Authors response

Journal: Earth System Dynamics, discussion paper esd-2024-16
Review Revised manuscript, 22 November 2024
Title: “The European summer heatwave 2019 – a regional storyline perspective”
Authors: Tatiana Klimiuk, Patrick Ludwig, Antonio Sanchez-Benitez, Helge F. Goessling, Peter Braesicke, and Joaquim G. Pinto
Recommendation: [Minor Revision]

GENERAL:
I want to thank the authors for their detailed and extensive reply to the points I raised when reviewing the original manuscript. As far as I am concerned, the authors have adequately addressed most of the issues, leaving just a few points that still require attention, before the manuscript is suitable for publication.

We thank Referee #2 for the valuable comments. We were glad to elaborate on the raised issues and have addressed the remaining points as follows:

REMAINING POINTS:
1. In reply to my previous point 15 regarding the, in my opinion, improper use of the wording “added value” the authors have removed one “added value” phrase, but retained the rest. In my perception, the reduction in temperature bias when going from the GCM to the RCM is merely the outcome of a using different physics formulations, and also because the GCM apparently performs very poorly for near-surface temperature. I don’t think the higher resolution itself plays a role in the improved skill of the near-surface temperature representation. Unless the authors can make plausible the bias reduction is caused by the higher resolution of the RCM wrt the GCM, I advise to avoid the wording “added value”, and use “improved”, “gain”, ”benefit” or comparable wording.

Reply: Thank you for the comment. We agree that in the context of our study, it is more appropriate to refer to “improvement” for near-surface temperature representation by dynamical downscaling rather than using the more specific concept of “added value”. In general terms, the added value of RCMs compared to GCMs is primarily due to a better representation of the physical processes, the orography and the surface characteristics, thus facilitating an improved perspective of the atmospheric circulation and its impacts at regional to smaller scales (rainfall, temperature, wind, and others) and thus closer to the observations.

The following sentences were rephrased in the manuscript:

L13/14: We provide evidence that the downscaling of global storyline integrations significantly *improved the representation of* present-day temperature patterns and reduced error in daily 2m temperature relative to observations in Central Europe.

L97/98: (1) How accurately can a regional event-based storyline simulation represent a recent event, and what is the *improvement* compared to the global spectrally nudged storyline simulation

L176/177: The root mean square difference (RMSD) to observational datasets (DWD and E-OBS) and its change between simulations of different resolutions (ΔRMSD) is chosen as a metric to *compare the representation of near-surface temperature by the models of our GCM-RCM-CPM chain in the present-day storyline.*

L192/193: *We compared the performance of the simulations within the model chain by calculating the* root mean square difference (RMSD) in June - August of the simulated 2m temperature with respect to DWD observations …

L203: *We* interpolated the ICON EUR-12 data to the grid of AWI-CM1 and compared the RMSD of both models to E-OBS.

L216: The nested convection-permitting GER-3 simulation was assessed by comparing the RMSD of the 2m temperature to observations with the RMSD of the driving EUR-12 simulation.

Caption of Figures 4 and 5: Performance assessment …

L228: Analogously to the EUR-12 simulation, the *evaluation of* GER-3 simulation…

L233: Given the *improved performance* by dynamical downscaling with ICON-CLM for present-day conditions,...

L325: (1) How accurately can a regional event-based storyline simulation represent a recent event, and what is *the improvement* compared to the global spectrally nudged storyline simulation

2. Lines 358-359 (the authors reply to my previous point 28): This is a possible explanation but without testing it remains highly speculative. The 2019 heat wave event was relatively short-lasting and soil conditions prior to the event were not particularly dry (unlike in 2018). In my opinion it is at least equally plausible that the region with highest future warming is slightly displaced with respect to the region with highest temperature in the reference run, because the overlying atmospheric flow pattern with highest temperatures in the future runs is slightly off. It is just very unlikely that the region with highest temperatures in the future runs precisely collocates with that region in the control run, and, thus, highest warming rates will always be found off the centre with highest temperatures in the control run. Please express that, without testing, the explanation you provide is highly speculative, and that alternative explanations (or combinations of them) are equally plausible.

Reply: We understand the concern of the reviewer and have modified the paragraph respectively.

In the marked-up version of the manuscript, lines L369-380 include the following changes:

Several factors may have influenced the spatially variable magnitude of the 2m temperature response, which is seen both for a single member (Fig. 9) as well as for all ensemble members (Fig. S16). One possible explanation for this behaviour is a heterogeneous

response of soil moisture-temperature coupling within and outside the heatwave's core (see e.g., Gevaert et al., 2018; Miralles et al., 2014). We hypothesise that the overall amplification of the warming rates during heatwave events and the extension of the diurnal temperature range may have been exacerbated due to soil-atmosphere feedback. However, a dedicated and detailed analysis would be necessary to demonstrate or dismiss this hypothesis, as other factors like small changes in atmospheric dynamics may have also played an important role.

OTHER POINTS:
Reply: Thank you for the thorough review of the text. We have elaborated on the points raised below and provide here the numbers of the lines in the marked-up *version* of the manuscript, where you can find the changes applied

1. Line 65:"from various models or model means". This is a somewhat vague formulation. In practice I would say the deltas are derived "from multi-model ensemble means or single-model multi-member ensemble means" (see de Vries et al. for examples).

Change in L66/67: On the other hand, one of the advantages of the PGW approach over the nudged storyline approach is the potential to avoid GCM-specific biases by repeating the experiment with deltas derived *from multi-model ensemble means or different single-model multi-member ensemble means (see e.g., Aalbers et al., 2023; Vries et al., 2024)*

2. Line 78: CPM stands for Convection Permitting Models (and not Convective Permitting Models). Please adjust here and throughout the remainder of the text.

Change: L80, L99, L216, caption of Figure 5, L332

3. Line 135/136 (and further down): The word dynamical in the phrase "the dynamical year" is somewhat confusing, although I presume it stands for "large-scale dynamical constraints inferred from ERA5". For clarity, please, make explicit what you mean at the first occurrence.

Change in L131/132: *Throughout the text, for all storylines, we refer to the years corresponding to the present-day circulation inferred from ERA5 as the "dynamical years" 2017–2022.*

4. Line 135:".. the 31st of September .." → either ".. the 30th of September .." or ".. the 31st of December"
Change in L130: Each storyline is simulated continuously from the 1st of January 2017 to the 30th of September 2022.

5. Line 156-157: I could not find a reference of your reply to my previous point 9 regarding the mapping of soil information from ERA5 to ICON in the revised manuscript. Please mention explicitly it in the text, at least including the reference to Prill et al.

Change in L160-163: *In ICON, the initialising soil data is pre-processed and remapped onto the 8-layer mesh by the built-in algorithm (Prill et al., 2023; Pham et al., 2021). To account for the possible discrepancy of soil types between ICON and ERA5, the volumetric soil*

moisture is transformed into the universal soil moisture index (SMI), which makes it independent of the soil type (Prill et al., 2023).

6. Lines 159-160: Change “lowermost soil level” into “lowermost soil layer” (or “bottom soil layer”)

Change in L166: Additionally, the temperature of the *bottom soil layer,* which is not prognostic *in the TERRA land module of ICON* but is set to the climatological annual mean near-surface temperature T_CL based on the Climate Research Unit data,…

7. Lines 159-162: Regarding your reply to my previous point 10 (on the meaning of T_CL) I am wondering if there is a similar treatment of bottom layer soil moisture. Could you spend one or two lines on that?

Change in L169-170: *The lower boundary condition for soil moisture is given by a free-drainage formulation and thus did not require additional adjustments (Prill et al., 2023; Chen et al., 2018; Zeng and Decker, 2009).*

8. Line 276: “... Autumn” → “early autumn” (simulations stop end of September); I also suggest to change “... other summers ...” by “... the other summer seasons (May-September) ...” (or (MJJAS))

Change in L286: In contrast, the temperature response appears to be distributed more uniformly during the day in spring, early summer, and *early autumn*. We obtained similar behaviour in other *summer seasons (May - September)* of the simulated period.

9. Line 355: “should cover” →”should have covered”
10. Line 355: “Nevertheless, ...”→”Still, ...”

Change in L366: We admit that for a more comprehensive km-scale investigation of this heatwave, this domain should *have covered* a larger fraction of France. *Still*, as this study is part of the Innopool SCENIC project…

11. Caption Figures 3, 7, 8: “five-member” → “5-member”
Change: see captions of Figures 3, 7, 8

12. Captions Figures S3 and S4: “a-d” should be “a-c”
Change: see captions of figures S3 and S4