Author response to Referee #2, Dr. Hervé Vicari

Manuscript: Mobility of dry granular debris flows over erodible beds: Experimental insights into the influence of flow–bed inertia

Erosion is an important process in both dry and water-rich landslides, as it can strongly affect volume and momentum growth. Although several erosion experiments have been carried out in the past, the present study provides a new dataset for dry granular flows by varying the composition of the erodible bed. To the best of my knowledge, dry granular flow experiments involving three contrasting bed material compositions have not previously been reported in this way. This is therefore welcome. The experimental dataset itself is interesting and potentially valuable. In particular, the analysis of deposit morphology is insightful, and the dataset may also prove useful as a benchmark for advanced numerical models.

However, the present framing and mechanistic interpretation of the experiments are incorrect, as I already explained in my community comment CC2 and as discussed in Issler et al. (2024). In Section 1 below, I briefly restate why the current interpretation cannot be maintained and indicate how it could be amended. In Section 2, I discuss the major changes that are required to reframe the manuscript. In Section 3, I provide more specific and line-by-line comments. Given the extent of the necessary reframing, I recommend major revisions and a further round of review before the manuscript can be reconsidered. My condition for further consideration is that the authors fully remove their reliance on the fundamentally incorrect model of Pudasaini and Krautblatter (2021) and instead frame and interpret the experiments using a mechanically sound framework.

Dear Dr. Vicari,

Thank you for taking the time to review our manuscript and for providing such a detailed, thorough, and constructive assessment. We greatly appreciate the considerable effort you invested in evaluating the manuscript and in formulating suggestions for its improvement.

We are pleased that you consider the experimental dataset, the methods, and the results valuable, and that you see potential for the experiments to serve as benchmark cases for future numerical modelling. We also appreciate that, despite your substantial concerns regarding the current theoretical framing, you recognize the scientific value of the experimental work and would generally support its publication. We have carefully considered your comments and will revise the manuscript substantially. We believe that the review comments will help us to improve the manuscript considerably, and we hope that the revised version will address the main concerns raised during the review process.

In the following we provide the review comments listed as [C1] to [C51] (black italic font) and our corresponding answers as [A1] to [A51] (blue normal font).

With kind regards,
The authors

Why the current interpretation is not valid

[C1] *I agree, in general, with the remark by Reviewer 1 that in geosciences one often uses simplified, approximate, or partly empirical frameworks that can still be useful in practice. For example, in depth-averaged landslide modelling, empirical entrainment laws have been proposed (e.g., Eglit and Demidov, 2005; McDougall and Hungr, 2005) that relate entrainment rate to flow variables such as depth, velocity or momentum through (non-physical) calibrated coefficients. Although such laws are not derived from first principles, their use is legitimate because they are explicitly empirical, and because they avoid the need to characterize bed material properties in the field, which may be a reasonable parsimonious strategy for large-scale hazard mapping. It is therefore meaningful to test against experiments whether they can reproduce observed erosion and mobility (see however also the opinion in Iverson and Ouyang, 2015). The situation is fundamentally different for Pudasaini and Krautblatter (2021). That model is not approximate or empirical, but the result of an incorrect mathematical derivation (Issler et al., 2024). The conclusions that follow from that derivation are therefore also incorrect. For this reason, the current attempt to frame the experiments as supporting or testing the legitimacy of the PK interpretation is not acceptable. Although the errors are already explained in Issler et al. (2024) and in my CC2, it is useful to restate the point here using a very simple mass-block model, analogous to the examples discussed by Iverson et al. (2011) and Iverson (2012), which I will extend to consider different flow and bed bulk densities and a possible finite initial bed velocity* u_b.

Consider a moving slide block of initial mass (per unit basal area) $\rho_m h_m$*, where* h_m *is the initial landslide thickness and* ρ_m *is the landslide bulk density, travelling downslope with initial velocity* u_m*. This block overrides and entrains a basal layer of mass* $\rho_b \Delta z_b$*, where* ρ_b *is the bed bulk density and* Δz_b *is the (pre-defined) entrained depth. The momentum change of the combined system (moving block plus newly entrained bed material) is then*

$$(p_m h_m + p_b \Delta z_b)(u_m + \Delta u_m) - (p_m h_m u_m + p_b \Delta z_b u_b) \,, \qquad (1)$$

where Δu_m *is the change in the whole landslide speed. Dividing by the time interval* Δt *over which these processes occur, and equating the result to the sum of the forces acting on the entire system, yields the momentum balance*

$$(p_m h_m + p_b \Delta z_b)\frac{\Delta u_m}{\Delta t} + p_b (u_m - k * u_b)\frac{\Delta z_b}{\Delta t} = (p_m h_m + p_b \Delta z_b) g sin\, \theta - \tau_b \,, \qquad (2)$$

where k = 1 (Issler et al., 2024; Iverson, 2012), and τ_b *is the shear stress acting at the base of the slide plus the newly entrained bed material (see derivation in Iverson, 2012; Issler, 2014). This form is well established and has been rigorously derived and discussed in the literature (Hungr, 1990; Erlichson, 1991; Fraccarollo and Capart, 2002; Iverson et al., 2011; Iverson, 2012, 2013; Iverson and Ouyang, 2015; Issler et al., 2024). By contrast, Pudasaini and Krautblatter (2021) incorrectly derive k = 2 (see Issler et al., 2024, for an explanation of the origin of this error), thereby violating momentum and energy conservation. They then propagate this error into further equations and conclusions concerning the influence of flow–bed density contrast on mobility. These conclusions on flow–bed density contrasts therefore cannot be used as a valid framework for the interpretation proposed by Wetterauer et al.*

At the level of the simple block model, Eq. (2) already allows one to discuss the influence of bed density on both landslide mobility and mass growth. The net driving force (right-hand side of Eq. 2), must be allocated between accelerating the landslide system and entraining the bed material (i.e., accelerating it from u_b *to* u_m*). Because* Δz_b *is prescribed in this example, that partitioning depends explicitly on* ρ_b*, as seen on the left-hand side of the equation. That said, this simple rigid mass-block analysis assumes that the entrained bed block fails along a pre-defined interface located at depth* Δz_b*, which may be the case for a competent rock layer sliding on a discontinuity, or of a granular material failing along a pre-defined weak layer. For a continuous granular material, however, the situation is more subtle. As* $\Delta t \rightarrow 0$*, depth-averaged models contract the entrainment layer to an infinitesimal layer* dz_b*, whose thickness is no* *longer prescribed in advance but is instead regulated by the available driving force. In that limit, the shear stress jump across the bed interface determines the mass of bed material that can be accelerated from u_b to u_m in a unit time:*

$$\rho_b E_b = \frac{max(0,\tau_m - \tau_b)}{u_m - u_b}. \qquad (3)$$

In the limit ∆t → 0, Eq. (2) then becomes[1]

$$p_m h_m \frac{du_m}{dt} + p_b(u_m - u_b)E_b = p_m h_m g sin\,\theta\; - \tau_b\,. \qquad (4)$$

A small ρ_b will imply a large entrainment rate E_b (cf. Eq. 3), but this does not change how the net driving force is partitioned between entrainment and acceleration, nor does it alter the momentum of the whole landslide (cf. Eq. 4). Nonetheless, a larger entrainment rate E_b leads to higher flow depths and larger flowing volumes, which may in turn generate a stronger downslope thrust through large longitudinal pressure gradients at the flow front $\left(-gh_m \partial_x(p_m h_m cos\,\theta\,)\right)$, omitted in the simplified block-model example of Eq. 4).

At the same time, Eqs. (2) and (4) also show that the shear strength within the erodible bed, τ_b, plays a primary role in determining the net driving force. If one assumes that the resisting stress follows a Mohr–Coulomb failure criterion, then in the dry case

$$\tau_b = (p_m h_m + p_b \Delta z_b) g\; cos\,\theta\; tan\,\varphi_b\;\;, \qquad (5)$$

where φ_b is the internal friction angle of the erodible bed. Weak beds with low φ_b can therefore strongly increase the net driving force in Eqs. (2) and (4), thereby promoting erosion and mobility. This is precisely what Wetterauer et al. observe when comparing the low-friction Glass beds with the higher-friction Quartz and Magnetite beds. By contrast, Pudasaini and Krautblatter (2021) incorrectly posit that the basal shear stress of the flow, τ_m (dependent on the flow friction angle φ_m), applies instead of τ_b. That assumption does not permit a correct explanation of the mobility pattern observed in the experiments (see also my CC2).

In real landslides, and also in flume experiments, the influence of bed density on mobility may nevertheless be more complex than the simple interpretation of Eqs. (2) and (4). For example, entrainment of lighter particles may induce density segregation, so that sediments originating from the erodible bed are transported away from the flow base, and subsequent mobility may then be controlled more strongly by the frictional properties of the original flowing material. Likewise, entrainment of particles finer than the flow material may promote basal sorting and enhance mobility through rolling effects (Makris et al., 2024). This is to say that qualitative interpretation of such experiments against simplified depth-averaged theories is not straightforward.

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[1]It is interesting to note that, in the Eulerian form of the depth-averaged momentum conservation equation, ρ_b does not appear explicitly (Iverson and Ouyang, 2015), because the inertial effects of entrainment are already accounted for implicitly on the left-hand side of the equation. If dilatancy effects are neglected, the (depth-integrated) flow density may change when an erodible bed with a different bulk density is entrained and the newly entrained material does not adapt its density to that of the flow. In that situation, determining the evolving (depth-integrated) flow density requires conservation of both mass and volume (see Eqs. 3 and 4 in Vicari and Issler, 2025). Combining these relations with the Eulerian momentum balance (Eq. 6 in Vicari and Issler, 2025) precisely yields Eq. (4). By contrast, if granular dilatancy occurs during entrainment, a granular bed with a porosity different from that of the flow may adapt its density during the entrainment process, which will also affect the momentum balances (Iverson and Ouyang, 2015).

[A1] Thank you for this detailed explanation. We acknowledge that the current manuscript overemphasizes the interpretation of the experiments in the specific framework of Pudasaini and Krautblatter (2021; hereafter called P&K). We agree that this framing needs to be revised substantially.

In the revised manuscript, we will remove statements that imply that our experiments validate, support, or test the legitimacy of the P&K formulation. Instead, we will describe P&K as one contribution that motivated the experimental design and will explicitly place it alongside alternative and critical formulations, including Iverson (2012), Iverson and Ouyang (2015), and Issler et al. (2024). We will also revise the terminology throughout the manuscript so that the experiments are primarily described in terms of low-, equal-, and high-density erodible beds, rather than as direct tests of three theoretical energy regimes.

At the same time, we do not consider it appropriate to remove all references to P&K. We acknowledge that Issler et al. (2024) provides a critical opposing statement to this model. However, P&K remains a peer-reviewed publication in Nature Communications, was already cited over 200 times, and has not been withdrawn or retracted by the publisher. Beyond this formal point, the publication is scientifically relevant to our study because it explicitly describes the potential consequences of contrasting bed inertia for erosion-induced changes in landslide mobility. This is precisely the conceptual motivation behind our experimental design, in which a constant dry granular slide overrides erodible beds of lower, equal, and higher density. This motivation is broader than the specific mathematical point criticized in relation to the factor 2 applied to the bed velocity term. The general question of whether and how the inertia and mechanical properties of entrained bed material alter mobility remains relevant also for alternative mechanically based erosion and entrainment models. We therefore consider a limited and balanced citation of P&K, together with the subsequent critique by Issler et al. (2024), to be more appropriate than a complete omission of the study from the manuscript.

We further agree that the interpretation of our results should give greater weight to bed resistance, shear strength, internal friction, particle shape, and entrainment mechanics. This point is already partly reflected in the present discussion, where we conclude that density contrasts alone are insufficient to explain the observed trends and that the low-friction, near-spherical Glass bed likely promoted stronger erosion and higher mobility, whereas the angular Qtz and Mag beds showed similar resistance despite their contrasting densities. We will strengthen this interpretation and relate it more clearly to mechanically based entrainment concepts.

We note, however, that our experiments were not designed to discriminate between competing mathematical derivations of erosion-induced momentum exchange. Our measurements are surface-based and do not directly resolve the slide-bed interface, basal shear stress, or grain-scale velocity profiles. We will therefore avoid taking our data as evidence for or against a specific coefficient in a depth-averaged momentum balance. Instead, we will present the dataset as an experimental basis for future testing of different erosion and entrainment formulations.

Accordingly, the revised manuscript will be reframed as an experimental study of dry granular flow mobility over erodible beds with contrasting density, friction, and particle-shape properties. The role of P&K will be reduced to the motivation and literature context, while the interpretation will be broadened to include the mechanically sound concepts and limitations highlighted in your comment.

Major remarks

Mechanistic framing. My main comment is that the manuscript must be reframed. The following major changes are required:

[C2] *<u>Remove all reliance on Pudasaini and Krautblatter (2021).</u> Any citation to Pudasaini and Krautblatter (2021), and any interpretation based on their inertia-contrast framework, should be removed. This also applies to subsequent erosion models by Shiva Pudasaini that build on the same underlying error. If the authors wish to interpret their results with respect to a depth-averaged* *entrainment theory, they should instead refer to the rigorous and correct formulations of Iverson (2012) and Iverson and Ouyang (2015). (However, note that jump conditions had already been proposed earlier by Norem and Schieldrop (1991), Gray (2001) and Fraccarollo and Capart (2002).) Given the seriousness of the PK errors, I do not consider the manuscript acceptable unless all reliance on Pudasaini and Krautblatter (2021) is removed. This reframing will actually go in the authors' favor, because it will provide a physically sound interpretation of the experimental results and thereby make the manuscript stronger and more convincing.*

[A2] As stated in our response to [C1], we agree that the manuscript needs to be substantially reframed and that it should no longer rely on P&K as the central theoretical framework. We will revise the Introduction, Discussion, and Conclusions accordingly and will include the alternative mechanically based formulations suggested by the reviewer.

However, we respectfully disagree with the request to remove all citations to P&K and related work. In our view, a complete omission of a peer-reviewed publication that directly motivated the experimental design would not provide a balanced representation of the scientific context in which the study was conceived. We acknowledge the critique by Issler et al. (2024), co-authored by the reviewer, and will explicitly discuss it in the revised manuscript. At the same time, P&K remains an available peer-reviewed scientific contribution in Nature Communications and has not been withdrawn or retracted by the publisher. As long as this publication remains part of the published scientific record, we consider it scientifically appropriate to cite it, while clearly acknowledging the subsequent controversy and critique.

We also believe that the fact that the reviewer is a co-author of the critical Arising Matter article should not by itself require the exclusion of the original publication from the manuscript. A balanced scientific treatment should allow both the original publication and the opposing critique to be cited and discussed, provided that the revised interpretation does not rely on the contested formulation. P&K addresses the question of how contrasting bed inertia may affect erosion-induced mobility, which is directly related to the motivation of our experiments. For this reason, we consider a limited and balanced citation of P&K, together with the critique by Issler et al. (2024), more appropriate than a complete omission of the study.

Accordingly, we will remove our reliance on P&K as an interpretive framework, but we will retain a limited and balanced citation to the study as part of the motivation and literature context. This will clarify that our study does not defend a particular theoretical model, but provides a reproducible experimental dataset that can be used to evaluate erosion and entrainment models more generally.

[C3] *<u>Reframe the experiments around bed properties, not "density contrasts" alone.</u> The manuscript should not present the experiments primarily as a test of bed particle density or of "flow–bed inertia". The experiments simultaneously vary both particle density and internal friction, and the results strongly suggest that bed friction is the dominant control on erosion and mobility. In the current manuscript, this only appears as a secondary caveat in Section 4.2, but in the landslide literature it is already well established that bed shear strength is a primary control on erosion and mobility through analytical theory (Iverson, 2012), depth-resolved modelling (Crosta et al., 2009), and experiments (Crosta et al., 2009; Iverson et al., 2011; Roelofs et al., 2023). This should be the central framing of the manuscript.*

[A3] Thank you for this comment. We agree that bed friction is an important factor influencing erosion and mobility and should be carefully considered when interpreting the experimental results. Indeed, our results suggest that frictional properties and particle shape play a central role in explaining the observed behavior.

However, the primary objective of the experimental design was to investigate the influence of density contrasts between flow and bed materials. While two of the material combinations differ predominantly in density, we acknowledge that the third material introduces additional differences in particle shape and frictional behavior. This limitation arose from the practical constraints associated with material selection,

was recognized during the study design, and is discussed extensively in the manuscript as a potential explanation for the observed deviations from the other scenarios (see Section 4.2 and Conclusions).
In the revised manuscript, we will further emphasize these experimental limitations. However, we consider the investigation of density contrasts to remain a central aspect of the experimental design and motivation of the study.

[C4] *Refer to the bed configurations by material composition. Throughout the manuscript, I suggest referring to the erodible bed configurations by their composition (Glass, Quartz, Magnetite), rather than by "inertially weak/neutral/strong" terminology. This would more honestly reflect the experiments, since both density and friction vary between these materials.*

[A4] Thank you for this suggestion. Since we already largely follow this approach throughout the figures using labels such as Qtz/f, Glass/f, and Mag/f, or low-density, equal-density, and high-density bed, respectively, we agree that using this more material-related terminology consistently throughout the manuscript would improve clarity. We will remove the inertia-related terminology and revise the manuscript accordingly (see also our response to [C24]).

[C5] *Rewrite the abstract, introduction, and main interpretation accordingly. The manuscript should be reframed as a study of erosion of dry granular flows over beds with differing sediment properties, especially frictional properties, and secondarily differing densities. A possible title could be: "Mobility of dry granular flows over erodible beds: Experimental insights into the influence of varying bed sediment compositions". The introduction could then motivate the study as follows: previous dry-flow experiments have mostly considered similar bed and flow materials (Mangeney et al., 2010); other studies (Farin et al., 2014) have examined the effect of bed porosity on erosion and mobility; but few studies have explored distinctly different bed sediment compositions, combining differences in density and friction. The manuscript could then highlight that such experiments provide a useful dataset for benchmarking advanced numerical models, including depth-resolved continuum simulations and discrete element simulations. To facilitate this, it would be highly useful if the authors could provide, through a repository, a digital elevation model of the flume and release volume, the scanned deposit surfaces, and an organized dataset of the main experimental results, which could then be used by other researchers to benchmark their models.*

[A5] Thank you for this constructive suggestion. We agree that the abstract, introduction, and main interpretation should be revised to better reflect the full range of material contrasts in the experiments. We will revise these chapters so that the experiments are presented as a study of dry granular flows over erodible beds with contrasting sediment properties, including density, frictional resistance, particle shape, and particle-size distribution.

We note, however, that the original experimental design was primarily motivated by density contrasts between the released slide and the erodible bed. The aim was to compare a constant dry granular slide overriding beds of lower, equal, and higher density. We acknowledge that the selected materials also differ in their frictional and grain-shape properties, and we agree that these differences need to be more clearly reflected in the revised framing and interpretation. In particular, the Glass material differs from Qtz and Mag not only by its much lower density, but also by its near-spherical particle shape and lower internal friction angle. This was not introduced intentionally as an additional variable, but resulted from the practical constraint that a distinctly low-density, reusable, separable granular material with frictional properties identical to natural quartz or magnetite was not available. We will therefore make clear that density was the primary design variable, while friction and particle shape are important accompanying material properties that likely contributed substantially to the observed mobility differences.

We appreciate the suggested title and will revise the title in this direction. A broader title such as "Mobility of dry granular flows over erodible beds: Experimental insights into the influence of varying bed sediment compositions" would indeed better reflect the revised scope of the manuscript. We will also revise the introduction to better position the study relative to previous dry granular flow experiments with similar slide and bed materials, studies on bed porosity, and experimental work on erosion and entrainment over beds with different sediment properties.

We also fully agree that the experiments may provide a useful benchmark dataset for numerical models, including depth-resolved continuum models and discrete element simulations. We would be very pleased if the experiments were used as benchmark cases for further developing and validating numerical models of erosion, entrainment, and dry granular flow mobility. The contrasts in density, and especially the combination of low density, rounded grains, and lower frictional resistance in the Glass beds, provide challenging and useful validation cases for models that aim to reproduce material-dependent erosion, entrainment, segregation, and runout behavior.

As stated in the Data Availability section of the manuscript, the experimental datasets will be made freely available in a public repository upon publication of the study to facilitate future analyses and model benchmarking. This will include the organized experimental results and the surface-based datasets required for reproducing and analyzing the experiments. In addition, the flume geometry and experimental setup are described in detail in the manuscript figures and methods section so that an event model of the flume and release configuration can be constructed straightforwardly.

[C6] *Move the discussion of density to a secondary level. The main interpretation in Section 4.1 should focus first on the effect of bed internal friction. The discussion of density may then be retained as an additional possible control, but framed using the correct framework discussed in Section 1 above.*

[A6] We agree that the frictional properties are an important factor when interpreting the experimental results and that their influence should be discussed more prominently. However, the primary objective of the experimental design was to investigate the influence of solid particle density contrasts between flow and bed materials. We therefore consider density to remain a central aspect of the study rather than a secondary one. In the revised manuscript, we will restructure the discussion to more closely integrate the potential effects of density and frictional properties rather than treating them in separate sections.

[C7] *Acknowledge previous work on erosion. The role of bed bulk density is not new. Farin et al. (2014) already performed experiments on erodible beds of differing porosities, and thus differing bulk densities.*

[A7] Thank you for pointing out this relevant study. We will acknowledge Farin et al. (2014) in the revised manuscript as an important previous contribution on density-related controls on erosion and mobility.

We would like to clarify, however, that the primary focus of our experiments is on solid particle density contrasts, while Farin et al. (2014) investigated the influence of bulk-density variations associated with differing bed porosities. We agree that bulk density is the relevant density term in depth-averaged model equations. In our experiments, however, we deliberately prepared the bed materials with comparable particle-size distributions, as shown in the manuscript, so that major sediment-distribution properties are similar among the scenarios. Consequently, porosity and solid volume fraction are expected to remain broadly comparable within each size fraction. Under such conditions, variations in bulk density mainly reflect variations in the true density of the solid particles.

Thus, while our experiments also imply variations in bed bulk density, these variations primarily arise from differences in solid particle density rather than from differences in bed porosity. This distinguishes our study from Farin et al. (2014). We will emphasize this distinction more clearly in the revised manuscript.

[C8] *Experiments with limestone beds. Because density and friction vary simultaneously in the current experiments, their individual effects cannot be isolated precisely. It could therefore be very valuable if the authors chose to perform their proposed additional tests using limestone beds, which would have* *lower density than quartz but similar frictional properties. Even a limited number of such experiments could significantly strengthen the manuscript. I fully understand, however, that this would require substantial additional work, and I therefore leave it entirely to the authors whether they wish to pursue this extension.*

[A8] We fully agree that additional limestone experiments would be highly beneficial and insightful. Indeed, limestone was considered during the initial planning of the experimental program because it could provide a lower-density natural material with frictional properties closer to those of quartz and magnetite than the expanded glass used in our experiments. This is also one of the reasons why we identify such experiments as a promising direction for future work in the discussion.

However, implementing limestone as an additional bed material would introduce substantial practical challenges. A central constraint of the present experimental design was the need to separate slide and bed material after each experiment to allow repeated, reproducible tests with the same material fractions. While quartz and magnetite can be separated magnetically, and quartz and glass can be separated by density contrast in a water bath, no similarly practical separation method is available for quartz-limestone mixtures. Dissolving limestone chemically would, in principle, be possible, but would be impractical for the large material volumes used in the experimental series and would not allow efficient material reuse. In addition, the density contrast between limestone and quartz would be much smaller than the contrast between glass and quartz, so that the resulting experimental signal might be more subtle and more difficult to separate from natural experimental variability.

For these reasons, we decided not to add limestone experiments within the current revision. Such an extension would require substantial additional material preparation, characterization, and experimental testing, and would go beyond the scope of the present study. We will, however, keep this point in the discussion as an important and valuable direction for future work.

[C9] *<u>Applicability to which type of landslides.</u> The manuscript is currently motivated primarily in terms of debris flows. However, the experiments involve dry materials on very steep slopes. I therefore suggest shifting the emphasis from debris flows toward dry granular flows, rock avalanches, or dry debris avalanches.*

The introduction currently devotes substantial space to physical mechanisms of erosion in debris flows. I suggest shortening this part and placing greater emphasis on erosion in dry granular flows and debris avalanches. In the field, erosion of dry coarse-grained sediments has been documented by Hungr and Evans (2004), although in many rock avalanches extreme mobility is also associated with the avalanche encountering saturated sediments on flatter areas (Hungr and Evans, 2004; Aaron and McDougall, 2019). On the other hand, significant erosion of fully dry sediment can also occur on steep slopes when the bed is metastable (Mangeney et al., 2010; Mangeney, 2011). A very intuitive example is the familiar experience of "scree running", where steep coarse-grained slopes can be easily mobilized under the weight of hikers, creating the feeling of "skiing the gravel" (see https://teara.govt.nz/en/photograph/9871/scree-running). Dry sediment on steep scree slopes may therefore be quite erodible if its friction angle is not much higher than the slope angle (see the Boua digl Cof channel described in Vicari et al., 2026).

Dry granular flow experiments (with pasta and rice particles) were also conducted by Crosta et al. (2009), who found that the presence of an erodible layer could decrease mobility when flow and bed had similar density. Such mobility reduction may be related to their horizontal experimental geometry (consistent with the results of Mangeney et al., 2010).

Because dry granular flows are less sensitive to some scaling effects than wet flows, a discussion of scaling would be useful (see Iverson, 2015; Makris et al., 2024). For example, the Savage number might *help characterize the flow regime – although it may be difficult to evaluate reasonably the shear rate on the smooth rigid bed if substantial basal slip occurred.*

[A9] Thank you for this valid and detailed remark. We will replace the term "debris flow" by "dry granular flows" whenever we refer to our experiments in the revised version. This is also consistent with the recommendation by Reviewer #1 (see [C2]).

We will include the suggested literature where appropriate. We will also add a more focused discussion of scaling. For our dry granular flow experiments, the choice of meaningful non-dimensional parameters is somewhat limited because many commonly used dimensionless numbers require properties of a fluid phase that is absent in our setup (as you mentioned). We will therefore focus on parameters that are meaningful for dry granular flows, in particular the Savage number, and compare them with values reported in the literature.

[C10] *<u>Depth-resolved models.</u> The manuscript currently places strong motivation on validating depth-averaged model assumptions. However, with modern computational resources, fully three-dimensional depth-resolved simulations are now feasible even at slope scale (Cicoira et al., 2022; Kyburz et al., 2025; Zhu et al., 2025). In such models, the flowing mass and erodible bed are represented explicitly through constitutive laws, and erosion and entrainment emerge naturally without the need for separate closure assumptions for fluxes and boundary stresses during entrainment. A strong motivation for the present study could therefore be that it provides a valuable benchmark dataset for such numerical models. A key paper that should be discussed in this context is Crosta et al. (2009), who performed depth-resolved simulations while varying the internal friction angle of the erodible bed. Lower bed friction led to greater erosion and higher mobility, in qualitative agreement with the present experimental results. This study should be cited and discussed. Additional references on 3D erosion modelling are provided in my CC2.*

[A10] Thank you for this suggestion and for the relevant literature. Our intention is not to restrict the use of our experiments to a specific model type, whether depth-averaged or fully 3D. We would welcome future simulations of our experiments with depth-resolved 3D models. We will include the suggested references, including Crosta et al. (2009), where appropriate, and mention that lower bed friction promoted stronger erosion and higher mobility in their simulations, which is qualitatively consistent with our Glass-bed experiments. As stated in response to [C5], the datasets will be made freely available upon publication to support future model benchmarking.

Minor remarks and specific comments

[C11] *Title (and elsewhere): The landslide classification of Hungr et al. (2014) defines debris flows as "Very rapid to extremely rapid surging flow of saturated debris in a steep channel". The term "dry debris flow" is therefore problematic. I suggest using "dry granular flows" instead.*

[A11] Thank you, we will adopt this terminology throughout the manuscript (see also our response [A9]).

[C12] *L3–7: This passage will need to be fully reframed in line with my major comments above.*

[A12] We will remove the reliance on P&K here and reframe this passage in line with our answer [A3] above.

[C13] *L28: "mass and momentum transfer, and bed weakening".*

[A13] Thank you, we will adjust this.

[C14] *L29: Deceleration during erosion is not synonymous with reduced mobility. Farin et al. (2014), for example, found that erosion could strongly affect the deceleration phase while still enhancing overall flow mobility.*

[A14] We agree that local deceleration during erosion is not necessarily synonymous with reduced overall mobility. While local deceleration indicates a temporary reduction in flow velocity, its effect on the final mobility depends on the subsequent evolution of the flow, including entrainment, volume growth, runout behavior, and deposition. We will revise the corresponding statement to avoid this ambiguity.

[C15] *L31–32: You should remove momentum production (that situation would only arise when the bed itself has an initial finite velocity; see Section 4.1 in Iverson (2012)), and instead add "application of the consistent basal shear resistance in the presence of eroding beds (Iverson, 2012; Iverson and Ouyang, 2015)". The reason is well explained by Iverson and Ouyang (2015), for example: "In depth-integrated models, decreased basal shear resistance can be the only source of flow momentum gains that sometimes accompany entrainment."*

[A15] Thank you for this clarification. We understand the concern that the term "momentum production" may be misleading, especially because true momentum production would require an initially moving bed. We will therefore revise the sentence and use a more neutral formulation, referring to mechanical effects on the momentum balance during entrainment. We will also add Iverson (2012) and Iverson and Ouyang (2015) at this point, as suggested.

[C16] *L32: The current discussion on entrainment implementation in numerical models is incomplete. Depth-averaged models often assume basal shear-dominated erosion, but more general frameworks can also include slope-normal momentum jumps (Iverson and Ouyang, 2015) and alternative frontal ploughing mechanisms (see Issler, 2020, and references therein). Depth-resolved models should also be discussed.*

[A16] Thanks, we will add the suggested aspects and cite the relevant literature accordingly.

[C17] *L34: I am not sure de Haas et al. (2020) is the best reference here, as that paper does not itself compare depth-averaged entrainment models. Shen et al. (2020) and Lee et al. (2022) may be more appropriate.*

[A17] Thank you for the suggestions. We will keep de Haas et al. (2020) in this case, as the concept on a "unified theory" is explicitly mentioned in the second paragraph of that paper and was also referenced in the introduction of Roelofs et al. (2023), who explicitly cite de Haas et al. (2020) in this context.

[C18] *L42: In addition to basal shear, recent studies have also examined the influence of collisional stresses on erosion (de Haas et al., 2022; Zheng et al., 2025).*

[A18] Thanks, we will add the suggested aspects and cite the relevant literature accordingly.

[C19] *L42: I would avoid stating that erosion increases linearly; a more cautious wording would be that erosion increases or is positively correlated with the relevant variable.*

[A19] Thank you, we will change it accordingly.

[C20] *L49: Replace "reduced effective basal friction" with "reduced bed internal friction".*

[A20] Thank you, we will change it accordingly.

[C21] *L55–56: This statement is insufficiently supported. I suggest removing it.*

[A21] Thank you for this comment. We agree that the statement regarding the model limitations is insufficiently supported and will remove the second part of the sentence. However, we believe it is appropriate to retain the statement that the experimental findings highlight the mechanical complexity and variety of processes involved at the flow–bed interface, as this conclusion is supported by the experimental observations presented above.

[C22] *L57–80: This section needs to be fully reframed in line with my major comments above.*

[A22] We will reframe this section in line with our answer [A2] above.

[C23] *L86–95: This section also needs to be reframed. It should state clearly that both bed density and bed internal friction are varied simultaneously, and that bed friction is expected to exert a major control on erosion and mobility (Iverson, 2012; Crosta et al., 2009; Iverson et al., 2011). The influence of bed density can then be addressed, building on the discussion presented in Section 1 above.*

[A23] Thank you for this suggestion. We will remove the reference in this section. As outlined in our response to [C3], we will retain density contrasts as a central motivation of the study, while highlighting the potential influence of frictional properties arising from the material selection more explicitly in this chapter.

[C24] *L96–101, Table 1, and elsewhere: Reframe the scenarios in terms of Glass, Quartz, and Magnetite beds, and state more explicitly, and with greater emphasis, that each bed differs in both solid density and frictional properties. In Table 1, the column "Relative inertia of bed to slide" should be replaced by "Scenario", with the corresponding entries indicated, for example, as "Low bed friction and low bed density", "High bed friction and intermediate bed density", and "High bed friction and high bed density".*

[A24] Thank you for this suggestion. We agree that the material-based terminology provides a clearer description of the experimental scenarios. Since we refer to Qtz/f, Glass/f, and Mag/f etc. also as low-density, equal-density, and high-density throughout much of the manuscript, we will adopt this terminology consistently for the scenarios as well.

[C25] *L98–100: Please specify clearly that these are solid particle densities.*

[A25] Thank you, we will add an additional clarifying statement.

[C26] *L103–105 and Table 1: Please clarify what the labels "/f" and "/c" refer to. As written, it is not fully clear whether the fine and coarse fractions apply only to the erodible bed materials, to the initially released quartz slide material, or to both the slide and bed materials within each experimental series. Are both Qtz/f and Qtz/c used for the initial sliding material? I also suggest avoiding the wording "sand" and "gravel" here, or at least explicitly defining it as particle-size terminology. Because sand and gravel seem to refer to grain-size classes, but sand and gravel may themselves be composed of quartz, this wording could be confused with the material composition, especially since quartz is also one of the tested materials. Please specify the relevant particle-size ranges directly, e.g., "fine fraction, 0.25–2 mm" and "coarse fraction, 0.25–8 mm".*

[A26] Thank you for this hint. Each suffix applies to both the erodible bed and the corresponding released slide. Fine slides were only tested over fine beds, and coarse slides only over coarse beds. We will clarify this more explicitly in the revised manuscript and Tables. We will also revise the particle-size terminology.

[C27] *L125: In addition to the release mass, do you have an estimate of the release volume? This would allow calculation of the initial slide porosity.*

[A27] The release volume was not measured directly. It may be derived from the infilled mass (130 kg) and the respective dry bulk density of fine (1360 kg m^{-3}) or coarse (1530 kg m^{-3}) Quartz, listed in Table 1 of the manuscript. This would result in a Qtz-slide release volume of 0.096 m^3 for all fine and 0.085 m^3 for all coarse experiments. We will add these numbers.

[C28] *L129–131: I appreciate the methodology used to allow gradual erosion. You could add a reference to de Haas and van Woerkom (2016), where strong erosion instead occurs near the upstream edge of the erodible bed.*

[A28] Good idea, we will add it.

[C29] *L142: Please specify whether the 4–5% water content is by mass or by volume.*

[A29] Thank you for pointing this out. It is by mass, we will specify this accordingly.

[C30] *L145 and later: You refer to flow heights as being measured slope-normal. Later, you refer to erosion depth (L162): is this quantity also measured slope-normal? Please use consistent terminology throughout the manuscript.*

[A30] Thank you for this comment. Yes, the erosion depth is also measured slope-normal, and we will specify this in the revised manuscript. We consider it appropriate, however, to retain the distinction between flow height and erosion depth, as they describe different quantities. Here, the flow height refers to the evolving flow during slide passage, whereas the erosion depth is a post-event measure of the resulting bed-level change (inferred from the flow-height record).

[C31] *L160 and Section 3.1.4: It would be more informative to calculate the front velocity separately for each consecutive section (e.g., reservoir flap to us1; us1 to us2; etc.). This would better highlight where along the flume the flow accelerates or decelerates.*

[A31] Thank you for this suggestion. We agree that calculating front velocities for consecutive flume sections could in principle provide a more informative measure of flow-front propagation. The travel distances and travel times between consecutive sensors in our flume, however, are very short, and the determination of the flow-front arrival time at a sensor is subject to a certain degree of uncertainty. Consequently, small timing uncertainties can result in comparatively large variations in the calculated section-wise velocities, particularly where individual particles saltate ahead of the main flow front of the dry granular flows. We therefore consider the front velocity from the reservoir flap to each sensor to provide a more robust measure for comparing the different experimental scenarios. Consistent with the recommendation of Reviewer #1 (see [C7] and [C10]), will revise the terminology throughout the manuscript to refer to these values as the "average front velocity from the reservoir flap to each sensor".

[C32] *L281 and Table 2: Because the long runout in the coarse cases seems dominated by dispersed particles, it would be useful also to report the runout distance of the deposit center of mass on the runout plane.*

[A32] Thank you for this suggestion. We considered this approach; however, a robust determination of the deposit center of mass is not possible in the experiments involving different particle densities, as this would require detailed mapping of the internal material distribution. Such detailed quantitative information is not available for the deposit bodies (including the main body of the Glass/c experiment).

We therefore prefer to retain the center of volume, which can be determined consistently for all experiments.

[C33] *Figure 5: A summary plot of deposit volume as a function of scenario would be helpful to visualize the results more quickly than reading Table 2.*

[A33] Thank you for this suggestion. We agree that such a summary plot could provide a useful overview of the deposited volumes. As these data are already summarized in Table 2 and the manuscript is already extensive, we prefer to retain the current presentation.

[C34] *L308–311: This will need to be removed in line with my major comments above.*

[A34] This part will be generalized in line with our answer to [C2].

[C35] *L316–320: This will need to be reframed in line with my major comments above.*

[A35] This part will be generalized in line with our answer to [C2].

[C36] *L320: The longer runout in the Glass scenario may also be related to increased flow volume through larger erosion (see my comment above on the role of longitudinal pressure gradients).*

[A36] Thank you, we will consider this in the revised discussion.

[C37] *L325: Please add a reference to density segregation.*

[A37] At this point, we describe a direct observation from our own deposit cross sections, namely the preferential transport of Glass particles toward the front and lateral edges. We therefore do not consider an additional reference necessary for the observation itself. However, where we discuss the possible mechanism of density segregation more generally, we will add suitable references to previous granular-flow studies.

[C38] *L326–327 ("Strikingly"): This is not at all surprising, since the Quartz and Magnetite beds have similarly high friction.*

[A38] We will remove the word "strikingly".

[C39] *L330–338: This section will need to be fully reframed in line with my major comments above. In particular, it should describe the role of bed internal friction in controlling erosion and entrainment, and discuss the role of bed density with reference to the established theory of Iverson (2012), or the framework I provided above.*

[A39] This part will be generalized in line with our answer to [C2].

[C40] *L342–344: This is not always the case in erosive situations; weakening of the bed can also lead to acceleration (Iverson et al., 2011).*

[A40] We will revise this statement to avoid implying that erosion generally increases flow resistance. We will add that, depending on bed conditions, erosion can also lead to acceleration and enhanced mobility, for example when bed weakening reduces basal resistance, as shown by Iverson et al. (2011).

[C41] *L355: Modify to "or low material friction and, possibly, low density".*

[A41] Thank you, we will adopt this.

[C42] *L355–359: You may also compare your erosion rates with those measured at Illgraben by Berger et al. (2011), and with those measured in the dry granular flow experiments of Barbolini et al. (2005). This latter study would also be worth citing and discussing in the manuscript.*

[A42] Good idea, we will include Berger et al. (2011) and Barbolini et al. (2005) in the revised manuscript and compare their rates with our measured values where appropriate. We note, however, that the erosion rates presented in the current manuscript are apparent mean erosion rates. A more detailed analysis of the temporal evolution of erosion rates, based on measured vertical velocity profiles, is planned for a follow-up publication.

[C43] *L361: The manuscript refers to segregation of coarse particles toward the front, but the results seemed rather to highlight density segregation, with lighter particles concentrating near the front. Maybe I am not understanding correctly your argument here?*

[A43] Thank you for this comment. The results actually show both processes. On the one hand, density segregation is evident in the experiments involving materials with contrasting densities and may have contributed to the observed accumulation of lighter particles near the flow front. On the other hand, grain-size segregation occurs simultaneously within the 0.25-2 mm and/or 0.25-8 mm grain-size distribution of each material.

The statement in this section refers to this grain-size segregation, whereby coarser particles tend to migrate towards the flow front and surface, while finer particles remain concentrated in the flow interior and deposit core. This behavior is observed both in experiments involving different materials (e.g., see the indicated coarsening-towards-the-surface arrows in Fig. 6) and in experiments using only quartz material, including the reference case.

We will try to clarify these differences more clearly in the revised version.

[C44] *L370: Add a reference to Farin et al. (2014) on the role of packing.*

[A44] Thank you, we will add the suggested reference.

[C45] *L375–376: Could the more dispersed coarse deposits also be related to stronger collisional stresses (Choi and Goodwin, 2021)?*

[A45] We will consider this possible interpretation in the revised discussion. However, in our experiments the longer runout of the coarse deposits may rather indicate lower effective energy dissipation during deposition. In granular mixtures, finer particles can increase the frequency of particle interactions and may therefore enhance collisional dissipation. The coarser material may consequently experience lower effective collisional dissipation and lower basal resistance on the runout surface, contributing to longer and more dispersed deposits. We will clarify this point and add the suggested reference where appropriate.

[C46] *L392–399: This will need to be reframed in line with my major comments above.*

[A46] This part will be generalized in line with our answer to [C2].

[C47] *Section 4.2: I suggest presenting all possible controls in the same section, beginning with the most important one, namely bed internal friction. The potential role of density can then be discussed as a secondary effect (based on the framework I discussed above in my Section 1), followed by the other factors already mentioned in the manuscript such as roundness, compaction, and segregation.*

[A47] Thank you for this suggestion. We agree that discussing the different potential controls within a single section will provide a clearer and more coherent interpretation of the results. We will therefore merge Sections 4.1 and 4.2 and discuss the various factors together, while using subheadings to maintain a structured overview, consistent with the recommendation of Reviewer #1 (see [C1]).

[C48] *L436: Add Farin et al. (2014) on the role of packing.*

[A48] Thank you, we will add the suggested reference.

[C49] *L485–493: A method similar to that proposed by Berger et al. (2010) could also be used; see also its simpler adaptation to flume experiments by Vicari et al. (2022).*

[A49] Thank you for this suggestion and for pointing us to these references. We agree that such approaches may provide valuable additional insights and will consider them in the design of future experiments.

[C50] *L499–501: This passage should be removed.*

[A50] We will generalize this passage.

[C51] *Conclusions: These points will need to be fully reframed in line with my major comments above. The conclusions should state that the study investigates contrasting erodible bed materials differing in both friction and density. The role of friction appears consistent with previous theoretical (Iverson, 2012), numerical (Crosta et al., 2009), and experimental (Iverson et al., 2011) work. Particle density may also influence how easily eroded material is accelerated to the flow speed. You may also highlight more succinctly your finding that Glass particles segregate toward the deposit front and margins.*

[A51] Thank you for this suggestion. The conclusions will be revised to reflect the revised manuscript structure and the corresponding changes in the discussion and interpretation of the experimental results.