Final author comments

Response to Referee #2 (received and published 17 June 2019).

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.

1a) Referee #2; C2, item 1: "P5 line 19 Include the correct reference to WRB (2015). Please see recommended citation in the preface of the manual."

1b) Response to Referee #2; C2, item 1: We agree.

1c) Change in manuscript after Referee #2; C2, item 1: 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.

2a) Referee #2; C2, item 2: "P5 L23 CW soil has 94% sand. This would classify the soil at this site in WRB as an Arenosol not a Cambisol."

2b) Response to Referee #2; C2, item 2: We agree.

2c) Change in manuscript after Referee #2; C2, item 2: We suggest that "Cambisol" is changed to "Arenosol" on Page 5, line 23.

3a) Referee #2; C2, item 3: "P5 L19-20 Please refer to the methods used for the determination of the particle size distribution and LOI".

3b) Response to Referee #2; C2, item 3: We agree.

3c) Change in manuscript after Referee #2; C2, item 3: We suggest the following addition is made on Page 8, line 9: "Soil samples were sub-sampled every 5 cm from each core at RFF and on each profile wall at CW. All samples were then oven dried overnight (105°C for 12 hours), grounded with a pestle and mortar, and sieved to discard the >2 mm fraction before being subject to particle size analysis and loss on ignition (LOI). Particle size analysis was conducted using a Beckman Coulter Laser Diffraction Particle Sizing Analyser LS 13 320 (pump speed: 70 %; sonication: 10 seconds; run-length: 30 seconds). For LOI, 5 g of each sample was placed in a Carbolite furnace CWF 1300 (550°C for 12 hours)." We then suggest that the text on Page 5, lines 19-26 ("The soils at RFF […] saprolitic sandstone") are cut and are placed here on page 8, so that the description of the soil profiles follows the methods.

4a) Referee #2; C2, item 4: P7 L 5 "...observation on the competency of the extracted material…" is a bit vague - how was the Saprolite or the soil/saprolite boundary determined exactly? A change in colour, consolidation, grain size? Please provide some further details".

4b) Response to Referee #2; C2, item 4: The methods we applied both in the field and in the laboratory were observations on the consolidation of the material supported by the penetrometer data.

4c) Change in manuscript after Referee #2; C2, item 4: We suggest a revision on Page 7, line 4: "…were later halved lengthways, and by observing the changes in the consolidation and physical integrity of the extracted material (i.e. whether it remained intact when removed from the core), together with the penetration resistance data acquired in the field, the soil-saprolite interface was demarcated.” We also suggest a revision on Page 7, line 9: “Observing the changes in the consolidation and physical integrity of the material down the profile wall, together with the penetration resistance data, the soil-saprolite interface was ascertained.”

5a) Referee #2; C2, item 5: “P7 L6 You sample at the soil-saprolite interface and 50cm below it in RFF. Please indicate the rationale for these paired samples. These samples are not differentiated in the results – so are they both used to be representative of this boundary and what are the implications for this? In table 1 the lower samples in some locations are showing active weathering indicated by greater soil formation rates”.

5b) Response to Referee #2; C2, item 5: They are not both representative of the boundary. The first sample (labelled A in Table 1) represents the soil-saprolite interface. Since reflecting on these responses, we have made revisions to Equation 1:

$$N = \sum_{i=sp,\mu_f,\mu^-} \frac{P_i(\theta)\ \cdot\ e^{-\frac{x}{\Lambda_i}}}{\lambda + \frac{\epsilon\rho}{\Lambda_i}} (1 - e^{-t\left(\lambda + \frac{\epsilon\rho}{\Lambda_i}\right)})$$

P are the annual production rates of ^{10}Be by spallation, fast muons and stopping muons (sp, μ_f and μ^-) at a surface with slope *Ɵ*; *x* is the mass sample depth (*ρ·z*); *p* is the density of overburden material; *z* is the depth of the sample; t is the age of the landscape (the age when the original surface was generated) λ is the decay constant of ^{10}Be with λ equalling In2/^{10}Be half-life; and Λ are the mean attenuation of cosmic radiations (Lal, 1991).

t is usually considered infinite. In this paper, we tested the best fit of *t* based on the data from RFF. To do this, we took two samples from the same depth profile and measured the concentration of ^{10}Be for both. At RFF, this showed that the landscape age (the time when the cosmogenic clock was reset) was >200 ka.

5c) Change in manuscript after Referee #2; C2, item 5: We suggest that Equation 1 is updated to:

$$N = \sum_{i=sp,\mu_f,\mu^-} \frac{P_i(\theta)\ \cdot\ e^{-\frac{x}{\Lambda_i}}}{\lambda + \frac{\epsilon\rho}{\Lambda_i}} \left(1 - e^{-t\left(\lambda + \frac{\epsilon\rho}{\Lambda_i}\right)}\right)$$

We also suggest that a revision is made to Page 7, lines 18-23: “where: *P* are the annual production rates of ^{10}Be by spallation, fast muons and stopping muons (sp, μ_f and μ^-) at a surface with slope *Ɵ*; *x* is the mass sample depth (*ρ·z*); *p* 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 ^{10}Be with λ equalling In2/^{10}Be 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*."

6a) Referee #2; C2, item 6: "P8 L23 An additional statement needed here to indicate the exclusion of other potential soil forming inputs (e.g. organic matter and/or aeolian dust)."

6b) Response to Referee #2; C2, item 6: We agree.

6c) Change in manuscript after Referee #2; C2, item 6: We suggest the following addition is made to Page 8, line 24: "…sufficiently low, nor did we account for any allochthonous inputs to the profile such as aeolian additions and organic amendments."

7a) Referee #2; C2, item 7: "P8 line 25 depth to bedrock or depth to soil/saprolite boundary? Did you only use the samples labelled A from RFF to indicate depth to saprolite? Please confirm in the text".

7b) Response to Referee #2; C2, item 7: Yes, we used the depth to the soil-saprolite interface.

7c) Change in manuscript after Referee #2; C2, item 7: We suggest the following revision is made to Page 8, line 25: "the observed depth to the soil-saprolite interface at each catena position was employed".

8a) Referee #2; C2, item 8: "P13 6 Did you undertake any geochemical analysis on the samples (XRF or spectroscopy?) I guess you would have reported it but it would have been really good to see some data (perhaps in another paper)."

8b) Response to Referee #2; C2, item 8: We did not undertake any further analyses on the samples in this study. However, we are considering further isotopic work that may further our understanding of soil formation rates, erosion and particularly colluviation. Nevertheless, it would be beyond the primary aim of this paper to report such analysis here.

8c) Change in manuscript after Referee #2; C2, item 8: We argue that no change is necessary.

9a) Referee #2; C2, item 9: "P13 line 27, 31; P14 L5 and 14. Check the notation for p values for the Mann-Whitney tests. For significant difference $p < 0.05$; for no significant difference $p > 0.05$."

9b) Response to Referee #2; C2, item 9: We agree.

9c) Change in manuscript after Referee #2; C2, item 9: All reported *p* values should be revised (i.e: where $p < 0.05$ appears, these are replaced by $p > 0.05$, and vice versa).

10a) Referee #2; C3, item 10: "P14 L3 Can you clarify if the sandstone dataset is from the temperate subset or from the whole global database? If the latter, then there is an interaction between climate and differences in sandstone lithology."

10b) Response to Referee #2; C3, item 10: The sandstone dataset was derived from the whole global, soil-mantled database. Coincidentally, all but seven data points for the sandstone dataset stem from temperate climates (as classified by the Koppen system). The remaining seven stem from A_w (tropical/savannah) and we believe that these should be removed from the figure so that we limit the climate signal as much as possible. (Incidentally, your point can also be made for the temperate climate dataset, reported on Page 13, line 29 onwards. The temperate climate dataset comprises rates for no less than six different parent materials).

10c) Change in manuscript after Referee #2; C3, item 10: We suggest that the seven data points not from temperate climates are removed from Figure 4c. We then suggest the addition of the following on Page 14, line 5: "Although the sandstone-derived data were derived from the global soil-mantled database, all data stem from sites in temperate climates which reduces the influence that climate may have otherwise had in this analysis on lithology." We also suggest the revision of Page 14, line 3: "(n = 57)"

11a) Referee #2; C3, item 11: "P16 L5. The toeslope also shows an Ap of 75 cm (p5 L20) which has also not been taken into account in the calculation due to the assumption that the top 30cm is representative of the current (active?) A horizon. If the top 30cm is removed then it could be argued there is still 'viable' topsoil at this location and thus the lifespan would be much greater than calculated (in addition to it also receiving colluvium)."

11b) Response to Referee #2; C3, item 11: We acknowledge that already on Page 16, line 9-10: "…lifespans at this position may be either longer than 2158 years or indefinite." We should point out that the Ap horizon of 75 cm is most likely, in part, colluvium. But we will make this clearer also on Page 8, too. Finally, we will run the lifespan model again for the toeslope and report an additional lifespan for this position, taking into account the 75 cm depth.

11c) Change in manuscript after Referee #2; C3, item 11: First, we suggest the following addition on Page 8, line 22: "…D = 30 cm across the catena. At the toeslope, an additional lifespan was calculated to account for the greater depth (75 cm) of the A horizon." Second, we suggest the following addition on Page 16, line 5: "…rather than thinning. This is supported by the fact that the depth of the Ap horizon at the toeslope is 75 cm, whereas it is 30 cm on all other observed landscape positions. Moreover, comprised within the upper stratigraphy of the soil profile down the catena is the Bunter Pebble Bed which can be found at approximately 30 cm on summit, shoulder and backslope positions but 70 cm at the toeslope. The depth to which this pebble bed occurs at the toeslope suggests that either colluviation has occurred or is still occurring. In a scenario where colluviation is no longer active, the lifespan of this 75 cm A horizon is finite and ranges from 345 – 4808 years, but lifespans here could be longer or indefinite is colluviation continues."

12a) Referee #2; C3, item 12: "P16 L6 Could the pebble bed offer some surface armoury that would reduce the rate of soil erosion once material above it has been eroded?"

12b) Response to Referee #2; C3, item 12: An interesting idea. We discuss the potential differences in erodibility with soil removal on Page 18, line 3, but we only considered erodibility to increase. We shall acknowledge the pebble bed armoury.

12c) Change in manuscript after Referee #2; C3, item 12: We suggest the following addition on Page 18, line 3: "…neither reflects the increase in the erodibility of subsoil horizons, characterised by a relatively weaker soil structure (Tanner et al., 2018) nor the potential role that the Bunter Pebble Bed may play in armouring the soil surface in the future. Moreover, they do not reflect the expected shift in erosivity…"

13a) Referee #2; C3, item 13: "P16 line 15 this is the sampling depth, which is the soil-saprolite boundary, not depth to bedrock (be consistent with the descriptions you have used in other parts of the manuscript).

13b) Response to Referee #2; C3, item 13: We agree.

13c) Change in manuscript after Referee #2; C3, item 13: We suggest a revision to text on Page 16, line 15: "the soil thickness applied here is the depth to the soil-saprolite interface measured…" We also suggest a change to Figure 5 caption; "…(light brown) and the depth to the soil-saprolite interface (bricks)."

14a) Referee #2; C3, item 14: P18 L8. Is the last sentence incomplete?

14b) Response to Referee #2; C3, item 14: We agree; incomplete but also superfluous.

14c) Change in manuscript after Referee #2; C3, item 14: We suggest the deletion of the final sentence on Page 18, line 8.

15a) Referee #2; C3, item 15: Figure 2 Please indicate what the error bars show. Also include the sample numbers on the figure or in the caption.

15b) Response to Referee #2; C3, item 15: We agree.

15c) Change in manuscript after Referee #2; C3, item 15: We suggest that the caption for Figure 2 includes the words: "The error bars represent one standard deviation". Further, we suggest a revision to existing text: "Rufford Forest Farm (blue; n = 4) and Comer Woodland (green; n = 4)."

16a) Referee #2; C3, item 16: Figure 3 If I have interpreted the sampling correctly then 4 of these samples are from 50cm below the soil-saprolite boundary. Does this figure therefore show sampling depth rather than depth to saprolite (for RFF there would be 4 pairs of samples with the same saprolite-soil boundary depth, one sample at the boundary and one 50 cm below).

16b) Response to Referee #2; C3, item 16: Yes, that is correct and requires an axis label change.

16c) Change in manuscript after Referee #2; C3, item 16: We suggest that the x axis label is revised to: "Sampling Depth (cm)"

17a) Referee #2; C3, item 17: Table 1 You state the average sample density. If you have measured the BD for each sample then what is the justification for using the average for all samples rather than the specific sample bulk density in the Be10 calculations?

17b) Response to Referee #2; C3, item 17: We have developed a model as part of some sensitivity analysis to be published soon. We have run the model for all sites and have incorporated the new results throughout the paper.

17c) Change in manuscript after Referee #2; C3, item 17: We suggest that results from our new analyses are incorporated throughout the paper: in Table, figures, all written analyses, etc.