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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.

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REPLY TO REFEREE COMMENT #3

We would like to thank the reviewer for the thoughtful and interesting comments, which we will address in detail in the revised version of our manuscript.

Comment:
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.

Reply:
We appreciate the reviewer's generally positive evaluation and will try to address his/her concerns as completely as possible in the replies below and by adjusting the relevant parts of the manuscript.

Major comments

Comment:
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.

Reply:
We fully agree that our approach lacks spatial differentiation. However, the root cause for this lack of spatial differentiation is not the method (i.e. the model) used. Rather, it

is the (un-)available data that limits a meaningful spatial differentiation.
The most crucial meteorological input, namely precipitation, is typically (and also here) not available on a spatially distributed basis, let alone for the actual source area of a specific debris flow. Remotely sensed precipitation will be very valuable to somewhat alleviate that problem in the future, but currently available remotely sensed time series (going back 15 years or so at best) are at this point insufficient given the very low debris flow occurrence frequencies. Given the highly localized nature of precipitation, especially in summer, calibrating a spatially distributed model on the basis of the available time series of – in our case – three measuring stations, all located at the valley bottom, would not generate reliable additional information. Furthermore, the calibration of a more distributed model would be more problematic and – in the case of fully distributed physically-based models – would encounter many other sources of uncertainties (e.g. model/parameter equifinality, scale of available field observations of physical parameters vs. scale of the modelling application/grid size, the suitability of the model equations for the scale of the applications (e.g. the Darcy-Richards formulation assumes equilibrium over the grid cell, which is only a valid assumption for scales < 1m as recently demonstrated by Or et al., 2015), etc.). These limitations have been acknowledged for a quite some time but no real progress to close the gap between simplicity and complexity has yet been made (e.g. Dooge, 1986; Beven, 1989, 2006; Jakeman and Hornberger, 1993; Sivapalan, 2005; McDonnel et al., 2007; Zehe et al., 2007, 2014; Clark et al., 2011, 2017; Hrachowitz and Clark, 2017). Further, the locations of the debris flows' initiation points are not known (the locations indicated in Figure 1 show the "center of deposition"). Thus even if spatially distributed input and model output was available, this could not be readily linked to the debris flows. In summary, while we agree with the reviewer that the applied model type is not perfect, by using a spatially explicit, fully-coupled model we would only trade-in the limitations mentioned by the reviewer against the (equally relevant) limitations listed above.
In spite of the uncertainties involved we believe that the results of our study allow us to gain more insight into debris flow trigger mechanisms, even though spatially dis-

tributed information is not available. As a side note, the very same problem does occur for every analysis that deals with debris flows – no matter if purely statistical or with help of a hydrological model (any model!). In statistical analyses, such as the intensity-duration thresholds, the implicit assumption is that the rainfall observed at some point is representative of the rainfall at the source area of a debris flow. This assumption is in many cases likely to be violated. We will add more discussion on this point in the revised version of the manuscript, better highlighting the advantages and limitations of the chosen modelling strategy.
As for our remarks on the difference of soil moisture conditions at lower vs. higher elevations (section 4.3.3, page 14, lines 4-11), we agree that there is some uncertainty around our reasoning. However, we somewhat disagree with the term "speculative" as this suggests that the statements were made without any supporting evidence. Clearly, soil moisture build-up can be subject to spatial differences. However, the large scale pattern in soil moisture dynamics (not necessarily the absolute values, though) are very likely to be similar within a region as the soil acts as a low-pass filter, attenuating a considerable part of the high (temporal and spatial) frequency fluctuations of incoming precipitation (e.g. Oudin et al., 2004; Fenicia et al., 2008; Euser et al., 2015). The gradual soil moisture build-up in feedback with drainage and evaporation in the wet seasons is therefore a metric for the general wetness state of a system. In an extreme, illustrative example, it can be considered highly unlikely that when one location in the system is at the highest soil moisture content of the year, that another location is at the lowest soil moisture content.

Minor comments

Comment:

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.*

Reply:

As indicated in Figure 3, the snow routine (fluxes P, Pl, Ps, M, storage Ssnow) is distributed into 100 m elevation zones, while the remaining processes are modelled on a lumped scale. Please note that a “distribution” does not necessarily mean a distribution of model parameters, but can equally refer to the distribution of moisture accounting (i.e. different values for input variables; e.g. Ajami et al., 2004; Fenicia et al., 2008; Euser et al., 2015), which was done here by elevation-adjusting the observed temperatures with an environmental lapse rate, which allows different snow accumulation and melt dynamics at different elevations. We will outline the model structure more clearly in the revised text.

Elevation-stratification of the snow processes and then lumping the remaining processes is the standard layout of a semi-distributed conceptual hydrological model. When necessary, the lumped part would include parallel components, modelling different hydrologically similar parts of the study area, for example differentiating between plateau, hillslope and wetland (Savenije et al., 2010) or other hydrological response units. In our case, we tested different levels of spatial distribution due to different hydrological response units, including for example a parallel wetland component. This did neither improve model performance, nor notably influence the runoff behavior. Thus we decided to go for the most parsimonious feasible model architecture, resulting in a model that consists of an elevation-stratified snow routine and a hillslope component.

Comment:

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.

Reply:

Lines 22-23 on page 8 refer to the exceedance probabilities, which would be highest if Pe=1, and lowest if Pe=0. However, Pe=1 corresponds to the lowest value ever measured/modelled in the study period (e.g. snowmelt M=0), which would suggest a low contribution of the system variable (in this example, snowmelt) to the debris flow triggering, while Pe=0 corresponds to the highest value and would suggest a high contribution. This is stated in the original manuscript on page 9, lines 12-15 as: “On days a specific variable reached values that correspond with a high exceedance probability (see above), the relative contribution of this variable to trigger debris flows was classified as having low relevance, while on days with moderate, low or very low exceedance probabilities, the relative contribution of this variable to trigger debris flows were correspondingly classified as having moderate, high and very high relevance.” We will clarify this in the revised version of the manuscript.

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