

Interactive comment on “The influence of residential wood combustion on the concentrations of PM_{2.5} in four Nordic cities” by Jaakko Kukkonen et al.

Anonymous Referee #1

Received and published: 27 August 2019

Referee Comment ACP-2019-564

General Comment: The paper presents a detailed study of residential wood combustion as a source of PM_{2.5} in four different cities, one each in of four different Scandinavian countries. The study encompasses measurements, emissions inventories, and dispersion modeling for each city and surrounding area. The authors have done a credible job of pulling together a number of disparate sets of measurement, emissions, and modeling activities into a single manuscript. This is both a strength and a weakness of the paper, but in this referee's opinion, the balance is strongly on the "strength" side. Given the large variety of data sources, they did a fine job keeping focus and

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consolidating where possible. I do have a few suggestions to improve the paper for the reader, which I will detail below. With the minor suggested improvements, I expect the manuscript will be publishable.

The major improvement I suggest is to tackle the numerous differences in the data sources in a much more head on fashion. The authors do a very good job of describing the differences in measurement methods in Section 2.1.2, the differences in emissions inventories in Section 2.2, and the differences in dispersion models in Section 2.3. What is missing in each case is a discussion of the significance of the differences. Are the differences important? Do they affect the results in any way? How might one evaluate the effects of these differences in the overall study. I could see a paragraph at the end of each section acknowledging the situation and evaluating its importance to the study and its results.

Specific Comments: Lines 442-446: This section could be described a little more clearly, and reasoning for the adjustment explained. Why did the measured background values need to be adjusted based on dispersion model calculations? What was the measured value before adjustment? How is the Bredkalen station considered background if its values need to be adjusted?

Lines 505-511: The authors state that the modeling system "has been evaluated", but give no indication of what the evaluation concluded. Was it determined to be accurate and appropriate for the study? Or were there problems? If there were problems discovered what were they and have they been addressed?

Line 520: Were chemical reactions and aerosol transformation processes included in the other dispersion modeling systems? If so, did they affect the results? What kind of impact might these processes have on the Helsinki results?

Lines 603-608: Figure 3 – I suggest the third set of panels for this figure. Add a top panel with the total PM_{2.5} for each of the sites organized by classification like the FB and IA panels. Line 650: The purple color for the highest emissions should be

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changed. Maybe use a brown or a brown-red?

Lines 734-737: There is no mention of topography anywhere in the text. It looks to me like topography is a major factor in the high concentration areas of Oslo. I did not do an independent check, but am guessing that the high concentrations in the eastern section of the domain are in a valley where wood smoke (and other pollutants) tend to “pool” during nocturnal and other inversions.

Technical Corrections: Line 48: Add the word “the” before “EU”. Line 75: Replace “contributed” with “attributed”. Line 125: Replace “evaluated” with “determined”. Line 686: Change “is” to “are”.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-564>, 2019.