

Author response to the review comments of Referee #1

Manuscript: Mobility of dry granular flows over contrasting erodible bed materials in laboratory experiments

We sincerely thank Referee #1 for the careful evaluation of our revised manuscript and for acknowledging that the comments raised during the first review round have been adequately addressed. We greatly appreciate the referee's constructive feedback throughout the review process, which helped us to improve the clarity, terminology, and overall presentation of the manuscript.

Author response to the review comments of Referee#2

Manuscript: Mobility of dry granular flows over contrasting erodible bed materials in laboratory experiments

Dear Referee #2,

Thank you for taking the time to review our revised manuscript and for providing your detailed comments. We have carefully considered the concerns raised in your report, particularly regarding the theoretical framing of the study and the extent to which the experimental design allows the effects of individual material properties to be distinguished. In revising the manuscript, we have also taken into account the assessment of the editor and the additional review by Referee #3, and have further broadened the theoretical and experimental context while maintaining the experimental focus of the study. Where appropriate, we have revised the manuscript to clarify the scope of the experiments and to distinguish more clearly between the original motivation of the experimental design and the interpretation supported by the experimental observations. In the following, we provide the review comments and our corresponding responses.

With kind regards,
The authors

[Ref#2] 1. The mechanical framework is incorrect

I am concerned that the authors still appear to regard the issue with Pudasaini and Krautblatter (2021) (hereafter PK) as a disagreement between different possible formulations of entrainment mechanics. This is evident from statements in the rebuttal such as “alternative mechanically based formulations suggested by the reviewer” and “competing mathematical derivations of erosion induced

momentum exchange”. This characterization is incorrect.

What I presented in my RC2 is Newton’s second law applied to an entraining system. The corresponding depth-averaged momentum balances were rigorously derived long before PK, e.g., by Norem and Schieldrop (1991), Fraccarollo and Capart (2002), Iverson (2012), and Iverson and Ouyang (2015). These are not “alternative” formulations competing on an equal footing with PK. Rather, PK introduced an alternative derivation that double-counts the particle-borne momentum flux associated with entrainment. Issler et al. (2024) demonstrated that the resulting model violates momentum and energy conservation. Once the fundamental errors in PK are corrected, the PK equations revert to the established formulation that had already been known for decades (i.e., Norem and Schieldrop, 1991; Fraccarollo and Capart, 2002; Iverson, 2012; Iverson and Ouyang, 2015).

The revised manuscript nevertheless states that “Subsequent discussion addressed mainly the specific kinematic assumptions of the model, particularly the velocity assigned to erodible bed material (Issler et al., 2024), rather than the potential mechanical role of density contrasts.” This sentence seriously misrepresents the issues identified by Issler et al. (2024). The criticism is not about a “specific kinematic assumption”. The erroneous factor of two derived in the PK model causes the resulting formulation to violate momentum and energy conservation, and the density dependent mobility conclusions follow directly from this error.

[A] We thank the referee for this clarification. We would first like to emphasize that the present experimental study does not aim to assess or resolve the general validity of the theoretical formulation of Pudasaini and Krautblatter (2021). As also noted by the editor and Referees #1 and #3, the experimental results presented here are independent of this theoretical debate. Pudasaini and Krautblatter (2021) provided the original theoretical motivation for systematically varying flow--bed solid-particle-density contrasts, whereas the interpretation of our experiments is based on the observed flow

and deposit characteristics and considers a broader range of material properties and processes, including those highlighted by the referee during the first review round.

We do not question Newton's second law or the established depth-averaged mass and momentum balances cited by the referee. On the contrary, following the referee's comments during the first review round, we broadened the manuscript to include established mass- and momentum-conserving entrainment formulations, including Iverson (2012) and Iverson and Ouyang (2015). We also acknowledge the referee's point that, when the disputed additional momentum contribution in Pudasaini and Krautblatter (2021) is removed, the resulting momentum balance reduces to the established formulation. Our intention was therefore not to present these formulations as competing alternatives to fundamental conservation principles.

We nevertheless agree with the referee that our previous description of the criticism by Issler et al. (2024) as concerning mainly a "specific kinematic assumption" was too narrow. We have therefore revised this passage. The manuscript now states that the momentum formulation underlying the proposed density-dependent mobility response was criticized by Issler et al. (2024), who argued that it is inconsistent with momentum and energy conservation. We further clarify that resolving this theoretical debate is outside the scope of the present study. Instead, the experiments examine how contrasting bed materials affect erosion, entrainment, and mobility, with particular attention to the role of solid particle density alongside other material properties.

[Ref#2] 1. The mechanical framework is incorrect

[...] Furthermore, I am very surprised by the authors' statement that: "This motivation is broader than the specific mathematical point criticized in relation to the factor 2 applied to the bed velocity term." The authors seriously misrepresent the role of this term in the PK paper that they so strongly defend and support. It is precisely the erroneous derivation of this term in the momentum conservation that generates the mobility regimes and density dependence on which the present experimental design is based and interpreted. [...]

[A] We agree that our previous wording was imprecise. We did not intend to suggest that the density-dependent mobility regimes proposed by Pudasaini and Krautblatter (2021) are independent of the disputed momentum term. Rather, our point concerned the broader value of the experimental design and dataset once the experiments were conducted. Although Pudasaini and Krautblatter (2021) originally motivated the systematic variation of flow-bed solid-particle-density contrasts, the resulting experiments provide observations of erosion, entrainment, flow evolution, and deposition that are independent of any particular erosion model. As also emphasized by the editor and Referee #3, the experimental results therefore do not depend on the validity of the Pudasaini and Krautblatter (2021) formulation and may instead provide useful benchmark data for constraining different mathematical models of erosion and deposition.

We have made this distinction clearer in the revised manuscript. In particular, we no longer characterize the experimental motivation as being broader than the disputed mathematical term. Instead, Pudasaini and Krautblatter (2021) is identified only as the theoretical motivation that led us to systematically vary flow-bed solid-particle-density contrasts. The interpretation and broader relevance of the resulting dataset are separated from that original motivation and are discussed in the context of established depth-averaged entrainment formulations, hysteretic erosion-deposition approaches, and non-depth-averaged continuum models.

[Ref#2] 1. The mechanical framework is incorrect

[...] The three density-dependent mobility regimes are therefore direct consequences of the erroneous factor of two (i.e., flawed momentum conservation) identified by Issler et al. (2024).

Once this error (and the error concerning the applicable boundary shear stress discussed in my CC2) are corrected, the basis for the three PK mobility regimes disappears and the momentum balance reverts to the established formulation discussed in, e.g., Iverson (2012), Iverson and Ouyang

(2015), and Issler et al. (2024). It is therefore incorrect to argue, as the authors do in their rebuttal, that the density-contrast motivation is somehow broader than the “specific mathematical point” concerning this factor of two.

Importantly, this does not mean that density contrasts cannot be considered within a mechanically correct depth-averaged entrainment framework. Their role is already incorporated in the rigorously derived mass and momentum balances of Iverson and Ouyang (2015), to which the authors may instead refer if they wish to discuss the possible influence of flow–bed density contrasts. The same point is also illustrated by the simple mass-block toy model that I presented in RC2, which follows directly from Newton’s second law and shows how density enters the dynamics without introducing the erroneous PK derivation. [...]

[A] We agree with the referee that density contrasts can be considered within established mass- and momentum-conserving entrainment frameworks, including Iverson (2012) and Iverson and Ouyang (2015). We would, however, like to clarify an important distinction concerning the role of Pudasaini and Krautblatter (2021) in the present study. Their work explicitly highlighted the possibility that differences in the grain density of the moving and entrained material may be associated with changes in mobility. This provided the original motivation for systematically varying flow-bed solid-particle-density contrasts in our experiments. The low-, equal-, and high-density scenarios used in the present manuscript should not be confused with the density-dependent mobility regimes proposed by Pudasaini and Krautblatter (2021). Our scenarios are descriptive categories of the experimental design, defined solely by whether the solid particle density of the erodible bed is lower than, equal to, or higher than that of the released Qtz slide. They do not imply a prescribed increase, absence of change, or decrease in mobility. Indeed, the experimental results show that such a direct mapping is not supported by the observed macroscopic response.

[Ref#2] 2. The experiments do not isolate the influence of bed density

The manuscript remains too strongly framed around flow–bed density contrasts. For example, the Abstract states:

“Here, we conducted dry granular flow experiments to investigate how contrasts in solid particle density between a moving mass and an erodible bed affect erosion, flow evolution, and deposition.”

The Introduction describes the experiments as “controlled density contrast scenarios”, Section 2.1 is entitled “Targeted density contrast scenarios and materials”, and the Conclusions begin by stating that the study “experimentally investigated the influence of solid-particle-density contrasts”. These statements go beyond what the experimental design can demonstrate. The fact that solid particle density was the intended experimental variable does not make it an experimentally isolated variable. The Glass, Quartz, and Magnetite beds differ not only in density, but, more importantly, in frictional properties. The Glass beds have substantially lower internal friction, precisely in the direction expected to facilitate bed erosion and flow mobility. The enhanced erosion and mobility observed over Glass therefore cannot be attributed to its lower density. Because density and friction vary simultaneously, the experiments do not isolate their individual effects. This is particularly evident in Fig. 1. Quartz and Magnetite differ by approximately a factor of two in bulk density, yet exhibit nearly identical travel angles, whereas the markedly different behaviour of Glass coincides with its substantially lower internal friction. The revised manuscript itself now states that:

“Among the accompanying material properties, bed internal friction provides the most plausible explanation for the contrasting erosion and entrainment patterns.”

I agree. However, if friction provides the most plausible explanation for the observations, it cannot simultaneously remain an “accompanying” secondary property while density remains the central physical interpretation.

For the same reason, the statement

“Likewise, the enhanced mobility over Glass is also qualitatively consistent with the mobility increase proposed by Pudasaini and Krautblatter (2021) for entrainment of

mechanically weaker bed material.”

should be removed. It reintroduces the PK density-contrast interpretation, but that interpretation is physically flawed. The Glass behaviour is instead consistent with its lower bed friction and mechanical resistance. Likewise, the statement that

“The experimental design was motivated by Pudasaini and Krautblatter (2021), who considered density contrast as a potential control on erosion-induced mobility changes.” should be removed..

[A] We agree with the referee that solid particle density was not experimentally isolated from the other material properties. We have therefore revised the Abstract, Introduction, Sect. 2.1, Discussion, and Conclusions to avoid wording that could imply an isolated causal effect of density. The experiments are now described as systematically imposed density-contrast scenarios, and we explicitly state that differences in friction, particle shape, and bed deformation must be considered in their interpretation.

We also revised the Discussion to clarify that bed internal friction provides the most plausible explanation among the material properties considered here. However, friction itself was not independently varied, and we therefore avoid attributing the observed response uniquely to either density or friction. Likewise, the response of the low-density Glass scenarios, considered alone, appears qualitatively consistent with the mobility response proposed by Pudasaini and Krautblatter (2021), but this interpretation is not supported by the complete dataset, particularly because the high-density Mag beds show no corresponding mobility reduction relative to Qtz.

We retain Pudasaini and Krautblatter (2021) only as the original theoretical motivation for systematically varying flow-bed solid-particle-density contrasts, consistent with the editor's assessment. The interpretation and broader value of the experimental dataset do not depend on this theoretical framework.

[Ref#2] 3. Recommendation

I want to reiterate that both Dieter Issler and I consider the experimental dataset interesting and potentially valuable. Neither of us has argued that the experiments themselves should be discarded. Instead, we made considerable effort in the open discussion and review stages to indicate how the manuscript could be salvaged and reformulated around correct mechanics. However, valuable experiments do not justify retaining a clearly incorrect interpretation. For the manuscript to become acceptable, the following changes are necessary:

- 1. Entirely remove Pudasaini and Krautblatter (2021) citations.*
- 2. Reframe the manuscript around contrasting erodible bed materials and their mechanical properties, rather than around the causal influence of flow–bed density contrasts.*

The manuscript therefore cannot be accepted in its current form. I would be willing to reconsider a substantially reframed version in which the incorrect PK framework is removed and the conclusions are restricted to what the experimental design can actually establish.

[A] As detailed in our responses above, we have substantially revised the manuscript to separate the original motivation of the experimental design from the interpretation of the experimental results. We agree that the experiments do not isolate solid particle density from all other material properties and have revised the Abstract, Introduction, Methods, Discussion, and Conclusions accordingly. The interpretation now explicitly considers the contrasting mechanical properties of the bed materials and restricts the conclusions to what can be established from the experimental observations.

We do not, however, remove all references to Pudasaini and Krautblatter (2021). In accordance with the editor's assessment, we retain a limited reference to this study as the original theoretical motivation for systematically varying flow-bed solid-particle-density contrasts, while clearly separating this motivation from the interpretation and conclusions of the present experiments..

[Ref#2] 4. Closing

I remain surprised that the authors continue to defend and cite PK as a physically legitimate framework, primarily on the grounds that it has been cited more than 200 times and has not (yet) been retracted. As explained in my reviews, in Issler et al. (2024), and as we have tried repeatedly to communicate at conferences and elsewhere, the PK derivation violates fundamental conservation laws. Once these errors are corrected, the equations revert to the established entrainment formulations developed in the earlier literature, and the claimed novelty disappears entirely. At this point, I strongly encourage the authors to reconsider the issue from first principles (i.e., Newton's second law) and to examine carefully the rigorous derivation by Iverson and Ouyang (2015). Continuing to promote PK as a valid (or possibly correct) alternative despite the demonstrated errors risks propagating a seriously flawed framework into landslide modelling. If such a formulation is eventually implemented in depth-averaged runout or hazard-assessment tools, the consequences could extend well beyond an academic debate and potentially affect the reliability of hazard predictions and public safety. Your responsibility here is therefore scientific rather than rhetorical: the interpretation should follow sound mechanics and experimental evidence, even when these do not support the hypothesis that originally motivated your study.

[A] We thank the referee for these concluding remarks. The points raised here have already been addressed in our responses above. Overall, in accordance with the editor's request, we have carefully considered the referee's final remarks and incorporated the relevant points to further improve and clarify the manuscript.

Author response to the review comments of Referee#3

Manuscript: Mobility of dry granular flows over contrasting erodible bed materials in laboratory experiments

Dear Referee #3,

Thank you for taking the time to review our manuscript and for your constructive and thoughtful feedback. We appreciate your positive assessment of the experimental design and the study's scientific value for the debris-flow and granular flow community. Our manuscript will certainly benefit from your comments, and the suggested clarifications will help us to further improve both the scientific interpretation and the clarity of the presentation. In the following we provide the review comments and our corresponding answers.

With kind regards,
The authors

[Ref#3] *This is an experimental paper describing the dynamics of granular materials as they flow down an incline over an erodible bed and out onto a flat runout pad. The aim is to investigate what effect the erodible bed has on the overall flow dynamics. The context is the paper of Pudasaini & Krautblatter (2021) who (if I understand the motivation correctly) predict that the erosive nature of the flow should be strongly dependent on the density difference between the parent flow and the erodible deposit. The problem of how to incorporate erosion and deposition into depth-averaged models for dry granular flows is a thorny one that many people have struggled with over the years, and there is still no consensus on how to do this. Indeed, Issler et al (2024) have claimed that the model of Pudasaini & Krautblatter (2021) violates energy conservation and is flawed. I won't get into this debate as part of my review -- as the experimental results in this paper don't depend upon it in any way. Indeed, perhaps the results will help to further constrain mathematical models in the future. I would recommend that the authors broaden the debate, as there are other approaches that seem to be moderately successful at modelling erosion and deposition in specific contexts that are not mentioned.*

[A] We thank the referee for this constructive assessment and, in particular, for emphasizing that the experimental results and their interpretation do not depend on the validity of the model proposed by Pudasaini and Krautblatter (2021). We agree that erosion and deposition in granular flows represent a broader modelling problem for which several different approaches have been developed. Following the referee's suggestion, we have therefore broadened the theoretical context of the manuscript and included additional experimental and modelling approaches to erosion and deposition in dry granular flows. We have aimed to incorporate these aspects comprehensively but concisely, thereby strengthening the theoretical context while retaining the experimental focus of the manuscript and avoiding a substantial increase in its length. These additions are described in detail in our responses to Comments 1-4 below.

[Ref#3] *1/. Incorporating basal erosion and deposition into depth-averaged avalanche/debris flow models is tricky. There should be more discussion about this. Early models ended up producing rocket like equations, where a deposit of mass, but no corresponding loss of momentum led to avalanches accelerating (Cannon and Savage, 1988; Hungr, 1990; Erlichson, 1991; Iverson 1997). It is also possible to set up equations in which there is no net acceleration or deceleration as material is entrained and deposited, see for instance the solutions for surface avalanches in rotating drums in Gray (2001). Some mention of this in the introduction would help to provide context.*

(a) Cannon, S. H., and W. Z. Savage, A mass-change model for the estimation of debris-flow runout, J. Geol., 96, 221–227, 1988.

- (b) Hungr, O., A mass-change model for the estimation of debris flow runout: A discussion, *J. Geol.*, 98, 791, 1990.
- (c) Erlichson, H., A mass-change model for the estimation of debris-flow runout, A second discussion: Conditions for the application of the rocket equation, *J. Geol.*, 99, 633–634, 1991.
- (d) Iverson, R.M. 1997 The physics of debris flows. *Rev. Geophys.* 35, 245–296.
- (e) Gray, J.M.N.T. 2001 Granular flow in partially filled slowly rotating drums. *J. Fluid Mech.* 441, 1–29.

[A] We thank the referee for pointing this out. We have added a short discussion of the early mass-change formulations and the associated rocket-equation problem. We further included Gray (2001) as an example showing that mass exchange through erosion and deposition can also occur without net acceleration or deceleration. This provides additional context for the erosion-deposition modelling approaches introduced in the following paragraph.

[Ref#3] 2/. Although the original motivation for the paper is erosion and deposition in debris flows, the title of the paper has now been changed to "Mobility of dry granular flows over erodible beds: Experimental insights into the influence of flow–bed density contrasts" and the experiments are performed solely with dry granular materials. There therefore needs to be a more thorough referencing of previous work on erosion and deposition in dry granular flows.

In particular, it has been shown that at low fluxes discrete mobile wave pulses can form (so called erosion-deposition waves) that are able to flow through a static erodible bed, but come to rest on a fixed substrate at the same inclination (Edwards & Gray 2015, Rocha et al 2019).

Moreover a discrete release onto an erodible substrate can (i) grow, (ii) propagate indefinitely or (iii) decay dependent on the thickness of the erosion layer and the bed inclination (Edwards et al 2017, Kokelaar et al. 2017, Viroulet et al. 2019).

Colour change experiments show that erosion-deposition waves can propagate indefinitely by eroding and depositing granular material in exact balance, i.e. a release of yellow sand onto red sand, produces a flow in which all the yellow sand deposits, but the wave propagates through the red sand indefinitely (Edwards et al 2021).

You can also get retrogressive failures that propagate upslope (Russell et al. 2019). Continuous releases of material at higher mass flux can also spontaneously self-channelize to form static lateral levees that prevent spreading and enhance runout (Mangeney 2007, Kokelaar et al 2014, Rocha et al. 2019). Although these experiments are in dry granular materials the same phenomena can also be seen in debris flows, see e.g. Aaron et al. 2025 and Wetter et al. 2026.

All of these experiments and field observations are helping to constrain depth-averaged models and should be mentioned. Indeed, all of these experiments are performed with erodible material that is identical to the release material, so I wonder whether this is saying anything useful about the forms used by Pudasaini & Krautblatter (2021)? What happens in that model when the densities are the same?

(a) Pouliquen, O. & Forterre, Y. 2002 Friction law for dense granular flows: application to the motion of a mass down a rough inclined plane. *J. Fluid Mech.* 453, 133–151.

(b) Mangeney, A., Bouchut, F., Thomas, N., Vilotte, J. P. & Bristeau, M. O. 2007 Numerical modeling of self-channeling granular flows and of their levee-channel deposits. *J. Geophys. Res.* 112, F02017.

(c) B. P. Kokelaar, R. L. Graham, J. M. N. T. Gray And J. W. Vallance (2014) Fine-grained linings of leveed channels facilitate runout of granular flows. *Earth Planet. Sci. Lett.* 385, 172-180

(d) A. N. Edwards & J. M. N. T. Gray (2015) Erosion-deposition waves in shallow granular free-surface flows. *J. Fluid Mech.* 762, 35-67

(e) A. N. Edwards, S. Viroulet, B. P. Kokelaar & J. M. N. T. Gray (2017) Formation of levees, troughs and elevated channels by avalanches on erodible slopes. *J. Fluid Mech.* 823, 278–315

(f) B. P. Kokelaar, R. S. Bahia, K. H. Joy, S. Viroulet & J. M. N. T. Gray (2017) Granular avalanches on the Moon: Mass-wasting conditions, processes, and features. *J. Geophys. Res. Planets* 122, 1893–1925.

(g) F. M. Rocha, C. G. Johnson & J. M. N. T. Gray (2019) Self-channelisation and levee formation in monodisperse granular flows. *J. Fluid Mech.* 876, 591–641

- (h) S. Viroulet, A. N. Edwards, C. G. Johnson, B. P. Kokelaar & J. M. N. T. Gray (2019) *Shedding dynamics and mass exchange by dry granular waves flowing over erodible beds*. *Earth and Planetary Sci. Lett.* 523, 115700.
- (i) A. S. Russell, C. G. Johnson, A. N. Edwards, S. Viroulet, F. M. Rocha & J. M. N. T. Gray (2019) *Retrogressive failure of a static granular layer on an inclined plane*. *J. Fluid Mech.* 869, 313–340.
- (j) A.N. Edwards, S. Viroulet, C. G. Johnson & J. M. N. T. Gray (2021) *Erosion-deposition dynamics and long distance propagation of granular avalanches*. *J. Fluid Mech.* 915, A9
- (k) J. Aaron, J. Langham, R. Spielmann, J. Hirschberg, B. McArde, S. Boss, C.G. Johnson & J.M.N.T. Gray (2025) *Detailed observations reveal the genesis and dynamics of destructive debris-flow surges*. *Commun. Earth Environ.* 6, 556.
- (l) Christoph Wetter, Fabian Walter, Brian W. McArde, Felix Blumenschein, Raffaele Spielmann, Jordan Aaron, J.M.N.T. Gray, Anne Obermann & Andreas Fichtner (2026) *Formation, propagation, and annihilation of surge waves in debris flows*. *Engineering Geology* 367, 108648.

[A] We thank the referee for this comment and for providing this extensive and very helpful set of references. We have expanded the Introduction to provide a broader overview of erosion and deposition in dry granular flows. The studies you mentioned provide important context for our experiments because they demonstrate different mobility responses even when the moving and erodible materials have identical properties.

Regarding the referee's question on identical flow and bed materials, Pudasaini and Krautblatter (2021) explicitly consider this case. They define equal flow and bed inertia as an "inertially neutral" condition. In their formulation, the momentum carried into the flow by the eroded material is balanced by the momentum required to incorporate the newly entrained mass into the moving flow. They therefore predict no net erosion-induced gain or loss of mobility for this condition. This result, however, relies on the momentum formulation questioned by Issler et al. (2024). Issler et al. argue that the momentum influx associated with entrainment is counted twice in the derivation, leading to the additional momentum contribution underlying the mobility criterion. The experiments cited by the referee are therefore particularly relevant, as they demonstrate substantial erosion-deposition dynamics even when flow and bed consist of identical material. A detailed assessment of the competing model formulations is beyond the scope of the present experimental study.

[Ref#3] 3/. *Introduction: It is probably worth pointing out that all of the above experiments have been modelled with a depth-averaged dry avalanche model that uses the hysteretic friction law of Pouliquen & Forterre (2002). Here explicit basal erosion/deposition terms are neglected in the mass and momentum balance equations and the material is assumed to be moving or static through its entire depth. The hysteresis allows static and dynamic regions to coexist, so you can model levees, erosion deposition waves and retrogressive failures etc. While this is not a panacea for all problems with erosion and deposition, it seems to work well for shallow flows where the erodible horizon of the substrate is well defined. A more general erosion-deposition closure needs to at least be able to model all the above scenarios mentioned in 2/. (above).*

[A] We thank the referee for pointing out this alternative modelling approach. We have expanded the Introduction to include depth-averaged avalanche models that incorporate hysteretic basal friction laws. We now explain that these models represent erosion and deposition through transitions between static and flowing states without explicit basal erosion--deposition source terms. We also note their application to erosion-deposition waves, self-channelization and levee formation, and retrogressive failures.

[Ref#3] 4/. *Introduction: The introduction should also mention another possibility -- that of non-depth-averaged models. My suspicion is that this is the only way to solve the erosion-deposition problem properly. However, one would have to say what the rheology of the flowing and erodible material was -*

- which is also problematic. Progress is being made in this regard, but I don't think that our understanding of the rheology of dry granular materials is sufficient to model all the phenomena in 2/. (above) in a non-depth-averaged way - let alone debris flows. However, simple flows in rotating drums and heaps (for example) have been modelled in this way (see Barker et al 2021, Maguire et al 2024, Blatny et al 2024, Lloyd et al 2025). In addition, Li et al. have used cohesion to model erosion-deposition wave formation in snow avalanches, which are also a form of granular flow. In these approaches, even defining where the interface between solid-like and fluid-like material is, is difficult, because there can be slow exponentially decreasing creep with depth (Komatsu et al. 2001) so it is not surprising that depth-averaged models are struggling, i.e. what do we really mean by the flow thickness and the depth-averaged velocity? Perhaps you can point some of this out?

- (a) T. Barker, M. Rauter, E. S. F. Maguire, C. G. Johnson & J. M. N. T. Gray (2021) Coupling rheology and segregation in granular flows. *J. Fluid Mech.* 909, A22.
- (b) E.S.F. Maguire, T. Barker, M. Rauter, C.G. Johnson & J.M.N.T. Gray (2024) Particle-size segregation patterns in a partially filled triangular rotating drum. *J. Fluid Mech* 979, A40.
- (c) L. Blatny, J.M.N.T. Gray & J. Gaume (2024) A critical state $\mu(I)$ -rheology model for cohesive granular flows. *J. Fluid Mech.* 997, A67.
- (d) H.A. Lloyd, E.S.F. Maguire, D. Mistry, G.K. Reynolds, C.G. Johnson & J.M.N.T. Gray (2025) On the formation of super-stable granular heaps. *J. Fluid Mech* 1002, A27.
- (e) Xingyue Li, Betty Sovilla, John Mark Nicholas Timm Gray & Johan Gaume (2024) Transient wave activity in snow avalanches is controlled by entrainment and topography. *Commun. Earth Environ.* 5, 77.
- (f) Komatsu, T.S., Inagaki, S., Nakagawa, N. & Nasuno, S. 2001 Creep Motion In A Granular Pile Exhibiting Steady Surface Flow. *Phys. Rev. Lett.* 86 (9), 1757–1760.

[A] We thank the referee for this suggestion and for providing additional references. We have expanded the Introduction to include non-depth-averaged continuum approaches based on constitutive rheologies. We now mention applications to rotating drums, granular heaps and collapse, as well as material-point simulations of erosion--deposition waves in snow avalanches. We also address the difficulty of defining the transition between flowing and apparently static material, particularly in the presence of slow creep with depth. This provides a useful contrast to the depth-averaged approaches discussed above.

[Ref#3] 5/. section 2.1: You say "Inertia describes the resistance of moving material to a change of its velocity in magnitude and direction. In continuum mechanics, inertia is, beside velocity, proportional to mass density for a given material volume. Consequently, debris-flow inertia can be modified by incorporating mass from an erodible bed with a different density, solid volume fraction, friction, or different velocity. To test the inertia-based mobility concept proposed by Pudasaini and Krautblatter (2021), we designed a setup for dry granular debris-flow experiments, where the dry debris flow overrides a dry erodible bed initially at rest. In the present study any change in inertia can therefore only arise from a change in density." I don't agree with that statement. You have an experiment in a gravitational field, so when you release the grains they accelerate, i.e. the velocity increases and there is a change in inertia. It therefore can not be true to say "any change in inertia can therefore only arise from a change in density." Doesn't that at least partially undermine the motivation behind your experiments?

[A] We thank the referee for this comment and agree that the statement would be too restrictive. We note that this comment appears to refer to the initially submitted version of the manuscript. The same issue was raised by the other referees during the first review round and was already addressed in the revised manuscript. In particular, the statement that any change in inertia could only arise from a change in density was removed. Changes in flow velocity and entrained mass also affect the flow dynamics and

cannot be excluded in our experiments. Section 2.1 therefore now describes the experiments as controlled density-contrast scenarios, with solid particle density as the primary design variable rather than the only factor affecting flow dynamics. The manuscript also no longer presents the experiments as a direct test of the inertia-based mobility concept of Pudasaini and Krautblatter (2021).

[Ref#3] 6/. section 120: You say "Accordingly, when the term "debris flow" refers to our tests, it specifically denotes these dry granular single-phase flows". This seems to be stretching the definition of debris flows too far. Why can't you just bill these as dry granular flow experiments? They are still interesting in their own right.

[A] We agree with the referee and now consistently describe the experiments as dry granular flow experiments. We note that this comment appears to refer to the initially submitted version of the manuscript. The terminology was also raised by the other referees and community comments during the first review round and was subsequently revised throughout the manuscript. In particular, the term "debris flow" is no longer used to describe our experiments, and the title was changed accordingly to "Mobility of dry granular flows over erodible beds: Experimental insights into the influence of flow--bed density contrasts". No further changes were therefore required in response to this comment.

[Ref#3] 7/. section 125: What is your plywood coated with?

[A] We have added a more precise description of the plywood surface in Sect. 2.2, specifying the phenolic-resin coating and distinguishing between the smooth film surface and the rough, screen-imprinted surface used in the experiments.

[Ref#3] 8/. section 265: You say "The reference experiments form elongated, well-confined lobes". It sounds as though the flow is starting to try and lay down levees (see Johnson et al 2012; Rocha et al. 2019)

[A] We thank the referee for this observation. Indications of incipient self-channelization and levee formation can indeed be recognized in several of our experiments, particularly for flows with longer runout, and are not restricted to the non-erosive reference experiments. We have therefore added this possible interpretation to Sect. 3.2.2 and included the suggested references to Johnson et al. (2012) and Rocha et al. (2019). We keep this interpretation tentative because levee formation was not specifically quantified and is only weakly expressed in the present experiments.

[Ref#3] 9/. section 270: You say "The Glass experiments produce elongated lobes with undulating surfaces, but their shape differs markedly between the fine and coarse debris flow." As well as levees the undulations are probably due to waves forming in your chute and then being brought to rest, see Edwards & Gray (2015).

[A] We thank the referee for suggesting this interpretation. We considered whether the undulating surfaces could reflect arrested erosion--deposition waves as described by Edwards and Gray (2015). However, our observations suggest a different origin. The undulations appear to develop during deposition through the interaction of the denser Qtz material with the lighter Glass particles that preferentially accumulate near the flow surface. We therefore prefer not to interpret these structures as erosion-deposition waves. As this depositional mechanism is not central to the present study and cannot be quantified with our measurements, we have not expanded the manuscript on this aspect.

[Ref#3] 10/. Figure 6 and section 290 & 295: You say "Lateral cross sections through the Qtz–Glass and Qtz–Mag deposits reveal distinct internal sorting and spatial patterns in both fine (Supplement Fig. S8) and coarse experiments (Fig. 6). The mixed slide–bed deposits show a finer-grained core that coarsens toward the deposit surface (cross-checks of the Qtz–Qtz and reference deposits confirmed comparable sorting). The sorting is particularly evident in the coarse experiments, where the particle-size spectrum is wider. The material distribution, however, differs between the bed materials. In the Glass experiments (Fig. 6a and Supplement Fig. S8a), a sharp boundary occurs between the two materials. Qtz–slide particles are concentrated in the interior of the main body, while Glass-bed particles form the lateral edges. With increasing runout distance, the lateral Glass edges widen and the central Qtz core narrows. In the intermediate sections, Glass particles appear laterally shouldered onto a thin Qtz layer and the central Qtz core becomes disrupted by distinct Glass ridges. Toward the distal end of the main body, the deposits consist almost entirely of Glass. In the fine deposit (Glass/f; Supplement Fig. S8a), Glass also appears centrally along the deposit base. In the coarse deposit (Glass/c; Fig. 6a), the frontal meter consists of a fringe mainly composed of loosely dispersed coarse Glass with only a few Qtz particles." You have some nicely segregated deposits. I was a little unclear whether this was being attributed to particle-size segregation or particle-density segregation? Some of the mixtures shown in figure 1 appear to have quite wide size distributions? Which mixtures are these?

[A] Both grain-size and material segregation occur in our experiments. We have revised Sect. 3.2.2 to distinguish these two patterns more clearly. The finer-grained core and coarsening toward the deposit surface are attributed to grain-size segregation, which is particularly evident in the coarse-particle experiments with the wider 0.25–8 mm particle-size distribution. In contrast, the spatial separation of Qtz and Glass or Mag is interpreted as material segregation related to differences in particle density and shape. We have clarified this distinction in the description and interpretation of the internal deposit structure.

[Ref#3] 11/. section 330: You say "Finally, consistent with the expectation by Pudasaini and Krautblatter (2021), our experiments show enhanced mobility and extended runout when the debris flows entrain material from the low-density beds in the inertially weak scenarios, relative to the inertially neutral scenarios. However, the entrainment of significantly denser bed material does not result in the anticipated mobility reduction in the inertially strong scenarios under the experimental conditions considered here. Instead, both equal- and high-density beds lead to comparable deceleration of the debris flow, suggesting similar and higher energy consumption and momentum loss during bed erosion and entrainment compared to the low-density material." The experiments of Edwards et al (2017), Kokelaar et al. (2017), Viroulet et al. (2019) show that the mobility of the flow can be enhanced or diminished dependent on the erodible bed depth, the angle of slope inclination and the size of the avalanche even with parent and erodible material of the same bulk density. Doesn't this also contradict the results expected by Pudasaini and Krautblatter (2021)?

[A] We thank the referee for this comment. We note that the quoted passage refers to an earlier version of the manuscript. Following the first review round, we substantially revised this part of the Discussion and no longer frame the observed behavior in terms of inertially weak, neutral, and strong regimes. We agree that the experiments of Edwards et al. (2017), Kokelaar et al. (2017), and Viroulet et al. (2019) provide important evidence that erosion-induced mobility can increase or decrease even when flow and bed consist of identical material. These studies demonstrate additional controls through slope inclination, erodible-bed thickness, and released volume. Whether these observations directly contradict the specific formulation of Pudasaini and Krautblatter (2021) cannot be assessed from these experiments alone, as they were not designed to test that model and systematically vary different control parameters. Likewise, these parameters were not systematically varied in our experiments, where slope inclination, erodible-bed thickness, and initial released mass were kept constant. However, we have added these studies to Sect. 4.1.1 to support our broader interpretation that density contrast alone does not determine erosion-induced mobility.

[Ref#3] 12/. section 335: You say "Consequently, when considering only externally observable flow and deposit characteristics, and not intrinsic process dynamics, our results based on density contrasts alone do not provide sufficient support for the inertia-based mobility hypothesis proposed by Pudasaini and Krautblatter (2021)." I think this is an interesting conclusion.

[A] We thank the referee for this positive assessment. Following the first review round, we have further developed this conclusion and reduced its direct framing in terms of the inertia-based mobility hypothesis of Pudasaini and Krautblatter (2021). The revised Discussion now emphasizes more generally that solid-particle-density contrast does not map uniquely onto mobility and that the observed response reflects coupled effects of flow-volume growth, bed resistance, bed deformation, and entrainment efficiency. No additional changes were made in response to this comment.

[Ref#3] 13/. section 365: In the reference list (Johnson et al., 2012; Edwards et al., 2023) you should also probably add references to

(q) Segregation-induced fingering instabilities in granular free surface flows.

M. J. Woodhouse, A. R. Thornton, C. G. Johnson, B. P. Kokelaar & J. M. N. T. Gray (2012) *J. Fluid Mech.* 709, 543-580

[A] We have added Woodhouse et al. (2012) to Sect. 3.2.2, together with Johnson et al. (2012) and Edwards et al. (2023), to support the discussion of grain-size segregation and the preferential transport of coarse particles toward the flow surface, front, and margins.