Dear editor,

Dear reviewers,

The authors thank the reviewers and the editor for their constructive comments. The comments are shown in black font, and our responses are in regular blue font. We indicated how we adjusted the manuscript in the part that is written in "*Italic*". Figure, page and line numbers in the reviewers' comments refer to the old manuscript, whereas they refer to the new manuscript in our responses. Replies are given in the following order: RC1, RC2 and EC.

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).

The following lines were added in L142-144 in order to illustrate what is new with the modelling approach compared to previous studies. "*Compared to earlier work by e.g. Nichols et al. (2002) or Rizza (2019), the advantage of our approach is the flexible set-up of the model whereby the user can tune the model complexity and its parameters easily as to adapt it to a specific study case.*"

The difference with the equations of Nichols is also pointed on L149-150: "*The radioactive decay of CRN becomes important for Middle to Late Pleistocene deposits, and is explicitly accounted for in contrast with earlier work by e.g. Nichols et al. (2002; 2005).*"

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 slow aggradation of fluvial deposits 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 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 fact, there are several reasons why we attributed the change in concentration between the top of U1 and the bottom of U3 to a long hiatus after the deposition of U1 (and not to slow aggradation of U2): (1) the top of U1/U2 is characterized by a fining-up sequence with strong upward decrease in the d50 and d90 grain size, with an infill of a shallow, abandoned channel (U2) that covers earlier active channel bar deposits (U1). (2) Weathering by oxidation is particularly strong in U1, as it apparent from the MnO and $\frac{Fe_2O_3+MnO}{Al_2O_3}$ ratio for oxidation, and is attributed to intraformational weathering during a hiatus between aggradational phases (see discussion in Vandermaelen et al., 2022). (3) the systematic co-variation between grain size, weathering, and in-situ ^{10}Be concentrations in U1 points to the presence of a long hiatus at the top of U1. We now include in Figure 4 pictures of the outcrop, where the gravels coated with Manganese (taken from U1) are shown (Fig. 4e, f). In short, based on information on sedimentary facies, elemental geochemistry and cosmogenic radionuclides in the profile, we concluded that there is a substantial interruption of the deposition process that explains the increasing upward trend in ^{10}Be in U1. However, we cannot exclude that slow aggradation could eventually have taken place within U2, as the unit was not sampled for CRN. The reason why we didn't sample U2 was based on its very shallow thickness (~25 cm in a ~7 m profile).

We have adapted the text in L441-444:

"Slow aggradation leading to ^{10}Be enrichment with depth (e.g., Nichols et al., 2002; Rixhon et al., 2014) is not directly observed in this sequence. However, it could eventually have occurred in the shallow and

sandy unit, U2, that was not sampled for ^{10}Be. Slow aggradation would further increase the total formation time".

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.

Our first objective was to constrain the depositional and post-depositional history depicted by the different scenarios presented in the paper by only using one very densely sampled depth profile. As the ^{10}Be generally presents lower uncertainties than the ^{26}Al, it was decided to opt for the ^{10}Be. It was decided to add a one ^{26}Al measurements per observed (or detected through simulation) ^{10}Be profile, i.e. one in the U1, one in the U3, and one in the U4/U5/U6.

As you correctly pointed out, the objective was to track ^{26}Al/^{10}Be evolution within the profile and to check if a better age could be constrained by adding ^{26}Al measurements, which appeared to be the case. The values also confirmed the likeliness of post-burial production and thereby invalidated a simple burial approach. One ^{26}Al/^{10}Be value further indicated the possibility of sediments could have been originated from deep where production ratios are > 6.75.

Some modifications where brought to precise the purpose of each CRN investigation on L219-222:

"Seventeen samples were processed for in situ produced CRN analyses: 14 for ^{10}Be to construct a depth profile and 3 for ^{26}Al to analyze the ^{26}Al/^{10}Be ratio in the main sedimentological units of the profile. Given that the uncertainties on ^{26}Al quantifications are larger than those for ^{10}Be, because of the necessity to perform additional measurements of stable ^{27}Al on ICP-AES, the number of ^{10}Be samples outnumbers the ^{26}Al samples".

The total uncertainty on the ^{26}Al quantification is given on L351-353. We accounted for the uncertainties on the ^{26}Al/^{27}Al ratio determined by AMS and the ^{27}Al concentration determined by ICP-AES:

"Considering the total uncertainties of 11 to 17 % on the AMS and ICP-AES measurements, only the ^{26}Al concentration of the uppermost sample is significantly higher than the two deeper ones (Fig. 6).

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 added the CRN depth profile observed after a stable episode beside the erosional depth profile in Fig. 2.

Adapted version of Fig. 2 (L107):

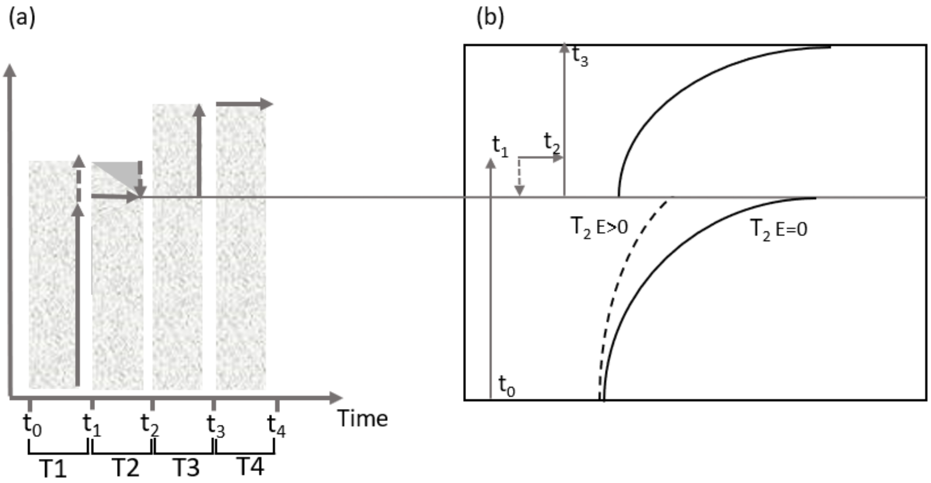


Figure 2: Illustration of the effect of discontinuous aggradation on the depth profile of cosmogenic radionuclide concentrations. (a) The sedimentary sequence consists of two aggradation phases (T1 and T3) that are interrupted by erosion or stability during T2. (b) The in situ produced CRN profile shows two superimposed classical CRN depth profiles. The lower part of the profile represents two depth profiles: the dashed line illustrates the CRN depth profile when T2 undergoes erosion (E > 0), the solid line the CRN depth profile when T2 corresponds to a stability phase (E=0).

- 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 have included information on pedogenic features in this manuscript. To do so, we have added information on the sedimentary facies in Figure 4, and moved the information on the CRN concentrations to a new Figure 6. This also solves the issue that the results on the CRN were already presented in a figure about ~100 lines before the text.

L234:

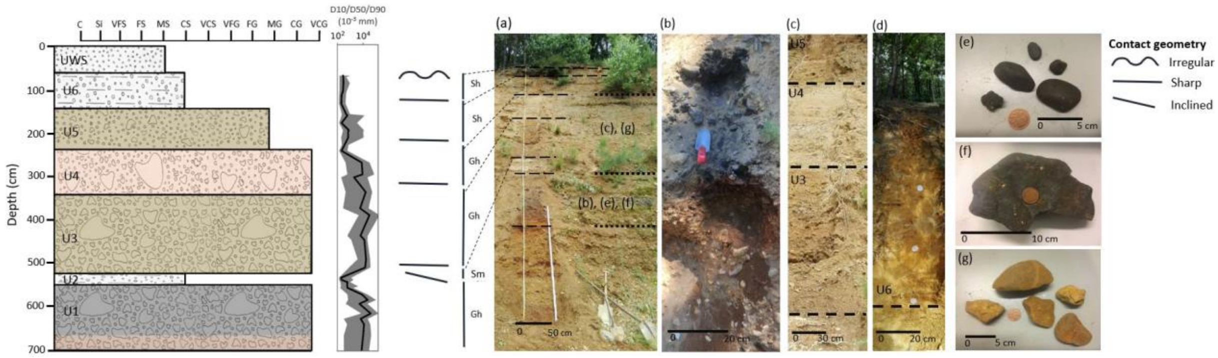


Figure 4.

L346 :

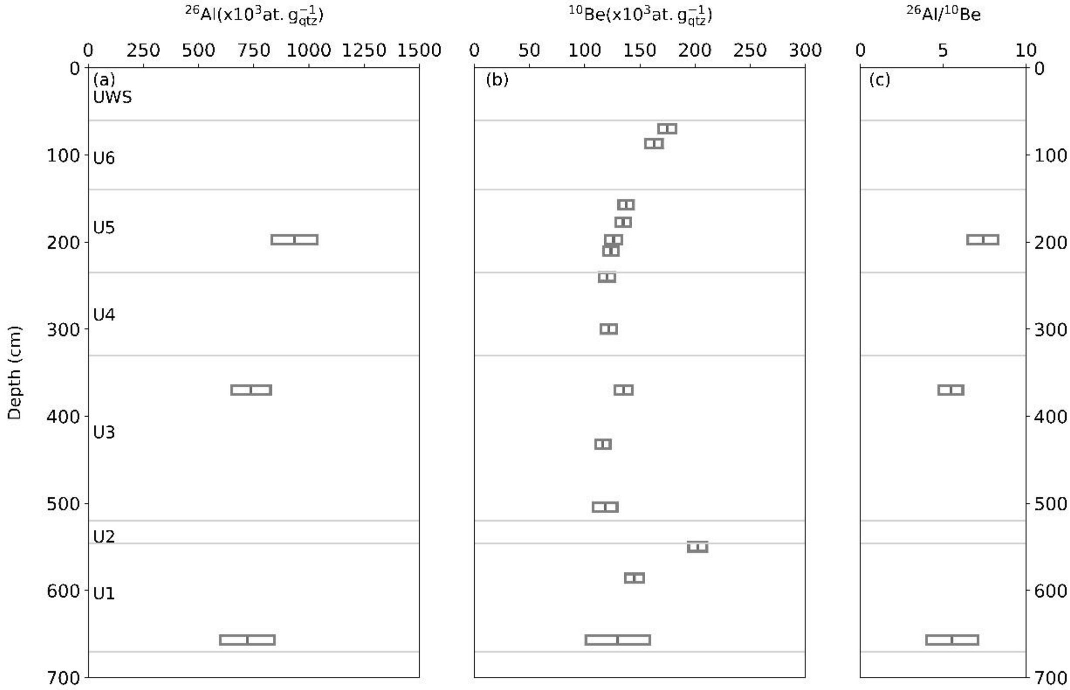


Figure 6.

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). See figure below. The reference was added in L180.

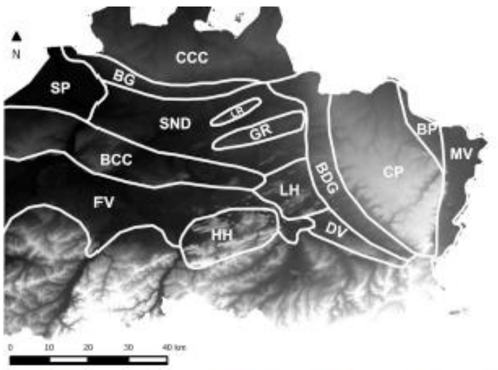


Fig. 12.2 Geomorphological units surrounding the Campine Plateau (from De Moor and Pissart 1992). *CP* Campine Plateau; *BP* Bocholt Plain; *MV* Meuse Valley; *BDG* Beringen-Diepenbeek Glacis; *SND* Schyns-Nete Depression; *LR* Lichtaart Ridge; *GR* Geel Ridge; *FV* Flemish Valley; *DV* Demer Valley;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

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 added reference to Beerten et al. (2018) and De Brue et al. (2015) on L181. They described the existence of Middle Pleistocene braided rivers.

“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).

Paulissen (1983) already described some of the different architectural elements of the Zutendaal gravels. Further research was done by E. Dehaen (2021) who specifically worked on the architectural elements and the sedimentary facies of the braided river deposits of the Zutendaal gravels. She also observed the local presence of overbank fines. The identification of the precited architectural elements all point towards a “Scott type” structure, as defined by Miall (1996). We added reference to Miall (1996) and Paulissen (1983) in the text. L184-187.

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.

More information has been added to the paper about the Weichselian coversands, in L190-193:

“whereby the gravel sheet has been covered by wind-dominated Weichselian sands of the Opgrimbie Member within the Gent Formation (Beerten et al., 2017). Optically stimulated luminescence dating indicates an age range between ca. 23 ka and ca. 11 ka, covering the late Pleniglacial and Late Glacial (MIS2; 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 extent from the end of U6 formation 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 was clarified in L397-403:

“The erosion of the overburden that was present on top of U6 took place before the aggradation of Weichselian coversands, and is estimated at 295^{+208}_{-27} and 325^{+151}_{-43} cm for inherited ratios of resp. 6.75 and 7.40 (Fig. 7f). Constant erosion of the overburden over the period of [500; 1000] kyr is not likely (i.e. scenario 3, Table 3), and a scenario with rapid erosion of the overburden results in significantly better model fit (i.e. scenario 4, Table 3). The current model setup fixed the time interval at [1, 10] kyr which results in optimal values of 295 and 325 m Myr^{-1}. Further research is needed to better constrain the timing and length of such an erosion phase, as they have a strong impact on the erosion estimates”.

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?

The erosion rate of U3’s overburden is now mentioned in L393-395:

"In contrast, the associated solutions for erosion are one order of magnitude higher than at the top of U2, with values of 395^{+120}_{-335} cm (94 m Myr^{-1}) and 430^{+76}_{-363} cm (96 m Myr^{-1})".

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.

We now included the erosion rates of all potential erosion phases in the text (see reply to comment above). It is correct that the erosion rates that are estimated by the model vary between ~4 and 100 m Myr^{-1}. The higher erosion estimates, ~300 m Myr^{-1}, for the overburden that was present at the top of U6 need to be analyzed carefully because of the poor constraints on the erosion length and timing.

We have added the erosion rates in the text, and added more information on the erosion estimate for the overburden of U6 (L397-403): *"The erosion of the overburden that was present on top of U6 took place before the aggradation of Weichselian coversands, and is estimated at 295^{+208}_{-27} and 325^{+151}_{-43} cm for inherited ratios of resp. 6.75 and 7.40 (Fig. 7f). Constant erosion of the overburden over the period of [500; 1000] kyr is not likely (i.e. scenario 3, Table 3), and a scenario with rapid erosion of the overburden results in significantly better model fit (i.e. scenario 4, Table 3). The current model setup fixed the time interval at [1, 10] kyr which results in optimal values of 295 and 325 m Myr^{-1}. Further research is needed to better constrain the timing and length of such an erosion phase, as they have a strong impact on the erosion estimates.*

The erosion rate can vary as a result of uplift and subsequent river incision on the Campine Plateau, but can also vary as a function of the nature of the sediments. We note that the grain size of the sediments varies between coarse sand and very coarse gravel (Fig. 1). It is not unlikely that the overburden that was eroded on top of U6 or U3 consisted of sandy material (as is observed in the fining-up sequence of U1-U2). In other sections in the Zutendaal gravels, clay plugs have been identified (Dehaen, 2021). In our view, the material that is now present in U3 and U6 does not necessarily have the same grain size as the material that was eroded.

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.

We have clarified this part, and now provide a summary at the end of p22 (L404-408) stating that

"Based on the CRN age-depth modelling, the deposition age of the bottommost deposits was constrained at 654^{+218}_{-62} and 669^{+272}_{-58} kyr for inherited ratios of resp. 6.75 and 7.40 (Fig. 7g & h). The total formation time of the sedimentary sequence, including the aggradation phases and the sedimentary hiatuses, is estimated at 90^{+37}_{-45} and 98^{+34}_{-30} kyr for inherited ratios of resp. 6.75 and 7.40 (Fig. 7g & h). The top of the Zutendaal Formation that is exposed in As has a deposition age of 562^{+211}_{-45} and 565^{+269}_{-44} ky for inherited ratios of resp. 6.75 and 7.40."

As pointed out by the reviewer "the manuscript focuses on the modelling procedure and its outcomes (in accordance with the journal's scope)". That is also the reason why we did not discuss the implications of the results for the overall geomorphological evolution of NE Belgium.

We have now added a discussion point at the end of the discussion to contextualize the obtained ages in the evolution of the Meuse valley (L511-537).

*"**4.4 Chronostratigraphical implications***

Based on the age of 654^{+218}_{-62} ka obtained for the base of the sedimentary sequence in As, the Zutendaal gravels exposed at the geosite in As were most likely deposited during MIS 16, and the lower part maybe even during MIS 18 according to Northwest European chronostratigraphical subdivision and correlation with the marine isotope record, Cohen and Gibbard (2011). The uppermost part of the deposits (U6, Fig. 6) in As is dated at 562^{+211}_{-45} ka, thereby yielding a youngest possible deposition age of 517 ka. This age would associate the uppermost deposits *with MIS 14. However, it cannot be ruled out that by that time the Meuse had already shifted its course towards the east of the As sampling site (Van Balen et al., 2000). If this is true, the uppermost depositional sequence in As should correspond to material deposited by another local river, rather than by the Meuse itself. It is possible that the Bosbeek occupied this part of the Campine Plateau after the Meuse had shifted to the east. A fossil valley floor of the Bosbeek River has been identified by Gullentops et al. (1993) only a few 100 meters away from the As sampling site.*

The absolute dating of the Zutendaal gravels contributes to constrain the chronostratigraphical framework of the Campine Plateau, and the Meuse River terraces. Several authors (e.g., Pannekoek, 1924; Paulissen, 1973) have formulated the hypothesis that the Zutendaal gravels may represent the Main Terrace in the part of the Meuse valley downstream of Maastricht. However, the correlation between terrace fragments located at different locations along the Meuse River is still subject of scientific debate. However, it remains interesting to compare the chronological framework of the Zutendaal gravels on top of the Campine Plateau with the Younger Main Terrace levels of the Meuse River in the Liège area and the Ardennes. Van den Berg (1996) used paleomagnetic techniques on the different sublevels of the Sint-Pietersberg Terrace to obtain age estimates of the Main Terrace in the region of Maastricht. The age of the uppermost sublevel was estimated at 955 ka by Van den Berg (1996) and at 720 ka by Van Balen et al. (2000) based on the same data. Both studies agree on an age of 650 ka for the next sublevel. Rixhon et al. (2011) dated the Younger Main Terrace of the Meuse River in the locality of Romont, ~ 25 km upstream of our sampling position, using in-situ produced ^{10}Be depth profiles. They obtained an age of 725 ± 120 ka (mean ± 1 SD). Although this age is somewhat older than the top of the Zutendaal gravels that we dated with CRN depth modelling (i.e., 562^{+211}_{-45}, optimal solution with 95% CI), it is not inconsistent as it overlaps with the CRN optimal solution considering the 95% confidence interval."

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 case study is not an appropriate analogue for Pleistocene aeolian dynamics in the European sand belt. However, we do believe that aeolian dynamics in the loess belt do not necessarily reflect those of the sand belt, where the studied section is located. Whereas loess deposition tends to be slow and continuous (see comparisons with deep-sea records), the dynamics of sand movement and stabilization is much more episodic, abrupt and of larger magnitude. Loess sections are usually much better preserved than aeolian sand sequences, since the latter is much easier to remobilize given the 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 added a couple of references in the new text to underpin these statements (L448-458).

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).

Correction: We replaced "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.

Correction: "10 m thick" has been replaced by "7 m thick" in L64

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

Correction: "deposit" has been replaced by "layer" in L78

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.

In L85 and 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 have systematically replaced "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.

L83-84 and L87: *In the following sections, the total length of all aggradation, erosional or stable phases is referred to as the "total formation time [kyr]", whereas the period of time after abandonment is cited as the "post-depositional time [kyr]". The sum of the two thus represents the exposure time [kyr].*

Line 94: "Aggradation is then …".

L95: correction accepted.

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

L100: the production of CRN as a function of depth is simulated in the model, and accounts for the density and thickness of the overburden. In most cases, the production of CRN is negligible below an overburden of 10 m. We report this value in the text 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.

L133: Nishiizumi (2004) has been specified as reference for the ^{26}Al half-life.

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

L172: "gravel sheet" has been replaced by "fluvial deposit'

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

L173: "relic" has been changed in "relict"

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

L177 and 180: reference to (Fig. 3b) has been added.

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.

L201 and caption: I think that the reviewer referred to Fig. 3. The T1/T2 legend has been changed in P1/P2 in the panel (b) and in the caption. The longitudinal profile indeed moved eastward and not northward, this has been corrected in the caption.

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

L218: a numerical depth range of 70 to 660 cm has been 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.

This has been done – see new figure 4 and 6

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.

L294-296: The UWS meaning has now been explicated in the sentence that follows.

"The two last time periods of any scenario should include 60 to 200 cm aggradation of Weichselian coversands (Unit Weichselian coversands, abbreviated "UWS"), followed by an erosion phase until the thickness of the coversands matches the present-day thickness. "

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?

L299-301: Steps involved in Scenario 2 have now been clarified as follows. "*scenario 1 represents a long and constant post-depositional erosion phase whereas 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 have now been added and the caption adapted with the text "or stability (horizontal arrow)."

Line 418: replace by "Ardennes-Rhenish Massif"

Correction: done on L447.

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

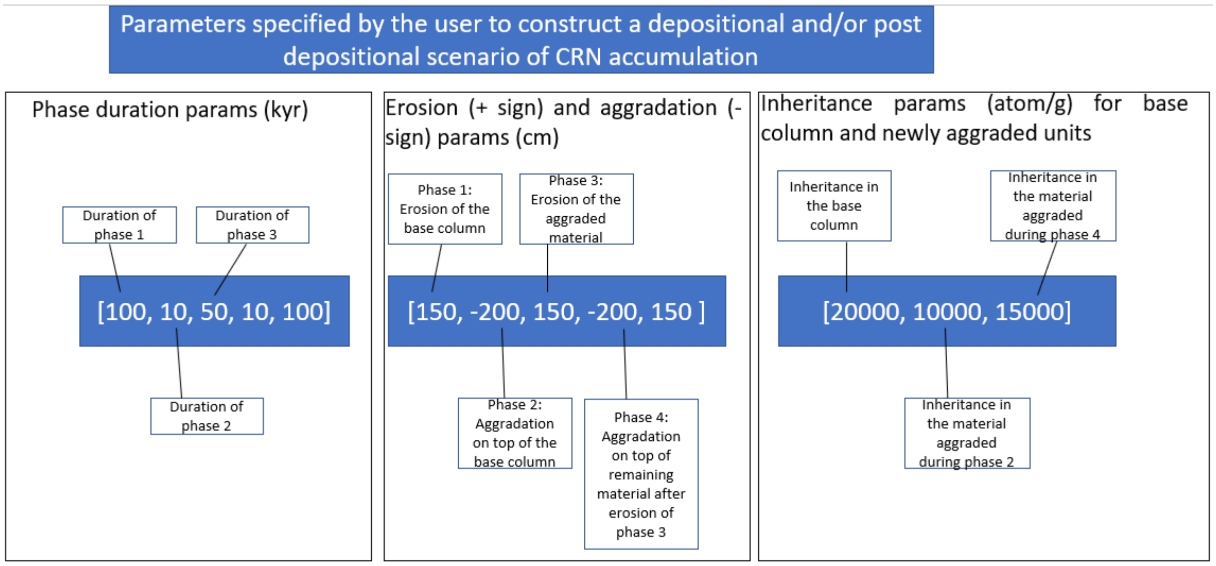
L478: "*to explain high $^{26}Al/^{10}Be$ ratios (i.e., > 6.75) measured at depth"*.

Reply to RC2 comments:

General 1) it is assumed that inheritance is constant over the multiple deposition phases for the specific application of this model. That seems reasonable in this case, but is there a mechanism for the model to incorporate different inheritances for the different deposition cycles? Will the model code also be made available to others?

It is correct that we kept the CRN inheritance constant over the deposition phases. In fact, we did several tests before the final simulations where we tested the possibility of having e.g. different CRN inheritance values for different deposition cycles. We did not include this possibility in the final model simulations, as the scenarios with best model outcomes always had very similar inheritance for the different cycles. We decided to fix this parameter in the model application.

However, the model can account for different inheritances for the different deposition cycles. The figure below illustrates how the model parameters can be tuned by the user. First, the duration of the different erosion/stable/aggradation phases needs to be defined. Second, the amount of material to be eroded (a positive sign by convention in the model, in cm) or aggraded (negative sign, in cm) needs to be specified. Third, the inheritance can be set for the base of the column (that is present at the onset of the simulation) and the material that is added over each aggradation phase.



We are happy to share the code when the paper is accepted. The code will be made available on Git Hub.

General 2) Uncertainties are reported throughout without indication of whether they are 1-or 2-sigma. Please indicate what they are, either with a global statement like "all reported uncertainties are 1-sigme), or with local statements if they are not all consistent.

In fact, the analytical uncertainties are given with 1σ, as is commonly done in CRN studies. We report the ages (obtained by the model) with 2σ to allow intercomparison with other geochronological work in the regio. We have further specified the uncertainty in the text, where appropriate.

We have now added this information in the text on L276-277: "*For each parameter, the value representing the highest density of solutions is given as the optimal model outcome, and the uncertainties are reported with 95% confidence intervals (2σ)".*

We also added this to the caption of Table 2, and Figure 7: "*For each parameter, the most likely value (i.e. value with highest density of significant solutions) is reported as well as its 95% confidence interval (2σ). The 95% confidence interval is derived from the 2.5 % and 97.5 % limits of the kernel cumulative density function."*

Lines 129-130: The 10Be half-live value shown here is the combined value of both Chmeleff et al. 2010 and Korschinek et al. 2010. Please cite both references. Also, I believe the 705 ka half-life for 26Al is from Nishiizumi 2004.

We agree with the reviewer, and added these references on L133.

Figure 3: I find the shaded region on panel (a) difficult to view. I also think this figure would benefit from a simple, regional scale map for context.

We have adapted the figure, and replaced the shaded region by a polyline that delimits the area.

Line 204: What is the geosite? Is it a cutbank? A mine? A little more context would be useful here along with a field photo if available.

It is an abandoned gravel pit (so-called "Hermans quarry", Bats et al., 1995). Since 1994, the site is part of a protected landscape. We have added more information in the text (L213-214): "*We sampled an abandoned gravel pit at the geosite "Quarry Hermans" (Bats et al., 1995) in As…"*

Field pictures have been added in Fig. 4 (L234), also to account for a similar comment from reviewer 1.

Line 210: Was there any reason, aside from cost, to not analyze 26Al for all 14 with the 10Be?

This issue was also raised by Reviewer#1. We copy here our reply :

Our first objective was to constrain the depositional and post-depositional history depicted by the different scenarios presented in the paper by only using one very densely sampled depth profile. As the ^{10}Be generally presents lower uncertainties than the ^{26}Al, it was decided to opt for the ^{10}Be. It was decided to add a one ^{26}Al measurements per observed (or detected through simulation) ^{10}Be profile, i.e. one in the U1, one in the U3, and one in the U4/U5/U6.

As you correctly pointed out, the objective was to track $^{26}Al/^{10}Be$ evolution within the profile and to check if a better age could be constrained by adding ^{26}Al measurements, which appeared to be the case. The ratio values also confirmed the the likeliness of post-burial production and thereby invalidated a simple burial approach. One ratio further showed the possibility of sediments originating from deep erosion layers where production ratios were > 6.75.

Some modifications where brought to precise the purpose of each CRN investigation on L219-222:

"Seventeen samples were processed for in situ produced CRN analyses: 14 for ^{10}Be to construct a depth profile and 3 for ^{26}Al to analyze the $^{26}Al/^{10}Be$ ratio in the main sedimentological units of the profile. Given that the uncertainties on ^{26}Al quantifications are larger than those for ^{10}Be, because of the necessity to perform additional measurements of stable ^{27}Al on ICP-AES, the number of ^{10}Be samples outnumbers the ^{26}Al samples".

The total uncertainty on the ^{26}Al quantification is given on L351-353. We accounted for the uncertainties on the $^{26}Al/^{27}Al$ ratio determined by AMS and the ^{27}Al concentration determined by ICP-AES:

"Considering the total uncertainties of 11 to 17 % on the AMS and ICP-AES measurements, only the ^{26}Al concentration of the uppermost sample is significantly higher than the two deeper ones (Fig. 6).

Figure 5: This is a great explanatory figure.

Thank you

Line 320-321: This statement of uncertainty is a bit awkward. I think it means most concentrations have an uncertainty between 5-7%?

The statement has been corrected on L333-334 as follows: "*The total uncertainties on the measured ^{10}Be concentrations are below 7%, with the exception of the lowermost sample (Heras-02).* "

Line 339-344: So, and I am assuming these are 1-sigma errors, 7.41 +/- 0.92 is statistically 6.75. I get why the authors want to describe the context of a potentially higher depositional ratio, but phrasing the end of this section with "we cannot discard the hypotheses" that they all had a ratio similar to 6.75 seems to imply that an alternative hypothesis is somehow preferable. I think the authors can more simply state that their measured ratio is consistent with that of near surface production for a range of moderate to high erosion rates.

We agree with comment and adapted the text on L357-358: *"The measured ratios are consistent with near surface production …".*

Table 2: Is the mass of the 9Be carrier mass the mass of the carrier itself or the 9Be? If it's the carrier, please indicate the concentration of the carrier in ug 9Be/g.

It is the mass of ^{9}Be that was added in mg. This is corrected in the text, and the column headers of Table 2.

Also, it is somewhat awkward to report ratios for the lab blanks in e-14 and e-15 and the samples in e-12. Also, why were there no blanks for the 26Al reported here? There appears to have been a correction made for a blank according to line 240—does this mean a batch blank is not used, just a general background value? Please explain.

> We have adapted Table 2, and now report all $^{10}Be/^{9}Be$ ratios in E−12, and all $^{26}Al/^{27}Al$ in E-13. For $^{26}Al/^{27}Al$, our blanks did not give sufficient current to be measured correctly at AMS. That's why we used the background value for the blanks from Lachner et al. (2014) as we followed the same lab procedure and were measuring at the same AMS. We have specified this on L251-253.

Reply to EC comments:

Both Reviewers find the paper to be an advancement concerning numerical modelling of cosmogenic nuclide depth profiles in fluvial (braided river) deposits to meaningfully decipher the depositional complexity of such deposits. In this regard, I welcome the paper and find it suitable for publication after some minor/moderate issues are fixed.

Reviewer #1 suggests to account and implement more published work (Nichols et al., 2005), for example […] The scientific implementation of some of the Nichols et al´s concepts however is obviously the most important issue here.

We agree with the Reviewer and editor's recommendations. We have added the references to Nichols et al.'s earlier work in L142-144 and L148-149 (see reply to Reviewer 1's comments).

Reviewer 1 has some more comments regarding insufficient referencing (e.g. field site, aeolian dynamics), so I expect that to be fixed.

Referencing has been added following Reviewer's demands:

About field sites :

- The cryopediment referred to on L170-171 is the Beringen Diepenbeek Glacis, described in Beerten et al. (2018). The reference was added in L179 of the adapted document.
- The braided river channels mentioned on L173-174 are described in De Brue et al. (2015) and Beerten et al. (2018). Both refs were added to the adapted document in L181.
- Architectural elements of the Zutendaal gravels are now supported by Paulissen (1983) and Miall (1996) in L183-186.
- More information is now provided about the Weichselian coversands in L190-193, with references to Derese et al. (2009) and Vandenberghe et al. (2009).
- More information has been given to the sampling site based on Bats et al. (1995) in L212-214.

About CRNs:

- Nishiizumi (2004) and Korschinek et al. (2010) have been added in L133 as reference to justify CRN half-lives.

I also agree with the Reviewer that the obtained age of the gravels should be put into a broader geomorphological context- this is in the end why numerical models are developed and refined- to improve an age, in this case. But, how useful is that age if not discussed and compared with other estimates?!

Based on your comments and reviewer's recommendation, we agree to add a part on the chronostrigraphical implications in the discussion of the paper (section 4.4, L511-537 of the revised document). See for more details comments addressed to Reviewer 1's rebuttal letter.

Reviewer #2 also raised several important points, e.g. regarding the treatment of inheritance and uncertainties, that will have to be clarified. Making the model code publicly available should be something self-evident.

We have addressed this in our rebuttal addressed to Rieviewer 2.

We are of course happy to share the code when the paper is accepted.

Finally, I also wonder how to deal with the much fewer number of 26Al measurements. I don´t expect the authors to go back to the lab and measure some more 26Al, but I need some sort of justification for that. The whole paper is a bit too focused on 10Be, for example you give references for sample treatment/processing that only deal with 10Be (Vanacker et al. 2007 and 2015), and 26Al is not specifically treated. Please fix this.

This issue was raised by both Reviewer 1 and 2, and was addressed in each rebuttal letter that was addressed to them.

With best regards,
Nathan Vandermaelen and al.

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