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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.

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We thank Dr Balco for his detailed review, which has helped us significantly improve the paper. Dr Balco begins his review with a number of contextual remarks that have helped us refine the paper. We won't reiterate all of these remarks here but instead will try to include responses to the components most obviously requiring modifications of the manuscript. Dr Balco's review was clearly carefully crafted, adds to the discourse surrounding computation of denudation rates based on cosmogenic nuclides, and provides evidence for why an open review process can be beneficial to the scientific process.

Pages C1 and C2 of the review provide contextual comments which we do not believe require a response, but we have adopted some of the language here in our correction of the former line 35 because it is a better statement of the problem than we had in the original manuscript.

Page C3 makes allusions to western cinema and the Star Wars franchise, which we very much appreciate. We are also glad the reviewer thinks our contribution is, in his words, not lame. The bottom of this page and page C4 goes on to discuss the dubious nature of assumptions required to calculate denudation rates, mirroring quite closely the comments of reviewer 1, in their first item. Please see our response to this reviewer about the various assumptions about the natural system we try to model and why they are imperfect. Specifically, we now have a section on how temporal and spatial variations in denudation rates add significantly to the uncertainty of the method, noting that this is not specific to our method, but rather endemic to all estimates of catchment averaged erosion.

Below are responses to itemized queries by the reviewer. Our responses are in *blue text.*

Abstract, first sentence. The first sentence can be removed – it would be more useful for the abstract to begin with the second sentence, which describes what the authors actually did in the paper.
Sentence deleted.

Line 31. The word 'cosmogenic' means originating from cosmic rays. The calculators don't originate from cosmic rays, so 'cosmogenic calculator' makes no sense. Suggest just 'calculator' here.
Done.

Line 35-ish. This is a bit oversimplified. In fact, there is not a lot of variation in ap-

proaches to computing the erosion rate (really there are only two: include muons, or don't). What there is a lot of variation in is how to compute P. Also, the phrase "representative parameters for the catchment" is unnecessarily complicated and also unhelpfully nonspecific. Specifically, what you are trying to estimate is just P. Be more specific here.
We have changed the text here to mirror what the reviewer has written on page C2 of his review, which succinctly states the process involved in calculating denudation rates.

Line 40-ish. Again, this is somewhat misleading as written. The mathematical definition of the catchment-averaged erosion rate (e.g., from Bierman and Steig, etc.) specifically has P in it, and there is no doubt about what this parameter is. Thus, it is incorrect to say that authors are trying to "choose a production rate that is 'representative' of the catchment." Instead what they are doing is trying to estimate the mean production rate in the catchment, either by computing that in some pixel-based way or by fudging the input to the online calculators to force them to compute the mean production rate internally. In any case, it would be helpful to clarify this a bit.
We have rewritten this section, it now says: "Production rates vary spatially, thus users of online calculators must calculate the effective production rate within a catchment using a weighted mean of the production in individual pixels. The manner in which these are fed to existing calculators vary, for example one must feed a single weighted mean production, after shielding corrections to COSMOCALC. In contrast, one must calculate weighted mean shielding corrections and pass them to CRONUS-2.2, and in addition must calculate a pressure or elevation that reproduces the mean production rate before shielding."

Line 53. I was confused by this discussion of the landslide scenario, because this scenario violates the assumptions inherent in computing the catchment averaged erosion rate from a Be-10 concentration. Thus, if you are going to compute the erosion rate using the method in Bierman, Steig, etc., you have already assumed that erosion is steady over time (although not spatially uniform), which is equivalent to stating that

landslides do not occur. Thus, landsliding doesn't cause a problem with computing P (as is implied by its inclusion in this section), it basically invalidates the entire method. To clear this up, I suggest dividing this section into two subsections: (i) issues that make it difficult to compute P (e.g., shielding, snow cover, etc.); and (ii) issues that invalidate the entire method by violating its basic assumptions (landsliding, sediment storage, etc.). Alternatively (and probably better), I suggest just pending the entire discussion of landsliding to a separate section at the end of the paper, in which you can discuss more generally the point that you could potentially use this code for all sorts of nonequilibrium scenarios.
We have chosen the latter option suggested here. We now append a separate section on transience and remove mention of landsliding from other parts of the manuscript.

Line 60. Don't you want to continue here to mention that in addition to computing production rates, the software also does the implicit solution for erosion rate given a measured nuclide concentration? Because as written this paragraph doesn't indicate that any erosion rate estimate happens. Needs improvement. In general, also, it would be helpful for the reader at this stage if you were to explain the overall procedure of inverting a forward model for nuclide concentrations to obtain an estimate of the erosion rate, as a preview of coming attractions.
Yes, now that you mention it, we do want to say that. The text now reads "Based on these calculations the software can then calculate the expected cosmogenic nuclide concentration from a basin given a spatially homogenous denudation rate. Finally, the software uses Newton iteration to calculate the denudation rate that best reproduces the measured cosmogenic nuclide concentration."

Line 80-ish. This brings up the subject of muons. In this work as in others, muons are responsible for 2% of surface production and 98% of suffering. The decision in this paper to use an exponential scheme for muon production for computational simplicity is, in fact, sensible. Unfortunately I found the explanation here to be incomplete and confusing.

We have attempted to follow the reviewer's comments within our manuscript, since they contain succinct statements of why the muon approximation may erroneous and also why errors in muogenic production do not play a significant role in overall uncertainty. See specific changes below.

Basically, the difference between the Heisinger integration scheme and a simple exponential approximation à la Braucher is that the latter is incorrectly representing the physics. What is really happening physically is that as depth increases, the mean energy of the remaining muons increases, so the instantaneous e-folding length for muon production continually increases with depth. You can't represent that with a finite sum of exponentials. If you have a bunch of summed exponentials, you can do pretty well at shallow depths, but there is some depth below which you are quite wrong. So that is what the actual difference is.

We now specifically state this: "The advantage of the Heisinger et al. (2002) scheme is that it tries to capture the physics of muon passage through the near surface, and specifically models how the mean energy of muons increases as one moves to greater depths in the subsurface. This affects muon production at depth in a way that is not captured by exponential approximations. Recent work by Marrero et al. (2016) has updated the scheme of (Heisinger et al., 2002) reflecting the muon production rates inferred from field studies. This method still has the disadvantage that it is computationally expensive, to the extent that this computational cost is prohibitive if one is to calculate muon production in numerous pixels across a catchment."

However, there are two reasons the exponential approximation is OK here. (The reviewer goes on to state why the approximation is okay).

We have added some text reflecting the reviewer's comments about why the approximation is okay, which hopefully will encourage skeptical readers to keep going: "Our approach is to approximate muon production using a sum of exponential functions. This approach has the advantage of being computationally efficient, but has the disadvantage of not reflecting the physics of muon production and therefore failing to capture *muon production well at depths beyond a few meters. This is unlikely to lead to large errors, however, because muon production makes up a very small percentage of the overall nuclide production at the depths where the physics-based models diverge from the exponential models. We specifically quantify this difference in Section 6.3, finding the exponential approximation to lead to differences between the physics-based approximation that are relatively small (for a wide range of denudation rates these differences are less than 2%)."*

A final point here is that there would potentially be lots of other ways to speed up the computation whilst still using the Heisinger scheme, if you wanted. Mainly this is because the production rate due to muons will not be very different between adjacent cells. Thus, it is a big waste of time computing muon production separately in each pixel. You can probably get away with doing the muon calculations in a very small minority of cells in a typical watershed and extending those results to the other cells simply by a regression formula in elevation, without loss of accuracy. Or do muon production on a much coarser grid than spallogenic production. Of course, this would be a big rewrite of the code, but if you really want it to run maximum fast it would be the next obvious strategy. If muons are 2% of surface production, why give them more than 2% of processor time?
These are all interesting suggestions for speeding up the Heisinger approximations, but our testing suggests the difference between the Heisinger method and our exponential method is around 2%, which is completely dwarfed by the rest of the uncertainties, so we don't think it would be particularly useful to spend the time optimizing this part of the code.

Line 105. Again, equations are not cosmogenic.
Removed "cosmogenic".

Line 120. Note again the implications of assuming that muon production is in steady state. Not likely to be true.
We now refer readers to our section on transience here.

Line 130, “self-shielding." I am not sure I understand what is going on here because under normal circumstances, the sediment leaving the catchment would be assumed to be from an infinitesimally small surface layer, so no integration in depth would be required. So this appears to me to be overly complex. My understanding of what is happening in this part of the paper is that the authors have just put in this capability to facilitate later use of the same code for a patchy-erosion model where finite thicknesses of sediment are removed at once (e.g., landslides). And then the discussion of steady state is confusing here as well, because, of course, if landslides are occurring then there is by definition not a steady state. Overall, more explanation needed here. As noted above, I think this would be clearer if all discussion related to the landsliding issue was deferred to a separate section.

We now state explicitly that for most applications an infinitesimal layer will be used ($d_t = 0$, but we have included it so that future users can devise clever ways to explore landsliding. We then state that landsliding is beyond the scope of this paper, but we acknowledge the uncertainties it introduces.

Line 160 and below. Topographic shielding. This is another example where the calculation is an precise representation of simplified physics, so gives illusory precision. Specifically, this code includes a quite precise calculation of topographic shielding under the assumptions that (i) the cosmic-ray flux is totally attenuated below the apparent horizon, and (ii) the zenith angle dependence of the cosmic-ray flux responsible for production is a cosine to a constant power. Neither (i) or (ii) is actually true. Because secondary particle production takes place throughout the atmosphere (including that part of the atmosphere that is between you and the apparent horizon on the other side of the valley), a nonzero amount of production will actually be due to cosmic rays originating below the visible horizon. In addition, the cosine-to-a-power dependence is highly approximate. See this paper: Argento, D.C., Stone, J.O., Reedy, R.C. and O'Brien, K., 2015. Physics-based modeling of cosmogenic nuclides part II–Key aspects of in-situ cosmogenic nuclide production. Quaternary Geochronology, 26, pp.44-55.

We now state explicitly the two assumptions that underpin our shielding model and cite *the Argento et al. paper noting that our method is an incomplete description of the physics in question.*

The point being that the very comprehensive analysis of discretization errors in the shielding calculation here clouds the fact that there exist larger systematic errors due to simplified physics. This issue has basically no practical relevance to the erosion rate calculation overall (because it is still much less important than violations of the basic method assumptions). However, the authors should note here that they are concerned with the precision of a representation of simplified physics, which may or may not be the same as the precision of the calculation relative to real life.
We now say this so there can be no doubt about what we have done. "Thus our model, while precise, contains a simplified version of the true physics of topographic shielding."

In addition, this could also be sped up a lot if you really wanted to... but although of historical interest, that is beside the point here.
Because this is open-source software, future authors can fork our code and make such improvements. Refactoring the code at this point, however, would take several months of effort not only rewriting this component but recalculating every measurement reported here, for minimal gain in accuracy. We don't think the reviewer is asking us to do this so we haven't.

A final important issue here is that it was not clear to me whether telescoping of the mean free path length on dipping surfaces (see Dunne, also Fig. 5 in Balco, 2014 in Quaternary Geochronology) is included in this calculation.
It isn't. We say so.

Line 200 et al. The issue of non-time-dependent vs. time-dependent scaling is actually more important than described here. The reason for that is that production rates are calibrated using data mostly from the last 20,000 years, and the Earth's magnetic field has been stronger than its long-term average during that time. Thus, at erosion rates low enough that the residence time of material in the soil profile is much longer than this (e.g., most normal erosion rates), the use of a non-time-dependent scheme likely creates a systematic error due to an underestimate of the long-term production rate. Basically this is yet another violation of the steady-state assumption. Again, this is a non-issue compared to much bigger issues in the application of this method, but the text is somewhat inaccurate here as written.

At the end of the paragraph containing the former line 200, we have inserted a few sentences explaining that for slow denudation rates the assumption of time-invariant production rates will introduce some uncertainty because of the high magnetic field intensity of the past 20 kyrs.

Line 215, section 2.6. Unfortunately, I simply don't understand why the calculation described in this section is necessary. I didn't look back at the Vermeesch paper, but if I am remembering correctly this whole procedure was just needed to make the equation relating erosion rate to concentration explicit so it could be solved analytically?? Here you don't need to worry about that, because you are only doing the forward calculation, so why are you doing this? I may not be remembering this correctly, but in other words, it seems to me that all the S's and F's needed for Equation 13 are known a priori, or should be. Typically one would compute scaling for spallogenic production and muons separately (for example, this is in the Stone (2000) scheme as published), and because they are different, that should take care of the fact that muons are less important at higher elevation. Each pathway has already been assigned its own attenuation length, so you can compute mass shielding for each pathway. Then it seems like all you need to do is decide how to compute topographic shielding for muons (I don't know the answer...in the 2008 online calculators it is just disregarded), and you are done. What am I missing here? In any case, this needs to be better explained.

We added two sentences explaining why this is done, but in short, in the equations you have an S term for each production mechanism, but our calculation of the production mechanisms is lumped. So this calculation is to convert lumped scaling terms into four separate scaling terms. There might be a better way to do this, but this way we *reproduce cosmocalc exactly, and judging by the minimal differences between CAIRN and CRONUScalc this procedure does not seem to be biasing the results.*

Line 270-ish. I should point out (in response to the editor's comments) that the issue of asymmetry of uncertainty distributions is really a total non-issue from the geological perspective. Anything with an e in it will have an asymmetric uncertainty distribution, of course, but it's hard to think of any cases where it's actually important from the geological perspective.
This comment will be useful to readers of the discussion, but does not seem to require a change to our text.

Line 280-ish. Numerical partial differentiation by repeatedly doing the full calculation is almost certainly overkill (especially because you've already linearized it). I would do this simply by assuming that the basin has one pixel with the effective P derived from the whole basin. I agree that it is interesting to do it once, though.
We agree that it is overkill but we did not know it would be overkill when we were writing the code (we expected the nonlinearities to be larger) and so programmed in the uncertainties the brute-force way. Changing that now would require significant changes to the code, and the uncertainty calculations are not the rate limiting step.

Line 300. In physical science, 'conservative' is typically used to mean that something is being conserved, e.g., mass or energy. This use in the context of uncertainty analysis is common but incorrect. Instead one should state that the uncertainty estimate is supposed to be an upper bound.
We have reworded this sentence to reflect that the uncertainty is an upper bound.

Line 315. This is an excellent point, that the divergence among various theoretical expectations of how snow shielding works is much less important than the practical difficulty of actually measuring the mean snow depth distribution throughout the year. Frankly, in my view it is not even necessary to mention the various models here, because that issue is pretty much totally unrelated to this paper. As an aside, I found the

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Delunel paper to not be persuasive because, as far as I can tell from reading the paper, we don't know the effect of snow cover on the energy dependence of the neutron monitors (snow is basically like changing the amount of polyethylene on the outside, which affects the spectral response). This would imply that it is likewise unknown whether or not the variation in monitor count rate with snow cover is applicable to cosmogenic nuclide production at all. But that is totally off topic.
The other reviewer seems keen on keeping this bit, so we have.

Line 320-ish. Again, I suggest moving all discussion related to landsliding and non-steady/ nonuniform erosion to a separate section at the end. First, implement the basic model; then, at the end, introduce the abilities to deal with complications that are half-baked at present, but potentially useful in future.
We have followed this advice and separated the discussion of nonsteady/nonuniform cases from the rest of the model description.

Line 345-ish. It is probably overkill to generate separate effective elevations and average shielding factors for input to the 2008 online calculator. I know that technically it's required because the elevation affects the muon proportion of total production and the shielding factor doesn't, but this issue is well down in the noise.
In other cases where we have made a computation that is more robust than really necessary: we don't feel the gain in computational time is worth the effort of refactoring the code at this point. We might do that in a future project to bring the tool online, but feel this level of tweaking is beyond the scope of the current paper.

Line 370. Again, no need to get into the details of snow shielding here. The point remains that it is unclear whether one can estimate the snow depth accurately in any case.
We have briefly mentioned the results of Zweck et al here in light of reviewer 1's comments but add no further details.

In general, in this part of the discussion (i.e., all of section 5) I think it would be easier to understand if you break down the discussion into two parts: things that are linear with respect to the production rate (e.g., topographic shielding), so can be pixel-averaged by themselves; and things that are nonlinear with respect to the production rate (e.g., elevation, latitude, snow shielding), that have to be converted into production rates, averaged, and then unconverted into an effective summary value. At present these two things are mixed up and it's hard to understand what is happening. Overall, this section could be made more clear.

We have edited this section and added subheadings to make it more clear. The basic structure remains unchanged because we follow the sequence of calculations that our software actually computes. Firstly, we must calculate self and snow shielding separately, because in CAIRN these are subsumed within the depth-averaging. So we start with a section based on those calculations. We then need to discuss how the different calculators ingest lumped parameters, since they do it differently. Thus we have followed the format of dealing with lumping for each calculator in sequence. To address the comments of the reviewer, we do specifically allude to the nonlinearity and the reason for calculating the effective pressure with the text: "The CRONUS calculators then calculate production using either an elevation or pressure. Production rates are nonlinear with either elevation or pressure, so we must compute an effective pressure that reproduces the mean production rate in the catchment. This is because the arithmetic average of either elevations or pressures within the catchment, when converted to production rate, will not result in the average production rate due to this nonlinearity. CAIRN calculates an effective pressure that reproduces the effective production rate over the catchment. The average production rate is calculated with: ".

Section 6. This sort of comparison is a terrible mess because of the need to sort out the differences between inherent properties of the algorithm (e.g., point approximation vs. full-basin calculation) and the input parameters (mainly the production rate and the muon interaction parameters). Overall, however, it is accomplished fairly well in this paper; I like the approach of selecting a few representative data sets rather than trying to show a global comparison over all of scaling and erosion rate space, and the

explanation is quite clear as regards which errors apply where in which comparisons. I only have a couple of comments about this section. Line 470. It is quite interesting that there is a systematic difference in shielding factors vs. those originally reported. Do you think this really is because of the averaging a- nonlinear-thing effect? But in any case, as discussed above, the precision of this measurement is overstated in any case due to simplification of the physics.
We have changed the wording here to simply state that our method produces greater shielding than the other studies. Since the details of those shielding calculations are not reported, we can't exactly diagnose why they are different, so merely mention that our greater shielding values are consistent with the results of our sensitivity analysis on the spacing of azimuth and inclination for the shielding calculation.

Line 480. I think production rate differences are much more likely responsible here than anything to do with shielding calculations.
We changed the working so that the production rate differences are mentioned first and topographic shielding is mentioned as a secondary concern.

Line 486. The snapping issue is by far the biggest problem I can think of in wholesale automation of this process. Especially potentially disastrous for literature data.
Yes, this is nasty. That is why the repository for CAIRN contains some tools for checking if your sampling point is in the right place.

Line 500+ and Figures 7-8. As noted by the other reviewer, this effect is nearly all due to differences in the input parameters (production rate and muon interaction cross-sections) and very little due to the spatial-averaging issue or any other aspects of the various algorithms. In large part this is my fault because I have been too much of a slacker to update these parameters in the online code (that is, make v 2.3), which is, frankly, embarrassing. Sorry. However, the need here for the purposes of this paper is to clearly separate these issues. I can think of two ways to do this. One, change the parameters in the CAIRN code to increase spallogenic production by a factor of (4.5/4) and muon production by a factor of (1/0.44). That will very nearly account for the var-

ious input parameter differences. Then do the comparison on that basis. Two, leave these figures unchanged but add in the background some lines showing the expected effect of those changes in the parameters (for elevations vaguely resembling the input data). In any case, this would be extremely helpful in distinguishing the various effects of differences in the algorithm itself vs. differences in the calibrated parameters. This would also make the discussion in lines 520+ more clear as well.
We have followed this advice and generated another figure that shows that the differences between CAIRN and CRONUS2.2 are almost entirely due to the different parameters, and that about a third of this difference is due to the different spallogenic parameters, with the rest being from muons. There still is a small difference between CAIRN and CRONUS2.2 but this is dwarfed by all the other uncertainties.

1. Reviewer's comment 3. It is not correct to say that 'the model of Heisinger overestimates muon production.' The model accurately estimates muon fluxes; the problem is that the cross-sections for production of Be-10 and Al-26 by muon interactions, when applied to those flux estimates, overestimate Be-10 and Al-26 production. In other words, it's not the model that's wrong, it's the cross-section measurements needed to convert the model prediction to a production rate. I think the present paper is mostly correct on this point.
See our response to reviewer 1's comment 3.

2. Reviewer's comment 7. The reviewer is correct here, and this is important. The options here are (i) to get all numbers properly standardized in this discussion, or (ii) preferably, to not get into the details here and simply note that best estimates of production rates are about 10% lower than they were 10 years ago because of improved calibration data.
We have changed the text following suggestion (ii) above.

Use of acronym 'CRN.' Do you really want to exclude stable cosmogenic nuclides? Because this code would work for them too. Perhaps, having written the paper, the authors could just globally search and replace 'CRN' with 'cosmogenic nuclide?'

Done.

'CAIRN' acronym.

In our group we amuse ourselves by coming up with acronyms that are Scots words. This is why we eventually rejected the previous acronym frontrunner of Dr NUT (readers can try to guess what that one stands for). Readers can look forward to future models and methods called NUMPTY and GLAIKIT. Readers who disagree with this approach are welcome to come to Edinburgh and discuss the issue over a dram of whisky.

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