We thank referee #2 for having taken the time and effort to read the paper in great detail, and provide us detailed suggestions. We address the most important reviewer's concerns below, and refer to the reviewer's text (green) when relevant.

As a preamble, we would like to state that there seems to be a misunderstanding regarding the main objective of the paper, which is (see lines 70-75 and copied below) to perform a Bayesian inference of a long-term erosion rate from a sampled CRN depth profile. The method is then illustrated using a 10Be concentration depth profile from NE Belgium. In the paper, we do not suggest that the results from the depth profile are representative for the entire Campine area, nor that we aim to determine the impact of landscape evolution in NE Belgium on potential nuclear waste disposal. The latter is not within the scope of this research paper, and we believe that the reviewer's comments related to nuclear waste disposal are therefore not relevant.

> Lines 70-75 of the discussion paper read as follows:
> *The overall objective of this study is to constrain within a Bayesian framework the rate and amount of post-depositional denudation of the headwaters of the Nete catchment. The latter is part of the larger Scheldt basin and is used herein to test and demonstrate the application of Bayesian inversion to CRN concentration vs. depth profiles. The Nete catchment is an interesting test case because the upstream areas of the catchment are located at the northwestern edge of the Campine Plateau, which is covered by coarse gravelly sand from the Early-Middle Pleistocene Rhine and thus constitutes a fluvial terrace from which the depositional age nor the exposure age of the sands is well constrained (Beerten et al., in press).*

MAJOR COMMENTS

Reviewer#2.

A large part of the manuscript is dedicated to discussing in detail the geological history of the study area in the last few million years, however, almost completely ignoring at least 5 cold periods in the past 0.5 Myr. The glaciations, which most probably affected this region in many ways on multiple time-scales were taking place just north of the area implying a complex geomorphological history (deflation, permafrost, loess deposition). Additionally, being situated close to the sea, the site might have experienced multiple transgressions due to isotactic rebound and sea level changes (not necessarily 0.5Myr ago or before). The study is also ignoring the fact that the Nete catchment is a highly urbanized area and not immune to recent neo-tectonism.

Reply: We agree with reviewer #2 that the long-term topographic evolution of lowland Europe is affected by periglacial processes during glaciations, landscape instability during glacial-interglacial-glacial transitions, uplift and (absolute) base level lowering, and will take this along in our revision of the manuscript. However, we do not agree with the suggestion that "glaciations…, loess deposition, … and multiple transgressions" might also have affected geomorphological processes in the area. Based on previous geomorphological and geological studies in NE Belgium, we can exclude that the northern part of the Campine area in Belgium was affected by an ice sheet during any of the time periods considered in the paper. Also, there are no indications that the area would have been covered with loess (as the site is located in the "European Sand Belt", see introduction and Figure 1). Next, as stated in lines 77-79, the area has not been experiencing marine conditions since the start of the Pleistocene, even though it is situated close to the sea as the reviewer correctly observes. Finally, it is correct that large parts of the Campine area are urbanized, but we believe that this is not relevant for inferring long-term geomorphological processes when sampling locations are adequately selected.

Based on the results from one depth profile, we cannot make conclusive statements on the long-term erosion rates of NE Belgium; but can formulate some hypotheses for further testing. We will clarify this nuance in our revised manuscript. We observe – for example – that the erosion rate at the sampling location is relatively large compared to global compilations of outcrop erosion rates. This result in itself is novel and diverges from current theories on landscape evolution of NE Belgium that suggested very low erosion rates for the Campine Plateau. In the paper, we suggest that erosion is related to the erosivity of the substrate and potential base level lowering in the North Sea.

Reviewer#2.
My major concern with the current publication is that it suggests a considerable methodological achievement with the Bayesian model and Monte Carlo type simulation based on only 7 data points (as 2 were discarded). However, using only the Be-10 concentration of the top sample in the profile and doing a quick exploratory calculation in CRONUS, using the same parameters as in the paper, yields almost the same value as the complex model, i.e. 31+/-3.11 m/Myr. The similarity in these values might be a simple coincidence, but both of them are equally valid given that there is no evidence to suggest otherwise (see Figure1)

Reply: We are a bit puzzled by the reviewer's statement that *"my major concern with the current publication is that it suggests a considerable methodological achievement with the Bayesian model and Monte Carlo type simulation based on only 7 data points (as 2 were discarded)".* To the best of our knowledge, this is the first application of the Bayesian framework to CRN depth profile analysis. As detailed in our reply to the comments of reviewer #1, we argue that Bayesian inference is a very powerful tool to rigorously evaluate how well the inferred parameters are resolved by the available measurement data. We refer to our reply to reviewer #1 for more technical details.

The reviewer suggests that CRN depth profiles are not necessary to obtain relevant information on erosion rates and states that "*using only the Be-10 concentration of the top sample in the profile and doing a quick exploratory calculation in CRONUS, using the same parameters as in the paper, yields almost the same value as the complex model, i.e. 31+/-3.11 m/Myr."* This comparison seems flawed to us, as it ignores the impact of pre-deposition inheritance on the total concentration of cosmogenic radionuclides. In alluvial landforms, such as the Campine Plateau, pre-deposition inheritance cannot be neglected (see e.g. Anderson et al., 1996; Braucher et al., 2009; Vassallo et al., 2011; Hedrick et al., 2013). Hence, the importance of CRN depth profiles for determining the exposure age and denudation rates of the sampling site on the Campine Plateau.

Furthermore, we noted that reviewer #2 derived a denudation rate of 31 m/Myr, that is actually different and smaller than the mode of our marginal posterior distribution (the maximum a-posterior (MAP) or most probable value) which is about 44 m/Myr. Also, we believe that the reviewer's uncertainty estimate of +/- 3.11 m/Myr (one sigma) is unrealistically small. Applying a Bayesian inversion framework to the 7 measurement data points, we obtain a standard deviation of 9 m/Myr (see lines 266-267 and Figure 7b). We therefore argue that our estimate and the reviewer's one are not equally valid, and consider our estimate to be much more accurate.

Reviewer#2: Note in Table 2, Page 18 10Be/9Be ratios have 4% uncertainties but 10Be concentrations have only 1% uncertainties. Obviously this is not possible, as concentration values are obtained from the ratios.

Reply: With respect to measurement uncertainty: there was indeed a mistake in the ^{10}Be concentration errors as they were reported in Table 1, and we thank the reviewer for pointing this out. The corrected values for the ^{10}Be concentration errors are in the range 6500 – 7000 atoms/g.

Reviewer#2: This would also imply that the prediction uncertainty interval section in Line 234-240 is incorrect as the authors add a max. analytical measurement error to the dataset of 2000 atoms/g, instead of ~ 20000 atoms/g. This error in data reporting also negates the conclusion of lines 274-282.

Reply: After correction, the ^{10}Be concentration errors are max. 7000 atoms/g (and not 20,000 as suggested by the reviewer). Since we infer σ jointly with the other parameters, this does not affect the derived posterior parameter distribution. However, it will modify the total predictive uncertainty (light gray band in Figure 9) in the sense of a reduction as the σ_m value in the equation of line 239 becomes 7000 atoms/g. We would also like to stress that this still illustrates the effect of model errors, although its effect is less strong. Our best fitting error is about 10000 atoms/g and the 3000 atoms/g difference can thus be attributed to model errors. We will correct Table 1, Figure 9 and the associated discussion in the revision.

Reviewer#2: I would also question the upward fining trend, as data presented in Figure 5 and Figure 6 are not entirely supporting this conclusion. Further, I would question the exclusion of two crucial points in Unit D and B, ie. MHR-II-04 and MHR-II-06 data points, which coincide with different stratigraphic layers, as these might represent erosional events or depositional events, (i.e., superimposed profiles).

Reply: Concerning the sample selection: out of the 9 samples, we selected 7 samples for the Bayesian model. Two samples were excluded from the analysis, as their CRN concentration was measured on the grain size fraction 250-500µm (instead of 500-1000 µm). Our results show that the CRN concentrations of these two samples are higher than expected when one assumes a monotonic decline of CRN concentration with depth. Currently, it is not possible to know the exact reason for the observed difference in CRN concentration, as this can either reflect a grain-size dependent CRN concentration (see e.g. Schaller et al., 2001 for European river sediments; Carretier et al., 2015), or non-stationary sedimentation. Further analyses are necessary to make a conclusive statement.

Reviewer#2 concludes that : *"The real benefit of the Bayesian approach presented here would be the ability of solving these problematic cases of depth-profiles, otherwise the top sample is enough for a rough age or erosion calculation. In summary, in my opinion, the Be-10 dataset is not suitable for the complex numerical analyses that it is subjected to. These data simply do not support the conclusions of this study."*

Reply: As pointed out above, we disagree with this statement. For the reasons mentioned above, one cannot infer a site-specific denudation rate and exposure age from one sample taken at the top of an alluvial deposit. In our opinion, the strength of the Bayesian approach is that it provides a robust way for exploring the data information content that is hidden in CRN depth profiles. Hence, we consider that our paper provides a fair illustration of the benefits of the Bayesian approach.

MINOR COMMENTS

Line 44: I think it is possible to resolve processes over the last 2Myr (See Balco et al 2013) instead of 1Myr as the authors mention. Also in terms of general cosmogenic Be-10 literature it might be more suitable to quote Dunai 2010 or Gosse and Phillips 2001 rather than Hancock 1999 and Heine 2009.

Reply: Correct, we will reformulate this sentence in the revised document, and insert the references suggested.

Line 128: It will not change too much on the results probably, but muon attenuation lengths are different (see Braucher et al 2011), and SLHL production rate closer to 4 at/g/y (see Borchers et al 2015).

Reply: (See also reply to reviewer #1): As written in lines 129-130 of our submitted manuscript, we used the same CRN production rates as the ones used by Rixhon et al. (2011). This choice was made for the sake of comparison – as in the study by Rixhon and coworkers a Middle Pleistocene Meuse terrace close to Liège (Belgium) was investigated. However, we now agree with the suggestion that we should use state-of-the-art knowledge on scaling and production rates of CRN's. We will use the updated data suggested by the reviewer and rerun the Bayesian model.

Lines 164-186: Section 3.3 (Bayesian inversion) is word by word the same as in Minet et al 2015.

Reply: The second author, Eric Laloy, has written the section on Bayesian statistics in the present manuscript and is the co-author who has done the Bayesian analysis in the paper by Minet et al. (2015), of which he is second author as well.

Line 262: "-0.13" is not a statistically significant correlation

Reply: When writing "significant" we did not mean "statistically significant", which only makes sense in the context ofd statistical hypothesis testing. To avoid any confusion, we will replace "significant" by "substantial" or similar in the revised text.

Figure 6: Note that unit layering conventionally is starting from top as "A" and progressing downwards, not the other way.

Reply: We believe that this is a matter of taste – examples of the ordering we use can be found in the literature.

Presentation of figures might require revisiting

Reply: We will check every figure for the revision and provide improvement if deemed necessary.

Reviewer#2 ends the review stating that : "One of the motivations (including funding of this project) behind this study was to understand the implications of long-term landscape evolution of this area on radioactive waste management. Unfortunately this study failed to address this aspect"

Reply: We believe that this statement is out of scope. As stated earlier, the aim of the paper is to constrain a site-specific denudation rate using a Bayesian framework. It is nowhere written in our manuscript that our paper contributes to understand the implications of long-term landscape

evolution on potential radioactive waste disposal. Research on long-term landscape evolution and denudation rates can be informative for land management, and waste disposal research.

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