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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Model development for simulating mudslide and the case study of the failure of the gypsum tailings dam in East Texas in 1966" *by* Tso-Ren Wu et al.

Anonymous Referee #2

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Wu et al. present a modified rheological model for the simulation of mudflows, exploring the role of rheology in the formation of a static and a fluid region. The main highlight from this work is its three-dimensional implementation, which might come useful in a non-flat terrain and when facing obstacles. The authors focus on the role of viscosity as a key parameter for describing the kinematics of mudflows, comparing the different implementations of three rheological models (e.g., BM, CBM, MBM). Overall, the implementation is promising, but the manuscript, in its current state, does not provide a strong message for supporting the modified bi-viscosity model (MBM) as an ideal rheological representation of mudflows. My main concerns on this work are:

1. The validations of the numerical framework and rheological models in Sec. 3 do not include the MBM, leaving aside the comparison with the proposed ideal model for mudflows. Moreover, the numerical modifications and assumptions for adapting the model from 3D into a 2D representation are not discussed nor evident.

2. It is unclear, why the authors choose to simulate the 1966 East Texas event. If the authors interest is to highlight how the model can be used for tailing hazard assessment, then a detailed description of the event and the mobilized materials is needed. Moreover, given the frequency of tailing failures, it is tempting to see the model being validated with more cases.

3. However, if the authors motivation with the 1966 event is to prove how the MBM rheology reproduce a more accurately a mudflow, the selection of a field event of limited information makes it difficult to assess the advantages of the rheological model. Then, the selection of a benchmark case as a dam-break model seems more suitable for this purpose.

4. I got the impression that the comparisons between the three rheological models on the 1966 event are not supported by direct measurements of the material parameters of each particular model. Also, it is not clear how these parameters are obtained and calibrated. These missing information makes a critical assessment of each model difficult and leaves the reader with a qualitative similitude.

5. The manuscript goal differs slightly between line 72 and line 293. I understand that the authors explore the formation of a plug and a sheared region within the mudflow, but disagree in referring to them as solid and liquid phases, respectively.

6. It is not clear the difference between the volume fraction r and the solid concentration Cv introduced at the end of Sec. 5. A discussion on how this parameter evolves and controls the stratification process might strengthen the authors message.

7. The authors claim in line 305 that the initiation and slip surface of the mudflow is

described in their model. However, I do not find information that supports this claim, as the event simulation assumes the sudden release of the tailing material. Therefore, the conditions leading to the tailing failure are not accounted for in their model nor studied.

Given these points and the need for further simulations or a deep reevaluation of the manuscript, I recommend the rejection of this manuscript but encourage the authors to address the previous points and submit an improved version.

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