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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The temporally varying roles of rainfall, snowmelt and soil moisture for debris flow initiation in a snow dominated system: the compound trigger concept" *by* Karin Mostbauer et al.

Anonymous Referee #3

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This paper aimed to clarify dominant triggers and their interactions that can initiate debris flow in a snow dominated mountainous area. The authors rigorously investigated results generated by their semi-distributed hydrological model as well as observed hydrometeorological variables and deduced several concepts on mechanism of debris flow initiation. Their reasoning on the concepts is interesting and deserves thoughtful consideration. I have several comments that are hopefully helpful for their further advancement.

Major comments:

Their aim was to identify triggering factors for debris flow initiation, and they classified past debris flow events into several groups of which trigger is different each other. Their attempt looks successful within the framework used in this study. However, as they realized and discussed in 4.3.4, their framework is based on the semi-distributed model, and thus it falls short of capability in identifying the differences between locations, while debris flow depends on the hydrological, meteorological and geographical conditions at the specific location of their initiation. For example, in 4.3.3, they discussed a difference between the events occurred in lower elevations and in higher elevations, and found the reason in the difference of soil moisture conditions in relation to the difference of surface soil layer. It may be true, but is a matter of speculation. This example clearly shows a limitation of the framework used in this study. A spatial explicit modeling in combination with a semi-distributed model may be helpful to give a solution to this question.

Minor comments:

1. I did not get the whole picture of the semi-distributed model used in this study. It seems to divide a target area into several zones based on elevations as is shown in Figure 3. If so, the model should have different parameter values for each of different zones, while the parameter values seem to be applied uniformly over the entire study area as is summarized in Table 1. Clear and straightforward description on the model structure is strongly recommended.

2. Lines 22-23 on Page 8: The authors introduce "exceedance probability (Pe)" and several classes based on Pe. I god confused about the classes, in which 1 >= Pe > 0.5 is defined as "high" and Pe <= 0.01 as "very low". In my understanding, Pe <= 0.01 is "very high" because Pe is exceedance probability (not NON-exceedance probability). In Result and Discussion part, they use "high" for Pe <= 0.1 (see Line 32 on Page 11), which is not consistent with their definition.

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