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Geoscientific Model Development Discussions

Interactive comment on "Validation of the Dynamic Core of the PALM Model System 6.0 in Urban Environments: LES andWind-tunnel Experiments" *by* Tobias Gronemeier et al.

Anonymous Referee #1

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General comments: This study has examined the representation of the turbulent flow characteristics reproduced by a newly updated CFD model, PALM6.0. The simulation results are compared with a wind tunnel experiment regarding the flow around a realistic building geometry in Hafencity. The results were generally well correspondence with the wind tunnel experiment, not only the simple mean wind speed but also the variance of the wind velocity and the spectral variation. This report will be useful for the users of this model. In addition, it also indicates some general issues in the simulation of the urban airflow. Therefore, this work is worth for publication. Besides, I still have some comments to be clarified before publication. One is about the model description. This study is motivated to analyze the model performance which is updated to version

6 of PALM. However, it is not sure if the core part of the model, which is related to the prognostic equation of the momentum (e.g. advection, diffusion, time-integration or wall boundary conditions, etc.) except the thermal effect, are updated from the previous version. Please describe more details about this point clearly. The results should also be more focused on the influence of the updated parts. Another point is the upwind condition. I think the results could be different near the upwind region if the inflow conditions are so different (Fig.2). However, the authors indicated that the observation points near the upwind region (e.g. station 2, 3, etc.) are almost same both in CFD and wind tunnel experiment. I am not sure why they are the same results irrespective of the different approach flows. Although it is fair to use the periodic boundary conditions in the streamwise direction, the effect of the inflow condition has to be more carefully examined (e.g. how far the direct influence of the inflow is observed?).

Specific comments: L7: "In the end, . . . " This could not be discussed from the materials shown in the present manuscript. L27: "drastic change . . ." I could understand there are many updates for the application parts but still not sure for the core prognostic parts. This is related to the general comment. L71: "25 locations . . ." How are these station chosen? For example, how the average of 25 vertical profiles becomes compared with the total horizontal average in the numerical simulation? L76: " we skip . . ." It has to be indicated here about which parts of the prognostic calculation were updated. L96: "1.54 m /s . . ." This is rather weak at the top of the boundary layer if this is in the real atmospheric scale. Are the present results really free from this main wind speed? L158, L167, . . .: "the approaching flow at a height of 50 m" "wind speed is 23 % less . . ." Please explain why the wind speed at this height is the reference. Since the correspondence of the magnitude of the vertical profile directly depends on this parameter, it needs justification to use this parameter as a representative velocity scale. L170: "located 0.5m lower" Why the values are not interpolated (e.g. linear interpolation, cubic spline, etc.) for the comparison? L240: "At roof top height" How is the effect of the mismatch of the local horizontal wind speed in the numerical simulation and the wind tunnel? I think the ratio of the building sizes, which will be related to the peak wavelength at the roof level, and that of the local wind speed will be different for the numerical simulation and the wind tunnel experiment.

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