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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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Thank you for your comments on our manuscript. On behalf of my co-authors, I would like to respond to your suggestions as to how we could take this manuscript further.

Please refer to the supplementary file for the equations set out below.

1a) Referee #3; C2-5, item 1: 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 sur-

face 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/cmËĘ3) 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/cmËĘ3. In my calculations I used a value of 1.8 g/cmËĘ3 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/cmËĘ3. 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/cmËĘ3 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/cmËĘ3 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.

1b) Response to Referee #3; C2-5, item 1: We have developed a model as part of some sensitivity analysis, using multiple bulk density measurements down the soil profile. We have assumed that the density of the overburden (soil profile) has not changed with time. (The model itself is to be published soon, elsewhere). However, we have used the model to re-calculate soil formation rates for both RFF and CW and intend to incorporate these results in the paper. We have also used the model to assess the importance of sample thickness, by calculating soil formation rates for three different sampling depths: the top, middle and bottom of the sample.

1c) Change in manuscript after Referee #3; C2-5, item 1: We suggest that results from our new analyses are incorporated throughout the paper: in Table, figures, all written analyses, etc.

2a) Referee #3; C5, item 2: "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".

2b) Response to Referee #3; C5, item 2: The compilation is our work, although it is largely based off existing inventories, namely Portenga and Bierman (2011), Stockmann et al. (2014) and Montgomery (2007). These are cited on Page 3, line 27.

2c) Change in manuscript after Referee #3; C5, item 2: We argue that no change is necessary.

3a) Referee #3; C5, item 3: "P4, L31: do you mean "small" instead of "soft" when describing the grain size of the sandstone at the CW site?"

3b) Response to Referee #3; C5, item 3: The word 'soft' here is an error.

3c) Change in manuscript after Referee #3; C5, item 3: We suggest the deletion of the word "soft" from Page 4, line 31.

4a) Referee #3; C5, item 4: 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.

4b) Response to Referee #3; C5, item 4: Both are south-facing sites, although the effects of insolation are obviously dampened at CW due to the canopy cover.

4c) Change in manuscript after Referee #3; C5, item 4: We suggest the following addition on Page 5, line 1: "Both RFF and CW are south-facing slopes, and sit in a temperate. . ."

5a) Referee #3; C5, item 5: 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?

5b) Response to Referee #3; C5, item 5: We agree.

5c) Change in manuscript after Referee #3; C5, item 5: We suggest that the citation on Page 5, lines 19 and 23 are changed to: "(IUSS Working Group WRB, 2015)." We will also provide a full reference in the bibliography.

6a) Referee #3; C6, item 6: 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.

6b) Response to Referee #3; C6, item 6: We agree.

6c) Change in manuscript after Referee #3; C6, item 6: We suggest that "LFH layer" on Page 5, line 24 is changed to "Litter Fermentation Humus layer".

7a) Referee #3; C6, item 7: 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.

7b) Response to Referee #3; C6, item 7: We cite on Page 5, line 8 that "the prevalence of similar deposits on the study hillslope has not been studied." However, we accept that a soil with 84-94% sand suggests the absence of glacial outwash deposits; that the soils here are residual soils that have formed from sandstone rather than allochthonous sources.

7c) Change in manuscript after Referee #3; C6, item 7: We suggest the following addition to Page 5, line 25: "The sandy composition of these soils suggests that proglacial outwash deposits have not contributed to the soils of the study sites and that, instead, the soils are largely residual."

8a) Referee #3; C6, item 8: 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.

8b) Response to Referee #3; C6, item 8: Please refer to Page 5, line 15 where we state that "RFF has been under an arable regime and in the last twelve years, the dominant crops have been Winter Wheat and Rye." Unfortunately we are unable to provide precise details as to the tillage operations (plough depth, disc type, etc).

8c) Change in manuscript after Referee #3; C6, item 8: We argue that no change is necessary.

9a) Referee #3; C6, item 9: "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."

9b) Response to Referee #3; C6, item 9: We agree and have now incorporated a time element, as shown in 9c.

9c) Change in manuscript after Referee #3; C6, item 9: First, we suggest the following addition is made to Page 7, line 16: "…smaller concentrations (Lal, 1991; Stockmann et al., 2014). We assume here that the production of 10Be and the erosion of the bedrock is at an equilibrium:" Second, we also suggest that Equation 1 is updated to: N= $\sum$_($i = sp$,μ_f,μˆ-)âŰŠ(P_i (θ) · eˆ(-x/Λ_i))/(λ+Ïţ/Λ_i) (1-eˆ(-t(λ+Ïţ/Λ_i))) Third, we also suggest that a revision is made to Page 7, lines 18-23: "where: P are the annual production rates of 10Be by spallation, fast muons and stopping muons (sp, μf and μ-) at a surface with slope Ït'; x is the mass sample depth (âĂćz); ÑĂ is the density of overburden material; z is the depth of the sample; t is the age of the bedrock surface (the age when the original surface was generated) λ is the decay constant of 10Be with λ equalling In2/10Be half-life; and Λ are the mean attenuation of cosmic radiations (Lal, 1991). t is usually considered infinite. In this paper, we took two samples from some of the sites to test if the data support this assumptions. RFF data is compatible with landscape ages >221 ka. Production rates, decay constants and attenuation lengths were calculated using field data and the CRONUS-Earth online calculator v2.3 Matlab code for the St scheme (Balco, 2008). As N can be measured using Accelerator Mass Spectrometry (AMS), Eq. (1) can be solved for ε by simple interpolation of N."

10a) Referee #3; C6, item 10: You could also be more explicit that the saprolite erosion

rate directly translates into the soil formation rate.

10b) Response to Referee #3; C6, item 10: For the purposes of using cosmogenic radionuclide analysis for deriving soil formation, we agree it does. However, we must (and do) consider that there are other extraneous inputs that may up-build soil profiles, which are not necessarily taken into account in a bedrock weathering rate.

10c) Change in manuscript after Referee #3; C6, item 10: We suggest the following addition is made on Page 3, line 20: "…measured and assumed to equal the rates of soil formation."

11a) Referee #3; C6, item 11: 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?

11b) Response to Referee #3; C6, item 11: They were vertical. We shall add this detail.

11c) Change in manuscript after Referee #3; C6, item 11: We suggest the following addition is made to Page 7, line 4: "…then proceeded to extract a series of vertical undisturbed core samples…" We also suggest that a similar addition is made to Page 7, line 8: "…a soil pit was manually dug vertically at each of the four sampling locations."

12a) Referee #3; C6, item 12: 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 10ËĘ4 years, and the Cs-137 measurements represent erosion rates for the past 75 years.

12b) Response to Referee #3; C6, item 12: We agree.

12c) Change in manuscript after Referee #3; C6, item 12: We suggest the following addition is made to Page 8, line 30: "It should be acknowledged here that the rates of soil formation represent timescales four orders of magnitude greater than those of soil erosion. However, if lifespans are to provide an insight into the sustainability of the soil profiles at RFF, the soil erosion rates must represent those from contemporary arable agriculture."

13a) Referee #3; C7, item 13: 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).

13b) Response to Referee #3; C7, item 13: Equation 2 is introduced to test whether the rates of soil formation are sensitive to changes in soil depth, and therefore, whether a constant formation rate should be used in the denominator of Equation 3 (Page 9, line 11) or whether the formation rate should change with decreasing soil depth. It is merely a preliminary test to best formulate Equation 3. To ensure maximum transparency and accessibility, we would argue that combining this into the lifespan equation is not the best course of action.

13c) Change in manuscript after Referee #3; C7, item 13: We argue that no change is necessary.

14a) Referee #3; C7, item 14: 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.

14b) Response to Referee #3; C7, item 14: We cite in the paper that gamma is a parameter that determines the thickness of soil when soil formation falls off by 1/e. Here, e is the exponent of the best fit exponential trend line that runs through our soil

formation rate data.

14c) Change in manuscript after Referee #3; C7, item 14: We suggest the following addition to Page 9, line 5: "The data for both the production rate (P) and the thickness of the soil (h) was used to calculate W and gamma using least squares regression."

15a) Referee #3; C7, item 15: 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.

15b) Response to Referee #3; C7, item 15: This is something we need to address. We have also noticed the inconsistency between Table 1 and Figure 2, and will make the necessary changes.

15c) Change in manuscript after Referee #3; C7, item 15: We shall prepare a revised figure and ensure that the data match Table 1.

16a) Referee #3; C7-8, item 16: 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."

16b) Response to Referee #3; C7-8, item 16: As part of the development of a new model to address the earlier comment on bulk density, we have also re-calculated soil formation rates assuming non-steady soil thicknesses, making use of Cs-137 derived soil erosion rates.

16c) Change in manuscript after Referee #3; C7-8, item 16: We suggest that results from our new analyses are incorporated throughout the paper: in Table, figures, all written analyses, etc.

17a) Referee #3; C8, item 17: P13, L26: do you want to say "soil mantled" or just mantled?

17b) Response to Referee #3; C8, item 17: Yes, soil mantled.

17c) Change in manuscript after Referee #3; C8, item 17: We suggest the following revision is made to Page 13, line 26: ". . .that of the soil mantled inventory. . ."

18a) Referee #3; C8, item 18: 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?

18b) Response to Referee #3; C8, item 18: We will add clarity to this section.

18c) Change in manuscript after Referee #3; C8, item 18: We suggest the following addition to Page 13, line 30: ". . .although there is no statistically significant difference

between those data and those we have measured for our UK study sites."

19a) Referee #3; C8, item 19: 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?

19b) Response to Referee #3; C8, item 19: We argue that it is important to place our UK data into context. With regards to this particular example, whilst no statistical difference was found, lithological variation may still influence soil formation rates. It may be that Type 2 errors are present here.

19c) Change in manuscript after Referee #3; C8, item 19: We argue no changes are necessary.

20a) Referee #3; C8-9, item 20: 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 10ËĘ5 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 10ËĘ5 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!

20b) Response to Referee #3; C8-9, item 20: Yes, that would be a very interesting output from the paired samples. However, our measurements are not precise enough to solve a model with an accelerated or decelerated soil production. Actually, the calculated erosion rates from the paired samples agree within one sigma, meaning that we would not be able to prove soil formation acceleration/deceleration from these data. Also, slight changes of other factors (e.g. the actual position of the surface before farming, density uncertainties, etc.) can also affect these apparent offsets, and we have no data to rule them out.

20c) Change in manuscript after Referee #3; C8, item 20: We argue that no change is necessary.

21a) Referee #3; C9, item 21: 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?

21b) Response to Referee #3; C9, item 21: Invariably, it is sampling depth. Many papers do not specify whether this is a depth to saprolite or to bedrock, so the best practice here is to put 'sampling depth'.

21c) Change in manuscript after Referee #3; C9, item 21: We suggest that the x axis labels are changed on all four panes to: "Sampling depth (cm)". We also suggest a revision in the caption: ". . .plotted against sampling depth."

22a) Referee #3; C9, item 22: P18: It may be appropriate to include something in your conclusion about how your results compare to the global data set you compiled.

22b) Response to Referee #3; C9, item 22: We agree.

22c) Change in manuscript after Referee #3; C9, item 22: We suggest that the following addition is made to Page 18, line 22: "Soil formation rates were found to fall within the range of those previously published for soils in temperate climates and on sandstone lithologies, but were found to be significantly greater than those measured

previously at Bodmin Moor. This is explained by the fact that the parent material at Bodmin Moor is a coarse-grained granite and therefore less susceptible to weathering than the sandstone materials underlying Rufford Forest Farm and Comer Wood."

Please also note the supplement to this comment:
https://www.soil-discuss.net/soil-2019-8/soil-2019-8-AC3-supplement.pdf

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