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Hydrology and Earth System Sciences
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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 #2

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 presents results of the analysis of potential factors responsible for debris flow initiation. The study explores possibilities of the use of semi-distributed conceptual

rainfall-runoff modelling results to identify possible critical values of triggering factors which could indicate or lead to occurrence of debris flows. The authors use measured (e.g. rainfall data) and modelled factors (e.g. snowmelt, different underground storages) and try to identify their potential role as debris flow triggering factors in view of corresponding exceedance probabilities.

I find the manuscript in line with aims and scope of HESS. Generally, the paper is well structured. However, there are some issues that need to be solved in order to improve the presentation and discussion of the results.

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

General comments

Comment:

While the main topic of the paper are triggering factors related to hydrological conditions, the authors should give some stress (in the Introduction section) also to other possible factors especially related to geological or hydrogeological conditions. These are only briefly mentioned on Page 3 (line 10). Namely, the geological setting strongly pre-define the possible effects of all the hydrological conditions discussed in the paper.

Reply:

We agree and will add this to the Introduction and Discussion sections. Indeed, it is an inherent strength of our method that – while the geological condition does influence the hydrological condition – the geological condition is not needed as explicit a-priori input. Rather, the role of geology is implicitly encapsulated in the posterior parameter distributions of the calibrated hydrological model. While geology is clearly a highly relevant control on debris flow occurrence, its effect does only become relevant once regions in distinct geological settings are compared. In the study area, the geology is rather homogenous – it is thus reasonable to assume that all parts of that region can be expected to trigger debris flows under similar conditions. However, we fully agree that if the results of this study want to be generalized to different areas, the differences in geology need to be factored in for a meaningful understanding of the underlying processes.

Comment:
The Study area and Data presentation (Section 2) as well as the Model structure and model calibration and validation process (Section 3.1) is concise and informative. Additional information on past successful applications of the proposed hydrological model structure for any other purposes (besides the analysis of debris flow triggering) would be helpful. There seems to be some discrepancies in the abbreviations used for the model parameters in section and the ones listed in Table 1 (e.g. metlf, M, Mgalcier etc). If I understand correctly, only free calibration parameters are listed in Table 1. All the model parameters mentioned in section 3.1 and in Figure 3 should be listed together in one place (Table) in order to enable reader easier understanding of the model structure. Otherwise, it is extremely difficult to follow the explanation of the role of different parameters that could be potentially considered as important in view of debris flow triggering analysis (Section 3.2).

Reply:
Modular and flexible modelling strategies have proven highly valuable for a wide range of studies worldwide in the past (e.g. Leavesley et al., 1996; Wagener et al., 2001; Clark et al., 2008; Fenicia et al., 2014, 2016; Gharari et al., 2014; Hrachowitz et al., 2014), as has the chosen model structure that is functionally equivalent to the wide-spread HBV model and similar models (e.g. Seibert 1999; Seibert and Beven, 2009; Fenicia et al., 2014; Berghuijs et al., 2014; Birkel et al., 2015; Nijzink et al., 2016) and which has been rigorously implemented and tested for the study area of this manuscript. We agree that the different abbreviations should be listed in one place. As suggested, we will include all abbreviations of figure 3 resp. section 3.1 in Table 1. As

for the discrepancies, we will add Mglacier to figure 3, in which it has been erroneously missing.

Comment:
The discussion on the relevance of potential triggering factors in Section 4.3 is relatively lengthy and extremely difficult to follow. It seems that most of the discussion relies on the authors pre-knowledge about the particular characteristics of the debris flow events and, unfortunately, many of the statements on authors speculations. I believe author should put more effort in extracting the most relevant information from the data analysis instead of commenting particular events in view of available measured and modelled data. On possible solution could be classification of the events based on some predefined criteria, one of them could be e.g. seasonality, as this could lead to possible easier identification of the relevance of discussed triggering factors during particular debris flow events (e.g. convective storms occur mostly in the late spring, summer or early autumn; snowmelt occurs in spring). Sections (4.3.1-4.3.2) discussing the role of high-intensity rainfall events and snowmelt could in my view directly fit into some predefined classification criteria (e.g. seasonality). The influence of seasonality is indicated in several parts of the manuscript but should be more clearly pointed out. Data shown in Figure 7 and discussion in section 4.3.4 could be very useful for developing further discussion in that direction.

Reply:
While we agree that some aspects of our interpretations (e.g. triggering by snowmelt with or without additional rainfall?) remain ambiguous, we think that in the vast majority of cases the results as extracted from the hydrological model did allow a conclusive reading. We clearly provided indicative levels of confidence for the interpretation with the "direct support by daily data" column in Table 2, which shows that for 20 out of 25 debris flow event days, our classification displays quite strong direct support by data. In the case of the interpretation of high-intensity short-duration rainfall as debris flow trigger, we also had information on the 10/15 min. precipitation intensities for several

events, which provided additional supporting evidence for our interpretations of high-intensity rainfall as dominant debris flow trigger solely based on the daily precipitation data. To avoid the notion that the interpretation provided is speculative, we think it is necessary to keep the admittedly quite long description, as only from this description our reasoning for direct data support becomes evident.
Concerning the structure, we agree that a classification based on seasonality would be an equally valid option. However, we on purpose decided on the structure at hand based on the dominant trigger (as derived from the results) since this appeared as the most logical structure for us. Of course, convective storms most commonly occur during late spring / summer / early autumn, which is the typical debris flow season. However, in such a high alpine environment, snowmelt does also occur in summer oder autumn, same as long-lasting rainfall. In these cases we believe this should be listed at the same place where snowmelt in spring / long-lasting rainfall in autumn is discussed since the snowmelt / rainfall, rather than the season, would be the most important characteristic. In any case, as the reviewer points out, the dominant trigger and seasonality are closely connected in our study area. As suggested, we will elaborate on this in more detail and more clarity in the revised manuscript.

Comment:
Although the proposed approach of using semi-distributed hydrological model in combination with relatively scarce data is interesting, my overall concern is, that the complementary effect of different triggering factors is not clearly demonstrated (the so called "compound triggering concept"). Namely, in many parts of the manuscript, authors clearly state that for many debris flow events, only single triggering factor was recognized as the prevailing one. It seems the complementary effect of different triggering factors has much smaller role as the authors try to present.

Reply:
We agree that the "compound triggering concept" has not been adequately demonstrated in the paper. As also suggested by the first reviewer, we will significantly

reduce the emphasis on this concept, while simultaneously better mapping out the complementary effect when applicable. For this reason, we also will skip the phrase "compound trigger concept" in the title.

Specific comments

Comment:
Page 2, Line 5: What is meant by "hydrological disposition"?

Reply:
The term "hydrological disposition" was adopted from Kienholz (1995) and refers to the antecedent hydrological conditions of a watershed or hillslope. This was also mentioned in the original paper (page 2, lines 11-12) as: "Yet, little is known about the influence of other factors such as snowmelt or the antecedent soil moisture, which may increase a catchment's susceptibility for debris flow initiation ("the disposition concept"; Kienholz, 1995)." We did not realize that this term is not commonly known (probably mostly by the natural hazard community) and will thereof clarify this in the revised manuscript.

Comment:
Page 11, lines 12: I believe it is not useful to discuss possible hourly threshold rainfall intensities derived from daily rainfall data.

Reply:
Our explanation here was insufficient and thus seems to have sparked a bit of confusion. The hourly rainfall intensities are not "derived", which would imply some degree of uncertainty around them. Rather, they are the physically lowest possible limit to hourly rainfall intensities. In other words, e.g. during a day with observed rainfall of 24 mm/d there must be at least one hour during which rainfall intensities are equal or exceed 1 mm/hr. We believe that the discussion with perspective to hourly rainfall intensities adds some value here, as debris flows are mostly linked to spatially and temporally highly localized events. Demonstrating and illustrating that the observed

daily intensities also map to sufficiently high intensities (without further assumptions or uncertainties involved) to exceed previously estimated hourly thresholds therefore helps in our opinion to provide a more robust justification of our interpretation.

Comment:

Page 13, line18: Related to general comments above, could precipitation be generally considered as a factor of low relevance for debris flow triggering during some seasons or maybe months?

Reply:

For the study region we definitely think that the results presented in the manuscript strongly suggest that there are situations, where precipitation alone is probably a necessary, but not sufficient factor for debris flow initiation. As discussed in the previous comment concerning seasonality, we will clarify this in the revised version.

Comment:

Page 13, lines 19-20: Do authors have any data that would support the speculations about the occurrence of convective cells?

Reply:

The lines in question discuss the occurrence of event no. 2 and 20, which we interpreted as triggered by snowmelt. As we write, however, "the absence of observed precipitation and – in case of No. 2 – only moderate maximum temperature, suggests that precipitation is likely to be of low relevance [. . .], although the occurrence of small convective shower cells cannot be fully dismissed." We disagree that this is a speculative statement as we merely do not – and, given the possibility of epistemic observational errors, – cannot exclude the possibility of the occurrence of convective (i.e. small scale) shower cells, as the occurrence of un- / underrecorded convective rainfall is a common debris flow trigger (section 3.2.1; see also the study mentioned of Borga et al. (2014) as pointed out on page 16, lines 4-6). This uncertainty is also reflected in the "direct support by daily data" column, where we marked no. 20 with "strong support"

(as considerable snowmelt was recorded), but no. 2 only with "moderate support" (as snowmelt may not have been sufficient for debris flow initiation).

Comment:

Page 14, line 14: In my view, the complementary nature of triggering factors is not so evident or significant as the authors try to present. Could they clearly demonstrate (e.g. for a particular debris flow event) possible evidences of the "complementary" effect?

Reply:

See reply to general comment on "compound triggering concept". We agree and will adapt this in the revised manuscript.

Comment:

Page 1, lines 24-27: The last sentence of the abstract is extremely long, contain too much information and is consequently unclear. I suggest to rewrite and shorten the sentence.
Page 3, line 17: "Meteorological conditions" instead of "meteorological forcing"?
Page 8, line 13: Related to my general comment on presentation of model parameters. What is parameter SI? As far as a can see, here it is mentioned for the first time and its explanation is give in line 9 (Page 9).
Page 9, line 12: ... on days when a specific variable ... (add when).
Figure 5: The meaning of red vertical lines should be explained in the figure's caption.

Reply:

We agree and will rewrite / include information as suggested.

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