

We would like to thank Richard Ott for the helpful review. If the manuscript is accepted for revision, we would be pleased to add clarifications and expanded justifications of the inversion approach and to implement the other suggestions.

Moore et al. present an interesting study trying to infer past peat thickness, denudation, and timing of peat accumulation in the Belgian Ardennes. The study is well written and in most parts also very thorough. However, parts of the methods were brief and thus I could not follow exactly what the authors did to run their MCMC inversion. Based on my understanding of what the authors did, I have concerns that the recovered parameters and the conclusions might be biased.

Inversion of CRN concentrations:

My major concern is the application of the MCMC inversion for this study. The methods section on the MCMC application was brief, and thus, I might have misunderstood something. **It was not clear what parameters were inverted for, what the prior ranges were, and whether samples were inverted jointly or separately.** The way I understood the manuscript, the authors use the MCMC to constrain the parameters of denudation rate, initial overburden thickness, overburden thickness, and time of change, in equation 5 INDIVIDUALLY for every hillslope sample. Thus, there are 4 unknowns for every sample (D , z_1 , z_2 , t), but only two equations with data (^{10}Be equation and ^{26}Al equation). This is an undetermined problem and cannot have a unique solution without additional information.

This can be somewhat seen in the lower panels of figure S1, where one would expect all parameter combinations that can explain the data to fall onto a curve. They do so for panel D, but the MCMC apparently did not explore the entire space for denudation rates. From personal experience I know that these MCMC random walks sometimes tend to cluster one side of the prior parameter space—for reasons I don't understand—, leading to the effect that they don't map the full line of parameter combinations. However, in theory, with 3 parameters one can calculate curves of valid parameter combinations analytically from the two nuclide build-up equations.

The only reason to apply a MCMC to this problem would be to invert all samples together, hoping that differences in the sample parameter combinations provide additional information. This is the approach that Hippe et al., 2021 used for 3 parameters and 2 nuclide equations. However, they unfortunately did not provide synthetic tests to prove that the inversion can still reliably recover synthetic parameters (I think it's great that the authors of this study included such a test!). My experiences from personally working with similar inversions for cosmogenic nuclide erosion histories is that the inversion only partially recovers initial conditions, for combined samples with 2 nuclides and 3 parameters.

A simple fix could be to hold values constant—assume a denudation rate and/or zero initial peat thickness—and then calculate the other two parameters for every sample, resulting in a range of plausible parameters.

This being said, I apologize, if I misunderstood part of the manuscript and the samples were indeed jointly inverted. In that case, some method clarification and a better documented synthetic test with several samples would be nice.

Introduction

We appreciate the critical evaluation of the MCMC inversion and agree that this is an under-determined problem since we invert for 4 parameters from 2 nuclides. It is true, a unique solution does not exist. Yet, one can still determine the joint probability distribution (i.e., the probability that certain parameter combinations occur together) using MCMC.

Our samples are indeed inverted individually, rather than jointly. This is because our model has no parameters that are constant between samples (i.e., we allow there to be a unique denudation rate, time of change, and different overburden thicknesses before and after the change for each sample). This differs from the main approach in Hippe et al. (2021) where the timing of the change in erosion rate is assumed to be constant across all samples. However, an individual inversion where the erosion rate is allowed to vary between samples is also presented in their supplement (their Fig. S4).

Markov Chain Monte Carlo has been applied to find joint distributions for models of cosmogenic nuclide data with more parameters than samples in a number of studies from the last 10 years or so, but, as rightfully pointed out, is not without limitations and is sensitive to the choice of priors (we will move our priors from the Zenodo repository to the main text to make them more explicit). It is an interesting question – how well does the MCMC find the “true” parameter values for our models?

Least-squares gives similar results to MCMC for LS2-LS5

One way to evaluate the MCMC is to simplify the model to involve only 2 parameters and solve using classical least-squares. Because the MCMC results indicate that the initial overburden thickness and timing of a change in over-burden thickness are unconstrained, it makes sense to neglect these two terms. If we do this and use the Levenberg-Marquardt algorithm, then we get the following results for peat thickness (in g cm^{-2}):

Peat thicknesses

ID	LS2	LS3	LS4	LS5
Least-squares	359.95	449.16	262.68	368.64
MCMC median	362.97	437.25	258.72	352.09
MCMC interquartile	333-411	400-477	236-282	308-400

and for denudation rates (in $\text{tons km}^{-2} \text{yr}^{-1}$)

Denudation rates

ID	LS2	LS3	LS4	LS5
Least-squares	2.502	1.151	2.802	3.930
MCMC median	3.18	1.78	3.09	4.89
MCMC interquartile	2.27-4.94	1.09-2.93	2.39-4.03	3.54-6.9

All results from the least-squares inversion fall within the MCMC interquartile range. Thus, the peat thicknesses and denudation rates determined from LS2-LS5 are not artifacts of the MCMC inversion.

Why use MCMC at all? The reason is that it is not possible to explain the concentrations measured in LS1, which falls below the secular equilibrium line on the two-isotope plot, using a steady-state exposure model. There must have been a change in shielding far enough in the past for decay to be important. Likewise, the catchment-averaged sediment model contains more than 2 parameters, and so

a unique solution is also not tenable. Although we do not really need the MCMC for LS2-LS5, for consistency we use the same model and inversion method for all the hillslope positions.

The case of LS1

Are the parameter estimates from the MCMC for LS1 reasonable, and why does Figure S.1c not show the expected trade-off curve between denudation rate and overburden thickness?

There are several lines of evidence indicating that the inversion results for LS1 are reasonable:

- 1. The extremely low denudation rate is consistent with the paleo-surface model from qualitative geomorphology.*
- 2. The inferred long-term peat thickness is in line with the values inferred from other samples and gives a reasonable hillslope profile (Fig. 3). Because the samples were inverted individually, these are independent measurements.*
- 3. We have independent knowledge that uplift occurred around the modeled time interval of the reduction in production rate. The onset of peat accumulation at this time seems like a plausible explanation for increased shielding.*

With respect to the denudation rate – overburden thickness trade-off curve (Fig. 1c), part of the explanation likely lies in the fact that the denudation rate cannot go too high. This is because as denudation rate increases the integration time decreases, and the signal becomes less able to capture the radioactive decay needed to generate ratios below secular equilibrium. Also, the values for the denudation rate are not controlled only by the trade-off with overburden thickness, but also interaction with the timing of the change and the initial overburden thickness.

Proposed changes to the manuscript

To clarify our approach we propose making the following changes to the manuscript:

- 1. We will move the priors from the data repository into a table in the manuscript. This will make more evident the values of the priors that we used and which parameters we are inverting for.*
- 2. We will provide more discussion of our MCMC methods in the text (section 3.5.4) and state explicitly that the samples are inverted individually.*
- 3. We will note that essentially the same results are obtainable for LS2-LS5 using classical least-squares methods. We will include the least-squares results in the supporting information and add the code used to calculate these to the Zenodo repository. We will also expand the caption to Figure S.1 to better describe the synthetic models.*

Changes Made:

- 1. Line 365 – We have inserted a new “Table 2” – “Parameter bounds used in the Markov Chain Monte Carlo models”**
- 2. Lines 266-281 – We have revised section 3.5.4 to read:**
“Equations 5-7 require solving for 3-5 parameters from two nuclide measurements and are thus under-constrained. Therefore, we use a Markov Chain Monte Carlo (MCMC) inversion to estimate the joint

probability distributions of the parameters (Andersen et al., 2023; Hippe et al., 2021). We adopt the inversion method of Andersen et al. (2023), which uses the Metropolis-Hastings algorithm to sample the joint distribution. Combinations that result in a reduction of the χ^2 statistic from the previous combination are accepted, whereas those that increase χ^2 are accepted or rejected based on a randomly varying acceptance threshold, which allows the algorithm to explore the parameter space between defined bounds. Prior ranges for each parameter are given in Table 3. We invert the samples individually, allowing all parameters to vary, using 10 random walkers and discarding the initial 4×10^4 accepted parameter combinations as a “burn-in” phase. Afterwards, the inversion is run until 10^5 accepted iterations per walker are achieved, for a total of 10^6 accepted parameter combinations. To demonstrate the suitability of the MCMC approach to this problem, we use Eq. 5 to calculate nuclide concentrations for an artificial parameter set and then invert for the parameter values. The algorithm successfully recovers the input parameters (Figure S.1). Furthermore, we use least-squares to invert a simplified version of Eq. 5 that assumes no change in peat thickness and thus has only two parameters: the peat thickness and denudation rate. This produces results that are broadly similar to the MCMC approach for most of the hillslope samples (Table S.2). The posterior probability distributions for the parameters obtained from the MCMC inversion are somewhat asymmetric, so we report the median of the probability distributions, which is robust to skewness, and the 50% credible interval as summary statistics.”

3. Lines 276-279 – We note that essentially the same results are reproducible using least-squares. We have added these results to the supporting data (Table S.2) and the least-squares code to the Zenodo repository.

Other comments:

Production rate scaling. The authors use Stone 2000 scaling. However, compared to the last few million years the geomagnetic field is comparatively strong at the moment. This may lead to an underestimation of long-term production rates. Maybe given the other uncertainties in the inversion this effect is small enough to be neglected. In that case, the authors could add a brief statement explaining their reasoning.

At the study site, which is located above 50° N, the latitude effect is muted. The difference between Stone (2000) scaling and the Balco et al. (2008) time-dependent version averaged over the last 1 million years calculated using CRONUSCalc (<https://cronus.cosmogenicnuclides.rocks/2.1/>) seems to be <1% for the study site.

There is a larger offset between the production rate predicted by the Lifton et al., (2014) scaling model over the last 1 million years (ca. 6.6 atoms g⁻¹ yr⁻¹ for ¹⁰Be) and Stone (2000) (ca. 7.2 atoms g⁻¹ yr⁻¹).

However, the impact on the parameter estimates is quite small. This is because under peat thicknesses greater than 260 g cm⁻² (the lowest median modeled value) the high-energy nucleon flux is severely attenuated. This is especially true when considering a mean free path of 110 g cm⁻² through peat, which implies spallation is attenuated to <10% of the surface value under 260 g⁻¹ cm⁻². Muon-induced production therefore contributes disproportionately to total production.

Recalculating LS2-LS5 using the least-squares approach and a spallation production rate of 6.6 atoms g⁻¹ yr⁻¹ gives for overburden thickness (g cm⁻²):

ID	LS2	LS3	LS4	LS5
Lifton, Sato, Dunai	350.96	440.84	253.49	359.82
% difference from St	2.50	1.85	3.50	2.39

and for denudation rates ($\text{tons km}^{-2} \text{ yr}^{-1}$):

ID	LS2	LS3	LS4	LS5
Lifton, Sato, Dunai	2.502	1.149	2.802	3.931
% difference from St	-0.02	0.17	-0.01	-0.02

These results differ by only a few percent. We will add a sentence or two discussing the relative insensitivity to the spallation production rate to section 4.

Line 344-345 – Added “Under these thicknesses, spallation production is severely attenuated and muon reactions are proportionately more important to total production. Thus, the results are only weakly sensitive to the choice of spallation scaling model or reference production rate.”

The ^{10}Be and ^{26}Al measurement data need to be reported in more detail to ensure reproducibility. Currently, only the final concentrations are reported in table 1. However, the authors should report all relevant data that were used to convert AMS measurements to concentrations (AMS ratios, sample weights, carrier concentration, blank values, etc.).

All of this data (ratios, weights, spikes, blanks, Al concentrations, etc.) is reported in the supplementary data tables in the Zenodo repository. We will either add a statement in the table caption to make it clear that this is where the full data is, or else add another table to the manuscript, whichever is most consistent with ESurf’s policies.

We have added a link to the Zenodo repository in the figure caption, as well as the reference to the citation list.

I was confused as to why the authors only focus on Si for the weathering rates. Why weren’t the total dissolved solids considered? Why was the standard CDF equation modified to Si, given that supposedly all other major elements were measured by ICP-OES?

We chose to focus on Si because it is more likely derived from rock weathering than the other dissolved elements in the stream water. Stream solute concentrations at the site are low and cyclic salts dissolved in precipitation constitute a significant fraction of the total dissolved mass flux. For example, nearly all the Na^+ in the stream water comes from precipitation, even though Na^+ is the major element with the highest concentration in the water.

The standard way of dealing with cyclic salts is to assume that Cl^- is derived only from precipitation, and that the element: Cl^- ratio in precipitation is the same as seawater and correct accordingly. However, we lack the necessary Cl^- data to do this.

Si is the most abundant element in the bedrock, was present at a high-enough concentration in the stream water to be reliably determined by ICP-OES, and has an essentially negligible concentration in precipitation. Thus, to estimate the bedrock denudation rate without knowing the cyclic salt corrections, it is best to focus on Si. Of course, there is considerable uncertainty with this approach.

Thus, we frame the solute-flux based denudation rates as a sort of “order of magnitude” check on the cosmogenic nuclide denudation rates. This is significant because if one were to compare the denudation rate inferred from simple interpretation of ^{10}Be concentrations alone with this solute-flux based rate, the two would disagree. We will make this point more explicit in the text.

We have made three small changes to the text to bring-out the point that this is a first-order comparison and explain why we focus on Si:

1. Line 69 – added “order-of-magnitude”
2. Line 131-132 – added “Stream solutes are derived partly from atmospheric deposition and partly from bedrock weathering.”
3. Line 424 – Changed wording to “broadly supported by the solute flux” to soften the claim.

Attenuation length: The authors calculated a separate peat attenuation length. This makes a lot of sense. However, the peat depth according to the authors is only 0.2-2.1 m at the moment. Meaning that a lot of production will occur under the peat and thus have a different attenuation length. Does the forward model in the inversion use both attenuation lengths (for peat and below peat)?

Yes, the forward model includes this difference in attenuation length. We will add this point to the text in section 3.5.2.

Line 221 – Added “We use 160 g cm^{-2} for the spallation mean free path in saprolite.”

L13: Seems like there’s a word missing after “explore”.

Line 13: Changed to “explore the use of”

L122: What was the grain size of the stream sediment samples?

We collected bulk stream sediment and then sieved and used different fractions for different analyses, described later.

L159: Here or in the cosmogenic nuclide result table, report the carrier concentration.

Line 162 – Added “ $1052 \mu\text{g Be g}^{-1}$ ”

L202: I suggest reporting the exponential parameters here or in the supplement. Otherwise, interested people would need to dig through the code.

We have added a table with these to the supplement and a reference to this table at line 208.

L214: Missing i- for production pathway

Fixed

L237: I presume the authors mean denudation rate, instead of erosion.

The distinction is a bit fuzzy because the cosmogenic signal in quartz gives a value that is intermediate between erosion and denudation. When chemical mass losses are significant, the uncorrected cosmogenic rate is probably closer to erosion than denudation. Here we develop an expression for erosion, so would like to keep the “basin-averaged erosion rate” framing.

L440: Please, elaborate on your argument. The authors note the “nature of these lenses” make them unlikely to have had sufficient shielding effect. This is a vague statement that could be explained better.

Line 456-457: Added “which were ca. 10s of meters in diameter in the Hautes Fagnes area and widely spaced”

Many thanks for reviewing the text in detail. We would be pleased to make all these corrections.

For questions about this review, feel free to get in contact.

Richard Ott

General comments

We would like to thank the reviewer for the comments and provide our responses and proposed changes to the manuscript in italics below:

Peatlands are important carbon reservoirs at the Earth’s surface; their shrinking or thickening, partially affected by human activities, can contribute to carbon release or sequestration and thus exert an impact on climate to some extent. This manuscript by Moore et al. focuses on the evolution of upland peat thickness in a slowly eroding area in Belgium, using paired ^{10}Be - ^{26}Al in underlying saprolite and nearby stream sediments. The ^{10}Be - ^{26}Al concentrations suggested lower values compared to steady-state conditions at current peat thickness, hinting at a thicker cover in the past. The authors attributed the shrinkage of the peatland to enhanced human activities. Note that I am not an expert in peatlands or burial dating, and thus I may not be able to comment on part of the paper. In general, the utilization of cosmogenic nuclides in tracking peat thickness based on its shielding effect on underlying saprolite seems to be a novel application. Nevertheless, such an application relies on some assumptions, as stated in the manuscript, and the degree of uncertainty they may introduce is difficult to quantify, which might affect the robustness of the data interpretation. I hope the authors find the comments below useful during their revision process.

Specific comments

1. Does this application rely on an assumption of time-invariant saprolite denudation? However, if peat thickness can vary—for example, from being completely removed to a thick cover—the denudation rate may also vary to a large extent, ranging from fast erosion by runoff to slow erosion due to the shielding effect of the peat cover?
2. The study area is characterized by a MAT of 6.7 °C. As such, I would assume that the temperature in the LGM could be very close to the freezing point (mentioned in the manuscript). In this case, even if there was no glacial coverage (please provide literature) in the past (e.g., 1 Myr), periglacial processes could play an important role in surface denudation, particularly during past glacial periods. How does this process potentially affect your modeled denudation rate?

This application does not rely on saprolite denudation rates being time-invariant, only that changes in denudation rate occur with a periodicity that is short in comparison with the integration timescale of the signal. The integration timescales range from 0.7 to 1.8 Ma (Table 3), whereas changes in denudation rate are most likely to occur over glacial-interglacial (100 ka) (in the case of periglacial

processes) or shorter timescales. Thus, changes in nuclide concentrations from climatically-driven changes in denudation rate should be highly damped and the rate we infer is essentially a time-averaged rate.

We will add a sentence describing this to section 4.3 where we explain the integration timescale.

Line 351-353: “This implies that the denudation rates integrate over all glacial-interglacial cycles since the mid-Pleistocene and that any signal from climate-driven oscillations in denudation rate is highly damped.”

3. Eq. (2). The calculation of the long-term denudation rate using water chemistry is indeed inappropriate. Beyond the timescale bias and effect of mineralogical sorting mentioned by the authors, the solute sources can be derived from peat, quartzite, and argillite, while the solid source only includes one source (either quartzite or argillite). This discrepancy might make the application of Eq. (2) for denudation rate calculation problematic.

This is a good point, although it seems that further weathering of primary minerals in the peat is probably low. Dry peat generally has a density of 0.1 g cm^{-3} and is ca. 10% mineral ash by weight. Thus, even meters thick peat corresponds to only a few g per cm^2 of mineral material that could weather and release Si.

Even if Si release from the peat were significant, this would mean that we have over-estimated the denudation rate of the saprolite by attributing all the dissolved Si flux to saprolite weathering. A lower denudation rate of the saprolite would be in even better agreement with the denudation rates inferred from the cosmogenic nuclides.

In the revised manuscript, we will try to frame the analysis of denudation rate from water chemistry and solid-phase mass balance more explicitly as an order-of-magnitude check on the cosmogenic nuclide results rather than a central line of evidence.

Line 69 – added “order-of-magnitude”

Line 424 – Changed the wording to “broadly supported by the solute flux” to soften the claim.

4. Line 188. Is there a seasonal change in water content, and how does it affect your estimate of atomic mass?

There is some seasonal change in water content at the site, but it is quite modest (Henrion et al., 2025). The peat likely remains saturated below ca. 30 cm depth except under extreme drought conditions.

Even in totally desiccated peat, hydrogen is still abundant because it is a major component of organic matter. Thus, the mean atomic mass of dry peat is likely closer to water than rock.

We will add a sentence in section 3.5.1 describing this.

Line 196-198 – “The mean atomic mass of organic matter is broadly similar to that of water (e.g., cellulose is 8 u), suggesting that the attenuation length is not significantly affected by the position of the water table or the exact peat porosity.”

5. Line 292: The large difference in immobile element ratios between saprolite and bedrock is indeed worrisome. But I agree it is difficult to find a convincing explanation.

6. Conclusions: Lines 577-578. Since the authors argue that the removal of peat is mainly caused by human land use in the last few hundred years, why not use a shorter-half-life nuclide such as ^{137}Cs (Jelinski et al., 2019, JGR), which may be more appropriate for this purpose? Note that I do not mean to suggest measuring an additional nuclide, but rather to provide a short paragraph on future directions.

It would certainly be interesting to consider the dynamics of shorter-lived fallout nuclides in this system. However, this is beyond the scope of this work, as it requires not only additional measurements, but different conceptual and mathematical models. We prefer to keep the message of the paper focused on applications of in-situ cosmogenic nuclides in peatlands, which, to the best of our knowledge, have not been previously explored, but we do mention in-situ ^{14}C as another interesting direction to explore in peatlands. Moreover, bomb-pulse ^{137}Cs is limited to studying processes since the mid-20th century. We believe the land-use history at our site extends further back in time and would thus be difficult to fully resolve using bomb-pulse nuclides.

Technical corrections

7. Fig. 1: Perhaps add a colorbar of elevation for the inset map of Belgium.

Added a colorbar to the map.

8. Table 1: Depth to top or bottom. What does top or bottom mean? Top of the peat or top of the saprolite?

We will add a color bar and clarify top and bottom by adding a note in the table caption.

Added a note in the table clarifying this as “Top and bottom of the sampled interval”