Carling et al

Ice buttressing-controlled rock slope failure on a cirque headwall, English Lake District

Earth Surface Dynamics 2023

This document is the authors response to comments RC1 and RC2.
The authors' replies to the commentary provided by the reviewer are in light blue

Reviewer comments

David Jarman
mountain landform research
Scotland

SUMMARY

Recommendation - accept with minor changes, subject to attention to the figures
(with scope for more substantial revision/improvement at editorial/authorial discretion)
This paper addresses an apparently trivial landform feature, a very small and unimpressive RSF, which (most unusually, in the British mountains and generally) happens to be discernible from a major road (M6). It analyses the slope stresses acting on such a slipped mass by an engineering geology technique Swedge, with and without support from glacier ice. It dates the outer face and the scar above, and from their significantly older and younger ages, it states that this corroborates a contention that the slipped mass could only have been emplaced thus with the support of a waning valley glacier.
The paper is in fact of great interest, and a novel contribution to geomorphology, for four main reasons: 1. case studies of individual RSFs in the British mountains are uncommon, and those analysing their engineering geology are vanishingly rare (and essentially confined to a couple of PhD theses on Scottish Highlands sites, now decades old). 2. few RSFs in the Lake District (two) and Highlands (a score or so) have been dated - all of them fully disintegrated rock avalanches, whereas this is an arrested translational slide. Even globally, quasi-intact rockslides have seldom been dated due to obvious sampling difficulties, with the emphasis on 'antiscarp' trench faces rather than outward-facing slopes as here. 3. glacier support for RSF emplacement is not an entirely original notion, but to the writer's incomplete knowledge this may be a pioneering study of a possible demonstration example, in Britain at least. 4. morphologically, RSFs in cirques form a small minority, with some of those on outer flanks rather than headwalls; and the distance travelled by the putative slipmass while remaining quasi-intact - 110 m - is remarkable, especially for such a small feature. (the authors are perhaps too modest here, and could increase the impact of the paper by placing its originality more firmly within the British and global montane RSF literature).

We thank Dr Jarman for his detailed and enthusiastic review of our submission. In particular, he indicates that the manuscript is of great interest and might be accepted subject to minor changes, involving attention to the figures. He provides four main reasons why the submission is a novel contribution to geomorphology. In brief, 1) the approach using 'engineering geology' is to be applauded; 2) we have dated our RSF which is unusual; 3) the suggestion of glacial support for a RSF, although not original, is here for the first time demonstrated as a possibility; 4) the distance travelled by the RSF is remarkable given the bedrock remained intact. It is for these reasons that we prepared the manuscript bringing a numerical stability approach to the subject of a RSF that seems to have descended remarkably intact from a steep failure plane and date the event.

The reviewer also concludes that 'the two dates currently available deserve mention as broadly supportive, with all due caveats, and as advancing the case for more systematic dating of this site and others to compare or contrast'.

The reviewer provides 15 pages of commentary, some of which is focussed on positioning this study into the broader context of RSFs and other slope failures within the Lake District and potentially further afield. Below we provide comment and have revised the manuscript where appropriate, but we have not replied in detail to those points that lie outside of the scope of the current submission. It is evident that the reviewer, in his enthusiasm, would like us to extend our study to make generalised statements that apply to other RSFs. We purposefully have not done so, as we believe that a larger corpus of case studies on the controls on RSFs is required before such generalizations can be made. Moreover, to extend the manuscript content and figure would lead to a submission in excess of the guidance on manuscript lengths.

Although the site is trivial in size - at 0.03 km2 in extent by a standard method it is only just above the threshold of 0.01 km2 for designation as RSF (see 'References') - this proves not to be a demerit, for it is in effect a scale model or field-laboratory experiment. Although this site is unusual in some ways, it is not *sui generis*, and the findings are relevant to the wider study of RSF behaviour. Again, these virtues could be made clearer.

Although the RSF is small, it is not trivial in-as-much as it offered the opportunity to explore the failure mechanisms within a numerical framework, which is novel, as indicated by the reviewer. We purposely did not over-emphasize the possibility of extending our conclusions more widely, as we acknowledge that such a model may not apply to many other RSFs.

While the paper is eminently publishable essentially as it stands, it relies on a number of assumptions, stated and unstated, which bear closer scrutiny. The paper could be greatly improved by adumbrating these assumptions; alternatively, the editors and authors may be well content with 'putting it out there' as an aunt sally to provoke and stimulate further debate and work. If the latter course is followed, it is recommended that at least the 'corroborated hypothesis' be expressed more circumspectly[1], with due acknowledgement that it does rest on a set of convenient assumptions, as befits a controlled experiment.

We address the issue of assumptions below at the point where the reviewer details his comments fully.

1 here it is noted that the paper has previously ben rejected because of its reliance on two single dates, and that the Associate Editor has already advised circumspection in this regard. It might help further if the emphasis is taken away from reliance on these dates, and the case study presented as a multi-pronged assessment, with the dates providing a measure of comfort, but far from essential to the conclusions reached.

Before exploring these assumptions in the COMMENTARY that follows, some practicalities can be dealt with.

REFERENCES

The lead author is now aware of a new overview paper on Lake District RSF, published as the present paper was being submitted :

Peter Wilson & David Jarman (2022) Rock slope failure in the Lake District, NW England: an overview, Geografiska Annaler: Series A, Physical Geography, 104:3, 201-225,
DOI: 10.1080/04353676.2022.2120261

In addition, the reference to Jarman (2005) should be 2006; this paper is largely superseded by an overview paper:

Jarman D, Harrison S (2019): Rock slope failure in the British mountains. Geomorphology 340, 202-233.

These two papers do not require any major alterations in the present paper, beyond updating the regional data, and perhaps noting that the RSF conceptual typology has been generalised since 2005/6 for much wider montane applicability. They do contain discussions of RSF timing and causation which may be of interest.

TEXT

The submitted paper is returned with 'sticky notes' for possible clarification, and marking a few trivial edits. They also identify passages discussed in the Commentary below.

We have addressed all the minor comments and 'trivial edits' shown on the marked manuscript. We have added the additional number of RSF in the Lake District supported by the references provided by the referee. We thank the referee for these references.

FIGURES

It is recommended that the visual presentation of the site be substantially upgraded:

Fig 1 the arrow is too crude and impinges on the feature - it would be more instructive to delineate the identified RSF site boundary, including 'cavity' or source area at rim, and basal extent, with a fine dotted line, and a fine arrow suggesting slipmass trajectory.

This fig. presents an overview of the Great Coum cirque shown without any labels to obscure the morphology (labels and lines are added in a slightly more close-up version presented in Fig. 4B). The N-arrow is clear and does not impinge upon any landforms discussed in this MS. The RSF is delineated as described by the reviewer in Fig. 4B.

Fig 2 this regional map is neither sufficiently regional nor adequately detailed at Lune gorge context scale. Two maps are needed:

 regional, including

 full named extents of Shap Fells and Howgill Fells, with Lake District as far west as say High Street;

It is not clear what would be gained by showing all of the Shap Fells and Howgill Fells in this figure. Those sites are not directly relevant to the focus of our MS, which is the RSF at Great Coum.

-- full extent of upper Lune basin, with general ice movements as long curvilinears, not crude arrows, and famous drumlin field just to SW identified and placed in ice outflow context;

The linear arrows clearly indicate ice flow direction. We intentionally generalised the ice trajectories in linear segments because adding curvilinear shapes would be unsupported speculation. The drumlin field is not relevant to our study.

-- ice movements not just coded but numbered 1-2-3 to clarify sequence (nb. present caption omits ST2; it could helpfully specify what all three codes represent)

Good suggestion. We have modified the arrow labels to indicate the temporal sequence and edited the caption accordingly.

-- extent of Silurian outcrop, with adjacent lithologies;

It is not clear what a lithological map would add. This MS does not aim to analyse the regional geology. A very large map would be required to include other lithologies as the region of Great Coum is all Silurian rocks.

-- RSFs, with small-large sizes, thus emphasising the High Street cluster and sparsity here (easily obtained from Wilson&Jarman 20222);

This MS sets out to examine the Great Coum cirque including a detailed engineering-based failure analysis. Other RSFs in the region are beyond the scope of this MS.

-- glacial cirques (from Ian Evans inventory), perhaps with grades/elevations, again emphasising sparsity around here and unusually low elevation of GC/LC;

Beyond the scope of this MS. We report the elevation of the ELA in the GC/LC area.

-- the M6 even !!

The meaning of this comment is difficult to decipher. The position of the M6 highway is not relevant to the study.

2 four additional small sites have since been identified from imagery in this area - see appendix
-- local, perhaps as broad as present but less N-S to zoom in closer in landscape format, including
-- named highest summits and elevations in each hill mass (Grayrigg Pike, not Forest, being the peak closest to the site)
-- elevations along Lune gorge floor and rims

None of this information is directly relevant to our study.

-- RSFs, distinguishing parafluvial cases in Howgill V-shape valleys (#9.03 A/B) from paraglacial locations as here and Cautley #9.01/02
-- cirques

This information is not related to the present study. We cannot agree to most of these suggestions. Figure 2 is a location map which is kept clear of unnecessary information, as stated above. It serves adequately to locate the study site. The reviewers' suggestions relate to placing the RSF into a much wider context which is not the focus of the submission.

Fig 3 these long profiles really require a proper map of the two cirques to locate them, which could very usefully also depict the glacial moraine features etc described - this might be includable on the 'local context map' requested above.

It is not necessary. These long profiles are short and self-explanatory; they run along the centrelines of the cirques as described in the manuscript. The caption to Fig. 3 includes the GPS end points of the long profiles such that the reader can easily find these on platforms such as Google Earth. The till and moraines mentioned in the main text provide context but are not required to develop the model, so we purposefully did not add this unnecessary detail to any figure.

Fig 4A the schematic wedge already gives one actual dimension, the 44º slope - why not add the other dimensions ?
(nb. the word 'debris' in nigh invisible)

It is important that definition diagrams allow the reader to see the essential detail alone. This schematic does this without the clutter of additional dimensions which are detailed in the main text and within Table 2. We have redrawn the figure to ensure the word 'debris' is clear.

Fig 4B we are brought very little closer to visualising the RSF than in Fig 1, and with the same GEarth image offering no change in perspective - a different angle / historical image, or a photo, would add considerably to our grasp.

We chose to use exactly the same image within Fig. 4B as in Fig. 1, so the reader can see the RSF without clutter in Fig. 1 and then see it again with the minimum additional necessary notation in Fig. 4B that allows the model to be established. Fig. 4B contains the essential information the reviewer noted was missing in Fig. 1.

(nb 1. it is not clear what the three components marked "c" intend - is a mappable fault trace actually observed to be displaced down the headwall, or exposed in it at a lower level by removal of the slipmass, or is this merely suggestive ?)

The text explains that the fault line defined by ‘c’ can be seen in the field and was mapped. It appears the reviewer has reviewed the figures without referring to the main text.

(nb 2. sample codes HW and OSF are stated in the caption and should be added to the figure) - ideally, with the actual dates (as concise ~ages) to aid our grasping that they are older above younger - the writer has to recheck this every time....

Good point. The sample locations for cosmogenic dating are shown in Fig. 4B. The notation HW and OSF are now added. The caption and the main text allow each one to be identified. We do not add the actual dates to this figure because these are not reported until a later section.

Fig 7 this useful diagram is very hard to decipher from the caption. Do we understand there to be two pentahedral volumes:
- potential, delineated by A-D (can it not be extended E-F for greater clarity ?)
- needed, toned purple
The term ‘pentahedral volume’ should be stated as ‘potential pentahedral volume’ at its first appearance in the caption, and might be toned lightly.

The caption has been revised as suggested. The potential pentahedral volume has not been lightly toned as the geometry is already clear due to the annotation A, B, C and D. Further tones would obscure the figure. For clarity, we only labelled those points required to define a pentahedral, so no further labels have been added.

The ‘needed pentahedral volume’ should be stated to be purple (why does its contact with the riser need to be curvy, in a schematic ?)

The volume has now been labelled as ‘purple’ in the caption. The curvy line is to indicate that the ice was unlikely to lie against the riser is a uniform manner. We have added a sentence to the caption to make this clear.

RECOMMENDED ADDITIONAL FIGURES

1. detailed site map - this map of Great Coum would define the RSF extent and components properly, in a way which the thumbnail sketch in Fig 4B cannot. It would mark and codename the two sample locations (their exact locations require to be identifiable, with hi-res grid refs;

We have explained above why we have not adopted this suggestion.

2. sample locations - photos of the actual outcrops sampled;

A good suggestion. We have added photographs to the Supplement.

3. geological correlation - the siltstone bands stated to match across headwall and RSF must be location-mapped, georeferenced, and presented in a standard geological section showing breadth and thickness of relevant exposures.

In our opinion a detailed site map is not required given the focus of the submission. The dimensions of the components of RSF are given in Table 2. The locations of the samples are already provided in Fig. 4B and the GPS locations are given in Table 2. A detailed stratigraphical log is not required to establish the model. Such an exercise would constitute a new study, leading to substantial stratigraphical information beyond the requirements of this submission that would warrant an additional publication. The site has never been stratigraphically mapped by any party (including BGS), as key exposures are not accessible except by abseil on unsafe rock faces. Correlation with outcrops at distance is not possible.

Photographic coverage - a Supplementary file of selected imagery would greatly help in comprehending this intriguing site and its setting (see eg. Wilson and Jarman 2022). The writer will be providing an annotated Powerpoint slide set to the lead author with imagery from a site visit (28 May).

Good suggestion, we have added a selection of photos to the Supp.

The writer's colleague in Lake District RSF studies, Peter Wilson, has now seen the paper, this review, and the Powerpoint slide set, and would encourage further development of this interesting paper along the lines suggested.

Reviewer's commentary is supposedly confidential until the Editor agrees to post it online.

In his enthusiasm the reviewer requests so much detail that the manuscript would far exceed the required submission length.

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COMMENTARY 1 - ASSUMPTIONS

This paper boldly asserts (emphases by the writer) :

(647)

Thus, our hypothesis '*a steep, faulted, and unstable rock slope has experienced buttressing by glacial ice*' as proposed in the Introduction is corroborated here.

We have demonstrated that a RSF in the headwall of a cirque in the Lune gorge occurred as a slow downslope movement of an intact rock mass due to the presence of a supporting glacial ice mass buttressing the failed slope.

The Abstract is a little more circumspect :

The 18.0 ± 1.2 ka date is consistent with a small buttressing ice mass being present in the cirque at the time of regional deglaciation. The exposure age of 12.0 ± 0.8 ka represents a minimum age, as the highly-fractured surface of the failure plane has experienced post-failure mass-wasting. Considering the dates, it appears unlikely that the cirque was re-occupied by a substantial ice mass during the Younger Dryas Stadial.

The 'hypothesis' is a plausible, instructive, challenging, and valuable one. However its 'proof' here rests on a number of important assumptions, which ought to be recognised. Indeed, the entire paper could be recast as testing the converse 'null hypothesis' that

'if the feature in Great Coum is an arrested translational rockslide form of RSF, its emplacement could have occurred at or over any timescale in the later Quaternary and did not require the supportive presence of glacier ice' (cf. the null hypothesis ‘there are no rock glaciers in the British mountains, active or fossil’ tested by Jarman et al 2013).

We are aware that a null hypothesis could have been applied. The suggestion offered by the reviewer is a compound hypothesis largely focussing on the timing of the failure. Our study is not primarily focussed on the timing although timing is an important consideration. The issue of ‘assumptions’ is addressed below.

THE ASSUMPTIONS

1 - the feature is an RSF.

First, let us call this an Anomalous Terrain Feature (ATF), a term even less genetic than Discrete Debris Accumulation (DDA) coined by Brian Whalley. This ATF is now included in the Wilson&Jarman Lake District Inventory as RSF #9.04, but without benefit of site visit, and with some reluctance. It may look like a duck, but does it quack like a duck ?
Evidence for it being an RSF is almost entirely visual (circumstantial). On walking over the site, the only clear positive indicator observed is an unusually fresh, jagged sub-vertical fracture-fissure (see photo). Basal springs, a common indicator of RSF, were not evident at the immediate slope foot (even beneath the dry gully descended), but this was in a very dry spell; OS 25k shows a group of smll watercourses emanating well below the talus zone inside the fenced enclosure, which may or may not be springs.

It is unclear just how much of the site was visited by the reviewer and how much time was spent on site. Evidence for the RSF is not ‘almost entirely visual’ as suggested by the reviewer. As was included in the original submission, as well as the vertical fissure (noted by the reviewer) there is also the evidence of three fault planes providing lateral constraint to the rock slope wedge as well as a bounding basal fault plane down which the wedge descended. The occurrence of pale silt bands in the RSF was carefully matched to the occurrence of the same silts bands in the headwall which is how the distance travelled downslope was derived. The dip of the strata within the RSF as well as within the headwall was measured to derive the small plunge. All this detail was included in our initial submission. Basal springs are present at the location but given the unusual drought we have witnessed this year no water has been issuing from these risings when the reviewer visited the site.

Against this being an RSF :

a. the ‘**tread’** is a prominent bench or shelf, not unusually wide, such as commonly occurs on a cirque headwall, especially at mid-lower levels where curvature eases slope, if resistant bands create rocksteps. Here, this may be a remnant of a wider rockstep. In particular, the shelf fades laterally until buried in talus (especially northwards), whereas an RSF slipmass would have more ‘emergence’ from the slope. It feels solid underfoot.

   As noted in the original submission the silt bands within the RSF match the silt bands within the headwall, so there is no possibility that the rock mass is a shelf. The lateral extent of the tread is partly buried by talus which has accumulated from small rock falls from the headwall. Any large rock mass will feel solid underfoot, so we are unsure what the comment about solid footfall is meant to convey.

b. the ‘**riser’** is actually stepped or tiered, in side profile very different from the rather sheer rim crags it is supposed to match; it is unusual to be able to promenade rather freely across an RSF slipmass on a steep slope, and to descend it easily top-to-foot; it has an air of solidity saying ‘in-situ bedrock’ except for the unusual fracture. In several places, large flakes have toppled from the mini-risers, revealing fresh faces with distinct in-situ feel, whereas in even a quasi-intact slipmass, deeper joint blocks would tend to come away because some internal rupturing and dilation must occur during translation (however firmly ice-buttressed).

The riser unusually is **not stepped** and this is clearly seen from the southerly aspect. A modest degree of stepping occurs close by the fissure that the reviewer states that he descended. The strata are disrupted to a very minor degree and plunge 2^{o} steeper than the headwall strata. The joints may have been introduced by minor dilation but equally these can be primary as was noted in the original manuscript. The limited degree of break-up of the slipped wedge despite the thinness of the beds and the steep failure plane is the first thing we noted as unusual about this RSF.

c. there is no obvious **source configuration** for **slipmass restitution** to match (this is a common oversight with RSF misidentifications): for the ATF to be an RSF requires either a planar source scar or an obtuse wedge-shaped cavity, neither of which can readily be demonstrated as restitutable (the writer here stops short of drawing diagrams showing fall-line trajectory in relation to ATF extent and rim topography, but is happy to consider proposals that would satisfy this key criterion).

We are surprised the reviewer makes this comment seeing he has visited the site. The original manuscript mentions the source area at lines 234-237 but the source area is clearly seen, especially when viewed from across the valley. We state in the original submission that the ‘The RSF caused headwall retreat in the vicinity of the present grassy slope, leaving the intact steep rocky sections of the backwall to either side,’.

d. there are almost no **dislocations** in the ATF, such as would commonly confirm a translated slipmass, especially of this travel distance, and even albeit this is a relatively small body. Typically, the top surface might present a degree of back-tilting, with an upstand edge, whereas this one (even allowing for talus accumulation) rolls over weakly. And the ‘riser’ might display antiscarp development, even on a sub-metric scale, as the emplaced mass dilates and disaggregates slightly (even if here, after ‘debuttressing’ rather than in transit). The one fracture observed (and noted in the paper) is intriguing, but as the rockmass has not come apart materially, it betokens quasi-in situ rock slope deformation (RSD) - possibly a local rebound response - rather than a rockslide.

The lack of major dislocation, as noted above, is the prime reason we suspected a slow descent for the RSF. The top surface (the tread) does locally show back tilting *i.e.*, antiscarp behaviour but some of the tread is covered by scree. The large vertical fissure is aligned with a BGS-mapped fault line that extends back into the headwall and beyond on the plateau above. The faulting in the parent geology might include post-glacial rebound but otherwise predates the Quaternary, as is evident from the references cited in the original submission – "Moseley (1968; 1972) considered the considerable complexity of the regional structure and noted folding, steep discontinuous local faulting, joint patterns". Without information on local rebound we do not wish to enter into speculation as to the role of rebound other than to note the effect of isostatic controls which we had done in the submission at line 729.

e. The ATF is split by a small central gully, rocky at the top becoming a grass chute (as descended), and by a much broader open swathe towards the north flank. It is surprising that there is no obvious differential downslope movement facilitated by these lineaments, in such a long-travel mass, especially if it is held to have rotated laterally - where *en echelon* or mare's tail side-scarps have been noted (eg. Sgurr na lapaich Affric).

   In our original submission, we report differential downslope movement of 2^{o} from field measurement of plunge and this difference was an emergent property of one of our simulations wherein the NW side of the RSF descends further downslope that the SW side of the failure.

The writer generally rates RSFs for inventory purposes as definite-probable-possible, although Wilson and Jarman 2022 do not do this. Here, Great Coum is not 'definite' on present evidence, and from field inspection remains nearer 'possible' than 'probable'.

This comment does not relate to the content of the manuscript.

2 - geological parameters require the ATF to be an RSF
Two features are invoked here:

a. lateral rotation, down-west - as revealed by the dip of the strata across the riser. This does indeed seem convincing, both on imagery and viewed in the field. However, there may a degree of optical illusion, compounding with the difficulty of reading 3-D structures presenting in 2-D. The marked eastward 'dip' of the headwall to the east of the ATF is also reinforced visually by the elevational decline east along the cirque rim. In fact, the ATF dip west looks broadly consistent with the headwall crag above, and with broken crags to the NW. The paper refers to anticlinal structures, which may account for some of this effect.

   We note that the reviewer finds that the plunge of the strata across the RSF is 'convincing'. 'Eyeballing' the plunge of the RSF and the plunge within the headwall and is clearly not adequate to reach a conclusion related to a small angle of plunge. Rather, the reported plunge is <u>not</u> due to inspection of images but due to measurements of plunge in the field. The elevation changes along the top of the cirque are due to regional structural constraints which are not relevant to the issue of local plunge within the RSF.

In any case, it is rather difficult to envisage how a slipmass of this bulk would rotate laterally while strongly ice-buttressed - its natural trajectory would be close to fall-line. Examples of rockslide masses displaying such lateral rotation (as if a 'foundering ship') are hard to recall.

It is not clear why the reviewer comes to this conclusion. The modelling we present indicates that the loading of the ice mass across the riser of the RSF can be variable so there is no issue with the RSF descending further down to the NW than the SE. We did not pursue this issue due to the 'infinite' ice loading possibilities, rather we decided to present a simple case as an example that is convincing. Adding complexity, such as varied ice loadings, goes beyond the capacity of the modelling and more especially goes beyond the constraints of the data that can be derived from field study. Such detail would be seen as 'going too far' in the view of many reviewers and is a step too far in the opinion of the current authors.

b. correlative strata - until the evidence for this is made available as requested above (which might clinch the RSF assumption) it can only be suggested that if these Silurian sand-silt deposits occur in rhythmic sequences, as quite often mentioned in geological literature, then it is possible that a visual impression of correlatable strata might be obtained from seeing such sequences within the headwall and riser that are actually 'pattern repeats'.

We kept the manuscript succinct. As evidence, we reported that pale silt bands found within the headwall are also found in similar position within the RSF and that bed thicknesses within the RSF do not correlate with bed thicknesses in the undisturbed outcrops to either side of the RSF. The level of detail requested by the reviewer goes beyond the focus of the manuscript, which is a slope stability exercise. The complexity of the Silurian succession would be the province of another publication altogether and difficult to complete due to the danger in accessing all the cliff face exposures.

3 - the RSF has a planar source configuration

The paper assumes, for the purpose of Swedge modelling, a simple planar source (the 'basal failure plane' on Fig 4a, or 'head wall' as sample code HW implies). However, the full width of the tread as indicated in the paper extends well beyond the main north-facing crag, westwards below a broad grassy couloir (which the paper at one point hints to be the source), and indeed further west until below a degraded portion of the NE-facing cirque rim. Leaving aside the difficulty of fitting the ATF back to where it came from, inspection from vantage points along the rim down the fall-line suggests some form of obtuse wedge slide configuration. Clearly this has important consequences for modelling, as the slope angle of the wedge axis is less than that of the flanks, which as they converge must impede translation - one reason why so many RSFs are 'arrested'.

The paper makes no assumption about a planar failure plane. As explain in the original main text, the failure plane is exposed at several locations and the plane extends both to the north (as noted by the reviewer) <u>but also the south</u> and the angle of this plane was measured. This information was included in the original manuscript. There is no difficulty in fitting the RSF wedge back into its source location as the pale silt bands allow for this to be done in terms of altitudinal adjustment. The headwall is now much degraded by post-glacial weathering – hence the scree – so a 'perfect' fit into a source alcove is not possible, although such an alcove exists to the NW of the RSF as was stated in the original submission at lines 235- 237.

Here the writer has much pleasure in recommending the unpublished PhD of Graham Holmes (1984)
Holmes, G., 1984. Rock-slope Failure in Parts of the Scottish Highlands. Ph.D. thesis. University
of Edinburgh (available online).
This was undertaken at the behest of Brian Sissons to demonstrate that RSFs associated strongly with his then- LLS limits (both of which proved to be wrong). 'Holmes' is now best known for its pioneering RSF inventory, including 'debris-free scarps' implying an earlier generation since glacially evacuated. His thorough exposition of the basic principles of slope stability in a British montane context is however crystal clear and superbly exemplified with field cases almost at lab-model scale. The effect of different source configurations is instructively set out (and complementary to the 'rock-toppling' PhD of Bob Watters 1972). His engineering geology methods would bear comparison with those adopted for this paper, including his measuring of a hundred joint aspects per site (which should be a thousand for serious projects !) to obtain spherical projections of joint sets available as slide surfaces.

Thank you for these references but to keep our submission focussed we have not extended the text in such a way as to require their inclusion. As detailed in the original submission, our RSF occurred on a fault plane and not along a joint set.

Here at Great Coum, it can be observed that although perhaps not technically 'metamorphic' in the lack of crystallisation, the robust Silurian sediments have undergone sufficient modification to endow them with mutiple angular joint sets in addition to the bedding plane and any orthogonal jointing (see photos). This may also affect the modelling process.

In the original submission, we reported the presence of joints in the RSF and within the source bedrock. However, it is not clear how such jointing might be included in a failure model when the slipped mass appears to be coherent down a failure plane defined by a fault line. In our opinion it is unlikely that a more sophisticated model than the one we present would add deeper insight to the failure mechanisms, indeed such a model would venture into speculation unsupported by data.

4 - the RSF has a planar sliding surface
The paper assumes for Swedge modelling purposes a 'basal failure plane' which is essentially smooth and 2-D, with an allowance for a minor degree of 'waviness' suggested by slickensiding. However, such ideal surfaces are increasingly becoming recognised as the exception, eg. in quartzite lithologies, or where through-going 'fault' discontinuities occur, perhaps lubricated with gouge. The paper alludes to thin partings of finer-grained sediment, which could assist in mobilisation, as do pelitic (micaceous) bands in coarse psammites in the Highlands. But the orientation of such partings would need to coincide closely with the inferred source configuration.

We do not assume a basal planar surface, rather one is exposed locally on which we measured the angle. The limited exposures show this to be smooth. The site may be an exception but that does not mean it lacks interest. Quite the converse, the simplicity of the geometry makes it ideal for investigation and modelling. The pale beds are not thin partings as the reviewer would 'wish', and we make no suggestion that they could assist in mobilization as the observed orientations are not concordant with the failure plane.

Generally though,
either a '**zone of crush**' is more likely to exist, as advocated for a major Lake District site Jarman, D., Wilson, P., 2015a. Anomalous terrain at Dove Crags cirque–Gasgale Gill, English Lake District, interpreted as a large pre-LGM rock slope failure complex. Proc. Yorks. Geol. Soc. 60, 243–257.
Clear evidence for such zones of crush is now being found in borehole investigations of RSFs in Norway, including (spectacularly, rig helicoptered in) through the Mannen RSF, Romsda;
and/or a '**stepped basal configuration**' where a shearing of the rugosities is invoked to generate an effective sliding surface or zone.
Vick Bohme Rouyet Corner in Landslides 2020 depict a number of N Norway RSDs, with long-sections identifying both processes as proven by drilling.
Quite what impact these realities might have on the modelling outcome here is unclear, but it could potentially go either way. (The paper describes the slipmass as presenting 'friable bedrock' as if shaly, but evidence for this - extent, depth, mechanical strength, photos - is not provided. The bedrock exposures seen in touring the rim, the headwall with its coarse debris runs, and the ATF instead rather impressed with the general robustness and coherence of the bedrock.)

The information presented here by the reviewer is very interesting but not relevant to the focus of the manuscript. As noted above, the exposed portions of the basal failure plane indicate failure along a pre-existing fault line. Other than a small amount of post-glacial spalling on the failure plane surface, there is no field evidence of a major crush zone and one would not be expected given the thinness of the failed rock wedge. We do not report the details of the mechanical strength of the Silurian beds, as this information is not required for our modelling framework. Rather we report that the frequency of joints and the presence of cleavage as this would lead one to expect an unsupported RSF to break up during descent. The level of information already supplied is sufficient for the purposes of the project. The headwall is heavily fractured and incompetent, as is witnessed from viewing the headwalls from below and as is evident in extensive scree development.

4 - the RSF is a wedge tapering to a pointed toe
This assumption is central to the argument, implying that without restraining ice, such a wedge would coast downslope more freely than a blunter object - possibly even disintegrating into a rock avalanche.
The null hypothesis states that the putative RSF slipmass is a more conventional rectilinear (cuboidal) slab that has detached from the headwall on a weakness broadly parallel to its pre-failure surface as exposed in the rim. Such a slab would be impeded in translation by reducing headwall slope angle and by its blunt toe ploughing into the mid-lower slope deposits (till, talus, friable weathered bedrock).

To demonstrate that this is indeed a wedge toe would require geotechnical investigations, whether invasive or eg. GPR (see the work of Tim Davies at Clough Head, QRA Guide 2015). Viewed side-on though, the impression is of a solid rectilinear rockmass, which could either be a solid outcrop, or a rockslide mass ploughing into or progressively buried by talus etc.

The wedge-shape is not an assumption. It is evident that the reviewer did not visit the SE side of the RSF where the wedge shape is clearly evident. There is no reason for the reviewer to state that a conventional failure is cuboidal, rather the literature shows all kinds of shapes for RSFs. There is no talus at the foot of the wedge which could have buried the toe.

5 - the RSF occurred entirely during the LGM and its deglaciation

The null hypothesis states that the putative RSF could have initiated earlier in the Pleistocene, migrating incrementally to its present position, with or without ice support, as all other factors interacted. For example, Great Coum might have been substantially excavated during the earlier cycles of cirque glaciation, and then tended to be dormant when suppressed beneath the great icesheet glaciations. Given its low elevation and contraposed iceflow directions, continuing cirque glacier erosion of the headwall might then have been rather limited, allowing an RSF to evolve over multiple cycles. In such scenarios, the whole issue of ice-supported translation becomes ever more complex and perhaps more of a secondary factor.
As an aside here, the writer once speculated that RSF could be initiated by the load of an icesheet several hundred metres thick (above summits) bearing on a fallible rim, thus a 'snap-off' like a boxcutter blade; happily this never appeared in print. Even so, it is an interesting thought experiment, when considering the rim of Great Coum and the ATF. The writer subsequently doodled a cartoon of 'all the forces acting on a mountain slope, including during icesheet glaciation and after' and hawked it around a score of experts home and abroad, meeting with no dismissal, indeed positive interest, but with no-one volunteering to resource a simulation to put some numbers on the vectors. This was in pursuit of the contention that RSF is primarily a rebound-driven response to Concentrated Erosion of Bedrock (see overview papers referenced). Here, excavation of Great Coum would generate rebound stresses in the footslope developing into fracture systems migrating up to the rim. But this usually envisages unusually rapid and recent cirque enlargement... unless the RSF is a slow-burner.

We do not suggest that the RSF could <u>not</u> have been initiated earlier within the Pleistocene. Such scenarios are legion, rather the thinness of the intact strata within the thin wedge leads the authors to conclude the RSF would not survive repeated cycles of glaciation and down slope motion. Rather we adopt Occam's Razor and conclude that the most likely scenario is a late glacial slow descent into the base of the cirque. Rebound stresses may well have led to the release of the wedge of rock, but it is bounded on all sides by faults which are pre-Quaternary (see references cited in the original submission).

COMMENTARY 2 - GREAT COUM ANOMALIES

The paper implicitly treats Great Coum as if it were a typical, representative Lake District RSF, from which conclusions of wider relevance for timing and failure mechanisms could be drawn.

We must take exception to this remark. Nowhere within the manuscript is it made explicit or implicit that the site is typical and representative of Lake District RSFs. We would not wish to make such an argument. This erroneous assumption colours the comments of the reviewer below.

This assumes that the putative RSF is not an anomalous 'outlier', to which we now turn.

1 - anomalous cirque in location and elevation

Great and Little Coum stand out on map and DEM as cirquefoms in a tract of intermediate hills where cirques are generally absent. Ian Evans has two marginal cirques up Borrowdale nearby, as well as these. That's all, westwards, until the great cirques of the High Street range. Eastwards, the Howgill Fells - attaining a markedly higher prevailing elevation of 500-600m asl - entirely lack cirques except for the remarkably well developed Cautley locus. The question must thus arise as to whether these are typical cirques, and why they might have originated at such a low elevation - even Cautley spans 650>200 m asl whereas Great Coum spans 470>150 m. One possibility is that they have originated not as conventional cirques but from a trough-flank scallop as well seen nearby in Bannisdale and the upper Shap Fells Borrowdale, where bold arcuate escarpments on their mid-SW flanks ar conspicuous on imagery. These little-studied 'nivation scars' (for want of knowing a more correct term - 'bananas' is suggested by a colleague) might suggest a transition from cold-based to warm-based ice coming off the low plateau and funnelling into the Kent-Lune discharge zone (compare Tweedsmuir and Dalveen, S Uplands). The Coums together have a comparable NE aspect, albeit their setting is at the low end of the Lune gorge. They could have initiated in this way, with segregation into clearer cirque forms for local reasons.

The origin of the cirque forms is not germane to the focus of the manuscript. The two hillside hollows have been recognized as cirques by many authors (Marr and Fearnsides, 1909; Moulson, 1966; King, 1976; Barr et al., 2017; Clark et al., 2018) and included in the BRITICE compendium. Harvey (2017) speculated that Great and Little Coums were only nivation hollows developed late in the Devensian, but these features are likely too large to have been produced by snowpatch processes and qualify as Type 3 or 4 cirques in the Evans and Cox (1995) classification.

2 - anomalous RSF in zone of sparsity and in low elevation

The Wilson & Jarman inventory depicts a broad lacuna in RSF incidence between the heads of Longsleddale/Haweswater and the eastern Howgill Fells, with Great Coum an isolated exception. This is despite comparable 'available relief' in these valleys including the Shap Fells Borrowdale.

The source elevation of Great Coum RSF at 460 m asl is also relatively low. Although there are 13 cases of lesser elevation, three are compact rock avalanches from lower-level crags in Borrowdale near Derwent Water, three are on peripheral escarpments rather than in glaciated troughs and cirques, and five are in parafluvial contexts (see below). This leaves for realistic comparison at such lower levels a small arrested rockslide on the Crummock rim of Mellbreak, and the remarkable Helm Crag RSF in the Grasmere trough, which is an RSD.

This statement supports the supposition that there well might well be a RSF in Great Coum. There is no issue to be addressed within the manuscript with reference to the referee's statement here.

3 - anomalous RSF-in-cirque, in a Lake District context
There are 11 RSFs in cirque contexts in the Lake District inventory, of which interestingly only two are disintegrated rock avalanches - one within the oddly capacious and isolated Dead Crags cirque, Bakestall, north of Skiddaw, the other being the large and idiosyncratic Burtness Comb complex (see paper in submission by Wilson et al with cos modates confirming the lower deposit as post-LGM and pre-LLS). Of the three slope deformations (RSDs), two are trivial and one on the eastern spur of High Street (Caspel Gate) is a larger RSD of 0.09 km2 on the Blea Water cirque flank as it extends into trough-head character. Of the five arrested translational rockslides within cirques that might compare with the Great Coum case, four are very small (0.01-0.03 km2) rimslips or 'rim nibbles' with high-level sources at 725-750 m asl, lowered by a few metres (Caudale Moor N, High Street NE, Black Sails, High Crag). #8.01 Eller Peatpot on Black Combe is a slightly larger site of 0.05 km2 which has a definite wedge lowered by ~10 m possibly nested within an earlier lowered berm and perhaps with a slide lobe into the cirque floor, subdued by cirque-glacial overriding. It would repay investigation as falling between Burtness Comb and Great Coum in possible evolution.
In the Scottish Highlands, about 10-15% of RSFs are in cirque contexts, but a tabulation prepared in 2012 regrettably does not identify type or headscarp heights. A brief search of possibly comparable areas such as Cowal does not yield comparable small long-travel intact rockslide slices from planar sources but doubtless they must exist. Generally, headscarps in the 50-100 m+ height range are rare and associate with larger RSFs.

This information is interesting but does not reflect on any statements we have made in the manuscript.

4 - an anomalous context for an anomalous cirque and RSF

Here, the coincidence between an anomalously isolated and low-level cirque, an anomalously isolated and atypical RSF, and the remarkable major landscape feature of the Lune gorge becomes compelling.

It is not clear from the reviewer's text developed above why the reviewer should consider the RSF in Great Coum to be anomalous. From the text above supplied by the reviewer it seems there are a variety of rock slope failure types across a spectrum of altitudes and environmental conditions.

The writer regrets not being *au fait* with the longer-term evolution of the Lune gorge, but would guess that it is not a glacial breach, but perhaps an antecedent fluvial incision responding to uplift along the Lakes-Howgills axis3. If so, it will have undergone adaptation to accommodate ice discharge across this axis whenever the local ice divide was displaced northwards or southwards. As the paper states, there is now a U-profile with some shaving of the former interlocking spurs, although glacial trough development is still immature, hardly more so than its modest Borrowdale tributary. Indeed, at the south end it retains a more fluvial (V-form) character, perhaps due to diffluence through the Dillicar gap - see photos.

The issues raised here are not directly related to the content of the manuscript. For the reviewer's information all these points have been addressed in Carling et al., 2023, *Proceedings of the Geologists' Association*, 134, 139-165.

3 this detail is not mentioned by Rob Westaway (2009) in his advocacy of uplift of this axis since the mid-Pliocene
The unusual character of the Lune gorge has echoes along the Highland Boundary Fault, where a sequence of main valleys exiting the uplands across it are likewise not true breaches but have rather irregular, immaturely glaciated courses - and in several cases have clusters of RSFs upstream from them; also see the steep southern side of the S Uplands, where its small RSF clusters occur.
It is thus possible that the continuing V>U conversion of the Lune gorge has been accompanied by RSF, within the classic glacial-paraglacial cycle - for which see
Jarman D (2009): Paraglacial rock slope failure as an agent of glacial trough widening. In Knight, J. and Harrison, S. (eds). Periglacial and paraglacial processes and environments. Geological Society of London Special Publication 320, 103-131. doi:10.1144/SP320.8.

Once again, this comment does not relate to the content of the manuscript. However, we agree that it is possible that the RSFs may play a role in widening of glacial troughs. However, the Great Coum RSF is the only recognized RSF in the Lune Gorge, such that such failures do not seem to be important in the local context. We do not wish to extend the manuscript into realms of speculation for which we do not have data in support.

If additionally, the foot of the gorge has seen cyclical glacier advance, retreat and fluctuation, then repeated stressing and destressing of the valley sides will have occurred. RSF does seem to occur at such loci, as noted in the Eastern Pyrenees at trough-head transitions (TH-T), or here, at a transition from upland to lowland. Furthermore, the potential for RSF cavities to seed cirques has been noted since Clough (1896) and was recently explored by Ballantyne (2013).

It should be evident from the existing manuscript that the 'particular' geometry of faults within the vicinity of the Great Coum headwall have conditioned the actual geometry of the RSF in Great Coum. We do not wish to draw any inference that the RSF in Great Coum is a major influence, or otherwise, on the development of that cirque. Nor, having only one example, would we wish to suggest that RSF are important in the development of cirques more widely.

Although the paper focusses on the ATF, dismissing other possible RSF indications around the GC-LC cirques as minor and not relevant, field inspection suggests that RSF may have been important in their evolution (see photos) :

-- the outer (eastern) corner of Great Coum has a distinct berm below the rim with a slightly protruding slope below, suggesting a short-travel rockslide, possibly ice-modified; it could have descended 30 m - assuming it is an RSF, of about 0.01 km2;
-- along the rim above and west of this berm there are several minor steps and grooves suggestive of incipient rim failure (but not above the ATF - and note that the intricate dissection around Grayrigg Pike is not RSD but probably selective erosion by glacial meltwater outbreaks - see also the remarkable grooved terrain NE of the telecom mast);

We are aware of these additional features which is why we make passing reference to these very minor mass failures in the manuscript. The grooving is structurally controlled, and we do not speculate on the origin as it has nothing to do with the focus of the manuscript. Other than indicating that other very minor rock falls and RSFs have occurred in Great Coum to either side of the RSF we have modelled, the information regarding these features is not relevant to the modelling and interpretation within our manuscript.

-- the north-facing head of Little Coum is an anomalous planar slope with fatly swelling terrain below, hinting at a broad rockslide (slab-slide) with both source and debris ice-smoothed since; the rim has little wedge cavities in its angle;
-- the short bold NE-facing crag in Little Coum looks like an RSF cavity of the debris-free type espoused by Ballantyne; there are very large angular blocks possibly of this origin on the outer apron;
-- the outer rim of Little Coum has recesses and lineaments suggestive of sub-RSF dislocation and slippage, perhaps ice-smoothed
-- the apron is elevated above that of Great Coum, and the pronounced step-down between them is marked by a curious linear scarp and berm, seemingly in bedrock; it could result from selective stripping by ice along conducive joints, or just possibly could be failed, thus a headscarp and berm to a lowered slice.
-- around the corner into Borrowdale there is a distinct cavity and slipmass, if sub-RSF in scale.

We are aware already of these details which are not relevant to the RSF in Great Coum.

All this might suggest that the GC-LC compound cirque could have originated not in the conventional way (whatever that is, a matter perhaps rather glossed over, but presumably exploiting fluvial valley heads with conducive aspect and concavity), but from one or two significant cavities created by proto-RSFs at this focus of CEB and slope stressing.

The origin of the two cirques is not the focus of the manuscript. Little Coum is certainly structurally controlled.

COMMENTARY 3 - FURTHER ISSUES
If this - already admirable and thought-provoking - paper is to have wider value and applicability to sites beyond that studied, some further issues arise.

Thank you for noting that the manuscript is admirable and thought provoking. We only wish to promote further research and constructive argument as to the mechanisms and the timing of RSF.

1 - RSF modes and contexts
Firstly, it is stated that "the slope would have failed catastrophically, if not supported by glacial ice" (30-31). A corollary might be that all the cataclasmic RSFs in the Lake District were unsupported by ice, and thus postdate final local deglaciation, which is not unreasonable. This only deals with the minority that are cataclasmic - 15 of 84 : conversely it could be taken to imply that the 27 translational rockslides would have collapsed without ice support. Of course their cavities, failure surfaces, and geology would all differ, but it would be useful to know just how sensitive Swedge - or other analytical techniques - might be to such parameters.

The reviewer is attempting to extend our findings more widely to apply to many RSFs for which we do not have data to consider their failure modes. The purpose of the manuscript is to promote such considerations, but we purposefully make no claims that our model can be applied elsewhere. However, we do encourage others to apply numerical stability models where this is possible.

Then there is the neglected fact that a significant minority of RSFs occur in non-glaciated valleys - almost 10% in the Lake District, and a majority in mid-Wales and most of the Southern Uplands (and of course the totality in non-glaciated ranges abroad). Here, they are typically in fluvial (V-form) side-valleys envisaged to have undergone rapid incision or deepening by peak meltwater discharges during deglaciation, with consequent slope destabilisation. They are thus termed fluvial RSFs, or 'parafluvial' if they are not responding directly to ordinary fluvial erosion at the slope foot, but are on fluvially steepened slopes. Of the nine Lake District cases, most are translational rockslides, little different in size range and form from their paraglacial counterparts. As they cannot have been supported by glacier ice, and despite some being lowered well down the slope, some other process for initiating and then arresting them must be found (one practical civil engineer simply responded 'they dried up') - and if this applies to all parafluvial RSFs, it will doubtless apply to some paraglacial ones.

We acknowledge that there are RSFs which are 'non-glacial'. Our manuscript does not address these.

2 - scalability
The 'Holmes model' of a quartzite block on a smooth tilted joint plane can readily be envisaged, both at its Peak Friction Angle while joint-cemented and at its Residual Friction Angle while unrestrained - and it can be seen at a scale of say 100x100x10 m (thus just qualifying as an RSF) to have moved quasi-intact in places as diverse as Glen Dessary, Glen Quoich, and Jura.4
4 Indeed a scale model of that has been played with, on slabs at differerent inclinations, to the writer's great satisfaction if sore arms. And demonstrated to a student on the slopes of Ben Vorlich, with a convenient slab at the 'tipping point' where an applied fingertip obtained translationshe was mapping the RSFs in her study area with no idea that they could have moved on any combination of joint sets, not just the foliation surface - and went on to join a firm of engineering consultants.

However, if the Holmes block is scaled up progressively, towards RSFs of average size (~0.20 km2) and beyond, it can be imagined as becoming both ever stickier, as the ideal sliding surface becomes 'noisier', and ever less able to remain quasi-intact, as its internal inhomogeneities proliferate and respond to the stresses of gravity and translation and so forth. This is why, we envisage, long-travelled quasi-intact RSFs are rare.

We appreciate that there have been other attempts to model RSFs making various generalized assumptions. Our model applies specifically to a pentahedral wedge of rock. The key point is that despite examining thousands of potential RSF geometries that are plausible within Great Coum, the result remains the same: the wedge cannot have been stable and would descend rapidly if not supported.

And that is without considering glacier ice support. Here, the downwasting of the ice cannot be expected to be smooth and uniform, nor can the slipmass be assumed to remain so closely in contact as to glide down the failure plane. They must surely play catch-up, with phases of ice wasting and stillstand, and of slipmass pausing and remobilising. Each cycle must expose the slipmass to internal and external stresses rendering it more liable to sticking fast - or vulnerable to progressive or calamitous disintegration. And the larger the slipmass, the more prone.

We acknowledge the lengthy time period associated with deglaciation and that the RSF may have descended in stop:start motion within ice over an unknown time period. We address the controls on such motion in the existing manuscript from lines 544 onwards whereby we consider in principle changing the ice load distribution, the presence of a bergschrund and lubrication; all of which are related to deglaciation. We also note the effects of weathering and brittle fracture that can lead to the RSF being stable or liable to downward motion. An extended period for lowering the rock wedge within an ice mass would likely lead to disintegration of the wedge which has not happened.

Thus, while this paper offers a fascinating analysis under near-ideal conditions at field-experiment scale, it must be wondered if the glacier-ice support scenario can usefully be scaled up to the generality of RSF.

Here the debuttressing argument comes to the fore. From conversations with both Tim Davies and Sam McColl, the writer sees the logic of their reasoning - developed of course in respect of very large RSDs in alpine New Zealand troughs - that glaciers are plastic and thus also deformable, and that a failed slope can sag extensively even while in contact with the glacier, and remain in that metastable position after ice withdrawal. The writer has not refreshed and updated the course of this debate, but recalls that this 'debunking of debuttressing' may have been rowed back from to some degree.

Its relevance here, in considering scalability, is that it seems intuitively more reasonable for a large glacier to hold a small RSF in place, controlling its translation, than for a waning glacier to restrain and control a large RSF. If so, the less scalable are the findings of this paper, and the more likely it is that medium and larger RSFs have not been ice supported, and could thus be synchronous with or later than final deglaciation.

We make no claim that the ice-supported scenario can be scaled up. This is just the reviewer's own thought process, and we agree that many large RSFs were likely not ice supported. It is our belief that our manuscript is a useful contribution to the issue of the mechanics of RSF with and without the presence of ice. We have demonstrated that one specific RSF had to have been ice supported. We do not claim any scaling up to larger failures, nor across a geographic region. It is clearly not going to be possible to make more sweeping generalizations until many more stability analyses have been published for a range of types of failure. It is for this reason that we have refrained from making claims about our findings that cannot be sustained at this time.

3 - the single date issue

Recourse to the journal website and its record of 'submission-in-progress' (a novelty for this writer) indicates that the single-date issue has led to a previous rejection and has already been taken up by the Associate Editor as requiring greater circumspection. The Supplementary files address this issue, but add little to the simple fact that 'dates cost money' especially when much bulkier samples are required to obtain datable material than with the usual quartz-knob method (the writer has assisted CK Ballantyne in homing in on such quartz-knobs, and has also sampled Dartmoor tors for conveniently back-packable flake sizes - but RSFs are rarely in quartz-rich granite alas).

The writer has discussed with Derek Fabel the vexed question of number of samples required for reliable dating. He suggested, ideally, collecting ten, processing the first five, and if statistically consistent, calling a halt (if not, carry on). Given the cost, he accepted that four or even three might give a close-enough approximation to the actual age. Ballantyne has published a set of rock avalanche ages for the Highlands and Ireland based on three dates per site, with the protocol that if two are close and one an outlier, that can be rejected (IS Evans confirms that this is statistically invalid, as the single date could be 'more right' than the pair).

Here, the author admits that a single date is not really adequate to give a reliable age, and that the headscarp date may be little more than a limiting age. However the writer has further concerns, which would persist even with (say) three dates (and even once the precise locations of the two single samples were provided and inspected) :

We welcome the opportunity here to explain the issue of limited sampling. It would not be appropriate to add this additional text to a manuscript. We are fully aware of the requirements for reliable terrestrial cosmogenic dating. Note that two of the authors are specialists within this field from a dedicated cosmogenic laboratory at the University of Aarhus. The RSF lithology is Silurian muddy fine-grained sandstone. It yields sufficient quartz to obtain a reliable date but only after a large lab effort. The cleaning required to process just two samples was an excessive number of days and no grant giving body would consider the funding that would be required to cost the time involved in multiple sample preparation. Given that we anticipated this problem we obtained two samples: one from the outer side (the riser) and one from the failure plane behind the RSF. Thus, at a minimum we have a date for the first exposure of the riser to cosmic rays and similarly for the first exposure of the failure plane. As reported within the manuscript, these two samples provided dates that are 'sensible' and interpretable within defined uncertainty ranges. The sample taken from the riser is from an (ice?)-smoothed surface. In principle it would have been possible to take more samples across the face of the RSF, but experience of similar sampling elsewhere indicates that applying the same procedures would result in very similar closely aligned dates from several samples. The failure plane, as we note in the manuscript, is losing material due to post-glacial weathering and there are no other sites that we considered suitable for sampling. Sampling the headwall behind the RSF is not possible as it is heavily weathered and has been subject to post-glacial spalling, as the screes witness. Given that the date for the first exposure of the outer face of the RSF is consistent with multiple cosmogenic dates for erratic granite boulders nearby (as stated in the manuscript and reference provided), it seems sensible to place this date on the record by publication, with suitable caveats being given within the Supplementary Information. The exposure age for the failure plane is younger than expected if the plane had not been subject to spalling. However, as frequent spalling is evident it is reasonable that the date for the failure plane is younger than that of the riser and this too is placed on the record. Overall, despite the uncertainty related to having just two dates, the temporal consistency is a spur to using cosmogenic dating of the risers and failure planes of other RSFs to obtaining the timings of these events for which, overall, we have poor understanding of the time controls.

-- the source area is not a simple, visible rock scar of homogenous form or character. As discussed above, it is unclear whence the putative slipmass has come, but several sampling points across the 100-200 m width of bold crag - grassy bay (does it possess outcrops ?) - weaker crag would be desirable;

The issue of the source of the RSF has been addressed above.

-- the 'riser' is likewise a staircase of treads and risers, of varying boldness laterally and vertically, which should be sampled to reflect average conditions;

The riser is not a series of steps but in the vicinity of sampling it is a smoothed surface so multiple sampling is not helpful.

-- there has been considerable wastage along these mini-risers, as mentioned, and it may be difficult to judge what facet(s) truly represent the unmodified post-ice surface, thus several sampling points are needed.

An ice?-smoothed surface was sampled which has not been subject to significant post-ice modification.

Ideally, it would be intriguing to sample vertically down both source scar and riser, to see if they 'young' downwards with progressive slipmass displacement and ice surface lowering;

Given the cost involved in sample preparation for this specific lithology, a case for funds from any grant giving body would be difficult to make.

5 this was proposed by the writer for the Dartmoor tor dating project, where a tor had been quarried into, thus revealing a profile through the former ground surface and on to a depth below cosmo-penetration; the principle was accepted...
the thick scatter of coarse angular blocks on the riser could be dated, to give an earliest age for its separation from the rim to reveal the extant crags.

Due to post-glacial weathering of the headwall Holocene dates would be obtained which are not relevant to the study.

Possibly Schmidt hammer dating would give affordable insights into some of these concerns, as conducted for the Burtness Comb UD-LD by Peter Wilson with some success.

We have used a Schmidt hammer in other studies and in other contexts. We considered and dismissed Schmidt hammer relative dating due to the friability of the bedrock and the lack of a range of cosmogenic dates to anchor the Schmidt hammer readings.

In this light, the two dates currently available deserve mention as broadly supportive, with all due caveats, and as advancing the case for more systematic dating of this site and others to compare or contrast.

Thank you for noting that our two dates should be put on record and publicly available within a publication. We have already included caveats to our two dates within the existing manuscript.