We would like to thank Andrew Mitchell, for this thorough review and the interest granted to our preprint. You will find in this comment the complete reply with our answers in italic type. I am also attaching a pdf of the same answers, this time in blue, if it makes it easier to read.

Overview:

The manuscript provides a detailed examination of flow-surge behaviour for both viscous fluid, and a mixture of viscous fluid with solid particles. The coupled SPH and solid particle modelling was used to replicate two lab experiments, and recover velocity and free-surface profiles. A simplified simulation of a single debris flow surge with poly-disperse boulders was also carried out, yielding realistic Froude numbers for the flow.

General comments:

1. It is unclear to me if the first part of the work focused on a viscous fluid without solid particles is applied to the part of the work with solid particles. Are these two independent model validations, as a lower viscosity is used in the cases where particles are present?

The lower viscosity is used in the 1st case due to scaling effects. We wanted mainly to answer the question : can SPH, traditionally used for medium to high Reynolds number flows, be used for slow laminar flow dominated by the viscous regime? Since the answer is shown to be positive, we apply the certainty that the fluid mechanics part of the viscous regime behaves accurately when we model a larger and more complex flow with debris. This is why we also take a side step to ensure that the addition of boulders to the fluid mechanics method yields accurate changes in the viscosity of the overall flow. Then the application of the debris flow model is reliable because each technical 'brick' has been proven to behave correctly. We could have had perfectly acceptable Froude numbers with a method that inaccurately represents creeping flows. Here we ensure that both mesoscopic and microscopic scales behave accurately.

This will be clarified a bit more precisely in the end of the introduction to explain the relationship between each section.

2. The flow of the paper may be improved if Section 2 started with the description of the SPH method and collision algorithm, then describe the two datasets used for model evaluation (Section 2.4, 2.5, 2.1, 2.2 and 2.3 for a suggested order). That way the information on the specifics of each comparison dataset are fresher in the readers' mind going into Section 3.

We agree, thank you for this excellent suggestion. This will be done in the revised manuscript.

Specific comments:

Line 22: Replace "understand" with "replicate".

OK, thank you, done.

Line 27: “to better understand hazard mapping” – models are typically used as an input to develop hazard maps.

Yes, we will change the text to “Such models are typically used as an input for the design of hazard maps on a debris fan.”

Lines 27 – 28: Depth-averaged models should be clearly linked to event-scale models.

We will add “Event scale models rely of depth averaged methods, where the flow is assumed to be 2D, simplified in depth. Depth averaged models can either be single phase …”

Line 42: Word choice with “in depth” – there may be confusion when discussing depth as in depth of material versus level of detail. This could be re-worded to “… are not designed to represent all the details of the internal mechanics of the flow.”

Thank you for pointing this out, we will change the text to your suggestion.

Line 56: Muddy debris flows are not introduced previously, it would be helpful to provide some background on muddy versus granular debris flows.

You are correct. With the changes done from the comments of reviewer 1, we will clarify a bit better that in the context of the european alps, we consider viscous-driven muddy debris flows in this paper. In this section we will change to “As a first approximation,muddy debris-flows, which are mechanically driven by viscous motion, can be studied at the macroscopic scale…”

Line 81: Replace “inconvenient” with “inconvenience”.

OK, thank you, done.

Lines 100 – 102: This sentence is out of place, move to line 105 or after to keep the information on previous work together.

OK, thank you, done.

Line 130: Provide a reference for the Krieger-Dougherty equation.

OK, thank you, done, added (Krieger and Dougherty 1959).

Krieger, Irvin M., and Thomas J. Dougherty. 1959. ‘A Mechanism for Non-Newtonian Flow in Suspensions of Rigid Spheres’. *Transactions of the Society of Rheology* 3 (1): 137–52. https://doi.org/10.1122/1.548848.

Line 135: The paper deals with significantly more than viscous laminar surges.

Reformulated to

“The aim of this paper is to model viscous laminar debris flow surges with Reynolds number close to the creeping flow limits ($Re\approx 0.1$). “

Section 2.4.1: It would be helpful to provide a brief description of the SPH method (i.e., discrete particles with a free surface interpolated from particle interactions). That would help with the discussion of the smoothing kernel for readers unfamiliar with SPH.

We will add between 2.4 and 2.4.1 : "Smoothed Particles Hydrodynamics is a computational fluid dynamics method based on the lagrangian framework. In SPH, the continuous domain is discretized into numerical nodes ('particles'), which are points of known information. Typical properties of the continuum (e.g. velocity, density, …) are associated to each of these points. The SPH method relies on the resolution of the Navier Stokes equation via interpolation onto these nodes. Particles interact with each other in a defined neighbourhood, named a smoothing kernel, by resolving the Navier Stokes equation system."

Lines 301 – 308: These two paragraphs seem to be methodology for the numerical study as opposed to results.

This is the presentation of the setup, we decided to include it in the results section so as to not divide the paper into three sections Setup / Results / Discussion with each repeating the three experiments. I agree that this could be moved to methodology, but we believe it will be harder to follow for the reader, as the convergence study is a crucial step of our investigation.

Lines 311 – 312: Could you provide some description or a reference for "parasite fluctuations"?

Yes, we will add : "With SPH, velocity of the continuum can only be estimated through the velocity of the particles in the discretized flow. With the movement of particles in the flow intrinsic to the SPH method, instantaneous measurements can be parasited by fluctuations in the positions of the particles within the sampling window. To avoid these instantaneous effects, the results are averaged over 5 seconds."

Figure 2: It is not clear what the optimum value from the convergence study is. From Tables 2 and 3, I can infer that 1.8 was selected for C_h, but it would be good to explicit state that choice in the discussion of this figure (similar comment for Figure 3).

In both cases, Ch 1.8 has a good convergence, with precision increasing or capping with decreasing value of dp. The other candidate to have a similar convergence is 2.2, however 2.2 leads to higher computational times, thus 1.8 was selected. This will be explained at the end of section 3.1 as a concluding sentence.

Lines 345 – 346: Repetition with "close to the free-surface" and "especially near the free-surface".

OK thank you, corrected

Lines 359 – 360: The statement “guarantee the correct rendering” is very definitive statement, and I would argue all models, no matter how sophisticated are significantly simpler than a real debris flow. I would suggest changing the wording to “develop a more accurate representation”.

OK thank you, corrected

Lines 408 – 410: This sentence is unclear and should be revised, I think the main point is that modelling each individual grain is not feasible computationally.

Reformulated to “Indeed, computational time is driven partly by the number of individual grains represented, the number of collisions and the size of the pores between the grains. Thus, modelling all the singular grains in the debris flow material is not reasonable computationally.”

Line 416: Given, as you say, the assumption of boulder relative density of 0.9 was arbitrary, was there any work done on testing this assumption? Using this relative density assumption is a very interesting method to represent the effect of boulders being supported on the smaller particles within a flow in a computationally efficient way.

Yes, values of relative density of 0.8,1 and 1.1 were also tested. We initially thought we would use a relative density of 1 but values of 1 and 1.1 did not perform as well as 0.9. This was difficult to quantify in a readable way, and it is more of a qualitative statement. In the end, our choice was a bit arbitrary so this step is not presented. Overall, there is improvement to be made to really understand and methodically explore this idea but it would be out of the scope of this paper.

Line 461: Wording around “ensuring features will be rendered correctly” is also very definitive. In my mind there is still a jump between the very detailed work done in this paper and having confidence that we have correctly accounted for all of the chaotic elements within a debris flow. This is in no way a criticism of the work done, which provides a very thorough testing of the numerical method, but I do think some of the more definite statements should be softened to reflect the uncertainty and approximations of any modelling study.

Yes we agree, we think the sentence is misleading : we represent very well our model material, our ‘mockup’ debris flow, but because we don't represent the whole complexity of the debris flow material, this is too definitive. We will rephrase to ” Validating some of the driving processes of the internal dynamics along with simple indicators of the macroscopic behaviour of the flow are steps forwards in modelling more realistically actual debris flows.”

Line 500: “viscosity is thus seen as a calibration parameter” – it was not clear previously that the viscosity was calibrated on a case-by-case basis.

Sorry this sentence is misleading, we meant that the overall viscosity of the surge is what was used as a criterion to validate the model. Reformulated to “viscosity is thus seen as a calibration criterion”.

Line 507: Replace “no” with “not”.

OK thank you, corrected

Lines 507 – 508: Another feature to note is the lateral movement of boulders leading to levee formation which isn't captured in the 2D model.

OK thank you, added :

"This confinement probably leads to an overestimation of the slowing down of the flow and also likely tends to under deposit grains, especially since it cannot represent lateral deposition and formation of levees. Moreover, there is a diminution of …."

Line 535: The creeping threshold is stated as ~ 1, but on line 135, 0.1 is given.

OK thank you, corrected to 0.1.

Line 546: Replace "potential" with "potentially".

OK thank you, corrected