The authors thank the reviewers for its constructive comments. The comments are shown in black font, and our responses are in regular blue font.

Reply to RC1 comments:

1. The studies carried out by Nichols et al. (2002-*Geomorphology*; 2005-*Journal of American Sciences*) are surprisingly disregarded given the key topic addressed in this contribution and the numerical model developed accordingly. In their second article, i.e., "*Late Quaternary history of the Chemehuevi mountain piedmont, Mojave Desert, deciphered using 10Be and 26Al*", they detail how 10Be and 26Al profile data are used to account for hiatuses in long-lasting aggradation with >10 ka-long erosional-stability phases.

1.1. I strongly recommend not only to integrate these references but also to have a careful look at their methodology that may have been partly re-used in this study (see in the fig. below how the 26 ka-long erosional episode matches the profile presented in Fig. 2b of this study).

> We fully agree with the reviewer's advice. We will integrate these references and precise in the text what are the common points and differences between our approach and the one used by Nichols et al. (2002).

1.2. Importantly, Nichols et al. (2005) theorised, observed, and modelled a decreasing upward trend in concentrations (also observed later by Rixhon et al., 2014) during slow aggradation episodes (possibly occurring in this study as well). This is apparently not the case here. Please comment on that, especially on the base of the profile presented here (U1 to base of U3).

> In theory, there is the possibility of having periods of slow aggradation that could result in a decreasing upward trend of CRN concentrations as shown in e.g. Nichols et al. (2002) and Rixhon et al. (2014). The reviewer is therefore right to be concerned about the transition between U1 and U3, and points to the fact that this could be related to slow aggradation (instead of a long hiatus that was proposed by the authors). In the eventual presence of slow aggradation of some units (like U2), the total formation time would have been higher.
>
> We will further elaborate this part in the discussion, and elaborate the text on the transition between U1 and U3 using additional information on weathering that was derived from geochemical proxies to back up our conclusions (Vandermaelen et al., 2022).
>
> We point the reviewer to the fact that we did not sample U2 for CRN analyses: the unit is very thin which did not allow to sample 3 depth intervals.

2. A large difference between the number of processed samples for 10Be (#14) vs 26Al (#3) is reported in lines 209-210. This divergence is disturbing because each nuclide is basically used for distinct purposes, and this is not convincingly presented/explained so far. Whereas the tight 10Be-vertical sampling is appropriately used to decipher the composite concentration profile, the three 26Al data are "merely" used to punctually follow the 26Al/10Be ratio at depths ranging from 2 to 6.5 m along the profile. This contradicts, for instance, the "cosmogenic depth profiles" announced in the title (only 10Be data are used for that).

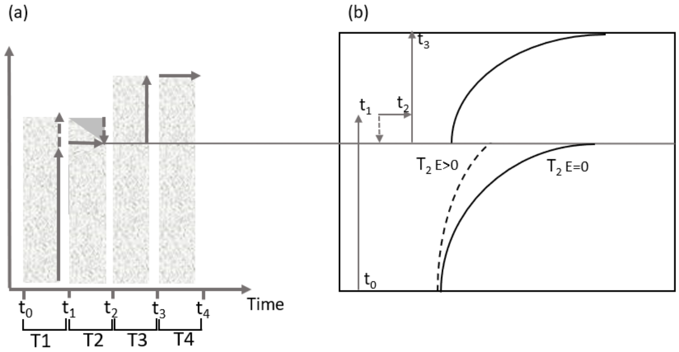
-> Please thoroughly justify why you adopted this sampling strategy (i.e., why not 14 26Al measurements as well?), check your complete manuscript, and change the title accordingly.

> The number of samples processed for ^{10}Be and ^{26}Al is not equal. We will adapt the title, and clarify in the text our strategy of sample processing for CRN. In fact, the detailed CRN depth profile is based on 14 ^{10}Be analyses, quite an exceptional number for 1 depth profile. We "only" processed three samples for ^{26}Al analyses – one from each of the main sedimentological units. The reasons for doing less ^{26}Al analyses are: (i) cost efficiency, (ii) higher uncertainty on ^{26}Al determination given the combination of uncertainty on AMS and ICP-AES measurements.

3. Fig. 2 nicely depicts the effect of discontinuous aggradation on CRN profiles, i.e., two aggradation phases interrupted by an erosional event. Long-lasting stability phases, however, also play a key role here, as pointed out in both Fig. 5 and Table 3, i.e., lowest chi-squared values for scenario 4. Two points here:

- in complement to the existing figure with an erosional episode, it would be highly welcome for the reader to conceptually show how a depth profile would look like if two aggradation phases were interrupted by a stability phase only, without erosion (again, see studies of Nichols et al.).

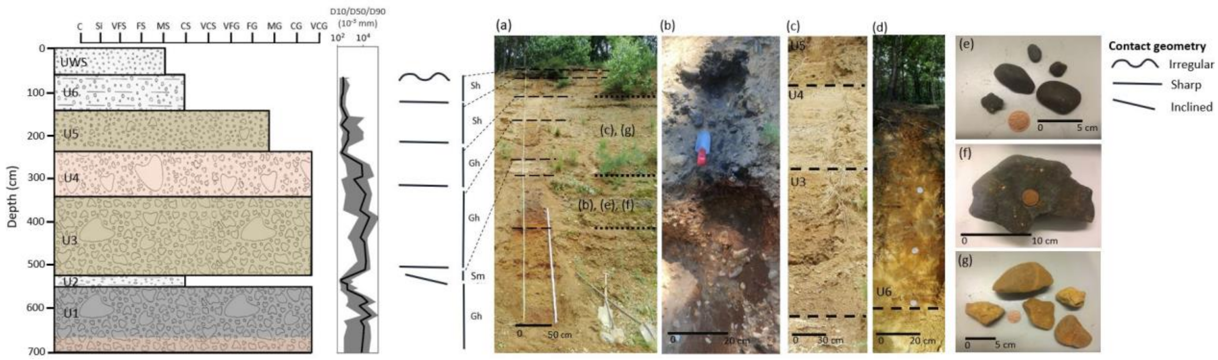
Rather than using two figures, we propose to add the CRN depth profile observed after a stable episode beside the erosional depth profile in Fig. 2. The adapted version of Fig. 2 should look as follows:

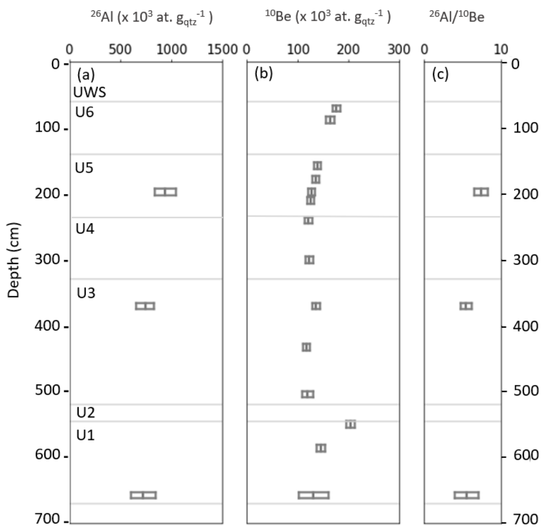


- if landscape stability is envisaged, one may expect pedogenic features/imprints in the sedimentary sequence. This topic may have been discussed in the previous publication by the same authors (Vandermaelen et al., 2022) but this is not tackled in this manuscript. Could you develop this here as well to support your scenario 4?

We agree with this comment, and we will include information on pedogenic features in this manuscript. To do so, we will add information on the sedimentary facies in Figure 4, and move the information on the CRN concentrations to a new figure. This will also solve the issue that the results on the CRN are presented in a figure about ~100 lines before the text.

The new figures would look as follows:





4. Description of the study area is insufficiently supported by existing literature.

4.1. Please provide references to following sentences:

- "In the southwest, the Campine Plateau is bordered by a cryopediment shaping the transition to the Scheldt Basin" (lines 170-171);

> This refers to the Beringen Diepenbeek Glacis, a cryopediment described in e.g. Beerten et al. (2018). We refer to the figure below. The reference will be added in the text.

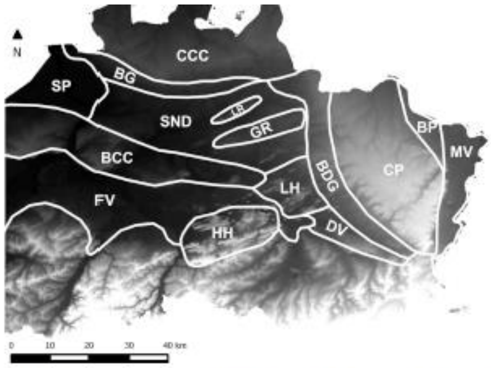


Fig. 12.2 Geomorphological units surrounding the Campine Plateau (from De Moor and Pissart 1992). *CP* Campine Plateau; *BP* Bocholt Plain; *MV* Meuse Valley; *BDG* Beringen-Diepenbeek Glacis; *SND* Schyns-Nete Depression; *LR* Lichtaart Ridge; *GR* Geel Ridge; *FV* Flemish Valley; *DV* Demer Valley; *LH* Lummen Hills; *HH* Hageland Hills; *BCC* Boom Clay Cuesta; *CCC* Campine Clay Cuesta; *SP* Scheldt Polders

> Source: Beerten et al. (2018)

- "By this time, the region corresponded to a wide and shallow river valley occupied by braided river channels" (lines 173-174);

> We will add references that are describing the existence of Middle Pleistocene braided rivers in Belgium. Beerten et al. (2018) and De Brue et al. (2015) are preconized to support this statement.

"Architectural elements that support the existence of individual aggradation phases include gravel bars and bedforms, channels, sediment gravity flows and overbank fines" (lines 177-178). This last one is particularly important because this association of features seems pointing to a specific braiding type, which should be supported by just more one reference to a master thesis (Dehaen, 2021).

> We refer to Dehaen (2021) as she specifically worked on the architectural elements and the sedimentary facies of the braided river deposits of the Zutendaal gravels for her MSc thesis. We will add more background literature, and refer to Paulissen (1983) who described some of the architectural elements of the Zutendaal gravels. We will also refer to the theoretical concepts on braided river deposits by Miall (1996).

4.2. What is the nature of the Weichselian coversands pertaining to the Ghent Formation (lines 183-184)? I guess they are aeolian deposits based on (i) what is mentioned in lines 167-168; and (ii) their age. More precision/information is needed because they are explicitly considered in the scenarios/model.

> The following information will be added to the paper about the Weichselian coversands: *"These sands belong to the Opgrimbie Member within the Gent formation (Beerten et al., 2017), and OSL dating showed an age ranging between 23 ka and ca. 11 ka, covering the late Pleniglacial and Late Glacial (Derese et al., 2009; Vandenberghe et al., 2009)".*

5. Although this manuscript focuses on the modelling procedure and its outcomes (in accordance with the journal's scope), one would expect more geomorphological discussion (manuscript's sections 3.2 & 4.1) of these meaningful chronological results along with more "Quaternary" contextualisation. Please develop and integrate following points:

5.1. Erosional episodes:

5.1.1. Whereas the description of the model's best fit (lines 368-383) mentions three erosion amounts, i.e., at the top of U2 and U3 and of the overburden (UWS I guess), the best fit scenario 4 of fig. 5 also considers an erosional episode at the top of U6 (time span between t5-t6). Why is it so? Please correct this important discrepancy.

> A precision needs to be made here. The "overburden" in this sentence corresponds to the material located on top of U6 during the hiatus that extended from the end of the formation of U6 until the beginning of the UWS aggradation. This overburden thus corresponds to the eroded part of U6, or to an overburden of aeolian origin deposited and then eroded before the Weichselian. There are thus 3 erosion phases that have affected the fluvial deposits (top of U1/U2, U3, and U6), and one erosion phase that removed a part of the Weichselian sands once they are deposited (top of UWS). This will be clarified in the text.

5.1.2. Based on the hiatus duration and the erosion amount, an erosion rate is computed for U2 (lines 370-371) but not for the other episodes? Why is it so? Could you please assess it for them as well?

> We have the erosion rates for all episodes, and will include the relevant values of hiatus duration, erosion amount and rate in the text.

5.1.3. Intensities of the erosional episodes largely differ, especially between U2 and U3/UWS. Here, supplementary information is needed to support these varying erosion rates, i.e., higher for the coarser material of U3 than for the finer material of U2. Different geomorphological processes in braided river systems? Please develop.

> There are indeed differences in the amount of erosion on top of each abovementioned unit. The reviewer correctly states that coarsest sediments are more erosion resistant than finer sediments. However, we do not know the characteristics of the eroded material, and it is not unlikely that the overburden that was eroded on top of U6 or U3 consisted of sandy or finer material. This is observed in the fining-up sequence of U1-U2, and clay plugs have been identified in other sections in the Zutendaal gravels (Dehaen, 2021). Therefore, the material that is now present in U3 and U6 does not necessarily have the same grain size as the material that was eroded.
>
> We will include the erosion rates of all potential erosion phases in the text (see reply to comment above).

5.2. Time for the onset of aggradation and of abandonment of these Zutendaal gravels are 654+218/-62 and 540+120/-52 ka, respectively. This important information should be contextualised in a broader geomorphological context: deposition/abandonment on the Campine Plateau has to be compared with the numerical age assessment of main terrace deposits located upstream (25 km southwards) that falls in the same range (725+-120ka; Rixhon et al., 2011). This is particularly interesting as the latter pertains to the terrace staircase mentioned in lines 418-419 that should be posterior to the abandonment of the Zutendaal. Please develop and comment on that.

> Our manuscript focuses on the modelling procedure and its outcomes (in accordance with the journal's scope). In our view, a contextualization of the results (age of abandonment of the Zutendaal gravels) is beyond the scope of the manuscript. Such a discussion should not only include a comparison with the main terrace deposits (Rixhon et al., 2011) about ~25 km upstream of the Zutendaal gravels but also with the Sterksel Formation and the deposits in the neighboring subsiding Roer Valley Graben.
>
> For the interest of the reviewer, the ages of the fluvial deposits in the locality of Romont (Rixhon et al., 2011) that were dated at 725 ± 120 ka are consistent with the range of solutions for the top of the Zutendaal Formation 562^{+211}_{-45} ka. The relation between the Zutendaal Formation, the terrace stratigraphy of the Meuse close to Maastricht, and the main terrace deposits (eventually with several sublevels following van den Berg 1996 and Van Balen, 2000) is still point of debate, and beyond the scope of this manuscript. That is also the reason why we choose not to include the implications of the results for the overall geomorphological evolution of NE Belgium.

5.3. Lines 425-431 briefly discuss the hypothetical and intermittent aeolian cover subsequent to the final gravel deposition. Instead of referring to the Asian systems which are totally disconnected from the study area, I strongly recommend focusing on the numerous studies which dealt with aeolian dynamics from the Eemian onwards in the lower Meuse area (i.e., 15-25 km southwards of the studied outcrop) to obtain useful information on a potential aeolian cover on the Campine Plateau. Please develop based on (among others):

Kesselt: see, e.g., *Van den Haute et al. 2003 (The Last Interglacial palaeosol in the Belgian loess belt: TL age record, QSR)*;

Romont: see *Zens et al. 2018 (OSL chronologies of paleoenvironmental dynamics recorded by loess-paleosol sequences from Europe: Case studies from the Rhine-Meuse area and the Neckar Basin, PalPalPal*) and *Rixhon et al. (2011, cited in this study)* who described a ~3 m-thick loess cover sealing Meuse terrace deposits assigned to the very same time range than the one here (see 5.2).

> We agree that the Asian systems are not necessarily an appropriate analogue for Pleistocene aeolian dynamics in the European sand belt. However, the aeolian dynamics in the loess belt do not necessarily reflect those of the sand belt, where our study area is located. Whereas loess deposition is usually slow and continuous (see comparisons with deep-sea records), the dynamic of sand movement and stabilization is comparatively more episodic, abrupt and of larger magnitude. Loess sections are also generally much better preserved than aeolian sand sequences, as the latter is much easier to remobilize given the loose packing of the material. Obviously, this adds to the uncertainty of the post-depositional evolution of the terrace deposits, which has been taken into account in the scenario analysis. We will add a couple of references, e.g. Paulissen (1973) and Vanneste et al. (2001) in the new text to underpin these statements.

Throughout the whole manuscript: replace "*in-situ* produced cosmogenic…" either by "*in situ*-produced cosmogenic…" (Granger & Muzikar, 2001, EPSL) or "*in situ* produced cosmogenic…" (Dunai, 2011, EPSL).

> We will replace "in-situ" by "in situ".

Line 64: "based on in-situ produced CRN data collected over a ~10 m thick sedimentary sequence" disagrees with both:

- Fig. 4 -> the studied sequence is 7 m-thick;

- Tab. 2 -> the sampling for CRN is performed along less than 6 m of overall depth (0.7 to 6.6 m). This discrepancy is probably related to the fluctuating thickness over time of the sampled fluvial and aeolian deposits (coversands) but this must be clarified.

> We will check for consistency in the text.

Line 78: "bottommost layer/unit" since six distinct stratigraphic units are recognised here.

> We will replace "deposit" by "layer".

Line 83-84: summarising the whole duration by "*total aggradation time*" is, to me, unfortunate as it is clearly stated that, beyond aggradation episodes, erosional and stable episodes are considered as well. I would suggest "*total buildup or formation time*" instead.

> Throughout the text, the "total aggradation time" indeed encapsulates the aggradation phases and the hiatuses that take place in between, and is used in opposition to the post-depositional time of the youngest fluvial unit (U6). For more clarity, we will systematically replace "total aggradation time" by "total formation time" in the text.

Following my previous remark -> figure and caption 1 (Line 88): what does "*(total) exposure time*" refer to here? I guess it means the "*total aggradation time*" previously mentioned (see previous remark about the naming) but it would be much easier for the reader to make all naming uniform.

> We agree with the reviewer on this point. We will adapt the text to make sure that: (i) the total length of all aggradation, erosional or stable phases will be referred to as the "total formation time [kyr], (ii) the

period of time after abandonment will be systematically cited as the "post-depositional time [kyr], and (iii) the sum of the two always represent the exposure time [kyr].

Line 94: "Aggradation is then …".

This correction will be made in the text.

Line 99: "buried at great depth": please provide numerical value(s).

In the text, we will report a value of 10 m as being the shielding thickness that is necessary to avoid significant post-burial production, following e.g. Erlanger et al. (2012).

Lines 129-130: please add the reference for the half-life used for 26Al since Chmeleff et al. (2010) determined the half-life of 10Be only.

Nishiizumi (2004) will specified as reference for the ^{26}Al half-life and Korschinek et al. (2010) in complement to Chmeleff et al. (2010) for ^{10}Be half-life.

Line 165: "…Zutendaal gravels, a gravel sheet…"; please reformulate to avoid the unnecessary repetition.

"gravel sheet" will be replaced by "fluvial deposit' in the text.

Line 166: please check whether "relic surface" is correct. Relict surface seems more correct to me.

"Relic" will be changed in "relict" in the text.

Lines 168 to 170: please refer to Fig. 3b where these main structures are shown.

A reference to (Fig. 3b) will be added in the text.

Lines 200-201-Fig. 4 caption: (i) northward or eastward; please clarify, (ii) discrepancy between the figure showing "T1/T2" and the caption referring to "P1/P2"; please correct.

We think that the reviewer referred to Fig. 3. The T1/T2 legend will be changed in P1/P2 in the panel (b) and in the caption. The longitudinal profile indeed moved eastward and not northward, and this will be corrected in the caption.

Lines 209-210: please provide numerical depth ranges for the sampling of both nuclides (in accordance with Fig. 4).

A numerical depth range of 70 to 660 cm will be added in the text.

Line 222-figure 4:

- field photos of the studied outcrop would be highly welcome to support the log and to clearly exhibit the successive units; presentation of CRN concentration data along the profile (figure) occurs 100 lines before the textual explanation; please change this unfortunate discrepancy. This could imply splitting the figure 4 into two parts.

See comment above. We will provide one figure with picture of the outcrops and sedimentological features, and have a separate figure with the CRN concentrations as function of depth.

Lines 281-282: I guess that the abbr. "UWS" means "Unit Weichselian coversands" but this has to be properly clarified. Why is this abbr. missing at the top of Fig. 4's log? Please add it.

The UWS meaning indeed means “Unit Weichselian Sands” and will be explicated in the text.

Line 297/fig.5:

- in complement to vertical grey arrows, scenario 2 depicts an horizontal arrow at the top of U4-U6 as well. I guess this means a stability episode but this has to be clarified somewhere.

- Vertical (sc.1/top of U6) and horizontal (sc.4/top of U6) arrows are missing in the grey areas; why is it so?

Steps involved in Scenario 2 will be better explicated in the text or in the caption of Fig. 5, to make clear that scenario 2 represents a long stable phase that is followed by a recent, pre-Weichselian, episode of rapid erosion.

The reviewer correctly pointed out that horizontal lines were missing in Fig. 5. They will be added to the figure and the caption will be adapted to specify the existence of stable phases.

Line 418: replace by “Ardennes-Rhenish Massif”

This will be replaced in the text.

Line 451: please clarify “high 26Al/10Be CRN ratios”

We consider the 26Al/10Be ratios as being “high” when they are above 6.75. This will be precised in the text.

References:

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Miall, A.D.: The Geology of fluvial deposits, Springer, Berlin, Germany, https://doi.org/10.1007/978-3-662-03237-4, 1996.

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