Earth **Surface**
Dynamics
Discussions

ESurfD

Interactive
comment

Interactive comment on "Short Communication: Increasing vertical attenuation length of cosmogenic nuclide production on steep slopes negates topographic shielding corrections for catchment erosion rates" *by* R. A. DiBiase

G. Balco (Referee)

balcs@bgc.org

Invited review of 'Increasing vertical attenuation length...'

1. Summary.

This is a thoroughly useful paper that does a great job of highlighting that if you have a complicated calculation, but you only do the easy complications and not the hard complications, you are probably getting the wrong answer. It's an important contribution to the field of erosion-rate measurement using cosmogenic nuclides that should be

published in approximately its present form. In the following text I have (i) some context for why this paper is valuable, and (ii) a few suggestions that I think would improve the paper.

In addition, I strongly encourage the author to check the calculations again. I don't see any reason to think anything is not correct, but I haven't attempted to fully replicate all the calculations. The main thing I haven't verified is the effect of the slope angle on the apparent attenuation length in the vertical direction – obviously, when you set the geometry up this way, Λ in the vertical direction has to converge on infinity for a vertical cliff, so the principle is clearly correct, but it would be embarrassing to get this wrong and I suggest checking it carefully.

Finally, it does not appear at present that the computer code used to do the calculations is included with the paper or otherwise available (or, at least, I couldn't find it via the journal web interface). This is a deficiency and the author should correct it.

2. Details.

So, anyway, first, some context and an explanation of why this paper is valuable. Initially when cosmogenic-nuclide-based estimates of erosion rates began to be used, people mostly ignored the entire issue of topographic shielding, because it was evident that (i) it was basically negligible for most normal watersheds, and (ii) even if it wasn't, it was much less important than other assumptions, particularly the assumption of steady uniform erosion for a time much longer than $\lambda + \epsilon/\Lambda$. Subsequently, several people, including myself, realized that it was kind of a fun exercise in ARC/INFO, or whatever other GIS package, to compute surface shielding factors for entire watersheds, but never really did anything about it. Eventually Codilean (2006) wrote a paper about how to do this. Most discussion of this in published literature, e.g., Balco et al. (2008) and Mudd et al. (2016), included a sentence to the effect that, oh yeah, although you can calculate the topographic shielding effect on the surface production rate, there is also an effect on the subsurface attenuation that we haven't taken into account, the

unstated subtext being that yes, we really did read Dunne (1999), and calculating the surface shielding factor from a DEM is kind of fun, but then having to do a numerical integration for all depths in all pixels is a lot of work and not really that fun any more. So the present paper is completely correct to point out that the existing state of the art only does half the calculation.

Personally, I did eventually attempt to determine whether doing only half of the calculation was important using the following fairly simplistic analysis:

1. Assume steady erosion, spallation only, and a stable nuclide, such that for a steadily eroding surface $N = P\Lambda/\epsilon$, where N is the surface nuclide concentration (atoms/g), P is the surface production rate (atoms/g/yr), Λ is an effective subsurface attenuation length (g/cm3/yr), and ϵ is the erosion rate (g/cm2/yr).

2. Topographic shielding reduces P at the surface, but it also increases Λ because the cosmic-ray flux becomes more collimated; if these offset each other equally then the overall effect on N would be zero. To determine to what extent these offset each other, consider the nuclide concentration at a single point located in a cylindrical hole of varying depth, such that it has an apparent horizon that cuts off the cosmic-ray flux below some angle. This is basically what is in Figure 2 in Dunne (1999).

I just re-did this calculation using the numerical integration code in Balco (2014, Quat. Geochron.) and, of course, got the same results as Dunne. For this very simplified case (which, of course, ignores the fact that pixels in real watersheds aren't flat), the increase in Λ only partially offsets the decrease in P (compare Figs. 1 and 2 of Dunne), so increasing shielding does, in fact, decrease N for a particular erosion rate. Thus, if one were to consider only the change in P in computing erosion rates, one would get the wrong answer, but it wouldn't be that wrong. So, based on this analysis, I have not ever been very concerned in reading papers about erosion-rate estimates whether authors did or did not take topographic shielding into effect; I concluded that not including it, or alternatively including the effect on P but not on Λ following Codilean and Mudd,

would both be slightly wrong, but not very wrong; the correct answer would be somewhere in the middle. I suspect this overall issue has so far not attracted very much attention because others have gone through some similar analysis and also concluded that it is not that big a deal, and there would be no significant overall science benefit in highlighting the fact that applying only the Codilean scheme is, technically, incorrect and misleading.

So the value of the present paper is that it shows that I, and perhaps others, are completely wrong in relying on a slightly-less-simple-than-Codilean-2006-but-still-too-simple analysis to conclude that this overall issue is probably not that important. This paper correctly points out that (i) increased attention to high-relief watersheds made possible by improved Be-10 measurements at low concentrations, as well as (ii) larger data sets and compiled databases that can be applied to quantifying relationships between topographic metrics and erosion rates, mean that this issue is potentially important. Then this paper actually carries out the more complicated analysis, that no one else seems to have done, of the effect of variations in Λ on nuclide concentrations for real(er) watershed geometries, and concludes that, in fact, considering only the effect on P and not the effect on Λ is significantly more inaccurate than considering neither. This is an important contribution that makes clear that everyone should have paid more attention to this issue.

Some comments that would improve the paper:

Page 1, line 15, in abstract. Using 'catchment mean effective shielding factor of one' is premature here and makes no sense to the reader, because you haven't defined it yet. Of course, you're not talking about the shielding factor as usually defined (P/P_0), you're talking about the effect of both production and attenuation ($P\Lambda/P_0\Lambda_0$). Instead you should say something like 'for flat catchments, the effect of increasing attenuation length due to shielding offsets the effect of decreasing surface production rate, resulting in no change in surface nuclide concentrations in relation to the unshielded case.' Something like that, anyway. Clunkier but necessary to be more clear for purposes of

the abstract.

Page 2, line 6. "In general shown to be robust." Actually, I am only aware of the one paper by Granger that actually validates the method against sediment fluxes. Are there others? In any case, I'm not sure this is true at all. Maybe omit this.

Page 3, line 25. Why Λ increases could be explained more clearly here. You never really say why this happens, which is twofold: for topographic shielding, you have excluded cosmic rays with oblique incidence angles that stop at relatively shallow depths (shallow in a vertical direction of course), leaving a higher fraction of near-vertical ray paths that stop at deeper depths, and then for sloping surfaces the vertical coordinate system means that points that are deep in the vertical coordinate aren't deep in a slope-normal direction any more.

Pages 3-4, end of section 2 and beginning of 3. I think it would be helpful here to introduce the simplest possible case, that is, stable nuclide, spallation only, as described above, where $N = P\Lambda/\epsilon$. That makes it clear that increasing Λ has an offsetting effect on reducing P. As it is you just jump right into the complicated watershed-with-many-pixels case without really making the basic relationship clear.

Page 5, line 15-20. This is a little misleading as written, because of course for a flat surface the depth-dependence is not exactly exponential either, it's just a good approximation. How good depends on how you define the angular dependence of the flux. See the Argento paper.

Page 7, top, and in general throughout sections 2 and 3 as well. It is rather important to understanding all this that the reader really realizes that the z coordinate is always vertical, rather than surface-normal. There is nothing actually wrong with the paper here, but I suggest reminding the reader of this more times than seems necessary at first. For example, this would be a good place to remind the reader of this.

Page 8, line 20-ish. For muons, the penetration depth is so long and the associated

time-to-equilibrium is likewise so long that there is really no plausible scenario in which the steady state assumption is ever correct. Thus, calculating the surface nuclide concentration due to muon production for a given erosion rate is pretty wildly speculative to begin with. Like, for rapidly eroding catchments where a significant fraction of production is by muons, you can get almost factor-of-two differences between, for example, assuming steady-state erosion and assuming that the erosion rate sped up recently. Possibly worth pointing this out here.

Page 8, line 30. This is funny because it is written from the perspective of people who care about steep, rapidly eroding basins, where one worries that the steady-state assumption is wrong because of stochastic landslides/slope failures. In contrast, if you are a person who cares about slowly eroding basins, instead you worry that the steady-state assumption is wrong because there is no way that erosion rates haven't been unsteady due to glacial-interglacial-scale climate changes. You should probably mention both things here.

Page 8, line 30. One shouldn't say "should" in papers (apparently only in reviews). Just state the facts — "The analysis here shows that accounting only for surface skyline shielding yields incorrect results." Let the reader decide what to do about it. The conclusions on the next page are much better in this regard.

3. One comment about the other review.

The issue of "foreshortening" commonly comes up in this discussion as a point of ambiguity because most people with an Earth science background are used to thinking about this effect in the context of remote sensing or radiative heating, in which a finite amount of incident radiation is spread out over a larger area when the incidence angle is lower. The key difference is that cosmogenic-nuclide production happens within a volume, not on a surface, so the radiation-incident-on-a-surface model is not the right way to think about this, and the treatment of foreshortening that would be used in that context is not applicable here. The clearest discussion of this issue is in Dunne, and

the present paper follows Dunne and gets it right.

Printer-friendly version

Discussion paper