Final author comments

Gosse et al., Muon Paleotopometry

2025-4370

Responses to RC3:

This is an interesting paper on an interesting subject. It can be condensed a bit by shortening the review sections, which are currently a bit of a weak point. Beyond its overall subject, the strengths of the paper are in the section on how deep samples can provide geomorphic information over larger areas and longer timescales than data from shallow samples, the exploratory data, and the proof-of-principle experiments. The accompanying pdf review contains suggestions and comments on various aspects of these and other parts of the paper. I recommend publication after addressing these as far as is practical.

We will address all general and specific suggestions from RC3.

(i) Use of "μ" as an abbreviation for muon, as in μ-paleotopometry, μ-TCN, μs, μf, sμs, etc. I found the frequent use of "μ" as a substitute for "muon" distracting. It substitutes for a 4-letter word, so very little space is saved. Various sections of the text can be condensed or eliminated (see below) to save much more space. I also found it confusing: I typically read the prefix "μ" as "micro", as in μs (microsecond), μg (microgram), etc, so almost every instance tripped me up. I kept reading "micro-paleotopometry" and "micro-terrestrial cosmogenic nuclide", which is obviously not what the authors intended.
We agree to remove all instances of μ in the paper, except for μbarns or μg as recommended.

(ii) Length and organization: Several sections are longer than they need to be and some could be eliminated without harm to the purpose of the paper. I suggest significantly reducing Section 2 (Background; lines 75-124) by referring to any of the dozens of good reviews or books in the cosmic ray literature which deal thoroughly with muon physics, muon production, muon interactions in air, water and rock, etc. The relevant material has also been summarized in almost all previous cosmogenic nuclide papers on muon-produced nuclides underground.
Will do.

I also suggest splitting off two new (shortened) sections starting at line 125. The first discusses how cosmogenic nuclides are used to estimate erosion rates (lines 125-185). The basic idea is very well established and could be greatly condensed. The second part of this, which discusses "knees" in nuclide concentration profiles, could be condensed or perhaps even eliminated (I found it very confusing; see comments below).
We will split into two shorter sections. However, we suggest that there is value in keeping in more of the discussion on the knees may be useful to others who may consider the same idea. We agree that the slow muon to fast muon inflection will not likely be useful (at least with the currently

measured isotopes, including 36Cl and noble gases, and their measurement precisions), but the shallower inflection may be useful, depending on the situation (environmental conditions, isotopes).

In addition lines 189-293, which contrast the use of muons for measuring erosion rates/changes in surface cover/etc, could become a new section. This section discusses most of the really new and important ideas in the paper - how muon-produced nuclides at depth are sensitive to geomorphic changes over much larger areas and over much longer timescales than spallogenic nuclides, how the the muon flux at depth is insensitive to all the complications of latitude-scaling and paleomagnetic variation that plague spallogenic nuclides, etc. This material deserves more prominence and clarity, which I think would result from condensing the previous 5-6 pages of review and placing it in a separate section. The first sentence (line 189: "Why consider deep muogenic nuclides?") could even be converted to the section heading.
Agreed. In fact, in an earlier version, the manuscript was written this way, including the subsection heading...

With regard to organization, I suggest moving the section on measurement precision to the end, or perhaps to an appendix. Its current position seems to me to break up the flow from Section 3, about the design of deep-sampling experiments, to Section 5.2, which describes examples of such experiments. [Note, there is no Section 4 currently].
We agree that the measurement precision does break up flow, and yes, we noticed the lack of §4 in the submitted version. We think that the limits of AMS precision are important as they do limit the applications of muon-paleotopometry, so we are proposing to move this to §5. We also will add a sentence on recent 36Cl precisions of 2% at 250 m, owing to high thermal neutron capture with high Cl content.

Section 5.4, about muon transport codes and flux models, might also go in a separate section or an appendix. This is an important part of the paper. Models like MUSUN and MUTE are important for calculating the muon flux at depths where range fluctuations become important, and where awkward surface topography complicates analytical muon flux calculations. However it seems out of place between the Swiss Alps example (Section 5.3) and the inverse erosion-rate calculation (5.5).
Agreed, we will switch the order of those subsections accordingly.

Specific comments from RC3

L.25 Last sentence of the abstract. As written, I don't understand this sentence. After eventually reading the material in Section 5.5, I think it is trying to say: "Cosmogenic nuclide concentrations are more sensitive to recent erosion than to erosion rates in the distant past". Also, if that's what the authors are trying to say, it's well known and probably doesn't need to be stated in the abstract.

Agreed. This line is too brief, without context. We will modify (in accordance with RC1's comment on §5.5 and our different model example.

L.49 and 54 Use the terms "mass depth" and "shielding thickness" for what I think is the same thing. I prefer the terms "depth" for conventional depths in centimeters or meters and "shielding depth" for the depth x density product in g cm^{-2}, but "mass depth" is so widely used in the cosmogenic nuclide literature that I'm pretty sure I'm out-voted. Note also that S_i and S_f are currently defined in the caption to Fig. 1.

We will define 'mass depth' as shielding thickness multipled by density with units of g cm^{-2}, as in this manuscript it appears that everywhere we use mass depth or shielding depth they are synonymous; but we will use 'depth' when using cm or m.

L.71 "samples deeply in crust with ..." should be "samples deep in the crust with ...". Maybe it would be better to say "at depths up to a few hundred meters". Petrologists would say that "deep in the crust" means tens of kilometers.

Agreed.

S.2 Curiously there's no mention of the muon half-life, though L.87 notes than muons can decay.

Agreed. Will add.

L.83 Attenuation of the muon flux has nothing to do with muons being "smaller" than fast neutrons. Fast nucleons (hadrons) interact via the nuclear strong force, muons do not, so muons move through matter much farther than protons and neutrons of the same energy.

Will modify.

L.85 and following: Because this is mostly about how muons produce cosmogenic nuclides, it would be simpler to divide the discussion into "fast muons" and "stopped negative muons" rather than "fast" and "slow" muons. The production mechanisms are well described in Heisinger's two papers in 2002, and in many previous papers. This, and all of the Section 2 preamble about atmospheric and near-surface cosmogenic nuclide production, could be condensed by referring to a few relevant papers or Tibor Dunai's textbook.

Will modify.

L.100 Decametres and further on, hectometres. I am all in favour of these SI prefixes, but because they aren't nearly as common as "centi", "kilo", etc it might benefit some readers todefine them the first time each is used in the text e.g. "greater than decametres (tens of metres) ...". [Note - both show up in the abstract, but should probably not be defined there].

We see no reason to modify and not use decametre or hectametre or define them. We will go with what the editor decides here.

L.100 "... uncertainty in the energy spectra for μ_f over million-year timescales ...". Is there any? If so, how is this known? Are uncertainties due to conflicting data? I am not sure what could be measured to characterise muon energy spectra in the distant past. Please provide a reference or references, or omit this statement. It's also worth noting that depth profiles that have been used to calibrate cosmogenic ^{36}Cl, ^{26}Al and ^{10}Be production by muons were generated over hundred-thousand- to million-year timescales. So these reflect some kind of average of the fast muon spectrum over their build-up histories.

We are seeking more information, but will either delete this sentence or cite a reference.

L.104-107 Two comments: (i) The Beacon Heights ^{10}Be and ^{26}Al calibrations definitely do not reproduce Heisinger's muon production parameters. For ^{10}Be, the mismatches in estimates of parameters f^* and s_0, for stopped negative muon production and fast muon production, are 0.27x – 0.46x and 0.44x – 0.79x respectively (the range for each parameter reflects the value of a used to calibrate the fast muon yield; a cannot be resolved from existing depth-profile calibrations). Why the parameters determined by Heisinger differ so dramatically from the depth-profile calibrations remains a mystery, as far as I know.

Our purpose here is to illustrate, using the BH core study, that there is some convergence between natural measurements and experimental results. However, we thought this was too strongly worded, so went with 'reasonable agreement'. Indeed, they are not in acceptable agreement, more work is needed, and this is one reason why we cannot simply extrapolate our knowledge from these studies to decametre depths, and we cited Balco 2017 for motivated readers. We will modify our use of the word reasonable and rephrase.

(ii) In addition to Balco (2017)
please also cite Borchers et al. (2016) Geological calibration of spallation production rates in the *Quaternary Geochronology*, *31*, 188-198. This was the first study to
CRONUS-Earth project. derive muon production parameters from the Beacon Heights depth profiles using Heisinger's production model.

Will do.

L.109 Should also mention MUSIC and MUSUN, a pair of codes for modeling the muon flux underground, used in lots of early muon tomography work. The citation is: Kudryavtsev, V. A. (2009). Muon simulation codes MUSIC and MUSUN for underground physics. *Computer Physics Communications*, *180*(3), 339-346.

Ok

L.128 Equation should probably be numbered. Should also note that this equation is an approximation that only applies to cases where erosion rates are high such that r e / L >> l. i.e. the full equation N = P / (l + r e / L) can be simplified by neglecting l. The simplified version shown is relevant to long-lived spallogenic nuclides for erosion rates greater than a few meters per million years. But not necessarily for nuclides produced at great depths by muons. For the purposes of this paper, the qualification is very important, because L_{μ} for nuclide production by muons is much greater than L_{sp} for near-surface production by spallation.

Good point. We were simply providing a well-known starting point for this erosion paragraph, starting with Lal's simplification. We did not mean to imply this is the equation we use, and we point out that Lal's approach is to use surface samples. But we agree, it may be useful here to point out that the longer muon attenuation length requires a different approach because of this reason.

At the depths dealt
with in this paper, where production is entirely by muons, nuclide build-up is much less sensitive to erosion than at shallow depths.

This has been pointed out the lack of sensitivity of deep samples, in lines 221, 274, and 353.

The table below shows approximate attenuation lengths for
nuclide production at depths in the range ~ 40 m - 300 m. The third column shows the loss rate to erosion for e = 10 m/Myr. At this erosion rate, radioactive decay is more important than erosion in determining nuclide concentrations at depths below ~ 150 m for ^{10}Be and below ~ 60 m for ^{26}Al. i.e. at these depths, the simple equation is wrong by roughly a factor of two (and gets increasingly incorrect at greater depths). In practical terms, at these depths only half the nuclideconcentration is sensitive to the erosion history of the overlying surface. Hence for example, for
^{10}Be in quartz at 150 m beneath a surface eroding at 10 m/Myr, the steady-state concentration will be roughly 1150 atom/g, but a factor-of-two change in erosion rate only changes the concentration by ~ 150 atom/g. This is about one quarter of the sensitivity implied by the equation currently in the text. The fractional change for ^{26}Al, with its larger decay constant, is even smaller.

Depth (m)	Depth* (g cm^{-2})	L (approx) (g cm^{-2})	r e / L (yr^{-1}) for e = 10 m/Myr	(r e / L) / l_{Be-10}	(r e / L) / l_{Al-26}
40	10000	1900	1.37E-6	2.74	1.38
80	20000	3210	8.11E-7	1.62	0.82
160	40000	6110	4.26E-7	0.85	0.43
240	60000	9120	2.85E-7	0.57	0.29
320	80000	12000	2.17E-7	0.43	0.22

* for r = 2.5 g cm^{-3}

Excellent. We have alluded to this in Line 195 and 251, but not as clearly as RC3 here. The point is that the mean attenuation length will increase with depth owing to higher energies needed to achieve this depth. I am not sure what the source (program code) is for these approximate attenuation lengths provided by RC3, but we are happy to use the same code to make this point, and to modify our over-simplified graphs in Fig 3 which use a constant minimum attenuation length. We note that these apply mainly to fast muonic interactions at these depths. The Fig 7 graph makes this point clearly. However, for this illustration, we have assumed just a constant attenuation length for fast muon interactions, but we can modify if RC3 provides the reference to a recommended code.

L.130 L is the attenuation length for the overall production process, not for the particles responsible.

Yes. We will revise to make clearer.

L.140-149 The language in this section is very complicated. It's simpler to observe that steady-state is reached after erosion of 2-3 L (again, for the case where r e / L >> l). This is why the spallation-dominated top of a depth profile reaches erosional steady state much more rapidly than the deep muon-dominated part (L_{μ} >> L_{sp}), and why even single-nuclide depth profiles can be used to solve for both the age and erosion rate of a surface, or the recent and initial erosion rates of a surface with a complex erosion history. I know reviewers are not supposed to refer authors to their own work, but this is covered in detail in section 5.2 of Stone et al. (1998) Cosmogenic chlorine-36 production in calcite by muons. GCA 62, 433-454. See in particular Figs. 8 and 11.

Agreed. This is a far better way to explain the difference. We will adapt, and have no problem with citing the well cited publication.

L.150-185 Discussion of "knees" in depth profiles. There are several problems with this section. (i) There is no mathematical definition of the "knees" referred to. The caption to Fig. 3 mentions a method of finding inflection points, but doesn't specify how the "knees" plotted in Fig. 3C were located.

The caption indicates that for Fig. 3C "a two-component crossing method was used". For this illustration, a fixed attenuation length was used for the fast nucleonic interactions and stopped negative muon interactions. We ignore all other interactions because they are relatively small contributions. We also indicate in the acknowledgements that ChatGPT5.0 was used on Sep. 4 2025 to improve the curve fit and precise depth of the inflection point of synthetic TCN concentrations for Figure 3C, and those ChatGPT5.0 outputs were verified by including the synthetic sample data along with their curves, and by independently calculating where the energetic nucleon and slow muon production curves should meet for a give erosion rate. For Fig.

(ii) The inflection point between spallogenic and muon-induced production would be much easier to see if the profiles in Figs. 3B and 3C were drawn in log(N) vs linear(z) co-ordinates as shown below.

I tried different approaches. I think the difficulty was trying to show not only the inflection point on a single curve, **but to show where it shows up on different erosion rates**. **Note that RC3's graph is not showing the total of the three components, but just the three separate components**. I will try the lin(Z) vs log(N) again to make sure it isn't better when we deal with the sum of the components for different erosion rates. For each of the three graphs in Fig 3, I used the axis coordinates that seemed to show these the best. In the end, we may not modify these much, but I can provide the figures in multiple coordinate systems for the final review.

(iii) I doubt there is a robust mathematical way to find or define an inflection point between parts of depth profiles where nuclide production by negative muon capture is significant and where fast muon reactions dominate. The difference in their depth profiles is slight, and in the cases of ^{10}Be and ^{21}Ne (shown above) production via stopped negative muon capture is minor (^{10}Be) or zero (^{21}Ne; see Balco et al. 2019).

We agree with this. We tried, and could not, as we wrote in the paragraph before Fig. 3. We will just remove the possibility of using the lower knee from Fig 3b, and from the text.

(iv) Fundamentally there is less information in the gradient of a depth profile than in the concentrations (gradient is the derivative of the concentrations). Calculating gradients is inherently sensitive to concentration errors, which will propagate significantly into locating an inflection point from a pair of gradients.

Completely agree. We have commented on some of the additional sources of uncertainty at depths for fast muon – negative stopped muon depths (e.g. the paragraph before Fig 3, and the discussion on AMS measurements at depth). So we will just delete this notion as per the above comment.

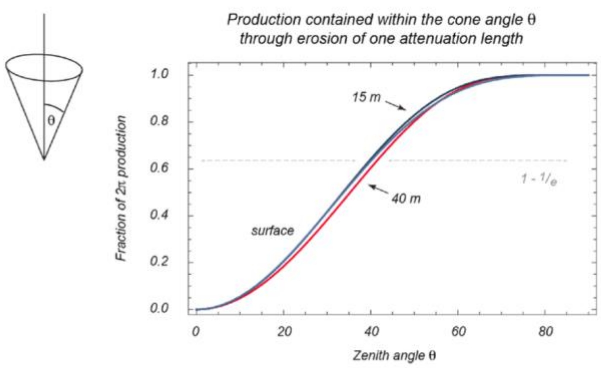
(v) The position of the spallation to muon-dominated inflection point is probably more sensitive to altitude of the profile surface than to its erosion rate (I haven't calculated this, but the sensitivity is clear - spallogenic production roughly doubles with every kilometer altitude; muon production at 3-5 m depth varies by less than 5% per kilometer).

Agreed. Again, the two muon components below ~ 3.7 m are difficult to separate, so we will delete the discussion of the possibility of using fast muons. This is true regardless of altitude. However, the inflection between spallation and stopped negative muon production (upper knee) will be distinct. In all applications of TCN we must use the appropriate production rates for a give lat, long, and altitude.

L.195-199 "... less than a hectare ..." Should this be less than ~1 m^2 ? Cosmic rays responsible for near-surface production pass through a narrow cone (~ 90% within a 60° cone around the zenith). A hectare is a circle with radius ~ 56 m. Even large objects (e.g. boulders) 56 m away don't significantly affect near-surface nuclide production. Effects are fractions of a percent.

The axes for the Radius (m) and Surface Area (km) are correct…Radius pertains to the dashed lines…for example, radius is 55 km (or 5.5 x 10^4 m) radius at depth of 1000 m depth with 89° cone, and 10000 km^2 surface area at the ground surface (the straight line). I think it might be better if I switch the radius (m) axis to the bottom? Or maybe we should just not use hectare, and instead use km^2?

L. 201 The same arguments carry over into this section, where it's argued that topography 10 km away has a significant effect on nuclide production rates deep underground. Again, I think this is mistaken. The flux of muons with range 10 km in rock is very close to zero. Even though the muon flux broadens with increasing energy, the increase in slant range for muons travelling at angles close to the horizon is so large (slant range increases as 1/cos(q), where q is zenith angle) that such muons contribute minimally to nuclide production. As shown in the diagram below,



Cumulative fraction of nuclide accumulation due to cosmic rays travelling at zenith angle q as erosion removes one attenuation length L from the overlying surface (i.e. in build-up of ~ 63% of the eventual steady-state concentration). Note L ~ 7.5 m at 40 m depth, for r = 2.5 g cm^{-3}

.

production by muons travelling at zenith angles > 60-70 degrees is insignificant in nuclide build-up at depths up to 40 m. I haven't done the calculation for hundred-meter depths, but I doubt the result would change significantly. To reach 40 m, a muon travelling at 60° to the zenith requires energy > 48 GeV. At 100 m, the required energy is > 130 GeV. Fluxes at such energies are much smaller than the near-vertical flux which reaches the same depths. See also Fig. 4 of Heisinger (2002) on fast muons, which shows how the $\cos^n$(q) distribution of muon arrivals underground narrows around the zenith at depths below 1000 hg cm^{-2} (roughly 40 m depth in rock).

*We agree with all of this. However, we are **not** at all suggesting that there is a significant effect on nuclide production at low incident angles. We were simply trying to make the corollarial point. We state that because of this large area (radii of >10 km) at the surface though which incident muons can penetrate, the deep samples are LESS SENSITIVE to small changes in mass depth. Absolutely the greatest flux will be at 0° zenith and diminish with lower angle. But they are still entering at these low angles.* ***We can provide John's relationship too, but this does not really add much to the benefit of going to deeper depths. We are discussing this further among the authors. We may just provide both.***

L. 225 [Figure 4]. See the discussion and figure above. It's not the surface area subtended by a zenith-angle cone that matters. The area has to be weighted by the fraction of nuclide production due to the muons that pass through it. It's neat that muon-produced nuclides at tens- to hundred-meter depths provide information about erosion over much larger areas than near-surface samples, but the underlying geometrical argument does not imply that there's information about erosion over thousands of square kilometers. Based on the same calculation as the figure above, the area contributing 90% of the production to a sample 40 m beneath an eroding surface is ~ 5000 m^2. This area is contained in the circle of radius ~ 40 m above the sample point.
I suggest omitting or replacing Fig. 4.

Our intention is not to suggest that this technique with a single sample below ground should be used for monitoring erosion rates over huge areas above it. We propose that this muon paleotopometry method can yield an estimate of the erosion rate of the surface immediately ABOVE the sample site. Again our point is that because muons penetrate further, and can enter *at a range of incident angles, that they are not sensitive to small changes in mass depth. So the vertical muon paleotopometry method is providing an average…and best for low relief settings. The horizontal method* ***is more appropriate*** *when variations in topography (relief generation owing to incision) are attempted to be monitored, because the samples from a vertical core attempts to dampen the effect of topographical change.*

L. 251-259 Discussion of geomagnetic effects on muons reaching hundred-meter depths, and Table 1. There's a lot more here than is needed. The discussion could be summarized by saying that muon energies required to reach hundred-meter depths (> 60 GeV) are significantly greater than geomagnetic cut-offs (< 18 GV). The primary particles whose interactions produced the muons had even higher energies. Hence there is no significant latitude effect on the muon flux reaching such depths.

Agreed. We make the point that the cutoff rigidities are exceeded in line 252.
True, there is no effect on latitude on these high energy incident particles. The reason that we listed the cutoff rigidities at different latitudes is because the cutoff rigidities do depend on latitude.

Re Table 1. I'm not entirely sure, but I think columns 3 and 4 are referring only to vertical muon spectra. If you integrate over zenith angle q, the median energy of muons reaching the depths in column 1 would be even greater than the values in columns 3 and 4. This strengthens the argument.

We are waiting for one of our authors who provided this table to confirm, but YES I am confident that these are vertical muon spectra linked to the vertical cutoff. RC3's comment would improve on this, so we can add a figure with the integrated 0-75° slant depth cone…showing that the energies would actually be greater if we considered incident particles with lower slant angles (but noting that the flux of these are lower because of the higher energy required).

L. 266-269. This seems like an unwise argument. If concentrations were really invariant (" ... serve as replicates ..."), they would be insensitive to erosion. What's being said is basically re-stating the discussion about the long attenuation length L for muon-induced production, covered in lines 140-149 above. It would be easier to say that deriving an erosion rate or erosion history from a depth profile at tens- to hundreds of meters will be most sensitive if the samples are widely spaced down the profile.

Absolutely. We used the term 'practically' invariant, knowing that they are not, but it is difficult to measure. Nevertheless, not a wise choice of words, lol. We will make this change.

L. 306 This also re-states discussion about cut-offs and the absence of latitude effects for deeply-penetrating muons (lines 250-259).
We will delete this and other repetition.

Fig. 7 (L. 370) Figure caption refers to "isoeroderes", which I think is a made-up word. The curves on the figure are depth profiles calculated for different erosion rates, which seems like a simpler description. Also, the figure and the final sentence of the caption could be used to replace a lot of the complicated text in the section on L and its effect on sensitivity of the depth profile to surface erosion (lines 140-149; see my notes and table above). The statement that "The wider spacing [between profiles] toward the top provides the improved resolution of erosion rates." is again demonstrating that the profile is more sensitive to erosion where L is shorter, and less sensitive deeper in the profile where L is long.

Regarding isoeroderes, we will delete it throughout the manuscript, and replace it with depth profiles calculated for different erosion rates.
Regarding the rest of this comment, as mentioned above, we have communicated this exact point in different ways.

The y-axis of Fig. 7 could be more clearly labeled. *e.g.* indicate units of [10^4 g cm^{-2}] rather than the "x10^4" annotation at the top of the diagram.
Done.

L. 382 The limitation for ^3He and ^{21}Ne is almost always the difficulty of distinguishing small amounts of *cosmogenic* ^3He and ^{21}Ne from much larger amounts of nucleogenic ^3He and ^{21}Ne built up over the lifetime of the rock. The nucleogenic production rates are usually small, but in cases where the He and Ne closure ages are tens or hundreds of millions of years, build-up will likely swamp muon-induced build-up.

Agreed. We already make this point in line 322 (we state that for 21Ne we need quartz with minimum pathways to make non-cosmogenic 21Ne). But we can make this clearer here in line 382 where we discuss measurement limitations.

L. 400-404. See comment above. A similar limitation applies to ^{36}Cl if the mineral analyzed contains significant ^{35}Cl, resulting in $^{35}Cl(n,\gamma)^{36}Cl$ production from radiogenic neutrons. Accurate estimation of (α,n) neutron production is notoriously difficult. This is a significant source of uncertainty even for surface exposure dating of young, Cl-rich samples.

Agreed, the thermal neutron capture can be a significant factor. In where volumetric or quantitative water is high (e.g. upper 1-2 m of a granitoid with low fracture density like the one at Sudbury) the estimation of the thermal neutron derived 36Cl is difficult. However, in dry competent rocks, this is less so, and improves with depth. We will point out that sample sites should be pre-screened. For instance, we have measured total Cl content in feldspar separates from the same core samples that the quartz was extracted from for 10Be and 26Al. The Cl content is 400 µg through almost the entire core to 250 m. Lawrence Livermore National Lab achieved a precision of 2% on Cl, and I believe those values. However, as RC3 right points out, it doesn't really matter how will you measure this, the non-cosmogenic component of 36Cl from the high Cl concentration will completely shadow the relatively small concentration of cosmogenic 36Cl. In fact, in this core, we can't have confidence in the isolated concentration of cosmogenic 36Cl below about 3.1 m. However, 400 ppm Cl is a very high concentration for feldspars. If a future site has < 100 ppm, the impact is less significant, but limits precision cosmogenic 36Cl to the uppermost 10 m (albeit still muon produced). We can make this clearer and can use a graph with actual data to demonstrate this.

L. 426 "The mine stope runs laterally ...". I think the term should be "adit", "drift" or "tunnel". A stope refers to a vertical or cavernous opening. Adits, drifts and tunnels run horizontally.

Agreed. Stope is too generic. We will consult with the mine engineer to get their recommendation.

L. 441 / Fig. 9 Consider adding a depth scale to the edge of Fig. B or an indication of the depth between the valley axis and the line of samples. The necessary information is given in the bottom panel of Fig. 10, so this is not a big deal, but it would be useful in looking at Fig. 9B.

Done.

Fig. 10 Top panel and caption: What is being plotted as the "Erosion Rate" in the top panel? From the correspondence between the top and middle panels it looks like what has been calculated is a the 2p steady-state surface erosion rate that would correspond to each of the measured ^{10}Be concentrations. If so, I don't think it's a very helpful measure to be plotting. Based on the model sketches and description, the samples won't be at steady-state and don't have 2p exposure geometry, and the surface above each sample has a 2-stage (or more complicated) erosion history.

Agreed. We will explain the Erosion Rate panel more clearly. This was an overly simplified comparison, and indeed there is some influence of topography as the valley evolves. Obviously we have no way to constrain the evolution of the valley precisely, but at these depths this is less significant, and a simple geometric model can work (fixed valley profile and stream gradient as you say). We can use the MUTE program (Figure 13) to give us a flux as intervals as the valley deepens (it incorporates topographic shielding) to make this more robust. We were waiting for the latest version of MUTE which allows input of variations in rock density, but this still has not been completed, so we will use the existing version of MUTE and state that we are assuming constant density.

L. 455-520 Discussion of the horizontal transect experiment. Overall, this is a very neat experiment, but I got lost in the complicated discussion. It would be good to examine more realistic solutions based on the combined data (all of which are sensitive to the erosion history of the valley) rather than going through the data one-by-one. With a few geometrical simplifications (e.g. assuming a fixed valley profile and stream gradient), one could forward-model ^{10}Be concentrations at each sample position for cases such as slow, steady valley incision over millions of years, steady valley incision starting at some time in the past, very rapid incision at some time in the past, with slow or zero erosion before and after, etc. Going through the exercise should reveal whether any such geomorphic histories are more likely, which can be ruled in or out, whether data from surface samples would be useful, whether it would be helpful to obtain data from more samples underground, etc. It would be a good illustration of the goal of the paper, which is to show how deep, muon-produced nuclide concentrations can provide geomorphic information over large areas and farther back in time than can be obtained from surface samples. [Note - this is also a lot of work. While it would be a great addition to the paper, getting it done shouldn't be a barrier to publication].

Agree. We will shorten the discussion, and move much of the cosmogenic nuclide data to the supplementary file. However, we are waiting for a more rigorous version of MUTE to be coupled with PROPOSAL before conducting forward models of the actual landscape evolution. Computing different erosion histories for different geometries, and which scenarios best mimic the subsurface 10Be concentrations, is actually an ongoing PhD project, and will not be ready for this manuscript. There are numerous unconstrained variables, and we want to provide a more robust model for this location than something that is just an improvement over our current version. The main purpose of this experiment is to provide a proof of concept, which we have. However, we will modify the calculations with the new MUTE topography adjusted production rates, and forward model a more simplified version (discussed in the next section).

The word "isoerodere" is used again in line 484 when describing a depth profile. See note re line 370.

Agreed. Deleted everywhere.

Fig. 11. Inexplicable depth-profile comparisons. See two previous notes above - the data need to be considered as a whole, it makes no sense to consider them one-by-one. Fig. 11 compares ^{10}Be concentrations, which are very unlikely to be at steady state, to simple steady-state depth profiles. I suggest omitting it.

We can delete this figure. It was a means to simply illustrate what subsurface data from different locations might look like for different erosion rates (we know the relative erosion rates…i.e. samples below the deepest part of the modern valley should have faster erosion rates than the samples under the plateaus (although the geometric evolution will control how different)…too RC3's earlier point however, we have assumed constant attenuation lengths for the muons. So, if we adjust the energy dependent muon L with depth, and correct each sample separately for its incision shielding geometry evolution, we may come back with a more useful figure, but otherwise we can just drop it.

L. 543. "rock distances beyond 50 km" This has to be a misprint. As far as I know no experiment has ever observed a muon with a range of 50 km in rock. Even if such energetic muons are occasionally produced, their flux (and contribution to nuclide production) would be negligible compared to muons arriving from close to the zenith.Note also that topographic calculations out to 50 km distances need to take the curvature of the Earth into account. Topography 50 km away has to be taller than ~ 200 m before it's even visible over the horizon.

Agreed, the muon fluxes that are generated through these distances are negligible. It was our point that we intend to use just the 75° cone. We will delete the notion of extending the cone to such a shallow angle.

Fig. 13 caption Zenith angle range θ = 0-75°. Based on the slant range effects noted above (comments re lines 201-225) this should be adequate for almost all calculations. Only very specialized cases (e.g. involving steep slopes, depth profiles into narrow mesas, etc) are likely to need broader angular coverage.

We agree.

L. 583-620 4-stage erosion calculation. (i) For the hypothetical case considered, the discussion should also mention that sensitivity to erosion in the first 2 stages (8-4 Ma and 4-2 Ma) is limited by the nuclide half-lives. 4 Myr is nearly 3 half-lives of ^{10}Be and nearly 6 half-lives of ^{26}Al, so fewer than 1/8 of the ^{10}Be atoms produced in the first stage (and essentially none of the ^{26}Al atoms) remain in the present-day sample. Al-26 will barely remember the second stage (4-2 Ma) either. Coupled with the fact that the hypothetical sample was 70 - 90 m deeper (with production rates correspondingly lower) during these initial stages in the geomorphic history, there's very little chance of recovering accurate erosion rates so far back in time. [Realistically, the amount of ^{10}Be (and even more so, ^{26}Al) surviving in the sample from > 4 Myr BP is likely to be smaller than the uncertainty on the concentration measurement].

Agreed, we can raise this here, but we also mention it below.

L. 589 Simplifying the calculation to an exponential approximation with a fixed attenuation length of 5700 g cm^{-2} is not valid. This is the approximate attenuation length for production at ~ 30 m depth, but the calculation involves much greater depths (50–300 m) where the attenuation length is much greater. At 300 m, it is ~ 25000 g cm^{-2}.

Agreed. We will go back to the ravine incision event circa MPT, inverting for plateau erosion and incision of ravine, as an example. The actual incision may have began earlier, but we will attempt solutions for the past 1 Myr. This updated model can employ the MUTE topographically-adjusted muon energy/flux.

L. 611 "... searching for four erosion rates with two isotopes". The specific problems are: (i) the two isotopes have similar production profiles and attenuation lengths, so there is not very much sensitivity to depth in their accumulation rates. (ii) The ^{26}Al half-life is too short to be useful for the first two stages in the erosion history. See note above.

Yes, 26Al is not applicable beyond about 0.5 Myr, depending on erosion rate. But the point is that the shorter-lived isotope can provide evidence, for instance, that erosion rate has increased (or decreased) over the past 1 Myr (constrained by 10Be or noble gas). We will provide a new sensitivity analyses, to show the usefulness (or not) of this two-isotopes approach at deep depths, for a simple erosion history.

L. 641 Should read "Beacon Heights site, Antarctica" and should cite Borchers et al., 2016 as well as Balco, 2017. See note re L. 104-107 above.

Done.

L. 657 Low-level ^{10}Be carrier will be essential, but there's no indication that Al carrier needs to be from a deeply-shielded source. All commercial Al I've ever measured has ^{26}Al below detection limits (^{26}Al/^{27}Al < n x 10^{-16}). In fairness, it is always worth confirming that Al carrier (which typically comes from bauxite-derived commercial Al) is free of ^{10}Be. In the case of ^{36}Cl, Weeks Island Halite, which is widely used as a Cl carrier, (i) is from a deeply-shielded (salt-dome) source, (ii) has a ^{36}Cl/Cl ratio < 5 x 10^{-17} (Fifield, L. K. et al. (2013). Ultra-sensitive measurements of ^{36}Cl and ^{236}U at the Australian National University. *Nucl. Instr. Meth. B*, *294*, 126-131).

The commercial carrier we use has a similar ^{26}Al/^{27}Al. Yes, ours yields about 2 to 4 x 10^{-15} for process blanks, and the carrier itself is below this (in the 10^{-16} range). Yes, I have noted ^{10}Be in some ^{26}Al commercial carrier (we generally measure process blanks comprising just ^{10}Be, just ^{26}Al and with both ^{26}Al + ^{10}Be added, and sometimes see an additional ~2 x 10^{-15} for ^{10}Be/^{9}Be process blank). Unfortunately, the number of measurements have been too few to make a confident correction. I take your point that with the LLNL ams we are approaching current DL, not to mention that the uncertainty in those measurements are close to 20% or worse. Nevertheless, considering that some of our deeply shielded sample measurements are approaching this ^{26}Al/^{27}Al ratio, I don't know why it wouldn't be useful to attempt to achieve a lower ratio with a shielded Al crystal with no ^{10}Be (i.e. not using a bauxite) instead of a commercial carrier. Yes, we also have a low ^{36}Cl salt (the Winsor Salt from underground mine in Pugwash NS which often only generates a few counts at PRIME Lab, but we have not measured it at ANU, great that they can measure to < 5 x 10^{-17}). In the past decade we measure this salt blank as either a process blank or just a carrier measurement with each suite of ^{36}Cl samples. We do not quite have enough measurements over the years to go just with the salt carrier for our *blank subtraction, but this is the intention…again, supporting a deeply shielded source over a commercial source.*