# *Interactive comment on* "Arable soil formation and erosion: a hillslope-based cosmogenicnuclide study in the United Kingdom" *by* Daniel L. Evans et al.

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"Arable soil formation and erosion: a hillslope based cosmogenic nuclide study in the United Kingdom"

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Main Purpose of paper

The primary aim of this paper is to quantify soil formation rates for both agricultural land (RFF site) and for forested woodland (CW site) in the UK using the concentra-

tion of cosmogenic 10Be in saprolite from which these soils are derived. From the determined rates and previously reported soil erosion rates from agricultural land, the authors calculate soil lifespans, which can help inform policymakers and stakeholders about best land use practices.

Scientific significance

This paper contains a number of advances in our understanding of soil formation rates. 1. The second study of soil formation rates in the UK. 2. Understanding how soil formation rates vary down a hillslope transect, with varying levels of soil thickness, and for agricultural land. 3. A compilation of global soil formation rates from published papers that also utilized cosmogenic nuclides to determine soil formation rates. 4. Calculation of soil lifespans from soil formation and erosion rates.

These are significant accomplishments that warrant publication.

Scientific quality

This paper utilizes established methods, which are applied to novel sites in the UK. The methods are correctly applied and interpreted, and a sound logic flows from the structure of the paper. I think there is one small mistake in their calculations. It occurs when the authors are calculating the production rate of 10Be at the depth of their sample. In the method I propose, I get soil formation rates which are 7 to 29% higher than the authors' calculations. This does not significantly alter the main narrative, findings, or conclusions of the paper, and I think the paper is very worthy of publication.

Suggested correction for production rate calculations

Equation 1 is the correct equation to use to determine the saprolite erosion rate, which then translates into the soil formation rate. However, I would like to suggest a different way to calculate the production rate at a sample's depth (the numerator in the equation).

The authors have appropriately calculated surface production rates of cosmogenic 10Be due to spallation, fast muons, and stopping muons based on the Stone, 2000

scaling scheme. Then, to calculate the production rate of cosmogenic 10Be at the depth the samples were collected, the surface production rates are scaled with an exponential function based on the depth times the density of the overlying material. The product of depth times density is the "mass depth." In this paper, the authors appear to use the density of saprolite (2.2 g/cmˆ3) to calculate the mass depth of the samples. But the material that overlies the saprolite is soil, which should have a lower density than saprolite. I think the appropriate density to use to calculate the production rate at the sample's depth is that for soil because that represents the mass depth that overlies the soil-saprolite boundary, and the authors have (correctly) assumed that the soil thickness has not changed over time.

If one were to use the density of soil as the overlying material, instead of saprolite, the mass depth of the samples would be lower because the density of soil is lower. This would then result in a higher production rate at the depth of the samples. Then, when calculating erosion rates from equation 1, this would result in higher erosion rates because an increase in the numerator in equation 1 would require an increase in the denominator (where the erosion rate goes) to result in the same concentration of 10Be that was measured in the sample. I would like to emphasize that this impact is small, is fairly uniform across all the sample sites, and does not change the main findings of the paper.

I have recreated the authors' calculations, and performed my own calculations on the attached spreadsheet. In my experience with trying to measure soil density, soils typically have a density of 1.5 – 2.0 g/cmˆ3. In my calculations I used a value of 1.8 g/cmˆ3 as an approximate median value to my anecdotal evidence, but I would leave it to the authors to find an appropriate soil density value to use.

There are two important things to note in how I have done my calculations:

1) To calculate the mass depth, you want the depth times the density of the overlying material. For the samples at the top of the saprolite, this is simple, and is just the depth

times the density of the soil. But for the samples that are 50 cm below the top of the saprolite, this is the cumulative sum of the soil and the saprolite above the sample. This is also simple to calculate, it is the density of soil times the depth of the soil, plus the density of saprolite times 50 cm, because these samples were collected that far below the top of the saprolite.

2) This correction only applies to the numerator in equation 1. It does not apply to the denominator, which also has a depth times density term. In the case of the denominator, this is the place where erosion of the overlying material comes into the exposure model. The authors have concluded that the soil thickness does not change at a timescale that would affect the concentration of 10Be in the saprolite. I agree that this is a valid assumption, and the result is that only the saprolite changes depth with time in this exposure model. This means that only saprolite is "removed" as mass above the sample site, so the material that is eroded in equation 1 is saprolite. Thus, the density of the material in the denominator of equation 1 is correctly used at 2.2 g/cmˆ3.

The spreadsheet I have included has two tabs. The first tab on the left (Evans et al. Calculation) recreates the authors' calculation to verify that they used 2.2 g/cmˆ3 in the numerator and denominator of equation 1. The second tab contains my calculations to determine the production rate at the depth of the sample, and each corresponding new saprolite erosion rate. I've also calculated the percent difference between my calculations and those from Evans et al. Using the method I propose, the saprolite erosion rates are 7 – 29% higher than determined by Evans et al.

Although my proposed method results in higher saprolite erosion rates than those shown by the authors, the same trends discussed by the authors remain true, and the discussion and conclusions of the paper still hold. That is, the rates shown in figures 2, 3, and 5 would show the same general trends, but the numbers would be updated. Figure 4 that puts the calculated rates in context globally would have to be updated too, and that portion of the discussion could be quickly updated. Many of the tables would

need to be updated.

I suppose it's worth noting that Evans et al. have calculated the production rate of 10Be at the top of the saprolite sample. They could have made an additional correction for the sample thickness. I don't remember any discussion about sample thickness in the paper. This correction would be small, and would likely change all the numbers by only a percent or two Of course, this would depend on how thin or thick the samples were, and what the range of sample thicknesses was for the samples. I suppose that it is not necessary that they do this correction, especially if the samples are all about the same thickness and not more than a few centimeters thick. But it just occurred to me that this is missing.

Please let me finish by welcoming any discussion about my method, or that used by the authors. I think I have correctly calculated the production rate at sample depth, but I am open to discussion on the topic. If the authors think that 2.2 g/cm^3 is the correct density to use for the numerator, I would love to hear their thoughts on the question and would consider the other number.

Line by line comments:

P3, L28: You say only 252 of 1850 samples come from 10Be data. Did you compile all 1850 data points? This sounds like your compilation, and I wonder if there's more work that you've done that should be shared and part of this discussion.

P4, L31: do you mean "small" instead of "soft" when describing the grain size of the sandstone at the CW site?

P5, L3: What is the aspect of the sites? Is one north-facing and another south-facing? If you know this, it could be interesting to report as it could be a factor in the difference between the sites.

P5, L19: I don't think the citation for the FAO WRB is correctly formatted for this journal, but I'm not the expert. Is there a year?

P5, L24: I don't know what the acronym LFH stands for, and I'm not sure it's spelled out previously. If this is the first time it's used, please write it out fully.

P5, L25: This is simply a style thing, and certainly is due to my own biases. But as I read this page, I wanted to ask, "If the area had significant sediment transport from glacial outwash since the last glacial maximum, and there is a pebble layer in the stratigraphy, how certain are you that these soils are really derived from weathered saprolite?" I think the answer is, "These soils are still 82% and 94% sand, so there doesn't appear to be much input from glacial outwash into these soils." If I were writing this, I'd probably say something explicitly about this, but that's just my style and I don't think it's necessary to include this.

Also, what were the land-use practices at RFF? I thought something was written about tilling at that site, but I can't seem to find it now.

P7, L17: Equation 1 is the correct equation to use, but it does not have a time element in it. So your description of the equation above this seems a bit confusing. I think what you're missing is that once enough time has passed, the system will approach an equilibrium nuclide concentration that is the balance between the production and erosion rates. Assuming this has been reached, you can use equation 1.

You could also be more explicit that the saprolite erosion rate directly translates into the soil formation rate.

P7, L24: Were the soil pits dug and the samples collected from a vertical profile? Or were they collected from a slope perpendicular profile? Another way to put this is, was depth measured vertically or perpendicular to slope?

P8, L27: It may be worth adding a little discussion about the timescales of these measurements. The 10Be measurements represent soil formation rates that have been going on for order of 10^4 years, and the Cs-137 measurements represent erosion rates for the past ~75 years.

P9, L1: I'll admit that I'm not entirely sure why equation 2 is introduced. In this line you say you're going to derive equation 2 from the data, but you don't really ever come back to this equation with the results. I think the equation that shows up in figure 3 could be slightly altered to fit this form. It would be interesting to see something in your discussion that comes back to this equation, and the values of W and gamma that you derive, rather than seem to just assign (next note).

P9, L5: How was gamma calculated? Did it come from your data? Please elaborate. And if it came from your results, then please put it there. It is important to make your assumption that soil thickness does not impact soil production rates as sound as possible. And ultimately, you have to have that assumption to use equation 1.

P9, L15: I found the section on soil rates across the catena to be very well written and explained.

P10, Figure2: Something seems off between the graph and the data presented in Table 1. The summit of CW has a soil formation rate of 36 mm/ka in Table 1, but this appears to plot as just 30 mm/ka in Figure 2.

P13, L19: You're correct that the 10Be concentrations you measured would not be impacted by a recent landuse change, but the thickness of the soil could be changed, and this would throw off the production rate at the sample depth. As a simple example, at RFF, suppose that in the last 150 years of agriculture at the RFF site 20 cm of soil had been removed (reasonable for the Cs-137 rate, I think). The proper depth to use for the production rate would be 20 cm more than the current depth because that was the depth to the top of the saprolite for the tens of thousands of years the soil has been developing.

That is a really interesting thing to pursue. I suppose there isn't much to go on to support or negate this, but it might be worth a little bit of "error analysis" to pursue this. You could calculate the amount of soil that has been lost at RFF since agriculture started there, and include that as the steady-state soil thickness and recalculate the

production rates at the sample depths. The production rates would be lower, and the resulting soil formation rates would be lower too. You could then say something about "if we're wrong about the soil depths today being representative of the long-term soil depth, then the results would change by X percent."

P13, L26: do you want to say "soil mantled" or just mantled?

P13, L32: It would be interesting to see the data you've compiled plotted with precipitation rate. I'm also not sure I understand the discussion in this paragraph. To me, it seems like your median rate matches the median rate for the temperate climate subset. And if 44% of the temperate-based data are from regions with lower mean annual precipitation rates, that sounds like your sites are really close to the median precipitation rate of the data set. So it seems like both your precipitation rate and soil formation rate are close to this subset's median rates too. When you say there is no significant difference between the two data sets, do you mean between your results and the temperate climate subset? If so, then you do you really need to take much time explaining why you think they are different?

P14, L5: Similar to the last comment, if the data aren't statistically different, do you need to explain why you think there are differences?

P14: There does not appear to be any discussion about the results from the samples collected 50 cm below the soil-saprolite interface. These results are interesting and should be discussed. In some cases, they show faster rates than the samples from the top of the saprolite, and in other cases they are slower. In theory, they should show the same rates if soil production has been constant for a long enough time. The fact that they are different indicates that soil production hasn't been constant on the timescales these measurements record. The differences may be explained by something that has happened within the last order of 10^5 years. This is because the muon attenuation lengths are much longer than that for spallation, and muons are produced at much lower rates than by spallation. The result is that muons average over much longer

timescales than spallation. Thus, when the rate in the sample 50 sample below the soil-saprolite boundary are lower than from the top of the, that may indicate that recently (order 10ˆ5 years) soil production rates increased. And vice-versa if the rate from the lower sample is higher than from the top of the saprolite. You might double-check my logic, but I think that's really cool and warrants a paragraph in this paper!

P15, Figure 4: I'm a bit confused by "depth" in this figure. Is it depth to the top of the saprolite? Or just depth below the surface? It may be helpful to know how most of the samples in this global compilation were collected. Were most from the soil-saprolite interface? Or a mix of that and below the interface like you've done?

P18: It may be appropriate to include something in your conclusion about how your results compare to the global data set you compiled.

Please also note the supplement to this comment:
https://www.soil-discuss.net/soil-2019-8/soil-2019-8-RC3-supplement.zip

Interactive comment on SOIL Discuss., https://doi.org/10.5194/soil-2019-8, 2019.