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Earth Surface Dynamics Discussions

Interactive comment on "Constraining Quaternary erosion of the Campine Plateau (NE Belgium) using Bayesian inversion of an in-situ produced ^{10}Be concentration depth profile" *by* Koen Beerten et al.

Anonymous Referee #1

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The authors present a Bayesian inversion method for analyzing in situ 10Be depth profiles and use it to model data from a fluvial deposit on top of the Campine Plateau. They do a great job describing their inversion method and the framework of their Bayesian inversion approach appears sound. However, there are critical issues with how the model is applied to their dataset that need to be considered.

I think fundamentally there is some confusion over the information realistically preserved in cosmogenic depth profiles. In some cases, a measured depth profile can converge to a unique solution for both of age and erosion rate (as described by Braucher

et al. 2009). I would argue that these cases are very rare as it requires characterizing both the spallogenic and muogenic production pathways with the dataset. Most depth profiles in sediments cannot do this; typically because of large scatter in concentration with depth (which is the case for the profile presented here). Without such characterization and without other independent geologic constraint, a profile will not yield a unique solution for age and erosion rate, but it can still yield a minimum exposure age (or zero erosion age), and a maximum erosion rate for $t \rightarrow \infty$. I have included a graphic from Hidy et al. (2010) illustrating this point with an unconstrained depth profile simulation (Fig. 1):

In this graphic the red dots represent 100,000 Monte Carlo generated age-erosion rate solutions that fit within a 95% confidence of all parameter uncertainties; the blue dots represent the 500 best fits. Like many depth profiles, there is no unique age-erosion solution possible without adding some constraint to either age or erosion rate (in this case, a 30 cm net erosion cutoff was used to resolve an age). The same solution space pattern is found using the Campine Plateau dataset, but with different asymptotes for minimum age and maximum erosion rate. When the authors interpret the Campine data, they impose an age constraint of 0.5-1 Ma, which essentially restricts their solution space to the erosion asymptote and removes any variance in erosion that would be present if younger ages were permitted. It should be noted that this is perfectly OK to do if the age constraints are robust. However, depth profiles are not necessarily applicable to the timespan since deposition, and unfortunately that seems to be the case for the Campine data.

Vital to the depth profile technique is that erosion is assumed to be steady-state, and not episodic. A significant episodic erosion event (~3-4 m) can almost completely remove a previous exposure/erosion rate signal. Sure there will be some residual signal from the deeper muogenic component, but this signal will be fairly constant with depth and probably indistinguishable from inheritance. This means that for deposits where a complicated erosion history is likelyâĂŤparticularly in old deposits like those in the Campine areaâĂŤa depth profile's minimum exposure age is likely not linked to depositional age, and its maximum erosion rate is likely not applicable to the time since deposition. The age-erosion rate solution space is basically only relevant to the exposure conditions following the episodic erosion. Considering this, I have the following major concerns with how the authors interpret their depth profile:

1) The authors constrain the age of their profile to between 0.5-1 Ma based on pre-existing age constraints for deposition age. However, the landform that developed following deposition could be, and likely is, significantly younger. Any age constraint applied to a depth profile must consider the time over which the surface can be assumed to be in steady-state erosion. The authors realize there is a problem in the discussion, noting that if their calculated erosion rate was correct over that timescale then it would imply an impossible amount of erosion. The authors then invoke non-steady-state erosion as the likely explanation, suggesting erosion started after 450 ka. Notably this is outside the age constraints placed on the model. What then does the erosion rate of 44 mm/ka mean? Over what timescale is this relevant? This erosion rate was obtained when assuming the surface underwent steady-state erosion for 0.5-1 Ma, but the authors argue that this couldn't have happened. If the age constraint that led to resolving the erosion rate is not viable, then the erosion rate is also not viable. The probability distribution of the erosion rate parameter needs to be re-calculated without a constraint on a lower age limit.

2) In lines 254-258 the authors reject a minimal erosion rate because it implies an apparent exposure age of 21.5 ka that conflicts with constraints on the depositional age of the deposits (0.5-1Ma). I propose that this exposure age may actually be correct, or at least closer to the age of the stable surface. What if an episode of erosion occurred at $\sim$21.5 ka such that it wiped out earlier traces of exposure and the surface has been relatively stable ever since? How can this scenario be ruled out? Interestingly, this would suggest episodic erosion at the LGM, which seems at least plausible and shouldn't be outright dismissed. It might be useful to compare soil development at this site with data

from dated soils that may be available in the region to see if this is a scenario that can be supported or refuted.

Other minor comments:

1) Bayesian/Monte Carlo-style models have previously been applied to cosmogenic depth profiles (see version 1.2 of Hidy et al. (2010) described in Mercader et al. (2012) supplemental code; see Marrero et al. (2016), CRONUS web-based calculator). The specific MCMC algorithm employed here is new, but the authors seem to imply that this is the first depth profile model to explicitly treat parameter and model errors with Bayesian statistics, which is not the case. Work that is highly relevant to the content of this paper seems to have been ignored, starting with the original depth profile model paper of Anderson et al. (1996).

2) Thicknesses do not appear to be given for the profile samples. Or maybe I missed this? I was unable to reproduce their specific model parameter results without this information.

3) Our knowledge of production rate scaling has increased significantly over the past 5 years. The authors may want to use something more up-to-date based on all the recent calibration data. I would recommend pulling a site production rate from one of the many online calculators (e.g. CRONUS, CREP). I believe there was also an update to the Braucher muon scalingâĂŤsee Braucher et al. (2011), I think.

References: Anderson et al. (1996), Explicit treatment of inheritance in dating depositional surfaces using in situ 10Be and 26Al, Geology. Braucher et al. (2009), Determination of both exposure time and denudation rate from an in situ-produced 10Be depth profile: A mathematical proof of uniqueness. Model sensitivity and applications to natural cases, Quat. Geo. Braucher et al. (2011), Production of cosmogenic radionuclides at great depth: A multi element approach, Earth and Planetary Science Letters. Hidy et al. (2010), A geologically constrained Monte Carlo approach to modeling exposure ages from profiles of cosmogenic nuclides: An example from Lees Ferry, Arizona, Geochemistry, Geophysics, Geosystems. Marrero et al. (2016), Cosmogenic nuclide systematics and the CRONUScalc program, Quaternary Geochronology. Mercader et al. (2012), Cosmogenic nuclide age constraints on Middle Stone Age lithics from Niassa, Mozambique, Quaternary Science Reviews.

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