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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The behavior of high-CAPE summer convection in large-domain large-eddy simulations with ICON" *by* Harald Rybka et al.

Anonymous Referee #2

Received and published: 1 October 2020

Review of "The behavior of high-CAPE summer convection in large-domain large-eddy simulations with ICON" by Harald Rybka, Ulrike Burkhardt, Martin Köhler, Ioanna Arka, Luca Bugliaro, Ulrich Görsdorf, Ákos Horváth, Catrin I. Meyer, Jens Reichardt, Axel Seifert, and Johan Strandgren

Summary of paper:

The authors present results from 3 simulations of high-CAPE convective weather events over Germany with the ICON model at sub-km grid lengths. The simulations are evaluated in terms of ice cloud cover, ice water contents, and ice water paths against a range of ground-based and satellite-based retrievals. The model compares

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well against the retrievals, although ice contents are vastly overestimated when graupel is included. Also, anvil cloud coverage and lifetime are overestimated. Sensitivity studies show greater sensitivity to the driving model (particularly in terms of the timing of initiation of convection) than to the microphysics parameters.

Review summary:

This is a very well-written paper with high-quality figures. While the paper is rather long (the introduction is already 3 pages), it is necessarily so due to its attempt to disentangle the sensitivity of the simulation due to the driving model from the sensitivity due to microphysics assumptions. The paper covers this comprehensively so and it is not obvious that its length (or number of figures) could be reduced significantly without losing useful results or discussion. There are no obvious flaws or concerns about the methodology or interpretation of the results and the only "major" comment relates to a request for further context. Given the quality of the presentation of the manuscript and the nature of the comments below, the recommendation is to accept with minor revisions.

Major comment:

This is only major in the sense that it requires a small amount of work, but given the importance of CAPE for this study it would provide the reader with further confidence in the simulations if the authors presented a brief evaluation of the thermodynamic profiles for the three cases. Of course, such observed profiles may only be available at specific times and will require some cherry-picking of locations and times in relation to the convective activity. Nevertheless, the thermodynamic profiles would provide adequate context of potential model biases in (1) cloud top height and (2) timing of convection. Both of these are of interest in the sensitivity analysis comparing different driving models, so that it's worth revisiting the thermodynamic profiles in that section as well.

Minor comments:

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Line 45-48: To what extent can cloud resolving simulations be considered “truth”? Please include a few references that have explored differences between models and sensitivity for a given model (e.g. to resolution).

Line 71-73: The list of previous studies and their relevant topics is nice to see, but it does diminish the impact of the present paper. Please add 1-2 sentences describing what processes can be explored with these case studies specifically. What is so unique about a high CAPE environment that the prior studies didn’t explore?

Line 77: Remove parenthesis around the reference.

Line 90: The grid spacings for the driving models do not match the value cited in Table 2.

Line 140: “been reported for this day” – A reference would be great, more out of curiosity than out of scientific necessity.

Line 141-142: “convective inhibition” – Should this be explored in relation to the driving model and or temporal differences?

Line 190-192: Shorten this to (e.g.): “In addition to the three days of interest described in Section 2, we further. . .”

Line 200: Is the forcing from these other two driving models still 3-hourly? Please specify.

Line 203-206: The ice particle habits are only mentioned towards the end of Section 4. It is worth introducing them here and indicate which habits are directly affected by the change to ice particle geometry and fall speed (e.g. is “snow” affected or not?).

Line 234: “next sections” – “next sub-sections”

Line 237: “Lindenberg” – It would be helpful to the reader not familiar with Germany to indicate the location of this site and the other two in Figure 1.

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Line 244: “new method” – If not against ACP policy, it would be helpful to cite manuscript in preparation here, or make a stronger statement indicating that this method was developed “in conjunction” or “in parallel” with this study.

Line 251: “these wavelengths” – Presumably this only refers to the 35 GHz radar, so should be singular. Also, it is worth mentioning that the retrieval suffers from attenuation in profiles with heavy precipitation (as clearly evident in Figure 2 and mentioned in the caption).

Line 312: “both day and night” – Are there any previous studies exploring how seamless this retrieval is? Are there differences in errors and detection efficiency between day and night? It would be helpful to add these.

Line 316: The wavelengths are the same as APICS. Has there been an intercomparison study of these retrievals? It is helpful to cite known differences.

Line 333: As above, any known differences between these retrievals would be useful to cite now.

Line 385-394: While this is all correct, this text is rather superfluous as the CloudSat/CALIPSO retrievals are not used directly in this study. The text can be removed without loss of understanding.

Line 404: “radar/lidar-based” – Preferred “radar/lidar-trained” as the actual measurements are not radar or lidar.

Line 409-410: “and space” – How is the error in space considered in this analysis? All one can do with the grid-point comparison is assume that there is only an error in time.

Line 464: “RAMSES observations” – “retrievals”

Line 465-471: “well reproduced” – I appreciate that not everything needs to be shown, but it’s worth knowing which observational data rainfall was compared against.

Line 472-485: This evaluation against RAMSES seems to completely ignore earlier

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statements that RAMSES is primarily reliable in the lower reaches of the cloud and that it can suffer from “strong signal attenuation” (Line 462). It would be worth considering the potential that RAMSES significantly underestimates IWC in thick clouds, as suggested by the authors themselves.

Line 558: “(on average or that is peak reduction??)” – This is a good question!

Line 641-643: The authors mention the “wet moisture bias”, but are there generally differences in the thermodynamic profiles between the simulations run from the different driving models? Particularly in terms of convective inhibition?

Line 683-684: Please specify the location of the field campaign CRYSTAL-FACE.

Line 692-712: Is there a significant change in latent heat release from these sensitivity runs due to the increase in riming that could affect cloud dynamics and hence duration and anvil extent? A brief comment in the text would be appreciated.

Line 750: “deficiencies in the microphysical scheme” – Is there any chance that there could be deficiencies in the radiative effects of the anvil cloud? The effects of radiative processes and latent heating have been shown to affect cloud lifetime e.g. Gaparini et al. (2019)

Gasparini, B., Blossey, P.N., Hartmann, D.L., Lin, G. and Fan, J., 2019. What Drives the Life Cycle of Tropical Anvil Clouds?. Journal of Advances in Modeling Earth Systems, 11(8), pp.2586-2605.

Line 767-768: How do the authors envisage constraining the graupel estimates? Would weather radar observations help or revisiting campaigns such as COPS (or proposing a new campaign!)? In other words: What is needed to improve the representation of graupel?

Line 793-798: The concluding remarks focus on future satellite missions, but some words on ground-based would be appreciated here, too. The comparison against the CloudNet retrievals looks promising, even if only briefly considered in the paper. A

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consideration of more cases would eventually allow a statistical evaluation against the CloudNet sites. Separately, there may be more complementary information from the Julich multi-instrument site that could be exploited.

Figure 2: Please specify in the caption the reason for the regular failure of Cloudnet retrievals for Julich – is there something specific about the measurements at those times? Regarding “Temporal retrievals”, please specify the temporal frequency, e.g. every 5 minutes.

Figure 4: Please specify in the caption the meaning of tqi and tqf.

Figure 6: The x-axis of the second panel says “CLCH” instead of “ICC”.

Figure 9: It would be helpful to also specify in the caption which categories are directly affected by the change in microphysics parameters.

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