Point-to-point response to anonymous referee #1

Summary

The study "Sensitivity of precipitation and temperature over Month Kenia area to physics parameterization option in a high-resolution model simulation performed with WRFV3.8.1" by Messmer et al. submitted to GCM focuses on the analysis of several sensitivity experiments with the Weather Research and Forecasting model version3.8.1 for Mont Kenia for the year 2008. This work analyzes different parameterization options as well as different nesting strategies (number of nests and nesting ratios). The evaluation of the model performance is carried out in terms of precipitation and temperature by comparing the outputs from the WRF model with observational products from different gridded products but also using observational station datasets.

General comments:

The manuscript is very well written, making it easy to follow. The structure is appropriate with a complete description of the methodology and an adequate list of references. This kind of study is essential before completing climate simulations, being the area of study of high interest as it is a topographically complex region poorly study until now that requires high-resolution climate information to be properly described. In my opinion, the results found in relation to the nesting strategies are very interesting, this being a relevant aspect for properly configuring climate simulations at high resolution. Also, the method used to evaluate the model configurations by comparing the outputs of the model with different observational products seems to be adequate. However, there are several major aspects of the manuscript that should be clarified before its publication.

Thank you for reading our manuscript so carefully and for taking the time to review it. Thank you for asking critical questions, which help us to improve the quality of the paper.

My major concerns are mainly related to two aspects. The first one is the period used to analyze the WRF model performance for the different configuration options. Being the final goal of the study the selection of a "good configuration" to use WRF for climate runs, why did the authors select a 1-year period (the year 2008) to carry out this study? Did the authors test the model performance in other years with different precipitation characteristics? In my opinion, further analyses could be needed in order to corroborate the results from 2008. To do this, analyses for an additional year, for example, a year considered to be a wet year (as 2006) could be carried out.

We have decided to choose one year, as the selected domain setups are computationally quite heavy (38'000 CPU hours per model year and 7 TB of storage space), so we decided to run a large set of experiments, at the expense of a somewhat shorter simulation. We have tested 4 different domain setups and at least 5 parameterization options for each setup, which results in more than 20 years of simulation. This corresponds almost to a full climatology. Owing to the fact that the resolution is very high, longer and more simulations can only be afforded when reducing the number of parameterization options. Nevertheless, we agree that it is good to have one additional sensitivity test using a wetter year as well.

Following the suggestion of the reviewer, we have analyzed the year 2006, which presents wetter conditions than 2008. In this case, we run the simulation only for the best configuration ("Cumulus3 1-way"), and the results of this experiment are compared to the gridded datasets ERA5, IMERG and CHIRPS, and to the available weather station data for 2006. Overall, we find that the optimal setting applied to 2006 resemble the findings of 2008 with even higher pattern correlation to station data than for 2008.

We added a few paragraphs to the new version of the manuscript related to the results of the year 2006 at the end of Section 3.1 and in the conclusions. Figure R1.1 and Figure R1.2 are added as supplementary material to the manuscript.

On the other hand, I am a little bit confused with the "no cumulus" experiment setup. If I understood well, the authors here have used the WRF model with the cumulus parameterization switch off in all domains (from the 27km_D04/25KM_d03 to the finer domain of 1km of spatial resolution), but did the coarser domains (i.e., 27km and25km) also run without convection scheme? I think that more information is required in this regard. Also, why for this parameterization configuration option are there three of the nesting schemes (i.e., 27kmD04, 25kmD03, 9kmD03) instead of the four used in the other cases (i.e., 27kmD04, 25kmD03, 9kmD03, 25kmD02)?

Thank you for pointing out that the description of the "No cumulus" parameterization option is still unclear. For the "No cumulus" simulation we have turned off the cumulus parameterization in all domains, i.e., we have turned it off also in the 27/25 km and 9 km domains, where it is normally suggested to use a cumulus parameterization. Note further that the cumulus scheme is turned off for grid spacings equal or finer than 5 km in all the experiments.

This is also why there is no "No cumulus" parameterization experiment for the 5km_D02 setting. As the same parameterizations are used in both "No cumulus" and "Cumulus 3 1-way" except for the cumulus scheme, these two experiments are identical for the 5km_D02 setting since cumulus parameterization is switched off in both setups at that resolution. We understand that the text in the manuscript still leads to some confusion. We have updated Table 1 in the manuscript, as this table was imprecise in the former version of the manuscript. Now, we have removed the name of the cumulus parameterization if all the domains are in the convection permitting scale and we have written the domains in bold letters if a cumulus parameterization is used. Furthermore, we have included the fourth simulation of the "No Cumulus" set up and have added a footnote explaining that this last simulation is the same one as the last in the "Cumulus3 1-way" experiment. We have also added this information to Section 2.1. in the manuscript. We hope that we were able to clarify the misleading statements related to the different experiments for the reader.

Additionally, I think that some analysis concerning the ability of the model at a sub-daily scale would be nice to clarify if the "no cumulus" option provides an added value in relation to the other options used with convection physic schemes at this temporal scale.

This is correct, a sub-daily analysis is nice to further investigate the skill of each parameterization and in particular for the "No cumulus" parameterization such an analysis is interesting. Since most of the observations are only available on a daily basis, a sub-daily analysis is a bit difficult, but we performed it on a monthly basis with IMERG, as we have 1-hourly data available there. The mean hourly precipitation has been calculated over the entire domain (D4 and D2) for each sensitivity experiment and IMERG. Additionally, to ease the interpretation of the results, the local time in Kenya (+2 UTC) has been employed while plotting the data.

Figure R1.3 (left column) shows the daily cycle for D4. All the simulations show a similar daily cycle throughout the year, except for some differences in intensities. All the simulations describe precipitation throughout the day fairly similar, except for the "No Cumulus" experiment, which overestimates precipitation rates in the afternoon in most of the months. Since all the experiments resolve convective processes explicitly it is expected that the "No Cumulus" option does not add any value in D4. This is different if we compare the simulations in domain D2. Here, except for the "No Cumulus" experiment all the simulations have parameterized convective processes. Figure R1.3 (right column) reveals clearly the added value of not using a parameterization scheme, as peak precipitation of IMERG and the "No Cumulus" option agree, while the other options simulate the peak a few hours too early.

The results of domain D2 are very interesting and we have added a small paragraph regarding the timing of peak precipitation in the “No Cumulus” experiment to the results. As we do not show any result for D2 in the manuscript, adding Figure R1.3 to the manuscript would be misleading. We have added this figure to the supplementary material.

Specific comments:

L248-250: Please, indicate the method used to interpolate the gridded products.

We used bilinear interpolation when comparing the gridded products. This information is included in the new version of the manuscript.

We have added the exact interpolation method in Section 2.3 (L179.) as it is the place where the interpolation is first mentioned. We have adapted the sentence to:

“Please note that the gridded data sets are bi-linearly interpolated (Climate Data Operator (CDO; Schulzweida, 2019) to the grid of the WRF domain that it is compared to, and the nearest point to the station is considered afterwards.”

Figure 5: The colors selected for “Cumulus3 1-way”, “No Cumulus”, and “ERA5” are hard of difference sometimes, so I would suggest using an additional way (e.g., dotted lines for observations) to clearly show what data are represented in each case.

It is a valid point to add dotted lines for the observations, so we have adapted this figure accordingly.

Figure 6 onward: In order to clearly show what option is better, I would suggest adding the correlation patterns between CHIRPS and the different parameterization options, for example, at the bottom of each figure.

Thank you for this suggestion. This is a good idea, and we have included this number in the lower right corner of each panel. Apart from the pattern correlation against CHIRPS, we have decided to include the pattern correlation against the weather station data in each plot as well.

Technical corrections:

L115: Velasquez et al. 2020?

We addressed this technical correction in the new version of the manuscript as suggested by the reviewer.

L178: Please, move the Climatic Research Unit (CRU) definition from L215 to L178. It is the first time that CRU is named.

We have deleted the section on CRU, because Fig. 9-11 are adapted in accordance with a suggestion of reviewer 2.

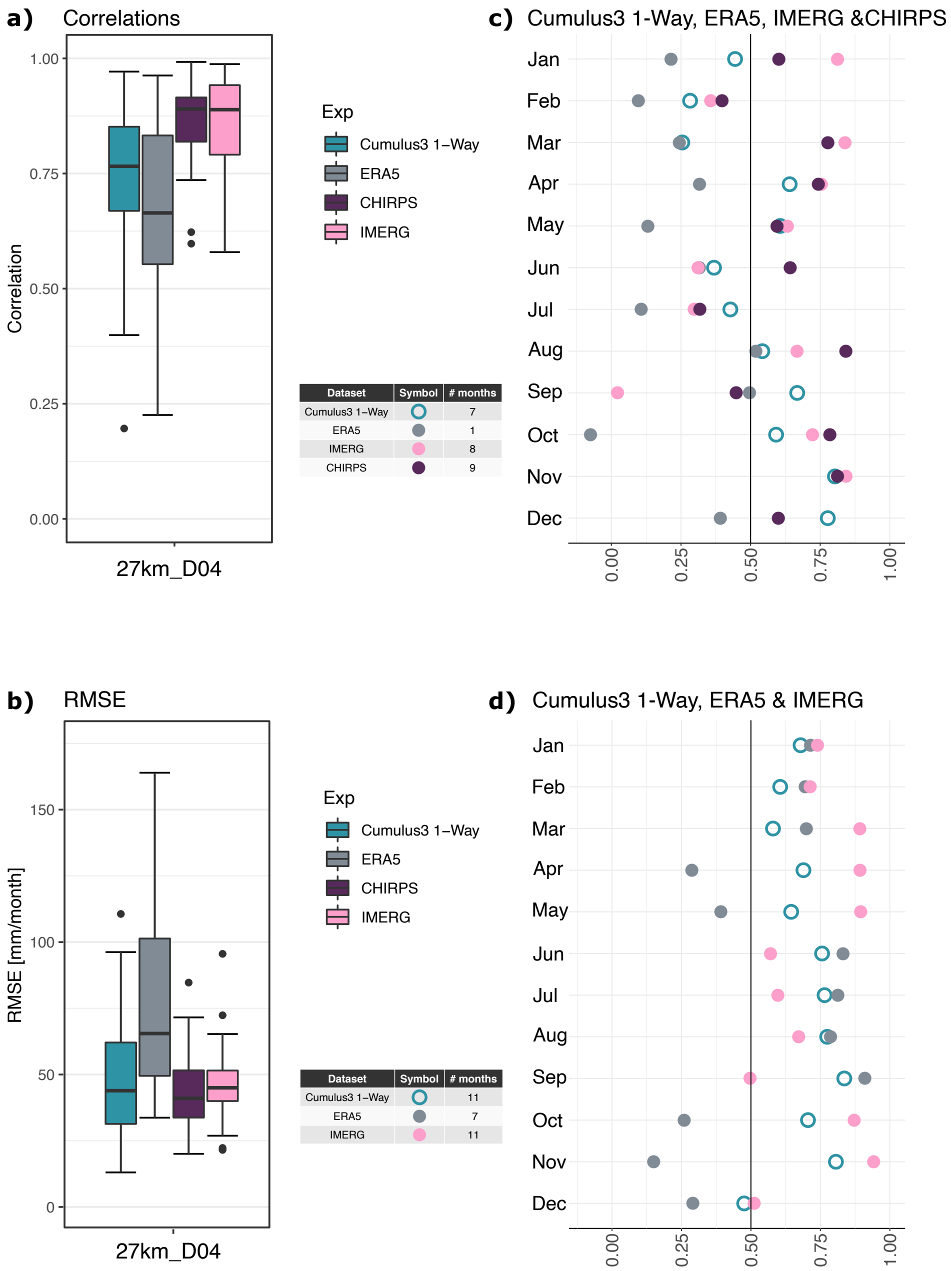

Figure R1.1: a) temporal correlations and b) root mean square error (RMSE) between the annual cycle of measured and simulated precipitation sums for 2006 at the nearest grid point to the station's location are shown for "Cumulus3 1-way", ERA5, CHIRPS and IMERG. The box and whisker plots show the values in relation to the stations included in the 1 km spatial resolution domain of the 27km_D04 setup. The whiskers extend to the value that is no more than 1.5 times the inter-quartile range away from the box. The values outside this range are defined as outliers and are plotted with dots. Pattern correlation of monthly precipitation sums between c) weather station data and the gridded data and d) between CHIRPS and the gridded data (interpolated onto the CHIRPS grid). The gridded data include ERA5, IMERG, CHIRPS and the WRF simulation "Cumulus3 1-way" for the year 2006. The labelling of the symbols is given in the table left of the two panels, along with the number of months (# months) in which the nesting option obtains correlation patterns above the reference value of 0.50 (a moderate correlation used to visually evaluate the performance of nesting options).

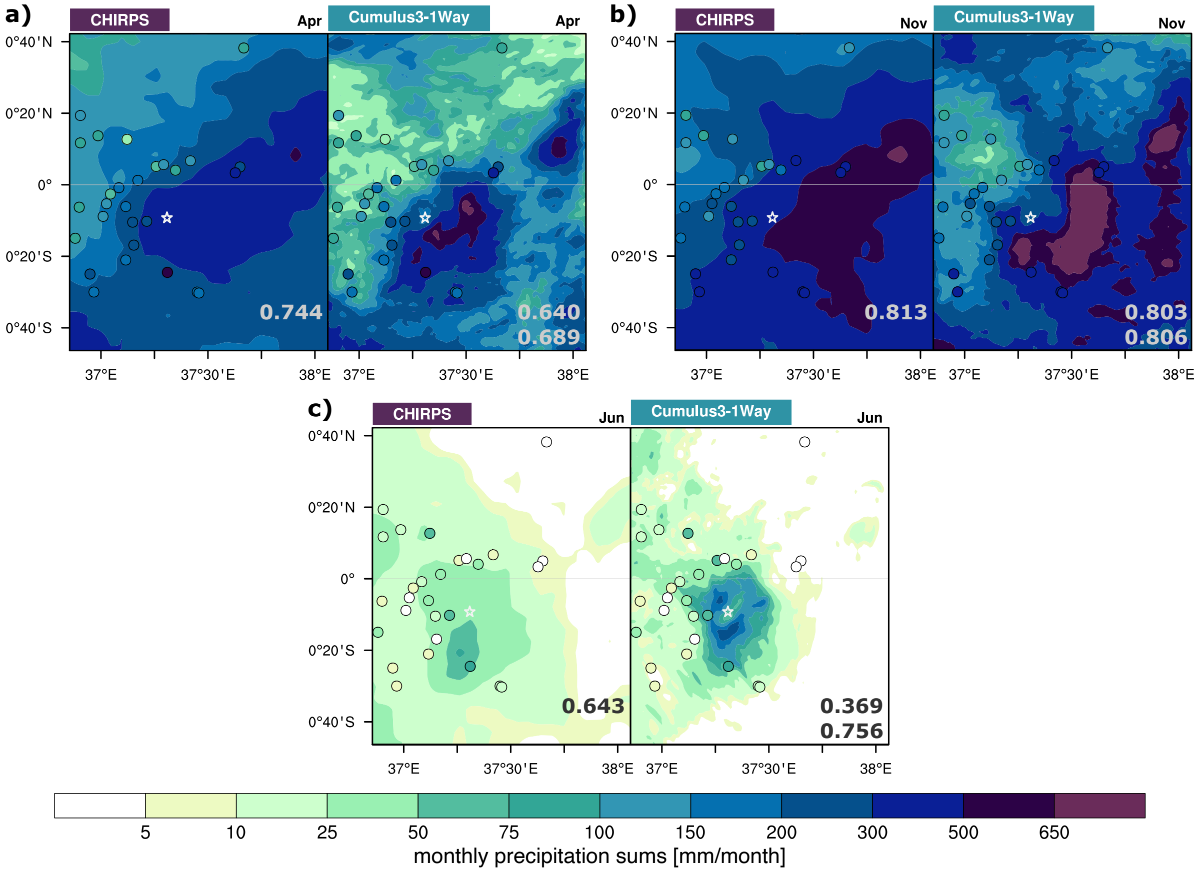

Figure R1.2: Monthly precipitation sums for a) April 2006 ('long rains'), b) November 2006 ('short rains') and c) June 2006 (dry season) in mm for the 1 km spatial resolution domain of the 27km_D04 setup. Weather station data are described in Table 2 of the manuscript. The white star indicates the summit of Mount Kenya.

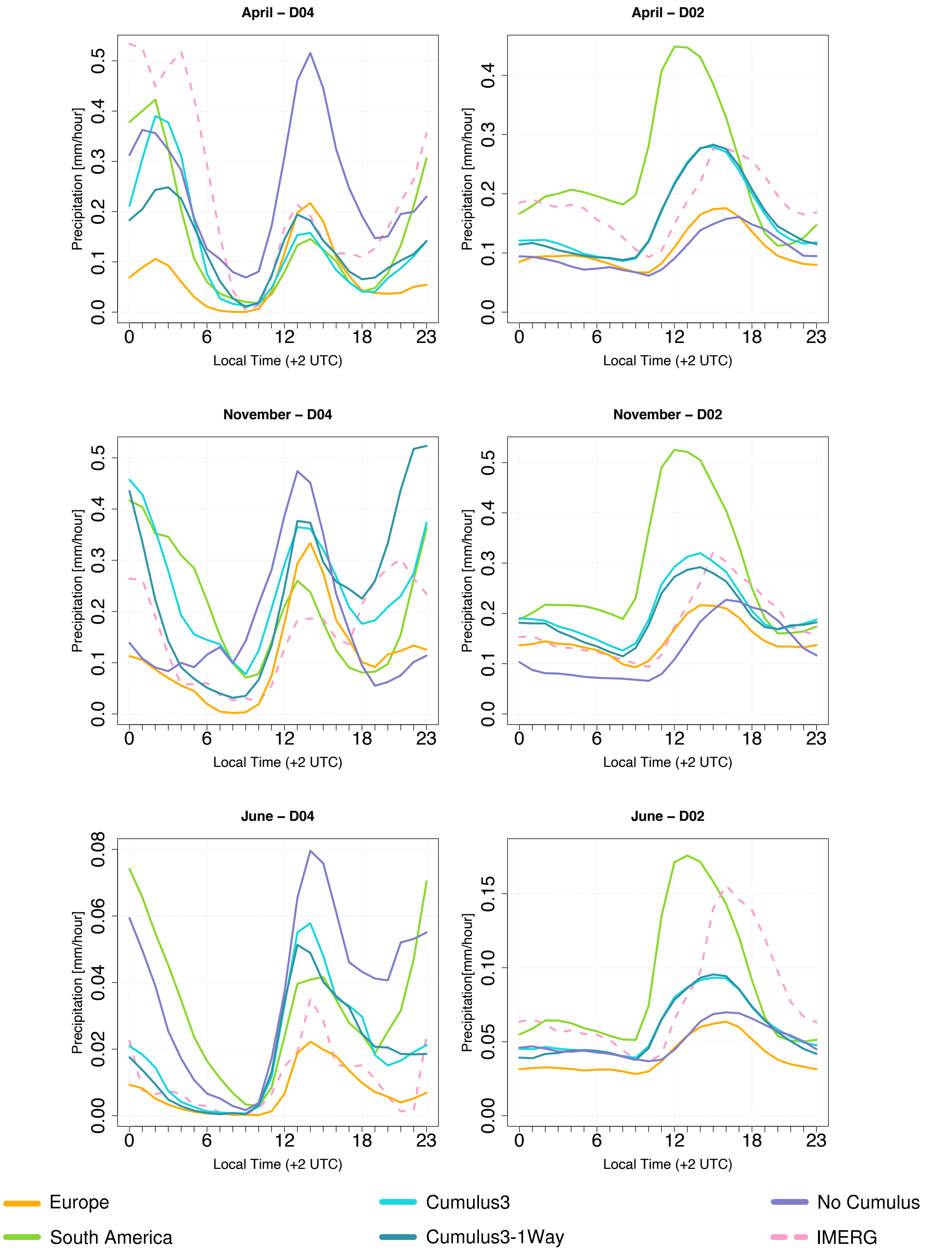

Figure R1.3: Daily cycle of areal mean precipitation in mm for April, November and June of 2008. The 1 km spatial resolution domain (D4, left column) and the 9 km spatial resolution domain (D2, right column) of the 27km_D4 setup are employed. The local time in Kenya (+2 UTC) is used in the time axis.

Point-to-point response to anonymous referee #2

General comments:

Messmer et al. present sensitivity simulations with a convection-permitting configuration of the atmospheric model WRF over Mt Kenya. The authors evaluate the impact of several parameterization collections and nest configurations (total number, ratios and interactions), with a focus on monthly total precipitation and monthly mean near-surface air temperature, for a study period of the year 2008. The presented work is intended to determine an optimal configuration for climate simulations over this region. Applying WRF at kilometer-scale grid spacing over Mt Kenya is a novel contribution, and an improved understanding of current and future variability in precipitation and water resources in this region is of high societal relevance. However, I have a number of concerns about the presented simulations and the analysis that need to be addressed before I can support the publication of the manuscript.

Thank you for going through our manuscript so carefully and for taking the time to review it. Thank you for providing insightful comments which improve the quality of our paper.

Regarding the simulations:

1. *The authors are using WRF V3.8.1, a version of the model that is four years old. The current version is 4.2.1 and numerous code improvements and bug fixes have been issued in the meantime. A brief look at the reported changes shows a major update to the Grell-Freitas scheme and bug fixes for Noah-MP (V3.9; https://www2.mmm.ucar.edu/wrf/users/wrfv3.9/updates-3.9.html) as well as updates to WSM6 (V4.1; https://github.com/wrf-model/WRF/releases). Although ongoing code developments are relevant for all published WRF studies, the older model version employed here may limit the current applicability of the presented results. I suggest that the authors perform an additional simulation of their best-performing configuration (identified as Cumulus3 1-way) with the latest version of WRF to assess the potential impact of model version on their conclusions.*

   The used version V3.8.1 is a stable and widely used version of WRF. Since we have started first simulations more than two years ago, we have decided to stick to the same version, in order to keep some level of consistency throughout the course of the experiment. Additionally, since the model is updated quite frequently (every year there is a major update, while there are new subversions on an approximately 1 to 3-monthly basis), it is unusual to work with the most up to date version. Especially because this version is also subject to errors and instability, as there is already a new version 4.2.2. available (https://github.com/wrf-model/WRF/releases).

   Nevertheless, we have run a simulation with the optimal setting using the most recent WRF version 4.2.1. The currently used WRF version 3.8.1 was compiled with Intel, but as the cumulus parameterization 3 (Grell-Freitas) is not running with an optimized compilation on our high-performance computer, we had to use a different compiler: PGI. Consequently, it must be kept in mind that small changes might be introduced because of differences in the numeric solution of WRF when using another compiler.

   The temporal correlation and root mean square error (RMSE) between the annual cycle of measured and simulated monthly precipitation sums at the nearest grid point to the station's location are shown in Fig. R2.1. This new figure includes the new simulation run following the suggestions of the reviewer. In this case, we will only compare the experiment V4.2.1 (light green) against the experiments already included in the manuscript and the observational datasets. The correlations obtained by this new experiment are similar to those obtained by the sensitivity experiments of the manuscript. However, the spread of the values is larger than for both "Cumulus 3" options. Focusing on the RMSE the values are larger than the ones obtained by the other experiments, and also the driving reanalysis

ERA5. Consequently, the improvements included in the new version of WRF are not enough to reduce the RMSE and to improve the temporal correlation against the weather station data in the area around Mount Kenya.

Figure R2.2 shows the pattern correlation of monthly precipitation sums between CHIRPS and the new WRF experiments compared to the pattern correlations of “Cumulus3 1-way” option. Focusing only on the comparison between V4.2.1 and “Cumulus3 1-way” (right panel), the pattern correlations are located above the reference value of 0.50 in 10 and 9 months respectively. However, it must be pointed out that this difference in the number of months is simply because the pattern correlation obtained by “Cumulus3 1-way” in November is 0.497 and not 0.5 to be above that threshold. The pattern correlations are rather similar between both experiments, but some differences are observed in specific months: The pattern correlation is worsened by the V4.2.1 experiment in January, April and July, while it is improved in May. Generally, the differences are rather small, and no systematic improvement is found, except for the fact that an overestimation in precipitation around Mount Kenya is evident when using the V4.2.1 (not shown). These results do not justify a complete change to a new model version.

2. *It appears that all of the configurations with cumulus parameterizations (CPs) used these schemes in the 3- and 1-km grid spacing domains. Conversely, the No Cumulus configuration did not use a CP in any domain, including the 27- and 25-km grid spacing ones. The established approach is to explicitly resolve convection when grid spacings are less than a few kilometers (e.g., Weisman et al., 1997). Although recent work shows that it might be appropriate to neglect CPs at coarser grid spacings than previously thought (Vergara-Temprado et al., 2020), excluding a CP in D4 could mean that convective processes are inadequately resolved (as the authors propose at line381). Overall, the authors need to provide a better justification for the CP settings in their tested configurations and, ideally, perform sensitivity simulations to illustrate the impact of (not) using a CP at 3- & 1-km (27- & 25-km) grid spacing.*

   It seems that there is some misunderstanding, and we have realized that Table 1 in our manuscript is imprecise. Hence, we have updated Table 1 and the text in the new version of the manuscript, so that it is clear that we do not use CP in domains that have a grid spacing of 5 km or less In detail this means that we do not mention a cumulus parameterization in Table 1 if the coarsest domain has a resolution of 5 km and we point out domains that make use of a cumulus parameterization in bold letters. This scale is convection permitting and hence, no parameterization is needed here, as the reviewer points out correctly. For the larger domains it is generally suggested to use a cumulus parameterization and here we already perform various experiments with different schemes and also turning it off as suggested by Vergara-Temprado et al. (2020). Therefore, we did not perform further sensitivity studies on the effect of cumulus parameterizations.

   We have included a clearer motivation in the new version of the manuscript, explaining why we turn off or make use of a cumulus parameterization in some sensitivity experiments.

3. *The finest resolution domain in the four-domain configuration is placed upstream of the main regional circulation systems, close to the lateral boundaries of its parent domain. It also appears to be more limited in extent than in the three-domain configuration. Both of these differences will impact the development of small-scale features. Furthermore, the experimental set up does not isolate the impact of the number of nests from the effects of the 1-km domain extent and proximity to boundaries.*

   The domain is more than 50 grid points away from the east boundary of D3 and more than 30 grid points from the northern edge of D3. This is much further away than the 5 to 10 grid points around the boundaries that are needed for the relaxation of the outer boundary conditions (Rummukainen, 2010). Hence, we do not expect any problems from the boundaries. Furthermore, as the grid spacing is already very fine, it is expected that the two domains have a rather similar solution as several processes are explicitly resolved.

In Figure R2.3 a zoom in into Fig. 1 of the manuscript is presented, as in Fig. 1 of the manuscript the domain borders are rather thick, so that they are well visible. Figure R2.3 focuses only on the D3 and D4 (3 and 1 km horizontal resolution, respectively). It allows to see that the boarders of D3 and D4 are well separated from each other.

A few additional comments on the numerical simulations are provided in the specific comments below.

Regarding the manuscript:

4. *The introduction is missing some literature (see specific comments). The analysis would benefit from considering higher temporal resolution data (only monthly totals or means are examined) as well as an enhanced focus on process understanding to provide more insight into the differences between configurations. For example, the differences in near-surface air temperature are attributed to precipitation differences without presenting any supporting analysis of, for example, simulated soil moisture, the latent heat flux, cloudiness, or net radiation. Finally, there is minimal discussion, including of any caveats that might impact the reliability of the results and conclusions (e.g., the sparsity of observations to the southeast of Mt Kenya and above 3048m, the choice of simulation year, etc).*

   We have expanded the introduction of the new version of the manuscript with the suggested literature. Further, we have also included a more extended discussion on the reliability of the data because of considering only 2008, or due to the reduced number of weather station data in Mount Kenya region.

   As already suggested by the reviewer 1, we have studied the daily cycle of each experiment for each month. To do so, the areal mean hourly precipitation was calculated over the entire D4 for each sensitivity experiment and IMERG. According to Figure R2.4 (same as Figure R1.3 in the reply to reviewer 1), most of the experiments follow similar daily cycles and especially the “No Cumulus” shows too much precipitation in the afternoon. We have included the results of the sub-daily analysis for D2 in the manuscript and have added Fig. R2.4 in the supplementary material.

   We have also repeated the temporal analysis of our study, but with finer resolutions including 10 days, pentads (5 days) or daily values. However, it must be kept in mind that some of the stations included in Schmocker et al. (2016) are only suitable for monthly analysis and not for finer temporal resolutions. Thus, 28 stations are included in the monthly analysis and only 21 in the finer resolutions, and consequently small differences in the distribution of the experiments between monthly and the other temporal resolutions are observed. Figure R2.5 shows the temporal correlation between the annual cycle of measured and simulated precipitation sums at the nearest grid point to the station’s location, and as expected, the correlations are reduced with increased temporal resolution. In any case, the distribution of the experiment is maintained, and the gridded datasets are always the ones obtaining the largest values. In most of the cases, “No Cumulus” and “Europe” obtain the poorest results.

   If we focus on RMSE (Figure R2.6), the errors are smaller for the coarsest temporal resolution and increase with finer resolutions. This is concordance with the behaviour of the correlations. This is again expected due to the higher variability of the high frequency data.

   We think that these figures should not be included in the manuscript, but the robustness of our results under finer temporal resolutions is commented at the end of Section 3.1.1 in the new version of the manuscript.

Regarding the temperature changes in the WRF simulations and the discussion thereof; we have investigated latent heat flux, cloud fraction, and soil moisture and they all confirm the main pattern that an increase in precipitation amount leads to an increase in soil moisture and latent heat flux. Around the summit of Mount Kenya, the signal is not always easy to interpret, which is probably related to cloud effects. As cloud effects are rather complex and the effect on temperature strongly depends on the altitude and daytime, it would need a study on its own to identify the exact processes that lead to changes in temperature. Hence, we have decided to weaken our statements in the paper, and we do not show any of these variables in the new version of the manuscript.

Specific comments:

1. *Line 27-28: The discussion of the impact of the Walker circulation changes on in-terannual precipitation variability should cite at least one of Stefan Hastenrath's papers on this topic (for example, Hastenrath and Polzin, 2004, 2005).*

   Thank you for pointing this out, we have included this in the new version of the manuscript.

2. *Line 38: the long rains are also associated with flooding and drought events (e.g., Kilavi et al., 2018)*.

   We have modified that statement in the new version of the manuscript to be more precise.

3. *Lines 39-42: the introduction makes no mention of the Indian Ocean Zonal Mode and its significant impact on moisture variability in East Africa (e.g., Behera et al., 2006; Nicholson, 2015; Saji et al., 1999; Ummenhofer et al., 2009).*

   We have included these studies in the new version of the manuscript.

4. *Lines 46-48: Wainwright et al., (2019) is relevant to the discussion of the long rains. Please, clarify what is meant by "downward trend".*

   We have clarified it in the new version of the manuscript.

5. *Line 55: Are the authors referring specifically to climate simulations?*

   Yes, we do. We have adapted it in the new version of the manuscript.

6. *Line 70: Collier et al., (2018) also performed a decadal simulation with WRF at convection-permitting resolutions in East Africa.*

   We have included this literature in our introduction as well.

7. *Line 71: The authors should clarify in this sentence already that it is not only the scale but the ability to neglect a cumulus parameterization that has an impact.*

   We have made it clear in the new version of the manuscript as suggested by the reviewer.

8. *Line 107: To be pedantic, WRF is an atmospheric model that can be used for many applications, including regional climate modelling. Please rephrase.*

   We have rephrased it to: We adopt the numerical weather prediction model WRF.

9. *Section 2.1: the details provided on the WRF configurations are insufficient to re-produce the study results. Please provide additional details, in Table 1 or elsewhere, including the grid dimensions, selected surface layer scheme, the moisture trigger used with the KF parameterization, diffusion option, and upper boundary condition. Ideally, sample namelists would be made available to interested readers.*

We have uploaded our namelist files to Zenodo, so that they are available for anybody. The fact that our namelist files can be found on Zenodo is indicated in the caption of Table 1.

10. *Line 116: Please provide information on the length and computational expense of the simulations.*

All the experiments used up around 750’000 CPU hours and they fill up around 140 TB of storage space. As the computational expenses are rather system dependent, we do not consider this information important for the publication and hence, we have added it to the acknowledgements.

11. *Line 127: The model top of 50 hPa is low for climate simulations (as per WRF developer recommendations and previous studies), especially over mountainous terrain.*

50 hPa is the standard best practice p_top value for the WRF simulation (https://www2.mmm.ucar.edu/wrf/users/namelist_best_prac_wrf.html). Additionally, it is one of the most widely used value also in recent studies over complex terrain, e.g., over Europe (e.g., Dörenkämper et al., 2020) and this level also depends on the available levels from the input data, i.e., ERA5 or CESM for our later studies.

12. *Line 130: Please provide the permitted timestep range for each outer grid resolution (or a general relation, e.g., from 3* to 8*dx). Also, did the simulations employ restarts? If yes, were they reproducible with the adaptive timestep?*

The range of the dt for the outermost domains was already provided in the previous version of the manuscript (L129-131 in the old version; L141-142 in the new version). If the reader needs more details about the exact setting, she/he can refer to the namelist files of each experiment, which are uploaded to Zenodo (https://doi.org/10.5281/zenodo.4090589).

We do use restart files and we have tested if the restart files are reproducible. For this we have used a restart file at the end of March, which is within the long rains and therefore convective processes occur more often. This is also the time where the model has to adjust dt more often in order to comply with the CFL criterion. We have run the simulation twice with the same initial and boundary conditions using the restart file for March. In Figure R2.7 you can find the dt that has been chosen by WRF (x-axis) for domain 1 and each timestep of a 7-day simulation (y-axis). The first two panels show the dt for the two runs and the last panel, shows the difference between the two runs. This difference is zero at every timestep and hence, it can be concluded that these restart files and the simulation are reproducible. Also, the two newly created restart files of the two experiments are absolutely identical (not shown).

We have added a sentence concerning the reproducibility of the simulation with adaptive time steps in Section 2.1.

13. *Line 145: Whether or not using the lake model improves regional precipitation in the presented simulations could be tested explicitly.*

We have run a new simulation with the optimal setting (“Cumulus3 1-way”), where the lake model is turned off. The correlations and RMSEs (Figure R2.1) obtained by the “Cumulus3 - No Lake” (red box) experiment are similar to those obtained by the “Cumulus3 1-way” (lake model is on) experiment. Both are in the range of the other sensitivity experiments. Consequently, the effect of using the lake model in the simulation is slightly better regarding the temporal evolution of precipitation.

Additionally, the spatial precipitation patterns of these two experiments (with and without lake model) are correlated for each month against CHIRPS (Figure R2.2, centre panel). Both experiments exceed the threshold of 0.5 in 9 months and generally the “Cumulus3 1-

way” experiment obtains slightly better pattern correlations. A remarkable difference can be observed in November (‘short rains’) where the experiment with the lake model obtains a clearly better pattern correlation compared to a turned-off lake model. Generally, the simulation without the lake model results in too little precipitation amounts (not shown).

Thus, the use of the lake model should be recommended for the study area. We believe that these figures should not be included in the new version of the manuscript, but the fact that a lake model is beneficial for precipitation around Mount Kenya is added to section 2.1 in the manuscript.

14. *Are the authors using the default land use and terrain datasets as input? How representative are these datasets of conditions on and around Mt. Kenya, including the forest belts and grasslands on the slopes? Mölg and Kaser (2011) reported improved high-elevation simulations with WRF over Kilimanjaro using updated land use datasets, and both updated land use and terrain datasets have been employed in recent WRF studies to better represent surface conditions (e.g., Collier et al., 2018, 2019).*

    It is true that there are other published studies where updated land use data is used. However, we decided to use the default data provided within the model, because in a next step of our study, we plan to run different climatologies for the region with different land use options. In order to identify the effect of changed land use, we first need a base state to compare it to.

15. *Line 142: Can the authors also provide which dveg option they are using with Noah-MP? There are issues with certain options for domains containing both hemispheres that could have a significant impact on the results: https://github.com/wrf-model/WRF/issues/707.*

    We have used the default option (dveg = 4) and we are also aware of the problem along the equator. However, when we started the first experiments, no solution for this issue was available except changing to another land surface model, something that we did not want to do. The chosen option can be inspected in the namelist file on Zenodo and hence we do not provide it explicitly in the text.

    We have run a new simulation with the optimal setting and option dveg = 9, to estimate how significant the impact on precipitation is. The correlations and RMSEs of the “Cumulus3 - dveg9” experiment (Figure R2.1, blue box) show similar distributions to those of “Cumulus3 1-way”. This is also true for most of the pattern correlations (Figure R2.2, left panel), where the differences between the two experiments are rather small except for April and December. In these two months, the default option performs better than the dveg9 option. It can be observed that the “Cumulus3 - dveg9” experiment exceeds the 0.5 threshold in 10 months and “Cumulus3 1-way” in 9. However, it must be pointed out that the pattern correlation of November for “Cumulus3 1-way” is 0.497 and for “Cumulus3 - dveg9” it is 0.518. There is a small change in temperature observable, as expected between the two hemispheres, but it is around 0.25-0.5 degrees in the monthly means. To conclude, the effect of the dveg option is rather small and hence, we do not further discuss this in the manuscript.

16. *Line 168: Why were these pressure levels (and not all available levels) between 1000 and 700 hPa selected? Does this choice significantly impact the simulations?*

    The plan is to use this model setup with input from a climate model, where we also have to interpolate different pressure levels. It is not meaningful to just interpolate various levels from a limited number of sigma-pressure levels. Our intention is to have the model run as freely as possible, as we will need this for the climate simulations. Also, other climate models from, e.g., CMIP5 and 6 do not provide this many pressure levels. Hence, the selected levels are sufficient to drive a regional climate model.

17. *Line 170 & Figure 2: the data need to be detrended, if significant trends are present, to examine anomalies. The months of April and May 2008 look very anomalously dry compared with the whole period, and the impact of choosing [only] 2008 as the study period on the results and conclusions needs to be discussed. Also, since the precipitation field in ERA5 is highlighted as being unreliable, would it be preferable to consider CHIRPS data for precipitation in Figure 2?*

    Thank you for this comment. We have investigated the data in more detail, and we think that detrending the precipitation over Kenya is not useful, as there is hardly any linear trend. We have added the detrended figure for temperature and precipitation below (Figure R2.8). For temperature a detrending is more useful, as there is a clear trend observable. Nevertheless, we would like to show the climate as it is. The evolution of the climatology (not detrended) helps to further justify our selection of the year 2008, as stated in the manuscript, because it seems to be a rather standard year in the recent climate (visible in the detrended Figure R2.8) below.

    Regarding CHIRPS: we have considered your suggestion and have decided to include the CHIRPS data rather than ERA5 for precipitation, as the results are slightly different, and CHIRPS is considered to be the more reliable data set. Also, the boxplots in Fig. 2c are now plotted with the CHIRPS data, and April and May are now a bit less extreme than with ERA5. In Fig. 2c of the new version of the manuscript you can see that we have included the results for the year 2006 as well. This year is now also included in the manuscript as suggested by the reviewer 1, to obtain more robust results. 2006 has a comparably wet April and a regular May, but WRF still performs well. The analysis highlights that the "Cumulus3 1-way" experiment shows for the year 2006 similar results to those presented in the manuscript. Consequently, this configuration shows a robust behaviour independent of the climatic conditions of the studied year. The figures of this analysis are included in the supplementary part of the new version of the manuscript and the results related to the year 2006 are added to the Section 3.1 of the new version of the manuscript.

18. *Line 178: Data should be interpolated from higher to lower spatial resolution, as interpolation does not add physically meaningful detail.*

    Depending on the data that we use as a reference in the manuscript, different approaches are followed with the datasets. For the temporal correlations and the pattern correlations against weather station data, the gridded datasets are bilinearly interpolated to the corresponding WRF grid, and then, the values of the nearest points to the station are extracted. For the pattern correlations against the gridded observational dataset CHIRPS, all WRF domains are bilinearly interpolated to that grid, as it is the reference. To have all the monthly precipitation and temperature patterns on the same grid, Fig. 6-7 (numbering of the new version of the manuscript) use the WRF grid, meaning that the gridded observational data sets are interpolated to the innermost domain of WRF. This is not clear enough in the manuscript, and we have clarified this in the new version of the manuscript.

19. *Section 2.3.3: CHIRPS merges satellite and rain gauge data. Do the authors know if any of the weather station data been assimilated into this product?*

    As far as we know the observations gathered by CETRAD are independent from any other used station data.

20. *Lines 241-243: Please move this information to the methods and provide an estimate of statistical significance where applicable.*

    We believe that it is better to have the interpolation method directly before discussing the respective results, so that the reader immediately knows how the results are derived. Since it is only one or two sentences, we would like to keep it where it is right now.

The statistical significance of the results was not included in the current version as only 12 monthly values were used to calculate the temporal correlations. Also, for a sub-daily analysis on a monthly basis we have 30 values, which is still rather small to perform a reliable statistic test.

21. *With regards to the statistical methods, the authors do not state which correlation they use and if it is appropriate to have a sample size of 12. In addition, the data have not been de-seasonalized, which means that a relatively high correlation is to be expected unless WRF fails to capture the seasonal cycle, which should be acknowledged. For their analysis, I suggest that the authors consider a finer temporal resolution, such as pentads, to provide a more detailed and robust assessment of model skill.*

We have already presented in comment 4 the assessment of the temporal correlations using different temporal resolutions (see also Figure R2.5Figure R2.6). As shown there, the correlations are reduced and the RMSEs are increased with increased temporal resolution, but the skill of the experiments in relation to each other remains unchanged in most of the cases. However, it must be kept in mind that if we increase the temporal resolution of our results in the manuscript, some of the weather stations that we use from Schmocker et al. (2016) are not suitable for finer resolution than monthly, and consequently, the robustness of our study would be reduced.

Regarding the deseasonalization of the data, we think that this is not possible with only one year, as a longer period is needed to capture a typical seasonal cycle.

To calculate the correlations, we use the Spearman correlation, which is a rank correlation, suitable for small sample sizes. We have clarified this in the manuscript.

22. *Lines 327-328: where is this result visible?*

This result is observable in the insets of Figure 5. There, it can be seen that the annual precipitation sums for the Cumulus 3 options is below the one from ERA5. We have added a label towards Figure 5 in the text, to make it clearer.

23. *Lines 331-334: this is the first time that a justification is provided for using an outer domain with a grid spacing of ~25 km. I suggest moving this information to the methods, so the reader understands why the authors include this aspect earlier.*

This is a valid point. We have moved those lines as suggested in the new version of the manuscript.

24. *Figures 6 to 11: The choice of months could be more robustly justified. In addition, both June and November show low pattern correlations between the observations and the gridded datasets, which undermines using CHIRPS as the main comparison (lines 335-337). Overall, these figures take up a lot of space in the manuscript without providing a great deal of new information. Some suggestions would be to remove the panels for ERA5 and Europe (the authors have already established the poor representation of precipitation) or to replace some map figures with elevational profiles, for compactness and so that the reader can more clearly see some of the reported findings (e.g., lines 403 to 405).*

We have rephrased the justification of the selected months in the manuscript. And we have changed the figures according to the suggestion of the reviewer. We have removed "Europe" and all the gridded data sets except for CHIRPS in the precipitation fields. For temperature we have kept ERA5 but removed "Europe" and CRU as well. As ERA5 and CRU show very similar results, but ERA5 seems to correspond to the station data somewhat nicer, we decided to keep ERA5. Now, Figs. 6-8 are merged into Fig. 6 and Figs. 9-11 into Fig. 7.

25. *Lines 393-396: Please move this information to the methods.*

    We have moved this information to Section 2.3.5 as suggested.

26. *Line 459: The authors repeatedly mention that the underestimation of precipitation in the Europe configuration stems from the LW and PBL parameterizations – could they discuss what difference in process representation might be underlying the difference?*

    As suggested by the reviewer, we have reduced the number of panels in Fig. 6 to 11. This results in the fact that Europe is no longer presented and also the discussion related to it has been removed.

27. *Line 464-465: Can the authors please clarify what they mean about one-way vs two-way nesting?*

    In the two-way nesting option, the results from the nest overwrite parent domain results. In the one-way nesting, the results from the nests do not overwrite the data in the parent domain and independent results are obtained in each domain. We have made this difference clear at the end of section 2.1 in the new version of the manuscript.

Technical comments:

1. *Line 129: The sentence "This is, because..." is unnecessary, please remove.*
2. *Line 132: Please change "in order to improve" to "to optimize."*
3. *Line 164 to 167: The forcing variables are well known and documented, so I suggest deleting these sentences.*
4. *Lines 190 and 201: It is clear that data are only compared for the study period, please remove.*
5. *Lines 234 to 237 are repetitive and could be removed.*
6. *Line 341: CHIRPS is not a model, please rephrase.*
7. *Line 344: "Bearing"*
8. *Line 436-437: Please clarify that these parameterizations are not varied independently.*

We have addressed all these technical comments in the new version of the manuscript as suggested by the reviewer.

Figures:

Figure 1: I suggest adding the weather station locations to the plots of D3/D4, as the reader does not see where they are located before encountering Figure 6. Also, is Figure 1 referenced in the text?

We have included the observations that we use in our study in Figure 1 in D4 as suggested by the reviewer, and we have also added the missing reference to that figure in the text.

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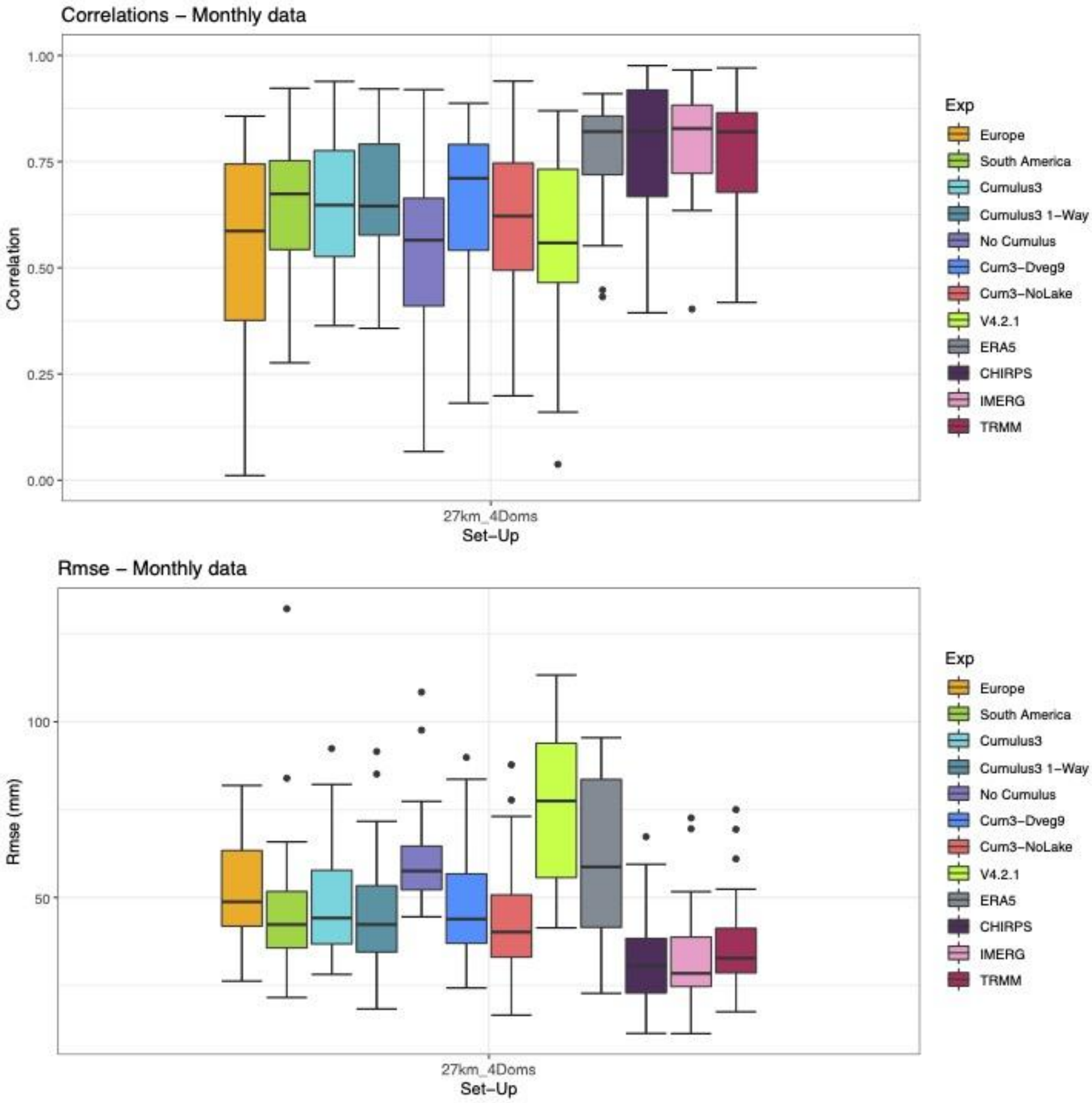
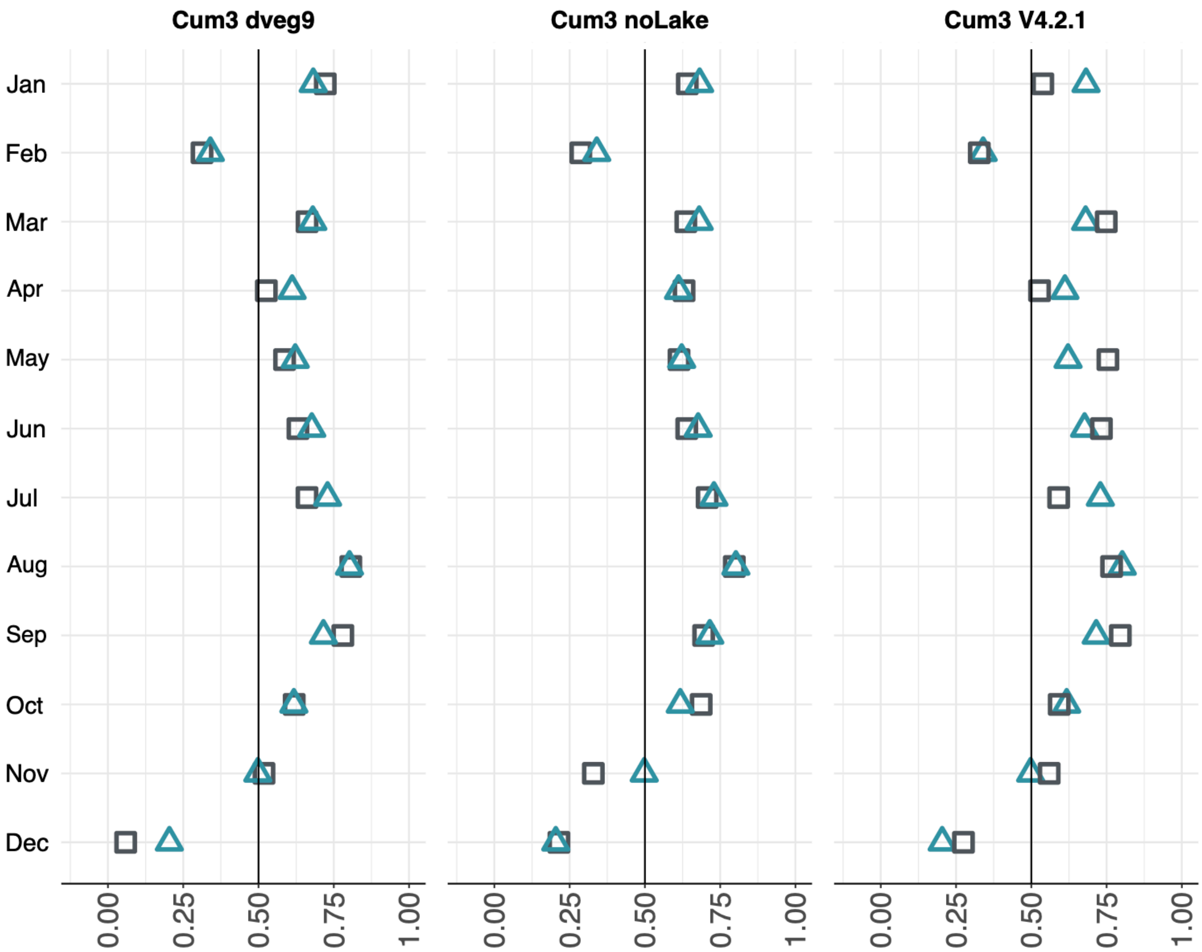


Figure R2.1: The temporal correlation (a) and root mean square error (RMSE) (b) between the annual cycle of measured and simulated monthly precipitation sums at the nearest grid point to the station's location are shown for the different parameterization options. The box and whisker plots show the values in relation to the stations included in the 1 km spatial resolution domain of the 27km_D04 setup. The whiskers extend to the value that is no more than 1.5 times the inter-quartile range away from the box. The values outside this range are defined as outliers and are plotted with dots.



Parameterization	Symbol	# months
Cum3 dveg9	□	10
Cumulus3 1–Way	△	9

Parameterization	Symbol	# months
Cum3 noLake	□	9
Cumulus3 1–Way	△	9

Parameterization	Symbol	# months
Cum3 V4.2.1	□	10
Cumulus3 1–Way	△	9

Figure R2.2: Pattern correlation of monthly precipitation sums between CHIRPS and the new WRF simulations compared to the pattern correlations of the "Cumulus3 1-way" option. The labelling of the symbols is given in the table below each panel, along with the number of months (# months) in which the nesting option obtains correlation patterns above the reference value of 0.50 (a moderate correlation used to visually evaluate the performance of nesting options). Only one setting is shown (27 km_D04).

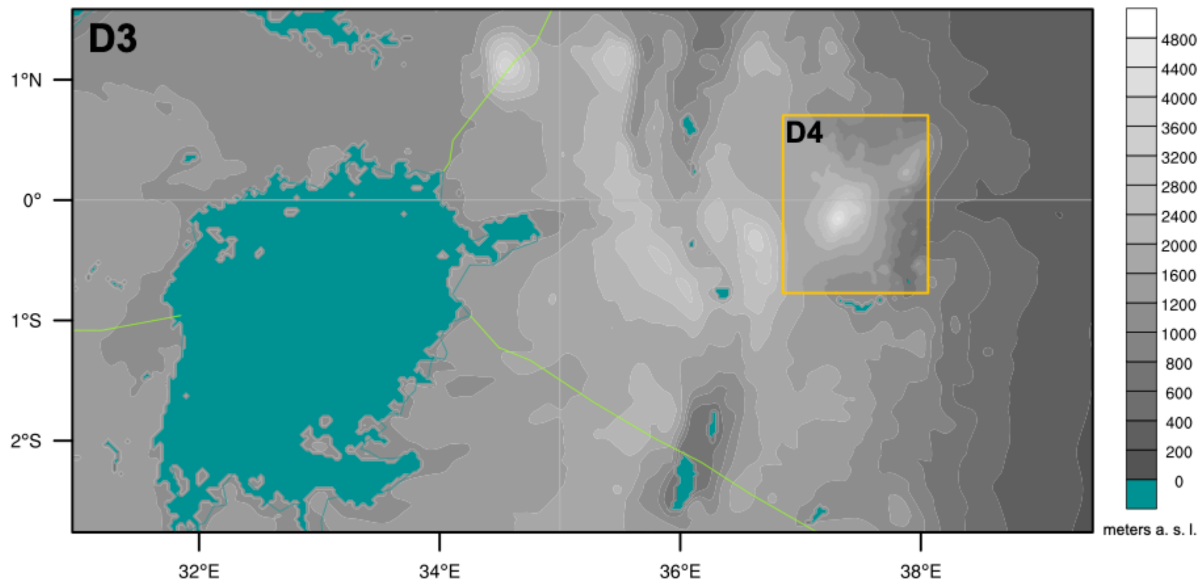


Figure R2.3: A zoomed in version of the D3 and D4 nests for the 1:3 nesting ratio setting.

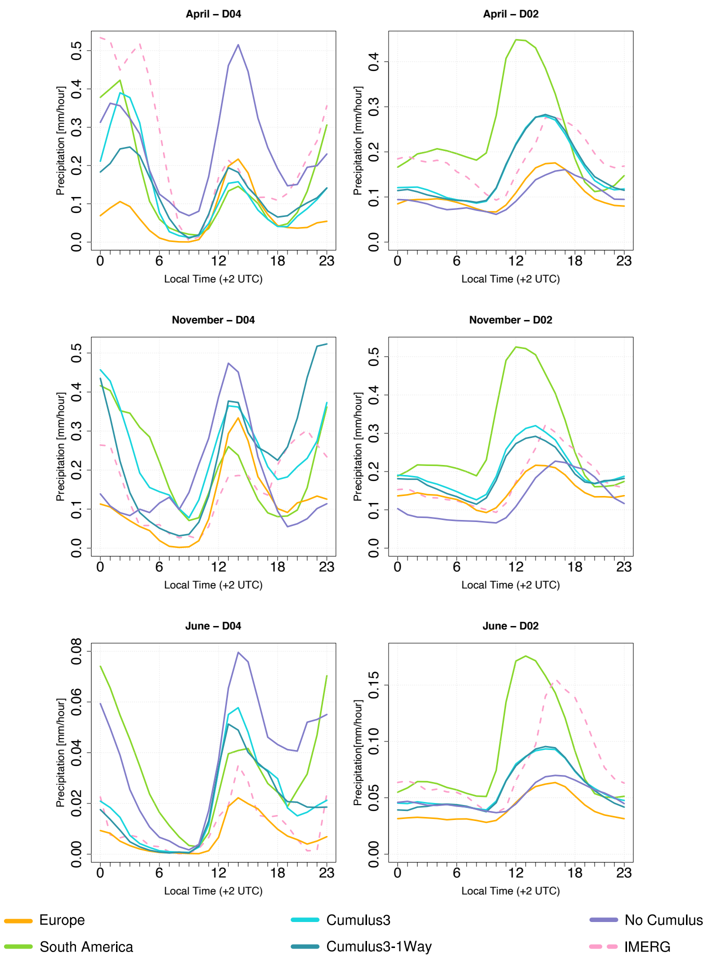


Figure R2.4: Daily cycle of areal mean precipitation in mm for April, November and June of 2008. The 1 km spatial resolution domain (D4, left column) and the 9 km spatial resolution domain (D2, right column) of the 27km_D4 setup are employed. The local time in Kenya (+2 UTC) is used in the time axis.

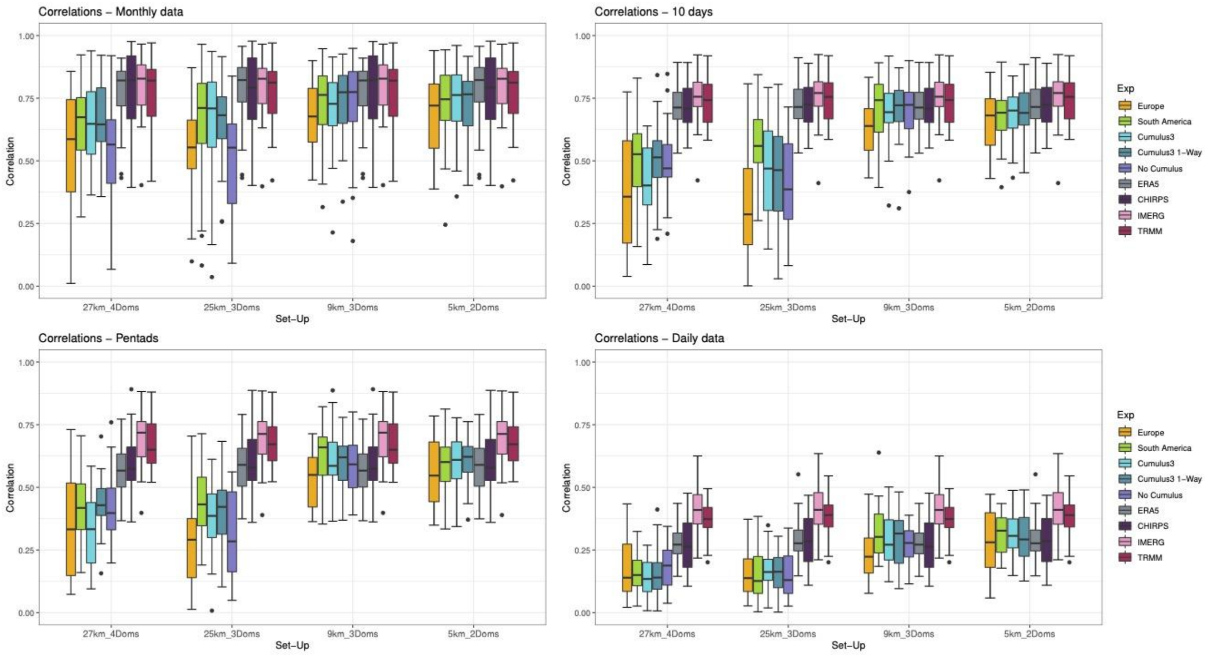


Figure R2.5: The temporal correlation between the annual cycle of measured and simulated precipitation sums at the nearest grid point to the station's location, grouped by the different temporal resolutions. The box and whisker plots show the values in relation to the stations included in the 1 km spatial resolution domain of the 27km_D04 setup. The whiskers extend to the value that is no more than 1.5 times the inter-quartile range away from the box. The values outside this range are defined as outliers and are plotted with dots.

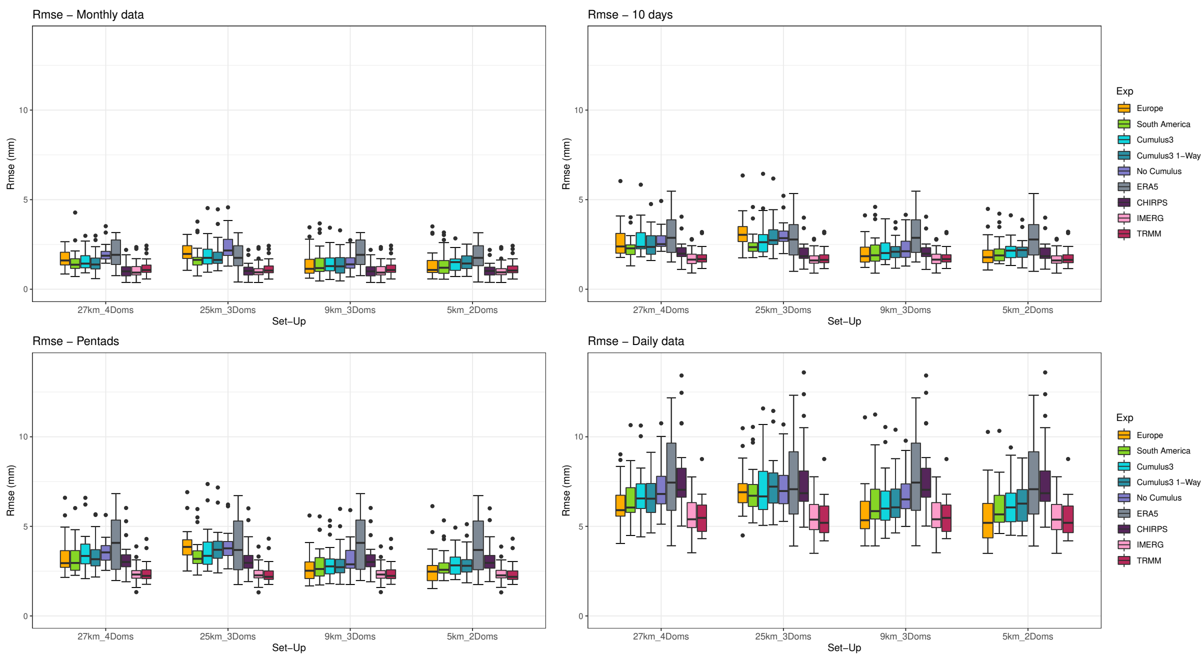


Figure R2.6: Same as Figure R2.5 but for RMSE. Note that RMSEs are given as daily sum values irrespective of the temporal resolution, in order to have consistent units between the different panels.

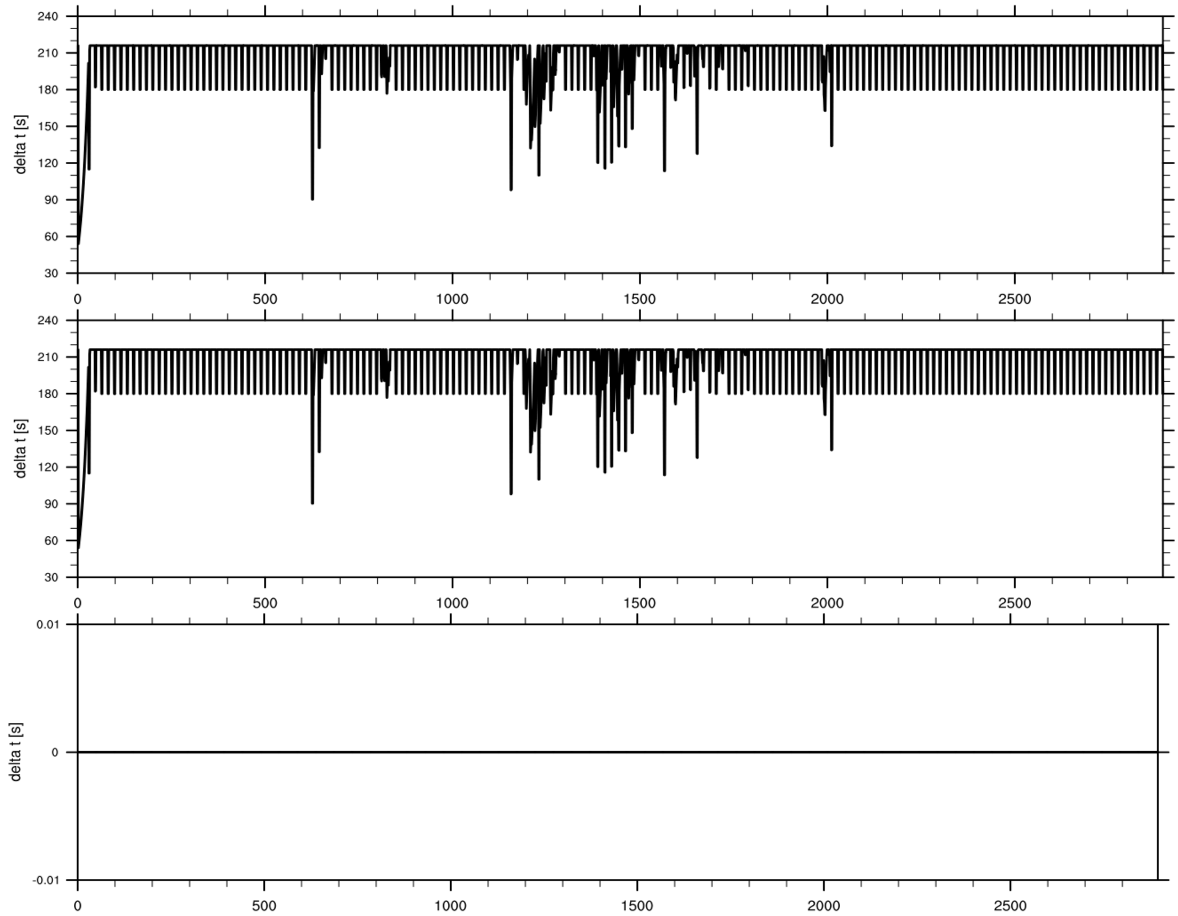


Figure R2.7: The two upper panels show the dt for each time step (x-axis) of a 7-day simulation, restarted on the 21st of March 2008. The last panel shows the difference between the upper two panels.

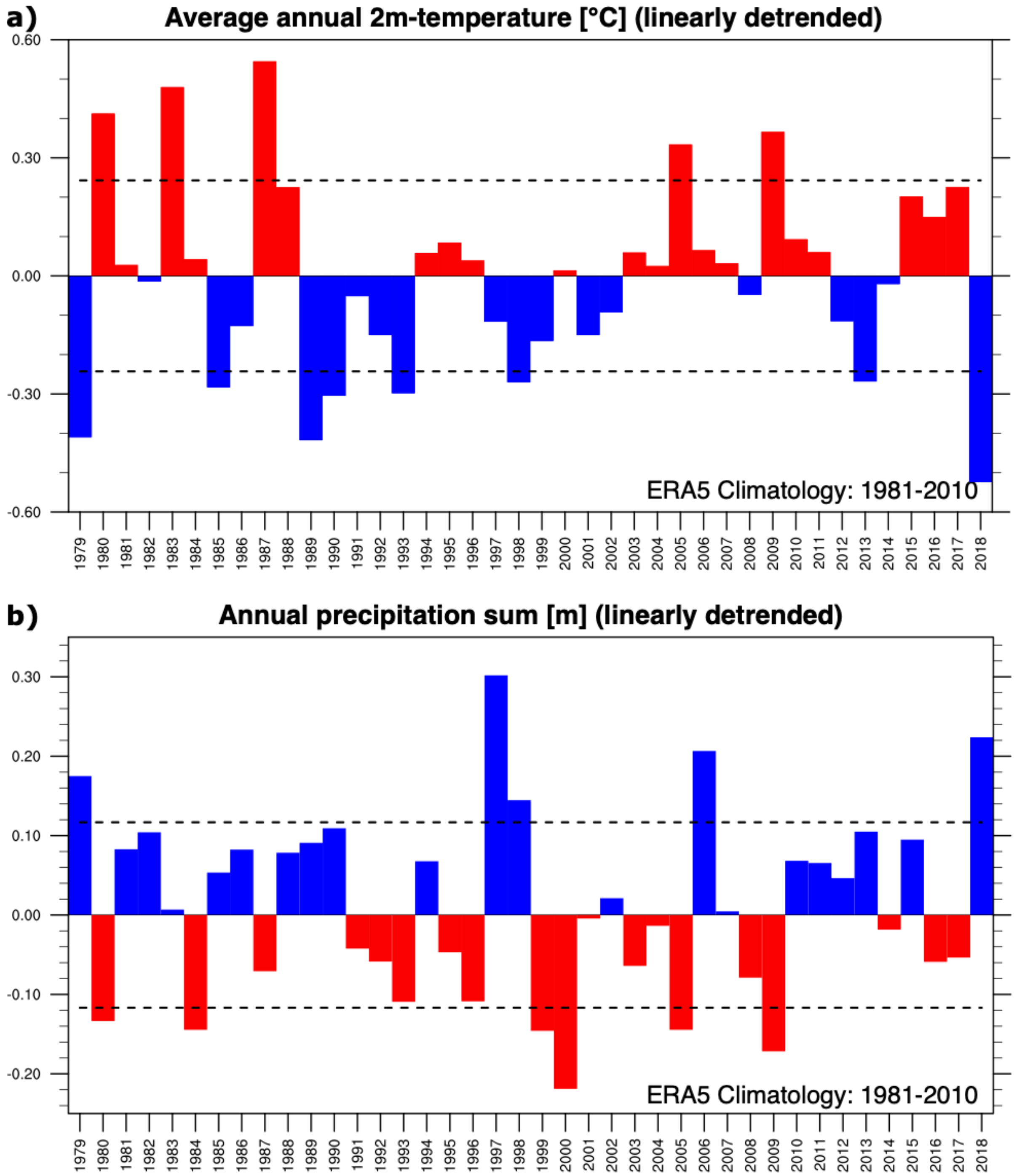


Figure R2.8: a) Annual anomalies of mean 2m-temperature (in °C) and b) of precipitation (in m) from ERA5. The anomalies are calculated using detrended annual values and with respect to the detrended climatological mean of the years 1981 to 2010. The stippled lines illustrate plus and minus one standard deviation.