Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-197-AC1, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Geoscientific Model Development Discussions

Open Access

GMDD

Interactive comment

Interactive comment on "Modeling of land-surface interactions in the PALM model system 6.0: Land surface model description, first evaluation, and sensitivity to model parameters" *by* Katrin Frieda Gehrke et al.

Katrin Frieda Gehrke et al.

gehrke@muk.uni-hannover.de

Received and published: 1 February 2021

We would like to thank the anonymous referee for the time and effort that was devoted to reviewing the paper. The original manuscript was suffering from technical flaws and could be significantly improved upon.

Reviewer Comment (RC): Within this study, the authors introduce the land-surface model (LSM) implemented in PALM model, and evaluate the performance using two-day in-situ observations. They conduct a series of sensitivity experiments to explore

Printer-friendly version

Discussion paper

the impacts of model parameters on simulating the boundary-layer profiles, the surface energy balance, and nearsurface meteorological variables. Despite the detailed description about the LSM and useful information for PALM users, the results are very preliminary. As the manuscript reads now, the authors touched some subjects only briefly without really adding any scientific merit. It is more like a graduate's project essay than a scientific paper.

Author's reply (AR): In principle, the manuscript is a technical model description paper (which fits nicely in GMD in that aspect). The study and sensitivity experiments serve both to provide a first evaluation and give an idea how sensitive results are to the selection of various parameters of the model. With that in mind, we do not agree that there is no scientific merit as the manuscript was not aiming at communicating any new scientific findings (and that is also not was we would expect in a model description / validation paper). Being authors of (together) more than 40 peer-reviewed papers, we are fairly sure that the quality of the manuscript is adequate for a scientific paper.

Specific comments:

RC: The manuscript needs significant restructuring in order to establish a better focus within this paper. I suggest the authors seriously consider the sensitivity experiment design first. Of course every parameter in a model would have more or less impacts on the simulations. To fit this study, no need to take the radiation scheme (RRTMG & CLEARSKY), large-scale forcing (ADV_tq), or resolution (Dz_2) into account. Other case like EMIS_95 and EMIS_100, even not being mentioned in the manuscript. These unnecessary results make the paper more difficult to follow and do not have much scientific merit being covered so briefly. Removing the relevant content would be better, in my opinion.

AR: Modelers often face a situation in which they want to model a real case scenario but not all input parameters of the site are known. With this in mind, the sensitivity experiment was designed. In this respect, the choice of the radiation scheme might

CC BY

be important. Case ADV_tq is needed to explain a mismatch between simulations and observation in the total water mixing ratio (L528-529 of initial manuscript). Case Dz_2 is used to discuss a possible explanation of the mismatch of the nocturnal boundary layer (L472-476 of initial manuscript). In our opinion, it is justified to address the mentioned 4 cases, which are not directly linked to the land surface. The other 17 cases are selected as endpoints of realistic values for each parameter. It is a very valid point that cases EMIS_95 and EMIS_100 were not mentioned in the original manuscript. In fact, both cases are always among the gray lines in Figs. 3-6, however they do not influence the energy balance components. We have added this to the discussion of the relevant section.

RC: Second, the results section seems poorly phrased. I see a little conjecture and repetition in Section 5. For example in L430-445, this portion could be removed (at least be shortened), as it does not provide much "facts" to convince readers. If I correctly understand, the point is the observed H and LE might be underestimated due to the limitation of eddy-covariance method, which partially explains the overestimated H and LE by model. Fig. 7 can be removed as well because we've already got those information from Figs. 3-6. The black line of RES term just indicates the measurements are of bad quality. Plus, a repeated statement about Bowen ratio in the end; authors have mentioned that in L396-400. For L472-489, after going through this paragraph, I still have no idea why the model is not able to reproduce the nocturnal boundary layer, even feel a big unsolved issue existed in the LSM or the atmospheric model. Authors should not do like give a hypothesis, reject it, and then say we in actual don't have much confidence in the rejection. This discussion won't help raise one's interest in the model.

AR: We acknowledge that the information shown in Fig. 7 can be deduced from Figs. 3-6, nevertheless the reader benefits from this figure, because it points out an important issue of simulation -observation comparisons: which is correct, the model or the measurement? With this figure we intend to emphasize this important is-

Printer-friendly version

Discussion paper

sue, and further we intend to give a rough measure of the uncertainty which comes along with such a model-observation comparison. The authors have high confidence in the observation dataset of CESAR. “The black line of RES term” rather indicates the energy-balance-closure problem, which shows that about 25% of the available energy is missing. Similar or even higher residuals can be found in all flux observations. We agree that, to a certain degree, it was difficult to identify the key points of the results in the original manuscript. To better guide the reader, subsections have been added to section 5. Paragraphs have been restructured and a sentence about the Bowen ratio has been deleted to avoid repetition. Regarding the issue to simulate the nocturnal boundary layer, we need to stress that this is a common finding for atmospheric models (see e.g., van Stratum, B. J. H. and Stevens, B.: The influence of misrepresenting the nocturnal boundary layer on idealized daytime convection in large-eddy simulation, Journal of Advances in Modeling Earth Systems, 7, 423–436, https://doi.org/10.1002/2014MS000370, 2015). There is abundant other literature on this issue and not related to the LSM or the particular LES model in use. One of the main problems with the nocturnal boundary is the representation of the dominant turbulent eddies. As turbulence is damped during nighttime by stratification, the dominant scales are much smaller than during daytime. As a consequence, a smaller grid spacing is usually required. In the scope of the present work, it was not possible to run the simulations at much higher resolution, so that we might ascribe some effects during nighttime to the coarse resolution.

RC: The third point is to be correct. Like L355, I assume ALBE_24 is the sensitivity experiment featuring a decreased albedo in comparison to the REF. But following L232, the shortwave albedo is set to 0.14 in REF. Please double check the setup of your experiments. L398: I doubt the discussion "cases HUMID_sat and LAI_05 show significantly lower Bowen ratios compared to observations". From Figs 4&5, I see larger H and lower LE which means a larger Bowen ratio (H/LE) than observations. In L509, it is the low temperature leading to stable boundary layer, not "stable layer, hence the low temperature". Likewise later in L513, the convective boundary layer started devel-

CC BY

oping because of the surface heating in the morning than "the stable layer is eroded and temperature can rapidly increase".

AR: Thank you for spotting these errors, we have corrected them accordigly. Regarding the albedo sensitivity experiment, the naming was misleading and ist now consistently based on the shortwave albedo (was longwave albedo before).

RC: Lastly, seriously improve the English writing.

AR: The manuscript was now proof-read by a native speaker.

Technical comments

RC: L1: PALM is an acronym?

AR: Even though the PALM developers do not want to use the long name (abbreviation for Parallelized Large-eddy Simulation Model) anymore and this paper is part of a special issue featuring PALM, we have included a note in the revised manuscript, because readers, who are unfamiliar with the model expect some kind of explanation.

RC: L2: "For this" -> "To this end"

AR: As suggested by a native speaker, we removed this phrase.

RC: L4: Add "with observations" after "agree well"

AR: Done.

RC: L8 & L47: "By this" -> "In this way"

AR: We removed this phrase.

RC: L235: What is CESAR?

AR: Cabauw Experimental Site for Atmospheric Research (CESAR) - was added.

RC: L263-264: Rephrase the sentence to "The CESAR site is well equipped with the vegetation and soil information which provides a good opportunity to evaluate the land-

Printer-friendly version

Discussion paper

surface parameterization proposed in the present study."

AR: Thank you for this suggestion, we rephrased the sentence accordingly.

RC: L267-271: Change to "The land surface scheme configuration is given in Table 4" and then add the information you don't have in Table 4.

AR: Redundant information was removed from the text.

RC: L314: "One the one hand" -> "On one hand"

AR: We removed this phrase.

RC: Fig.2: Crowded figure. May be plotted as Fig. 9, one time in one panel.

AR: In the revised manuscript, the profiles are depicted in two separate figures with only one day plotted at a time.

RC: L326, L334 & L337: Add "with observations" after "agree well"

AR: Done.

RC: L377: "Moreover, the simulated H ..., respectively" -> "The model overestimates H

AR: Thank you.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-197, 2020.

Printer-friendly version

Discussion paper