Earth Surface Dynamics Discussions

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Comment on esurf-2021-27

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Referee comment on "Escarpment retreat rates derived from detrital cosmogenic nuclide concentrations" by Yanyan Wang and Sean Willett, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-27-RC2, 2021

This paper is quite simple in principle: it points out that a cosmogenic-nuclide measurement in fluvial sediment leaving a basin, which is usually interpreted as a mean erosion rate in the basin, could also be interpreted as a horizontal retreat rate of an basin fronting a retreating escarpment. The cosmogenic-nuclide measurement is telling you is the mass flux out of the basin, and the mass flux could equivalently be the result of either vertical "erosion" or horizontal "retreat." Of course this is correct, and retreat makes more sense than erosion if you are trying to figure out what is happening to an escarpment. The main complication is only that relating a mass flux out of a basin to a horizontal "retreat" of the basin is geometrically fairly complicated, and much of the paper is devoted to unscrambling this issue.

Overall, I think this paper is good and I'm supportive of publishing it. I only found one major issue in review, as follows. Basically, relating a cosmogenic-nuclide concentration to an erosion or retreat rate has two halves: (i) figuring out the cosmogenic-nuclide production rate in the basin, and (ii) parameterizing the mass flux out of the basin. This paper is all about (ii), and simply adopts (i) from previous literature. However, there is a sizeable section of the paper (section 2.3.1, starting line 240) devoted to explaining (i). This is a bit of a problem in the paper, because it contains some vague and confusing elements that make this explanation more confusing than it is in the standard literature describing basin-scale production rate calculations. Specifically, these elements are confusing:

-- Line 249, 'generally taken to be exponential.' In fact this isn't true, because the fact that muon energy increases with increasing depth means that the exponential attenuation length continually increases with depth. You can't approximate this accurately with a finite sum of exponentials. What you can do in integrating production for this application is assume that there is a single exponential function that, when integrated, gives the right answer...but the parameters of that exponential function vary with the erosion rate, so this approach is implicit. I don't think it's necessary to get into this level of detail, but the point is that this statement is oversimplified and confusing.

-- Line 250. I don't understand the sentence " The penetration distance is in a general direction...." What does this mean?

-- Line 288-89. "...most shielding is local...and therefore sums to zero during integration.' This statement implies that a shielding factor less than one at some location within the watershed will be balanced by other locations where the shielding factor is greater than one. Shielding factors greater than one are impossible by definition, so this statement makes no sense. This may have something to do with the fallacy that the total cosmic-ray flux impinging on a basin is the same as the flux passing through a horizontal plane at the top of the basin. This isn't true.

-- Line 291-92. It's fine to simplify the math by ignoring radioactive decay, but if you do this you need to be specific about when this assumption is inaccurate. Specifically, this assumption is OK when the quantity E/L (erosion rate in g/cm2/yr divided by attenuation constant in g/cm2) is a lot bigger than the nuclide decay constant lambda (1/yr). I would choose a typical nuclide (Be-10) and specifically indicate for what range of erosion rates this is true. Note this also needs to be dealt with later at line 369.

The point of all this is that section 2.3.1 doesn't add anything to previous literature -- it's not intended to, that isn't what this paper is about -- but it contains several confusing sections that make the reader more confused than if they had just referred to existing literature. I strongly recommend greatly simplifying this section to use a simple form of the integral production equation and refer everything to existing literature rather than getting into the details here. Sure, the question of whether or not to consider muon production is important in understanding the accuracy of an erosion or retreat rate inferred from a Be-10 concentration, but it has nothing to do with what this paper is about, which is representing mass flux as retreat instead of erosion. Summary: greatly simplify this section.

Other than that, I found this paper interesting to read and I'm supportive of publishing it.

Other minor comments:

Line 384 -- in this section you don't derive expressions for cosmogenic-nuclide production. Rethink this sentence.

Figures 9 and 10. It would be helpful to label the methods used for each polar plot (basin projection, local scalar product) on the figure itself, so that the reader doesn't have to refer back and forth between the caption and the figure.

Note: I did not carefully review the math in the isostatic-uplift section.