This contribution proposes an advanced approach to account for composite cosmogenic nuclide-depth profiles resulting from successive aggradational, erosional and stability episodes building up a fluvial sequence (braided river system). Apart from a few existing references which tackled similar issues (e.g., Nichols et al., 2002 & 2005; Rixhon et al., 2014), an exhaustive treatment of this complex topic is still lacking. Owing to the thorough numerical modelling procedure, along with its detailed presentation (somehow missing in the aforementioned references), this contribution is accordingly welcome. Importantly, it also demonstrates the need of high-resolution CRN sampling to decipher complex aggradational/erosional/stability episodes in thick fluvial sequences deposited by braided river systems. However, some comments/issues developed below must be considered before publication in *Gchron*.

General remarks

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 ^{10}Be and ^{26}Al*", they detail how ^{10}Be and ^{26}Al 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).

356 *K. K. Nichols and others—Late Quaternary history of the*

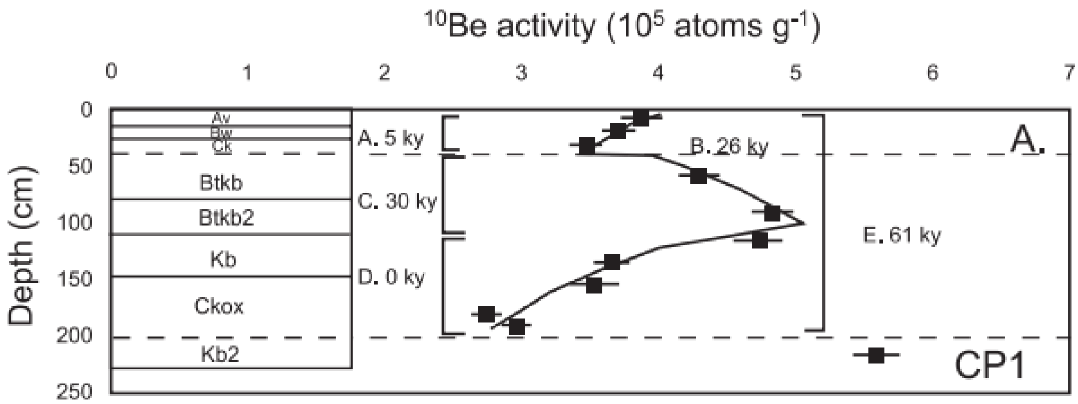

Fig. 6. Nuclide data, ^{10}Be data (A) and ^{26}Al data (B), and soil horizons for CP1 showing three stable/eroding surfaces and three depositional episodes. Dashed lines represent top of buried soil horizons. Data points represent mid-point of depth interval and black lines show model fit. Error bars represent 1σ analytical uncertainty. Interpreted history represented by lettered intervals: A. Most recent depositional episode, a pulse of sediment, followed by 5 ky of stability. B. The second stable or slowly eroding surface representing 26 ky. C. The second depositional event, slow aggradation at 19 ± 1 mm ky^{-1} for 30 ky. D. The third depositional event, a rapid pulse of sediment that was deposited quickly at 61 ky. E. Total time represented above 190 cm is 61 ky. The third stable/eroding surface is represented by a buried soil at 190 cm that lasted at least 36 ky.

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).

2. A large difference between the number of processed samples for ^{10}Be (#14) vs ^{26}Al (#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 ^{10}Be-vertical sampling is appropriately used to decipher the composite concentration profile, the three ^{26}Al data are "merely" used to punctually follow the ^{26}Al/^{10}Be 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 ^{10}Be data are used for that).

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

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.).
- 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?

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

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.

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.
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?
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.

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.

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).

Specific remarks:
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).

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.

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

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.

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.

Line 94: "Aggradation is then …".

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

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

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

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

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

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.

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

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.

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.

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?

Line 418: replace by “Ardennes-Rhenish Massif”

Lines 437-440: important statement indeed.

Line 451: please clarify “high $^{26}Al/^{10}Be$ CRN ratios”