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Interactive comment on "The role of large-scale dynamics in an exceptional sequence of severe thunderstorms in Europe May/June 2018" *by* Susanna Mohr et al.

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#1 Review for WCD-2020-1 (RC1 from 11 Feb 2020)

This study describes an exceptional period of thunderstorm activity in western Europe during summer 2018 and explores the associated synoptic-scale conditions; in particular, the role of blocking and associated upstream cut-off lows. The event is also placed in a climatological context using long-term records from surface stations, upper-air soundings, and reanalysis. This is an interesting, thorough and well-written piece of work, which I have no hesitation in recommending for publication, subject to a few minor revisions as detailed below. I would like to thank the authors for their efforts,

which made for an easy and enjoyable review.

AC: We thank the reviewer for the time taken to review our manuscript and for his useful comments. We are pleased that the reviewer finds the manuscript an interesting, thorough and well-written piece of work.

Specific Comments: 1. My most significant comment relates to your conclusion regarding the slow movement of convective systems during the event. You provide clear evidence (in Fig. 9) that storm motion was, on average, much lower than is typically observed in this region and at this time of year and suggest that this was key to the extreme rainfall totals. However, storm motion is not the only relevant factor for heavy precipitation. As discussed by Doswell et al. (1996), accumulated rainfall depends on two things: average rain rate and total rainfall duration. Slow cell motion contributes to long rainfall durations, but one must also consider system size (in the direction or storm motion) and the existence of back-building convection, both of which may lead to echo training. Rainfall rates must also be considered. Given that you have hourly gauge data and radar observations, it should be possible to assess all of these things. While this might not be practical for the whole study period, you should consider doing so for the most extreme events noted in Table 1. One option would be to add an extra column to the table, providing a brief description of the storm(s) that caused the rainfall totals (including their estimated motion). At the very least, it would be good to demonstrate that the general characteristic of slow storm motion applies to some of the individual extreme events.

AC: The reviewer is right; the slow propagation speed is only one factor for locally high rainfall; we will add a comment to the conclusions. We will also add an additional column in Table 1 with storm motion and size / duration of the convective systems as suggested. However, a detailed analysis of the reasons for heavy rainfall would go beyond the scope of this paper.

2. You state in your abstract that low vertical wind shear "prevented thunderstorms

from developing into severe organized systems". However, reports of hail up to 5 cm in diameter (L234; L239) suggest that this is not entirely true. Clearly, wind shear in certain areas and on certain days was sufficient for the development of organised convection (probably supercells). As such I think this statement needs revising.

AC: That is partly correct; the correct formulation would have been that "low vertical wind shear prevented "very often" thunderstorms from developing into severe organized systems". We will modify this linguistically. Furthermore, we will look again at the cases with 5 cm hail (time/location, associated V500/wind shear) and address this part/exceptions separately. Note, even if there are many days with hail reports (Fig. 2), this does not necessarily mean that high shear was necessary, since hail – but usually smaller hail – is also observed with low shear situations (see Kunz et al., 2020). Note 86 % of the hail events during the long investigation period are <= 3 cm.

3. Please provide some citations for reports of storm impacts (in the opening paragraph of the introduction and section 3.1). These could simply be links to online news or social media reports.

AC: Here, we will put together a few examples from the media (however, mostly in German). (First examples are WetterOnline: https://www.wetteronline.de/extremwetter/unwetterserie-ende-mai-ganze-ortschaften-verwuestet-2018-05-31-us & https://www.wetteronline.de/extremwetter/unwetter-treffen-suedwesten-regenfluten-spuelen-autos-weg-2018-06-01-ju; MDR Sachsen: https://www.mdr.de/sachsen/chemnitz/vogtland/unwetter-sturm-im-vogtland-100.html)

4. The second paragraph of the introduction doesn't really fit and has only limited direct relevance to your study. As such I would suggest removing it (although parts of it could potentially be incorporated elsewhere in the introduction).

AC: In the course of the revision of the introduction (see also comments of Reviewer 2) we will take these into account (delete or include elsewhere).

5. I know that 1981–2010 is a standard 30-year climatology period, but for this study you should consider using the full ERA-Interim record (1979–2017) in order to provide a more complete historical context for the 2018 event. I believe the sounding data will go back this far as well.

AC: Our intention was to use a consistent period of 30 year, which is homogenous in the different analyses in the paper, to calculate the climatological mean, as is common in several studies. When calculating the return periods (Fig. 13/15), where this aspect is important, we have already taken into account longer time series (more or less what was available). We will test this aspect regarding Fig. 14 & Fig. 16 (for 1979/1981–2017) and clarify whether this results in significant differences.

6. On Line 140 you claim that surface-based lifted index (SLI) offers "the best representation of convective environmental conditions in central Europe". I would expect CAPE to provide a more robust measure of surface-based instability, given that it considers the full column rather than just a single level (see discussion in Doswell and Schultz 2006). Certainly it has been shown to usefully discriminate between severe and non-severe convective environments in various parts of the world, including Europe (Pucik et al. 2015; Taszarek et al. 2017). It is also available as a diagnostic from ERA-Interim, so could easily be analysed spatially as well as from the point soundings. I appreciate that repeating all of your instability analysis using CAPE would be time consuming and is likely to show comparable results, so I will not request this. However, you should provide some further justification for why you chose to use SLI over CAPE.

AC: For Europe, there are many studies showing that SLI can be used as well as CAPE (e.g., Huntrieser et al., 1997; Westermayer et al., 2017; Rädler et al., 2018; Sanchez et al., 2009). Some studies also showed that the skill to predict thunderstorms and/or their sub-peril can better as using CAPE (e.g., Kunz, 2007; Mohr and Kunz, 2013; Haklander and van Delden, 2003; Manzato, 2003;). In addition, CAPE has the disadvantage that its distribution function is skewed and that the CAPE values can be zero (or small) despite (high) instability in the atmosphere. Our experience (in our last studies) has shown that SLI is more robust in our different analyses, so we prefer to use it. Furthermore, we assume that the key messages in this paper do not change significantly when analyses are performed with CAPE. But we will test this sporadically. In addition, we will add further justification in text as desired.

7. You use a very high reflectivity threshold (55 dBZ) for identifying and tracking convective storms. Such a value is more characteristic of hail than intense rainfall. As such I wonder if the majority of storms went undetected, leading to an unrepresentative velocity estimate. One simple way to check this would be to see if the storms that produce some of the extreme rain accumulations listed in Table 1 were detected. At the very least this should be noted as a limitation of your radar-based analysis.

AC: That's right, a threshold of 55 dBZ prevents weaker cells from entering the sample. However, as we are focusing here on heavy rainfall, this threshold is appropriate; we will explain this in the text. Note that during the study period from 22 May to 12 June the tracking algorithm identified 480 individual storm tracks in Germany, whereas only 84 reports (several from the same storm) are archived in the ESWD. We assume that the largest part of the storm tracks was not associated with hail. However, we will follow the suggestion and we will check the samples listed in Table 1. Furthermore, we will add some comments on this in the next version.

8. It would be good to include a figure showing the different weather regimes discussed in section 2.3 (or, at least, the ZO, EuBL, and AR regimes that dominated during the study period). Perhaps this can be found elsewhere. If so please refer to the specific figure(s) in the relevant paper(s).

AC: The illustration of the Atlantic-European weather regimes can be found in Grams et al., 2017 – however in the Supplementary information (Supplementary Figure 1) and only for the winter season. We will provide the typical patterns (for the summer season) as supplementary material.

9. In discussing the persistence analysis in section 2.5, and the associated results

in section 5.2, I found the reference to "cluster length" confusing, in part because K-Means clustering is used in the analysis of weather regimes. I would change this to just "event duration" or "event persistence".

AC: I can understand your point. However, we have already introduced the wording in PIP16 and I would like to keep it for reasons of consistency. However, you suggestion with "event duration" or "event persistence" would be a good alternative.

10. Could you provide a few more details on the origin of the "basic" and "strict" criteria for SLI and mid-tropospheric wind speed, so that the reader doesn't have to go to PIP16 for this information? Also, I'm not a fan of the notation TH BC and TH SC for these and would argue that they can be eliminated (you can just refer to the basic/strict criterion).

AC: TH BC and TH SC are introduced in PIP16 – this is the reason why we used this notation here. We will add a comment to the definition and check if we can only use the formulation "basic/strict criterion" in the revised script.

11. I recommend using V500 (rather than v_500hPa) to indicate the 500hPa wind speed.

AC: We will implement this suggestion.

12. I suggest using "total" or "accumulation" when referring to precipitation amounts rather than "sum" (e.g. in Table 1 and on L260–261).

AC: We will implement this suggestion. We will also unify the wording on this.

13. I'm confused as to why you focus the opening paragraph of section 3.3.1 and the first three panels of Fig. 4 on the two weeks before your main study period. In my view it would make more sense for Fig. 4 to show more regular snapshots from the study period, so that the reader can more easily see the temporal evolution described in the subsequent two paragraphs. In particular, it would be good to include one snapshot that shows the second cut-off low (C2) and one close to the end of the study period.

AC: We think it is important to highlight the synoptic situation also prior to the event in order to emphasise that the severe convection during the study period was embedded in a longer lasting unusual large-scale flow situation. The synoptic conditions prior to the event featured blocking, the formation of cut-off lows, and facilitated the advection of warm-moist air into central Europe, which was a key ingredient for the high-impact event. Therefore, we decided to keep the synoptic discussion of May/June along with the characterisation in terms of unusual geopotential height anomalies and IWV (Fig. 5) and the evolution of weather regimes (Fig. 6). As C2 did not strongly affect central Europe we compromised not to show it (now we state in line 296 "(C2, not shown)". Instead we provide a detailed synoptic discussion of the impact of C3 in section 4.

14. In section 5.1, you note that several of the rainfall return period maxima in Fig. 13 "have an almost circular shape with the highest value located in the center" and suggest that this characteristic "reflects the very slow propagation of the thunderstorms". However, could it instead be an artefact of insufficient gauge density? If only one gauge recorded the event, this information would be spread laterally by the gridding procedure, giving the impression of a small circular shape. It might be worth overlaying the gauge locations on this plot to check how many gauges are associated with each maximum.

AC: REGNIE gridded data are based on approx. 2,000 stations (more or less all ground-based observation networks of DWD). Comparing the distribution (cf. Fig. 2b in Rauthe et al., 2013, MZ) with our estimated RPs shows that the majority of all events are captured by several stations and not by only one. However, we will follow the suggestion and will plot a Figure with including all climate station to test your hypothesis and we will include a comment on that (or probably delete the relation to the slow propagation).

15. The description of Fig. 14 at the start of section 5.2 is rather confusing and should be revised. In particular, I found it hard to understand how the distributions for the climatological period were derived.

AC: We will rewrite this description to make this aspect more precise and to make it more understandable and less confusing.

16. Several of the figures could be improved in a few ways. Specifically, I recommend the following changes:

- Fig. 3: The top and bottom rows could arguably be combined. In this case, rather than colouring the symbols on the map by rainfall amount you could just make them blue for > 35 mm/h and red for > 60 mm/3h; then use these same colours for the bar plot (with red bars overlaid on blue bars).

AC: We will combine Figs. 3a and 3c in one histogram (good suggestion), so that the number is also easier to compare. However, we would like to keep Fig. 3b and 3d separate, since this way a spatial representation of the maximum precipitation per station (especially for the 3h panel) is maintained.

- Fig. 7: I don't think it's necessary to state the two thresholds within the plots; this information can be provided in the caption (with reference to the dashed lines).

AC: Personally, I'm a friend of putting a lot of information in figures (if they don't irritate too much), so that – if the figure is used elsewhere – all information is included. However, we can also remove it here – as requested – and provide the info only in the caption of the figure.

- Fig. 12: I would get rid of the hatching showing the objectively identified cut-offs and use a darker contour for the pressure vertical velocity.

AC: We will revise the figure. Definitely we will change the current green line (omega) to a darker and thicker contour. As the second reviewer asks for the buffer zone to be marked in the figure, we will check different variations (with/without hatching PV; with/without buffer zone), which is best suited to illustrate the relevant results.

- Fig. 14: It would be helpful to use different colours for the box-and-whisker plots corresponding to the study period and the 1981–2010 climatology. Use the same colour convention for Fig. 9.

AC: We will implement this suggestion.

- Fig. 15: In the legend, rather than putting "(2018: N days incl. M skip days)" for each station I would just put "(N/M)" and then explain what these numbers indicate in the caption. So, for example, for Essen you would put "(17/3)" instead of "(2018: 17 days incl. 3 skip days)".

AC: Good suggestion; it reduce double information/text in the figure. We will implement this suggestion.

- Fig. 16: Rather than plotting the percentage difference from the climatological frequency (which is confusing because it is a percentage of a percentage), I recommend expressing this difference in terms of the standard deviation of the climatological frequency. This will highlight whether the 2018 frequencies were exceptional in the context of typical year-to-year variability.

AC: We concur, that a percentage of a percentage would be confusing. However, in Fig. 16 we show in shading the ((absolute frequency in May/June 2018)-climatological frequency May/June 1981-2010)). Thus, over Northern Spain absolute frequencies exceed 50 %, which would be relatively speaking a change of more than 1000 % compared to climatology. We can clarify this in the figure caption. Additionally, we will check the suggestion with the standard deviation of the cut-off low frequency and possibly implement a second panel to Fig. 16.

Technical Corrections:

AC: We will consider and will implement all following (small) technical suggestion and questions.

1. There are a few issues with tenses in the text. For example, the opening sentence of the introduction is written in the present perfect tense (use of "has been"), but should be in the past tense ("was"). The same goes for L261. The last sentence of the opening

paragraph of section 3.3.3 is written in the simple present tense but should be in the past tense.

AC: The manuscript was checked by an editor service; but we will check the tenses in the text again. Thanks for the careful reading.

2. L23: Parentheses are (are not) for references and clarification (saving space) (Robock 2010). Please modify this sentence accordingly.

AC: We will change this as proposed.

3. L36: "...serve to precondition the thermodynamic environment."

AC: We will change this.

17. L39–42: To which event are you referring here: 2018 or 2016? I suggest rewording this paragraph to make this clear. Similarly, you should state explicitly the event you are referring to on L71.

AC: 2016. The whole paragraph is focused on the 2016 event. In the course of the revision of the introduction (see comments of Reviewer 2) we will take these into account and formulate them more clearly.

18. Line 60: Lifting will only lead to the release of CAPE (i.e. convective initiation) if it is sufficient for parcels of air to reach their level of free convection; however, it may still act to destabilise the column (increase CAPE) and erode lids (reduce CIN).

AC: We will rewrite this.

19. Line 90–91: Suggest revising the end of this sentence as follows: "...based on reports from storm chasers, eyewitnesses, voluntary observers, meteorological services, and news media."

AC: We will change this as proposed.

20. L97–98: "... the Météo-France (1223/1935 stations with hourly/daily data)..."

AC: We will change this as proposed.

21. Line 100: These are the national meteorological services of all the countries in your study so I don't think you need this last part of the sentence.

AC: We will delete the last part.

22. Line 132: "wind speed and direction"

AC: We will change this.

23. L146: What is the altitude of the lowest level?

AC: 1 km above ground is the lowest level and 12 km the highest.

24. L199: Change "fewer" to "less".

AC: We will change this.

25. L239–240: Suggest revising this sentence as follows: "Many of the record-breaking 1h and 3h rain totals occurred within this period (see Sect. 3.2)."

AC: We will implement this suggestion.

26. L255–256: "...the latter on the day with the second most ESWD severe weather reports (cf. Sect. 3.1)." Rather than referring to the previous section here please specify the actual date.

AC: We will implement this (31 May).

27. L257–258: You deal with the variations in large-scale forcing for convection later in the paper so I don't think it is necessary to include this here (unless you want to explicitly refer to the relevant sections).

AC: We will include a reference for the later part in the paper.

28. L271: Change "were" to "where".

AC: Thanks. We will change this.

29. L272–273: This sentence is confusing and should be revised.

AC: We will revise this sentence to avoid confusion.

30. L357: Change "vast parts of" to "much of".

AC: We will change this.

31. L362: Change "exemplarily shown for" to "exemplified by".

AC: Good suggestion. We will use this formulation.

32. Line 380–381: Change "and was already mentioned at the end of Section 3.2" to just "(Section 3.2)".

AC: We will change this.

33. L510: Advected where?

AC: That's right, that's too inaccurate. We mean that mostly on the upstream side of the blocking, we observed the advection of warm, moist and unstable air masse favouring thunderstorm development. We will rewrite this and make it clearer.

34. L526: Get rid of "(global/regional)".

AC: We will delete this.

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