Atmos. Chem. Phys. Discuss.,
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Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

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 #1

Received and published: 30 August 2020

This is a well-organized and mostly well written paper describing an evaluation of summer convective events in large-eddy simulations over Germany using the ICON model. With respect to the difficult problem of predicting the evolution of convection, little new scientific insight is found. The conclusion from the findings in this study that this depends more heavily on the uncertainty of the large-scale dynamical state based on data assimilation rather than on microphysical parameters/schemes has long been established. Furthermore, the description of the sensitivity experiments with regards to the forcing datasets and their forecast impacts is rather vague and do not shed light on what aspects of the thermodynamic state are sufficiently or deficiently resolved in the various forcing datasets . The most significant aspect of the paper seems to be for doc-

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umenting the ICON-LEM performance for the experiments performed here. The model is claimed to be a cutting edge tool for improving next-generation NWP models. The simulations are evaluated with a variety of ground-based measurements and satellite observations of cloud properties. The evaluations are reasonably thorough and the authors have done a nice job of assembling and describing the observational datasets, which are state of the art. While I can't comment on the model itself, the methods and data used in the evaluation are robust and presented in an informative way. Despite the somewhat limited significance of the study with respect to improving convective weather forecasting, I recommend that the manuscript could be published with minor revisions. While I am not a modeler, it seems to me that the manuscript could be improved by better describing the forcing datasets, their relative differences, and by better assessing and describing the impacts of these differences on the forecasts.

Other comments:

Line 227: Minnis et al 2008 would be more appropriate than the 2011 reference

Minnis, P., L. Nguyen, R. Palikonda, P. W. Heck, D. A. Spangenberg, D. R. Doelling, J. K. Ayers, W. L. Smith, Jr., M. M. Khaiyer, Q. Z. Trepte, L. A. Avey, F.-L. Chang, C. R. Yost, T. Chee, S. Sun-Mack, "Near-real time cloud retrievals from operational and research meteorological satellites", Proc. SPIE 7107, Remote Sensing of Clouds and the Atmosphere XIII, 710703 (13 October 2008); https://doi.org/10.1117/12.800344

Lines 286 and 988: Minnis 2020 should replace Minnis 2011

Minnis, P., S. Sun-Mack, Y. Chen, F.-L. Chang, C. R. Yost, W. L. Smith, Jr., P. W. Heck, R. F. Arduini, S. Bedka, Y.Yi, G. Hong, Z. Jin, D. Painemal, R. Palikonda, B. Scarino, D. A. Spangenberg, R. Smith, Q. Z. Trepte, P. Yang, and Y. Xie, 2020: CERES MODIS cloud product retrievals for Edition 4, Part 1: Algorithm changes. IEEE Trans. Geosci. Remote Sens., doi: 10.1109/TGRS.2020.3008866.

Line 558: there is a question from a co-author that should be addressed (on average

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or that is peak reduction??)

Line 571: approx. should be approximately

Line 573-584 (and A2): it is stated: "Nevertheless, lower cloud top heights of up to 10 or 11 km are likely underestimated in the simulation. " I think that you mean the occurrence of lower heights? It isn't obvious from the text or figure 5 why you've reached this conclusion though. How do you know this isn't a problem with the observations? In fact, you state that the observations underestimate on lines 569-570. With the exception of CALIPSO, most other observing systems underestimate glaciated CTH and therefore would have higher frequencies of occurrence for the lower heights than actually occur. Thus, without other information, it seems to me that model CTH frequencies may in fact be more accurate than the observations. Unless I missed this in the text, it would be helpful to further support the contention that the lower CTH's are 'likely underestimated' in the simulations.

Line 592: seems like you could refer directly to 5.1.2 rather generally to 5.1

Lines 593-595: again. how do you know anvil heights are overestimated in the simulations?

Line 646: would it read better to say: "not captured in the default" ?

Pg 50: Fig A2 caption. Reconcile top/bottom with left/right figures

Line 828: "resp." ??

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