

## ***Interactive comment on “Overview of the PALM model system 6.0” by Björn Maronga et al.***

### **Anonymous Referee #2**

Received and published: 21 October 2019

In the manuscript “Overview of the PALM model system 6.0” the authors provide an exhaustive overview of the most recent version of the PALM model, an Open Source atmospheric microscale simulation model. The overview covers not only the components that are integral components of PALM, but also models that are distributed independently of PALM. PALM has been under development for a couple of decades and it represents a widely used, comprehensive, validated, and well documented microscale modeling capability. The manuscript focuses on model’s capabilities without addressing their validation. The details of model components as well as their validation will supposedly be addressed in a number of separate manuscripts under preparation, or submitted to the special issue of Geoscientific Model Development Discussions.

### General Remarks

An earlier version of PALM was already presented in the journal Geoscientific Model

Printer-friendly version

Discussion paper



Development, however, the new developments included in the version 6.0 are significant and warrant an update. Overall the manuscript is well written, but in an attempt to be exhaustive, it turned out excessively long. By referencing a number of manuscripts under review or in preparation for the special issue of Geoscientific Model Development Discussions the authors avoided addressing all the details, including model validation. However, without these references the manuscript would not stand on its own. These references include detailed description of modeling capabilities and should be published before the overview paper. Furthermore, the paper is already too long and it should be shortened by omitting description of standalone models (Sections 4.10 and possibly 4.11), also Subsection 5.2.5. has low information content and can therefore be omitted.

Taking all the above into account I do not recommend the manuscript for publication in the present form. The manuscript could be published in Geoscientific Model Development after suggestions for major and minor revisions are addressed and all the essential referenced manuscripts are accepted for publications.

### Specific Remarks

Page 2, line 7 - Lilly 1967 is the first published paper about LES, however, it did not include any simulations, so it is not a proper reference here. Also, Deardorff published first LES results in 1970. Page 2, line 12 - Small scales are not parameterized, but the effect of small, unresolved scales on large, resolved scales. Page 3, line 14 - This sentence should start a new paragraph. Page 11, Equation (35) – Should it be  $1/(\phi_M)^3$  or perhaps  $\phi_H/(\phi_M)^3$ ? Page 12, line 6 - Basu and Lacser (2017) point to the potential problems with specifying surface fluxes and using the Monin-Obukhov similarity to determine surface temperature under stably stratified conditions. How is this addressed in PALM? Page 13, Table 4 – For integrated similarity function symbol  $\psi$  is commonly used. Using capital  $\Phi$  symbol for two different terms and differencing them by subscript can be confusing, it would be better to use a different symbol. Page 14, line 4 - It is not clear what is meant by "identical velocity and length

[Printer-friendly version](#)[Discussion paper](#)

scales," perhaps for all the spatial directions - is that an assumption of isotropy? Page 15, line 4 - The sentence starting with "See Noh et al. (2004)..." is redundant it repeats the same information contained in the previous sentence. Page 15, line 16 - This should probably be "rain water mixing ratio" instead of just "rain water mixing." Page 19, line 30 - WRF-LES has already been used for some time with land surface models. Page 20, Table 5 - Perhaps a different symbol can be used for the source/sink terms. Page 22, line 22 - Papers that have not been published should not be cited. This comment applies to a number of references made related to yet to be reviewed and published manuscripts throughout this manuscript. Page 23, Equation (78) - The longitude,  $\lambda$ , needs to be expressed in radians not degrees, so it must be divided by 180 and multiplied by  $\pi$ . Page 25, Equation (84) - Is this equation correct? For  $\psi = 0.5$  this results in a discontinuity in  $\alpha_{sw,dir}$ . Page 25, Section 3.5 "Wind turbine model" - The description of the ADM is imprecise and lacking. Page 25, line 24 - It would be more accurate to say that the actuator disk model accounts also for torque. Page 26, line 1 - The velocity,  $U_{rel}$  is affected by local smearing of the drag, so it is not really representative of the free flow velocity assumed by an ADM. Page 31, line 8 - More information could be provided about the wall functions. Page 31, line 27 - A reference to a paper that has not been reviewed/published yet. Page 32, line 13 - A reference to a paper that has not been reviewed/published yet. Page 32, line 30 - A reference to a paper that has not been reviewed/published yet. Page 33, line 18 - A reference to a paper that has not been reviewed/published yet. Page 34, line 23 - Instead of "in pair" it should be "on par." Page 34, line 24 - It would be important to mention what the scaling is for RTM 1.0. Page 34, line 29 - It would be important to provide a reference. Page 36, Figure 2 - It would be better to use the same color scale for all four plots so that relative importance can visually obvious. Page 40, line 5 - Instead of "highly recommendable" better would be "strongly advised." Page 40, line 7 - It is not clear why is this called "self-nesting" and not just "nesting." Page 41, line 13 - Here, in "... domains do always..." "do" should be omitted. Page 41, line 31 - It is not clear what is meant by "model" here. Is it nest or physics model, or something entirely dif-

[Printer-friendly version](#)[Discussion paper](#)

ferent? Page 42, line 8 – A reference to a paper that has not been reviewed/published yet. Page 43, line 27 – A reference to a paper that has not been reviewed/published yet. Page 43, Section 4.10 “Multi-agent system” - The paper is already very long and this may not belong in PALM description since the agent model is a standalone model. There are too many details about a standalone model, however, little is said about its utility and how it is coupled to PALM. Page 44, line 12 - Since it is a standalone program it is not clear that it should be included in description here. Perhaps it would be sufficient to mention it. Page 46, Section 4.11 “Human biometeorology” – Is this a standalone model? If yes perhaps it should be just mentioned and not described. Is this an online or an offline calculation? Page 46, line 25 – A reference to a paper that has not been reviewed/published yet. Page 47, line 7 – A reference to a paper that has not been reviewed/published yet. Page 47, line 13 – What about parallelization? Is this done for each part of the domain computation for which resides one specific processor core? In general, this could be written more clearly, perhaps with a few more details. Page 48, line 30 – Since pre-processing capability does not exist yet it is not clear that it should be mentioned. Page 50, line 8 – Is any interpolation used? Page 50, Subsection 5.2.5. – This section has low information content, and therefore could be omitted. Page 52, line 14 – Instead of "owns" it should be "includes." Page 52, line 20 – Section 6. “Conclusions” - Instead of "Conclusions" what is provided here is "Summary" and "Future Developments" or "Future Directions." It would be better to split this section in two sections. Page 53, line 54 – Instead of “immersive” more commonly used term is “immersed.”

---

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2019-103>, 2019.

Printer-friendly version

Discussion paper

