Improving boundary layer flow simulations over complex terrain by applying a forest parameterization in WRF

Reply to comments of anonymous referees of manuscript wes-2019-77

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1 Introduction

We thank both anonymous referees for their comments and acknowledge their effort to improve our manuscript. We are very sorry to inform you that in spite of the detailed suggestions of the reviewers we are not able to finish the manuscript. The reason for this is that the main author left the field of atmospheric research at the end of December 2019 and started a new job, which is not connected to research activity anymore. He was not able to finish the manuscript due to time reasons, which means that we will stop the review process and let the paper in its discussion mode. We are very sorry for this and want to thank the reviewers for their work and time. In the following we answered the major comments very shortly.

2 Major comments reviewer 1

1. Major issue 1). Performing 48-day simulations without re-initialization or nudging to control the error growth in the model domain is inappropriate. It does not conform to the best practices in atmospheric modelling. As recommended by Warner (2011), one should "understand the limitations to the predictability of the phenomena being modelled". After about 72 hours, the WRF simulations probably lost most of their predictability. Luckily, the prominent topographic features at Perdigão are a dominant control of the evolution of the flow. Thus, it is not essential if WRF captures all the details of the synoptic flow correctly. It is probably only necessary to get the right large-scale flow direction for the terrain to force the small-scale flow in the right way. However, the use of such long un-nudged simulations gives the wrong impression to the readers. The setup of

the simulations is not appropriate for what the manuscript wants to show. I understand that the goal of the paper is not to show that forest canopies improve weather forecasts (of course, it probably does). But calculating correlation coefficients that include all masts, different heights and different sites together give that impression. (I will come back to this point in comment 2). So, I suggest downplaying (or entirely removing) the correlations and focus on a more interesting analysis of the results.

⇒ **We agree that analysis nudging would generally improve the model results. To reduce boundary effects in our model simulations we chose a very large outer domain D1 (see Fig. 1a in Wagner et al. 2019) which allows the development of their own flow dynamics in the inner domains D2 and D3. At the boundarys of D1 ECMWF analysis data are used as boundary conditions every 6 hours to adapt the WRF solution to the ECMWF analysis. We also think that the effect of analysis nudging would not be so large, as the synoptic condition was dominated by calm high pressure systems during the IOP (see Wagner et al. 2019), which means that local flow systems could develop in the innermost domain D3, which are not affected much by the dynamics at the boundary of domain D1. In addition it was not possible to rerun all long-term simulations again with analysis nudging due to limited computational ressources.**

2. Major issue 2). I think it a pity to combine all masts and heights in Figure 9. I believe there is much more to be understood. The added friction of the forest parameterization reduces the winds in places, but Figure 9 cannot show that. I think a more in-depth analysis of the results, via for example wind speed distributions at relevant sites and heights will be much more exciting and valuable. I don't see the point of the analysis of potential temperature either. My recommendation is that the manuscript might be acceptable after significant revisions: explaining and supporting the decisions made in the model setup and expanding the analysis of the long simulations.

⇒ **Thank you very much for your comment. In our manuscript we wanted to show the effect of a forest parameterization on the flow over complex terrain and think that at the end the "total" effect of this parameterization has to be shown by comparing all masts. This shows the net-effect of the parameterization. We agree that it would also be interesting to distinguish between different masts to see why at some places there is a better or worse correlation. Due to limiations in personal ressources we, however, cannot do this. The idea concerning potential temperature was to see effects of different turbulent mixing at the surface and related differences in stratification due to the forest parameterization. We agree, however, that further analysis should have been done here.**

3 Major comments reviewer 2

1. Nudging of the simulations When looking at the namelists that the authors provided as supplementary material it becomes clear that an analysis nudging is not applied in neither the short nor the long simulations. In case of the short simulations this might still be meaningful but for the 1.5 months simulation, a nudging should be applied to reduce the model error growth. This could in particular also improve the correlation that is investigated for the long term simulations.

⇒ **We agree that analysis nudging would generally improve the model results. To reduce boundary effects in our model simulations we chose a very large outer domain D1 (see Fig. 1a in Wagner et al. 2019) which allows the development of their own flow dynamics in the inner domains D2 and D3. At the boundarys of D1 ECMWF analysis data are used as boundary conditions every 6 hours to adapt the WRF solution to the ECMWF analysis. We also think that the effect of analysis nudging would not be so large, as the synoptic condition was dominated by calm high pressure systems during the IOP (see Wagner et al. 2019), which means that local flow systems could develop in the innermost domain D3, which are not affected much by the dynamics at the boundary of domain D1. In addition it was not possible to rerun all long-term simulations again with analysis nudging due to limited computational ressources.**

2. Wind power density discussion. Especially in situations with a local jet, the wind power of an assumed turbine of a certain size instead of the wind power density is much more meaningful. The rotor is integrating the power over the rotor area. Thus, I suggest to calculate the wind power using a rotor equivalent wind speed method here.

⇒ **Thank you for this comment. We think that you are right and would have done these computations, if our personal ressources would have been available.**