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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Large scale analysis of changing frequencies of rain-on-snow events and their impact on floods" *by* D. Freudiger et al.

D. Freudiger et al.

daphne.freudiger@hydrology.uni-freiburg.de

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Overall Comments:
While the topic of RoS is certainly interesting and of global relevance, I find that this analysis lacks firm roots in compelling, generalizable research questions and interpretation of the results in the context of hydrologic processes. Furthermore, while the identification of criteria for selecting RoS events is compelling, the approach applied herein appears very arbitrary, without justification or validation. Additional weaknesses relate to inadequate description and defense of the methods.

Reply:
We thank Referee 2 for a number of comments to improve the manuscript. We will

specifically improve the development of the research gap and justification of the niche this work aims to fill and will clarify the methodological aspects, which seem to have caused some misunderstandings (see details below).

General comments

- Study questions are not terribly compelling or generalizable, and the Results don't clearly map to the study questions. It appears that some interesting study questions might be embedded in the Results, but the authors need to identify them explicitly and then reorganize the Results and Discussion around those student questions.

  Reply: We did phrase two clear objectives: to derive criteria for those RoS events that have the potential to cause floods, and to analyse historical trends in the driving hydrometeorology of RoS events with such criteria over the past decades. We think that such a data-based study at the large (almost continental) scale is justified because the RoS process is often mentioned as a potentially increasing contribution to the flood hazard in warmer winters. In the revised manuscript, we will review and improve the introduction to better define the corresponding research gap and research questions leading to these objectives and also make the link to them clearer in the discussion section where necessary.

- The authors regularly reference flood risk (e.g. page 17, lines 15-16), but their analysis does not actually consider flood risk. Flood risk is characterized by a probability of an event occurs AND the consequence of that event (e.g loss of life, economic damages). I believe this seriously weakens the paper to use flood risk as a justification and then to not actually consider flood risk. While I am not suggesting that evaluating flood risk is necessary for this paper as it can require a large effort, I am suggesting that it is not a valid basis on which to justify this study. Instead, the figures could contribute more by relating the variability in RoS events to hydrologic processes, which could be transferrable to other areas and time periods.

Reply: The word 'risk' only appears twice in our manuscript and in rather general motivational phrases. One mentioning refers to McCabe et al. (2007), who use the term throughout their paper, where possibly they should have used the term 'flood hazard', but we referred to the term they used. We agree that what we look at is trends in the hydrometeorological drivers of flood hazard from a specific flood generation process, i.e. RoS, and not at flood risk (which would have to include damage potential) and will change the terminology in the second case. We don't fully understand the second part of the comment but refer to our detailed comments on the involved hydrological processes below.

- Results: This section reads as a blow-by-blow (or day-by-day) account of precipitation depths, which makes it dull to read and difficult to identify the key findings. Why should the reader retain any of this content? The authors should rewrite the section, pulling out only the key findings so the reader doesn't have to wade through all of the non-essential details to understand the key points. Unfortunately, because the Methods and Results were so difficult to read, I could not easily assess the validity of the interpretations.

Reply: The evolution of the hydrometeorology and hydrology (RoS and resulting flood) of the 2011 event is the key to the derivation of definition and critical thresholds of flood causing RoS events at the scale of large river basins in central Europe. Hence, we think that a detailed re-analysis of this event is an important component of the study that can be presented as the first part of the Results section. We will better clarify its role in the revised paper and rephrase the section.

- The interpretation of the Results lacks any deep consideration of hydrologic processes. Aside from elevation, what explains the variability that the authors are reporting between sub-basins? For example, there is no discussion on how geology, land use, or connectivity to groundwater varies between the basins. To say (e.g. page 14, line 20) that a decreasing trend in RoS events for a single basin tells the reader very little that can be applied elsewhere.

Reply: The (almost) continental scale of the study requires considering processes dominant at that scale. In the case of our study these are the hydrometeorological rain-on-snow magnitude and temporal evolution processes relevant to flood generation at the large scale. We will improve the description of dominant processes of RoS events at different scales to justify the exclusive focus on the hydrometeorological processes. Our study cannot contribute to an improved description or understanding of runoff generation processes at the plot, hillslope or small catchment scale, for which additional, more high-resolution data would be essential. Even at the catchment scale, however, the input (RoS) is judged the main driving factor during such events (see for example the conclusions in a current HESSD paper by Rössler et al. doi:10.5194/hessd-10-12861-2013)

- A major element of this paper is proposing criteria for identifying a RoS event, but I find the criteria to be arbitrary and their evaluation to be missing. For example, in Section 2.4: The authors give very vague set of thresholds (bottom of page 8), with a reference to Kohn et al. 2013, for defining RoS events. Despite the fact that these may be published elsewhere, more details, even briefly, are needed regarding how those thresholds were developed. Rather than simply saying "significantly" or "substantial," as was given (but not justified) in the abstract. The authors come back to this on page 12 (lines 5-7) and in the Discussion (page 17), but there is no justification for using 10mm SWE, 20

Reply: The abstract does not contain the terms ‘significant’ or ‘substantial’. The term ‘significant’ is used throughout the manuscript only, where we refer to statistical significance in the trend analysis. We can change this into ‘statistical significance’. The term ‘substantial’ is used in the methods section to describe the general approach towards finding the thresholds for flood generating RoS events. Here is where most likely the referee misunderstood: in the methods section we describe HOW we will find the thresholds through the reanalysis of the 2011 RoS caused flood event. The thresholds themselves, i.e. the numbers we determined by the analysis of this event are the result of this analysis. Hence they are not at all arbitrary. We will clarify this in the revised manuscript: The way they were determined is described in the results sub-section 3.2. In Figure 3, first, all potential events when rain fell on snow are shown, regardless of the amount. But most of these won’t have any detectable impact on the discharge, i.e. did not and will not cause a flood. In this study we wanted to identify only the RoS events which cause floods and therefore determined the thresholds based on the known flood event in 2011. The selection of the thresholds was then ‘validated’ on a number of other past events, that were known to have been caused by rain-on-snow processes (described e.g. in reports, Table 1). We regret that the use of the term ‘validation’ may have caused different expectations and will clarify that this is what we meant by validation.

- The Introduction reads like a literature review, not a statement of the status quo and justification for this study. Also, consistent with the rest of the text, there is no discussion of mechanisms or processes in the Introduction.

  Reply: We agree that some sections could be improved to emphasize the “what” rather than the “who”. The general structure indeed describes the little that is known about the rain-on-snow process at the chosen spatial scale first, and then what is known on large-scale trends, which so far have not considered flood-

generating RoS events specifically. Hence the effect of these known trends is unclear and this is the gap our study addresses (see earlier comment).

Technical issues

Reply: All technical issues that refer to terminology, phrasing etc. will be addressed in the revised manuscript and commented on at re-submission. Here, in the online reply, we only reply to the more fundamental ‘technical issues’ raised by Referee 2:

- Figure 1 – it would be helpful to add a delineation of the sub-basins. The location of the gauges isn’t really enough to show the size and shape of the basins. Also, if there is any way to show topography, maybe as an inset figure, this would also help understand the basins’ drainage patterns. Also, please give the elevation ranges for the three HAD zones, either in the figure caption or in the text. The elevations referenced within the text (bottom of page 5, top of page 6) do not seem to map to Figure 1 at all.

  Reply: The purpose of the figure is an overview of the basins and locations referred to throughout the paper. More basin boundary lines, esp. of nested basins, would make this figure very busy. However, the subdivisions in alpine, upland and lowland are clearly shown with the hatched background. As suggested by Referee 1, however, a generalized river network could be added for a better overview of drainage basin shapes. The topography ranges in the text as well as the zoning are those given by HAD, which however only covers Germany, and hence shouldn’t be plotted into the map. A topographic map in the Figure, again, will make the figure too busy.

- Page 5, line 22 – “only a small part of Southern Germany” – please be specific regarding how much is a “small part” by giving the area. This is important because it may influence the reliability of the results for the alpine sub-basin type.

Reply: This sentence or the entire paragraph is indeed confusing and needs to be re-written (see previous comment). What is meant is that the upper basins of Rhine and Danube herein referred to as the ‘Alpine’ subbasins consist of Swiss and Austrian parts of the basin plus a very small area in Germany, which the HAD classified as “alpine”.

- Page 6, lines 17-19: this sentence on quality control has grammatical issues and is confusing. What criteria were used for suitability in this study? And what kind of post-flood corrections were made? Since this study emphasizes floods, substantially more information is needed on how “minor” the corrections were, how many corrections were made, and how they were made.

Reply: The sentence was a bit too dense. We did not make any corrections ourselves. What was meant is that in some cases the most recent data was only the raw data. The authorities usually use hydraulic modelling or revise rating curves to correct some of these peak discharge values later on (in the years after a flood or whenever they get around to it). This may change some of the actual peak values in future records. What we meant to say is that a few m^3 more or less will not change the result of the trend analysis. The section will be rephrased.

- Please be sure to define all variables (e.g. Ta, Mf) within the text.

Reply: All variables are defined within the text. We will review if some definitions can be moved closer to the equation, but we don’t think that a repetition would be wanted by the journal.

- The temperature index model is quite simple and there has been a pretty extensive body of work describing the issues with it. The authors should at least

acknowledge this body of literature and some of the limitations of the model then justify why those limitations are not important to answering the questions posed here. Furthermore, the justification given, based on Ohmura (2011) that the model is “sufficiently accurate for most practical purposes” is far too vague.

Reply: We will add more references to respective large-scale applications for better support.

- Applying the constant value of Mf = 3mmC-1day-1 seems like a pretty substantial assumption that is only qualitatively justified with a statement that snow melts quickly in Germany. Can the authors think of a more compelling (perhaps quantitative?) way to verify that this is a valid assumption? Did the authors perform the sensitivity analysis for Mf as they did for Tb?

Reply: A sensitivity analysis was performed for Mf as well for the trend analysis and the parameter was rather insensitive, since the trend analysis considers relative changes.

- Page 7, Line 20: When and who did the “before” work to demonstrate the lack of sensitivity to Tb?

Reply: Our own study, i.e. the sensitivity analysis to Tb. See previous comment. We will clarify this in the text.

- Page 7, line26: What does “from 2 to 1 August of the following year” mean? Also, does your hydrologic year start on August 1? If so, why? I thought hydrologic year always started on Oct. 01.

Reply: We agree that this is phrased poorly. Since August is the month with the least likely snow accumulation, snow pack was reset to 0 everywhere in August. There are no calculations whatsoever that accumulate or relate to the year in any way. Therefore, any mentioning of a hydrological year is not necessary. We will rephrase this section.

- It isn't clear to me how the 300 gauges were used, since only 12 gauges were used to define the relationship between discharge and precipitation depth.

  Reply: They were used in the analysis of the January 2011 flood. The return periods were calculated and mapped in Figure 2 to illustrate the hazard of the flood event of January 2011. For the rest of the analysis, only the discharge records at the outlet of the selected large sub-basins (12 gauges) were used.

- Could table 1 be more quantitative? That is, what does "flooding all over the river" mean? What is a "large floodings" or "large scale floodings" relative to "small floodings?" Ideally, the authors would give area or return year intervals to qualify what large or small is. Also note that flooding should be singular, not plural. Floods can be plural, but flooding cannot.

  Reply: 'Floodings' will be replaced by 'floods'. Historic RoS events that caused floods are overall not well documented and the information on these events comes from diverse textual information sources and unfortunately cannot be quantified more precisely.

- Page 9, line 20: please be more specific about what "trends were calculated." The relationships between what? And why were these calculated? I don't see how they fit into the study objectives. Also, why do the authors conclude that the relationships are linear? This assumptions needs to be justified. Finally, what is

the expected value based on?

  Reply: The analysis of time changes (trends) in fact is THE main objective of this study as stated at the end of the introduction (see earlier comment). As explained on page 13239, lines 17-19, temporal trends were calculated for the equivalent precipitation depth, the snow cover, the rainfall, and the snowfall over the time periods 1950 -2011 and 1990-2011. The assumption of a linear trend with time is indeed a strong assumption, which however is very often made in hydrometeorology for pragmatic indication of direction and magnitude of a change. The reason why a linear regression was chosen is the presence of censored data, which make the trend analysis complicated with methods where outliers are statistically disclosed. This is explained on page 13250 lines 4 to 10 in the discussion and the discussion can be improved in the revised paper to include the assumptions.

- Page 12, lines 7-9: I don't follow how the 6 day duration of the Jan 2011 storm leads to "the basin response time for the selection of RoS events." How does this helps to select RoS events? And how is this used in the analysis?

  Reply: In the case of January 2011, all basins responded within a maximum of 6 days. Hence, we allowed the response times for all basins to vary between 1 and a maximum of 6 days depending on the event, which allows taking into account different antecedent conditions and different concentration times for the different basins. The explanation will be clarified (see also reply to Referee 1)

- Figure 3 needs further interpretation. What is the difference between selected and historic events? What does a negative correlation (3j) indicate? What can the authors say more mechanistically about why discharge from the alpine and upland sites would be better correlated with equiv. precip. depth?

Reply: See previous comment on derivation of RoS event criteria. We will improve the terminology here. A lower correlation shows that runoff generation is not strongly RoS driven. The better correlation in the Alps shows that the Alpine discharge is largely snowmelt driven and therefore the role of RoS events is also more important.

- Page 13, lines 12-13: "amplifies the observation in Fig. 3 that RoS events do not necessarily cause the highest floods." I don't see this in Figure 3, and don't see where it is explained in the text. Can the authors more explicitly point out where they see evidence of this in Figure 3?

  Reply: As answered to Referee 1, in Fig. 3, only the equivalent precipitation depth during RoS events is presented, i.e. the sum of rain and snowmelt during the RoS events. As both Referees found this confusing, the explanation will be improved in the revised paper.

- Figure 7: It is generally not acceptable statistically to evaluate discharge records for <30 years. Please justify the selection of this short period of time. It is particularly important since the authors are finding such radically different trends for the long-term vs. short-term periods.

  Reply: The WMO defines the period 1961-1990 as a standard reference for impact studies on climate change. Therefore the authors find it of interest to study the entire period 1950-2011 as well as the period after the reference period, even if the time lapse is shorter than 30 years, it still gives some indication about the variability of RoS events in a context of climate change.

  The 30-year rule mostly refers to the derivation of climatological normals and can be transferred to frequency analyses of hydrological characteristics as well, yes. Trend analysis, however, is more governed by statistical assumptions and

the hypothesis on the trend or change. As the Referee commented on above, the assumption of a linear trend is a strong assumption. One common approach to assess non-linear trends is to analyze linear trends for a number of different, potentially overlapping, time periods. This is what we have done here. The reasoning for the choice of sub-period is given in the text.

- Page 20, lines 4-16: I don't find this justification compelling at all, especially given the very brief (1 sentence) explanation of the sensitivity analysis (which was performed only for Tb, right?). I am thus not sure that any of the trend analysis is terribly insightful.

  Reply: We disagree and in fact as previous comments by both Referees show, the low sensitivity of the trend analysis to the parameterization of the snow model is important to justify the approach (also see above).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 13231, 2013.