In this manuscript, the authors apply a Bayesian inversion model on a Be-10 depth profile at a single site to investigate the erosion rate of an approx. 3000km^2 area. 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.

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)

^{10}Be results:

Results not dependent on spallogenic production rate model:				*Erosion rates -- constant production rate model:*			
				Scaling scheme for spallation: Lal(1991) / Stone(2000)			
Sample name	Shielding factor	Production rate (muons) (atoms/g/yr)	Internal uncertainty (m/Myr)	Erosion rate (g/cm2/yr)	Erosion rate (m/Myr)	External uncertainty (m/Myr)	Production rate (spallation) (atoms/g/yr)
Belgium	1.0000	0.076	0.43	0.00527	32.92	2.49	4.27

Erosion rates -- time-varying production models:

Scaling scheme for spallation:	Desilets and others (2003,2006)			Dunai (2001)			Lifton and others (2005)			Time-dependent Lal (1991)/Stone (2000)		
Sample name	Erosion rate (g/cm2/yr)	Erosion rate (m/Myr)	External uncertainty (m/Myr)	Erosion rate (g/cm2/yr)	Erosion rate (m/Myr)	External uncertainty (m/Myr)	Erosion rate (g/cm2/yr)	Erosion rate (m/Myr)	External uncertainty (m/Myr)	Erosion rate (g/cm2/yr)	Erosion rate (m/Myr)	External uncertainty (m/Myr)
Belgium	0.00495	30.92	14.13	0.00491	30.72	3.11	0.00501	31.34	3.09	0.00512	32.03	2.62

Therefore, I would question if the rather complex calculations presented in the paper are valid if the underlying data is scarce and might be also incorrectly presented. Note in Table 2, Page 18 ^{10}Be/^{9}Be ratios have 4% uncertainties but ^{10}Be concentrations have only 1% uncertainties. Obviously this is not possible, as concentration values are obtained from the ratios. 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 *"It is interesting to note that with a range between 1.4 × 103 and 2 × 103 atoms/g, the analytical measurement errors are more than 5 times smaller than the values taken by σ. This nicely illustrates the effect of model errors. If the model would have been perfect, the achieved RMSE values and σ*

distribution should indeed have been within this measurement error range of 1.4 ×103 to 2 × 103 atoms/g. "

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).

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.

Minor issues

(1) 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.

(2) 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).

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

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

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

(6) Presentation of figures might require revisiting.

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.