Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
https://doi.org/10.5194/gmd-2021-274-RC2, 2022

Comment on gmd-2021-274

Anonymous Referee #2

Referee comment on "Evaluation of a forest parameterization to improve boundary layer flow simulations over complex terrain. A case study using WRF-LES V4.0.1" by Julian Quimbayo-Duarte et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-274-RC2, 2022

Review of "Evaluation of a forest parameterization to improve boundary layer flow simulations over complex terrain" by J. Quimbayo-Duarte et al., submitted to Geoscientific Model Development

The paper makes a very interesting simulation over the complex terrain area of the Perdigao-2017 experiment, impressive in terms of computation power when looking at the horizontal resolution and the length of the integration. The sensitivity of the results to the introduction of a parameterization of the canopy in the lower 30 meters above the surface has a clear positive impact for the campaign-long simulation and shows improvement for the high-resolution cases.

To my opinion, the paper is worth publishing in GMD, although some modifications would be needed. The paper is somewhat uncompensated, as the 49-day run is very succintly described while the changes are relevant, while the high resolution runs are discussed extensively but more superficially. Also the Hovmöller plots are hard to follow as the details explained in the text are difficult to be seen. Sensitivity tests would have been interesting, varying the height of the canopy, and some discussion on the effects of the changes in the temperature profiles (not shown) would have been very welcome.

Some specific issues are:

1) A discussion on the choice of the vertical resolution, especially the location of the lowest point at 10 m above the surface. Why the lowest level is at this height? Couldn't the first level be located closer to the surface as other studies in complex terrain do? Is there a computationally-related reason, such as numerical instabilities on steep slopes, or is it because of some choice related to the applicability of the similarity theory?

2) In fact no explicit explanation is given about the use of the similarity theory near the surface, and what are the equations employed.

3) Justify the choice of the Lalic-Mihailovic shape for the leaf-area density. Is this the only available possibility or are there others and this one has shown to be the best option?

4) A more elaborated discussion of what it means to use a canopy-drag compared to surface roughness would be appreciated. In the latter case it is costumarily assumed that a logarithmic profile is imposed between the lowest point and z0 and that the similarity expressions hold (even if they were originally derived for flat terrain). Instead introducing the LAD profile and the corresponding drag breaks this conceptual model. I think the paper would benefit of a reflection on how the surface layer is different in both approaches, perhaps showing comparison profiles. Also, is similarity theory still used in the lowest level below the canopy profile?

5) The statistics with the modified run are much better than the standard one for the along-valley wind, while the cross-valley is not significally improved. The latter is attributed by the authors to the use of a too high canopy and to the heterogeneous distribution of forested areas. This aspect is superficially commented and could have been complemented with some sensitivity runs of short duration (only two levels with canopy during one day for instance), as it is an important point in the results.

6) I find the plots of the short-term simulations far from clear when differences between using forest or not are commented. For instance, to my eye it is difficult to find the "southwest flow observed near the ground on the lee of the topography" (lines 195-196). Perhaps a circle on the figure would make the task easier for the reader. In general fig 6 is not good to follow the explanations given in the text, maybe vertical profiles at the points of interest comparing the two runs would be more informative. As it is now, I find myself believing what the authors say. I suggest that the whole discussion concerning figures 6 to 8 is remade with more clear grafic evidence of the results that the authors indicate.

7) when comenting figure 9 it is hard to see almost anything that is commented in text. I would suggest to show perhaps only the diferences between simulation and observation so that the relevant issues jump to the reader easily. Some graphical information on temperature -and the corresponding discussion- is missing too, not only here but all along the text.

8) Instead I find Figure 10 a good summary representation of what is going on, it would be nicer if it was given together with average profiles for wind and temperature in order to see in what parts of the column the effect of the changes is more evident for wind and temperature.