Author reply to RC1 (nhess-2022-96)

General Comments

The paper consists many different aspects of the case, which have valuable information but also makes it a bit fragmented. I urge the authors to reconsider, if every part is really needed to support their conclusions and to try to shape it to be more coherent. Now it feels like different short analyses listed together.

We thank the reviewer for the valuable comments and suggestions to improve the manuscript. We acknowledge that the previous version of the manuscript needed more coherence and that some parts were too large. This was also pointed out in the second revision.

This has been a consequence of our team effort where different working groups have obtained the different results presented. To overcome this issue, we have restructured and reduced the text, following the reviewers' guidelines. The main changes are summarized as follows : a) Section 2 has been divided into two sections presenting the data and methods separately; b) all methodologies are explained in Sect. 3 alleviating the text in the results section; c) we have put together all information concerning the case description, i.e. meteorology, impacts and index analysis; d) the most relevant outcomes of our work, e.g., the moisture source analysis, attribution experiments, etc. have been highlighted being given their own subsection.

We expect these changes to have improved the readability of the text and its structure making it easier for the potential readers to access the relevant results of our work.

Major Comments

Section 2: I suggest this section to be restructured, even spitted into two

This has been implemented. We divided Section 2 into two new separate sections, "Data" and "Methods"

I would consider putting subsection 2.2 into the results section and mentioning COSMO-REA6 at the reanalysis section.

Also implemented (see new version of the manuscript).

In the 2.3 subsection it is not well explained why the authors used three different models. What is the added value in using so many models, could it not be done via one model? Also, the analysis done on WRF simulation could it not be done on the COSMO-CLM simulation, or the aerosol simulations with either COSMO or WRF?

This question raised by the reviewer needs explaining. For some purposes of the different analyses, as the reviewer points out, the same model could have been used, e.g., ICON could have been used for the aerosol experiments as well as the water vapour analyses in Fig. 5. We are conscious that this affects the papers readability and length that could have been reduced with the use of just one model.

We planned this research to be able to profit from the different expertise in such a large collaboration. The participating research groups had implemented their analyses and methods on numerical models best known to them or used in their respective institutions. Therefore, different models were used for different experiments. This aspect is intrinsic to our collaboration and the approach of our paper which is somehow unavoidable if we are to combine so many different methods.

On another note, we believe our results to be model independent and we cross-validated our model simulations before starting our analyses. All models showed the same dynamical situation concerning the synoptic evolution, the location of precipitation, the moisture transport and convective activity. Therefore, we are confident that the use of different models does not hamper our drawing of conclusions with the added value of profiting from different analytical methods.

We believe this aspect is worth mentioning, and the following remark is added in the conclusions

"An example of this, is the use of different numerical models. We decided to use different models to profit from the techniques best known to the different working groups. Moreover, we are confident that our results are model independent since all models showed a similar dynamical evolution of the event. "

I also suggest to separate better the models description from the actual experiment set ups.

This has been implemented. We have split the model description from the experiments' description.

Section 3: I would suggest to move the methodology description in subsection 3.2 into the Method section and subsection 3.3 into 3.1.

Implemented. Besides, Sect. 3.4 "Impacts during the 29 June 2017 case" has also moved into Sect 3.1 to provide the description of the case in just one section, now named "Event analysis and climate context.

Subsection 3.4 consist some paragraphs which are describing the presented Table 1 and Figure 8, which feels unnecessary to me, I would suggest the authors to list more conclusions here.

We have rewritten former Subsection 3.4 (now contained in Sect. 4.1 "Case analysis and climate context") to convey the conclusions more than a description of the charts.

Subsection 3.5.2 also consists some methodology description, which should be moved to the Method section (PSI, cell tracking)

Implemented

What is the contribution of the aerosol experiment to this case study, why is it a valuable part of the analysis.

We believe the aerosol experiments to be a relevant part of this analysis since they show another potential impact of anthropogenic activities on heavy precipitation, in addition to global warming. The impact of aerosol on heavy precipitation is of relevance to the 29 June 2017 HPE since it occurred in an urban area which tend to suffer more frequently from pollution.

Specific Comments

Page 2 Line 22 (last sentence) A bit vague.

It has been rephrased as:

"For instance, the link between the unique meteorological conditions of this case and its very large return periods or the extent to which it is attributable to already-observed anthropogenic climate change."

Page 3 Line 3: I would not call Vb cyclones small-scale disturbances

We totally agree with the reviewer. We believe the "small-scale disturbances" part of the sentence was inherited from a previous version of the manuscript. It has been corrected.

Page 9 Line 217: How do you know that the spin-up was sufficient?

In this sentence, "spin-up" was supposed to refer to the spinning-up of small-scale (convective) precipitation features in the convection-permitting (0.025°) model. For example, in a downscaling from 15 km to 3 km (we have 12 km to 2.8 km) over the central United States, Wang and Skamarock (2016; Fig. 2b) found a value of about 6 - 12 h for the spin-up of small-scale convective precipitation features. In our convection-permitting simulations, the analysis period begins 8 hours after the model is initialised from its 0.11° parent model.

Figure 1 illustrates the spinning-up of small-scale precipitation features in our 0.025° simulations. Here we see the temporal evolution of the spatial standard deviation of (i) column-integrated cloud graupel, (ii) column-integrated cloud ice, and (iii) column-integrated cloud water. Note that these variables (on model levels) were all included in the initial conditions provided by the 0.11° model. A high standard deviation would represent high small-scale variability of these variables. As can be seen, there is a rapid increase in small-scale variability after initialization, with the period of rapid growth completed before the start of the analysis period. For this reason, we believe that the spin-up prior to the analysis period is sufficient.

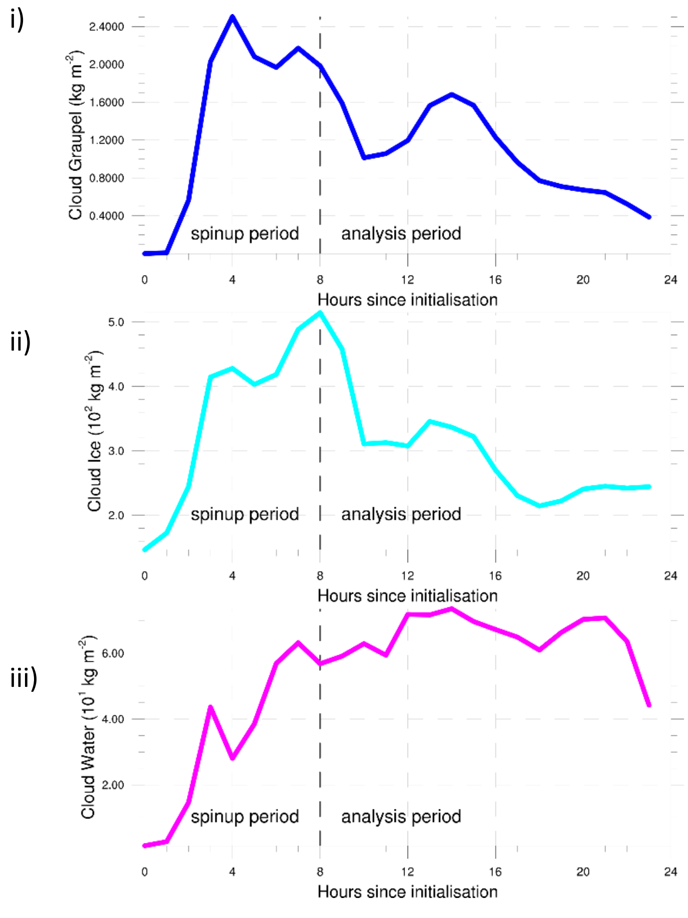

Figure 1. Temporal evolution from initialization for the spatial standard deviation of (i) column-integrated cloud graupel, (ii) column-integrated cloud ice, and (iii) column-integrated cloud water. The curves represent the 0.025° convection-permitting model, which was initialized from its 0.11° parent model at 2300 UTC on 28th June 2017. The analysis period -- marked with a vertical black line -- starts at 0700 UTC on 29th June 2017, eight hours later. The spatial standard deviation is computed over the analysis region shown in Figure S4.

Page 12 Line 293: I would suggest to use "29 June 2017 HPE" instead just "29 June 2017" (not just here, but in the whole paper)

We have accepted the suggested terminology for the whole paper, where applicable.

Page 12 Line 300: originated in the night from 28 to 29, would be clearer.

Suggestion accepted.

Page 22 Line 483: (Tab. 2, number 6 in Fig. 11)

Suggestion accepted.

Page 24 Line 518: "which exceeds the increase in precipitable water we find", in where?

The sentence has been rephrased:

"which exceeds the 7.5 % increase in precipitable water (time average; not shown) which we find over our eastern-Germany analysis region (Fig. S4)."

Page 26 Line 551: Which experiments fits better the observations? What are the conclusion related to this event?

The probability distribution of CLN shows a better agreement with observations for intensities below 80 mm/d as is it can show larger probabilities of precipitation below that value. Both intensities over 80 mm/d both CLN and POL show a similar behaviour. Regarding the spatial distributions shown in Fig. 13 both CLN and POL showed a similar performance and agreement to observations.

These results indicate the a more polluted scenario over an urban area (POL) is likely to shift the precipitation's PDF towards more extreme values, i.e., larger precipitation intensities and lower probability of moderate rates for this event.

Page 26 Line 567: "between Poland and northern Italy" strange wording

Has been rephrased to: *"Lagrangian backward trajectories determined that the continental region between southern Poland and northern Italy was the major moisture source feeding the systems, with uptakes up to 4.6 mm"*

Page 26 Line 568: What does "this area" defines here? Not clear to me.

The region in central Europe between Poland and the Alps/Adriatic Sea. The sentence has been changed to: "Lightning activity was especially active over the same region area between 05 UTC and 19 UTC (29 June)."

Page 28 Line 615: Is this a plane for further work?

Yes, these are possible ideas for further investigation.

Figure 7 Why are there so little lightning from the area of Berlin, where the precipitation has its peak?

This event did not show a strong correlation of heavy rainfall and the location of lightning. As stated by Kuhlman et al., (2009), it has been previously observed that lightning activity can have a significant spatio-temporal variation to its accompanying perils. In this sense, a high spatio-temporal lightning density does not necessarily mean a large amount of precipitation at the same spot. Although statistical correlation between cloud to ground lightning and precipitation is generally expected (e.g. Katsanos et al., 2007, Matsangouras et al., 2016).

In our case, the event is characterized by embedded convection on the upstream side and it is possible that precipitation occurred after lightning activity (after charge separation in the cloud). This can explain the lack of co-location of both perils.

References

Kuhlman, K. M., MacGorman, D. R., Biggerstaff, M. I., & Krehbiel, P. R. (2009). Lightning initiation in the anvils of two supercell storms. Geophysical Research Letters, 36(7). doi: 10.1029/2008GL036650

Wong, M., & Skamarock, W. C. (2016). Spectral characteristics of convective-scale precipitation observations and forecasts. Monthly Weather Review, 144(11), 4183-4196.