I thank reviewer #3 (D. Scherler) for the thoughtful review, and address the reviewer comments below:

1) *My main point is in line with what reviewer #1 already mentioned. I think it is important to emphasize that the reference frame, in which the attenuation length increases with increasing slope angle, is vertical with respect to the geoid and not the surface itself. That basically means that we assume that all particles approaching the Earth's surface follow trajectories that are normal to the geoid. While this assumption appears reasonable for hillslope angles <30° or so, to me it appears unreasonable for very steep hillslope angles, where the described effect is most pronounced. When standing in front of a rock face that is inclined 60° or more, I guess that most people would think the rock wall retreats and not that it lowers. The resulting particle trajectories would thus be less steeply inclined with respect to the surface and the effective attenuation length would not be that large. As a result, the shielding effect would likely be significant, hence lowering the surface production rates; but there would be no counter-acting effect due to increasing attenuation length. Dylan Ward and Bob Anderson, for example, looked at steep hillslopes in glaciated landscapes and assume slope-normal trajectories (Ward and Anderson, 2010, Earth Surf. Process. Landforms 36, 495-512). I think this is an important point that needs to be better exposed in the beginning and discussed later on.*

Response: The vertical (with respect to the geoid) reference frame was chosen for three reasons. First, most studies report erosion rate as a vertical lowering rate and assume primarily vertical exhumation pathways. Second, treatment of slope-normal processes introduces a grid-scale dependence of erosion and shielding calculations that varies with topographic roughness (Norton and Vanacker, 2009). Third, for the case of uniform erosion rate, the resulting shielding calculations do not depend on the choice of reference frame, as long as the orientation of Λ_{eff} and E are defined similarly. I agree with the reviewer here that shielding calculations in landscapes dominated by cliff retreat are poorly suited for treatment in a vertical reference frame—in addition to the challenges articulated by G. Balco in his comment, such landscapes are characterized by spatially variable erosion, a complication that is not captured by the treatment in this manuscript. In revising the manuscript, I have added detail to the intro and discussion to make these assumptions and limitations clear.

2) *My second point is related: is it meaningful to show on Figure 4, curves for inclinations up to 80°? I would argue that there hardly exist catchments with mean hillslope angles of >40°. Such angles may exist locally, but are they relevant for the problem that you discuss? One solution could be to have the y-axis in log scaling, to emphasize the curves with angles <40°, which currently are hard to decipher. As you rightfully note in your discussion, the effect of topographic shielding is small in most cases. All the curves >40° are thus steering the readers attention towards cases that actually don't matter.*

Response: Although it is true that few catchments exist with slopes >50-60°, I think it is important to highlight the extreme cases to emphasize: 1) the catchment shielding correction is not simply smaller than previously assumed, but cancels out entirely for most watersheds; and 2) the spatial variability of factors that control surface nuclide concentration on steep hillslopes. I also find it helpful to better intuit the model behavior by including a wide range of slopes.

I tried changing the y-axis on the plots in Figure 4 to a logarithmic scale, but this does not actually help much the visualization as there is only a factor of 4-5 variation in the parameters being plotted.

3) *P2, Line 18: You cite Norton and Vanacker (2009), but you don't discuss the main point of their paper in any detail later one. I think you should, because they propose that topographic shielding measured from coarse DEMs may underestimate the actual shielding. If that were true, does it mean that, after taking different attenuation lengths into account, there might still be a net shielding effect?*

Response: I now include an additional citation to Norton and Vanacker in discussing the potential influence of rough topography:

> "However, while not entirely transferable to arbitrarily rough topography (e.g., Norton and Vanacker, 2009), Fig. 4c suggests that for slopes less than 40°, the total effective shielding factor does not vary significantly across the hillslope."

Note that the slopes measured on coarse DEMs are also typically lower than those of high-resolution DEMs, such that the increase in attenuation length will be commensurately smaller. It is not straightforward to model the effects of surface roughness, but my intuition is that these effects will cancel out for rough surfaces and lead to similar interpretations of (vertical) erosion rate.

4) *P3, Line 28: Probably here you could mention more explicitly the assumed particle trajectory. You actually say "vertical depth below the surface", but that's ambiguous. Vertical with respect to the surface or the geoid?.*

Response: This is a good point to make explicitly. At this point in the manuscript, I have not yet introduced complications associated with sloped surfaces. I will add a note about vertical exhumation pathways on Page 6:

> "$t_{surface} = t_0 + \rho z_0 / E$ is the time it takes for a rock parcel to travel from depth z_0 to the surface (assuming a vertical exhumation pathway)."

5) *P4, Line 12: Mention here already if the model valley is inclined?*

Response: I revised this sentence to emphasize the model geometry:

> "Because the ridgelines have uniform elevation, there is no net dip to the catchment; the effect of valley inclination will be assessed in Section 3.3." (Page 5, Line 3-4)

6) *P5, Line 11: How good is this approximation?*

Response: It depends on the application, and so it difficult to state concisely here. Mainly, I use this as a way to frame the need for characterizing the effective mass attenuation length numerically according to Eq. (10).

7) *P8, Line 17: The factor 3 emerges only for hillslopes >80°. I think it would be better here to refer to commonly observed hillslope angles, given the title of this chapter, and not extreme cases.*

Response: The factor of 3 and 30% values are both for extreme cases – I added a sentence to highlight a more typical range of effective attenuation length increase due to collimation and slope-effects:

> "However, the magnitude of changes in the effective mass attenuation length due to shielding-induced collimation is at most 30% (Dunne et al., 1999), compared to the potentially factor of 3 or more increase due to shorter oblique radiation pathways on very steep slopes (Fig. 1c; Fig. 4b). For hillslope gradients commonly observed in cosmogenic nuclide studies of steep landscapes (30-40°), the increase in effective mass attenuation length due to shielding-induced collimation and slope effects are 2-5% and 6-15%, respectively (Dunne et al., 1999; Fig. 4b). The dependence of Λ on atmospheric depth, which is typically not accounted for in catchment erosion studies, is minor (<10% for extreme case of catchment with 4 km of relief (Marrero et al., 2016)) compared to the above slope effect for most landscapes." (Page 9, Line 4-11)

8) *Figure 5: I'm curious whether it is ok to refer to mean hillslope angles? Pixel-based hillslope angles are often measured using the steepest descent algorithm. In other words, this algorithm will give you always the maximum slope angle possible. Is that the one you want to have for inferring attenuation length effects? Or would you rather want to refer to hillslope angles measured by fitting a plane to each pixel and its surrounding neighbors, or something like this?*

Response: For catchment-mean hillslope angles, there is not too much difference between measuring local slope along a steepest descent path versus fitting a plane to a local neighborhood. The biggest difference in resulting values is related to the difference in the scale of measurement (i.e., calculating over 2 pixels vs. 3 pixels or more). For the case of a planar slope, the two measurements are of course equal. For the data presented in Figure 5, I suspect the difference would be imperceptible, and much smaller than issues related to DEM quality/resolution.