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Interactive comment on "Sensitivity of precipitation and temperature over Mount Kenya area to physics parameterization options in a high-resolution model simulation performed with WRFV3.8.1" *by* Martina Messmer et al.

Anonymous Referee #2

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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.

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 (v.3.9; https://www2.mmm.ucar.edu/wrf/users/wrfv3.9/updates-3.9.html) as well as updates to WSM6 (v. 4.1; 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.

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 line 381). 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.

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.

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).

Specific comments:

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

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

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).

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”.

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

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

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.

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

9. Section 2.1: the details provided on the WRF configurations are insufficient to reproduce 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.

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

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.

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?

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

14. Are the authors using the default landuse 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 landuse datasets, and both updated landuse and terrain datasets have been employed in recent WRF studies to better represent surface conditions (e.g., Collier et al., 2018, 2019).

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.

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?

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?

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

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?

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

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.

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

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

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 335âĂŤ337). 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).

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

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?

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

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.

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?

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