Some comments on the Authors response to the Reviewer's questions for manuscript nhess-2022-173

I appreciate the effort done by the authors to answer the question that I propose. However, maybe due to a not well-posed question, some questions are still unclear to me.

The main one concern the validation. In the laboratory experiment used for the validation, a well-mixed stony granular debris flow is reproduced. One of the main characteristics of this debris flow is that the energy dissipation is due to the collision between the particles and not by the viscosity of the fluid (e.g. Iverson 1997, Takahashi 2007, Armanini 2013). However, in the authors' response to my comment #1, it is highlighted that "From the characteristics of RNG k-ε model [that is used in all the manuscript], the type of debris flow involved in this study was determined as mudflow or viscous debris flow, in which a single-phase non-Newtonian fluid was assumed and solid particles were treated as suspension and mixed with the fluid phase well". This statement is completely in contrast with the used laboratory experiment used and consequently, all the section devoted to the validation of the model is meaningless since the author used a model that could not represent correctly the physical processes involved.

Another point of the validation part regards why the authors do not show the time history of the impact force. Since one of the characteristics of a debris flow impact process is its dynamic changes in time as the experiments of Song et al. 2021 show (the time history is quite complex and is not only represented by a single value!), the "simple" peak value is not sufficient for validating the model used. For this reason, I think that the authors' response "It is demonstrated that the RNG and GMO coupled model in FLOW-3D are able to describe the peak impact force and fluid surface effectively" is not fully trustable.

A third comment regards the author's answer to comment #6 in combination with #12. I know well that long simulations use a high quantity of memory and take long computational times, so for this reason it could be, in some cases, acceptable to use high fixed discharge for a short time. However, I think that the 10-second duration used by the authors is not fully appropriate at least for some of the simulations used. For example, it is clear from Figure 17 that for the simulation with 45° of orientation (Or45) the peak impact force is the last value of the plot (i.e. at 10 second, so at the end of the simulation) but the force has a trend is still increasing! Also for the cases of 60° and 30° (i.e Or60 and Or30), the trend of the force is still increasing and at the end of the simulation (i.e. the end of the plot), the values are very close to the peak values. This means that, at least, in these three simulations (but I think that the same problem arises also in lots of other simulations done by the authors as the one shown in Figure 21) the authors have to increase the time of the simulation until a significant (a few seconds?) decreasing, or at least constant, value of the impact force is visible.

The last comment regards the author's response to comment #9 regarding the force. For me, it remains unclear the meaning of the number that represents the force. Moreover, since the target building is a complex geometry, where these "numbers" are applied? It is quite different if this "number" is applied only to a single surface (e.g. only on the wall with the opening) or it is applied with different values on different surfaces (e.g. on the wall with the opening plus the roof) because the possible consequences are completely different! Here, I speak about "number" since, as in comment #9, I underline that the force is a vector, so a simple "number" does not represent the force: it is still missing the direction of this force and the point where the force is applied!