

Second-round referee report on “Mobility of dry granular flows over erodible beds: Experimental insights into the influence of flow–bed density contrasts” by Wetterauer et al.

23 August 2026

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.

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. This can be seen directly in PK itself (Pudasaini and Krautblatter, 2021, pp. 6–7). The erroneous factor of $k = 2$ is highlighted in red below, together with the **corresponding conclusions that PK draw concerning the influence of flow–bed density contrasts on mobility**. PK state:

“Inertially neutral erodible bed substrate results in no change in energy and mobility.

For this, the inertia of the bed material is equal to the inertia of the material in the flow: $\rho^b \alpha^b = \rho^m \alpha^m$. Thus, we obtain $\lambda^b = 1/2 \lambda^m$, which implies that $\lambda^b = 1/2$, and $2\lambda^b - 1 = 0$. **In this situation, there is no gain or loss of momentum or energy, and thus, the landslide mobility remains unchanged even for the erosive landslide.** This is a very special situation, however, less likely to occur in nature. Moreover, note that the value $\lambda^b = 1/2$ that distinguishes between increased or reduced mobility arises from the term inside the brackets of (19) when $\rho^b \alpha^b = \rho^m \alpha^m$.

Inertially weaker erodible bed substrate results in enhanced energy and mobility. The inertia of the material in the bed can be lower than the inertia of the material in the flow: $\rho^b \alpha^b < \rho^m \alpha^m$. Then, we obtain $\lambda^b > 1/2\lambda^m$, which implies that $\lambda^b \in (1/2, 1)$, and $2\lambda^b - 1 \in (0, 1)$. In other words, this is the situation in which the change in inertia of the landslide in incorporating the inertially (mechanically) weaker material is less than its change in inertia if it would have incorporated inertially equally stronger material. **This suggests that the erosion-induced gained momentum or energy results in the enhanced-mobility of the erosive landslide.**

Inertially stronger erodible bed substrate results in reduced energy and mobility. The inertia of the bed material can be higher than inertia of the material in the flow: $\rho^b \alpha^b > \rho^m \alpha^m$. This implies $\lambda^b < 1/2\lambda^m$, and $\lambda^b \in (0, 1/2)$. Thus, $2\lambda^b - 1 \in (-1, 0)$. So, for this, the change in inertia of the landslide in incorporating the inertially stronger material is higher than its change in inertia if it would have incorporated inertially equally stronger material. **This implies the erosion-induced momentum loss or energy loss, and results in the reduced mobility even for the erosive landslide."**

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.

The authors emphasize in their rebuttal that PK "remains a peer-reviewed publication in Nature Communications", "was already cited over 200 times", and "has not been withdrawn or retracted". None of these points has any relevance on whether the governing equations conserve momentum and are physically/mathematically correct. Citation count is not a mechanical principle. Publication status cannot turn an incorrect derivation into a correct one. I therefore have to ask the authors very explicitly: is Newton's second law an optional or debatable principle? If not, then a formulation that violates momentum and energy conservation cannot simultaneously be retained as a valid mechanistic basis for interpreting the experiments. In fact, the authors themselves cite Iverson (2012) and Iverson and Ouyang (2015), while also retaining PK. But these formulations are mutually inconsistent in precisely the terms under discussion (see my CC2). They cannot both be mechanically correct.

I also want to clarify that this is not a request for the authors to preferentially cite Issler et al. (2024). I am a co-author of that paper, but I have no interest in obtaining additional citations to it. What matters scientifically is that the authors must use the correct conservation equations, i.e., Iverson (2012) and Iverson and Ouyang (2015), and consequent interpretation of the experiments. My objection is to presenting PK as a legitimate physical basis for the study when its claimed novelty and its density-dependent mobility regimes arise from a seriously incorrect derivation.

For this reason, I maintain my previous requirement: citations to PK must be removed entirely. The manuscript does not depend on PK in any essential way, as also emphasized by Dieter Issler, and removing it would strengthen rather than weaken the paper.

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.

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.

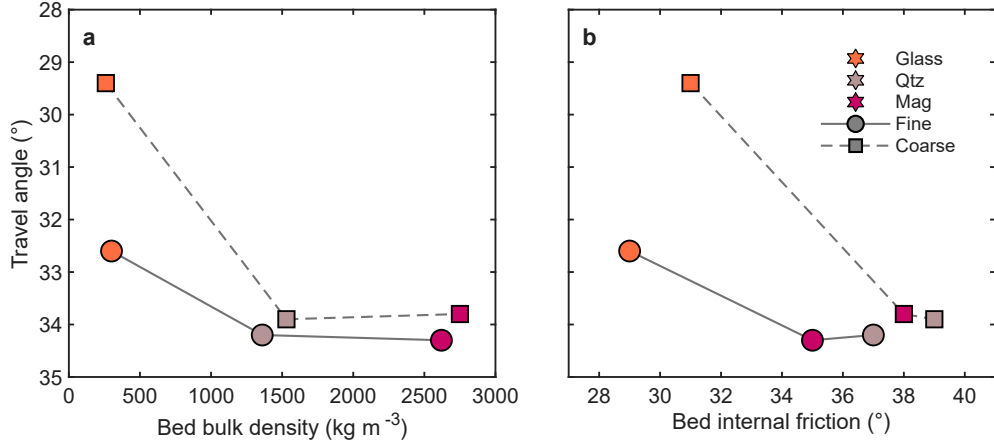


Figure 1: Wetterauer et al. experimental data: average travel angle as a function of (a) bed bulk density and (b) bed internal friction for the Glass, Quartz, and Magnetite beds. Quartz and Magnetite differ strongly in density but have similar frictional properties and exhibit nearly identical travel angles, whereas the more mobile Glass beds also have substantially lower internal friction.

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.

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.

Best regards,
Dr. Hervé Vicari
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