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Geoscientific Model Development Discussions

Interactive comment on "Validation of the PALM model system 6.0 in real urban environment; case study of Prague-Dejvice, Czech Republic" *by* Jaroslav Resler et al.

Anonymous Referee #1

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General considerations

In this contribution, the authors use the recently developed PALM-4U urban LES model at very fine grid spacing (2 m) to model details of atmospheric properties, wall/surface temperatures etc., and compare the results to a detailed (dedicated) data set from the city of Prague. This is, first of all, a tremendous achievement – with respect to the modeling effort, as well as the dedicated observational campaign. It constitutes a major challenge for a numerical model to 'reproduce' various point measurements in an environment as complicated as an urban canopy layer.

Overall, the observations and also the modeling approach are well and comprehen-

sively described (there are a number of editorial issues, though. See major comment 1). With respect to the description of the obtained results, however, the paper looses its conciseness. By showing too much material (major comment 2), the 35 (!) figures are spent on just showing and describing the outcome of the simulation, rather than trying to identifying the true potential of PALM-4U – and also its limitations. According to my judgement, one may indeed conclude from the present results that, provided the correct wall and surface, etc. parameters are available, one can reproduce even the details of the wall temperatures, etc. If it comes to the flow characteristics (within the canopy!), temperature is pretty well reproduced (rms's likely below 1 K even for the bad cases). The magnitude of wind speed is largely improved over the driving (WRF) diagnosed wind speed, but the details (timing of local maxima etc.) are essentially not captured. As for air pollution (which of course is the greatest challenge), we get no more than what we can expect from a 'good dispersion model' (minor comment to l. 817). If the major problems were addressed (major comments 3-5) rather than just describing the output, one could further assess the potential of these fascinating simulations. For example, one of the relevant questions would be to decide what has more potential: to further improve the model (parameterizations) or to assess all the necessary surface parameters at the best possible detail.

Major comments

1) Editorial issues. The material is not very well described. Most of the figure captions are not complete (I have added a number of specifically missing explanations in the detailed comments – but certainly not all). The authors should carefully go through all the figure captions and assess whether all the lines, symbols etc. are explained. The most important missing information for most of the figures is, which simulation (parent / child domain) is shown. Furthermore, essentially all the figures having a color code, this is not large enough to allow to distinguish the numbers (e.g., Fig, 17, 26, 28, ..). Finally, a lot of material is provided in the supplementary material (which is good in principle) but should be better identified (see detailed comment to l. 144)

2) In fact, the authors are showing much too much material. Even if they have already put quite a substantial part of the examples to the supplemental material, it remains much too much what is being presented. The problem with this is that it produces lengthy descriptions of each example (where and when which variable shows under/overestimation etc.). Even an interested reader will get lost with all the unnecessary information (e.g., on the date, site, etc.), which of course has to be provided when showing all the individual examples. But as an 'interested researcher' I am not interested in learning whether PALM underestimated the daily cycle on July 15 (and not on July 16) in the afternoon, at site 11 while the underestimation was not so great at site 07 on the same day, but with slight differences on the next). I am interested in learning i) the typical (average) behavior and ii) the exceptions. Thus, rather than showing so many different sites (e.g., for wall surfaces it is Fig 13-15 and associated text, and Fig 18-21 plus associated text), one should seek a form to best characterize the mean performance and show one example and then provide statistics on the other sites (the reader could then see what is presented in Fig. xx and judged 'average performance', and learn that this figure is associated with a mean bias of yy and an rms of zz (whatever the statistics are to best characterize the behavior). This would leave more room (and attention) for the exceptions – e.g., for the wall temperatures there is this example with 'old buildings' having been insulated in the meantime (since the data base has been produced) and thus producing wrong wall heat fluxes and consequently temperatures.

3) To some degree, the authors suffer from a quite common weakness in 'model validation studies', i.e. that they find everything 'well reproduced' (in the conclusions, for example, 'all is well represented' (lines, 840, 843, 848, 856, 862). Even if some 'exceptions' are usually mentioned, this is not really helpful. Most often, one learns much more from the cases, in which the model fails. Examples of issues that could/should be addressed are: the 'opposite wind direction' (major comment 5); the strange peaks in wall temperatures (e.g. Figs 13, 14, 15, 18) – which might be related to the wall heat flux (Fig. 30); the wrong attribution to surface characteristics (see the above insulations

issue); and the reflection (non-Lambertian reflection missing in the parameterization). In all cases, the problem is 'mentioned' (sometimes with a hypothesis why it occurs, sometimes not). If possible the origin of the failure should be discussed – and also some statistics should be provided on how often (how strongly etc.) this occurs.

4) The authors provide vertical profiles in comparison to a radio sounding – but the sounding is not within the modeling domain. This, in principle, is a 'no go' in a model validation study. Either I have data that is appropriate for a comparison, then I compare it - or I leave it. In the present case, I have no clue, whether the differences are due to a model deficiency or just due to different locations being compared (and the differences are substantial, e.g. Fig. 9 for wind speed 20.7. 00:00). Thus the options are: only show the model in order to characterize the situation. Alternatively, the coarser model results (WRF) could be used to estimate the height, above which the grid point-to-grid point variability 'vanishes' (i.e., the model results are more or less spatially representative over the larger 'urban' area) – and then make the comparison only down to that height.

5) Comparison to wind speed at the highest building (referring to l. 510: 'This confirms the disagreement of wind speed'): if I understand Section 3.5 correctly, this analysis uses the nearest grid point in the PALM model (and we would be interested to learn which domain, see minor comments). When I check the wind direction at the times when PALM has what the authors call an extraneous (but probably mean extraordinary) peak (e.g., afternoon of 21.7., morning of 4.12., etc.) it is always when the wind direction is plus/minus opposite (or at least largely deviating). This brings us back to the (minor) comment to l. 232: even if wind speed was measured on the tallest building, if the observation is close to roof level, the flow will be influenced by the building itself. The flow impinging on the building will detach, form a wake, re-attach etc. – the details of which depend on the building structure, the flow of course – and the direction (see, e.g. Oke et al. / Climates of Cities / Chapter 4, e.g. Fig, 4.3). If the grid spacing is 2 m, all this is resolved by the model. If the overall (synoptic) wind direction is correct, the

model might still not capture the details of the near-roof flow structure. I consequently think that i) this type of situation deserves a more in-depth analysis and ii) the reason for the wrong wind direction needs to be found. If we take, e.g., Fig S17, 27.11.: the wind direction is almost constantly 'opposite'. If this is not wrong already in the WRF simulation (what I cannot imagine), it probably has to be a local (very local) type of reversed flow or similar close to the roof. A first step therefore would be to compare the WRF wind direction (and compare to the synoptic) over these periods of 'wrong wind direction' in PALM.

Minor comments

l. 11 for certain. . .

l. 36 the UHI effect. . ..

l. 83 The Prague Dejvice. . ..

l. 116 The majority. . .

l. 117 The lancover map. . .

l. 143 . . .by the infrared camera: only one? Or rather 'by infrared cameras'? (if only one: by an infrared camera. . ..)

l. 144 Tables S2 and S3: after having found those tables in the myriad of files provided in the supplemental material (it would probably be helpful to somewhere state that all the supplemental figures and tables can be assessed by following the 'index link '), the tables contain a number of unexplained acronyms (what is MV, HF, AQ, etc., also LCZ is not explained [can be guessed when reading the caption. . ..])

l. 167 what is the TYRSY01 system (not introduced)

l. 229 proposed? Rather 'positioned'

l. 232 60 m high: more important is the height above roof. Even if the building where

the sensor is mounted is high (the highest), it will by itself impact the flow. Also, in Fig. 1 the location is not labelled 'FSv' (but 'anemometer'). See major comment 5

l. 235 of the inner domain

l. 236 in Fig 1 the 'drone site' is labelled 'autumn only'

l. 246 The radiosonde. . .

l. 254 The PALM. . .

l. 272 the stand-alone. . .

l. 297 in Fig. S1

l. 318 wherever possible

l. 322 The Prague. . .

l. 326 using data from the terrain mapping campaign

l. 336 described in Tab S6

l. 345 initial and boundary conditions. The authors only provide the source (WRF) for the BC, but not the type of BC (also: at the domain top). Furthermore: how are the 49 WRF levels mapped to the PALM levels (especially within the canopy)? For example, the lowest WRF level (probably at 10 m [?]), is in the NOAH LSM meant to be above the canopy (and a bulk canopy approach is used), while in the lowest PALM level it is 'between the buildings'. The same probably is true for the second lowest level. Finally: how are the BCs interpolated in time?

Fig. 3 caption: what are the green and red rectangles?

l. 423 the 'weather balloons' are usually referred to as 'radiosondes' (later occurrences)

l. 462 (i.e., caption Fig. S16): first, it should be mentioned that 'modelled' refers to the CAMx simulation (that's at least what I assume). The reference to Fig. Supp13 should likely read Fig S10. How is the 95% confidence interval determined (from the distribution of all the sites and days?)

l. 465 morning and evening peaks are. . . and appear. . ..

l. 466 than in observations. . . .. But the CAMx model. . .

l. 468 play a more. . .

l. 473 at the southern edge. . .. . . with the model simulation

l. 474 necessarily to match

Fig. 9 apparently, the modelled results are averages from the parent domain (caption). When explaining (in the text) that the comparison is 'not fair' (because the sounding is from a different location) this should be mentioned there, too.

l. 482 is much smaller: indeed, not only much smaller, but also has a completely different shape. For the midnight sounding, the maximum wind speed is 10 ms-1 at about 800 m height, while PALM has very low wind speeds. This must be detectable in the WRF simulation (PALM can likely increase the drag, but I wouldn't expect this to be so dramatic. Can the authors comment on this?

l. 486 these discrepancies (or this discrepancy)

Fig 11 caption: state, which side is the summer and which the winter episode. Also, it should be stated which simulation (parent, child) is used here.

l. 504 'extraneous': the authors probably mean extraordinary? (same on l. 508, 509, ...)

l. 509 is somewhat overpredicted

Fig. 12 caption: south-west (not west-south). Also, the colour scale is hard to even detect (nothing to say about being able to read the numbers)

L 516 of about 290 K

Fig. 13ff what is the grey shading? (day/night?)

l. 551 when the surface temperature. . .

l. 554 two sharp peaks: these are 10 to 15 K (which is quite substantial) – and it also occurs on the day before. Can the authors offer an explanation for these peaks? (if we look at Fig 8, it does not seem to be a radiation effect. . ..)

l. 555 it is striking to note. . .. Here the horizontal surfaces are probably referred to.

Fig. 17 soil moisture panel, inlet: this is definitely too small to be distinguished (in fact, all the inlets are too small)

l. 595 was adjusted: based on what?

l. 666 the first two examples: in the caption of the corresponding figures (also Fig 24) it would be helpful to explain what influence is being shown (the identification of the site number is not extremely helpful for the reader who does not know all the sites).

l. 690 The terrain. . .

l. 712 Two further consequences. . .. are an altered. . .

l. 720 due to their local nature

l. 731 with the modelling episode

l. 736 the wall temperature: shows again – as in Fig. 14 for site 12 – these stratage peaks. These are also visible in the heat flux. Could this be a starting point to evaluate the origin of those peaks?

Fig 32 caption: the yellow band. . .: this is only visible in a few of the sub-plots. Also, there seems to be a black dashed line associated with the yellow band, which is not explained (all the same for Fig. 33)

l. 791 in accordance. . ..: overall, the most relevant observation, i.e. that (diagnosed) wind speed from WRF largely overestimates magnitude and amplitudes in the time series' (at all sites, all episodes) is not even made.

l. 800 when concentrations. . .

l. 806 . . .aerological soundings at 06:00 UTC 21 July (Fig. 33): I don't think Fig. 33 shows results from the aerological soundings – nor does it show a (particularly strong) underprediction.

l. 817 . . .fulfil the criteria for dispersion models. . .: it must be noted, however, that the 'dispersion models' of Chang and Hanna are simple 'Gaussian' or hybrid plume dispersion models. Thus with an immense increase in computing time and effort, the results are not convincingly better. Can the authors comment on that?

Fig. 35 what are the numbers in the inlet (identifying the '+' signs) referring to?

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