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Geoscientific Model Development Discussions
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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.

Anonymous Referee #2

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Summary:

This paper describes the land-surface model in the PALM model system. The subject matter is approprite for the GMD journal and the text is well written. The details about the model are provided in appropriate amount of detail, equations provided are also appropriate. As part of the paper, there is a "first evaluation" which I think could/should be improved upon, as detailed in my comments below. Even though this is a "first evaluation" I think it can be done with more rigor and there are, in my opinion, a few technical flaws. While I think the description of the model is ready to be published, the evaluation of the model (and comparison with observations) needs improvement before that portion of the paper should be published.

General Comments:

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1. Most land-surface models vs observation comparisons I am aware of use the model run in "single-point" mode with the observed tower data driving the model. However, the model output from the PALM LSM has been averaged over the spatial domain. This spatial seems to confound the comparison (e.g., l.337-340). Is there a reason the spatial averaging of the model data is necessary?

2. The 2-day period seems too short to do a thorough evaluation of the model. There are many decades of Cabauw data, but only a short 2-day period is used. Even for a "first evaluation" this seems like a weakness of the paper. According to the authors, this particular period was chosen was because (l.236) "...the forcing from the surface was dominant and larger-scale advection played a minor role." However, many times later in the paper they attribute problem with the model-observation comparison to larger-scale issues (e.g., l.333, "...which could be caused by advection processes in reality modifying the residual layer"). A much stronger statemtent would look at many days when the surface forcing is dominant and then contrast this with many other days when the surface-forcing is not dominant. Then the authors could actually show the model does better (or worse) when surface forcing dominates rather than simply making a vague statements about it.

3. The smaller observed H and LE fluxes than modeled flux during the daytime (ie, Fig. 7) is almost certainly related to the choice of a 10-min averaging period to calculate the turbulent fluctuations. The authors acknowledge that there are low-frequency issues with the fluxes (ie, l.247-250) and during the daytime the time-scale involved for the

fluxes are longer than 10 minutes. Perhaps the Kaimal correction they describe fixes this issue, but using a 10-min window to calculate the fluxes is certainly a problem in the daytime (probably ok for nighttime). Why use a model to try and fix a methodology shortcoming? If a longer time window is used (e.g., either 30-min or an hour), it will make daytime obs H and LE larger and in closer agreement to the modeled fluxes.

4. p.3, l.71, why is the heat capacity assumed to be zero for vegetation-covered surfaces? Heat capacity has recently been shown to be an important consideration in land-surface models (e.g., Swenson, et al 2019). Getting the heat capacity of the storage terms (soil, biomass) correct is an important consideration to properly close the SEB (e.g., Lindroth, et al 2010, Leuning, et al 2012). These so-called "smaller" terms are important because they tend to have a phase shift (in terms of the diurnal cycle) relative to the other Rnet/H/LE flux terms. The authors appear to focus on the issue of low-frequency contributions to the fluxes, and do not talk about the heat storage terms as a possible problem (in fact, since heat capacity of the vegetation biomass is set to zero can the biomass storage term even be considered?).

5. The model has been designed to have many options and work with many different land-surface types (e.g., Table 1)...however, the evaluation is only done for one specific land-surface type. This is a very limited test of the validity of the model over the paramater space—I realize article length is an issue–but, what about evaluations of other surface types? At least maybe cover more than just one?

6. Though there is good information in Section 5.2, it seems like this section would benefit from subsections that guide the reader a bit better. As it is, I find it difficult to extract the key points the authors want to make from the comparison.

Specific Comments:

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* does PALM stand for anything? Is this an acroynm? If so, it should be stated when

first introduced...

* p.4, l.98, remove parentheses with Duynkerke, 1999 reference

* p.6, l.159, is "high" vegetation, tall vegetation? such as trees?

* p.8, Table 1, why is C_0 set to 0.00 for all vegetation types?

* p.11, l.220, do waves have any effect on the transfer coefficients over water?

* p.12, l.244, "..but means of a Fourier extrapolation." Is there a reference for this method?

* p.12, l.244, Was a soil heat flux plate also used at some depth below or near the temperature measurements? If so, this is not clearly stated. Flux plates are typically used for measuring the soil flux while the soil temperature profile is used for the heat stored in the soil (e.g. see Eq. 7 and discussion in Leuning, et al 2012). [I now see this discussed on p.21, l.405-406]. Perhaps I don't fully understand this, but it seems like the comparison of the modeled and observed soil heat flux needs futher consideration. Are the same quantities actually being compared in Fig. 6?

* p.12, l.260, Fig. 2 is mentioned before Fig. 1.

* p.13, l.282, are the root fraction values based on measurements or assumed?

* p.16, Table 5, how were the specific values for each variable selected? For example, LAI has values of 0.5 to 3 m2/m2. Are these realistic or reasonable values? Furthermore, if you want to truly look at the sensitivity to LAI (or other variables), why not vary them between the endpoints, e.g., in steps of 0.1 m2/m2 between 0.5 and 3?

* p.18, l.345-374, I understand there is a difference in Rnet which is presumably due to an incorrect modeled surface temperature. But, I'm not sure what to take away from the discussion following this—is the suggestion that the LAI should really be 0.5 m2/m2? Is the problem with the observations since the radiative flux divergence is not included?

* p.21, l.410, "It strikes", should be "It is striking"?

* p.21, l.413-416, are you suggesting that the observed LAI is incorrect? If you increase LAI, LE should increase at the expense of H..this is not surprising.

* p.24, l.472, for more info on grid spacing of models in stable conditions, see Sullivan, et al 2016.

* p.28, l.575, "differences of up to 50% are possible.". Differences in which variable?

* p.29, l.602-603, I didn't see how step-like orography is implemented in the LSM? Was this described somewhere in the paper?

References:

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Discussion paper