

Review 1

The authors have addressed all of my comments satisfactorily, with the exception of my main concern regarding the use and interpretation of the MCMC inversion framework. The authors use Equations 5–7 to invert their samples in an underdetermined problem. As expected, the posterior distributions show strong correlations among parameters (for example, in Figures 5, 6, and S3). In this context, I do not understand what additional insight is gained from the MCMC inversion framework relative to direct exploration of the analytical solution. Since the authors present the analytical equation (Eq. 5), it seems that they could instead solve this equation directly over the range of parameters deemed reasonable. The resulting parameter trade-offs would then define the family of admissible solutions consistent with the measured nuclide concentrations.

My concern is therefore not necessarily with the use of MCMC itself, but with the interpretation of the posterior distributions in a problem that appears to be only partially identifiable. In such cases, MCMC may still be useful for mapping the admissible solution space, but posterior density variations should not automatically be interpreted as evidence for uniquely preferred or physically meaningful solutions. Instead, apparent maxima may arise from posterior geometry, prior truncation, or differences in parameter sensitivity and scaling, rather than from genuine resolution of the underlying parameters.

I suspect this may be happening in the present manuscript, which is otherwise well executed. I illustrate this concern with a figure I recently generated using an almost identical inversion framework.

Using a formulation equivalent to Equation 5, but simulating a step change in erosion from E_1 to E_2 at time t , I calculated the analytical solution for the combinations of parameters that explain the in situ ^{14}C and ^{10}Be concentrations of a single sample. I then ran a Hamiltonian gradient based MCMC to estimate the posterior distribution. Similar to the results presented by the authors in their rebuttal, parameter recovery is generally good in the sense that the posterior follows the analytical solution closely. However, the posterior nevertheless shows a clear posterior mode. Based on the analytical solution, we know that this mode is not evidence of unique parameter identification. Rather, it is better interpreted as a pseudo-mode arising in a practically non-identifiable, or only partially identifiable, model with ridge-like geometry, potentially amplified by prior truncation and by differences in parameter sensitivity or scaling (e.g., the extent to which a change in one parameter affects the solution relative to a change in another) (Raue et al., 2009, 2013).

A similar behaviour may be present in Figures 5 and 6 of the main text, and in Figure S3, which looks very similar to the example shown here. Consistent with these known issues in non identifiable inverse problems, the posterior distributions in Figure 4 span much of the prior space, yet still show maxima that the authors interpret as meaningful. Based on the inversion framework presented, however, I do not see sufficient justification for treating these maxima as physically interpretable regions of higher support. Even if the sampler concentrates in some regions of parameter space, this concentration should not automatically be interpreted as evidence for preferred solutions when the problem remains practically non-identifiable.

My concern, therefore, is that several of the interpretations derived from the posterior distributions may reflect inversion artefacts rather than features uniquely supported by the data. I therefore encourage the authors either to justify more explicitly why the MCMC framework provides additional insight beyond direct exploration of the analytical solution space, or to change the interpretation of posterior maxima and frame the results primarily in terms of admissible parameter trade-offs rather than preferred parameter values.

Response to Review 1

Part I: Introduction

We appreciate the critical evaluation of the inversion framework and agree with much of the comment. We fully agree that in under-determined problems there are parameter trade-offs and that these are important to consider. These trade-offs can be formalized statistically using joint probability distributions, which show the likelihood of different parameter combinations. We have added more discussion of trade-offs in the description of the inversion:

Line 274-276: “The joint-distribution describes the likelihood of different parameter combinations and allows the trade-offs between parameters that are inherent in under-determined problems to be assessed.”

Importantly, it is the full joint posterior distributions (i.e., the admissible parameter space) that we interpret as physically meaningful, rather than the posterior modes. However, to discuss the results quantitatively in the text we need to use summary statistics. For ease of reading, we chose to give only the distributions’ medians in the text and refer to a table for the inter-quartile ranges and the figures for the shapes of the key distributions. We would be happy to add the interquartile ranges to the text if this would improve the clarity of this point.

We also agree that Eq. 5 is analogous to a model with a change in denudation rate, and that the parameter trade-off is similar. This trade-off is not shown in our existing figures, so we have added one to the supporting information and a note in the main text. We believe that this does not affect the robustness of our conclusions for reasons discussed in Part II and III of this response.

Line 365-367: “There is a trade-off between the time of the change and the overburden thickness after the change such that ages greater than ca. 1.4 Myr are allowable only at the lowest possible overburden thicknesses (Figure S.3).”

Finally, we agree it is possible to solve the equations over the full range of parameters deemed plausible and to find the solutions that most closely fit the data. This is often referred to as the “brute-force approach” to parameter estimation because it requires substantial computational power. We demonstrate that this approach yields identical results to our MCMC inversion in Part II.

Advantage of MCMC

The advantage of MCMC over the brute-force approach is that it is more computationally efficient. To illustrate this, consider a 4 parameter problem, such as that defined by Eq. 5. Computing the solution at 1000 discrete intervals for each parameter would require solving for the ^{10}Be and ^{26}Al concentrations $1000^4 = 10^{12}$ times. The MCMC approach, even with 10 chains seeking 100,000 accepted parameter combinations each and with 40,000 burn-in iterations requires solving for the concentrations only 1.4×10^6 times, which is about a factor of 700,000 less. This is useful during the initial stages of model exploration because an initial solution using, say, 10,000 accepted iterations from 2 or 3 chains, can be computed rapidly. This allows different models and scenarios to be evaluated quickly.

An additional advantage of MCMC is that it enables most of the computation to be focused on exploring the highest probability part of the parameter space. This allows for a higher resolution

sampling of the region of interest than would be feasible with the brute-force approach. This is especially important for LS1, where the acceptable solution space for the denudation rate is quite small, and slight changes in the denudation rate make a big difference for the other parameters. We have tried to clarify our description of the MCMC inversion further at lines 272-285.

“Equations 5-7 require solving for 3-5 parameters from two nuclide measurements and are thus under-determined. Therefore, we use a Markov Chain Monte Carlo (MCMC) inversion to estimate the posterior joint-distribution of the parameters (Andersen et al., 2023; Hippe et al., 2021). The joint-distribution describes the likelihood of different parameter combinations and allows the trade-offs between parameters that are inherent in under-determined problems to be assessed. We adopt the inversion method of Andersen et al. (2023), which uses the Metropolis-Hastings algorithm. In this approach, parameter combinations are iteratively proposed and nuclide concentrations forward modeled. Combinations that more closely match the measured nuclide concentrations than the previous combination are accepted, whereas those that do not are accepted or rejected based on the degree of misfit and a randomly varying acceptance threshold. Given a sufficient number of iterations, the distribution of accepted parameter combinations will reflect the posterior joint probability distribution, with more accepted samples coming from the higher probability regions in the parameter space than the lower probability regions. The prior ranges used (i.e., allowable parameter bounds) are given in Table 3. We invert the samples individually, allowing all parameters to vary, using 10 chains. We discard the initial 4×10^4 accepted parameter combinations for each chain as a “burn-in” phase. Afterwards, the inversion is run until 10^5 accepted iterations per chain are achieved, for a total of 10^6 accepted parameter combinations.”

We believe that the other issues raised by the reviewer are important considerations for inverse-problems, but they are not specific to MCMC. For example, prior truncation effects can happen in the brute-force approach, as this still requires specifying parameter bounds. Likewise, the geometry of the solution in parameter space and parameter sensitivity are controlled mostly by the mathematical structure of the underlying model rather than the parameter estimation technique. We report posterior medians (50% of probability greater and 50% less) and inter-quartile ranges as the summary statistics (Table 3) rather than the posterior modes to avoid issues with preferring pseudo-modes.

Part II: Specific inversion results.

Stream and valley-bottom sediment samples (Eq. 6 and Eq. 7):

For the inversion of Eq. 7 illustrated in Fig S.3, it is true that the problem is essentially non-identifiable. We referred to this problem as “ill-posed” at Line 364, but we essentially mean non-identifiable, which may be a better term here. We have changed this terminology in the text:

Line 375-376: “This indicates that the problem, which requires inverting for 5 parameters from two nuclide measurements, is non-identifiable”

The only claim we make on the basis of the inversion results of Eq. 7 is that long-duration burial in the valley bottom is probable. The same conclusion would be drawn by considering the problem in terms of simple burial dating. For example, if one assumed that the valley-bottom sediment was buried with concentrations similar to the modern stream sediment and calculated a simple burial age while ignoring post-burial production this would give 1.4 Ma.

Eq. 6 is slightly better constrained because the model involves fewer parameters. However, there are trade-offs between parameters as noted by the reviewer. We have already presented the key cross plots

directly in the manuscript (Figure 6) to allow the reader to visually evaluate these trade-offs. We have added a verbal description of the trade-offs in the text and a cautionary note:

Line 373-374: “The peat thickness and erosion rate covary negatively. Additionally, the posterior distribution is multi-modal and should be treated with caution (Section 5.4).”

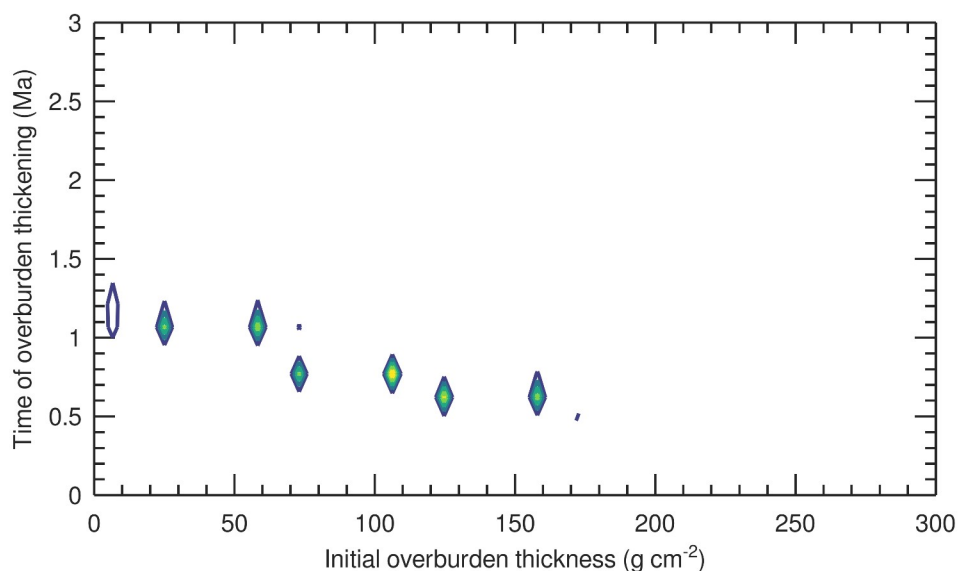
More generally, we try to frame the inversion of the stream sediment as loosely supporting evidence rather than a main point of the manuscript. The results appear to be consistent with the behavior we find on the hillslope scale (slow erosion and thick peat), but there are issues with catchment-averaging not working in this context due to the non-linearity of the shielding signal, significant radioactive decay, and reworking of long-buried valley bottom sediments (section 5.4). We would like to emphasize that the inversions for Eqs. 6 and 7 should not be over-interpreted and added the text:

Line 578-579: “Therefore, the results from the stream sediment should not be over-interpreted.”

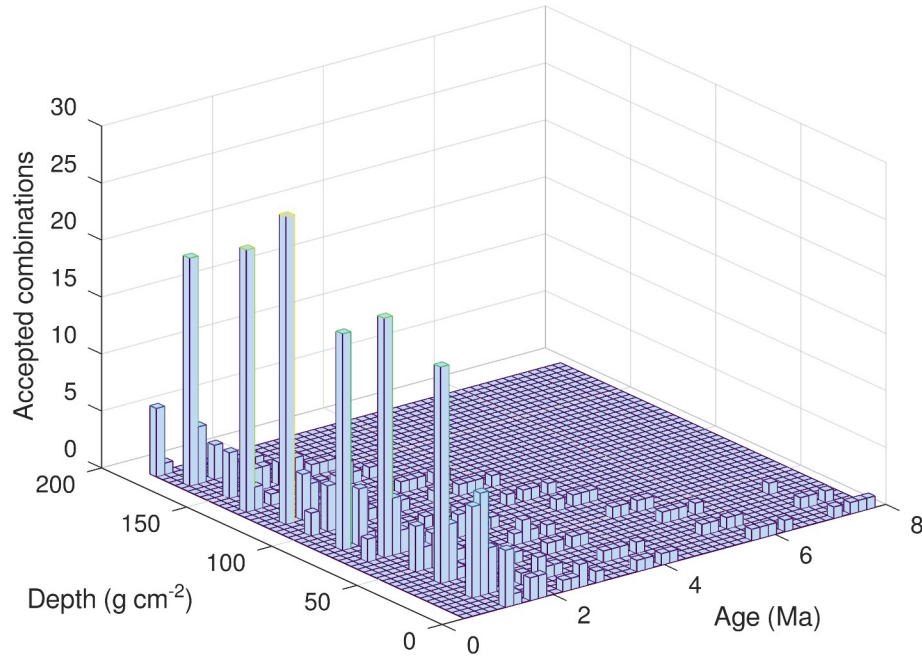
Hillslope Samples (Eq. 5):

On the other hand, we disagree that the results of the inversion of Eq. 5 presented in Figure 4 are not meaningful because of strong parameter trade-offs in a non-identifiable system. As discussed in our previous response, the same results are achievable if we simplify the problem to 2-variables, making it a determined system for which an exact solution is possible, and then solve by least-squares. We believe that the good agreement between the results obtained by the MCMC inversion and the least-squares solution to the determinable problem supports that the inversion results are robust.

For LS1, the problem cannot be reasonably simplified to 2-variables because the sample plots below secular equilibrium. Thus, for this sample, we have also tested the brute-force approach for a computationally tractable 50 intervals for each parameter over the same prior range used for the MCMC. Accepting all combinations of parameters that produce both ^{10}Be and ^{26}Al concentrations that are within 2 sigma of the measured values and plotting the results using the same binning and contouring as in Fig. 5 produces:

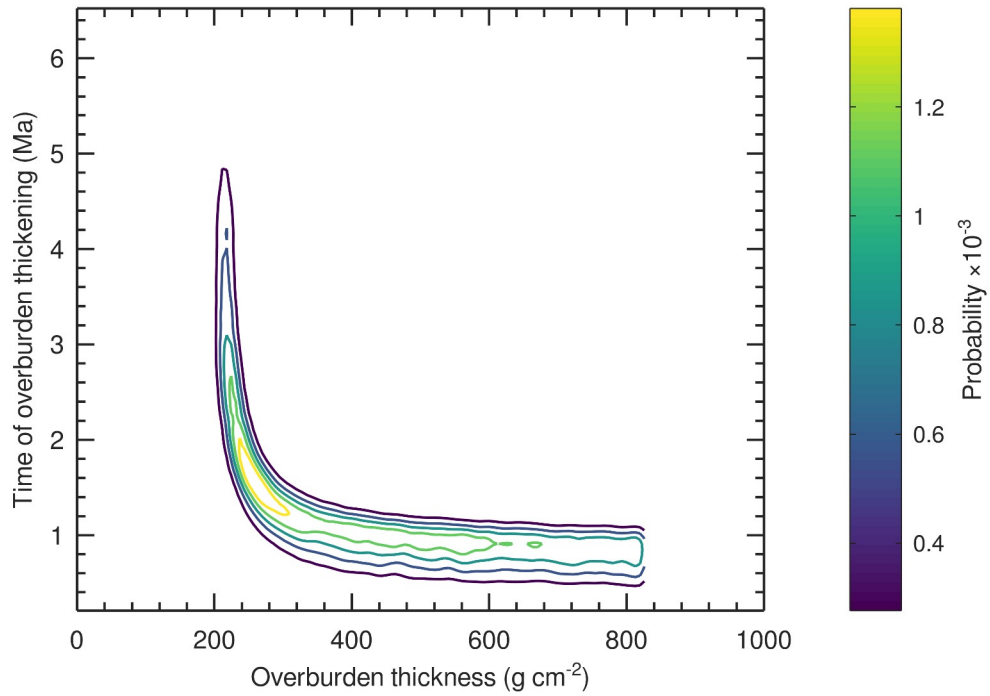


Or, if viewed as a histogram:

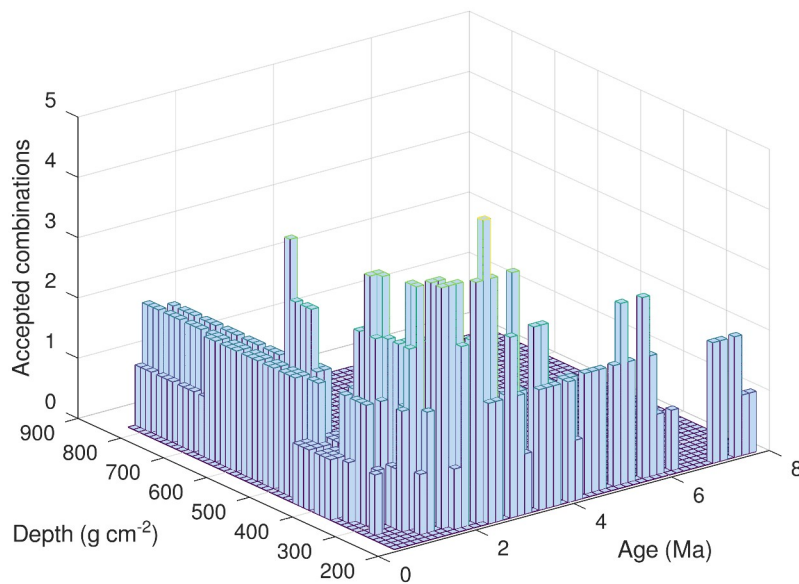


These results are nearly identical to those from the MCMC in Fig. 5, with a strong clustering around ~ 1 Ma and a diffuse tail towards greater ages (this is not evident on Fig. 5 due to the binning interval, but the tail factors into the summary statistics and leads to the median being greater than the mode as discussed in section 5.3). The modes from the brute-force approach and MCMC are within 1 time-step, (1 Ma in the case of brute-force and 0.93 Ma for MCMC).

For LS1, a plot of overburden thickness after the change vs. the time of the change shows roughly the same trade-off as illustrated in the figure in the comment, where the ages above ca. 1.4 Ma that form the diffuse tail in the previous figure are permissible only for the lowest overburden thicknesses. This tail tapers off above 5 Ma (see following figures):

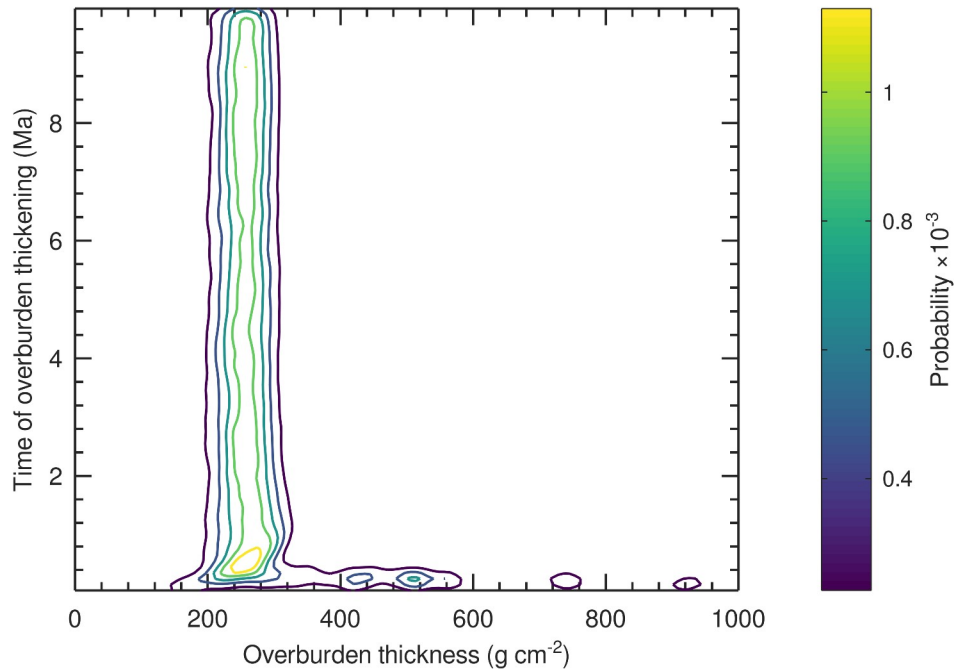


Once again, the brute force approach produces nearly identical results:



We have added a note in the text about the trade-off at low overburden thicknesses and a figure in the supplementary information (new Figure S.3 and Lines 365-367).

The same behavior is not evident for LS2-LS5. In these cases, the probability of a change in thickness is nearly evenly distributed across the prior range of 0-10 Ma:



The point we wish to make for LS2-LS5 is actually that the solution for the time of the change *is* non-identifiable. This supports there not having been a secular change in overburden thickness within the signal integration timescale. Only the sample with the longest integration timescale (LS1) can clearly identify this change.

Finally, it is true that the denudation rates and overburden thicknesses from the hillslope samples span much of the prior ranges. However, the restricted prior ranges were set to speed-up the burn-in time required for the final run with a large number of iterations after initial exploration of larger ranges. The results are essentially the same using larger ranges.

Part III – Consistency with independent evidence

In proposing using paired nuclides to infer long-term peat depths in this manuscript we try to evaluate the results against independent constraints:

1. We find that the denudation rates are low. This agrees with the qualitative geomorphic history of the site, as well as the order-of-magnitude of the stream solute gauging/mass balance-based denudation rate.
2. We find that peat thicknesses were greater on average in the past. This agrees with our knowledge of the land-use history, which suggests substantial peat loss in the last few centuries.
3. We find that the onset of peat cover coincided with the uplift of the Ardennes. This uplift chronology has recently been confirmed by isochron burial dating on terraces of the Meuse and the Rhine (Da Silva Guimarães et al., 2026; Terraza et al., 2026). This is the least well supported of the three main claims as it relies on inversion of a single sample and the assumption of a step change in thickness. For this reason, we try to treat this as an intriguing hypothesis with tentative support in the discussion.

That the inversion results for the hillslope samples agree with independent constraints supports that they are not inversion artifacts.

Part IV – Summary

1. We fully agree that there are parameter trade-offs in under-determined problems and have tried to make this more explicit in the text. We have illustrated parameter trade-offs through cross plots between parameter pairs. Given the large number of possible parameter combinations we cannot provide 2d cross plots for every possible parameter pair, but we believe that with the inclusion of a new supplementary figure (Figure S.3) we have captured the most important ones. We would be happy to add the interquartile ranges to the text if this would clarify that we are not interpreting posterior modes, but rather regions in probability space that are consistent with the measurements.
2. MCMC is advantageous over the brute force approach in that it allows a more computationally efficient exploration of the parameter space and a denser sampling of the high-probability region of interest. However, we also demonstrate that the MCMC and brute force approaches give similar results in our case and that these results are consistent with independent constraints.
3. Simplifying to a determinable problem of 2-variables, as discussed in our previous response, yields the same key results as the MCMC inversion. Thus, our key findings that denudation rates are low and peat thicknesses were greater in the past are not results of over-interpreting inversion of a non-identifiable problem. We agree that Eq. 6 and especially Eq. 7 are partially to non-identifiable problems and that their results should not be given much weight. We have added text emphasizing this point.

References:

- Da Silva Guimarães, E., Busschers, F., Roda-Boluda, D., Akçar, N., Schlunegger, F., Kasse, C., Demoulin, A., Vockenhuber, C., Christl, M., Van Balen, R., 2026. Mid-Pleistocene uplift-induced acceleration of incision in the lower Meuse valley constrained by cosmogenic burial ages. *Quaternary Science Reviews* 383, 109978.
<https://doi.org/10.1016/j.quascirev.2026.109978>
- Terraza, M., Wolff, R., Hetzel, R., Ritter-Prinz, B., Binnie, S.A., Preuss, J., Hoselmann, C., Weidenfeller, M., Heinze, S., 2026. Accelerated uplift of the Rhenish Massif (central Europe) since 700–800 ka revealed by isochron-burial dating of strath terraces. *Geomorphology* 110334.
<https://doi.org/10.1016/j.geomorph.2026.110334>

Review 2

“Long-term peat thickness from cosmogenic ^{26}Al and ^{10}Be , Hautes Fagnes, Belgian Ardennes”
submitted by Moore et al.

I enjoyed reading this manuscript and its innovative way to use $^{26}\text{Al}/^{10}\text{Be}$ ratios to investigate the long term evolution of upland peatlands where rates of geomorphological processes are (very) slow. The study area is particularly well suited for this goal since the Hautes-Fagnes (HF) Plateau is located upstream of a rejuvenated landscape due to enhanced fluvial incision during the Middle Pleistocene. As requested by the editor, I have focused primarily on the regional context, the geomorphological significance of the results, and their integration within the existing body of knowledge. The manuscript presents novel and compelling findings that substantially improve our understanding of long-term landscape evolution in the Ardenne. My comments therefore aim to strengthen the regional framework and the discussion rather than challenge the study's main conclusions. I believe the points outlined below should be addressed before publication in Earth Surface Dynamics.

We would like to thank Gilles Rixhon for the review and have implemented the suggested changes.

1. Introduction/Study area

While the methodological section is thorough and well documented (and rightly so), the regional setting requires a stronger connection to the existing geological and geomorphological literature. Several key references are currently missing, and the discussion would benefit from a more comprehensive presentation of the local geological framework and the current understanding of Quaternary landscape evolution in the Hautes-Fagnes region. Similarly, the study area and sampling sites are insufficiently illustrated. At present, Figure 1 is the only figure providing geographical context, and it consists solely of an annotated orthophoto and a field photograph. Given that the manuscript contains only six figures, there is ample scope for additional or enhanced illustrations. Improved geological and geomorphological mapping would strengthen the manuscript and facilitate interpretation of the results.

- Lines 55-57 and lines 62-63: these two sentences are largely redundant; please merge them into a single one.

We have removed lines 55-57.

- Lines 63-65: this important sentence must be supported by existing literature: Sougneux and Vanacker (2011, HESS), cited in this study but only later, and Beckers et al. (2015, ESPL), not cited in this study and must be integrated (see later remarks).

We added these references at revised line 64.

1.1. Geology

- The Cambrian quartz-bearing rock formations are too briefly presented: name them at least and that they belong to the Stavelot Massif/Inlier which is the main geological structure underlying the HF Plateau. This is important as their weathered products turned into regolith/saprolitic material were sampled and analysed.

We have added more detailed description of the geology:

Line 89-90: “The bedrock at the site consists of low-grade quartzite and argillite of the mid-to-late Cambrian La Venne formation and the late Cambrian La Gelize formation of the Revin group. Structurally, these rocks belong to the Stavelot inlier.”

- Lines 87-88: though I am not a specialist of the local geology, I cannot remember having read somewhere that the Stavelot Massif exhibits a synclinal structure in this region. This would also appear counterintuitive with the fact that this it builds the highest Belgian topography. In the referenced text (Goemare et al., 2015), they just state “The NW flank of the SVM is characterized by a flat-lying thrust fault (Bless et al. 1990).” Explanation and geological cross-section provided by Bless et al. (1990) rather point to an anticline structure. Please check your information and correct if necessary.

Although the regional structure is dominated by anticlines associated with the fold-and-thrust belt, the provisional geological map of the study area indicates that the structure in the study valley is synclinal, with an outcrop pattern of younger rocks (latest-Cambrian, La Gleize) surrounded by older rocks (mid-to-late Cambrian La Venne). To the SW of the study area, Ordovician rocks are preserved in the center of the structure, confirming this. We have clarified that this is a local structure by adding “in the vicinity of the study site” at line 91-92. We have also added a geological map (Figure 1d).

- All these aspects related to structural geology and quartz-bearing rock formations are absent in the existing figure and should be represented in a new or enhanced figure 1.

We added a geological map modified from the provisional geological map of Marion et al. (2018) and Demoulin et al., 2018 (Figure 1d). We assume that the contacts in the provisional geological map are nearly entirely interpolated because of extensive peat cover. In particular, the extent of the Cretaceous sediments must be over-estimated, as our coring did not encounter any in the upper part of our study transect, where they are mapped by Marion et al. (2018). A less generous distribution of Cretaceous sediments is given in Demoulin et al. (2018) and we have adopted something closer to this in Figure 1d.

1.2. Geomorphology

- Lines 95-97: refer also to the study of Da Silva Guimaraes (2026, QSR) published in the meanwhile: they produced Mid-Pleistocene cosmogenic age estimates for Meuse terrace deposits. - Lines 97-99: beyond the necessary reference to Sougnez and Vanacker (2011, HESS), one further key reference must be integrated here: Beckers et al. (2015, ESPL) who modelled knickpoint migration through the drainage system.

We have added Da Silva Guimaraes et al. (2026) at line 100 and Beckers et al. (2015) at line 102.

- Lines 101-102 (and later in the discussion): there is indeed abundant evidence of former periglacial conditions in the Ardenne and this must be supported by existing literature beyond the sole Pissart et al., 2010: see e.g. Rixhon and Juvigné (2010, *Geologica Belgica*) for hillslope processes/deposits in a neighbouring Ardennian valley and Demoulin et al. (2018, ch. 7 from the book *Landscapes of Belgium*, Springer, and references therein) for the latest review (as far as I know) on HF lithalsas.

We have added these references at line 107.

- Fig.1: The geomorphological background must be better illustrated: the only information we have is the knickzone along the Hoëgne river. On that note in existing Fig. 1a, please better highlight the stream and name it. The caption must also quote the references for the position of the knickzone and

the delimitation of the Pre-Senonian surface were taken from (I guess Sougnez and Vanacker, 2011 and Demoulin et al., 2018, respectively). Here, a slope raster would be highly informative (i) to support your statement in lines 99-100; (ii) highlight hillslope convexity along the investigated transect and (iii) better grasp the topographic duality between relict and rejuvenated landscapes.

We have thickened the streamline and added a label for the Hoëgne river and a slope raster map (Figure 1e). The knickzone we interpreted from the increase in stream and hillslope gradients, as evident from the new slope raster map. The pre-Senonian surface was determined from a 3 point problem (we have noted this in the caption). This approach gives similar results for the study area as mapped by Demoulin et al. 2018.

1.3. Peatlands - Since long-term evolution of upland peatlands is targeted here, one would at least expect a map with peat occurrence across the HF plateau and the position of the investigated transect. See map #2 in the report from Wastiaux (2002, available here: https://orbi.uliege.be/radar_HF_rapport). On that note, a kind of “peatland typology”, such as briefly presented in lines 105-107, would be likewise highly welcome to differentiate between hillslopes with long-lasting peat cover (such as the one investigated here) and peat-filled, post-LGM ombrotrophic bogs corresponding to lithalsa remnants on summit surfaces (see previous remark).

We have replaced the PSS (which has been moved to Figure 1e) on Figure 1a with a layer showing all soils mapped as peat or raised bog in the digital soil map of Wallonia.

We would prefer not to comment on the summit surfaces as we lack data there. This would be an interesting area for future investigations.

- Lines 119-120: as the peat-cover thickness varies by one order-of-magnitude along the investigated hillslope, more information at this stage on how it evolves from up- (rather low in summit position) to downslope position (thickest peat accumulation) according to Fig. 8 in Henrion et al. (2024) is needed.

Added, “and are greatest at the footslope (LS5) and least at the topslope (LS2) (Table 1)” – Line 125-126

- Important remark: while the presented data and their interpretation seem convincing, please justify how this single transect can be considered representative of long-term evolution of peat-covered hillslopes across the whole HF Plateau and potential morphological variability (e.g. gradient of steepness, slope convexity vs concavity).

Forthcoming work is in preparation that will address this question in detail. We would like to keep the focus of the present manuscript on interpretation of paired cosmogenic nuclides in peatlands.

2. Methods-sampling

- A table compiling samples of all kinds would be highly welcome as they were collected at different positions and for different purposes (stream water, bedrock/saprolite geochemistry, cosmogenic nuclide analysis).

We have included all this information in the tables in the data repository, consistent with FAIR recommendations. In principle, we agree that a table compiling all samples would be good. However, we find it difficult to report all the geochemistry data neatly in a single table because the dataset spans

several data types. At the same time, we don't want to distract too much from the cosmogenic data by adding several additional data tables. Therefore, we have not added these tables.

- Lines 120-121: could you please provide field photos of the pits dug for saprolite/bedrock geochemistry?

As far as I am aware, we do not have field photos of these pits.

- To remain in line with the description provided in lines 123-124, modify the sample type of LS4 from saprolite to bedrock in Table 1. In the same wake, we learn later in the manuscript (lines 257-258) that sample LS6 was actually collected in the "stream floodplain", contradicting the statement provided in line 122 (i.e. saprolite sample). I doubt, however, that the Hoëgne could have developed a floodplain sensu stricto upstream of the knickzone/so close to its headwaters. Geomorphological interpretations based on cosmogenic concentrations measured from "in situ saprolite" (it is stated in line 214 that "downslope transport occurs exclusively within the peat layer") or fluvial sediments with potential storage time in the valley bottom as suggested here (see final part of the discussion), would actually differ. Again, field pictures and more documentation could greatly help clarifying all of this.

We have changed the label in the table, and removed "saprolite" at line 127 to generalize the quartz-bearing samples to include valley bottom sediment material. We changed "floodplain sediments" to "valley-bottom sediments" throughout.

- Lines 125-126: longitudinal sampling of riverborne sediments and their amalgamation for cosmogenic analysis represent an unusual strategy: please provide some details (for instance at what sampling interval) and justify why you did so.

Lines 132-133: Changed the text to "Bulk stream sediment was collected from several locations spaced a few meters apart along the stream bed at the base of the study hillslope and amalgamated to obtain a well-mixed sediment sample (LSR-1)."

- Lines 157-158: clarify and make uniform (see previous remarks) the sample nature (all saprolite vs saprolite+bedrock+"floodplain") and sampled lithology (sandstone here vs quartzite/argillite previously).

Changed to:

Lines 164-165: "To isolate quartz for cosmogenic nuclide measurements, the saprolite (LS1-LS3, LS5), bedrock (LS4), valley-bottom sediment (LS6), and stream sediment (LSR-1) samples were first washed and (except LS4) sieved."

Lines 165-166: "Vein quartz and quartzite pieces were hand-picked from the >2 mm fraction of the saprolite and valley bottom sediment because of small available sample sizes"

- Lines 248-249: a bit tricky to justify a methodological step with results from this study.

Changed to:

Lines 256-257: "However, at the study site quartz concentrations in the peat layer are low and radioactive decay is expected to be important."

3. Results and discussion

- Lines 343-344: see previous remark suggesting an enhanced morphological description of the investigated hillslope to support your modelling results for LS3 and 4.

We have added additional maps (Figure 1d, Figure 1e, and updated Figure 1a) and description of the study hillslope as recommended previously.

- Lines 425-435: the pre-Quaternary (Neogene) landscape evolution of the Ardenne remains poorly constrained. Nevertheless, two arguments would support the onset of river incision upstream of the knickzone prior to the Mid-Pleistocene uplift, yet linked to very low denudation rates such as proposed in this study. First, regarding an earlier phase of Neogene landscape evolution, Rixhon and Demoulin (2018, Chapter 10 in Landscapes of Belgium, Springer) stated that “the modern course of the Meuse from the Paris Basin to Liège was already established at least in the Late Miocene”. This would imply that the Ardennian drainage network, including the Ourthe catchment and (sub-)tributaries such as the Hoëgne, was broadly in place by that time. Second, Rixhon et al. (2020, Geomorphology) reported a ~3.3 Ma burial age for cave-deposited alluvium (hence Ardennian fluvial deposits) in the lower Ourthe Valley. This age is consistent with both the cave's elevated position at the plateau margin and the Neogene age attributed to adjacent planation surfaces: it suggests that at least modest incision and drainage organisation were already underway during the Pliocene.

Changed to:

Lines 444-446: “This incision must have begun before Pleistocene uplift of the Ardennes because the Pleistocene knickzone on the Hoëgne is located downstream of the study area. This is consistent with the establishment of the Ardennian drainage network in the Late Miocene to Pliocene proposed by previous workers (Rixhon et al., 2020; Rixhon and Demoulin, 2018).”

- Line 443: coarse stream sediment appears somehow misleading I think: one could understand bedload/cobbles here.

We note here (Line 459) that we mean the coarse bedload fraction (>2mm)

- Lines 453-460: see also Demoulin et al. (2018) to support your message.

Added this reference at Line 464

- Lines 456-457: again, a peatland map would be particularly helpful to support your sentence here.

We have added a peatland map (Figure 1a).

- Lines 491-496: see also the final map in Demoulin et al. (2018, reference above) proposing palaeoclimatic reconstruction for the Younger Dryas.

- Lines 535-540: again, new age results from Da Silva Guimaraes (2026, QSR) will strongly support your justification here.

We have added this reference and revised this section accordingly:

Lines 543-551: “Interestingly, the timing of this event corresponds closely to the uplift of the Ardennes recorded by the fluvial terraces of the Meuse and Rhine rivers, which cross the Ardennes-Rhenish massif. Abandonment of the East Meuse valley and an increase in Meuse terrace gravel content indicate that gradual uplift began in the early Pleistocene (da Silva Guimaraes et al., 2024). An acceleration in uplift is recorded by the abandonment of the Younger main terrace, a prominent terrace at mid-elevation in the Meuse and Rhine valleys. This has been dated to 0.73 ± 0.12 Myr in the Meuse valley using cosmogenic nuclide depth-profile dating (Rixhon et al., 2011) and 0.52 ± 0.11 Myr in the Meuse valley and $0.76 \pm 0.19/-0.17$ Myr in the Rhine valley using $^{26}\text{Al}/^{10}\text{Be}$ burial dating (da Silva Guimarães et al., 2026; Terraza et al., 2026).”

- Lines 566-572: although based on a single sample, the depleted $^{26}\text{Al}/^{10}\text{Be}$ ratio measured in fluvial sediment is an intriguing result, as it suggests prolonged sediment storage (possibly $> 1\text{Ma}$) within a relict landscape. The authors may wish to strengthen this interpretation by referring to Jautzy et al. (2024, EPSL), who reported similarly depleted $^{26}\text{Al}/^{10}\text{Be}$ ratios (up to 1.3) in riverborne sediments from the Vosges Massif. They attributed these signatures to (very) long sediment residence times, potentially enhanced by glacial shielding, and highlighted the resulting limitations of deriving catchment-wide denudation rates solely from ^{10}Be concentrations. This study therefore provides an interesting analogue and supports the note of caution expressed here.

Added this reference:

Lines 582-584: “Similarly depressed $^{26}\text{Al}/^{10}\text{Be}$ ratios in stream sediment and spuriously high single nuclide denudation rates have been reported in other European massifs, supporting this cautionary note (Jautzy et al., 2024).”

I apologise for the slight delay in returning this review. I enjoyed evaluating this contribution and I remain available to the authors and editor for any further clarification or discussion if needed.

Gilles Rixhon