Author reply to the Editor (wcd-2022-11)

We thank the editor, D. Domeisen, for the useful comments and revision of our manuscript. We are happy to have selected Weather and Climate Dynamics as we feel the subject of our manuscript fits the scope of this journal and its special issue on past and future European atmospheric extreme events under climate change.

In the following, we provide a point-by-point answer to the editor's comments to be added to our answers to the reviewers to complement the revised version of the manuscript and the corresponding track-changes version.

Editor Comments

In the first round of reviews, reviewer three asked about the scale dependency of thermodynamic processes and suggested to look into outgoing longwave radiation. I don't see this comment addressed in section 6 of your revised version, could you please make sure to include it and clarify?

The results and discussion of our analysis on Outgoing Longwave Radiation (OLWR) is included in Sect. 7.3 (Soil-atmosphere interactions).

We applied the composite analysis method to OLWR as suggested by reviewer 3, and the results are shown in Figs. 12 and 13 and Figs. S14 and S15 of the SM.

Overall, the composite analysis showed that OLWR differences are linked to surface temperature differences between RCM and CPM, irrespective of the analysed precipitation EOF or season. That is why, generally, OLWR is larger in RCM than CPM (up to 9 W m^{-2}). However, for summer events over the Po Valley these differences can disappear or even be larger in CPM in agreement with the warmer-drier amplification shown by CPM over this area in our study and Sangelantoni et al., (2020) during heat waves.

This information is conveyed twice in Sect. 7.3 for Winter and Summer events:

"Finally, the higher temperatures over land and sea in RCM induce larger outbound long wave surface radiation than CPM, by ca. 10 W m^{-2} (Fig. 12f). This, similarly to surface temperate, applies to all analysed composites except one."

". Finally, the outbound long wave radiation, similarly to Winter events, shows larger values by RCM, compared to CPM (Fig. 13f)."

And in the conclusions

"Regarding differences in surface temperature, RCM showed for most of the analysed EOFs a warmer surface level (by about 1.5 °C). This, in turn, brought larger emissions of outbound long wave radiation in RCM compared to CPM, up to 9 W m^{-2}."

"Finally, the larger specific humidity north of the Alps in CPM leads to larger CAPE over land, whereas outbound long wave radiation is larger in RCM, linked to the warmer surface level in this set-up."

Line 406: there seems to be something missing in this sentence, please check.

We agree with the editor and have corrected the sentence. The paragraph now reads:

"The profile and surface humidity and temperature validation has shown that: a) COSMO-CLM performs well in simulating the humidity and temperature lapse-rates, albeit small biases up to 0.2 g kg^{-1} in humidity and 0.5 °C (warm bias) in temperature exist; b) CPM simulates slightly better the vertical humidity profile with a steeper gradient than RCM; c) CPM reduces the positive surface relative humidity bias over locations north of the Alps, e.g., western France, the Czech Republic and eastern Austria."

Lines 404 - 407: in the manuscript, please briefly comment on the model compensation errors that reviewer 3 mentions, with respect to the lapse rate.

This aspect is described between lines L386 and L392 although, we agree, not so clearly. We adapt this lines in the new version of the manuscript to read:

"The humidity (Fig. 7.c) and temperature (Fig. 7.d) profiles show a wetter mid-troposphere (between 700 hPa and 925 hPa) in RCM than in CPM and a very similar temperature profile between both simulations. CPM simulates slightly better the vertical humidity profile than RCM with a steeper humidity-height gradient. This was also observed in earlier studies with COSMO and COSMO-CLM (Caldas-Alvarez and Khodayar, 2020; Caldas-Alvarez et al., 2021). COSMO-CLM compensates the modelling errors simulating a wetter lower troposphere in RCM to help activate the deep convection parameterization scheme (Tiedtke, 1989). Being of the low-level control type, the Tiedtke deep convection scheme requires a sufficient moisture amount below the cloud base to initiate convection (Doms et al., 2011). By doing so RCM simulates precipitation totals of the same order as CPM that relies more upon the intensification of vertical wind speeds than humidification to simulate convective precipitation. Furthermore, the larger humidity in the mid-troposphere helps reduce the simulated dry-air entrainment increasing the total simulated precipitation. Both simulations show a reliable performance considering the decadal timescales"

For the comment from reviewer 3 about Figure 10, the response to reviewers specifies a change to this sentence, however the indicated corrected sentence is not part of the new version of the manuscript. please clarify.

In our last answers to reviewer 3 we included a sentence from the first submitted version of our manuscript. We included this sentence to show that we aimed to relate the different model variables between one another but that we needed to improve this aspect.

Her/his concern was that "some of the effects illustrated are very closely linked together, e.g. the effects seen in nearsurface specific humidity and surface latent heat flux.". And that this should be pointed out clearly.

In the second submission we conveyed this concern in the detailed description of model variables' differences between RCM and CPM in 7.3 and more generally, in the conclusions. For instance, we related the differences in latent heat fluxes with differences in surface specific humidity and ultimately cape. Likewise, we linked differences in sensible heat fluxes to the differences in surface temperature, affecting in turn OLWR.

We hope to have replied to reviewer 3 and include here again the two related conclusions:

"For Winter events, latent heat fluxes in CPM were larger over land than in RCM (up to 15 W m-2) on the day prior to severe precipitation. Over the Sea, the opposite occurs, and RCM overestimates the latent heat fluxes compared to CPM (30 W m-2 more). The consequence is an overestimation by CPM of surface specific humidity over land areas north of the Alps compared to RCM (1 g kg-1). However, RCM simulates more specific humidity over the Sea and Italy. The wind transports the moisture excess

in RCM inland. Regarding differences in surface temperature, RCM showed for most of the analysed EOFs a warmer surface level (by about 1.5 °C). This, in turn, brought larger emissions of outbound long wave radiation in RCM compared to CPM, up to 9 W m-2."

"For Summer events, CPM simulates larger latent heat fluxes over land than RCM, although now restricted to locations north of the Alps. Surface sensible heat fluxes, on the contrary, are larger over land in RCM than in CPM (up to 20 W m-2 more), although these differences are weaker over the Po Valley. The consequence is that CPM simulates larger surface specific humidity north of the Alps whereas RCM simulates larger specific humidity over the Mediterranean and Italy. The different partition of heat fluxes leads to a higher surface temperature in RCM than in CPM over the Alps and northern Europe. Over the Po valley and Italy these differences are weaker or even favourable to CPM. Finally, the larger specific humidity north of the Alps in CPM leads to larger CAPE over land, whereas outbound long wave radiation is larger in RCM, linked to the warmer surface level in this set-up."

Additional private note (visible to authors and reviewers only): Overall, the tracked changes version does in many places not correspond to the new version of the manuscript. In the new revision round, please make sure that these versions correspond to each other to allow the reviewers and editors to properly assess your manuscript.

Yes, we agree that there were changes in the "clean" version in the manuscript, which were not included in the track-changes version. The reason is that the implemented changes were so many, including figures, text, author comments and replies that the track-changes versions became too heavy to work with. We experienced computer problems opening and sharing the track changes version in the latest stages of our revision, so the latest comments and changes had to be included in the "clean" version.

We apologize for any inconvenience and are will obviously make sure to submit coherent versions of the track-changes and "clean" manuscripts in the new round of revisions.

Author reply to RC1 (wcd-2022-11)

General Comment

The authors properly addressed my previous comments (and those of the other reviewers) and the manuscript is considerably improved with respect to the former version. I have no further technical objections and in my opinion the paper can be published. However, I still see some weaknesses in the English style, but on this issue I leave the decision to the Editor.

We thank the reviewer for revising again our manuscript and her/his advice on improving the English style. We will do as suggested and improve the English language.

Author reply to RC2 (wcd-2022-11)

We thank the valuable comments and corrections suggested in this second revision.

In the following we reply to all raised issues and include all corrections in the new version of the manuscript.

Major Comments

I do not fully agree with the statement about the overestimation of precipitation over the Alps (L22/L542). For this argument, the authors primarily refer to studies that apparently support their claims. However, the authors of these studies are more careful about that statement, and typically refer to uncertainties related to the gridding procedure, sampling biases due to the gauges being primarily located in valleys, and the prominent under-catch issue by gauges during HP events. I agree with these studies that, while generally useful, the current datasets/observations are not fit for making the "HP overestimation in CPMs" claim in the Alps.

The statement that our CPM simulation overestimates heavy precipitation intensities is based on the analysis introduced in Sect. 4 and Fig. 5 (empirical PDFs of precipitation). Figure 5 shows that over study region SGer in the period 2000-2015, CPM had some probability to represent precipitation intensities over 210 mm d^{-1} which is larger than the observed in HYRAS-5km.

The cited publications referred a similar effect in their CPM set ups. Although the differences in the modelling set ups between experiments are numerous, we believe it is worth mentioning that a similar effect has been observed. For instance, Kendon et al., (2012) mention that their 1.5 km decadal simulations in the UK "have a tendency for heavy rain to be too intense". Likewise, Berthou et al., (2018) describe that "mean precipitation is increased over the Alps and becomes larger than in the observations". Finally, Vergara-Temprado argue that "even at grid spacings of 1 km, convective processes will not be fully resolved […] which might help explaining the overestimation in extreme precipitation intensities at high resolutions".

We acknowledge that we need to improve the description of the uncertainties in such analyses to fairly describe the problem. Hence, we will mention the under-catch issue, sampling problems and gridding procedures that the reviewer and the other publications mention.

This is improved in Sect. 4

"It should also be noted that even for grid resolutions down to 1 km the updrafts might not me simulated with the right intensity, which can help explain the overestimation of precipitation at these high resolutions (Vergara-Temprado et al., 2020). Also, the comparison against observations must be done carefully as heavy rain measurements might suffer from under catchment, which can reach even 58 % in the worst scenarios (Vergara-Temprado et al., 2020). Furthermore, problems associated with the gridding of precipitation observations and the fact that rain-gauges in the Alpine region tend to be located at the valleys, add uncertainty to the estimation of precipitation and any validation of model data."

I am also a bit concerned about the discussion of IWV and its relation to HP (L30 - 35 and L565 – 587). In particular, I did not fully comprehend the presented argument. That is because the authors primarily present results, rather than their idea, and leave most of the interpretation to the reader. It would be helpful if the assessed hypothesis were concisely stated in the introduction, and then assessed in Chapter 8. I currently interpret the results as if the authors alluded to IWV (and remote sensible and *latent fluxes over the ocean) being an important driver for differences in HP between RCM and CPM. I don't think that the presented analysis would convincingly outline how such a mechanism would work.*

Thanks for this comment. In our study we were interested in knowing what the differences between RCM and CPM were, regarding simulated variables and processes that affect heavy precipitation. Therefore, we studied model differences of IWV and surface fluxes in the days prior to heavy precipitation.

In the paragraphs mentioned, we present the observed model differences which in general were already present in the seasonal signal. Hence, we cannot attribute the modelling differences in HP to differences in IWV, surface fluxes etc, as they were present in other types of weather regimes. This is why we do not present that mechanism but describe our results which, we believe, could be useful for other modellers that could come across similar differences.

Minor Comments

L75ff: The argument presented by Hohenegger et al. (2009) has meanwhile been augmented and better understood. In particular, the sentence starting on L77 is now outdated. More recent hypothesis actually involve a spatial scale dependency ;-) (e.g., Taylor et al. 2012), that is actually represented in kilometer-resolution climate simulations (e.g., Leutwyler et al. 2021).

We extend the discussion in the introduction to include the results in these publications

"Regarding the soil-moisture-precipitation feedback, past work has shown that RCM tends to show a positive sign (Hohenegger et al., 2009; Leutwyler et al., 2021) whereas CPM can show both negative and positive signs at the sub-continental and continental spatial scales, respectively. The reason is that wetter soils induce more frequent precipitation at RCMs but more intense events in CPM with, however, a weak impact on frequency (Leutwyler et al., 2021). CPM seem to better agree with observations as previous observations showed a negative sign of the feedback due to an increased sensible heat flux over drier soils, and mesoscale variability in soil moisture which intensifies afternoon convection (Taylor et al., 2012)."

L181/L252: In the revised version of the manuscript the 80th all-day percentile is used, right? Maybe thus mention "all-day" explicitly in the text?

Yes, we agree. We include it in the text

L303: The text reads as if the authors are talking about the probability of exceedance. Why not show this metric instead (Figure 5)?

There are small differences in our analysis as the probabilities shown are empirical. They stand for the number of times a certain precipitation intensity is simulated (or observed) in the data set.

Section 6/Figure 9: The authors explain what they did, but I do not fully comprehend what they want to demonstrate. For unfamiliar readers (like me), it might be worthwhile to add a sentence or two explaining the intent at the beginning of the Section (same for L440ff), and a few words at the end summarizing the findings.

We agree. We add the following introductory sentence

"To understand how differently RCM and CPM represent the main spatial patterns of heavy precipitation we use PCA (Sect. 2.3.2) on events detected in HYRAS-5km in the period 2000-2015. We

do this to observe differences in the spatial distributions of heavy precipitation during the most frequent precipitation modes."

And concluding remark

"Our analysis shows that RCM and CPM simulate similarly the main precipitation modes up to the fourth principal component in Winter and the third in Summer. These precipitation modes account for 47 % of the precipitation variability in Winter and 37 % in Summer, implying that a large part of the precipitation differences belongs to the secondary modes of precipitation."

L485: I do not understand the role of the green shading. Is there something awkward with "negative" sensible heat fluxes? I think the authors need to explain better what the problem is. Also, I am not familiar with the term "surface directed fluxes" used in the caption of Fig 12, and I do not immediately grasp what it means. Please explain it in the caption.

The problem with negative heat fluxes are the differences between RCM and CPM. If at a certain location RCM shows a large negative heat flux but CPM simulates a weak negative flux, the difference RCM-CPM will be negative. Compared to the other plots, the reader could understand that a negative heat flux stands for a larger emission of heat flux in CPM than in RCM, which is not the case.

We will add a clarification in the text.

"Figure 12b illustrates these results where differences over the sea are close to zero and green colours denote no positive outbound heat emissions over land. Inbound directed fluxes are dismissed to avoid confusion with the interpretation of the signs in the difference plots. "

L497 – L507: I think these paragraphs need to be rewritten. They read like notes rather than actual paragraphs, also I am confused what the underpinning message is.

We will rewrite them to clearly show the message.

L572: "overestimates" and "overestimation". I think these words can only be used when comparing against observations. What is wrong with the word "larger"?

We agree with the reviewer, we correct this in the following version of the manuscript.

L576: "The wind transports the moisture excess in RCM inland." How do you know that? I think such a statement would require a moisture budget, possibly even including a trajectory analysis.

We agree that we do not provide quantitative evidence for the moisture transport. We will mention it as a plausible hypothesis explaining the humidity differences observed over Italy.

"However, RCM simulates more specific humidity over the Sea and Italy, possibly due to the effect of the southerly winds"

L595: Maybe the original studies merit citation instead? Chubb et al., (2015) provide a nice summary.

Yes, thank you for the reference.

Suggestions

Title: "Regional Climate and Convection-Permitting Modelling of heavy precipitation ..." Maybe better: Convection-Parameterizing and Convection-Permitting Simulations of Heavy precipitation […]. The term RCM usually refers to the limited-area extent of the computational domain rather than to the parameterization of convection.

We agree we adapt the title.

L97: Maybe add a sentence outlining how the method will be exploited in the presented study?

Yes, we agree, the following sentence has been added:

"In this work we will derive composites of relevant model variables and study differences between both modelling set-ups."

L335: Maybe explain what you mean with "event scale"?

The sentence has been rewritten to

"In the previous section, we assessed an overestimation of grid-point heavy precipitation for the convection-permitting simulation CPM, but a good performance in detecting severe precipitation events in a 44-year climatology. Here we evaluate the performance of CPM at the event scale validating eight chosen events"

L404ff: One might summarize the surface T and QV verification by stating that the event scale verification is consistent with the well-known too dry too hot bias of CPMs. In particular because most of the assessed events are in summer.

We include this remark.

L566: I thought you wanted to remove the term "moisture excess" from the manuscript.

Yes, we correct it in the manuscript.