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

Anonymous Referee #1

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The manuscript (ms) by Mudd et al. is a timely and well written contribution aimed at providing the Earth surface community with an open-source tool to calculate consistent and reproducible basin-averaged denudation rates from the cosmogenic nuclide concentrations of stream sediment samples. The ms is well suited for this journal. I anticipate that it will help to foster the determination of reproducible catchment-wide denudation rates in the future, which would be a significant step forward. I have a list of minor comments (see below), which the authors should address.

1. Assumptions of the method: To provide accurate denudation rates from cosmogenic

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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.

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.

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.

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.

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.

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 $\sim$160 g/cm2). 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/cm2 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.).

Beniston et al. (2003). Snow pack in the Swiss Alps under changing climatic conditions: an empirical approach for climate impacts studies Theor. Appl. Climatol. 74, 19–31 (2003) DOI 10.1007/s00704-002-0709-1

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.

7. Change of production rate by seemingly ∼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.

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)

COMMENTS tied to line numbers 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. Line 71: maybe insert "local" or "site-specific" before production rate. Line 113: I see that d is shielding depth, but what exactly is d0? (maybe I overlooked it). Line 274: Typo, propAgation. 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? Line 413: Typo; to infer a(N) denudation rate. Line 434: replace "geomagnetic" by "time-dependent". 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.

Line 503: Typo; "denudation" is spelled wrongly. Line 530: I guess "differences" would be more appropriate than the term "errors", which is used twice in this line.

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.

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?

c) I agree with the Associate Editor that some of the figures could be combined.

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

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.

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.

Table 3 and 4: I suggest to also provide the absolute muon production rates at SLHL (not only the F values).

Algorithm 1: should the ">" not be reversed to "<" ?

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