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Attribution of precipitation to cyclones and fronts over Europe in a kilometer-scale regional climate simulation

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Response to the Reviewers' comments:

We thank both reviewers for their constructive and helpful comments that helped to further improve the presentation of our results.

Below are the detailed replies to the individual comments.

Reviewer #1

This is a very timely paper on attribution of precipitation to main rain-bearing systems. It is not the first attempt to associated precipitation to various synoptic features, but this time it is more detailed and done using outputs of a convection-resolving model. I also like that results show annual and seasonal data for all 4 seasons, as there are important seasonal differences. The manuscript features excellent literature review and is well written.

Many thanks for these positive statements!

I have relatively minor comments listed below. I am most concerned about attributing precipitation to high pressure systems. As the authors say, it is most likely associated with convection, so I made a few suggestions on that in the comments. Another suggestion is to add a threshold on the size of frontal areas, as there are many very small frontal features in the examples. Finally, I am interested if similar approaches are applied to ERA5 (or other reanalyses), how the results will be different. The latter might be outside the scope of this paper, so I wish to see such comparison sometime in the future.

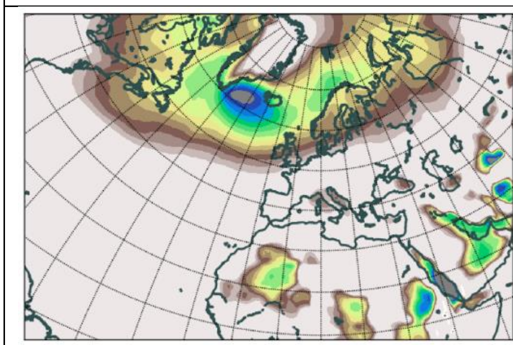
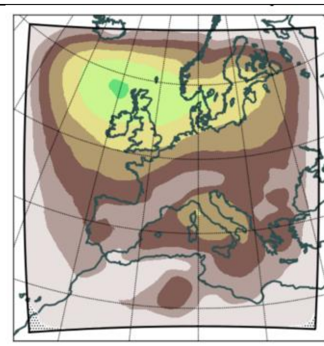
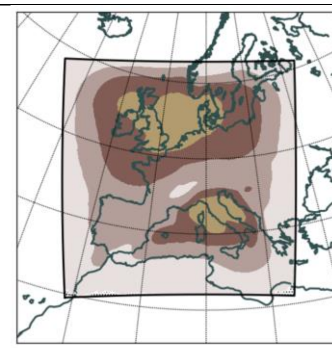
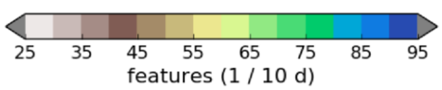
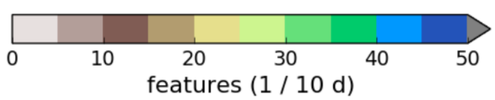
We address all these points below.

Comments:

1. l.158, 185: The 12 km domain is not significantly larger than 2.2 km domain. Did you consider merging with ERA-interim? It might be particularly good for getting cyclones and high-pressure systems right.

While indeed the difference in domain size between the 2.2 km and the 12 km simulation is not huge, it still makes a large difference for the cyclone identification because the influence of the domain boundary on cyclone feature growth is largest in the direct vicinity of the boundary.

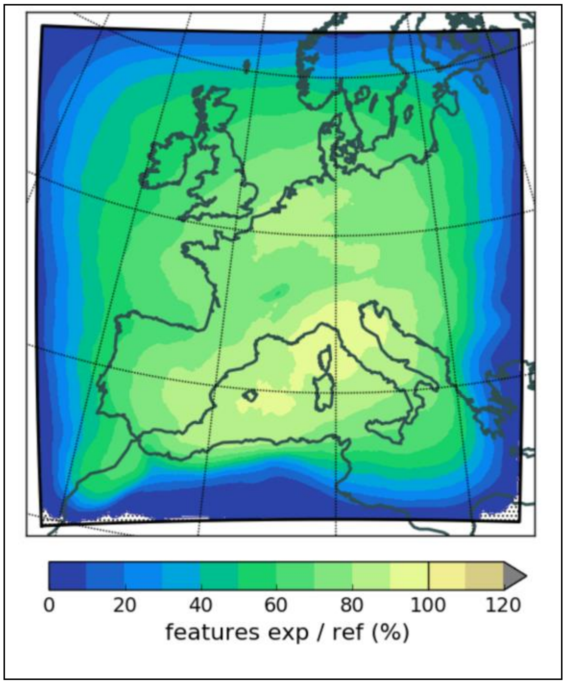
In fact, we have previously investigated the influence of the domain boundary on the cyclone identification (see Rüdisühli (2018), https://doi.org/10.3929/ethz-b-000351234), comparing cyclones identified in ERA-Interim (on a global grid), the 12 km simulation, and the 12 km simulation with a reduced domain size (minus 40 grid points in all directions, corresponding to the domain of the 2.2 km simulation, but without including any 2.2 km data). The absolute cyclone frequencies are shown in the following figure:

ERA-Interim	12 km	12 km – 40 pt
		
		

Note that the absolute frequencies between ERA-Interim and the 12 km simulation are not directly comparable, as we did not account for differences in grid resolution etc. Also note that differences in feature frequency are largely caused by differences in the mean feature size rather than differences in the occurrence frequency of the features. In other words, the 12 km simulation has a similar number of cyclones as ERA-Interim, e.g., near Scotland, however they are smaller (because of the limited domain) and therefore the frequencies are lower.

But most relevant here is the comparison of the two 12 km composites. It is obvious that the reduced domain size has a large impact on the cyclones over the North Atlantic, whereas the frequencies are very similar over the southern half of the domain. The influence of the boundary becomes even more obvious if we look at the ratio of the above cyclone frequency fields from the 12 km simulation:

(12 km – 40 pt) / 12 km



The figure clearly reveals the strong boundary influence, as the cyclones very rapidly decrease in size (and thus composite frequency) very close to the boundaries.

These comparisons give an idea of the benefit of increasing the domain size, in particular for the northern part of the model domain where cyclones typically propagate rapidly across the domain. Properly tuned, increasing the domain size of the 12 km domain using ERA-Interim – as suggested by the reviewer – may have a similar effect as increasing the 12 km domain from reduced to full size, with a large effect in a narrow boundary zone and a smaller effect in the interior of the domain.

In conclusion: Yes, there would likely be a benefit on the identified cyclones by increasing the domain further with ERA-Interim, but already the relatively small increase from the 2.2 km to the 12 km domain has a substantial positive effect on the cyclones in the analysis domain, removing the worst of the boundary effect (as shown above). This is why we decided that the additional effort of incorporating ERA-Interim data was not worth it; while certainly not perfect, for our purposes (of distinguishing the zone close to the cyclone center from the rest of a cyclonic system) the identified cyclones are good enough.

For our high-pressure areas, on the other hand, extending the domain further would not make any difference, as they are based on the local values of the geopotential field and its gradient (see also our answer to comment 5).

2. l.193-194: This not clear. Please explain better what you mean by allowing 20% of contours to cross the boundary before 'halting further feature growth'.

We agree and have changed the text to express this more clearly.

Old:
[...] We opt for a compromise by allowing one in five contours of a feature (20 %) to cross the boundary before halting further feature growth.

New:
[...] We opt for a compromise by allowing up to 20 % of the contours of a feature to be boundary crossing. For example, if a pressure minimum is enclosed by 16 closed contours, then at most four additional boundary-crossing contours may be added to reach the 20 % threshold at four out of twenty contours.

3. l.200: In the abstract it is said that local thermal fronts are removed, here you say that fronts are categorised at synoptic and local. Are local fronts removed then?

Yes, the local fronts are removed for this analysis. We have added a sentence to emphasize this: “The local fronts are then removed and only the synoptic fronts are used in this study.”

4. l.215: What is the threshold value on theta-e gradient based on and why all values in Table 1 are whole numbers?

This is a good question, pointing to the challenge of reasonably choosing the theta-e gradient thresholds. To the best of our knowledge, there is no fully objective procedure to determine these thresholds. We found the monthly threshold values subjectively by examining multiple years of data. Specifically, we have evaluated the fronts based on a range of possible thresholds, deduced monthly “best estimates” based on how well the front features matched the meteorological fields, and based on these determined the thresholds listed in Table 2. Given their subjective and approximate nature, and their magnitude, there was no reason not to settle on whole numbers. We mention this challenge of choosing appropriate thresholds also in the third bullet point of our conclusions.

5. l.252: It is not clear to me how high-pressure systems are defined. One may think that you mean anticyclones (i.e. an area similar to cyclones with high pressure in the middle circled by a closed contour), but ‘high pressure’ systems in fig. 3 look confusing. In fig 3 (summer) the green area looks like the subtropical ridge (there are big and small white areas within green stippling - what do they represent?), in fig. 5 (winter) I would suspect an anticyclone defined using the MSLP field. These systems need to be better described, both their identification procedure and physical meaning. There is a recent paper by Poujol et al. (2020) on a separation between convective and stratiform precipitation. It might be interesting to check if the precipitation within high pressure systems can be classified as convective using their approach. Discussion around lines 400 and 463 may benefit if you mention possible convective nature of high-pressure precipitation, that is prevalent in summer. Given the frequency of high-pressure ‘components’ (fig. S4), which cover 50% of your domain 50% of time in summer, these systems need to be explored in more detail.

The identification procedure of the high-pressure areas is described in Sec. 2.4: They are areas with high pressure (Φ at 850 hPa above threshold derived from monthly values in Table 2, which have been found by a similar subjective evaluation as the frontal gradient thresholds in Table 1) and a flat pressure distribution ($\nabla\Phi < 0.02$ m s^{-2}). As opposed to fronts and cyclones, the high-pressure areas are simple masks, without any sophisticated feature identification or tracking.

The white areas within the green stippling in Fig. 3 are therefore regions where either the geopotential is locally too low and/or its gradient is locally too strong.

We agree that the motivation, physical meaning, and name of the high-pressure areas are not explained in sufficient detail. We have revised and extended Sec. 2.4 to more clearly convey these points.

Old:

Precipitation not only occurs near cyclones and fronts, but also in areas of weak synoptic forcing typically characterized by relatively high pressure or by a flat pressure distribution, for example with diurnal summer convection over the continent. We explicitly identify such high-pressure areas based on geopotential Φ and its gradient $\nabla\Phi$ at 850 hPa. Seasonal feature frequency composites are provided in the supplementary material (Fig. S1).

The Φ field is first smoothed with a Gaussian filter. A mask is derived by applying a minimum threshold that varies over the year to account for the seasonal cycle in Φ. Analogous to the seasonally varying frontal threshold, the Φ threshold values are defined in the middle of each month (Table 2) and linearly interpolated to each hour in-between. Then, $\nabla\Phi$ is computed, and the resulting field is smoothed again. A second mask is derived by applying a constant maximum threshold of 0.02ms−2 to $\nabla\Phi$. The high-pressure area corresponds to the overlap area of the Φ and $\nabla\Phi$ masks. All threshold values have been determined subjectively based on thorough manual testing.

New:

Precipitation not only occurs near cyclones and fronts, but also in areas of weak synoptic forcing typically characterized by relatively high pressure and a flat pressure distribution, for example with diurnal summer convection over the continent. When attributing precipitation only to cyclones and fronts, such precipitation would not be captured and become part of the residual. Our original method without high-pressure areas, however, often misclassified diurnal summer convection as front-related (specifically far-frontal, as defined in Sec. 2.5). To prevent this, we explicitly identify such areas characterized by high pressure and a flat pressure distribution – henceforth simply called high-pressure areas *– based on the geopotential Φ and its gradient $\nabla\Phi$ at 850 hPa. Seasonal frequency fields of the identified high-pressure areas are provided in the supplementary material (Fig. S1).*

Computing the high-pressure areas at 850 hPa involves the following steps:

1. *Smooth the Φ field using a Gaussian filter with a standard deviation σ=3. Then compute a Φ mask covering areas with high pressure, based on a minimum threshold, which varies over the year to account for the seasonal cycle in Φ. The threshold at a given time step is derived by linear interpolation from the mid-monthly threshold values listed in Table 2.*
2. *Smooth the Φ field again using a Gaussian filter with a standard deviation σ=20, then compute $\nabla\Phi$, whereby the gradient at each grid point is computed across multiple unit grid distances using offsets of ($i\pm10$, $j\pm10$). Then compute a $\nabla\Phi$ mask covering areas with a weak pressure gradient, based on a constant maximum threshold of 0.02 m s^{-2}.*
3. *The high-pressure area corresponds to the overlap area of the Φ and $\nabla\Phi$ masks.*

All threshold values have been determined subjectively based on extensive manual evaluation of multiple years of data.

We thank the reviewer for pointing us to the paper by Poujol et al. (2020), as their classification approach looks very promising. However, we do not think that it would add much to the characterization of the high-pressure area precipitation, because it is fairly obvious to us (from extensive visual analysis of precipitation fields during method development) that most of this precipitation is convective. But their separation of precipitation types could be a great extension of our attribution method, which could be addressed future studies. We have added such a remark to the end of the “Conclusions”.

New:

Finally, methods that separate precipitation types like convective and stratiform (e.g., Poujol et al., 2020) could be combined with our feature-based attribution, which would enable a more in-depth characterization of the different front-cyclone-relative precipitation components

References:

Poujol, B., Sobolowski, S., Mooney, P., and Berthou, S.: A physically based precipitation separation algorithm for convection-permitting models over complex topography, Q.J.R. Meteorol. Soc., 146, 748–761, https://doi.org/10.1002/qj.3706, 2020.

6. Fig.3: The example is very good, but I have numerous suggestions on plotting:

6.1 [Fig. 3]: The red outline stands for local fronts, while red filling (in slightly different shade) - for warm fronts. It would be good to use different colours.

6.2 [Fig. 3]: A bold black contour also circles the cyclone area, is that right? I think it is not mentioned in the caption.

6.3 [Fig. 3]: Blue filling of cold fronts is very similar to precipitation 0.2-1 mm/h, please use different colours.

6.4 [Fig. 3]: I am not sure I can see red filling well for the warm front (it works better in fig. 5). Is warm front in fig. 3 a 'local' front, not synoptic? If this is the case then the separation between local and synoptic fronts is probably not working very well.

6.5: It would be good to remind the reader that frontal systems within the high-pressure system do not count as rain-bearing (i.e. this precipitation is attributed to the high-pressure system only).

We agree that the plotting can still be improved and thank the reviewer for the specific suggestions. We will review the various color choices and improve the plots for the revised version of the paper, with a special focus on trying to avoid color clashes like the one between the blues of the precipitation and the cold fronts pointed out by the reviewer. Likewise, we will revise the figure caption.

7. Fig. 3 Makes me think that it would be good to have a threshold on the size of the frontal area to remove very small features.

We definitely tried that. The problem with an explicit feature size threshold is that it harms as much as it helps. While it would surely remove some spurious features that we'd prefer to get rid of, it would also remove many that we do want to retain, for instance fronts associated with small cyclones over the Mediterranean, or fragments of large fronts that are not connected to the main feature. (Fragmentation is fairly common for all but the largest fronts, given most of our domain is over land.)

We tried many different approaches and combinations of criteria, and finally settled on the two criteria described in Sec. 2.3: "typical feature size" and "stationarity". The former does indeed consider feature size, but for whole tracks rather than individual time steps, which makes it more robust in case of fragmentation. The stationarity criterion allows for, e.g., the mobile fronts associated with small Mediterranean cyclones to be classified as non-local and thus make it into the analysis. Fig. 3 actually illustrates that these criteria work fairly well, as most small-scale features are classified as local (red outlines) while the larger, precipitating fronts are classified as synoptic.

8. l.412, Fig 8 vs Fig 9, high pressure precipitation: In figure 8 high-pressure precipitation is over the land only (with an exception for the Bay of Biscay), but for relative precipitation there is a large proportion of convective precipitation over the Mediterranean Sea. Can you explain this?

Yes, we can. Fig. 8 shows absolute precipitation amounts of the components, starting at 0.25 mm/d, while Fig. 9 shows the relative contributions of the components to the absolute precipitation amount. In summer, there is hardly any precipitation at all over the Mediterranean Sea: less than 0.75 mm/d overall, and none of the four shown components exceeds 0.25 mm/d. However, as Fig. 9 shows, there is some precipitation, and a substantial fraction of it is associated with high-pressure areas (i.e., presumably convective). In fall, on the other hand, there is substantially more precipitation over the Mediterranean Sea than in summer with about 1.5 mm/d on average, and the high-pressure contributions locally exceed 0.25 mm/d in several places. The modest about 10% high-pressure contributions in Fig. 9 are thus consistent.

9. Fig. 9: I find it odd that cyclone and far-frontal precipitation are combined in this plot. I am not sure if this information is valuable. Is it possible to separate them?

Yes, it is possible to separate them. We opted to combine them to reduce the number of plots; the focus of the figure is mainly on the other contributions (frontal, high-pressure, residual), so we combined cyclonic and far-frontal into “other front/cyclone-related”.

Upon reflection, we do agree that this is probably not the most meaningful way to combine these groups. We will address this in the revised version.

10. l.438: Are you able to explain high amount of residual heavy precipitation comparable to, e.g., cold-frontal heavy rainfall?

The residual is especially large in spring, which is when fronts (especially warm fronts) already occur less frequently than during their peak in winter, but high-pressure areas have not yet reached their peak frequency in summer. Given the larger residual heavy precipitation in spring compared with the other seasons, especially over land, some of this may be due to early convective precipitation events, which are not captured by our high-pressure areas to the degree they are in summer.

Similarly, over Sweden, the large amounts of heavy residual precipitation in spring coincide with a lower cyclone frequency compared with summer. Possibly, convective precipitation events in spring are triggered by other processes than cyclones, while in summer, many are associated with cyclones when those occur with high frequency.

In summer, the residual is distributed fairly evenly across the domain and roughly comparable to the total frontal contributions, while the cyclonic and high-pressure contributions are substantially larger. This is not surprising given the frequency minimum in both cold and warm fronts in summer.

In fall, residual contributions are relatively large over the Baltic states, where the front and cyclone frequencies are much lower than further west. In addition, this is close to the upper-left corner of the domain, so in addition to natural decay of these systems, boundary effects on the feature identification may also play some role.

Finally, from fall through spring residual precipitation is relatively frequent and heavy along the North African coast. Only few cyclones and fronts occur in this region, and the high-pressure area frequency is also much lower than in summer, so most precipitation is classified as residual. However, since this region is drier than most areas further north, large relative residual contributions still translate to relatively little residual precipitation in absolute terms.

Minor comments:

11. Fig. 1 and possibly other plots: Please add lon/lat values.

We agree that the grid lines should be labeled in Fig. 1. We will redo this figure with grid line labels.

As for the other plots, we are of the opinion that grid line labels do not offer much benefit (the location of the domain is obvious given the European coastlines, the shown grid lines are only major and thus easily deduced from the coastlines, and the domain is the same in all plots), but major downsides (if placed in the plots, it would fill them up even more and make it even harder to deduce details as it already is given their small size, while if placed outside the plots, it would increase the size of the multi-panel figures, potentially necessitating even smaller maps).

12. l38: ", high-pressure systems, extratropical cyclones, fronts, orography ... contribute to precipitation" - I'd avoid starting with high pressure systems as they are not the main rain-bearing systems

We agree and have changed it to "[...] extratropical cyclones, fronts, orography, high-pressure systems, and their interactions [...]".

13. l45, l.78, 80: I think it should read "such resolution", "such attribution"

We agree and have changed it as proposed.

14. l47: I'd rather say "interplay between fronts and steep orography in producing precipitation"

We agree and have changed it as proposed.

15. l53-55: I doubt this sentence is needed

We agree and have removed the sentence.

16. l.98: Re-phrase 'on a continental-scale domain'; perhaps, 'for a continental-size domain' or "on a scale of a continent'

We agree this could be phrased better and have changed it to "at a continental scale".

17. l.114: 'domain covering most of Europe' - I disagree, though it is hard to get the area by eye. Given the size of Eastern Europe (former USSR seems to be excluded from analysis) and Scandinavian counties, my feeling is that the domain covers roughly half of Europe.

We agree that the domain does not cover Eastern Europe, but Western Europe and a large fraction of the Mediterranean. We have therefore changed "the comparatively large domain covering most of Europe" to "the decade-long simulation on a computational domain capable of representing the evolution of these systems over Western Europe, the eastern North Atlantic, and the Mediterranean".

We note that the computational domain covers an area of about 11,000,000 km^2, which is a bit larger than the European land area.

18. l.124: "this attribution" replace with "their contribution"

We partially agree and have changed it to "these contributions".

19. l.133: 'can be found' instead of 'is found'

We agree and have changed it as proposed.

20. l.156: Replace interpolate with extrapolate

Given this transformation step is only performed over the part of the 12 km grid covered by the 2.2 km grid, where we have data, we think that "interpolate" is indeed the right word. However, we concede that the whole explanation of the procedure could be clearer (see also next comment) and have thus rephrased it.

21. l.165: "the features are interpolated back onto the original 2.2 km grid". I do not think this is the right way of describing it. My understanding is that you first create a mask based on 12 km field and then use it on 2.2 km scale.

Indeed, the feature masks are first created on the 12 km grid based on the "hybrid fields" and then used at 2.2 km scale to attribute the precipitation fields from the 2.2 km simulation to the features. Technically, this involves interpolating the feature masks from the 12 km to the 2.2 km grid (where the data in the interior of the domain has originally come from, thus the "back"). We have rephrased the explanation of the whole procedure (see also previous comment) to make it clearer and more precise.

Old:
[...] In order to exploit the advantages of both simulations, the 2.2 km and 12 km data are merged in the following three-step procedure:

1. *Interpolate the 2.2 km fields onto the 12 km grid. This retains the exact position and extent of the cyclones and fronts in the 2.2 km simulation while increasing the signal-to-noise ratio to the level of the 12 km simulation.*
2. *Paste these into the 12 km fields to obtain hybrids comprised of 2.2 km simulation data in the center and 12 km simulation data beyond the boundaries of the inner nest.*
3. *Introduce a blending zone along the boundaries in the inner domain with a smooth transition from the 2.2 km data to the 12 km data. It extends 50 coarse grid points (~60 km) into the inner domain and is based on the logistic function1/(1 + exp−k×x) with k= 0.8.*

This retains the exact position and extent of the cyclones and fronts in the 2.2 km simulation while increasing the signal-to-noise ratio to the level of the 12 km simulation. The resulting hybrid fields reside on the grid of the 12 km simulation and thus benefit from its large domain and relatively low noise level, while being meteorologically consistent with the 2.2 km simulation within the analysis domain in the inner nest. We use them to identify cyclones (Sec. 2.2) and fronts (Sec. 2.3). Before conducting the precipitation attribution analysis (Sec. 2.5), however, the features are interpolated back onto the original 2.2 km grid.

New:
[...] In order to exploit the advantages of both simulations, the 2.2 km and 12 km data are merged in the following procedure:

1. *Interpolate the 2.2 km fields to the part of the 12 km grid covered by the domain of the 2.2 km simulation.*
2. *In the interior of the domain at a distance of at least 50 coarse grid points (~600 km) from the boundary of the 2.2 km domain, use these fields from the 2.2 km simulation.*
3. *Outside the 2.2 km domain, use the fields from the 12 km simulation.*
4. *In-between, blend the fields with f= 0.1/(1 + exp (−0.8 × (10x − 5))), where x increases linearly from 0.0 at the inner boundary of the blending zone to 1.0 at the outer boundary and f increases logistically in the same range, corresponding to the fraction of 12 km data*

The resulting hybrid fields possess the bigger domain and lower noise level of the 12 km simulation, which allows for a more robust feature identification over the analysis domain, especially close to the boundaries such as over the North Atlantic. At the same time, the hybrid fields are meteorologically consistent with the 2.2 km simulation. We use the hybrid 12 km fields to identify cyclones (Sec. 2.2), fronts (Sec. 2.3), and high-pressure areas (Sec. 2.4), and then use the resulting feature masks at 2.2 km for the precipitation attribution analysis (Sec. 2.5).

22. l.392: Change to 'selected'

We agree and have changed it as proposed.

23. l.558 and throughout the manuscript: I would avoid saying that summer precipitation is 'associated' with high-pressure systems, though technically this is what the paper shows. As you say, it is most likely associated with convection. I'd rather say that summer precipitation is often detected within high pressure systems.

We agree and will adapt the text accordingly.

24. Fig. S2: Why do you need 'track frequencies', would simply 'frequencies' not be enough?

No, "frequencies" would be ambiguous. As explained in the respective caption, the "track frequencies" are computed by compositing the track masks (which comprise all grid points that have encountered at least one feature belonging to the track at least once), as opposed to the complementary "feature frequencies" (e.g., Fig. S1), for which all individual feature masks are composited.

25. Fig. S4: Components of what?

"Front-cyclone-relative" components, as in Fig. S3. The domain is separated at each time step into seven masks corresponding to these components (before these masks are applied to the precipitation field). Figs. S3 and S4 show frequency composites of these masks. We have adapted the Figure captions to express this more clarity.

Old:
Figure S3. Frequencies of front-cyclone-relative components during (0) the whole year, (1) winter (DJF), (2) spring (MAM), (3) summer (JJA), and (4) fall (SON) 2000–2008. Shown are the (a) cold-frontal, (b) warm-frontal, (c) collocated, and (d) far-frontal components.
Figure S4. Like Fig. S3, but showing the frequencies of the (e) cyclonic, (f) high-pressure, and (g) residual components.

New:
Figure S3. Frequencies of front-cyclone-relative component masks during (0) the whole year, (1) winter (DJF), (2) spring (MAM), (3) summer (JJA), and (4) fall (SON) 2000–2008. The masks are obtained at each time step by separating the domain into seven components as described in Sec. 2.5. Shown are the (a) cold-frontal, (b) warm-frontal, (c) collocated, and (d) far-frontal components.
Figure S4. Frequencies of front-cyclone-relative component masks as in Fig. S3, but showing the (e) cyclonic, (f) high-pressure, and (g) residual components.

References:

Poujol, B, Sobolowski, S, Mooney, P, Berthou, S. A physically based precipitation separation algorithm for convection-permitting models over complex topography. Q J R Meteorol Soc. 2020; 146: 748–761. https://doi.org/10.1002/qj.3706

Reviewer #2

General comments

The aim of this study is to use high-resolution data to quantify the precipitation associated with different weather systems over Europe. In general, the authors have achieved this aim. The paper is clear, and the analysis well presented. However, the abstract does not reflect the

quantitative aspect of the paper and simply lists the qualitative results, many of which are supported by previously published work in the literature. What is novel about this study is the development of a methodology which can be used to quantify the extent to which, for example, cold fronts produce more heavy precipitation than warm fronts. This kind of quantitative result should be included in the abstract. Furthermore, there is not enough motivation/context for the work, or inclusion of the wider implications. How might the methodology and results impact forecasting, model development, understanding of precipitation? *How might the methodology be used in the future to investigate precipitation in a changing climate?* Finally, while the conclusions contain a nice summary of the methodology and its limitations, no such caveats are applied to the discussion of the climatological results. This study is based on only 9 years of data and there are many studies that have shown that decadal variability in cyclone frequency and location exists. Therefore, these caveats must be included in the discussion since conclusions based on 9-years of data may not represent a longer climatology.

We thank the reviewer for the in-depth assessment of our manuscript and for raising many valid points of criticism. Most of them are addressed in the specific comments below.

As for using the methodology to investigate precipitation in a changing climate, we've discussed this aspect in the last paragraph of the initial submission: "It is, however, an open question whether the attribution to the components will be the same in the future climate. First steps to apply our approach to future climate simulations have been taken; first results have been published (Hentgen et al. 2019) and further publications are underway."

References:

Hentgen, L., N. Ban, N. Kröner, D. Leutwyler, and Schär, C., 2019: Clouds in convection-resolving climate simulations over Europe. J. Geophys. Res. – Atmos., 124, 3849–3870. https://doi.org/10.1029/2018JD030150

Specific comments

1. Lines 70-75. Regarding the interaction with orography, it seems amiss that reference to the seeder-feeder mechanism for generating localised heavy precipitation is missing (e.g. Browning et al. (1973).

We agree and have added the following Sentence: "In the warm sector ahead of the cold front, precipitation from low-level orographic clouds can be strongly enhanced via the seeder-feeder process (Bergeron, 1965) by precipitation from aloft (Browning et al., 1974, 1975)."

References:

Bergeron, T.: On the low-level redistribution of atmospheric water caused by orography, Suppl. Proc. Int. Conf. Cloud Phys., Tokyo, pp 96–100, 1965.

Browning, K. A., Hill, F. F., and Pardoe, C. W.: Structure and mechanism of precipitation and the effect of orography in a wintertime warm sector, Q. J. R. Meteorol. Soc., 100, 309–330, https://doi.org/10.1002/qj.49710042505, 1974.

Browning, K. A., Pardoe, C. W., and Hill, F. F.: The nature of orographic rain at wintertime cold fronts, Q. J. R. Meteorol. Soc., 101, 333–352, https://doi.org/10.1002/qj.49710142815, 1975.

2. Line 117. The authors state that the model is free to evolve precipitation systems that may differ from reality despite being forced at the boundaries by re-analysis data. Have they performed analysis of individual precipitation events? Are convective rather than synoptic scale events more likely to be different from reality? Does this affect the conclusions?

The reviewer is raising a valid concern. As the simulation is driven by imperfect reanalysis data, and uses an imperfect limited-area model, individual precipitation events may significantly deviate from reality. Earlier studies on the topic suggest that for computational domains similar to ours, the quality of RCM simulations is comparable on average to that of an operational 2–3 d operational NWP forecast in terms of the 500 hPa RMS error, both during summer and winter (Lüthi et al., 1996). In terms of daily precipitation, the result will be similar, although the details of high-resolution convective events will be much more strongly affected by the chaotic nature of the underlying dynamics (Hohenegger and Schär, 2007). Nevertheless, as the associated level of error is comparable to or somewhat larger than that of a good reanalysis, we believe that overall the climatological characteristics (i.e., based on decadal statistics) are well captured by the presented simulations.

A detailed case study on the representation of the Kyrill storm in the 12 km and the 2.2 km simulations using the same modeling system is provided in Leutwyler et al. (2016). Analysis of the same storm using the same model has been presented by Ludwig et al. (2015), albeit on a smaller domain. Both studies demonstrate a striking difference in the representation of frontal precipitation between convection-resolving and convection-parametrizing simulations. Both of these studies conclude that the representation at convection-resolving resolution is more physically consistent.

We have added the following sentences to the manuscript (original l.115) to reflect this discussion: "The representation of frontal precipitation in the Kyrill storm was assessed in previous studies (Ludwig et al., 2015; Leutwyler et al., 2016). They concluded that performing simulations at convection-resolving resolution yields a more physically consistent representation of frontal precipitation."

Assessing the representation of deep convection at convection-resolving resolution is a long and ongoing effort (see Hohenegger et al., 2009; Ban et al., 2014; Prein et al., 2015). For the presented simulation, a validation is provided in Leutwyler et al (2017), as indicated on l.133 of the original submission.

Regarding the occurrence and development of the fronts and cyclones themselves, we have not conducted any evaluation against observations, nor systematically investigated differences between the 12 km and the 2.2 km simulations. Based on our experience, however, we conclude that large-scale systems like North Atlantic cyclones are largely driven by the boundary conditions and thus represented well compared with reality. The farther from the boundaries and the smaller the systems are, however, the freer they were to evolve independently of the boundary conditions.

We have encountered such an example by chance. A small cyclone developed in the northern Mediterranean, then quickly moved around Italy and hit the Greek coast. Both the 12 km and the 2.2 km simulation simulated this cyclone; however, it developed one day apart in the two simulations almost by the hour. The conditions set by the boundaries were apparently favorable for the genesis and development of this cyclone, but the model had considerable freedom as to the day on which this would happen. (We have not investigated which of the two simulations was closer to reality.) This illustrates that (i) the model has considerable freedom in the interior of the domain to evolve differently than the driving simulation, but (ii) that the boundary conditions still sufficiently constrain the simulation to evolve in a consistent way.

References:

Ludwig, P., J. G. Pinto, S. A. Hoepp, A. H. Fink, and S. L. Gray, 2015: Secondary Cyclogenesis along an Occluded Front Leading to Damaging Wind Gusts: Windstorm Kyrill, January 2007. Mon. Wea. Rev., 143, 1417–1437, https://doi.org/10.1175/MWR-D-14-00304.1.

Ban, N., Schmidli, J., and Schär, C. (2014), Evaluation of the convection-resolving regional climate modeling approach in decade-long simulations, J. Geophys. Res. Atmos., 119, 7889– 7907, doi:10.1002/2014JD021478.

Prein, A. F., Langhans, W., Fosser, G., Ferrone, A., Ban, N., Goergen, K., Keller, M., Tölle, M., Gutjahr, O., Feser, F., et al. (2015), A review on regional convection-permitting climate modeling: Demonstrations, prospects, and challenges, Rev. Geophys., 53, 323– 361. doi:10.1002/2014RG000475.

Hohenegger, C., P. Brockhaus, C. S. Bretherton, and C. Schär, 2009: The Soil Moisture–Precipitation Feedback in Simulations with Explicit and Parameterized Convection. J. Climate, 22, 5003–5020, https://doi.org/10.1175/2009JCLI2604.1.

Lüthi, D., Cress, A., H.C. Davies, C. Frei and C. Schär, 1996: Interannual Variability and Regional Climate Simulations. Theor. Appl. Climatol., 53, 185-209

Hohenegger, C. and C. Schär, 2007: Atmospheric predictability at synoptic versus cloud-resolving scales. Bulletin American Meteorol. Soc., 88 (11), 1783-1793, http://dx.doi.org/10.1175/BAMS-88-11-1783

3. Line 143. How frequently are the boundaries forced by ERA-Interim data?

Thank you for pointing out that this information is missing. They are updated every 6 h (as available from the ERA data set). We have added this information to the text.

Old:
The coarser COSMO simulation in turn is driven at the boundaries by global ECMWF Interim Reanalysis data available on a 1° grid (Dee et al., 2011).

New:
The coarser COSMO simulation in turn is driven at the boundaries by global ECMWF Interim Reanalysis data available on a 1° grid every 6 h (Dee et al., 2011).

4. Line 145. Is the model orography at different resolution for the 12km and 2.2km resolution simulations? If so, does this affect the results?

The orography differs between the driving ERA-Interim data, the 12 km simulation, and the 2.2 km simulation. While in response we expect significant local differences in precipitation, for instance in the vicinity of the Alps (see Heim et al. 2020 for a more thorough discussion), the larger-scale differences are expected to be small and locally confined, as the simulation is still sufficiently constrained by the lateral boundary conditions.

We have added these remarks to the text.

Regarding the analysis presented in the paper, please note that fields from the 12 km simulation (which are influenced by its orography) are only directly used during front and cyclone identification near and beyond the boundary of the 2.2 km simulation (hybrid fields described in Sec. 2.1; see also comment #21 by reviewer #1). The differences in orography may thus have some influence on the identified fronts and cyclones in places that expose high orography and are located close to the domain boundary. However, given that the 2.2 km fields are first interpolated onto the 12 km grid and in many cases additionally smoothed before the identification step, we do not expect a substantial impact on the identified features. The precipitation analysis in turn only uses data from the 2.2 km simulation and is thus not affected by the difference in orography.

References:

Heim, C., D. Panosetti, L. Schlemmer, D. Leuenberger, and C. Schär, 2020: The Influence of the Resolution of Orography on the Simulation of Orographic Moist Convection. Mon. Wea. Rev., 148, 2391–2410, https://doi.org/10.1175/MWR-D-19-0247.1.

5. Line 158. What do the authors mean by 'Paste' these into the 12km fields?

We have rephrased the explanation of the whole procedure for more clarity in response to comment #21 by reviewer #1.

6. Line 176 and 241. What is the width of the Gaussian filter? Was this an arbitrary choice or was some sensitivity testing performed to gain an optimal choice?

We have used the function "gaussian_filter" from the Python package "scipy.ndimage" (see https://docs.scipy.org/doc/scipy/reference/generated/scipy.ndimage.gaussian_filter.html) with standard deviations parameter "sigma" of:

- 7 for the geopotential field used to identify cyclones (l.176),
- 3 for the geopotential field used for the pressure component of the high-pressure areas (l.241), and
- 20 (on top of the 3) for the same geopotential field before computing its gradient for the pressure gradient component of the high-pressure areas (also l.241).

Note that we have revised the section on high-pressure areas extensively in response to comment #5 of reviewer #1, including a more detailed description of when which fields are smoothed to what degree.

The smoothing parameters were found by extensive manual testing, i.e., they were neither arbitrary, nor optimized by some objective measure, but an optimal choice based on subjective judgement.

7. Line 208. Which input fields are the authors referring to? Could they be more specific please?

This is indeed formulated misleadingly, thank you for pointing this out. We have adapted the text to clearly portray that only the thermal input field (theta-e) is smoothed.

Old:
Input fields are smoothed with the diffusive filter described by Jenkner et al. (2010) with 160 repetitions. Further noise reduction is achieved by computing the gradient at each grid point across multiple unit grid distances using offsets of (i±4,j±4)instead of (i±1,j±1).
The frontal areas are derived from a thermal and a wind component:

- *The thermal component is based on* $|\nabla\theta|$ *at 850 hPa, from which a mask is derived by applying a minimum threshold. The latter varies over the year to account for the strong seasonal cycle of humidity (and therefore of* θ_e*) leading to substantially lower cross-frontal* θ_e *gradients in winter than in summer, and thus far fewer winter than summer fronts for a given threshold (Rüdisühli, 2018). A threshold value is defined in the middle of each month (Table 1) and linearly interpolated to each hour in-between.*
- *[...]*

New:
The frontal areas are derived from a thermal and a wind component:

- *The thermal component is based on* θ_e *at 850 hPa. The* θ_e *field is first smoothed with the diffusive filter described by Jenkner et al. (2010) with 160 repetitions. Its gradient* $\nabla\theta_e$ *is then derived by computing the gradient at each grid point across multiple unit grid distances using offsets of (i±4, j±4) (instead of (i±1, j±1)) in order to achieve further noise reduction. Then a mask is derived from* $|\nabla\theta_e|$ *by applying a minimum threshold, which varies over the year to account for the strong seasonal cycle of humidity (and therefore of* θ_e*) that leads to substantially lower cross-frontal* θ_e *gradients in winter than in summer and thus far fewer winter than summer fronts for a given threshold (Rüdisühli, 2018). A* $|\nabla\theta_e|$ *threshold value is defined in the middle of each month (Table 1) and linearly interpolated to each hour in-between.*
- *[...]*

8. Line 225. Why are short-lived fronts discarded in this study? Do they contribute to local precipitation, for example precipitation can sometimes be seen at the leading edge of sea-breeze fronts which are short lived?

Some short-lived fronts looked rather spurious (they just fulfilled the front detection threshold at one time step) and we decided to remove them. This comes at the cost that also some physical short-lived fronts, e.g., related to the see-breeze circulation have been eliminated from our analysis.

9. Line 231. I didn't follow the reasoning for defining local fronts by their size and stationarity. Surely it would be more logical to present the definition of synoptic fronts as large and non-

stationary and thus assume the remainder are local (if they occur close to orography or coastlines) rather than the other way around.

The reason why the criteria focus on the local fronts is that the primary motivation to introduce this distinction in the first place was to remove the local fronts from the data set as they were so abundant, especially before we switched from using the 2.2 km data to the hybrid fields on the 12 km grid. However, we agree that it would indeed be more intuitive to focus the grouping on the synoptic rather than the local fronts, and have adapted the text accordingly. (We have also inverted the definition of *stationarity* such that it increases, rather than decreases, with higher values.)

Old:

[...] Local fronts – largely produced by differential heating along topography and coasts – are generally smaller and more stationary than synoptic fronts. These properties can be expressed by a pair of criteria (on which we have settled after extensive manual testing):

- *The* typical feature size *of a track is calculated by first combining, at each time step, the sizes of all features that belong to the track; and then calculating the median of these total sizes over all time steps. Front tracks are considered local if the* typical feature size *does not exceed 1000 km^2.*
- *The* stationarity *of a track is determined as its total footprint area (defined by all grid points that belong to the tracked front at any time) divided by the* typical feature size*. Front tracks are considered local if the* stationarity *does not exceed 6.0.*

All tracks fulfilling one or both criteria are considered local fronts, and thus small and/or stationary. All remaining tracks are considered synoptic fronts, and thus both large and non-stationary.

New:

[...] Synoptic fronts are generally larger and more mobile (i.e., less stationary) than local fronts, which are largely produced by differential heating along topography and coasts. These properties can be expressed by a pair of criteria (on which we have settled after extensive manual testing):

- *The* typical feature size *of a track is calculated by first combining, at each time step, the sizes of all features that belong to the track; and then calculating the median of these total sizes over all time steps. Front tracks are only considered synoptic if the typical feature size is at least 1000 km^2.*
- *The* stationarity *of a track is determined as the typical feature size divided by the total footprint area (defined by all grid points that belong to the tracked front at any time). Front tracks are only considered synoptic if the stationarity is below 0.167.*

All tracks fulfilling both criteria are considered synoptic fronts, and thus both large and mobile. All remaining tracks are considered local fronts, and thus small and/or stationary. Only synoptic fronts are used for the precipitation attribution analysis, while local fronts are removed.

10. Line 267. For the far-frontal precipitation, do these features need to be also within a cyclone mask, or are both local and synoptic fronts included in this classification?

Local fronts are not included in the analysis, only those fronts classified as synoptic. We have added a sentence stating this explicitly at the end of Sec. 2.3 (see answer to comment #9).

The front-cyclone-relative components are defined in the order listed in Sec. 2.5. The far-frontal component is defined second-to-last before only the residual, and thus does not include any grid points that have already been assigned to any other component, including the cyclonic.

(We're assuming that by "features", you mean "precipitation features", rather than front or cyclone features.)

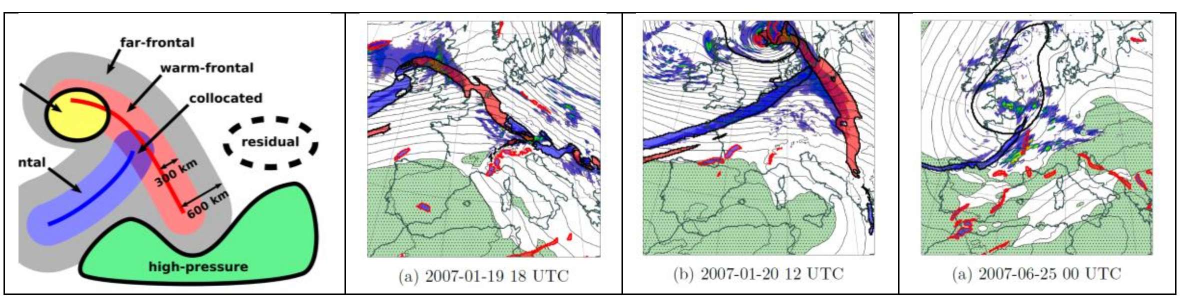
11. Line 273. During the subjective evaluation of the distance thresholds, was any seasonality identified? I.e. did similar thresholds capture the frontal precipitation in both winter and summer?

We did not specifically evaluate the seasonality of the distance of the precipitation to the fronts. However, if there were a pronounced seasonality that substantially exceeded case-to-case variability, we would probably have noticed it. But it must be stressed that such constant distance thresholds don't easily capture all precipitation even within a given system -- let alone for different systems -- regardless of the season, which is part of the reason we opted for a two-threshold approach in order to focus on the precipitation close to the fronts while still capturing that at a greater distance as "far-frontal".

12. Figure 2. This schematic implies that cyclonic and cold frontal precipitation are mutually exclusive. I guess this is not necessarily true, especially during the early stages of cyclone evolution. Also, given the cyclone is part of a larger-scale wave pattern, the location and shape of the high-pressure region in the schematic seems a little odd. What is the reasoning behind the shape and position of the high-pressure region in the schematic?

Cyclonic and cold-frontal precipitation are, by our definition, indeed mutually exclusive. Of course, there is also precipitation which is simultaneously cold-frontal and cyclonic, and in principle we could further subdivide the cyclonic contributions into "purely cyclonic", "cyclonic/cold-frontal", etc. However, this would only further increase the number of components, which is already high enough at seven.

As for the schematic, it aims to represent some characteristics of high-pressure areas (as we defined them) as observed in our data and represented in the case studies (see figure below). In addition, the schematic shows the high-pressure region to overlap the far-frontal area, which highlight its precedence over the latter.



That being said, we do agree that the shape of the high-pressure area in our schematic turned out a bit "funny" and we will improve it in the revised version. We plan to make the outline of the high-pressure area more oval, move it out of the warm sector, and instead have it overlap the frontal areas near the southern tip of the cold front (similar to both case studies) to illustrate the precedence of the high-pressure over the frontal components.

13. Figure 3. This figure is too small to see the detailed frontal precipitation features.

Since the reader can zoom in into the high-quality PDF, it should be possible for them to see the important features.

14. Line 289. I do not see the warm front identified in figure 3b. If I understand correctly, this would be a red filled black contour. Where is this feature on the figure?

The warm front is at this time step indeed not identified as a synoptic warm front, only as a local one (red contour). That's why we refer to it in the text merely as "a feature", which may be local or synoptic. "Warm front" in this sentence refers to what we know is there, not to what the algorithm identifies (or doesn't). We concede that this sentence is not clear enough and have adapted it.

Old:
The warm front east of the cyclone, now detected as a feature, is much weaker than the cold front and produces no precipitation, except close to the cold front, where occlusion may have commenced.

New:
The weak warm front east of the cyclone – now detected, albeit only as a local front – is much less pronounced than the cold front and produces no precipitation, except close to the cold front, where occlusion may have commenced.

15. Lines 295-300. In figures 3b and 3c there is a lot of precipitation that would generally be associated with the occluded/bent-back warm front which is not associated with frontal features using the objective criteria, nor within the cyclone feature contour. Which classification does this precipitation fall into? From figure 4 it looks to fall into the residual. This does not seem correct to me but is not referred to by the authors.

In Fig. 3b, the whole bent-back portion of the precipitation band – actually most precipitation – is inside the cyclone contour and therefore classified as cyclonic. Note that we do mention that this missing front does not seem correct (l.295ff, *"The precipitation band along its bent-back portion wraps almost completely around the cyclone center, much farther than the respective front feature, which suggests that not the whole front has been detected as a feature by our algorithm."*), although in this case it would not make a difference as the cyclonic component takes precedence over the frontal ones in our algorithm.

In Fig. 3c, on the other hand, the remnants of the precipitation behind the cyclone center fall just outside the cyclone contour. However, there is a small cold-frontal feature east of Scotland, so at least some of this precipitation will be cold- and far-frontal. The southern part of this precipitation area presumably contributes to the residual precipitation feature in that area shown in Fig. 4h.

It is true that we do not explicitly refer to the residual precipitation in Fig. 3c. However, precipitation that should subjectively have been attributed to a cyclone or front but wasn't because the algorithm is not perfect is an inherent part of the residual component. Given the miss in Fig. 3c is, in our opinion, not egregious, we did not specifically comment on it. It would have been a completely different story, of course, if indeed the whole precipitation area bent around the cyclone center in Fig. 3b had been misclassified as residual; that definitely would require a comment.

16. Figure 5. Similar to the comment above, in figure 5a there is a lot of precipitation close to the developing cyclone centre along a bent-back warm front. However, because this cyclone does not have a closed contour it is not captured by the cyclonic criteria. Would this just be assigned to the residual?

In Fig. 5a, only the precipitation beyond 600 km from the outline of the warm front would be classified as residual, which likely captures most of this precipitation, so it will be classified as a mixture of collocated, warm-frontal, and far-frontal. While there is indeed some residual precipitation in this area, as shown in Fig. 6h, that precipitation mostly stems from post-frontal precipitation and the secondary system visible around the British Isles in Fig. 5b and c.

17. Line 327. What do the authors mean by the 'dry gap region between the fronts'? Is this the warm sector of the cyclone?

No, this refers to the region between the tip of the cold front and the warm front that is oriented perpendicularly to it. However, it is indeed not well visible at the selected time steps – it would be more clearly visible in-between Figs. 5a and 5b. We have removed this sentence because it is indeed more confounding than helpful.

18. Line 330. Browning and Roberts (1997) has a nice description of these cold frontal line features.

Thank you for pointing this out, this is very interesting indeed. We have added a brief reference to that paper.

Old:
In the cold sector behind the cyclone, there is widespread patchy precipitation, some of it associated with a relatively shallow cyclone near the British Isles.

New:
In the cold sector behind the cyclone, there is widespread patchy precipitation, some of it associated with a relatively shallow cyclone near the British Isles in a way reminiscent of secondary cold-frontal lines as described for instance by Browning et al. (1997).

Reference:

Browning, K. A., Roberts, N. M., and Illingworth, A. J.: Mesoscale analysis of the activation of a cold front during cyclogenesis, Q. J. R. Meteorol. Soc., 123, 2349–2374, https://doi.org/10.1002/qj.49712354410, 1997.

19. Line 340. It would be interesting to speculate if any of the precipitation occurring along the northern flank of the Alps was enhanced by precipitation from the frontal clouds falling through orographically generated clouds.

We agree that this would be interesting, but such an analysis is beyond the scope of this study. It would also require cloud and precipitation data at multiple levels, which have not been archived for this simulation and would therefore have to be re-simulated.

20. Line 362. There are also large precipitation amounts over fairly modest topography in the domain. For example, in the UK.

We agree that some of the topography experiencing large amounts of precipitation is short of "high" but merely "modest", and have rephrased it accordingly.

Old:
The largest amounts, however, occur over high topography, especially the Alps, the Dinaric Alps, the Norwegian Alps, the Scottish Highlands, and the Pyrenees.

New:
The largest amounts, however, occur over modest to high topography, especially the Alps, the Dinaric Alps, the Norwegian Alps, the Scottish Highlands, and the Pyrenees.

21. Figure 9 and lines 410-415. In this section the relative contribution from different features to the total precipitation climatology is discussed. This quantitative analysis is surely one of the most novel parts of this work and should be reflected in the abstract.

We fully agree and thank you for pointing this out. We will adapt the abstract accordingly.

22. Lines 420-425. The difference between the regions dominating heavy precipitation and overall precipitation is very interesting.

Thank you, we agree!

23. Line 436. Do the authors have a hypothesis for why cyclonic precipitation is not enhanced by topography in contrast to cold frontal precipitation?

No, unfortunately we don't have a convincing explanation for this.

24. Figure 7d. Does the lack of heavy precipitation associated with collocated fronts mean that ascent of warm conveyor belt over the warm front does not lead to heavy precipitation? This is surprising to me.

We also expect that the ascent of WCBs over the warm front can lead to heavy precipitation, and Fig. 6d provides a nice example for this. Our assumption is that climatologically this ascent over the warm front occurs more often over the identified warm fronts than the relatively small frontal segments classified as "collocated".

25. Lines 495-508. This section is a repetition of your results and not a conclusion. I suggest removing this text.

We agree that this section is not strictly necessary and have removed it.

Old:
[...]
The meteorological results of the precipitation attribution show that different components are important in different geographical regions and in different seasons. When considering precipitation over the entire year, the most relevant weather systems are cold fronts near the Alps, warm fronts and cyclone centers in the North Atlantic and Western Europe, and cyclones in the Mediterranean, in particular near Italy and the Balkans. A substantial residual exists (about 20–30 %), indicating that our weather system categories do not encompass all precipitation-producing flow situations and that the attribution to the target systems is not perfect. Strong local enhancement occurs over high topography compared to the surrounding flat areas, 500 which is especially pronounced over the Alps and for cold-frontal precipitation. From a seasonal perspective, (i) cold fronts are important contributors in all seasons (especially over the continent), while warm fronts primarily contribute in winter and fall (especially over the North Atlantic); (ii) the largest cyclonic contributions shift from the Mediterranean in winter to Northern Europe in summer; and (iii) high-pressure precipitation is confined to summer over the continent, with pronounced local enhancement over the Alps. Focusing only on heavy-precipitation events reveals substantial differences to total precipitation: (i) Rather than over high-topography, heavy precipitation is particularly enhanced over land compared to sea; (ii) cold fronts also contribute substantially to heavy precipitation, whereas the relevance of warm fronts diminishes; (iii) cyclones are particularly important for heavy precipitation over the ocean; and (iv) the summertime high-pressure systems further gain in significance, in particular for continental summer convection. The results can be summarized concisely for several distinct geographical regions. [...]

New:
[...]
The meteorological results of the precipitation attribution can be summarized concisely for several distinct geographical regions. [...]

26. Lines 510-550. This section is interesting but should be strongly caveated by the fact that only 9-years of data has been used to create the climatologies. For example, there are many studies demonstrating decadal variability in the latitude of the storm track which would have a large influence on these conclusions.

As regards summer precipitation events, previous studies suggest that the climatology is reasonably well captured by 10-year-long simulations (e.g. Ban et al. 2015, supplemental information), but for the winter seasons significant decadal variations of the NAO indicate that longer periods are indeed desirable or needed to compile a “real climatology”.

To make this point, we have added the following sentences after the regional summary (line 559):

New:
When summarizing these characteristics, it is important to mention another caveat, the comparatively short analysis period of nine years. While interannual variations in summer precipitation appear reasonably well covered with such simulations, variations in the North Atlantic oscillation suggest that longer integration periods are desirable or needed in order to adequately cover decadal variations of the winter season. A significant challenge of such analyses is the costs of storing high-resolution output of multi-decadal simulations. It is thus desirable to use an online analysis approach that performs the respective analysis while the simulation is running (Di Girolamo et al., 2019; Schär et al., 2020), rather than storing the relevant output data. Such an online analysis tool can also be highly beneficial when extending the feature-based analyses in

three dimensions, e.g., by defining fronts in 3D and/or by considering the vertical structure of clouds and microphysical processes.

References:

Di Girolamo, S., Schmid, P., Schulthess, T., and Hoefler, T.: SimFS: A Simulation Data Virtualizing File System Interface, in: Proc. of the33rd IEEE Int. Par. & Distr. Processing Symp. (IPDPS'19), IEEE, http://arxiv.org/abs/1902.03154, 2019.

Technical corrections

27. Line 110. Why does period have a – afterwards rather than a comma?

We agree that commas do as well a job here as the long dashes and have changed the text accordingly.

References

Browning, K.A., Hardman, M.E., Harrold, T.W. and Pardoe, C.W., 1973. The structure of rain bands within a mid-latitude depression. Quarterly Journal of the Royal Meteorological Society, 99(420), pp.215-231.

Browning, K.A., Roberts, N.M. and Illingworth, A.J., 1997. Mesoscale analysis of the activation of a cold front during cyclogenesis. Quarterly Journal of the Royal Meteorological Society, 123(544), pp.2349-2374.