

“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*.

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.

- Lines 63-65: this important sentence must be supported by existing literature: Sougnez 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).

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.

- 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.

- 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.

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.

- 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.

- 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.

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).

- 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.

- **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).

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).

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

- 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.

- 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 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).

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

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.

- 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.

- Line 443: coarse stream sediment appears somehow misleading I think: one could understand bedload/cobbles here.
- Lines 453-460: see also Demoulin et al. (2018) to support your message.
- Lines 456-457: again, a peatland map would be particularly helpful to support your sentence here.
- 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.
- 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 > 1Ma) 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.

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