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Reply on RC2

Tobias Wolf et al.

Author comment on "Dispersion of particulate matter ($PM_{2.5}$) from wood combustion for residential heating: optimization of mitigation actions based on large-eddy simulations" by Tobias Wolf et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-81-AC2, 2021

General comments:

The study aims to use a LES model to identify the impacts of mitigating wood stove particulate emissions on air quality over Bergen. The study can have interesting scientific and policy-related implications. However, the current version lacks a proposer discussion and analyses of the mitigation impacts and the possible extrapolation of the results in other regions as it is limited to Bergen only. Additionally, although references to previous studies are provided with respect to model description, it is still necessary to include some features of the model regarding fx other emission sources, initial and boundary conditions,

and meteorological drivers. Finally, it would be good to compare the simulated PM2.5 levels with observations to better discuss the contribution of wood stoves and their mitigation. I would also recommend revision in the language as there are some section that are difficult to follow. Given the above concerns, I still encourage publication in ACP.

We thank the reviewer for very good indication of weak parts of our study. We now extend the discussion with respect to intercomparison to over wood-burning stove and emission studies in a broader global context. We also went through the modeling section and included more details here, specifically we explain the boundary conditions for PM2.5 and connection between PALM conditions and meteorological drivers.

Finally, we compare the simulated and observed PM2.5 levels for scenario conditions. More accurate intercomparison and analysis is however a subject for future (more technical) publications.

Specific comments:

Abstract:

Lines 20-22: Do you mean that the observed levels were higher than simulated levels?

This then cannot only be attributed to long-range transport as there are also biases in simulated local levels?

Indeed, this sentence was ambiguous. We are talking only about model results. It is now changed to:

"The simulated concentrations were larger than the concentrations obtained only due to the local PM2.5 emission …"

Section 3.1:

How about other sources of pollution such as traffic and other residential combustion sources? Are they (and how) treated?

What kind of meteorological information is used to drive the transport? These might be described in earlier publications but needs to be described briefly here for context.

We agree that this information is very important. Very detailed description with respect to meteorological conditions and diverse pollution sources and species has been published in

Wolf, T., Pettersson, L. H., & Esau, I. (2020). A very high-resolution assessment and modelling of urban air quality. Atmospheric Chemistry and Physics, ***20****(2), 625–647.*

We do not think that this paper is a good place to repeat this discussion again. We deliberately not to include other pollutants and other types of sources to look at the wood-burning stove effect. Nevertheless, we added a few comments on the matter here as well. In particular, we better address the surface, lateral and forcing conditions of our simulations. These additions are included in more suitable Sections 3.2 and 3.3.

Line 153: Correct "shell" to "shall" or replace with "will"

It is corrected now.

Line 156: Is there a reference for this "own user-code"?

This user code is described in

Wolf, T., Pettersson, L. H., & Esau, I. (2020). A very high-resolution assessment and modelling of urban air quality. Atmospheric Chemistry and Physics, ***20****(2), 625–647.*

The files can be sent in response to the request.

Line 159: Change to "prescribed" or "prescribing"

It is corrected now.

Lines 163-164: So a bulk PM25 is emitted directly from the sources?

We are not sure how to understand this comment. All PM2.5 is emitted from chimneys as sources of pollution in these simulations. Each chimney adds at each time step a certain mass of PM2.5 in the grid volume where it is located.

Lines 164-166: How are the sources treated, as area sources per grid cell? How do you then distinguish between the "old" and "new" stoves? Is it not possible to treat the chimneys as separate sources as you have this information on 1 m resolution from DEM? Is it not possible to calculate an assign a fraction of new vs old stoves per grid cell?

The source treatment is described in the text with sufficient details. Each chimney adds at each time step a certain mass of PM2.5 in the grid volume where it is located. Old and new stoves are different in the amount of PM2.5 per unit time. Yes, it is possible to separate chimneys at 1 m resolution but our model is run at 10 m resolution, so we can only separate households. To avoid sharp concentration gradients – poor for numerical schemes in the model – we further aggregate the emission in 3x3 cells arrangements. It is possible to calculate the assign fraction of stoves, but we do not see any need to do this as our analysis is focused not on the situation around a specific building but at larger spatial scales.

Line 216: "… installation OF only the new…."

It is corrected now.

Line 216: Why do you aggregate and average in 3x3 cells rather than using the absolute values in each grid cell?

The use of absolute values will lead to sharp concentration gradients and numerical instability in the model.

Line 218: So the initial conditions are set to zero? This should be further clarified with regards to consequences. Is it not possible to use a typical "background concentration" from for example "clean days"? This also allows to more realistically evaluate the contributions of local chimneys with respect to observed levels. How about the boundary conditions? Is there transport of PM2.5 to the domain from outside?

The argument looks reasonable when taken into a theoretical discussion. In practice, what would a typical background concentration in a clean day? Stoves do not work and rain cleans up air. So, measurements from Måledata for luftkvalitet | NILU – Norsk institutt for luftforskning" show the concentrations of 1-2 mkg/m3 – that is insignificant for our study.

There is no transport of PM2.5 into the domain, and this is arguably good approximation as Bergen domain protected by mountains and open sea.

Line 237: Replace "correspondence" with "agreement"

It is corrected now.

Lines 235-239: It would be interesting to have lower and upper bounds of this

mean and represent them in the simulations with extra scenarios.

We agree that such a powerful tool as PALM opens for exploration of different settings and scenarios, but such a study would be outside our present scopes.

Section 4.1:

A brief model evaluation is needed here, although earlier publications are refereed.

We think that additional focus on model will distract attention form the scopes of this study.

We add only the following sentence:

The maximum simulated (observed on 11.02.2021) PM2.5 concentrations were 76.7 µg m^{-3} *(81.2 µg* m^{-3}*) at Danmarksplass, 53.4 µg* m^{-3} *(59.2 µg* m^{-3}*) at Klosterhaugen in the city center, and 26.1 µg* m^{-3} *(18.6 µg* m^{-3}*) at Rådal. This agreement demonstrates reasonably good capture of the spatial variability and accumulation of PM2.5 in the scenario simulations despite the accepted simplifications, assumptions, and uncertainties.*

Lines 271-273: This artificial pollutant methodology is not clear and need a bit more details on what it is based on. Is it a way to transport pollutants outside the model domain or within?

We add more details to the description there. The methodology does not have anything to do with the transport of pollution, emission or diffusion as such. It only makes distinction between pollution from each district traceable throughout the simulations.

Section 4.2:

This section requires deeper analyses and discussion of the different mitigation scenarios. Currently, it reads like a summary of a previous study rather than stand-alone results from the present work.

This comment is surprising for us. We would argue that this Section presents only of the most important results of this study – a demonstration that plausible measures limited to just to some areas may eliminate the concentrations above a given threshold (Figures 6 and 7). Moreover, we present only own results between lines 330 and 355 so that about 75% of the Section total length. Nevertheless, we introduce some changes to emphasize the sovereign results of this study.

Conclusions:

The section is missing an interpretation of findings with respect to existing literature on similar works in other parts of the world in in order to put the present study in a more regional and global context. Currently, the impression is that the general interest to the results seems to be rather limited.

Indeed, this is important weakness to be corrected. Now, we include more on the regional and global context too, generalize the conclusions and methodology, and emphasize scientific contribution of this study to the common body of knowledge. However, there are important methodological differences that set a barrier to point-by-point intercomparison with the other studies. Previous studies did not consider specific meteorological scenarios for the highest PM concentrations. And this is for a good reason, they are based on the meteorological models that are deficient under the stably stratified atmospheric conditions, i.e., under the conditions when the highest concentrations are found. Therefore, the previous studies focus on the mean concentrations, whereas the largest (but short-term) impact is associated with the highest concentrations (e.g., Grange et al., 2013).

Grange, S. K., Salmond, J. A., Trompetter, W. J., Davy, P. K., & Ancelet, T. (2013). Effect of atmospheric stability on the impact of domestic wood combustion to air quality of a small urban township in winter. Atmospheric Environment, ***70****, 28–38.*