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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The MetVed model: Development and evaluation of emissions from residential wood combustion at high spatio-temporal resolution in Norway" *by* Henrik Grythe et al.

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Discussion paper

Overview

In this response, reviewer comments are in blue, while the response from the authors is in black. Where applicable, text from the paper that have been changed are in red. A document with tracked changes to the original manuscript has been added. We thank reviewers for insightful comments and suggested changes, which helped improve the manuscript.

The paper presents a description and evaluation of the MetVed model, a tool that allows estimating residential wood combustion emissions for Norway at high spatial and temporal resolution. The strength of MetVed is without a doubt in its ability to combine very detailed datasets that allow reducing the uncertainty in the spatio-temporal distribution of residential wood combustion emissions, which play a key role in the PM urban levels. The paper is well written and clear and a good contribution for ACP.

The following comments should be taken into account before accepting the paper.

General comments

The manuscript should be accompanied by a figure that illustrates/summarizes the general structure / workflow of the MetVed model (i.e. inputs,main functions, outputs). The amount of information used by the model is quite large, and sometimes it is difficult to follow how all this information is combined (and how the different datasets are supplemented). This figure will be very useful for the reader to understand better how the multiple data are combined to derive hourly gridded emission data.

We agree with the reviewer and have added a figure showing the data flow and

the various calculations done in MetVed. It is Fig. 1 in the current version of the manuscript.

Emission factors are established for three categories as a function of the appliances (fireplace, old stoves and new stoves). Several studies have shown that emission factors can also largely vary according to the type of wood being burned (e.g. maritime pine, eucalyptus). An example of this are the results obtained inthe AIRUSE LIFE project (http://airuse.eu/wp-content/uploads/2013/11/R09_AIRUSE-Emission-factors-for-biomass-burning.pdf). Why the wood type influence is not taken into account in the MetVed model? Is it because only one type of wood is being used in Norway? Or because this information is not available? This topic should be discussed in the paper.

As the reviewer points out there are several types of wood used in Norway, and emissions (and heat output) will vary with type of wood used. Large scale production of fuelwood and sales of wood (estimated 70% of consumption) is nearly exclusively birch. However, there are no detailed description of the type of wood gathered by the estimated 30% privately produced wood. This bulk of wood must be assumed to be assorted local species which certainly include but is not limited to birch.

The assumption of birch only in MetVed stems mainly from official reporting numbers and the desire to keep in line with national reported numbers, and therefore their emission factors. There are some small changes in several parts of the document, detailing this.

The model estimates emissions for several pollutants (i.e. CO, CH4, PM10, PM25.,BC and PAH) but the paper does not mention other species that are also relevant in terms of air quality such as Organic Carbon (OC, only appears in Figure 3.D), NMVOC (which influence the formation of secondary organic aerosols) or NOx or climate change (CO2). Is there a specific reason for that? Are NMVOC and NOx

emission from RWC considered when performing the air quality modelling exercise?

Generally carbon emissions from firing wood is not counted as CO2 emissions as it is considered carbon neutral. Compounds emissions are presented but given limited presence in the paper for other species than PM and BC as the focus was on validating against observations. The limitations of the dispersion model used in regards to chemistry also make challenging to model some of the species. Therefore, with spatial and temporal resolution the same, little is gained by giving too much weight to other compounds.

When performing the atmospheric dispersion modelling exercise, the MetVed emissions are used as input data to the EPISODE model. It is not clear how EPISODE treats the formation of secondary particles (inorganic and organic), which may have an impact in the modelled PM2.5 concentrations. Also, it is not clear which source apportionment method is used (is it a brute force approach?). Residential wood combustion emissions can contribute considerably to the atmospheric organic aerosol burden, particularly in regions with cooler climates, through both primary emissions and significant secondary organic aerosols (e.g. $https://www.nature.com/articles/srep27881$). The formation of SOA due to RWC emissions should be discussed in more detail when evaluating the results.

The model EPISODE in the current setup have no secondary aerosol formation or aerosol chemistry. The simplification is somewhat counterbalanced for PM as the emission factors from Seljeskog et al.,(2013) are designed so that particles are measured when the plume is cooled and diluted and so the particle mass will include significant condensed mass. With no aerosol-chemistry in EPISODE source apportionment is trivial and a straight forward decoupled (”or brute force”) method can be applied.

Specific comments

P4 L19-21: This sentence should be revised. The manuscript clearly states that the authors had to perform a huge work in terms of collecting all the input data required by the model (which in some cases was facilitated through personal communication and not through open data portals). In this sense, the application of the tool to another country/region may not be as versatile and transferable as stated. Similarly, it can not be say that the model can be transferred to other emission sectors. MetVed is explicitly designed to estimate emissions for residential wood combustion emissions. Other emission sources (e.g. traffic) would require other type of input data, algorithms and work flows, that MetVed does not currently include.

Rewrote the section substantially: As the above data-sets constitute the basis for the analysis of RWC in Norwegian households we provide a detailed description of each dataset in this section. The utilisation of high resolution data is important for the MetVed model to produce valuable results. The principles behind building an emission model with more bottom up principles relies heavily on gathered underlying data. Thus to achieve accurate emissions new avenuefor data gathering is an important field of development.

P7 L23: Can the MetVed model use gridded outdoor temperature provided by a numerical weather model?

Yes, we are considering this possibility in further developments. For a forecast it would require to relate consumption / emissions to temperature for a given grid. The way the model is set up the consumption per year is fixed and a posterior distribution of given consumption.

P10 L15: A citation should be added (I recommend Quayle and Diaz (1980)).

Quayle R.G., Diaz H.F., 1980. Heating degree day data applied to residential heating energyconsumption. J. Appl. Meteorol. 19 (3): 241–246.

Reference to Quale and Diaz (1980) has been added as suggested.

P11 L13: This is not shown in Figure 3.a (comparison of emissions reported by CLR-TAP and estimated by MetVed)

The correct figure reference has been added (old Fig.2 now Fig.3)

P13 L26: Spatial and temporal distribution

Added and temporal to text.

P13 L27: Not all the emission inventories used for comparison are Norwegian (i.e.TNO-MACC-iii and EMEP are European emission inventories). Also, it should be stated how subdomain emissions have been derived from the original gridded inventories (e.g. for each subdomain only grid cells completely within the domain have been considered, or all the grid cells with the centroid within the domain, etc.).

"Norwegian emission inventories" changed to "Emission inventories covering Norway"

Also added Total emissions within each domain are computed by 1st order conservative remapping emissions for each inventory to the domain (e.g. Jones 1999).

P13 L29: EMEP and TNO-MACC-iii are not urban emission inventories. This adjective should be remove in all the discussion.

changed text to emission inventories available to do urban modelling For the remainder we have kept "urban domains" as this does not speak to the intent but rather

the fact that the chosen domains cover urban areas.

P14 L1: replace 0.1 by 0.1x0.1 (and remove 7km, it is not true). Also, add a reference to the EMEP inventory.

Changed to the correct resolution as suggested by reviewer. Specifications for the EMEP emissions appear not to be publicly available and do not have a proper reference. However, they are (also gridded) openly available at the "Centre on Emission Inventories and Projections" through https://ceip.at/.

P14 L8: Not the same spatial resolution, slightly different (i.e. 0.125x0.0625). Also,replace TNO-MACC by TNO-MACC-III

Changed to the correct resolution as suggested by reviewer.

P14 L35: Previously it has been stated that TNO-MACC-iii downscaling is based on population density, not dwelling density.

Changed statement to population.

P15 L27: Add a reference to the EPISODE model.

Hamer et al. (in prep.) added as a reference.

P15 L29: background concentrations should be replaced by boundary conditions.

Text edited as suggested by reviewer.

P16 L3-8: This information should be moved to the previous section (5)

This information is specific for PM2.5 and it is our opinion that the text belongs in section 5.1 rather than the more general section 5 which also include information on the other simulations, for which this is not relevant / correct.

P16 L23: The correlation improvement is mainly occurring in 3 stations. In the other cases differences are rather small and not statistically significant.

We agree that this point was perhaps overstated in the text, and modified the statement to reflect this more moderately tends to improves correlation at most stations.

P16 L31: Remove "for the MetVed modelled concentration".

Text edited as suggested by reviewer.

P17 L16-19: What happens in terms of model performance when changing the emission vertical distribution? Is the overestimation observed in Oslo (Table 2) reduced? Maybe this feature (vertical allocation of emission) could be the main reason for the general overestimation of PM2.5.

We also had a lot of interest in the vertical distribution of emissions. Changing the vertical distribution higher up, reduces model surface concentrations and lowering it increases model surface concentration. On the whole having higher emission altitude would reduce the bias in PM2.5 concentrations. It would however act to reduce (especially) hourly correlation, due to decreased seasonality of concentrations and diurnal concentration.

While we have no solid evidence of one emissions being too high or low, we went

with altitudes that seemed logical considering average building height of different types adding typical plume rise. We have not succeeded finding research material well documenting what the ”correct” altitude ”should” but have argued that separating apartment from others is sensible and produces improved results in our domains.

P20 L1: In terms of emissions, which is the main source contributing to total BC emissions? Considering the low BC fraction used in MetVed, I would think that probably road transport is the main contributor. Then, maybe the uncertainty comes from this emission source. Also, the uncertainty can be related to the BC fraction used in the Metved model.

As these are road near measurements, the report by C. Hak (2017) shows less than 10 % biomass burning derived BC, the remaining predominantly from traffic (fossile). We feel the distinctive diurnal and seasonal cycle makes a strong case for the separation, supporting the instruments partitioning of the two. Though we accept your point that a small partitioning error of traffic concentrations would lead to large concentration differences of BC_{BB}. We agree that there are a number of sources of uncertainty, but believe more in the latter cause you give (BC fraction).

P20 L25-27: This sentence should be removed. See comment on P4

we modified the text here in accordance with comments on P4.

L19-21. Figure 1: The text that appears in Figures 1.D,E and F is not self-explanatory (should be replaced by other options such as D. Wood burning installations, E. Density of woodburning installations and F. Share (%) of wood based installations. Also, the legend in Figure 1.E is not specified.

We have followed the reviewers recommendations and redone the figure and

text.

References

Hak, C.: Vurdering av Black Carbon (BC) og CO2 langs veg i Oslo. NILU/OR Report 11/2017. Norwegian Institute for Air Research, Kjeller, Norway. (In Norwegian), 2017.

Jones, P. W.: First- and second-order conservative remapping schemes for grids in spherical coordinates. Mon. Wea. Rev., 127, 2204–2210. doi: 10.1175/1520-0493(1999), 1999.