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Earth Surface Dynamics Discussions

Interactive comment on "The CAIRN method: Automated, reproducible calculation of catchment-averaged denudation rates from cosmogenic radionuclide concentrations" *by* Simon Marius Mudd et al.

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We thank reviewer 1 for their comments, which have helped us improve the paper. Our responses are in *blue italics*.

Reviewer 1

1. Assumptions of the method: To provide accurate denudation rates from cosmogenic nuclides, several assumptions must be strictly met. For instance, sediments should be well mixed, there should be a steady state between nuclide production and removal by

erosion and decay, a uniform quartz yield etc. These assumptions should be briefly mentioned in the introduction of the ms along with a few references where readers can find more details (e.g. Dunai, 2010; already cited). As in many natural situations the required assumptions are partly violated, the general accuracy of denudation rates is probably limited to $\pm$20 to $\pm$30 $\%$ (Dunai, 2010, p. 124) and this should be mentioned. The general error of 10-20$\%$ (mentioned on line 429) is too optimistic in my opinion.
The second reviewer also raises this point. We agree that the computation invokes a number of assumptions (mentioned above) that are almost certainly not met and we do not wish to deceive users of the code that the results are more accurate than they in fact are. On the other hand, it is difficult to quantify the uncertainties generated by these assumptions. We thus have added a new section to make it clear that the general error is a minimum error that captures only known errors in AMS uncertainty, production rate uncertainty, etc. This new section follows the advice of reviewer 2.

2. Density: To calculate denudation rates in units of length per time one must make an assumption on density (as already pointed out by the Associate Editor). I agree with the editor that the authors should elaborate on this issue in the discussion. From discussions I had in the past, I got the impression that it is not very clear what values are appropriate, because soil thickness varies within catchments and around the globe. Maybe the authors can suggest some recommendations.
We certainly agree with this comment and there is no good way to arrive at a density estimate of soils if you do not have measurements. However, in the discussion we have used density values for rock, which is less spatially variable than soil density, and thus the erosion rates reported in length per time are rock equivalent erosion rates (surface erosion rates will be significantly higher since soil is less dense). We now state this explicitly in the manuscript. In the summary of calculations section we now state explicitly that CAIRN reports denudation rates in g cm^{-2} yr^{-1} because this requires no assumption of density, and then in the discussion we add text with notes on what density means. We have seperated this section on density in the discussion. Some figures still report denudation rates in length per time in order to remain consistent with

rates reported in other papers.

3. Nuclide production by muons: The production of muons is mentioned in several places of the ms (e.g. lines 77ff, 298, 507, Fig. 9). As it is known since many years that the model of Heisinger overestimates muon production (shown by the depth-profiles published by Braucher et al. and other studies, which are already cited in the ms), I suggest to make this point clear from the beginning of the ms and not only at the end (507ff). Hence, on line 79 the authors could rephrase the respective sentence to "... field-based estimates of muon production demonstrate that Heisinger et al..." or something similar. Likewise, lines 297-300 should be rephrased to provide a clear picture of that issue.
We have stated more clearly on the former lines 77ff and 297ff that field studies show the Heisinger et al. model overestimates muon production, but have not made major changes based on the comments of reviewer 2.

Balco et al. (2008) provide a nice plot (their Fig. 8), which highlights that the importance of muons relative to neutrons depends on elevation and the rate of erosion. Muons are particularly important at low elevation and at high erosion rates. I think it would be worth stating this more clearly somewhere in the ms.
We now state this.

4. Landsliding: Landsliding introduces considerable complications for interpreting cosmogenic nuclide concentrations in terms of denudation rates (see also comment made by Associate Editor). It seems to me that the approach which the authors propose in order to deal with the issue is too simplistic. In fact, they state on line 324 that their "landsliding module is admittedly rudimentary". Hence, I suggest to omit the respective parts from the ms (i.e. lines 149-158 and 251-254). It is sufficient to mention the landsliding issue and cite a few relevant studies (as the authors have done). I do not think that this will weaken the paper in any way.
We have followed the advice of reviewer 2 and now have a section devoted to transient scenarios. We removed text about landsliding from the former line 251 and 324.

5. Snow shielding: It is not yet widely acknowledged that water or snow have a significantly shorter neutron attenuation length than rocks (the value for the latter is $\pm$160 g/cm^2). Therefore, I appreciate that the authors cite the work of Delunel et al. (2014) and Zweck et al (2013). In addition, I suggest to mention the value of 109 g/cm^2 for the neutron attenuation length of snow explicitely (cf. Zweck et al. 2013). Fig. 7 of Delunel et al. (2014) shows that the attenuation by snow may be even more significant, which could also be mentioned. In my opinion, it would strengthen the numerical code, if a lower attenuation length of snow (as compared to rock) would be implemented into CAIRN (and the user can thus make a choice). Remote sensing will most likely be increasingly used to map snow depth in mountains (e.g. Beniston et al. 2003 and references therein). Could the authors check the literature and cite 1-2 recent papers on this subject in lines 330ff (I am quite sure there are more recent studies than Beniston et al.).
We have now cited the Beniston paper and mention that Zweck has a reduced attenuation thickness in the section "Spatial averaging for the CRONUS calculators". There are papers more recent than Beniston but these mainly use GCMs to model changing snow conditions and we feel scientists who want to calculate denudation rates are unlikely to use such models to reconstruct snow thicknesses. We also refer to the comment of reviewer 2: the uncertainties in the snow thickness through time vastly outweighs the uncertainties in attenuation lengths so we do not feel that changing the code in regard to snow calculations will improve accuracy of the model. We do however add text stating that users can replicate the changing attenuation thickness suggested by Zweck by modifying the snow raster fed to CAIRN.

6. Hardware/software requirements and standardization: Are there any hardware or software requirements for running the code? (as the Associate Editor downloaded the code and it appeared to work, I did not check it myself). If yes, this should be described somewhere (maybe near line 59). When using the CronusEarth Online calculator, one has to chose an AMS standard. The authors should mention whether CAIRN has the same option.

Indeed CAIRN forces users to choose the AMS standard, we have now stated this in the section on "Summary of CAIRN parameters for denudation calculations". We have also included a short note on software/hardware requirements on the former line 59.

7. Change of production rate by seemingly $\pm 20\%$ (line 453ff): The 20% change in production rate mentioned here gives a somewhat negative impression. In fact, about half of this change is related to a new standardization, which was required after the study of Nishiizumi et al. (2007). In other words, a 10Be exposure age (or erosion rate) calculated with the OLD standardization and a production rate of ± 5.0 at/g/yr yields basically the same age (or erosion rate) as a 10Be age calculated with the NEW standardization and a production rate of ± 4.5 at/g/yr. Details can, for instance, be found on the website of PRIME Lab (Purdue University) at: http://www.physics.purdue.edu/primelab/News/news0907.php. This issue should be clarified.
Good point and also raised by reviewer 2. We have followed that reviewer's advice and simply reported that production rates have changed along with AMS standards over the last decade, and we have also included a figure that shows the differences between CAIRN and CRONUS2.2 are mainly due to different parameter values rather than factors related to the underlying mathematics.

8. In section 2.2, it may be useful to provide the simple equation 11 of Lal (1991) in the form: Denudation rate = (Prod. rate / Conc. – lambda) (attenuation length / density)
We have added a modified version of this equation at the end of this section and have explained why it is inadequate for a catchment averaged calculation.

Line 63: The term "solution of CRN" in the title is a bit strange. One could rephrase e.g. as "Deriving/quantifying denudation rates at a single location" or something similar.
We have renamed this section "Quantifying denudation rates at a single location."

Line 71: maybe insert "local" or "site-specific" before production rate.
Added the word "local".

Line 113: I see that d is shielding depth, but what exactly is d0? (maybe I overlooked it).
It is the initial shielding depth, now stated in the manuscript.

Line 274: Typo, propAgation.
Fixed.

Line 294: Here a production ratio of 6.1 is mentioned. What does the factor of 1.106 mean? The 10Be and 26Al production rates given in Table 3 imply a production ratio of 7.2. Can the authors explain the reasons for this discrepancy?
Thanks for highlighting this: we did not do a very good job of explaining what we were doing in the previous version of the manuscript. We have corrected this passage to reflect what is actually happening in the code (basically we have a fixed production uncertainty of 8.7% which is taken from CRONUS2.2, but which we apply to our updated production rates.

Line 413: Typo; to infer a(N) denudation rate.
Fixed.

Line 434: replace "geomagnetic" by "time-dependent".
Done.

Line 440: please refer to one of the studies by Riebe et al. (2001, 2003). Riebe, C.S. Kirchner, J.W., Finkel, R.C. (2003). Long-term rates of chemical weathering and physical erosion from cosmogenic nuclides and geochemical mass balance. Geochimica et Cosmochimica Acta, 67, 4411-4427. Riebe, C.S. Kirchner, J.W., Granger, D.E. (2001). Quantifying quartz enrichment and its consequences for cosmogenic measurements of erosion rates from alluvial sediment and regolith. Geomorphology, 40, 15-19.
We have included the 2001 Riebe et al paper.

Line 503: Typo; "denudation" is spelled wrongly.
Fixed.

Line 530: I guess “differences“ would be more appropriate than the term “errors“, which is used twice in this line.
Changed the wording from error to difference here and elsewhere.

FIGURES a) At the beginning of the captions for Figs. 2, 3, 7, 8, 11, 12 the term “Errors“ is used. I believe that the word “Differences“ would be more appropriate.
We agree this is a better word and have made these changes.

b) The colors of the symbols used in Figs 2, 3, 4 etc for the individual studies are inconsistent (i.e. different colors are used in different plots for one and the same study). Shouldn‘t the color coding be consistent?
Done.

c) I agree with the Associate Editor that some of the figures could be combined.
We have combined the former Figures 2 and 3, and the former Figures 7 and 8.

d) Fig. 5 needs to be increased in size.
Done.

TABLES As the first part of the ms is mainly focussed on the description of the equations parameters etc and the data from selected studies are only discussed later, I suggest to reverse the order of Table 2 and Tables 3, 4.
We have changed the order so the former Table 2 (the default parameters) appears before the data from selected study sites. Table 4 remains in the same place since we felt it only makes sens in the context of comparison of the different calculators, which sets the ordering of the tables.

Table 3: Should not the Braucher et al. 2011 EPSL paper be mentioned here (instead of Braucher et al. (2009)? It is the 2011 paper, which gives the SLHL muon prod. rates in Table 6.
Fixed.

Table 3 and 4: I suggest to also provide the absolute muon production rates at SLHL

(not only the F values).
We have not made this change since it is simply one line multiplied by another and therefore redundant.

Algorithm 1: should the $>$ not be reversed to $<$?
We are glad you caught that! Fixed.

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