Referee Comment on egusphere-2023-1194 ”The contribution of residential wood combustion to the PM2.5 concentrations in the Helsinki Metropolitan Area” by Leena Kangas et al.

General Comment

The paper presents a multiyear analysis of the contribution of residential wood combustion (RWC) to fine particulate matter (PM2.5) in the metropolitan area of Helsinki. The study builds on a previous study on the contribution of RWC emissions to urban fine particulate matter in Nordic cities (Kukkonen et al., 2020), but extends the scope to the year-to-year and seasonal variations, and a comprehensive validation against measurements.

The study concludes that holiday seasons should be considered when developing the temporal profile of RWC emission, mostly based on a single case when high concentrations were predicted during local holidays. The dynamics of the contribution of RWC emissions should be investigated more systematically given that a large dataset of 6 years is now available. In an upgraded analysis, the authors should compare modelled and measured winter PM2.5 separately for weekdays, for weekends and for holidays. They should also discuss the representativeness of the measurements used for comparison of the model results.

Specific Comments:

1.) Introduction (P2, line 54-58): Please provide increase/decrease of residential biomass consumption as percentage value in the European Union (EU) and for the mentioned European countries.

2.) Introduction (P2, line 58-64): Due to the phase-out of coal in Germany and other countries, there is a trend to replace coal by wood for energy production. Is this additional wood combustion relevant for Helsinki? At least, energy production should be mentioned as potential new urban wood combustion source.

3.) The method to distribute annual RWC emissions over time is not very clear. The basis for the temporal variation seems to be questionnaires for 2013-2014 and a survey on fireplace usage for 2008-2009 (P7, line 168-170). Did the authors use the two surveys separately for different years or did they use the average result of the two studies? It is also not clear whether ambient temperature data was used directly or indirectly for the temporal distribution or not at all. It would be better to use the heating degree-day concept for all kinds of RWC emissions, based on the daily outdoor air temperature, which should be available for any city. The heating degree-day concept has a robust basis, is applied in many air quality models, and is used for the temporal profiles of the CAMS-TEMPO database (Guevara et al., 2021).

4.) RWC emission, after plume rise, were injected at a height of 7.5 m (P7, line 181-184). How was the vertical distribution of RWC emissions evaluated? The exhaust from wood burning should be a warm plume, which rises by buoyancy, in particular in winter when air temperature is below zero. There might be situations of stable inversions in winter as well, but these should not occur so frequently. The injection of RWC emission at one height, without further vertical distribution seems questionable.

5.) Were the regional background concentrations computed with SILAM simply added to the concentrations of the urban dispersion model in the post-processing or were they used as boundary condition for the urban simulation?

6.) Suggest splitting of Table 2 in two parts, the first table for the stations when data is available for all 6 years and the second table for the stations when only one year is available for comparison to observations. The second table could be arranged differently, with the statistical indicators as column headers, and therefore be reduced in space.

7.) P14, line 309-315: How can we be sure that under predicted PM2.5 in summer is not due to missing wood burning sources such as for example sauna stoves?

8.) P17, line 345-352: The predicted high daily concentrations at -4 °C are explained in the text by high predicted RWC emissions during a weekend. The question is then: why did the other weekdays during the winter holiday season not also show high predictions of PM2.5?

9.) Finally, shipping could be a significant contributor to PM2.5 concentrations in the harbor areas of the peninsula as is also stated in the beginning section 2.2. Predicted PM2.5 seems to be rather low in the port areas of the maps in Figure 4. Suggest reminding the reader about the potential ship contribution in section 3.2.

Technical Corrections:

P9, line 252: “Factor-of-two …” I assume this refers to the definition of F2. This should be stated here. F2 is commonly referred to as FAC2. Please add a note if F2 is the same as FAC2.

Figure 3: The coloring in the plots of figure 3 are extremely weak. The colors in the other figures are also quite weak (grey instead of black) but in the scatterplots of this figure, the weak color saturation impedes the reading of the plot and the regression equations.

Page 22, line 431: Replace “clearly higher” by “much higher”.

References:

Guevara, M., Jorba, O., Tena, C., Denier van der Gon, H., Kuenen, J., Elguindi, N., Darras, S., Granier, C., and Pérez García-Pando, C.: Copernicus Atmosphere Monitoring Service TEMPOral profiles (CAMS-TEMPO): global and European emission temporal profile maps for atmospheric chemistry modelling, Earth Syst. Sci. Data, 13, 367–404, https://doi.org/10.5194/essd-13-367-2021, 2021.