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Storm type effects on super Clausius-Clapeyron scaling of intense rainstorm properties with air temperature

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Second Response to Editor/Reviewers:

The itemized response is listed below. The main addition to the paper is a new Fig 3 in Supplementary materials that shows the relations of precipitation intensity to dew point temperature for the station Wynau (see also response below). In addition to the itemized responses below we have made some small grammar changes in the text and completed one reference to a now published article (Fatichi et al., 2015).

REFEREE 1: a) The only major comment I have relates to the uncritical presentation of trends for the gauges in the period 1981-2011. The authors should add some cautionary statements about the relatively short period used for the investigation, and that the trend might very well be due to natural oscillations rather than a change in climate.	RESPONSE: We agree with the referee that the reported trends in the number of intense events in the period 1981-2011 may be due to natural oscillations. In fact we did not intend to imply that this result is a consequence of climate change. In line with the referee's concern we have reworded the last paragraph of Section 3.4 to read "The change in the frequency of convective events in our study, defined by the number of events per year during which the peak 10-min intensity exceeded the threshold I*, shows that 90% of the stations exhibited upward trends and 30% were statistically significant in the period 1981-2011 (Fig. 11). Statistically significant trends (Mann-Kendall test) were distributed across the country, suggesting that a shift towards more intense (convective) storms in this part of Europe in the April-September period over the last 30 years may have taken place. This increasing tendency may be connected with a general warming and rise in convective available potential energy and more lightning (Romps et al. 2014), but also be part of natural variability and oscillations connected with large scale atmospheric circulation patterns (see also Fatichi et al. 2015). Searching for possible changes in atmospheric circulation and the occurrence of intense convective rain in the Alpine area remains an important open research question."
REFEREE 1: b) Minor comments: Figure 8 caption should state that this is for I_m statistics.	RESPONSE: That is correct. We have added the variable notation to the caption in the manuscript and supplementary materials.
REFEREE 2: a) First of all, it seems that the two different ways of classifying events (lightning and beta methods) produce quite different results (Fig.6, Table 1 and 2), which to me suggests that the results are quite heavily dependent on the method of distinguishing between convective and stratiform events.	RESPONSE: The main result of our work comes from the direct comparison of lightning and no-lightning event subsets. The convectivity index is used only as a reference to see if it captures the same effects of increasing slope with higher beta - and it does. The convectivity index is an imperfect measure because it directly uses rainfall intensity to estimate beta. The idea is that it can be used as an approximation when there is

Could you elaborate a bit more on that?	no long-term lightning data available. What is relevant is that for large beta the station-averaged slope estimate approaches that of the lightning subset. To clarify this better we have added the statement "Analyzing the different event subsets based on the convectivity index beta shows that the highest convectivity range (beta>0.8) approaches the station average of the lightning subset, especially for the 1-hr data, but with much higher variability in scaling slopes between stations. This suggests that the index may be used as a first approximation to identify convective events when long-term lightning data are not available, provided a reliable estimate of I* can be found." in Section 3.2.
REFEREE 2: b) Secondly, the authors do mention that moisture availability might be an issue at certain stations. I would be interested to see how the results change if the analysis were repeated for Td rather than T. Perhaps the authors can briefly touch upon this by adding a figure of slopes for Td rather than T?	RESPONSE: We estimated the slopes also for dew point temperature Td at a few stations and present an example for station Wynau in Fig 3 in Supplementary materials. The results do not change: the difference between lightning and no-lightning sets remains. However the variability in the slopes is greater and the fits appear less good for Td. We do not observe a clear threshold temperature effect for high Td at the stations we tested. We are of the opinion that redoing the whole analysis for dew point temperatures would go beyond our original aim and not change the message of the paper. We have added the statement "This difference between no-lightning and lightning slopes is evident also when dew point temperature is used instead of air temperature (see Fig 3 in Supplementary materials)." in Section 3.2 when referring to Wynau. The humidity limitations are addressed in Section 3.3. We have added here the statement "As an alternative to using precipitation-dew point temperature relations to identify moisture limitations it is possible to define moisture-limiting threshold temperatures from data such as presented in Fig. 9 for different daily rainfall sums and/or event totals."

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