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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.

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REPLY TO REFEREE COMMENT #1

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:
The paper is interesting and well written.

Reply:
We highly appreciate your positive evaluation!

Comment:
However, being based on modelling results (that, as also the authors acknowledge, is an oversimplification of reality) the assessments presented in the results and discussion section are somehow speculative.
I believe that less emphasis (i.e. by not mentioning it in the title, for instance) should be given to the so-called "compound triggering concept" that, in my perspective, is quite obvious and possibly over-rated. As matter of fact, Authors have honestly demonstrated (and clearly synthetized in Fig. 7) that in the majority of the debris flows cases they have considered there is a "dominant" trigger (which in most cases is, as usual, precipitation). Thus, despite their modelling effort, I have the feeling that still it is impossible to demonstrate/quantify, without having field monitoring data, the extent to which the other factors where co-influential at the time of triggering.

Reply:
In general we agree with this observation and we will put less emphasis on the term "compound trigger concept", also removing it from the title. Indeed, for the majority of debris flows precipitation has been identified as the "dominant" trigger. Yet, our results (see especially figure 6) clearly suggest how an analysis based on precipitation only is not sufficient for regions as the inner Pitztal and that complementary hydrological information helps to better understand the debris flow initiation process.
We also agree that it is not entirely possible to quantify the extent to which the factors were co-influential (which is the reason why we used three indicative classes of relevance (high, moderate, low) rather than providing numerical values). We would nevertheless here also like to put this comment into some other perspective. Of course a model (actually any model) describing environmental systems is subject to various sources of uncertainty, which were also quantified in our analysis. The actual problem here, from our point of view, is not the model per se as it captures the main features

and dynamics of the hydrological response relatively well. Rather, we think that much of the inconsistencies are driven by epistemic errors in the available precipitation and temperature data, linking to the ever-recurring problem that debris flows are highly localized and thus need highly localized information on these variables –which are typically not available. The same problem will therefore be present in any type of debris flow initiation study, no matter if a model is used or not.

Comment:

In general Figure 4 - together with fig. 5 (and other similar graphs and plots provided in supplementary material) are the "key" to estimate how significant are the Authors findings. However, there is little or no description and discussion in the paper about the NON-EVENT days. It is actually quite clear already from Fig. 4, that the days with debris flows are not that much different (in terms of the analyzed parameters) from many other days in the series. So, please, integrate the discussion.

Reply:

We are not entirely sure to fully understand the reviewer's comment here. Does the comment mean that (a) it is not clear how we include the non-event days in our analysis or that (b) we should discuss why no debris flows have been observed on days with system variables similar to those on debris flow days?

If (a), we have realized in discussions that the probabilistic concept (cf. Berti et al., 2012) used in our paper may not be obvious at first. The key figure is figure 5, where we compare the system variables on the event days (i.e. date of event no. 1, no. 2, etc.) with the marginal distribution of the variables (May 15th to October 15th, 1953-2012). We will try to clarify this in the revised paper.

Thereof, the non-event days are always included in our analysis, as these days (together with the event days) were used to calculate each system variable's marginal distribution, which is plotted on the upper x-axis of the plot. For all events where our interpretation is marked with a ++-confidence, the conditional probability is significantly increased, i.e. the relevant system variable at debris flow occurrence is substantially

different from non-debris flow triggering days. For example, for the events identified as primarily triggered by snowmelt (and interpreted with ++-confidence), 16% of the debris flow events (4 out of 25) were observed when snowmelt was more than ca. 7 mm/d, while the marginal probability for such snowmelt to occur would be only 1%. In Bayesian terms this means that the posterior (conditional) probability of a debris flow to occur if the snowmelt exceeds the threshold value would be 16 times higher than the prior probability (in absolute numbers: 4.5% vs. 0.28%; p(Fisher's exact test) = 0.000095).

Of course, the posterior probabilities for the events interpreted with less confidence, are not as clear. For rainfall, this is due to the temporal (and also spatial) averaging, as we have outlined in the paper (page 11, lines 6-16; page 15, line 32 – page 16, line 6). Here our interpretations were also based on the absence of other system variables that were notably increased (i.e. neither high snowmelt, nor high antecedent soil moisture), which indirectly again considers the non-event days, as the assessment of "no high snowmelt" or "no high antecedent soil moisture" is based on the respective marginal distributions. Conversely, a non-clear attribution of a dominant trigger points, besides potential effects the influence of epistemic errors, towards compound triggering, which we indicated in table 2 by listing triggers by their relevance and visualized in figure 6.

Please note, that due to the limited sample size and the focus of the paper not being on providing probabilities for debris flow occurrence (and thus a blueprint for a prediction model), but to analyse the event's triggering conditions, we did not explicitly provide the posterior probabilities (as demonstrated above) in the paper for our detailed analysis (fig. 5 resp. table 2).

If (b), we on purpose did not address this issue since (as stated above) providing posterior probabilities is not the key focus of our paper. Of course on the majority of days no debris flows occurred although the system variables have been similar to those on debris flow days. This can largely be attributed to non-hydrological factors such as sediment availability. Actually, a Bayesian approach (fig. 6) explicitly considers the fact that not all potentially triggering events do lead to debris flow initiation (cf. Berti et al., 2012, page 16-17).

Comment:
Moreover, Fig. 5, plot "f" clearly indicated substantial difference between the modelled and recorded runoff on 3 out of 6 debris flow events during which observed runoff was available. I believe that, also this fact, deserves some comments/discussion.

Reply:
While we did address this issue shortly on page 16, lines 9-11, we agree that this fact deserves a more detailed discussion, which we will include in the revised version of the paper.

Comment:
I also somehow question the fact that (as mentioned in page 8, lines 24 to 28) the exceedance probability of precipitation was analyzed over the limited period May-October. This choice should be more clearly explained/justified. Also: (i) it is not clear if this probability is based only on the 15 may- 15 oct period of years with debris flows or – rather – of any year in the series. (ii) Is May 15 as lower limit correct ??, as the plots in fig 4 and supplementary material, seem to start in march 15. Please check.

Reply:
The May 15th to October 15th period represents the typical debris flow season in an Alpine environment (e.g. Stoffel et al., 2011), i.e. the period in which debris flows have been reported for the study region. As we base our analysis on comparing the system variables (precipitation, snowmelt, etc.) of days with debris flow occurrence with the marginal distribution of these variables, an analysis only comprising the debris flow season to generate the data for the general distributions was found more applicable. This was stated in the original paper (page 8, lines 24 to 27) as: "Due to the generally very low occurrence probability of 25 debris flow events (i.e. 25 events over 60 years), which potentially may in the following lead to instable and overly discontin-

uous statistical models, we limited the definition of exceedance probabilities (and all other probabilities estimated hereafter) to the period of the year in which all debris flow events occurred [. . .]". We will further clarify this.
(i) The probability is based on all days of the May 15 – Oct 15 period in all years, i.e. 1953-2012, as stated in the caption of figure 5. We will add this to the text.
(ii) Yes, May 15th is correct. We provide the time series plots (fig. 4) from March 15th to show a more "complete" picture, i.e. start and amount of rainfall and snowmelt, as this does subsequently considerably influence the antecedent soil moisture of the corresponding year. (We did not include late fall and winter, though, as the information of how much snow fell during these seasons is already implicitly expressed in the snowmelt data and would only have decreased graph readability). However, we did not realize that this indeed leads to some confusion. We will thereof add a note to the caption of figure 4 to clarify this issue.

Comment:
At least one Figure (picture) showing the physiographic setting of the study area should be added.

Reply:
This is a great suggestion! We agree and will include a picture in the revised paper.

Comment:
In caption of Figure 3, please include descriptions of Abbreviations (now, the reader is posted to Table1 and sect.2.2, thus making it difficult to follow in case – during editorial setup - these elements are placed in different pages)

Reply:
We agree and, as suggested by reviewer No. 2, will include all abbreviations used in this figure resp. section 3.1 in Table 1.

Comment:
Figure 6 should, in my opinion, be eliminated, as it does not really add much real

information, as the concept of combined probability is quite easily understandable even without such scheme.

Reply:

We would strongly prefer to keep this figure in, as it provides an intuitive visualization of the potential (simultaneous) influences of different factors.

References

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