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Interactive comment on "Future extreme precipitation intensities based on historic events" *by* Iris Manola et al.

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We thank the reviewer for her/his valuable comments and useful citation suggestions. We have revised the document accordingly and addressed each of the comments and suggestions in this response.

Many studies purport the best way to project rainfall for a future climate. In this study an honest and unambiguous assessment is presented comparing different methods of storm projection. As far as I am aware, this is the first study of its kind, and will be of interest to both the research community and practioners alike. The commensurable results are very promising, and the uncertainty in the results presents a need for a greater understanding in this field. This is a very worthwhile contribution. My suggestions are

very minor and primarily focus on expanding the literature cited and ensuring all assumptions have been documented. I look forward to seeing the published manuscript. Minor comments:

I think you need a reference or two and a sentence on the assumption of constant humidity. I have no doubt that this assumption is fine but on Page 3, Line 32 you state you assume constant humidity and then in Line 21 of Page 10 you state humidity is expected to change. I would insert a sentence or two on the predicted changes of humidity on Page 3 and reference accordingly (say from the IPCC reports) so the reader can then make an assessment of the validity of this assumption. I stress that this assumption is valid – it just needs to be communicated.

We thank the reviewer for this comment. In the initial manuscript we state this assumption on page 8 lines 24-27: "A multi-decadal observational analysis in the Netherlands shows that the trend in extreme precipitation can be explained by changes in dew-point temperatures (Lenderink et al., 2011). In the same study, a similar long-term trend between T and Td indicates an almost constant relative humidity with time, which implies that changes in T scale with changes in Td. Also the KNMI'14 scenarios project no change to a small decrease in the future relative humidity, depending on the scenario." We will include these two references in page3, line 32 to make clear that it is a valid assumption.

Section 3 would read better without the subheadings. It currently feels a little disjointed and repetitive. The reason is that you start by comparing the results of the Pi-Td scaling to the Harmonie model and then repeat the presentation of the Harmonie results in Section 3.2. This could all be synthesised into one section. Presenting all the panels up front in Section 3 would read better and grouping the results for the overall precipitation intensity in one paragraph would also read better.

We agree and have now merged the different results section to avoid repetition. The new results section would then read as follows: " 3. Results

Fig. 4a shows the historic event as simulated by a representative ensemble member in Harmonie, at 9am. The relevant future event for the same member as simulated by Harmonie model is shown in Fig. 4b, as resulted by the Pi-Td method in Fig. 4c and by the linear delta transformation in Fig. 4d. It is shown that the maximum Pi is clearly increased in all three methods. As the Pi-Td and linear delta methods only modify precipitable areas, the future spatial pattern remain unchanged compared to the historic simulated event. Conversely, the simulated future event differs in both intensity and precipitable pattern. The main body of the precipitable area is shifted towards the northeast in this member, mainly due to changes in horizontal winds. The variability between the different members primarily results from alterations in the horizontal winds and the convection, due to changes in the surface temperatures, which may shift or change the structure of the clouds. As the event evolves in time, the dynamic heat fluxes and the rapid drying of the soil induce temperature deviations that reach ±4°C locally, thereby influencing the convection and the horizontal winds. One interesting outcome in the simulated future weather method is that, despite the temperature increase and the moisture supply, the overall size of the future precipitable domain in all members remains relatively similar to the historic event. A possible explanation could be that, due to the stronger updrafts (caused by extensive warming, and resulting in increased convection and Pi), stronger downdrafts might be imposed at the outskirts of the clouds, thereby preventing them from expanding further. This may also explain the low or negative scaling that is observable in the low percentiles: as the Pi grows faster spatially within the same domain-size and reaching higher maxima in the future event, there are smaller chances of finding light precipitation. The box-plots of Fig. 5 depict the intensity increase of the three methods compared to the simulated historic event for all seven members and for various precipitation percentiles at 9am and supplementary at 2pm, when the event goes towards its decaying phase. In the Pi-Td method, following the observed scaling of Fig. 2, the lower percentiles (25th) increase with a rate of ΔPi around the CC rate (7%/°C). The medium percentiles (50th) increase between 2CC and over 3CC, and the high percentiles increase from 2CC up to 3CC. The rate of

increase decreases slightly for the very high percentiles, reaching a maximum rate of 2CC. There are no considerable differences between the intensity increase at 9am and 2pm, while some variance is observable between the different members, due to slightly different initial conditions of Pi and Td across the ensemble. In the linear delta method the increase is a constant 11.8%/°C with no the variance between the members. The overall duration of the event in both Pi-Td and linear delta remain unchanged compared to the historic event. On the other hand, the simulated future weather method in Harmonie in Fig. 5 shows deviations in the response of the model in the morning and in the afternoon. The main Pi increase takes place during the first hours of the event, while the rate of increase later reduces, possibly due to the reduced moisture supply that results from the extensive precedent rain. In more detail, the very high percentiles in the morning increase at a rate that lies between 2CC and 3CC, the high percentiles even exceed 3CC and the medium percentiles cover the range of both the high and the very high percentiles. The ΔPi for the lower percentiles varies considerably between the different ensemble members, ranging from a negative ΔPi to a 3CC rate. In the afternoon, the overall rate of increase is substantially decreased, with an average intensity increase of CC or lower, while some negative values appear in all percentiles. Overall, the total increase in the precipitable water for the entire event duration for a 2°C of warming in the Pi-Td method is 36%, which is about 17%/°C, the total increase in the future weather method is 27% (or 13%/°C) and the total increase in the linear delta transformation is 25% (or 11.8%/°C). ”

I think somewhere in the discussion or conclusion the fact that storms may change in their duration/type/frequency should be acknowledged as something that isn’t considered here e.g. Molnar et al., (2015). We touched upon this briefly in the conclusions section, page 17, lines 5-10. This section is now enriched with the conclusions from Molnar et al. 2015 as follows:

“The Pi-Td method also has limitations, as it focuses on the precipitation-intensity changes, while it does not answer questions on spatial distribution and time evolution. Different precipitation types may also show different precipitation behavior with the temperature increase, as seen in Molnar et al. (2015), where observations showed that the intensity increase with temperaturein convective events is higher than that of the synoptic storms. It should be stated that none of the three methods include information on changes in return-period of events, or changes in the synoptic state of the atmosphere. For example, it is suggested that in the future rate of precipitation, intensities with temperature may decline over the UK, due to the more frequent occurrence of anticyclonic systems (Chan et al., 2016), indicating that there is a possibility for some change in the future Pi-Td scaling, in some places.

The conclusion (and assumption) of non-changing spatial patterns/size needs to be discussed with in line with the current literature. See Guinard et al., (2015); Wasko et al., (2016) ; Lochbihler et al., (2017). This will help strengthen the findings presented here.

The relevant text is now revised as follows:

“Nevertheless, in the model, the total precipitable coverage remains practically unchanged with temperature change, as is also assumed in the two statistical methods. This case study finding might be contradicting with the recent observational study of Lochbihler et al., (2017), where Dutch radar precipitation data were used, to conclude that on average the precipitable cells increase with increasing temperature and precipitation intensity, especially at higher dew point temperatures. On the other hand, Wasko et al. (2016) found evidence that precipitation intensity in Australia increases with temperature, while the storm’s spatial extent decreases, as a redistribution of moisture toward the center takes place at the cost of the outer region of the precipitable area. The model study of Guinard et al. (2015) supports that the changes in precipitable structures with temperature are sensitive to the climatic region and the season.”

None of the figures have the panels labelled (e.g. a, b, c, d) – Figure 5 is not-top/bottom.

This is now fixed.

Line by line comments:

Page 1, Line 20: The line break isn’t needed.

Is now corrected.

Page 1, Line 31: Changing antecedent conditions is also important to understandin this context and should be acknowledged, e.g Ivancic and Shaw (2015) and Wasko and Sharma (2017).

These citations are included in line 24 of page 1 as follows:

“Different types of flooding may result from extreme precipitation, while the antecedent soil conditions also play a role on stream discharge levels (Ivancic and Shaw (2015) and Wasko and Sharma (2017)”

Page 2, Line 20: I would cite Fowler et al (2007) here.

Fowler et al. 2007 citation is added at this line.

Page 3, Line 9: I think Lenderink and Attema (2015) needs to be cited alongside this reference.

Lenderink and Attema (2015) citation is added at this line.

Page 5, Line 1: A reference to these changes in storms would be beneficial here.

The relevant citation for these findings is KNMI’14, now stated more clearly in the text.

Page 7: Line 5: Global studies could be cited here, see the following papers: 10.1029/2011GL048426; 10.1002/2016GL071354

The citations are included and the text is modified as follows:

“For example, the relation between extreme precipitation intensity and temperature has been found to reach two times that of the CC scaling, i.e. up to 14% per degree of warming (Lenderink& Van Meijgaard, 2008; Sugiyama et al., 2010; Panthou et al., 2014; Attema et al., 2014; Allan, 2011; Berg et al., 2013). This scaling relation shows someÂălarge spatial inhomogeneity (Wasko et al., 2016), with the strong scaling found mainly in the mid- and high latitudes, while in the tropics extreme precipitation intensities are found to exhibit even a decrease with increasing dew point temperatures (Utsumi et al., 2011)."

Page 7, Line 9 and Line 21: Both Molnar et al., (2015) and Wasko et al., (2015) show different types of artefacts related to increased short duration convective rainfall at higher temperatures resulting in higher scaling.

Those citations are now added in the text.

Page 7, Line 28: The statement that the sample size is large is vague – maybe state the number. Also state explicitly that all precipitation pixels were used. I couldn't tell from the text but I assume this is the case.

This temperature range includes 97% of 8 years of hourly summer data of 1x1km2 resolution for the Netherlands. Indeed, all precipitation pixels are used. This is stated now more clearly in the text.

Page 8, Line 5: Another manuscript which comments on this explicitly is Bao et al (2017).

This citation is now included.

Page 8, Line 15: I think you need a reference on the statistical artefacts – one such paper is Wasko et al., (2015) which relates to embedded storms, another is Molnar et al., (2015) relates to mixing of storms. Also Hardwick-Jones et al (2010) is usually cited in relation to moisture limitations.

The citations of Wasko et al., (2015) and Hardwick-Jones et al (2010) are now added in the text. The statistical artefacts referred at this sentence refer to the levelling off of the CC scaling, while Molnar et al. attributes an observed increase in this scaling

to the mixing types of the storms. We believe that this citation fits better in the "Pi-Td relation" section and is suggested ti be added there.

Page 8, Line 17: remove "the"

The "the" is now removed.

Page 8, Line 25: Again also cite Lenderink and Attema (2015).

Lenderink and Attema (2015) citation is added at this line.

Page 10, Line 32: The unchanged spatial pattern is also true for the delta changemethod – could be stated here.

The results section is re-arranged, as seen the second point of this revision and this statement is now more clearly put for the reader.

Page 14, Line 14: Around here a reference back to Figure 5 would be beneficial.

A reference to Fig. 5 is now made to make this point of the discussion more comprehensive.

References:

Bao, J., S. C. Sherwood, L. V. Alexander, and J. P. Evans (2017), Future increases in extreme precipitation exceed observed scaling rates, Nat. Clim. Chang., 7(2), 128– 132, doi:10.1038/nclimate3201. Fowler, H. J., S. Blenkinsop, and C. Tebaldi (2007), Linking climate change modelling to impacts studies: recent advances in downscaling techniques for hydrological modelling, Int. J. Climatol., 27(12), 1547–1578, doi:10.1002/joc.1556. Guinard, K., A. Mailhot, and D. Caya (2015), Projected changes in characteristics of precipitation spatial structures over North America, Int. J. Climatol., 35(4), 596–612, doi:10.1002/joc.4006. Hardwick Jones, R., S. Westra, and A. Sharma (2010), Observed relationships between extreme sub-daily precipitation, surface temperature, and relative humidity, Geophys. Res. Lett., 37(22), L22805, doi:10.1029/2010GL045081. Ivancic, T. J., and S. B. Shaw (2015), Examining why trends in very heavy precipitation should not be mistaken for trends in very high river discharge, Clim. Change, 133(4), 681–693, doi:10.1007/s10584-015-1476-1. Lochbihler, K., G. Lenderink, and A. P. Siebesma (2017), The spatial extent of rainfall events and its relation to precipitation scaling, Geophys. Res. Lett., 44(16), 8629– 8636, doi:10.1002/2017GL074857. Molnar, P., S. Fatichi, L. Gaál, J. Szolgay, and P. Burlando (2015), Storm type effects on super Clausius–Clapeyron scaling of intense rainstorm properties with air temperature, Hydrol. Earth Syst. Sci., 19(4), 1753–1766, doi:10.5194/hess-19-1753-2015. Wasko, C., A. Sharma, and F. Johnson (2015), Does storm duration modulate the extreme precipitation-temperature scaling relationship?, Geophys. Res. Lett., 42(20), 8783–8790, doi:10.1002/2015GL066274. Wasko, C., A. Sharma, and S. Westra (2016), Reduced spatial extent of extreme storms at higher temperatures, Geophys. Res. Lett., 43(8), 4026–4032. Wasko, C., and A. Sharma (2017), Global assessment of flood and storm extremes with increased temperatures, Sci. Rep., 7(1), 7945, doi:10.1038/s41598-017-08481-1.

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