qualitative comparisons of distributions is dangerous, and I suspect (based on experience) that several of the distributions show in Fig. 10c will be statistically identical. If this section remains in the paper (which I hope it does), it's important the authors conduct a simple KS or Kuiper test to see if in fact the different distributions show in figure 10c are different or not. This comment could also potentially impact the conclusions presented in section 4.5. The text in section 4.4 and 4.5 is good, but simply needs support from a more quantitative comparison. Finally, as mentioned above for comment G2, this model result should be compared to observations that were collected from roughly this same area (Enkelmann and Ehlers, 2015).*

In the initial version of the manuscript, our comparative analysis was primarily qualitative as our goal is to describe the form of the detrital age distributions and link it to processes (i.e. sediment transport and erosion). In section 4.4, we compare the detrital age distributions over 4 regions within the frontal moraine to the catchment bedrock age distribution, and discussed the discrepancies that occur. The validity of statistical tests is not a given (e.g. Vermeesch, 2018), partly our concern regarding statistical tests. However, in the revised version of the manuscript we use two statistical tests (Kolmogorov-Smirnov and Kuiper tests) to compare our modelled detrital age distributions with the catchment bedrock age distribution. Given, this, we also highlight some contradictions between the inferences made from the two statistical tests.

G5. The text is in general well written and clear. However, there are many small wording issues / grammatical problems throughout the text (e.g. missing articles, subject/ verb agreement) that are understandably hard to catch for a non-native speaker. I recommend a native speaker/co-author give the text another thorough read to correct these.

We thank the Pr. Ehlers to have pointed out such wording and grammatical issues. We brought a particular attention to these issues in the revised version of the manuscript.

SPECIFIC COMMENTS:

0. *For clarity - all axis labels with 'Age' on them should say what age (e.g. "AFT Age") you are plotting since you work with two different systems in this study.*

Done.

1. *Abstract should make it clear if the langragian particle tracking is only for ice flow, or subglacial water.*

We now state in the new version of the manuscript (page 1 – line 15): "Sediments are tracked as Lagrangian particles formed by bedrock erosion, where their transport is restricted to ice or hillslope processes until they are deposited".

2. *Page 2 paragraph starting at line 17: Also relevant to the content of this paragraph, and the study area investigated is the study by Yanites and Ehlers 2016 that documents how glacial sliding relates to bedrock thermochronometer ages (in a neighbouring valley in the Coast Mountains). Yanites, B. J. and Ehlers, T. A.: Intermittent glacial sliding velocities explain variations in long-timescale denudation, Earth and Planetary Science Letters, 450, 52–61, doi:10.1016/j.epsl.2016.06.022, 2016. Also - this manuscript is highly relevant to the following previous work and the authors should consider citing it in the introduction or discussion. Herman,F.,et al., 2018. The response time of glacial erosion. JGR - Earth Surface. https://doi.org/10.1002/2017JF004586*

We thank the Pr. Ehlers for suggesting the studies relevant for our manuscript. We have added a small paragraph that mention the results of the study of Yanites and Ehlers 2016 (Page 2 -lines 20-24).

We also now mention the study of Herman et al. (2018) when we discuss the kinematics of our sediment transport model (Section 4.2) and the characteristic timescale for the glacier dynamics (Page 20 – lines 28-29).

3. *Page (pg) 3, line (ln) 12 - It might be worth clarifying here for readers that the Enkelmann and Ehlers 2015 studied sampled ICE across the ablation zone, and the Ehlers et al. 2015 study sampled glacial OUTWASH. This would help readers understand why you can not directly compare model predictions to one of these sets of observations..*

We have made this clear by modifying this sentence. (Page 3 – lines 16-19).

4. *Pg5, 6 model description. Hydrology effects on sliding are described here, but please also add a sentence or two that says if this is also included in the particle tracking for making SPDFs of cooling ages. Maybe you address this later.*

We have added the sentence "We stress again that we neglect the transport of particles by meltwater and focus only on the ice and hillslope processes." (Page 7 – lines 22-23).

5. *Pg5, 6 - It is also important that this section says how you have calibrated the model for the subsurface hydrology. This aspect of the model is likely very important for how the data are interpreted, so some text on this aspect would be useful.*

We have added the sentence "We calibrated our hydrological model by a trial-error process by varying k_0 (Eq.2) to lead to a reasonable value of basal ice sliding velocities (Table S1)." We have also added a short description in the supplementary material associated to Table S1 to show the parameters used for our calibration approach for the Tiedemann glacier. (Page 6 – lines 7-8, and Supplementary Table S1).

6. *Pg.6 - Your approach assumes all sediment comes from quarrying. I'm more or less ok with this (note fine sediment fractions were present in what we sampled for this glacier). However, please explain here if you account for the comminution (breaking down) of plucked material. Why could this be important? If 20x20cm rock is plucked in the upper reaches of the catchment it will break down during transport and provide fine grain material that was sampled. If a 20x20 cm rock is plucked from 100 m from the sample point - it wouldn't show up in sample. The material sampled for the Tiedemann glacier ice cored moraine and outwash was a 'bulk' sediment sample, but with nothing greater than _2x2cm size in it. I would be great if your modelling approach accounted for comminution, but I'm guessing this is not the case. So this effect needs to be acknowledged and the potential implications of it discussed in a model caveats section.*

We do not account for the comminution in our transport model as this would lead to computer-memory issues due to the too large number of particles tracked by the model. We have added a short paragraph to discuss the issue of such process in the sampling approach, in section 4.1 which deals with the limitations of our modelling approach (Page 20 – lines 12-15).

7. *Pg 6/7 - section 2.3. As indicated above, please explicitly state that water transport of detritus is not accounted for. Fluvial systems mix sediment very efficiently and the flow rates on these outlet rivers are high (rounded cobbles were in the river bed and appeared transported by it).*

This comment is similar to the specific comment 4 that we have already answered to (Page 7 – lines 22-23). We also remind this issue page 20 – line 16.

8. *Pg. 8 section 2.3. Please add some text saying how the glacial mass balance (and climate inputs) are calculated and refer to your table.*

We have added a Table (S1) in the supplementary materials and we explain how different parameters values have been chosen to calibrate our model. Furthermore, a short text was also added to describe the computation of the glacier mass balance (Supplementary material – Table S1).

9. *Fig. 4c, d - I suggest labelling the x-axis with the age plotted (e.g. "AHe Age" for c). Also - for the caption, mention what uncertainty you used for making the PDFs since this influences the smoothness of the curves relative to panel B. Caption should also explicitly say the data come from glacial outwash, not moraine.*

We have changed the figure label accordingly. However, the data used for the building of bedrock age distributions are the in-situ central ages of the AHe and AFT systems, through the age-elevation profiles, from Ehlers et al. (2015) and Enkelmann and Ehlers (2015) respectively, and thus are not from the glacial outwash sample presented in Ehlers et al. (2015). We describe these bedrock SPDF Page 12 – section 3.1.

10. *Pg12 ln7. Please say under what conditions the equilibrium state was calculated, and how closely it matches the present-day thickness and length of the glacier.*

We explain our calibration approach for the synthetic Tiedemann glacier Page 8 – lines 15-18. We also refer the readers to Figure S3, which shows the comparison between the iSOSIA model and the results of the ITMIX experiments (Farinotti et al., 2016) that predicts the ice thickness of many glacier around the world, as the Tiedemann glacier.

11. *Fig. 5. Please provide a more descriptive caption of what model this is at the start of the caption. Also - what are you actually doing with the 'hillslope' vs. 'glacial' parts of the catchment (Fig. 5a). It's not clear from the text (or caption) if you are also feeding hill slope material into the SPDFs calculated.*

Changes in the caption of Fig. 5 have been made to make it clearer (Figure 5 and Page 12 – line 26). We also added a sentence in the main text: "Particles are sampled independently of their source origin (hillslope vs glacial, Fig. 5a)."

12. *Pg13, ln1-2. Reword sentence please.*

Done. (Page 15 – lines 6-7).

13. *Fig. 6 - caption needs a starting sentence saying in general what is plotted and what model it comes from. Also- again for panel c, d - indicate the age type ("AHe Age") on the x-axis. Please do this for all other figures if this is also the case. It makes it much easier to read the figures quickly.*

Additional sentence and axes labels have been modified according to the comment for all figures.

14. *Pg.14 ln1. Please explain better what you mean by "model with uniform erosion". I'm confused because I don't understand how you made the ice model have uniform erosion - and where the uniform erosion was applied (e.g. Hillslopes and glacial areas?). Fig. 7e kinda gets at this, but the text should explain it better. Thanks.*

We forced uniform erosion by setting the production of particles in each model cell with a constant erosion rate. The erosion is applied in the entire model. The different colours in Figure 7a (i.e. red and blue) identify the sediment sources (hillslopes and glaciers). According to this comment and the following one, section 3.3. was confusing. We have therefore entirely rewritten it to make our results clearer. The model with uniform erosion is now explained more specifically at page 15 – lines16-17.

15. *Pg. 16 top. After reading this page (related to previous comment) I'm still confused and some clarification is needed on this paragraph and what was actually calculated. The start of the*

paragraph needs to explain better what the objective of this comparison is (hillslope vs. glaciers) . Also ln3-4 are confusing because I thought this section only about uniform erosion, but this sentence says you are comparing a detrital SPDF to the uniform erosion model. Are you talking about the OBSERVED detrital SPDF? Perhaps make it clear throughout the text by always using 'observed' vs. 'modeled' detrital SPDFs

As mentioned in a previous comment we have rewritten Section 3.3. We compared the modelled detrital SPDF with the modelled catchment bedrock SPDF resulting from the uniform erosion model. We differentiate between the detrital SPDF and the catchment bedrock SPDF clearer.

16. *Pg16, ln10+. This paragraph is also unclear. After reading all of section 3.3 - I'm confused as a reader. Please rewrite this section to make it clearer of a) what is the logic behind the experiment / comparison conducted, b) what is the first and second order main trends in the results and what data / model results (bedrock vs. Detrital you're looking at, and c) summary sentence(s) with the key observation to take away.*

We took into consideration this comment and have rewritten section 3.3. The rationale behind the model of uniform erosion was to characterise the form of the detrital SPDF at the glacier front resulting from a continuous spatially uniform production of particles (which differs from the model presented in Figure 5 that consider only a pulse of particle production). In this case, we should expect some effect due to the transfer time of sediment particles as we can see with the shifting of the mean detrital SPDF toward younger ages (Fig. 7). We discussed this effect in section 4.2.

17. *Maybe I missed it earlier, but after reading the results section - I think the methods section needs to be expanded some to explain how you look at (or calculate) hillslope vs. Glacial contributions to the detrital cooing ages.*

The particles can be sampled according to their source origin (each particle as a tag according to the source when forming that can be retrieved during the sampling process). We now specify this at the end of section 2.4 (Page12 – line 4).

18. *Pg16 ln 29. I don't understand how "we computed a new: : :". How did you compute this? Assuming uniform erosion? Using a diffusion based hill slope transport law? Please elaborate.*

We calculated a new bedrock SPDF using the age-elevation relationship convoluted to the hypsometric distribution of the hillslope sources, as we assume uniform erosion. We now refer to this bedrock SPDF as the "hillslope bedrock SPDF" in the main text. (Page 16 – lines 10-11)

19. *Pg.17 ln5-10. This result is entirely dependent on the assume hill slope transport law used, and diffusivity,: : :.right? Perhaps mention this, and also make it clearer (per my previous comments) how the curve in Fig. 7 is calculated.*

Figure 7 displays results obtained with a uniform erosion model. There is therefore no dependency on the erosion law used for the hillslopes. Concerning a potential sensitivity to a transport law, because we consider steady-state SPDFs, our results are assumed independent of the transport processes or rate. We make this clearer by performing sensitivity tests by varying the diffusivity value used in this study (K_h = 5 m² yr^{-1}). The results of these tests are presented in the supplementary materials. The mean detrital SPDFs (in the frontal moraine) resulting from particles originating from hillslopes are computed following the method presented in section 2.4. The hillslope bedrock SPDF is computed using the age-elevation relationship and the hypsometric distribution of the hillslope sources.

20. *Pg. 18 ln1. Several times in the paper you refer to or tune the model to an erosion rate 1 mm/yr. Why did you use this value? This should be explained in the methods section.*

We have used a value of 1 mm yr^{-1} as it is in the range of natural values and it allows a continuous production of particles while maintaining a reasonable simulation time. We have added these sentences at the top of section 3.3 (Page 15 – lines17-18).

21. *Pg.18 ln1-4. I don't understand this sentence and where these numbers you cite are coming from. For example, where is 31 mm/yr coming from? Where are the uncertainties in the numbers later in the sentence coming from?*

We meant that despite the mean erosion rate is 1 mm yr^{-1}, local deviations to this average value occur due to the heterogenous pattern of erosion. The value of 31 mm yr^{-1} is the maximum local erosion rate shown in Fig. 9 (in the revised version of the manuscript). Next, we calculated the standard deviation for each sediment sources (hillslope vs glacial) to the average erosion rate of 1 mm yr^{-1}. We clarify this in the revised version of the manuscript. (Page 18 – lines 17-19).

22. *Pg. 18. Ln6. Please rewrite this sentence. I don't understand it.*

The sentence has been rephrased and split into 3 sentences (Page 18 – lines 19-21)

23. *Pg. 19 ln1. "traducing"? Not sure what you're trying to say.*

This sentence has been changed to clarify this.

23. *Pg 19 ln20. Please expand this thought (particle tracking is in ice, not sub-glacial drainage). I would describe in general (qualitative) terms how the results could differ if subglacial outwash was sampled (e.g. how Ehlers et al. 2015 sampled).*

We have done so, see page 20 -lines 16-21.

24. *Pg. 19. Ln 24-25 "spatial erosion pattern can be biased on the detrital SPDF (e.g. Ehlers et al. 2015)". Please remove the "e.g. Ehlers et al. 2015". You may be correct that there is a bias, but you haven't shown this because the Ehlers et al. samples were from OUTWASH, not from ice. Also - your timescale arguments for bias later in the paragraph would likely be severely decreased if outwash is sampled because transit times of water to the outlet are significantly faster than for ice. So, please either show that Ehlers et al. 2015 have a bias in their outwash sample interpretation, or remove the reference to this paper to be fair. Final note concerning the general conclusions you are trying to make here about timescales - while your description is accurate for the simulations you present, it's hard to know if this is really a general result with any sensitivity tests to your model parameters presented in the study. Please consider adding sensitivity tests in the supplemental material and referring to them in the text. Table 1 - please be more specific about what you mean by "variable" in some rows. I think what you mean is that these variables are 'internally calculated'*

The reference has been removed. The sensitivity tests have been added in supplementary materials, section 2. They show that the time to reach equilibrium, neglecting fluvial sediment transport, is of the same order than presented in the text (10^3 years), when varying the model parameters. Obviously, considering fluvial transport would decrease drastically the response time of sediment transport. Yet, because we are only considering the detrital signature of moraines, which are glacial sediment deposits and not fluvial ones, we do not expect a significant impact of considering fluvial transport on our results. We have revised the scope of our conclusion to account for this important point (Page 20 – lines 28-32 and Page 21 lines 1-11). Moreover, Table 1 has been modified according to the comment: "Variable" is replaced by "Model outcome".

25. *Pg. 21 ln29-30. This isn't really a surprise is it that moraines will more closely represent glacial erosion than hill slopes? Also - doesn't this statement and paragraph sort of contradict the previous section where you say there would be a bias towards low elevations in SPDFs? Maybe*

I'm missing something here - but perhaps more text (in the previous section about biases) relating to what section 4.3 is saying would help clarify things more.

In the case of debris-cover glaciers where the major sediment sources are hillslopes, thus moraines likely represent hillslopes erosion. We want to point out here that because glacial sediment particles are likely to be produced close to the ice streamline showing high velocities, the transfer time of such particles will be small compared to supraglacial sediment particle. Thus, in the case where the amount of erosion is the same for supraglacial and subglacial sources, we expect that the glacial sources to be over-represented, and thus the detrital SPDF to be bias toward younger ages if they are located beneath the ice (Page 23 – lines 13-16).

26. Pg.22 section 4.4. This is nice that you done this analysis. However, to do this type of comparison you need to test if the distributions show in Fig. 10c are in fact statistically different. To do this, you need to apply a KS (or Kuiper) test of the distributions.
Please include this type of analysis to see if these distributions really are in fact different at the 95% confidence level. This comment also potentially impacts the interpretations presented in section 4.5.

We already reply in the general comment G4.

27. Pg. 24 ln 1-2. This conclusion is correct for the simulations presented, but how variable is this result if there are variations in some of the model parameters. A sensitivity test of model inputs would help readers see if this result is general, or specific to your model simulations.

We have made such tests and discussed the results in section 2 of the supplementary materials. The results do not significantly differ from those presented in the main text.

28. Pg. 24 ln22-23 "However, we also emphasis : : :" This statement should be removed or significantly expanded & justified. How many grains should be sampled is a significant topic on it's own, and this study doesn't address this. The typical "100" grains minimum that people site from Vermeesch is not actually correct for these types of detrital samples (we have a paper in preparation that goes through the statistics of number of detrital AHe samples needed for different catchment sizes and it's complicated).

We removed this sentence from the conclusion.

29. Supplementary materials. There is useful material presented in the supplement, but the figure captions are a too sparse for some figures. Please expand the figure captions some to be more descriptive of what is shown. For example, what model simulation is shown in the figure, and perhaps also what section of the text the figure is relevant to.

The captions of the figures have been rewritten, expanded and clarified for better description of the figures.

Reference

Vermeesch, P.: Dissimilarity measures in detrital geochronology. *Earth-Science Reviews*, *178*, 310-321, 2018.

Code availability

The version of iSOSIA (iSOSIA_3.4.7b) used for this study, as well as the external routine to compute the detrital age distributions, are now publicly available at: https://github.com/davidlundbek/iSOSIA.