Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

8th Sep 20

Dear Professor Puzrin,

First of all, please allow me to apologise for the delay in sending a decision on your manuscript titled "The delay mechanism of the Dyatlov Pass avalanche". It has now been seen by two reviewers, and I include their comments at the end of/attached to this message. (We note that reviewer 2 mentions an annotated manuscript that we have not received, we have contacted with the reviewer, and will send that as soon as we have it). They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Specifically, please quantify the uncertainties and caveats inherent in your analysis, ideally with a numerical model to probe the sensitivity of your analytical models to assumptions. (If that's not possible, an in-depth discussion may be sufficient). Please highlight all changes in the manuscript text file.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Heike Langenberg, PhD

Chief Editor
Communications Earth and Environment

On Twitter: @CommsEarth

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

Editorial Policy: Policy requirements

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

In the event that your manuscript is accepted, we will provide detailed guidance on our journal policies and formatting. You may however wish to ensure that the manuscript broadly complies with our house style at this stage. See our style and formatting guide Communications Earth & Environment formatting guide for reference. The main points are as follows:

* ABSTRACT: less than 150 words and accessible. It should include the background and context of the work, the phrase ‘Here we’ (present/show/suggest) to indicate where the description of your own work starts, and then the methods/data, main results and conclusions of the paper.

*The text must be split into:
- INTRODUCTION (<1000 words): includes the background and rationale for the work. The final paragraph should be a brief summary of the main results and conclusions. The results of the current

study should only be discussed in this final paragraph.
- RESULTS: split into subheaded sections; ensure that the subheadings are no longer than 60 characters including spaces.
- DISCUSSION: without subheadings.
- METHODS: split into subheaded sections; ensure that the subheadings are no longer than 60 characters including spaces

* SUPPLEMENTARY INFORMATION should be organised logically, with all items labelled as one of the following item types, and cited in the main article:
- Supplementary Figures, labelled and referred to as i.e. Supplementary Figure 1 throughout both the Supplementary Information and the main text
- Supplementary Tables, labelled as above
- Supplementary Notes, labelled as above
- Supplementary Discussion
- Supplementary Methods
- Supplementary References

Data: All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, and a list of examples, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.
In addition, Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).
In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate
- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Summary:
The authors investigate an unsolved accident from the last century and provide physical explanations to attribute this accident to an avalanche. They show that under certain conditions the progressive snow loading by wind could have caused the release of a small slab, hours after the digging of a platform for a tent, just above the victims which were severely but to lethally injured. More generally, this case study is used to support the development of new analytical (coupling of loading and sintering) and numerical methods (Material Point Method) in the field of avalanche.

Major comments:
The paper is almost presented as a police investigation on an intriguing and unsolved mystery, the Dyatlov Pass incident, and provides new insights in the field of avalanches. It makes the paper exciting but still with sound science. To my knowledge, the main novelty in terms of methodology is 1) to directly couple the progressive loading of wind-transported to snow on a slab and the progressive strengthening/stiffening of the transported snow, 2) to try to quantify the injuries caused by an avalanche with a numerical model. In addition, it provides a new recommendation to mountaineers in terms of wind protection measure. The results are reproducible and will impact the snow and avalanche community.

However (see details below), I have some concern on the presentation that may mislead the reader or is difficult to follow and I would encourage the author to counter-check their complex (and so prone to error) analytical model with a numerical model as the one provided in the last part of their paper.

1) The word "delayed release" is used throughout the text. I did not catch at the first read what was the origin of this delay. I know the recent work if Puzrin et al. (2019) where the delayed release of avalanche is due to rate-dependent processes. In the latter work, the "delay" is used the time between the trigger, the earth-quake, and the avalanche release. In the present work, the delay is between the time of digging the platform for the tent and the avalanche release. The triggering element is the additional load on top, so it becomes unclear. In the end, the question is "simply" what additional load (that also sinters) do you need to reach the critical stress in the weak layer on the wall to initiate failure in the weak layer. Is that correct? There is an unavoidable layer of

complexity to answer this question (stress distribution in the slab, strengthening effect, complex geometry of the slab, etc.) but I think that setting the initial problem with simple and clear words would benefit to the paper.

2) The analytical model is quite complex and it is difficult to follow and to detect potential errors. Sincerely, I could not check each step of te development. It appears sound to me but I would gain confidence in the model if it were evaluated with a numerical model (simple FEM as commonly used for avalanche release mechanics) or with the MPM model developed by the authors.

3) To my knowledge, direct evaluation of the impact of a snow avalanche on a human body is very new and the combination with results for the automotive industry very interesting. The used of MPM for snow was already evaluated and has been proven to be relevant. My concern is more about the MPM parameters used for the human body. I understand that the human body is simulated as an homogeneous hyperelastic material. Is this type of hyper-elastic well-simulated by MPM ?

P. Hagenmuller

Reviewer #2 (Remarks to the Author):

Please see the attached PDF file for comments.

Referee's Report on Manuscript COMMSENV-20-0295-T

J. Gaume and A. M. Puzrin

The delay mechanism of the Dyatlov Pass avalanche

General considerations

This is an interesting manuscript that may attract the attention of readers of *Nature Communications Earth and Environment* beyond the narrow field of snow and avalanche science because it cleverly combines the "Who done it?" of a detective story with advanced computational tools, some of which are of interest in a wide range of scientific disciplines. The manuscript is generally well written. While the Introduction, Results and Discussion sections are quite concise, the Methods sections is almost as long as the rest of the paper because the authors describe a large part of the derivation of the analytic model for avalanche release. Additional details on this model are given in the Supplementary Materials.

The authors do not claim to have solved the mystery of this case, but their results undoubtedly lend strong support to the avalanche hypothesis. Since the Dyatlov Pass accident is the hub of the paper, I think it is unavoidable that the readers will ask themselves questions like, Is there a plausible explanation why the garments of the victims were radioactive? Why did the expedition members flee in two different directions? Why would experienced skiers and mountaineers enter a steep ravine when fleeing from an avalanche on less steep terrain? These questions that are not directly relevant to the main scientific problems discussed in the paper, but nevertheless have a bearing on how plausible one will consider the avalanche hypothesis. Therefore I feel that the authors ought to address them at least briefly.

When reading the manuscript for the first time, my immediate reaction was that there is some mismatch between the advanced numerical modeling of the impact of snow blocks on humans on the one hand and the simplifying assumptions that had to be made to achieve an analytically solvable model for explaining the possibility of an avalanche release and its timing on the other hand. Based on the fall height, the weight and hardness of the avalanching snow, I could have accepted the statement that some expedition members suffered medium to severe but non-lethal thoracic injury without an MPM simulation. However, I do not wish to hold this imbalance against the paper because the chosen approach demonstrates the potential of advanced numerical methods convincingly. One might, however, ask why the authors did not attempt to simulate the build-up of snow drift behind the parapet numerically (in 3D) to investigate how much the simplifying assumption s in the analytical model influence the results. I am fully aware, however, that such a simulation would open several new cans of worms and would exceed the scope of a Communications paper.

Major comments

The authors claim to present a new physical mechanism for avalanche release. In my opinion, they misstate their accomplishment: The physical mechanism they exploit, namely fracture induced by an increasing load eventually exceeding the strength of the weak layer has been known both by avalanche researchers and practitioners for almost a century (the often cited statement attributed to Coaz, "Wind is the architect of snow avalanches", dates form the 19^{th} century). What is novel (to the best of my knowledge) is the authors' quantification of the process, paying attention to the stress distribution along the weak layer. This is a fine achievement and does not need an exaggerated or imprecise "advertisement".

The chief advantage of analytically solvable simplified models is the more precise insight they offer when it comes to the importance of different variables or mechanisms. The price one usually has to pay are simplifications that are unrealistic or leave important aspects of reality aside. In the present case, the problem is reduced to a two-dimensional setting whereas it clearly is three-dimensional, given the finite width of the parapet and tent. The snow deposit upstream of the parapet will not be an infinitely wide wedge as in the authors' simplification, but more like a pyramid, becoming narrower with increasing distance from the parapet. This is expected to have several important effects: (i) The extra load from the deposited snow does increase more slowly with *lw* than stipulated in the paper. (ii) Even if the wind is transporting a constant flux of snow particles, the deposition rate Q will decrease as soon as the length of the deposit is of the order of the parapet height. (iii) Due to the finite width of the deposit, the shear and tensile strength of the snow slab along its perimeter will play a significant role in determining whether a release is possible and when it will occur. Including such effects is likely to prevent a closed-form solution, so I do not object to the authors' choice, but the consequences need to be discussed because they directly bear on the central conclusions of the paper.

The authors quote a value of $Q \sim 0.01$ kg m^{-1} s^{-1} from the literature and use this value in the rest of the paper. As Eq. (3) shows, the delay of the release is inversely proportional to Q, thus the value of Q is central in the argument that the calculated delay agrees with the forensic estimate. Figure 10 of Ref. 15 cited by the authors shows that measurements of Q in the wind-speed range 4–10 m s^{-1} indeed vary by about three orders of magnitude. Even if one attributes much of this variation to the differences in methodology between the data sources, it is unlikely in my opinion that the variation in Q Is less than an order of magnitude. As far as I can see, this uncertainty makes the agreement between the computed and forensically inferred delay times coincidental. This renders the authors' conclusion much less compelling than it is presented in the manuscript and requires a critical discussion in the paper.

A quantity that plays a secondary yet still rather important role in the authors' argumentation is the sintering time scale Δt_s (the term "sintering rate" used by the authors is misleading because a rate is generally the change of a quantity per unit time). It determines how much of the snow drift has coalesced with the original snow slab and contributes to limiting the shear in the weak layer; in contrast, the not yet sintered deposit only contributes with an extra load, but no extra strength. This difference should be explained more clearly in the Methods section. This distinction between the sintered part of the snow drift from the parapet to the sintering front at $x = l_w - \Delta l_w$ and the loose snow drift further uphill is rather artificial because the snow does not typically accumulate as assumed in the model. This is acceptable for the sake of an analytically solvable model, but needs to be explained.

Minor comments

The English is generally good, but there are some issues that the authors need to correct, e.g., in several places a definite article is missing. I annotated such language errors in the PDF file whenever I caught them, but there might be more.

Since this journal apparently does not provide in-depth copy editing, the authors should pay attention to general typographical standards. For example, in a number of formulas, the units either are not SI-compliant ("sec" instead of "s") or are typeset in italics where they should be in upright type. Conversely, at some places variables mentioned in the text are typed upright where they should be in italics. The letter K in all its variants (capital or small, with or without index) seems to be the authors' favorite letter. While the different meanings are almost always matched to specific typographic features, it would help the reader a lot if different letters were used unless convention dictates the use of K (e.g., for the earth-pressure coefficient at rest, K_0 or K_o).

The bibliography is in better shape than in the majority of papers I have reviewed, yet it needs the authors' attention because page numbers are missing in some cases or little known publishers cannot easily be found. Many readers would certainly appreciate if DOIs were provided throughout.

The many references to documents published only in Russian present a particular problem: In my opinion, transcriptions to Latin letters should be provided for author names, titles and journals/publishers/newspaper names.

Please see the annotated manuscript for further comments.

Manuscript COMMSENV-20-0295-T
J. Gaume and A. M. Puzrin
The delay mechanism of the Dyatlov Pass avalanche

Authors response to reviewer comments

Reviewer #1 comments

The authors would like to thank the referee for insightful comments which helped us to improve our paper.

Comment

1) The word "delayed release" is used throughout the text. I did not catch at the first read what was the origin of this delay. I know the recent work if Puzrin et al. (2019) where the delayed release of avalanche is due to rate-dependent processes. In the latter work, the "delay" is used the time between the trigger, the earth-quake, and the avalanche release. In the present work, the delay is between the time of digging the platform for the tent and the avalanche release. The triggering element is the additional load on top, so it becomes unclear. In the end, the question is "simply" what additional load (that also sinters) do you need to reach the critical stress in the weak layer on the wall to initiate failure in the weak layer. Is that correct? There is an unavoidable layer of complexity to answer this question (stress distribution in the slab, strengthening effect, complex geometry of the slab, etc.) but I think that setting the initial problem with simple and clear words would benefit to the paper.

Response

We have now clarified this aspect in the abstract (lines 13-16), in the introduction (lines 73-75) as well as in the results (lines 153-157). In particular, it is now clear that progressive snow accumulation is the cause of delay between the moment the hikers made a cut in the slope and the onset of snow slab release.

Comment

2) The analytical model is quite complex and it is difficult to follow and to detect potential errors. Sincerely, I could not check each step of te development. It appears sound to me but I would gain confidence in the model if it were evaluated with a numerical model (simple FEM as commonly used for avalanche release mechanics) or with the MPM model developed by the authors.

Response

Responding to the comments of the second reviewer and the editor, we used more realistic assumptions with respect to the snow accumulation on the slope. Remarkably, this resulted in a much simpler analytical model now completely described in the Methods without the need of any supplement. We also made an effort to better clarify the model in the main text. This makes the model much easier to follow. In addition, we followed the advice of both referees and validated the analytical solution using our MPM numerical model. In particular, we show that the critical wind deposited height obtained using MPM lies with the bounds giving by the analytical model (see Figure 1 below). This numerical validation is mentioned in the Methods (lines 477-478) with details given in a new supplement (SM4) in which Figure 1 is presented as well.

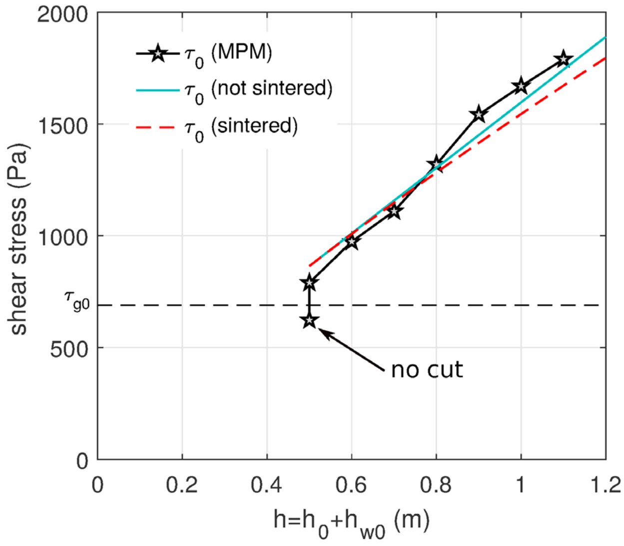

Figure 1. Shear stress as a function of slab height for the MPM simulation and the two bounds of the analytical model.

Comment

3) To my knowledge, direct evaluation of the impact of a snow avalanche on a human body is very new and the combination with results for the automotive industry very interesting. The used of MPM for snow was already evaluated and has been proven to be relevant. My concern is more about the MPM parameters used for the human body. I understand that the human body is simulated as an homogeneous hyperelastic material. Is this type of hyper-elastic well-simulated by MPM?

Response

In our simulations, we indeed simulated the body using a hyperelastic model (St-Venant Kirchhoff) whose parameters are calibrated based on crash test experiments performed in the automotive industry (elastic modulus calibration to reproduce chest deflection). This choice allowed us to simulate the body as a bulk without representing individual bones which would require a resolution incompatible with the scales of avalanche dynamics. Concerning hyper-elasticity and MPM, let us reply in two steps:

1) hyper-elasticity is designed for materials in which the elastic deformation can be very large. In addition, the existence of the strain energy density function made hyper-elasticity easy to implement in FEM dynamic simulations using finite strain theory. As a consequence, it has been largely adopted as well in MPM algorithms used for simulating large deformation problems and which are also based on the discretization of the weak form of the momentum balance equation. Hence, hyper-elasticity is very well simulated by MPM, but the question of the referee may also concern the accuracy of the solution, in terms of stresses, which we address below.

2) historically, MPM is known to have stress inaccuracies, in particular due to discontinuities induced by linear shape functions which were originally used. Since the early development of MPM by Sulsky and co-authors, a lot of progress has been done, in particular with respect to shape functions. In fact, most recent MPM models use higher order shape functions (iMPM) which prevents this issue. In our work, we use cubic B-spline shape functions which guaranties the continuity of the gradients at the nodes (C^2 functions). We also have convergence under mesh refinement, but we must accept that the convergence rate in MPM is not as good as with FEM as shown in many studies (e.g. Vaucorbeil et al.

2019). Finally, to show the quality and accuracy of hyper-elastic models in MPM, we show below the results of a uniaxial tension simulation of a unit length box with elastic modulus E=10MPa and Poisson ratio 0.0 for different mesh resolutions. The agreement with the analytical prediction is excellent and improves under mesh refinement.

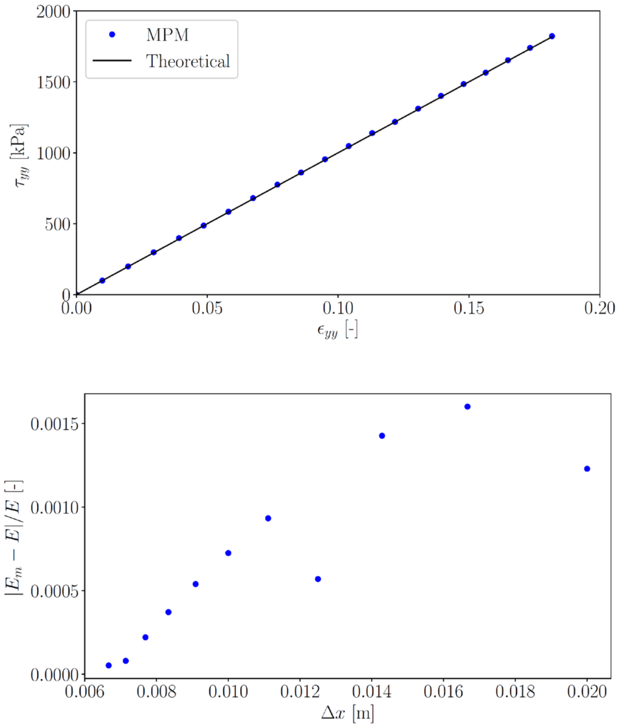

Figure 1. Top: Kirchhoff stress component τ_{yy} as a function of deformation ε_{yy} for a uniaxial tensile test of a unit length solid of elastic modulus E=10 MPa, a Poisson's ratio ν=0.0 and a Δx=0.02 m. Comparison between theoretical and MPM predictions. Bottom: convergence analysis showing the relative error on the obtained elastic modulus as a function of mesh resolution.

This is now briefly mentioned in the Methods related to MPM analysis (lines 560-561).

Reviewer #2 comments

The authors would like to thank the referee for very valuable and constructive remarks and for his detailed analysis which helped and inspired us to significantly rework our model and improve our paper.

Comment

Since the Dyatlov Pass accident is the hub of the paper, I think it is unavoidable that the readers will ask themselves questions like, Is there a plausible explanation why the garments of the victims were radioactive? Why did the expedition members flee in two different directions? Why would experienced skiers and mountaineers enter a steep ravine when fleeing from an avalanche on less steep terrain? These questions that are not directly relevant to the main scientific problems discussed in the paper, but nevertheless have a bearing on how plausible one will consider the avalanche hypothesis. Therefore I feel that the authors ought to address them at least briefly.

Response

Initially, we did not want to address all the controversial elements which led to several conspiracy theories in this scientific paper. Yet, we fully agree with the reviewer that these questions are inherently connected to our conclusions and that we should address them briefly in our manuscript. In fact, we decided that this should be the conclusive statement of our paper as it leaves some mystery to the reader (see lines 289-295).

Comment

When reading the manuscript for the first time, my immediate reaction was that there is some mismatch between the advanced numerical modeling of the impact of snow blocks on humans on the one hand and the simplifying assumptions that had to be made to achieve an analytically solvable model for explaining the possibility of an avalanche release and its timing on the other hand. Based on the fall height, the weight and hardness of the avalanching snow, I could have accepted the statement that some expedition members suffered medium to severe but non-lethal thoracic injury without an MPM simulation. However, I do not wish to hold this imbalance against the paper because the chosen approach demonstrates the potential of advanced numerical methods convincingly. One might, however, ask why the authors did not attempt to simulate the build-up of snow drift behind the parapet numerically (in 3D) to investigate how much the simplifying assumption s in the analytical model influence the results. I am fully aware, however, that such a simulation would open several new cans of worms and would exceed the scope of a Communications paper.

Response

As explained in details below, we made substantial changes both to our analytical method and to the set-up of the numerical model in line with the relevant and constructive criticisms raised by the referee. Following the Referee's suggestions, we made more realistic assumptions with respect to the shape of the wind deposition profile and its temporal evolution (the details are provided in the response to the major comments below). In addition, we followed the advice of both referees and validated the analytical solution using our MPM numerical model. In particular, we show that the evolution of the shear stress in the weak layer (at the vicinity of the cut) with increasing thickness of the sintered snow slab is good agreement with that obtained using MPM (see Figure 2 below). This numerical validation is mentioned in the Methods (lines 476-477) with details given in a new supplement (SM4) in which Figure 2 is presented.

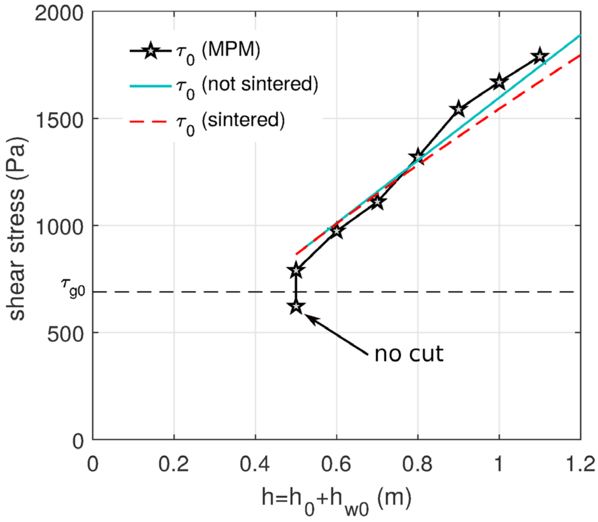

Figure 2. Shear stress as a function of slab height for the MPM simulation and the two bounds of the analytical model.

Major comments

Comment

The authors claim to present a new physical mechanism for avalanche release. In my opinion, they misstate their accomplishment: The physical mechanism they exploit, namely fracture induced by an increasing load eventually exceeding the strength of the weak layer has been known both by avalanche researchers and practitioners for almost a century (the often cited statement attributed to Coaz, "Wind is the architect of snow avalanches", dates form the 19th century). What is novel (to the best of my knowledge) is the authors' quantification of the process, paying attention to the stress distribution along the weak layer. This is a fine achievement and does not need an exaggerated or imprecise "advertisement".

Response

We agree with the referee and removed all statements about the novelty of the mechanism and instead emphasize its quantifiable nature. This also happens to be in agreement with editorial guidelines of *Communications Earth & Environment*.

Comment

The chief advantage of analytically solvable simplified models is the more precise insight they offer when it comes to the importance of different variables or mechanisms. The price one usually has to pay are simplifications that are unrealistic or leave important aspects of reality aside. In the present case, the problem is reduced to a two-dimensional setting whereas it clearly is three-dimensional, given the finite width of the parapet and tent. The snow deposit upstream of the parapet will not be an infinitely wide wedge as in the authors simplification, but more like a pyramid, becoming narrower with increasing distance from the parapet. This is expected to have several important effects:

(i) The extra load from the deposited snow does increase more slowly with l_w than stipulated in the paper.

(ii) Even if the wind is transporting a constant flux of snow particles, the deposition rate Q will decrease as soon as the length of the deposit is of the order of the parapet height.

(iii) Due to the finite width of the deposit, the shear and tensile strength of the snow slab along its perimeter will play a significant role in determining whether a release is possible and when it will occur.

Including such effects is likely to prevent a closed-form solution, so I do not object to the authors choice, but the consequences need to be discussed because they directly bear on the central conclusions of the paper.

Response

Responding to the comment of this referee, we used more realistic assumptions with respect to the snow accumulation on the slope. In particular, the detailed analysis of the local topography around the tent (Figure 2 of the paper) suggests, as already mentioned, that the tent is located around 100 m below a shoulder separating a relatively flat plateau from a steeper slope. This topographical feature would lead to a rather homogeneous snow deposition below the shoulder above the tent. The difference of the new model is that the snow accumulation occurs mainly because of the presence of the shoulder and not due to the parapet. In this new model, the parapet simply becomes an optional feature that would enable to reduce the delay.

This major modification largely removes the issue related to the 3D shape of the snow deposited by wind above the tent due to its limited size. It also reflects the fact that the thickness of the snow deposit reduces away from the tent, as suggested by the reviewer and is more realistic (see Figure 3 in the paper reproduced below). In more detail, the analytical model assumes a 2D geometry which in this case can be justified by the fact that the length of the shoulder controlling snow deposition is much larger than the length of the tent. Yet, we agree that the 2D profile of the deposited snow has been simplified for the sake of obtaining a closed-form solution and this limitation is now explicitly stated in the discussion (lines 258-259).

Surprisingly, the more general assumption made in the revised version of the paper resulted in a much simpler analytical model leading to a closed-form solution validated through MPM numerical simulations. This model is now completely described in the Methods without the need of any supplement. We would like to express our gratitude to the referee who contributed in such a significant improvement of our paper.

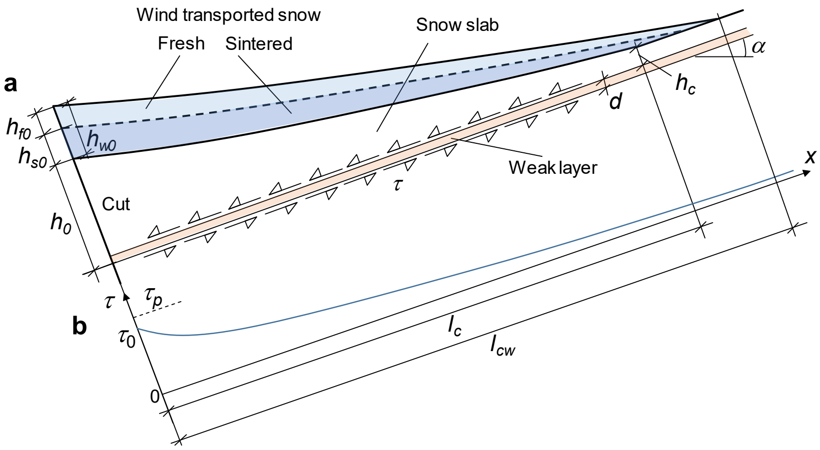

Figure. 3. The mechanism of delayed release of a wind-induced avalanche. a, geometry: A planar weak layer of thickness d is inclined at an angle α to the horizontal. It is buried below a snow slab of variable depth, decreasing from h_0 at the cut ($x = 0$) to h_c at the straight upper.

Comment

The authors quote a value of $Q \sim 0.01$ kg m^{-1} s^{-1} from the literature and use this value in the rest of the paper. As Eq. (3) shows, the delay of the release is inversely proportional to Q, thus the value of Q is central in the argument that the calculated delay agrees with the forensic estimate. Figure 10 of Ref. 15 cited by the authors shows that measurements of Q in the wind-speed range 4-10 m s^{-1} indeed vary by about three orders of magnitude. Even if one attributes much of this variation to the differences in methodology between the data sources, it is unlikely in my opinion that the variation in Q is less than an order of magnitude. As far as I can see, this uncertainty makes the agreement between the computed and forensically inferred delay times coincidental. This renders the authors conclusion much less compelling than it is presented in the manuscript and requires a critical discussion in the paper.

Response

We acknowledge the high uncertainty with respect to choosing a suitable value of Q for predicting delays. Therefore, in the revised paper, we use our model to back-calculate the rate Q from forensic estimated delays. The back-calculated value of Q=0.008 kg/m/s corresponds to a range of velocities of 2 - 12 m/s which is close to the forensically estimated range of 4-10 m/s. This has been added in the paper (lines 195-201) together with a discussion about uncertainties (lines 259-264).

Comment

A quantity that plays a secondary yet still rather important role in the authors argumentation is the sintering time scale (the term sintering rate used by the authors is misleading because a rate is generally the change of a quantity per unit time). It determines how much of the snow drift has coalesced with the original snow slab and contributes to limiting the shear in the weak layer; in contrast, the not yet sintered deposit only contributes with an extra load, but no extra strength. This difference should be explained more clearly in the Methods section. This distinction between the sintered part of the snow drift from the parapet to the sintering front at $x = lw-\Delta lw$ and the loose snow drift further uphill is rather artificial because the snow does not typically accumulate as assumed in the model. This is acceptable for the sake of an analytically solvable model, but needs to be explained.

Response

The new model (Figure 3 above) captures snow accumulation more realistically. Following the referee’s suggestion and in order to avoid confusion, the sintering time which determines the thickness of the fresh and sintered snow layers, does not enter the formulation explicitly anymore, especially because we only consider two extreme cases of fully sintered or fully fresh snow. Yet, please note that the model can be straightforwardly extended to accommodate a finite sintering time.

Minor comments

Comment

The English is generally good, but there are some issues that the authors need to correct, e.g., in several places a definite article is missing. I annotated such language errors in the PDF file when-ever I caught them, but there might be more.

Response

Many thanks for the detailed corrections provided in the PDF. Even though we initially sent our paper to a professional copy editing service (ENAGO), small language error can still be present and thus really appreciate the effort made by the referee. In addition, we corrected some additional typos we found.

Comment

Since this journal apparently does not provide in-depth copy editing, the authors should pay attention to general typographical standards. For example, in a number of formulas, the units either are not SI-compliant (sec instead of s) or are typeset in italics where they should be in upright type. Conversely, at some places variables mentioned in the text are typed upright where they should be in italics. The letter K in all its variants (capital or small, with or without index) seems to be the authors' favorite letter. While the different meanings are almost always matched to specific typographic features, it would help the reader a lot if different letters were used unless convention dictates the use of K (e.g., for the earth-pressure coefficient at rest, K0 or Ko).

Response

In the revised manuscript, we made an effort to bring all the formulas and units to the *Communications* journals' editorial standards. In the new model, all K letters have been removed except for K_0, the lateral earth pressure and K as bulk modulus which is used in the numerical analysis (standard notation in mechanics).

Comment

The bibliography is in better shape than in the majority of papers I have reviewed, yet it needs the authors' attention because page numbers are missing in some cases or little known publishers cannot easily be found. Many readers would certainly appreciate if DOIs were provided throughout. The many references to documents published only in Russian present a particular problem: In my opinion, transcriptions to Latin letters should be provided for author names, titles and journals/publishers/newspaper names.

Response

Russian references have been all transcribed to Latin letters. The rest of our Bibliography has been brought to the *Communications* journals' editorial standards.

Comment

Please see the annotated manuscript for further comments.

Response

All the comments in the annotated manuscript have been implemented in the revised paper and indicated by the red color font.

25th Nov 20

Dear Professor Puzrin,

Your revised manuscript titled "The delay mechanism of the Dyatlov Pass avalanche" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Heike Langenberg, PhD

Chief Editor
Communications Earth and Environment

On Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have taken into account my previous comments and have managed to simplify and evaluate the analytical model, which makes the methodology clearer and sound.

However, the answer to one of my comment (n° 1) is not fully convincing. I questioned the use of the term "delay" in the title and throughout the paper. In this paper, the authors refer to delay as the time between the cut in the snowpack and the avalanche release. This might be misleading since the triggering factor is snow accumulation and not the cut in the slope. If the mountaineers would have made no cut in the slope, a progressive loading by snow accumulation would still potentially trigger an avalanche during the night after they set up. Here, the snowpack changes in time so the release probability also evolves. If we caricature, one may as well define a delay between the beginning of the winter season and the avalanche release... I still understand that the Dyatlov incident lies in a narrow range of snow conditions (stable enough enough so that the avalanche does not release when cutting a pit, instable enough so that limited accumulation leads to snow failure) but this needs proper clarification in the text.

For instance, in the abstract, the words "a physical mechanism for a delayed slab avalanche caused by progressive snow accumulation" are misleading as the delay is not defined yet and one may expect a new complex mechanism, which is, in the end, the well known loading by wind-drifted snow. I suggesting defining the delay directly in the abstract. Moreover, the paper is not all about "this delay" but also explaining the limited injuries, the relatively low slope in the release area, the role of the parapet in snow accumulation. So I would reword the title to "The mechanism of the Dyatlov Pass avalanche", which does not reduce the interest and quality of the paper.

Except this comment, and to my opinion, the paper can be accepted as it is.

Pascal Hagenmuller

Reviewer #2 (Remarks to the Author):

Due to shortage of time, I have not checked all equations in detail and not looked at the Supplementary Materials. I am nevertheless positive that the revised manuscript represents a major improvement over the original version. It is in particular much easier to understand, and the geometrical assumptions made in the analytical model are now reasonably realistic. Concerning the length of the delay from the cut in the slope to the slab release, I welcome that the authors no longer claim that they have reproduced the delay established by the forensic investigation, but instead show that this delay requires a reasonable snow deposition rate in their model.
Please see the attached annotated manuscript for minor comments, language points and spelling corrections.
I recommend accepting the manuscript for publication with small corrections.
Dieter Issler

The delay mechanism of the Dyatlov Pass avalanche

Gaume, J.[1,2] and Puzrin, A.M.*[3]

[1] EPFL, Lausanne, Switzerland
[2] WSL Institute for Snow and Avalanche Research SLF, Davos, Switzerland
[3] ETH, Zürich, Switzerland

The Dyatlov Pass incident is an intriguing unsolved mystery from the last century. In February 1959, a group of nine experienced Russian mountaineers perished during a difficult expedition in the northern Urals. A snow avalanche hypothesis was proposed, among other theories, but was found to be inconsistent with the evidence of a lower-than-usual slope angle, scarcity of avalanche signs, uncertainties about the trigger mechanism, and abnormal injuries of the victims. The challenge of explaining these observations has led us to a physical mechanism for a delayed slab avalanche caused by progressive snow accumulation on the slope above the hikers' tent. Here we show how a combination of irregular topography, a cut made in the slope to install the tent and the subsequent deposition of snow induced by strong katabatic winds contributed after a suitable delay to the slab release, which caused severe non-fatal injuries, in agreement with the autopsy results.

*corresponding author: alexander.puzrin@igt.baug.ethz.ch

Introduction

During the night of February 1, 1959, nine Russian hikers died under unexplained circumstances during a skiing expedition in the northern Ural Mountains. The group had decided to set up their camp on the slope of the Kholat Saykhl (Fig. 1a); the name means “Dead Mountain” in the local Mansi dialect. Something unexpected happened after midnight that caused expedition members to cut the tent suddenly from the inside and escape towards a forest 1 km downslope (Fig. 1b), without appropriate clothes, under extremely low temperatures (below −25°C, see supplementary material SM3), and in the presence of strong katabatic winds induced by the passing of an arctic cold front[1].

Twenty-six days to two months after the tragedy, search teams found bodies in the forest and on their way back to the tent. According to the 1959 Soviet criminal investigation, "a compelling natural force" led to the death of the Dyatlov group. However, the nature of this force has not been identified. The mystery arises from numerous unexplained observations. While hypothermia was determined to be the main cause of death, four hikers had severe thorax or skull injuries; two were found with missing eyes and one without tongue; some were almost naked and barefoot; traces of radioactivity were found on some of their clothes; and signs of glowing orange spheres floating in the sky were reported that night[1].

Several theories have been proposed to explain this incident, including infrasound-induced panic, animals, attacks by Yetis or local tribesmen, katabatic winds, a snow avalanche, a romantic dispute, nuclear-weapons tests, etc. The originally popular avalanche theory has been questioned due to several contradictory pieces of evidence[1]: 1) no obvious signs of an avalanche or debris were reported by the search team that arrived 26 days later; 2) the average slope angle above the tent location was not sufficiently steep for an avalanche (lower than 30°); 3) the hypothetical avalanche released during the night, at least nine hours after the cut was made in the slope; and 4) the thorax and skull injuries were not typical for avalanche victims.

In 2015, the Investigative Committee of the Russian Federation (ICRF) re-opened the investigation and in 2019 concluded that a snow avalanche was the most probable cause of the accident[2,3]. The results of this investigation have been challenged recently by the office of the Prosecutor General of the Russian Federation, who in 2019 started its own investigation and in July 2020 came to same conclusion as ICRF. Both investigation have not, however, provided scientific explanations for the four counterarguments listed above and therefore keep being challenged by the relatives, public and researchers. In particular, a 2019 Swedish-Russian expedition disagreed with the ICRF conclusions, instead proposing that the direct impact of katabatic winds on the tent was the main contributing factor.

Based on the significant amount of published material[1-9], it seems that previous investigations lack an important ingredient: a quantifiable physical mechanism that can reconcile the avalanche hypothesis with seemingly conflicting evidence. Identifying such a mechanism may provide new insights into the nature of storm-triggered snowpack instabilities, which is another important motivation for this work.

Here, we show that—even though the occurrence of an avalanche at this location is unlikely under natural conditions—the combination of four critical factors allowed the release of a small snow slab directly above the tent. These factors include (Fig. 1c): (1) the location of the tent under a shoulder in a locally steeper slope to protect them from the wind; (2) a buried weak snow layer parallel to the locally steeper terrain, which resulted in [illegible]inning snow slab; (3) the cut in the snow slab made by the group to install the tent; (4) strong katabatic winds that led to progressive snow accumulation due to the local topography (shoulder above the tent) causing a delayed failure. Furthermore, the possible construction of a parapet[1] above the cut (a classical safety procedure to protect the tent from the wind, Fig. SM3) could have accelerated the failure process. The proposed physical mechanism couples the onset of dynamic shear-fracture propagation in the weak snow layer with wind-induced snow transport. [illegible] model shows that the conditions for an avalanche release were met after a delay of 7.5 to 13.5 hours from the moment the hikers made the cut in the slope, in agreement with the forensic evaluation of the time of death. Dynamic avalanche simulations suggest that even a relatively small slab could have led to severe but non-lethal thorax and skull injuries, as reported by the post-mortem examination[1].

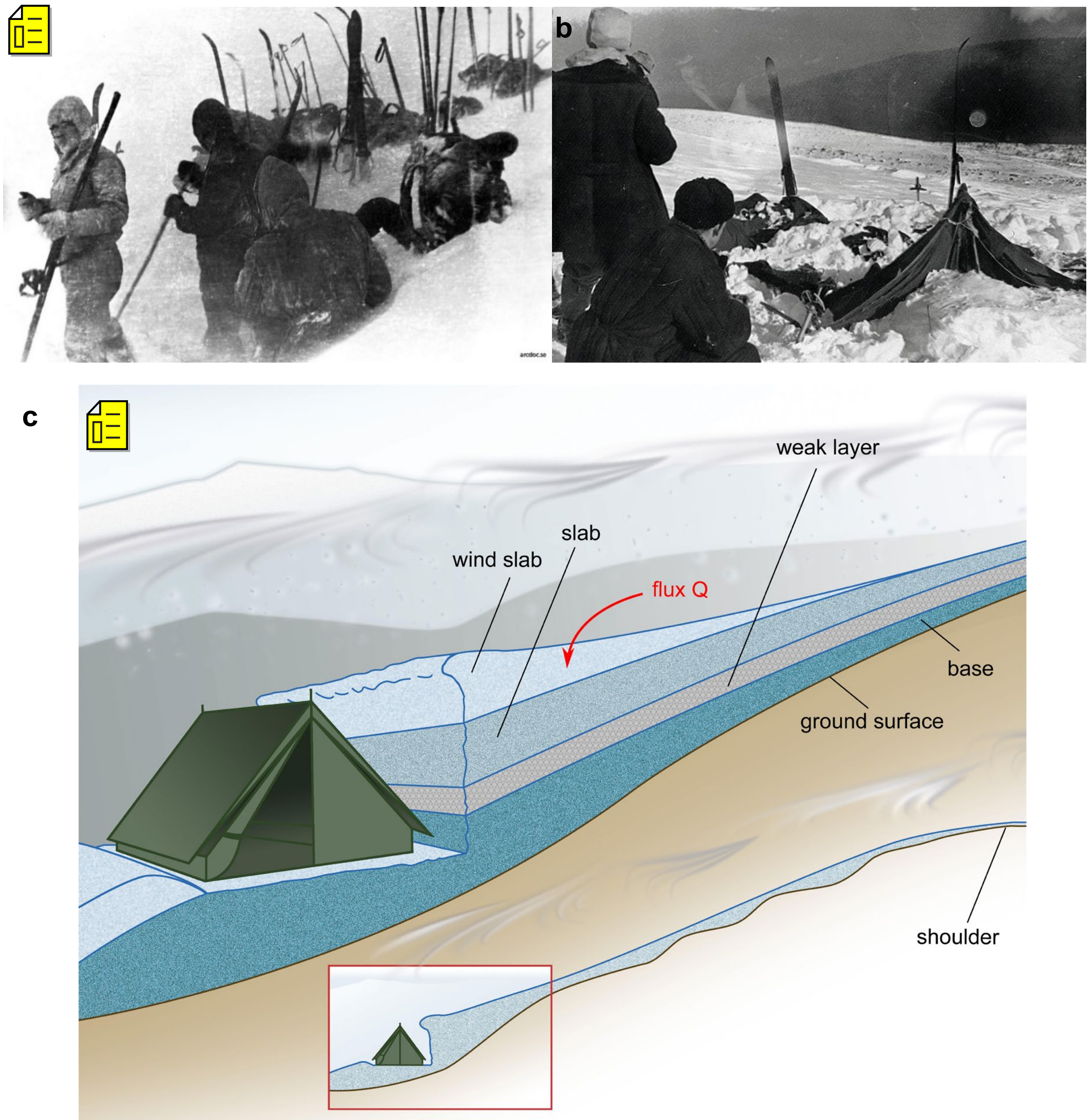

Fig. 1. Pictures taken at the tent location shortly before and weeks after the incident, and a sketch of the slope before the incident. a, last picture of the Dyatlov group taken before sunset, while making a cut in the slope to install the tent. **b,** broken tent covered with snow as it was found during the search 26 days after the event. **c,** configuration of the Dyatlov tent installed on a flat surface after making a cut in the slope below a small shoulder (see Fig. 2a). Snow deposition above the tent is due to wind transport of snow (with deposition flux Q).

Results

Local topography

The mountain slope at the location of the tent is highly irregular. Around 100 m above the tent, there is a shoulder which separates a rather flat plateau and a steeper slope below (Fig. 2a). This slope consists of 4-6 m high steps[1] (Fig. 1c and Fig. SM2) and the tent was installed below one of them, where it was easier to make a cut in a locally flatter slope. The choice of the tent location was also likely driven by the fact that the larger scale shoulder would protect them from the strong winds. In reality, as we show below, this choice of location could have contributed to the accident: small scale topographic variability resulted a locally steep weak snow layer while the larger shoulder contributed to significant wind-driven snow accumulation above the tent, eventually leading to an instability.

Slope angle and friction

Major arguments against the avalanche hypothesis include insufficient signs of the occurrence of an avalanche (no apparent deposit or crown fracture) and the relatively mild slope (~23° +/− 2°). It appears, however, that the ground surface in the vicinity of the estimated tent location was steeper (up to 30°) than the average snow slope. Subsequent snowfalls smoothed the irregular terrain to the observed average 23° slope. This implies directly that the slab above the layer of depth hoar was thinner uphill, which has three main consequences: 1) It results in the average slope of the buried weak layer being about 28°, increasing the probability of slab release (Fig 2b). 2) It reduces tensile support at the top of the slab, considerably limiting the avalanche volume. 3) It makes it more difficult to trace avalanche signs, especially 26 days after the incident.

While a 28° slope may be perceived as being too mild to cause an avalanche—based on the implicit rule of thumb that a minimum of 30° is required[10]—in reality, the critical slope angle can be as low[11] as 20° (Fig. 2b), provided the dynamic friction angle (sometimes called crack-face friction[11]) of the snow is sufficiently low. In fact, field measurements have shown that the dynamic friction angle of snow can be as low as 15°, especially for very low temperatures[12]. In particular, the buried crystals of depth hoar, which were reported by the investigation on the site (see SM3), tend to exhibit rather low friction values[13] (Fig. 2c).

Additional load due to wind-transported snow

In spite of the fact that the 28° inclination of the buried weak layer was higher than the angle of friction, the snowpack did not fail for at least nine hours after the slope was cut (see SM1), due to cohesion in the weak layer. In principle, overcoming cohesion does not necessarily require additional loading. Recent work on delayed snow avalanches[14] has proposed a release mechanism driven by rate-dependent processes in the snow slab and the weak layer[10], which can develop under constant loads. It cannot, however, accommodate a nine-hour delay, due to the relatively short extent of the slab. It follows that the true mechanism must involve additional loading of the slope. Given the extremely low temperatures and strong katabatic winds, it is unlikely that anyone would have climbed above the tent during the night, disturbing the weak layer. In the absence of significant snowfall, the only way to accumulate additional load is through wind transport[15].

Snow accumulation above the tent resulted from katabatic winds and the presence of a shoulder located above the tent (Fig. 2a). A possible construction of a small snow parapet by the Dyatlov group (classical safety measure for snow camping, Fig. SM3), could contribute to additional loading.

Below, we present an analytical model—which reproduces the forensic estimate of delay—for a thinning snow slab that is gradually loaded by wind-transported snow above the cut in the slope. Subsequent numerical modeling confirms that the observed injuries of the Dyatlov group members are consistent with the failed-slab dynamics.

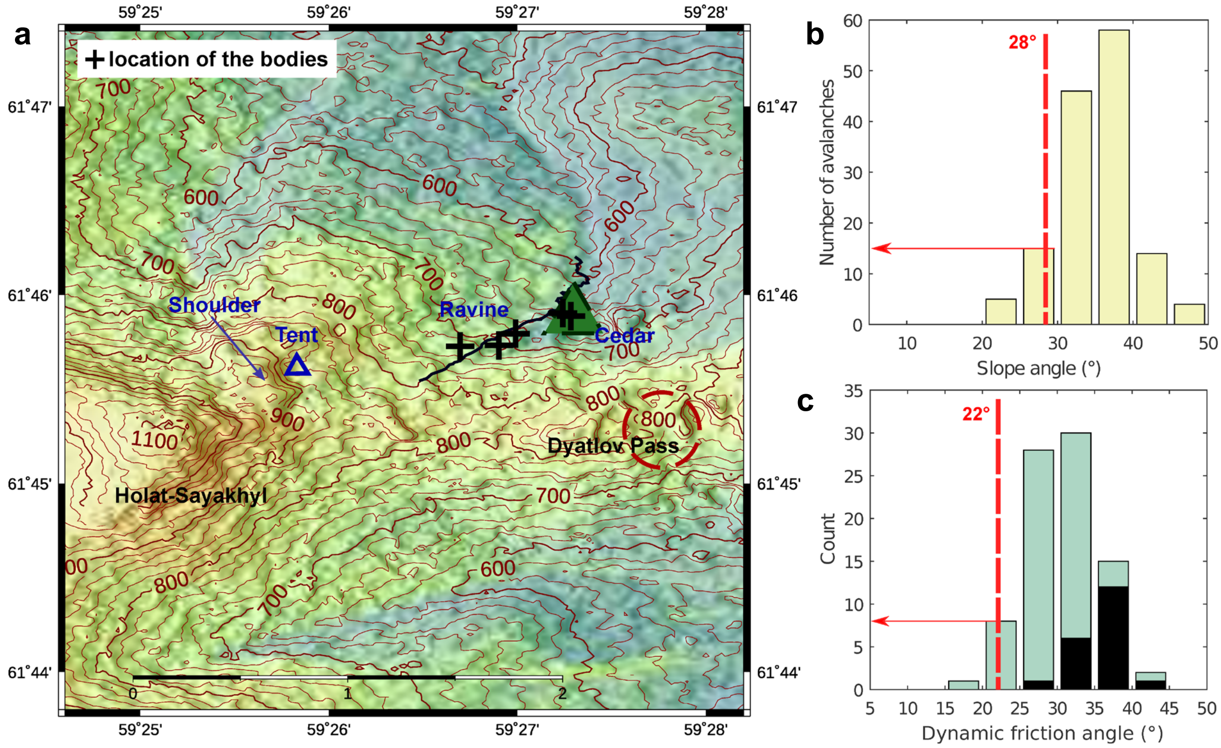

Fig. 2: Local terrain in the Dyatlov Pass, reported snow dynamic friction values, and typical slope angles in human-triggered avalanches. a, Terrain map of the Dyatlov Pass – indicated by the red circle (based on JAXA aster data, from USGS Earth Explorer); blue triangle and black crosses indicate locations of the tent and of the hikers' bodies, respectively. **b**, Slope angles reported for 139 accidentally triggered avalanches[11]. **c**, Dynamic friction-angle distribution based on van Herwijnen et al.[13]. Green = faceted types of crystals (depth hoar, faceted crystals, rounded facets, and surface hoar); black = new snow, decomposed and fragmented crystals, and rounded grains.

Conditions for a delayed avalanche

The proposed delayed-release mechanism for a variable thickness slab (based on the local topography) is illustrated in Fig. 3. After the expedition members made the cut in the slope (Fig. 3a), the shear stress in the weak layer did not immediately reach the peak shear strength. Due to wind-driven snow deposition, the slope gradually becomes loaded over the growing length l_{cw}, causing growth of shear stresses in the weak layer. This growth is affected by the gradual increase of the slab thickness due to the partial snow sintering[16], and continues until the shear stress in the weak layer reaches the peak shear strength τ_p in the vicinity of the cut (Fig. 3b). A further snow deposition does not mobilize additional shear resistance in the weak layer; on the contrary, it initiates rapid softening of the weak layer close to the cut, resulting in the loss of equilibrium, dynamic uphill growth of the basal shear fracture, and slab release.

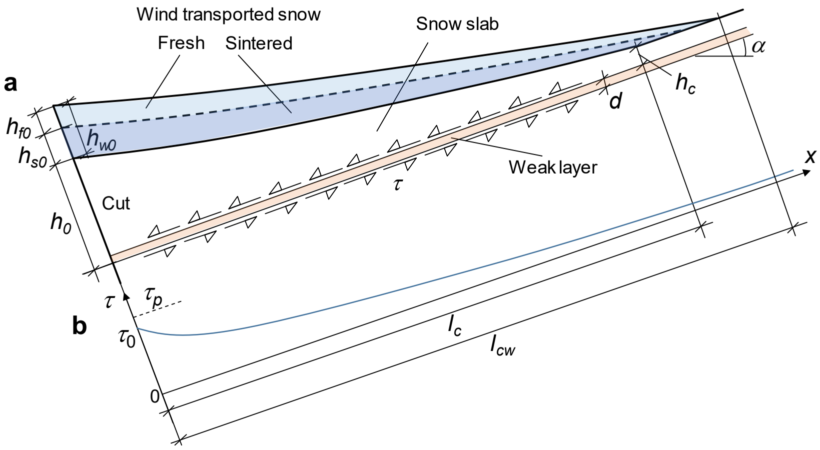

Fig. 3. The mechanism of delayed release of a wind-induced avalanche. a, geometry: A planar weak layer of thickness d is inclined at an angle α to the horizontal. It is buried below a snow slab of variable depth, decreasing from h_0 at the cut ($x = 0$) to h_c at the straight upper slope ($x \geq l_c$). After the cut is made, a wind-transported snow layer of the height h_{w0} (at the cut) loads the slope, causing local thickening of the slab over the height h_{s0}. **b**, shear stresses in the weak layer.

The slab release condition is derived in the Methods as a solution of the plain strain (2D) boundary value problem of a variable thickness cohesive slab under a variable thickness cohesionless snow load, when the shear stress in the weak layer at the cut τ_0 reaches the peak strength τ_p:

$$\tau_0 = \tau_{g0} + r_1 \frac{P_{g0}}{L_0} + \frac{r_3}{r_3 + 2} \frac{P_{w0}}{L_0} \geq \tau_p \tag{1}$$

where $L_0 = l_c\left(1 - \sqrt{h_c/h_0}\right)^{-1}$ is a characteristic length, with quantities l_c, h_c and h_0 defined in Fig. 3a. Parameters r_1 and r_3 are given by:

$$r_1 = \frac{1 + \sqrt{1 + 4\lambda_0}}{2}; \quad r_3 = \frac{1 + \sqrt{1 + 4\lambda_s}}{2}; \quad \lambda_s = \frac{\lambda_0 h_0}{h_0 + h_{s0}}; \quad \lambda_0 = \frac{GL_0^2}{E' h_0 d} \tag{2}$$

where quantities h_{s0} and d are defined in Fig. 3a; G is the shear modulus of the weak layer; E' is the plane strain elastic modulus of the slab. Forces and stresses in Eq. (1) are defined as

$$P_{g0} = \frac{1}{2}K_0 \rho g h_0^2 \cos\alpha; \quad P_{w0} = \rho_w g h_{w0} L_0 \cdot \sin\alpha; \qquad (3)$$
$$\tau_{g0} = \rho g h_0 \sin\alpha; \quad \tau_p = (\rho h_0 + \rho_w h_{w0}) g \cos\alpha \tan\varphi + c,$$

where φ and c are the angle of internal friction and the cohesion of the weak layer; ρ and ρ_w are the densities of the original slab and of the wind-transported snow, respectively; h_{w0} is the thickness of the wind-deposited snow at the cut; K_0 is the at rest lateral snow pressure coefficient.

Figs. 4a–4b provide a conceptual framework for calculating the time of avalanche release. It has been shown (see Methods) that delayed slab release is only possible when the angle of internal friction in the weak layer φ falls within the following range (Fig. 4a):

$$\tan\varphi_{min} < \tan\varphi < \tan\varphi_{max}(\lambda_0);$$
$$\tan\varphi_{min} = \tan\alpha - \frac{c}{\rho g h_0}\sqrt{1+\tan^2\alpha} + r_1\frac{K_0 h_0}{2L_0}; \qquad (4)$$
$$\tan\varphi_{max}(\lambda_0) = \frac{1+\sqrt{1+4\lambda_0}}{5+\sqrt{1+4\lambda_0}}\tan\alpha\,,$$

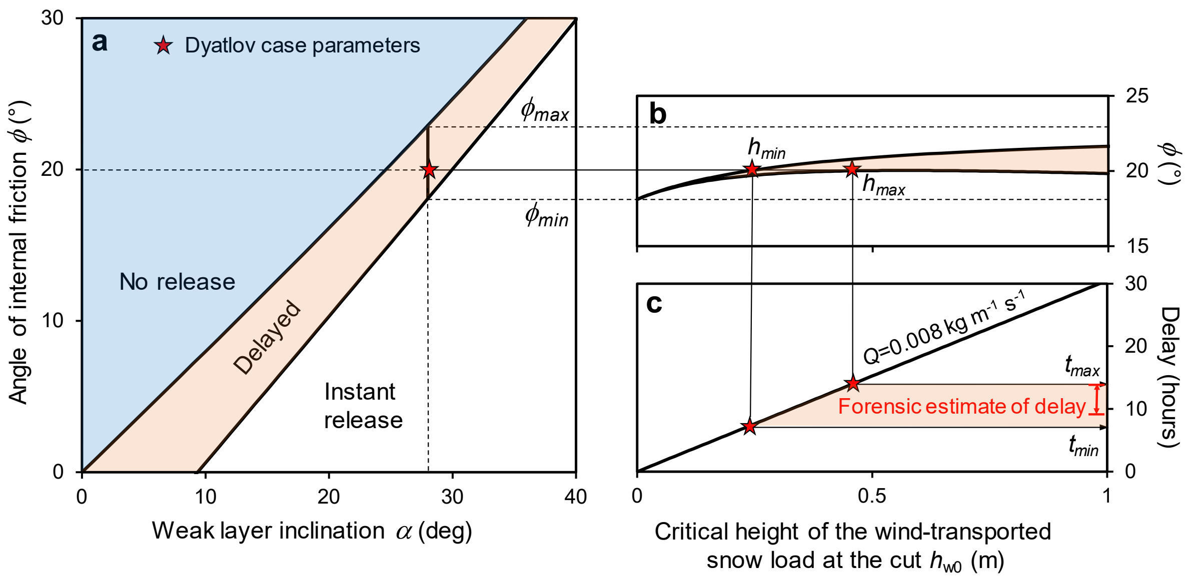

Fig. 4. Conceptual framework for calculating the time of avalanche release. a, conditions for delayed slab release. **b,** bounds for the critical height of the wind-transported snow load. **c,** predicted *vs.* forensically estimated delays.

For a value of φ within the above range, the shortest and longest possible delays occur when h_{w0} reaches the minimum $\left(h_{w0,min}\right)$ and maximum $(h_{w0,max})$ critical values defined by infinite and zero sintering rates, respectively (Fig. 4b):

$$h_{w0,min} = h_0 \frac{\rho}{\rho_w} \frac{\tan\varphi - \tan\varphi_{min}}{\tan\varphi_{max}(\lambda_0) - \tan\varphi} ;$$

$$h_{w0,max} = h_0 \frac{\rho}{\rho_w} \frac{\tan\varphi - \tan\varphi_{min}}{\tan\varphi_{max}(\lambda_s) - \tan\varphi} . \quad (5)$$

Finally, we obtain the bounds for the time of delayed release (Fig. 4c) by substituting the bounds for h_{w0} into the equation

$$\Delta t = \frac{\rho_w h_{w0} L_0}{3Q}\left(1 - \left(1 - \frac{l_c}{L_0}\right)^3\right) \quad (6)$$

where Q is the snow deposition rate.

The proposed analytical framework allowed for back-calculation of the wind deposition flux Q from the range of possible delays between 9.5 and 13.5 hours based on forensic data (see SM1). For the geometric and physical parameters of the Dyatlov case (presented in the Methods and discussed in the SM6), a reasonable fit of the observed delay range (Fig. 4c) is given by $Q = 0.008 \text{ kg m}^{-1}\text{s}^{-1}$. Extensive research on snow accumulation shows[15] that this deposition flux requires average wind velocities in the range of 2–12 m s^{-1} which is in agreement with data from nearby weather stations on the night of the accident (see SM3).

Impact on a human body

In addition to explaining the delay, our proposed mechanism provides the pre-failure slab geometry, which can help in understanding how a relatively small slab caused the severe but non-fatal injuries reported. We address this question by combining a novel numerical model with existing data for human-thorax injuries from impact tests performed by the automotive industry[17,18].

Three-dimensional numerical simulations based on the Material Point Method (MPM) and finite-strain elastoplasticity[19] (see Methods) show that this small-slab avalanche impacted the hikers lying on the tent floor and filled the excavated space but did not have a significant runout, consistent with the reported lack of clear avalanche signs (Fig. 5). The predicted length of 5.0 m for tensile failure of the slab is in agreement with the analytical model. The simulated snow slab reached a velocity around 2 m/s upon impact (see Extended Data Fig. 1, SM5 and supplementary videos 1-2). At this velocity, an impact on a human thorax of a typical snow block with a volume of 0.125–0.5 m^3 and density 400 kg/m^3 results in a maximum thorax deformation between 28% and 34%, corresponding to the lower range of values reported from crash tests by Kroell *et al.*[18] for a 10 kg mass impacting the thorax at a speed of 7 m/s (Fig. 5, SM5 and supplementary videos 3-6). According to the Abbreviated Injury Scale[18] (AIS), these deflections would mostly lead to non-fatal thoracic injuries from moderate to severe, in agreement with the autopsy report of the Dyatlov-incident criminal investigation. Such injuries are not usually observed in avalanche victims, because impacts rarely occur against stiff obstacles. In the Dyatlov case, the victims were trapped between the falling slab and the tent floor, which was placed on compacted snow reinforced by skis[1].

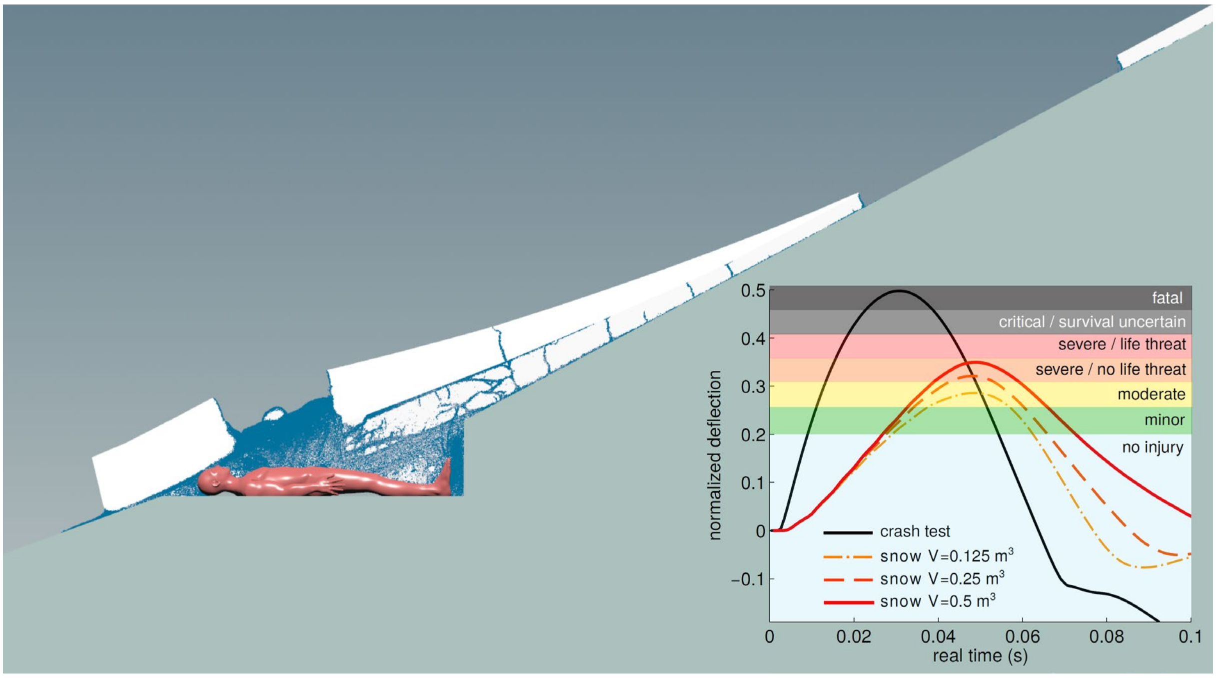

Fig. 5. MPM simulation of the dynamics of a snow-slab avalanche and its impact on a human body. Blue snow particles represent regions in which the yield condition has been met. The inset shows a simulated (3D MPM) thorax deflection (normalized by an original thorax height of 20 cm) for a crash test with a 10 kg rigid mass impacting the thorax at a 7 m/s velocity (black curve) and for impacts by 0.125, 0.25, and 0.5 m^3 elastoplastic snow blocks of 400 kg/m^3 density and velocities of 2 m/s (orange dash-dotted curve, red dashed curve, and red solid curve, respectively). Injury levels correspond to the AIS[11].

Discussion

Significant progress in snow and avalanche research over the past two decades[19-23] has allowed better understanding of avalanche dynamics and of the processes related to snow-slab avalanche release[24-28]. Nevertheless, no mechanism similar to the one inspired by the Dyatlov mystery has been explored in the literature, and its physical quantification required new theoretical developments.

In our analytical model, these developments include a snow slab with a spatially variable thickness and its evolution due to sintering of the wind-transported snow, which affects the instability of a buried weak snow layer. This is highly relevant for the study of natural storm-triggered slab avalanches[29] because its application is not limited only to wind blowing snow events but can also account for additional loads due to a snowfall. The variable slab geometry resulting from irregular local topography and the cut made in the slope play a critical role in determining whether or when an instability will occur. Our numerical simulation of the impact of a snow avalanche on a human body constrained by an obstacle combines advanced elastoplastic constitutive models with large-deformation dynamic numerical analysis (MPM) and biomechanical modeling of the human body. This opens new perspectives for research on the effects of snow avalanches on human health and safety.

Needless to say that our models are based on a number of assumptions, which can be justified for this particular case study and relaxed for future research. For example, given the very low reported temperatures, we assumed a brittle behavior for the weak layer which allows neglecting the effects of the process zone[30]. Moreover, weak layer volumetric collapse[19, 22, 23] did not have to be accounted for in our approach because this layer remains completely intact before the onset of instability. Furthermore, the analytical model assumes a 2D geometry which in this case can be justified by the fact that the length of the shoulder controlling snow deposition is much larger than the length of the tent. Yet, the 2D profile of the deposited snow has been simplified for the sake of obtaining a closed-form solution. Another important source of uncertainty lies in the dependency of the wind deposition flux on the average wind velocity. The available research[15] shows a very wide range of measured deposition fluxes for a relatively narrow range of the average wind velocity. Nevertheless, the range of wind velocity back-calculated using the analytical model and the forensic estimation of the delay is in good agreement with the range reported in nearby weather stations during the night of the accident.

Concerning the numerical models, because our main focus was the global thorax response, the skeleton and individual ribs were not analyzed. Focusing on the thorax impact, the MPM simulation is initiated at the onset of slab release and the weak layer is not explicitly modeled. In spite of these simplifications, both the analytical and numerical models independently predicted a similar size of the failed slab, providing additional validation for the new mechanism. In addition, while our simulations show that in principle, the observed injuries could have resulted from the avalanche impact, the impact-induced deformations of the thorax would be rather sensitive to the size of the disintegrated slab blocks (Fig. 5) and thus to the relative positions of the bodies with respect to the cut and slope direction. Given this uncertainty, it is also possible[1] that the thorax injuries were the result of a later snow impact in a very steep ravine where the bodies of the victims, escaping the avalanche area, were found.

Solving the Dyatlov Pass mystery is an enormous task, which is far beyond the scope of this paper. We hope, however, that our work may contribute to determining the plausibility of the avalanche hypothesis. More importantly, it allows the quantification of conditions that can help to prevent similar incidents. Clearly, for a cut in the snow slope to cause a delayed slab release, it requires a relatively rare combination of: i) a sufficiently steep, weak layer at the base of the snowpack, ii) a cut in the slope and 3) significant snow accumulation after the cut due to wind transport. However, once these conditions are present, the occurrence of a delayed release requires fairly common values of geometrical and mechanical parameters (see Fig. 4), and Dyatlov-related investigations have indeed reported a non-negligible number of similar accidents[1] (see SM2). This implies that building a tent even on a relatively mild slope (less than 30°) can be dangerous and should not be recommended when combined with a cut in the slope. Instead, digging a snow cave may be a safer solution, as confirmed by the increasing use of this practice for winter camping in recent decades[31].

In conclusion, our work shows the plausibility of a rather rare type of snow slab instabilities that could possibly explain the Dyatlov Pass incident. Yet, we do not explain nor address other controversial elements surrounding the investigation such as traces of radioactivity found on the victims' garments, the behaviour of the hikers after leaving the tent, locations and states of bodies, etc. While possible explanations are given in multiple published sources[1-9] as well as by both the Investigative Committee and the Prosecutor General of the Russian Federation, we believe that this will always remain an intrinsic part of the Dyatlov Pass Mystery.

Online content

Any methods, additional references, Nature Research reporting summaries, source data, extended data, supplementary information, acknowledgements, peer review information; details of author contributions and competing interests; and statements of data and code availability are available at https://doi.org/10.1038/XXX.

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Methods

Analytical model of delayed avalanche release

Problem formulation

A simplified delayed slab release model is based on the mechanism illustrated in Fig. 1A. We assume a plane strain problem with a cut in a curved slope, with a planar slope-parallel weak layer of angle α, thickness d at the depth h described by a parabolic equation:

$$\begin{cases} h(x) = h_0\left(1 - \dfrac{x}{L_0}\right)^2 & \text{for } x < l_c \\ h(x) = h_c & \text{for } x \geq l_c \end{cases} \tag{7}$$

where h_0 is the depth of the weak layer at the cut ($x = 0$); h_c is the constant depth of the weak layer at the upper straight portion of the slope ($x \geq l_c$); l_c is the distance from the cut to the point on the slope where the slope surface becomes parallel to the weak layer. The characteristic length L_0 is defined from the condition $h(x = l_c) = h_c$:

$$L_0 = \frac{l_c}{1 - \sqrt{h_c/h_0}}; \tag{8}$$

The choice of the parabolic slope approximation has been based on the following considerations. Firstly, it reflects a smooth uphill steepening of the slope. Secondly, it leads to a second order Euler-Cauchy differential equation with a simple analytical solution. Thirdly, it is geometrically rather close to the simplest linear approximation of the slope between the cut ($x = 0$) and the beginning of the constant thickness slab ($x = l_c$).

After the cut is made, the snow keeps accumulating on the slope behind the tent due to the wind transport, with $h_w(x,t)$ being the thickness of the wind transported snow at the time t after construction of the parapet. Assuming that the surface of the new snow layer can also be described by a parabolic function, the thickness of the wind transported snow is

$$h_w(x,t) = h_s(x,t) + h_f(x,t) = \left(h_{s0}(t) + h_{f0}(t)\right)\left(1 - \frac{x}{L_0}\right)^2 \quad \text{for } x < l_{cw}$$
$$h_w(x,t) = h_c \quad \text{for } x > l_{cw}; \qquad l_{cw} = L_0\left(1 - \sqrt{\frac{h_c}{h_0 + h_{s0} + h_{f0}}}\right) \tag{9}$$

where h_f is the portion of the new snow that is still fresh, while h_s is the portion which is already sintered, with h_{f0} and h_{s0} denoting the thickness of each portion at the cut ($x = 0$). The parabolic approximation for the surface of the wind-transported snow reflects the fact that snow deposition due to the wind transport reduces away from the tent. Note that the tent represented an obstacle for the wind flow, in particular if a parapet was built above the cut to protect the tent against wind and snow.

The initial (before the cut) internal lateral force P_g in the snow slab is:

$$P_g = \sigma_g h = \frac{1}{2} K_0 \sigma_n h = \frac{1}{2} K_0 \rho g h^2 \cos\alpha \qquad (10)$$

where ρ is snow density; K_0 is the at rest lateral snow pressure coefficient. In general, the value of K_0 depends on the history of snow deposition, but can be bounded (for horizontal snow layer) between the estimate via Poisson's ratio ν: $K_0 = \frac{\nu}{1-\nu}$ (elastic) and the estimate given by Jaky's formula[32]: $K_0 = 1 - \sin\varphi_s$ (for a normally consolidated granular material), where φ_s is the angle of internal friction of the snow slab.

The initial stress state (before the cut) in the weak layer is:

$$\tau_g = \rho g h \sin\alpha; \quad \sigma_n = \rho g h \cos\alpha \qquad (11)$$

where τ_g is the initial shear stress, causing the initial displacement (δ_g); σ_n is the normal stress. In the following, we adopt a net value $\Delta\tau = \tau - \tau_g$ for the shear stress τ in the weak layer, and the net values $\Delta P = P - P_g$ and $\Delta\delta = \delta - \delta_g$ for the internal lateral force P and displacement δ of the snow slab, respectively.

Neglecting inertia forces, the equilibrium condition for the snow slab is given as:

$$\frac{\partial \Delta P}{\partial x} = \Delta\tau - \tau_w(x); \quad \Delta\tau = \tau - \tau_g; \; \Delta P = P - P_g \qquad (12)$$

The shear response of the weak layer is assumed to be linear:

$$\Delta\tau = \frac{G}{d}\Delta\delta; \;\; \Delta\gamma = \frac{\Delta\delta}{d}; \;\; \Delta\delta = \delta - \delta_g \qquad (13)$$

where G is the shear modulus of the weak layer and

$$\delta_g = \frac{d}{G}\tau_g; \quad \tau_g = \rho g h \cdot \sin\alpha; \quad \tau_w = \rho_w g\left(h_s + h_f\right) \cdot \sin\alpha \qquad (14)$$

where δ_g is the initial displacement of the slab, resulting from the constant initial shear stress τ_g (before the cut).

Extension in the snow slab is also assumed to be linear:

$$\Delta P = \Delta\sigma h = E' h \Delta\varepsilon = E' h \frac{\partial \Delta\delta}{\partial x} \qquad (15)$$

where $E' = E/(1-\nu^2)$ is the plain strain modulus of the snow slab. Combining Eqs. (7), (13) and (15) gives:

$$\Delta P = \frac{E'hd}{G}\frac{\partial \Delta\tau}{\partial x} = \frac{E'h_0 d}{G}\left(1-\frac{x}{L_0}\right)^2 \frac{\partial \Delta\tau}{\partial x} \tag{16}$$

and

$$\Delta P = \frac{E'(h+h_s)d}{G}\frac{\partial \Delta\tau}{\partial x} = \frac{E'(h_0+h_{s0})d}{G}\left(1-\frac{x}{L_0}\right)^2 \frac{\partial \Delta\tau}{\partial x} \tag{17}$$

for the curved portions of the original and the thickened slab, respectively.

It is assumed that after the slope is cut, behaviour in the weak layer is linear elastic, with the shear stress τ in Figure 1B not yet reaching the peak strength τ_p. Due to the wind driven snow deposition h_w, the slope gets loaded by additional shear stress τ_w, until at a certain time Δt after the cut the shear stress in the weak layer τ reaches the peak strength τ_p in the vicinity of the cut. Further increase in h_w will not mobilise additional shear resistance in the weak layer, on the contrary, it will initiate rapid softening of the weak layer close to the cut, increasing the downhill lateral load on the still intact portion of the weak layer. This will result in the loss of equilibrium, unstable uphill growth of the basal shear fracture and the release of the slab.

Conditions in the snow slab immediately after the cut

In this and the following sections we neglect the straight portion of the slope and assume that within $0 \le x \le L_0$ the whole slab is parabolic. It can be shown that for $h_c/h_0 \le 0.2$ (consistent with the Dyatlov case), this simplification has a negligible effect on the solution.

At the moment of the cut, the thickness of the slab is given by equation (7), therefore $\tau_w = 0$, and differentiating Eq. (12) with respect to x and substituting Eqs. (16) into it gives a differential equation for net lateral forces:

$$(1-\bar{x})^2 \frac{\partial^2 \Delta P}{\partial \bar{x}^2} = \lambda_0 \Delta P \quad \text{for } 0 \le \bar{x} \le 1 \tag{18}$$

where

$$\bar{x} = \frac{x}{L_0};\ \ \lambda_0 = \frac{L_0^2}{l_{e0}^2};\ \ l_{e0} = \sqrt{\frac{E'h_0 d}{G}} \tag{19}$$

Eq. (18) is the well-known second order Euler-Cauchy differential equation, with solution given by

$$\Delta P(\bar{x}) = C_1(1-\bar{x})^{r_1} + C_2(1-\bar{x})^{r_2} \tag{20}$$

$$r_1 = \frac{1+\sqrt{1+4\lambda_0}}{2}\;; \quad r_2 = \frac{1-\sqrt{1+4\lambda_0}}{2}$$

where C_1 and C_2 are constants to be found from boundary conditions. In order to have a finite solution at $\bar{x} = 1$, C_2 has to be set to zero, and from the boundary condition at $\bar{x} = 0$, where the lateral force is equal to zero ($\Delta P = -P_g|_{\bar{x}=0}$) due to the cut:

$$\Delta P|_{\bar{x}=0} = C_1 = -P_{g0};\ P_{g0} = \frac{1}{2}K_0 \rho g h_0^2 \cos\alpha \tag{21}$$

Substituting these constants into equation (20) provides stress conditions in the slab and the weak layer after the cut in the slope:

$$\begin{aligned} P_c(\bar{x}) &= P_g(\bar{x}) + \Delta P(\bar{x}) = P_g(\bar{x}) - P_{g0}(1-\bar{x})^{r_1} \\ \tau_c(\bar{x}) &= \tau_g(\bar{x}) + \frac{1}{L_0}\frac{\partial \Delta P}{\partial \bar{x}} = \tau_g(\bar{x}) + \frac{r_1}{L_0}P_{g0}(1-\bar{x})^{r_1-1} \end{aligned} \tag{22}$$

with the boundary conditions at the cut $\bar{x} = 0$:

$$P_c|_{\bar{x}=0} = 0; \quad \tau_c|_{\bar{x}=0} = \tau_{g0} + \frac{r_1}{L_0}P_{g0} \tag{23}$$

For the slab not to fail immediately after the cut the following condition should be satisfied:

$$\begin{aligned} \tau_c|_{\bar{x}=0} &= \tau_{g0} + \frac{r_1}{L_0}P_{g0} < \tau_{p0} \\ \tau_{p0} &= \sigma_n \tan\varphi + c = \rho g h_0 \cos\alpha \tan\varphi + c \end{aligned} \tag{24}$$

where φ and c are the angle of internal friction and cohesion in the weak layer. In this case the normal force and shear stress from Eqs. (22) will serve as initial conditions for the next stage – loading of the slab by the wind transported snow.

Failure of the undercut slab loaded by the wind transported snow

After the wind transported snow reached the thickness h_w given by equation (9), the additional shear stress τ_w in Eq. (12) is now larger than zero:

$$\begin{aligned} \tau_w(\bar{x}) &= \rho_w g h_w(\bar{x}) \cdot \sin\alpha = \frac{P_{w0}}{L_0}(1-\bar{x})^2 \\ P_{w0} &= \rho_w g\left(h_{s0} + h_{f0}\right)L_0 \cdot \sin\alpha \end{aligned} \tag{25}$$

where ρ_w is the density of the wind transported snow. The portion h_s of the new snow has sintered, increasing the thickness of the original slab, as reflected in Eq. (17). After substituting $\tau_w(\bar{x})$ into Eq. (12), differentiating it with respect to x and combining it with Eq. (17) we obtain the following differential equation for net lateral forces in the sintered slab:

$$(1-\bar{x})^2\frac{\partial^2\Delta P}{\partial\bar{x}^2}=\lambda_s\Delta P+2P_{w0}(1-\bar{x})^3\quad \text{for } 0\le\bar{x}\le 1 \tag{26}$$

where

$$\bar{x}=\frac{x}{L_0};\;\lambda_s=\frac{\lambda_0 h_0}{h_0+h_{s0}} \tag{27}$$

Solution of Eq. (26) is given by

$$\begin{gathered}\Delta P(\bar{x})=C_1(1-\bar{x})^{r_3}+C_2(1-\bar{x})^{r_4}-\frac{2P_{w0}}{\lambda_s-6}(1-\bar{x})^3\\ r_3=\frac{1+\sqrt{1+4\lambda_s}}{2}\;;\quad r_4=\frac{1-\sqrt{1+4\lambda_s}}{2}\end{gathered} \tag{28}$$

where C_1 and C_2 are constants. Again, in order to have a finite solution at $\bar{x}=1$, C_2 has to be set to zero, and from the boundary condition at $\bar{x}=0$, where the lateral force is equal to zero due to the cut:

$$\Delta P|_{\bar{x}=0}=C_1-\frac{2P_{w0}}{\lambda_s-6}=0 \tag{29}$$

substituting these constants into equation (28) provides solution for the net normal forces:

$$\Delta P(\bar{x})=\frac{2P_{w0}}{\lambda_s-6}[(1-\bar{x})^{r_3}-(1-\bar{x})^3];\quad r_3=\frac{1+\sqrt{1+4\lambda_s}}{2} \tag{30}$$

This leads to the following normal forces in the slab and shear stresses in the weak layer:

$$\begin{aligned}P(\bar{x})&=P_c(\bar{x})+\Delta P(\bar{x})\\ &=P_g(\bar{x})-P_{g0}(1-\bar{x})^{r_1}+\frac{2P_{w0}}{\lambda_s-6}[(1-\bar{x})^{r_3}-(1-\bar{x})^3]\\ \tau(\bar{x})&=\tau_c(\bar{x})+\tau_w(\bar{x})+\frac{1}{L_0}\frac{\partial\Delta P}{\partial\bar{x}}\\ &=\tau_g(\bar{x})+\frac{r_1}{L_0}P_{g0}(1-\bar{x})^{r_1-1}+\frac{P_{w0}}{L_0}(1-\bar{x})^2\\ &\quad-\frac{2P_{w0}}{\lambda_s-6}\left[\frac{r_3}{L_0}(1-\bar{x})^{r_3-1}-\frac{3}{L_0}(1-\bar{x})^2\right]\end{aligned} \tag{31}$$

At the cut $\bar{x}=0$, using $\lambda_s=r_3^2-r_3$ we obrain

$$P_0=0;\;\;\tau_0=\tau_{g0}+\frac{r_1}{L_0}P_{g0}+\frac{P_{w0}}{L_0}\frac{\lambda_s-2r_3}{\lambda_s-6}=\tau_{g0}+\frac{r_1}{L_0}P_{g0}+\frac{P_{w0}}{L_0}\frac{r_3}{r_3+2} \tag{32}$$

and when $h_{w0}=h_{f0}+h_{s0}$ becomes sufficiently large for this shear stress to reach the peak strength:

$$\tau_0 = \tau_{g0} + r_1 \frac{P_{g0}}{L_0} + \frac{r_3}{r_3 + 2} \frac{P_{w0}}{L_0} \geq \tau_p = \tau_{p0} + \tau_{pw} \tag{33}$$

the slab will fail. Shear stresses and normal forces in Eq. (33) are defined as

$$\begin{gathered} P_{g0} = \frac{1}{2} K_0 \rho g h_0^2 \cos\alpha; \quad P_{w0} = \rho_w g \left(h_{s0} + h_{f0}\right) L_0 \cdot \sin\alpha; \quad \tau_{g0} = \rho g h_0 \sin\alpha \\ \tau_{p0} = \rho g h_0 \cos\alpha \tan\varphi + c; \quad \tau_{pw} = \rho_w g \left(h_{s0} + h_{f0}\right) \cos\alpha \tan\varphi \end{gathered} \tag{34}$$

Relationship (33), has been validated numerically for the Dyatlov case parameters using the Material Point Method (see SM4).

Time to failure

The time to failure is a function of the area A_w of the wind transported snow at failure derived by integration of Eq. (9) and of the snow deposition rate Q, which is a function of the average wind speed $\langle v \rangle$:

$$\Delta t = \frac{\rho_w A_w}{Q(\langle v \rangle)}; \quad A_w = \frac{1}{3} h_{w0} L_0 \left(1 - \left(1 - \frac{l_c}{L_0}\right)^3\right) \tag{35}$$

where the small area of new snow between l_c and l_{cw} in Figure 1A has been neglected. After the wind transported snow sinters, it causes the thickening of the original slab, which constrains the slab displacements and slows down the growth of shear stresses in the weak layer. Therefore, if the snow sinters immediately, the peak strength in the weak layer is reached slower. It follows that the largest amount of the wind transported snow $h_{w0,max}$ required for the avalanche release can be calculated assuming that the snow sinters instantaneously over the entire thickness h_w, i.e. $h_{f0} = 0$. The upper bound of delay can then be expressed as

$$\Delta t_{max} = \frac{\rho_w h_{w0,max} L_0}{3 Q(\langle v \rangle)} \left(1 - \left(1 - \frac{l_c}{L_0}\right)^3\right) \tag{36}$$

where $h_{w0,max} = h_{s0}$ is determined by solving numerically Eq. (33), with stresses and forces s[illegible]ses and forces substituted from Eqs. (34):

$$h_{w0,max} = h_{s0} = h_0 \frac{\rho}{\rho_w} \frac{\tan\varphi - \tan\varphi_{min}}{\tan\varphi_{max}(\lambda_s) - \tan\varphi} \tag{37}$$

where

$$\tan\varphi_{min} = \tan\alpha - \frac{c}{\rho g h_0} \sqrt{1 + \tan^2\alpha} + r_1 \frac{K_0 h_0}{2 L_0} \tag{38}$$

$$\tan\varphi_{max}(\lambda_s) = \frac{r_3}{r_3+2}\tan\alpha\,;\quad r_3 = \frac{1+\sqrt{1+4\lambda_s}}{2};\quad \lambda_s = \frac{\lambda_0 h_0}{h_0+h_{s0}}$$

Using the same l[illegible]s, it follows that the smallest amount of the wind transported snow required for the avalanche release $h_{w0,min}$ can be calculated assuming that the new snow does not sinter. Substituting $h_{s0} = 0$ into Eqs. (37)-(38) gives the equation for the lower bound of the thickness of the wind deposited snow at which the slab will fail:

$$h_{w0,min} = h_{f0} = h_0 \frac{\rho}{\rho_w} \frac{\tan\varphi - \tan\varphi_{min}}{\tan\varphi_{max}(\lambda_0) - \tan\varphi} \tag{39}$$

where

$$\tan\varphi_{min} = \tan\alpha - \frac{c}{\rho g h_0}\sqrt{1+\tan^2\alpha} + r_1\frac{K_0 h_0}{2L_0}$$
$$\tan\varphi_{max}(\lambda_0) = \frac{r_1}{r_1+2}\tan\alpha\,;\; r_1 = \frac{1+\sqrt{1+4\lambda_0}}{2} \tag{40}$$

which substituted into Eq. (35) gives the lower bound for the time to failure:

$$\Delta t_{min} = \frac{\rho_w h_{w0,min} L_0}{3Q(\langle v\rangle)}\left(1-\left(1-\frac{l_c}{L_0}\right)^3\right) \tag{41}$$

For a delayed release to be possible, two conditions have to be satisfied. The first one is that the slab does not fail immediately after the cut, which according to Eq. (24), after substitution of forces and stresses from Eq. (34) imposes the following restriction on friction:

$$\tan\varphi_{min} < \tan\varphi \tag{42}$$

where $\tan\varphi_{min}$ is defined in Eq. (38). For Eqs. (37) and (39) this implies that the finite positive time of delay is only possible when:

$$\tan\varphi_{min} < \tan\varphi < \tan\varphi_{max}(\lambda_s) \le \tan\varphi_{max}(\lambda_0) \tag{43}$$

where $\tan\varphi_{max}$ is defined in Eqs. (38) and (40).

The Dyatlov case

The following parameters for the Dyatlov case have been adopted (see SM6):

$$\alpha = 28°;\; h_0 = 0.5\text{m};\; h_c = 0.1\text{m};\; h_p = 0.5\text{m};\; l_p = 0.5\text{m};$$

$$l_c = 4\text{m (21.6° slope at the cut)};\; l_{e0} = 1.0\text{m};\; g = 9.81\text{ m s}^{-2}$$

$$\rho_w = 400\text{ kg m}^{-3};\; \rho = 300\text{ kg m}^{-3};\; K_0 = 0.5$$

A reasonable value of the friction in the weak layer[20] (see the SM6) is $\varphi = 20°$. This sets the upper bound of cohesion, for which a finite $h_{w0,max}$ can still be determined, at $c \approx 440$ Pa. This is well within the range reported by Jamieson and Johnson[33], and from Eqs. (37)-(40) we obtain

$$h_{w0,min} = 0.24\text{m};\ \ h_{w0,max} = 0.44\text{m}$$

By adjusting the flux rate to $Q = 0.008\ \text{kg m}^{-1}\ \text{s}^{-1}$, from Eqs. (36) and (41) we determine

$$\Delta t_{min} = 7.2\ \text{hours};\ \ \ \Delta t_{max} = 13.5\ \text{hours}$$

which overlaps with the forensically estimated range of $\Delta t = 9.5 - 13.5$ hours (see SM1). Extensive research on snow accumulation shows[15] that this deposition flux requires average wind velocities in the range of 2–12 m s^{-1} which is in agreement with data from nearby weather stations on the night of the accident (see SM3). The possible range of friction angles in Eq. (43) leading to delayed slab release is shown in Figure 2A as a function of the slope inclination. Figure 2B shows the corresponding range of the critical heights of the wind transported snow load that cause a delayed release for two cases: with and without sintering, described by Eq. (37) and Eq. (39), respectively. Using Eq. (35), the times of delay are plotted as a function of the height of the wind transported snow in Figure 2C.

Dimensions of the failed slab

After the shear fracture propagated into the weak layer, the slab may fail in tension at the distance l_t from the cut, over the width B along the cut. Here we determine the width B of the failed slab, assuming that the tension crack formed at the boundary of the wind transported load defined in Eq. (9):

$$l_t = l_{cw} = L_0\left(1 - \sqrt{\frac{h_c}{h_0 + h_{s0} + h_{f0}}}\right)$$

where the thickness of the slab is h_c. The areas A_0 and A_s of the initial and sintered slab cross-sections between the cut and the crack are derived by integration of Eqs. (7) and (9), respectively:

$$\begin{aligned} A_0 &= \frac{1}{3}h_0 L_0\left(1 - \left(1 - \frac{l_c}{L_0}\right)^3\right) + h_c(l_{cw} - l_c) \\ A_s &= \frac{1}{3}(h_0 + h_{s0} + h_{f0})L_0\left(1 - \left(1 - \frac{l_{cw}}{L_0}\right)^3\right) \end{aligned} \quad (44)$$

and the equilibrium at the onset of the tension crack formation is given by

$$(A_0\rho + (A_s - A_0)\rho_w)Bg\cos\alpha(\tan\alpha - \tan\varphi) = h_c B\sigma_t + 2l_{cw}h_c\sigma_s \tag{45}$$

In Eq. (45) the left side represents the net driving force acting on the slab, while the right side is the sum of the tensile resistance at the back of the slab and the shear resistance at the sides of the slab, with σ_t and σ_s are the tensile and shear strength, respectively. It is assumed that the contribution of the wind transported snow to side resistance can be neglected, and the slab is thinning at the sides to h_c. Equation (45) can be rewritten to determine the width of the failed slab B:

$$B = \frac{2l_{cw}h_c\sigma_s}{(A_0\rho + (A_s - A_0)\rho_w)g\cos\alpha(\tan\alpha - \tan\varphi) - h_c\sigma_t} \tag{46}$$

For the parameters of the Dyatlov case:

$\varphi = 20°$; $\sigma_t = 6.0\text{kPa}$; $\sigma_s = 5.2\text{kPa}$; $l_c = 4.0\text{m}$; $h_c = 0.1\text{m}$; $h_0 = 0.5\text{m}$; $h_{w0} = 0.5\text{m}$

the tension crack can form at $l_t = l_{cw} = 4.95\text{m}$ resulting in $B = 8.8\text{m}$, consistent with the observed B/l_t ratios[34] and comparable to the width of the cut $b = 6.5 - 7.5\text{m}$ (for a 4.5 m long tent, the cut had to be around 1.0-1.5m longer on each side).

Modeling the impact of the avalanche using the Material Point Method

We simulate the snow slab dynamics and snow block impacts using the model recently developed by Gaume *et al.*[19]. This model uses the Material Point Method, finite strain elastoplasticity and a Cohesive Cam Clay constitutive relationship to simulate snow and avalanche mechanics. We recall here the main characteristics of the model.

Numerical method

The mass and momentum balance equations are solved using the Material Point Method (MPM)[35] and finite strain elasto-plasticity. MPM is a hybrid Eulerian-Lagrangian method which is well suited to study problems involving large deformations, collisions and fractures. It is thus suitable to study the impact of a snow slab on a human body. "Particles" are used to track position, velocity and deformation gradients and a fixed background mesh is used to solve the balance equations. Here we used the Affine Particle In Cell (APIC) method for the particle to grid and grid to particle transfer[36] which allows an exact conservation of momentum and angular momentum. In addition, we use cubic B-splines as shape functions which guaranties the continuity of the gradients at the nodes. More details about the MPM model and finite strain elastoplasticity can be found in Gaume *et al.*[19].

Snow model

A mixed-mode yield surface[20] defined in the space of the p-q invariants of the stress tensor is used. The pressure p is defined as $p = -\mathrm{tr}(\boldsymbol{\tau})/3$ where $\boldsymbol{\tau}$ is the Kirchhoff stress tensor. The von Mises equivalent stress q is defined as $q = (3/2\,\mathbf{s}\!:\!\mathbf{s})^{1/2}$ with $\mathbf{s} = \boldsymbol{\tau} + p\mathbf{I}$ the deviatoric stress tensor (**I**: identity matrix). The cohesive Cam Clay yield surface is given by:

$$y(p, q) = (1 + 2\beta)q^2 + M^2(p + \beta p_0)(p - p_0) = 0, \tag{47}$$

in which p_0 is the pre-consolidation pressure, M is the slope of the critical state line and β controls the resistance to tension. At the end of the stress increment, if $y(p, q) < 0$ the material is elastic and follows Hooke's law (with a Young's modulus E and Poisson's ratio ν) implemented in the framework of hyperelasticity with a St Venant – Kirchhoff model with Hencky strain[37].

If the snow fails, hardening or softening is simulated by expanding or shrinking the yield surface, respectively, through variations in p_0. Compression leads to hardening, promoting compaction, while tension leads to softening, promoting fracture. The hardening law is formulated as a function of the volumetric plastic strain ϵ_V^P according to:

$$p_0 = K\sinh\big(\xi\max(-\epsilon_V^P, 0)\big), \tag{48}$$

in which K is the bulk modulus and ξ is the hardening factor.

Human body

We model the human body as an hyperelastic solid (St Venant – Kirchhoff model with Hencky strain[37]). The human body is simulated as a bulk solid with material properties taken from thorax impact tests performed by the automotive industry[18]. This approach allowed us not to simulate all individual bones and organs of the human body.

Geometry of the model

The geometry of the slope is taken to be the same as in the analytical model (Fig. 3). The bed surface satisfies a slip boundary condition. The thinning snowpack and the size of the wind transported slab was also implemented according to the analytical model (see section 1 of the Methods). The human body of a male of average size (1.70 m) lying on its back was simulated on a rigid bed surface.

Simulation procedure

In order to calibrate our model, we first simulate the impact of a 10 kg rigid block (0.15 x 0.15 x 0.06 m) moving with velocity 7 m/s on a 3D human thorax with restrained back (see SM5 and Supplementary Video 3). This setup corresponds to the automotive crash experiments performed by Kroell *et al.*[18]. We adjust the elastic modulus of the body in order to reproduce the same maximum normalized deflection of 0.49 that resulted in fatal injuries. Next, we perform a high resolution 2D avalanche simulation using the main features known about the Dyatlov tent configuration (Fig. 1) so as to evaluate the snow slab impact velocity (up to 2 m/s, see SM5 and Supplementary Video 2) and the range of typical snow block dimensions (up to 0.5 m^3, see SM5 and Supplementary Videos 4-6). Next, we simulated the impact of snow blocks of 0.125, 0.25, and 0.5 m^3, 400 kg/m^3 density and impact velocity 2 m/s (same mechanical properties as the wind-slab) on a human thorax with restrained back. Finally, the severity of injuries was determined by relating the maximum normalized deflections to the Abbreviated Injury Scale (AIS, see SM6) presented by Kroell *et al.*[18].

Dyatlov case parameters

The following parameters for the Dyatlov case have been adopted (see SM6):

$$\alpha = 28°;\ \ h_0 = 0.5\,m;\ h_c = 0.1\,m;\ h_w = 0.5\,m;\ \ l_{wc} = 5.0\,m.$$

The simulations were performed with a background mesh size $dx = 0.005\,m$, 4 particles per element in 2D and 8 particles per element in 3D.

We used the following snow and bed surface mechanical properties for the Dyatlov case (see SM6):

$$\rho = 300\,\frac{kg}{m^3};\ E = 8\,MPa;\ \nu = 0.3;$$

$$\beta = 0.2;\ M = 0.8; p_0 = 30\,kPa\,; \xi = 5;$$

$$\rho_w = 400\,\frac{kg}{m^3};\ E_w = 17\,MPa; \nu_w = 0.3; \beta_w = 0.1;$$

$$M_w = 0.8; p_{0w} = 100\,kPa\,; \xi_w = 5;$$

$$\varphi_{bed} = 22°$$

As mentioned above, mechanical properties of the human body are chosen based on thorax impact tests performed on human cadavers with restrained back[18] to match the corresponding maximum deflection of 0.49 obtained upon impact of a 10 kg rigid mass with 7 m/s impact velocity. We used the following elastic modulus and Poisson's ratio (see SM6):

$$E_{hb} = 0.215\ MPa;\ \nu_{hb} = 0.35;$$

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Authors contributions

J.G. conceived the key idea of the study, performed the numerical analysis and co-wrote the manuscript. A.M.P. developed the analytical model of delayed avalanche release and co-wrote the manuscript.

Data availability

All data needed to evaluate the conclusions in the paper are present in the paper and the electronic supplementary material. Data supporting the plots of the manuscript are available from the corresponding author upon request.

Code availability

The code necessary to compute the delay for slab avalanche release and avalanche size can be found at https://doi.org/10.5281/zenodo.4088052. Description of the MPM model can be found in a previous publication at https://www.nature.com/articles/s41467-018-05181-w.

Competing interests

The authors declare no competing interests.

Additional information

Supplementary Information is available for this paper. Correspondence and requests for materials should be addressed to Alexander Puzrin.

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J. Gaume and A. M. Puzrin
Mechanisms of slab avalanche release and impact in the Dyatlov Pass incident in 1959

Authors response to reviewer comments

Reviewer #1 comments

The authors would like to thank the referee for additional comments which helped us to improve our revised paper. We are pleased by the positive evaluation of our revised manuscript and the acceptance recommendation.

Comment

1) The authors have taken into account my previous comments and have managed to simplify and evaluate the analytical model, which makes the methodology clearer and sound.

However, the answer to one of my comment (n° 1) is not fully convincing. I questioned the use of the term "delay" in the title and throughout the paper. In this paper, the authors refer to delay as the time between the cut in the snowpack and the avalanche release. This might be misleading since the triggering factor is snow accumulation and not the cut in the slope. If the mountaineers would have made no cut in the slope, a progressive loading by snow accumulation would still potentially trigger an avalanche during the night after they set up. Here, the snowpack changes in time so the release probability also evolves. If we caricature, one may as well define a delay between the beginning of the winter season and the avalanche release... I still understand that the Dyatlov incident lies in a narrow range of snow conditions (stable enough enough so that the avalanche does not release when cutting a pit, instable enough so that limited accumulation leads to snow failure) but this needs proper clarification in the text.

For instance, in the abstract, the words "a physical mechanism for a delayed slab avalanche caused by progressive snow accumulation" are misleading as the delay is not defined yet and one may expect a new complex mechanism, which is, in the end, the well known loading by wind-drifted snow. I suggesting defining the delay directly in the abstract. Moreover, the paper is not all about "this delay" but also explaining the limited injuries, the relatively low slope in the release area, the role of the parapet in snow accumulation. So I would reword the title to "The mechanism of the Dyatlov Pass avalanche", which does not reduce the interest and quality of the paper.

Except this comment, and to my opinion, the paper can be accepted as it is.

Pascal Hagenmuller

Response

In line with the reviewer's concern and suggestions, we removed the term "delay" from the title and from the abstract. The title of our paper has been modified (also following editorial suggestions) to: "The mechanisms of slab avalanche release and impact in the Dyatlov Pass incident in 1959".

Reviewer #2 comments

The authors would like to thank the referee for additional detailed remarks and corrections which helped us to improve our revised paper. We are pleased that the reviewer appreciated the significant modifications we made to our paper, in line with the previous comments of both referees.

Comment

Due to shortage of time, I have not checked all equations in detail and not looked at the Supplementary Materials. I am nevertheless positive that the revised manuscript represents a major improvement over the original version. It is in particular much easier to understand, and the geometrical assumptions made in the analytical model are now reasonably realistic. Concerning the length of the delay from the cut in the slope to the slab release, I welcome that the authors no longer claim that they have reproduced the delay established by the forensic investigation, but instead show that this delay requires a reasonable snow deposition rate in their model.
Please see the attached annotated manuscript for minor comments, language points and spelling corrections.
I recommend accepting the manuscript for publication with small corrections.
Dieter Issler

Response

We modified our paper to take into account all the minor corrections from the annotated manuscript. In particular, we brought more bibliographic information about some Russian books and references and we brought our equations to the standards of Communications journals. In addition, we modified some sentences of the paper, highlighted by the reviewer, to better reflect our new approach, i.e. back calculation of the wind deposition flux based on the forensic estimation of the delay between the time of the cut and the time of the avalanche.