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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "The use of FLO2D numerical code in lahar hazard evaluation at Popocatépetl volcano: a 2001-lahar scenario" *by* L. Caballero and L. Capra

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This paper is a good attempt to provide new knowledge when delineating affected areas of previous lahars, which can be used, for example, in later studies of several scenarios of lahar hazard assessment in the town of Santiago de Xalitzintla, Mexico.

These are some specific comments about mistakes and correction suggestions :

Pag 4584: according to the Statistic and Geography Nationals Institute (INEGI) of Mexico the population of Santiago de Xalitzintla is less than 3.000 inhabitants. The 10.000 inhabitants correspond to the total population of the municipality of San Nicolas de los

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Ranchos. Xalitzintla town is just part of this municipality.

Pag 4585, Line 1: The authors affirm that “nowadays a glacier is no longer present on the Popocatepetl cone”, which contradicts the findings of other authors like Muñoz-Salinas et al. (2010).

Pag 4586. Section 2: FLO2D is a Two-dimensional One Phase model. Lahars are a two phase flow. Its bulk properties cannot be accurately (at least) captured by one-phase models, as they assume that the solid and fluid phases form a single phase material (Iverson, 1997). Two phase flows can have parts of them in the frictional regime, which are better described by models that use approaches like Mohr-Coulomb (Savage and Hutter, 1989; Patra et al, 2005). For parts with low solid concentrations, one phase models or even hydraulic approaches can be enough (Chow, 1969). Intermediate concentrations need to account for the different properties and interactions between phases, like the inter-particle drag (Dobran, 1991) that cannot be accounted for in one-phase models. Thus, I suggest that the authors should mention the limitations of the used model.

Pag 4586, Lines 20 to 25: The rheological parameters used in FLOD2D are empirical. They are limited for clay, silt and very fine sand (O’Brien and Julien, 1993). In the revised paper the authors pointed out that the solid material of the mixture are clasts within a matrix of sand, silt and clay (Line 25 in page 4585). If the authors are using the empirical data of O’Brien and Julien (1988), it would mean that they are neglecting the presence of clasts. A clear justification of this assumption is needed. Even the parameters used by the authors mentioned in line 2 page 4587 have the same particle size constrains.

Page 4587, lines 7 to 11: A 30 m DEM flatters the topography in such a way that narrow gorges cannot be accurately represented. The result of the use of flattered topography representation is an artificially overspread flow prediction. It is advisable to chose the use of high resolution DEMs.

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Page 4587, line 25: At high concentrations like the used by the authors (0.5), the interphase interactions cannot be neglected. Even more, is it possible that at such concentrations, the effect of particle-particle friction could become important. Particle-particle friction cannot be described by pure fluid models even with the use of empirically adjusted stress tensors, viscosities or "particle temperatures" (like in Dobran, 1991, or Bagnold, 1966, and others), Thus, as described in the paper, it is not clear the accuracy of the predictions, in special if the assumed concentration corresponds only to the head of the flow (Figure 4 in the revised paper) instead of its average.

Page 4588, Section 2.2: The Manning approach is valid for pure fluid flows and floods. Thus, the validity of an approach that extrapolates the use of Manning coefficients originally estimated for inundation flood plains to lahars is not clear. A justification and verification of their use in high particle concentration flows is needed

Page 4588, Section 2,.3: same comment as in Page 4586, lines 20 to 25.

Page 4589, Section 2.4: the assumption of a constant Froude number means that there are no changes in velocities, topographic slopes, cross sections or hydraulic jumps along a natural path. Lahars flowing in natural channels show very complex patterns. In reality, there are several gorges, variations in terrain slopes in the path of a flow towards Santiago de Xalitzintla that should not be disposed of in a model. It is better for this paper if the authors justify the assumption of neglecting the complexity of the lahar flow profile, despite the topographic complexities of the flow path.

Page 4590, Line 18: The total volume of the flow almost coincides with the volume estimated by Miranda (2005).

Page 4590, Lines 19 and 20: If the amount of water is 2.5*10ˆ5 m3, the sediment load 1,6*10ˆ5 m3, and the total volume is 4.1*10ˆ5 m3, the concentration of the flow corresponds to 0.39, instead of the 0.5 initially assumed in the model. This is a contradiction that needs to be reviewed by the authors.

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Finally, according with Flo-2D Reference Manual-2009, the program was developed for hydraulic and hydrological modelling of flood routing, which is in agreement with this revision. Therefore, I suggest that in order for it to be published, the authors should constrain the flow concentration to low concentrations (less than 0.1 as common practice in hydraulic engineering) in order to avoid modelling out of the range of the limits of development of the Flo2D, and keeping the flow within the range of validity of the mathematical approach of Flo2D.

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