

## ***Interactive comment on* “The influence of residential wood combustion on the concentrations of PM<sub>2.5</sub> in four Nordic cities” by Jaakko Kukkonen et al.**

### **Anonymous Referee #2**

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This manuscript estimates the impact of residential wood combustion (RWC) on PM<sub>2.5</sub> over 4 Nordic cities using several data collection and modeling approaches. By integrating emissions data and modeling tools for each urban area, the authors characterize and compare the contribution of RWC to air pollution at different locations. This represents a significant undertaking to address an important research question. However, the manuscript's quality must be improved before being considered for publication. Specifically, a clearer description of methods and results must be provided, and the conclusions stated should be better supported. The limitations of the study's methods should be acknowledged and reported estimates better placed into context. Major and minor concerns associated with the current manuscript are listed below.

Major comments:

1) Use of English must be improved. The language used throughout the manuscript can be clearer and more concise. The overuse of passive voice often makes sentences confusing.

2) Figure quality should be improved. Figures lack labels and are low resolution (specific comments are listed below).

3) The introduction and literature review are disorganized. In the introduction, the authors fail to clearly convey the key points of the study and its motivation. Instead, the introduction jumps between locations, methods, and data sources. In its current form, it is difficult to understand the current state of knowledge on this topic and how this study fits into the larger body of research.

4) Several elements of the methodology are unclear. The intent of the study often gets lost in the language and details, some of which are unnecessary and do not address the more important aspects of the work. The manuscript is long and yet information important to understanding the work appears to be missing. Clarification and reorganization throughout the text should be considered to improve readability and highlight the study's main contributions.

5) As it stands now, the study largely focuses on the modeling approaches – descriptions of aspects of the different modeling setups for each urban area and their predictions are a large component of the manuscript. Nevertheless, as a modeling study, the analysis often can be superficial and incomplete. Conclusions are drawn from a patchwork of four different modeling frameworks. Still, the modeling systems are only briefly described and limitations/differences between them not clearly identified. Comparisons between them should be better consolidated and considered/discussed in the analyses of the modeled PM<sub>2.5</sub> fields.

6) Inventory compilation is not discussed at depth. The methods used for RWC use

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and emissions estimates can be unclear.

7) The RWC impacts on PM<sub>2.5</sub> levels, the paper's main contribution, are not presented until page 24, after an overly verbose description of less relevant elements. The manuscript devotes a large amount of text to the total PM<sub>2.5</sub> modeled fields, rather than RWC impacts. Beyond justifying the use of these modeling systems to simulate atmospheric pollution at these locations, this seems to distract from the main objective of the manuscript. A clearer focus on the RWC-attributable PM<sub>2.5</sub> concentrations and deeper analysis of these (e.g. a spatial map of the RWC PM<sub>2.5</sub> instead the fractional contribution in fig. 7, a description of seasonal/monthly variability, and estimated of maximum daily impacts) would strengthen the manuscript.

8) A clearer explanation of why only some urban emissions sources are included in each urban area's simulation is required. Has previous work shown that the sources considered capture the majority of urban emissions and pollution? Additionally, it is unclear if the regional simulations used to estimate Helsinki, Oslo, and Copenhagen background concentrations include the urban area's emission from the urban areas themselves. If so, how does the study avoid double counting these emissions when finer-scale dispersion from urban sources is added to the regional background concentrations?

9) There is no mention of atmospheric chemistry, in particular, secondary aerosol formation. Prior research has shown that indoor combustion emissions can generate substantial secondary particulate matter, depending on combustion and atmospheric conditions. However, this is not discussed. While, the modeling tools applied at the urban scale do not include chemical or physical transformations, at a minimum the authors must discuss this limitation and how they would expect the inclusion of atmospheric chemistry to impact their results.

10) The conclusions section is mostly a repetition of the results. The authors should provide a deeper discussion of the significance of their RWC quantification and how it

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fits into the large body of research, including insights, implications, and limitations of the work, as well as future research needs.

11) Policy is mentioned in the abstract (as a strongly worded recommendation) and briefly discussed in the introduction, yet this implication discussion was moved to the appendix. If this is an important implication, it should be discussed in the main body of the article and the authors should identify what specific policy implications the work has.

Additional comments:

- Line 24: The text “the higher long-range transported background” is unclear and should be reworded.
- Line 40: Little discussion or reasoning is provided to specifically support this statement: “Stricter and more efficient emission regulations should be set in the Nordic countries with respect to RWC”.
- Line 45: Support this statement with references.
- Line 57-58: Remove this sentence: “Clearly, the burning of oil, coal . . . scope of RWC”
- Line 75: Throughout the manuscript, the word “contributed” is used incorrectly. The correct word here is “attributed”.
- Line 82: Where were the Nordic observations reported?
- Line 105-107: This sentence is unclear and should be reworded.
- Line 110: Again “corresponded” is used incorrectly. Here, the intended meaning is “was responsible for”.
- Line 115: Replace “ca.” with “approximately”. I would also recommend avoiding “viz.”
- Line 127: Replace with “colder half of the year”
- Line 127-129: This sentence is poorly written.

- Figure 2: The figure is low resolution and low quality. Making colors, labels and features uniform among panels would help. Panels are not labeled.
- Section 2.1.1: This section is not very informative with respect to main findings, and the information within could be moved to the Appendix to improve conciseness
- Line 241: The term “addressed” appears to be used incorrectly.
- Line 245: What do the authors mean by an “urban background site”?
- Line 257: What criteria were used to determine that these 3 stations are RWC influenced (and the others not)?
- Line 286 and 288: Again “assessment” appears to be used incorrectly.
- Line 298-299: Cite the prior work on measurements and emission factors that this mentioned.
- Line 300-305: Why is temperature and resolution discussed in this paragraph, is a connection trying to be made?
- Line 347-349: Better describe how the different data sources were combined.
- Line 354: Here and throughout the manuscript, the word “evaluated” (similar to “assessed”) could be replaced with “estimated” to add clarity.
- Line 439: How were the background concentrations “slightly adjusted”?
- Lines 442-446: The adjustment applied and its justification are unclear.
- Line 476-479: The description of the use of the OSPM model is unclear.
- Line 530-536: Were meteorological fields interpolated? If so, how?
- Line 560-562: Are urban Copenhagen emissions, in particular, RWC emissions, not included in the emissions inventory? If so, how is double counting these avoided?
- Section 2.4: Why were these 2 metrics selected for evaluation?

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- Figure 3: Markers are inconsistent, panels are not labeled.
- Line 609-610: This sentence is unclear.
- Line 619-621: The authors should discuss what they believe is the reason for inferior performance at these stations.
- Line 625-628: Is there specific evidence supporting the statement about the resolution being responsible for lower IA?
- Figure 4: The quality of the figure can be improved and reorganized to more easily compare across cities. In the caption, change “long-term” to “annual”.
- Figure 5: Label panels. The color scale is misleading, with some intervals representing a 0.02 ton difference and others 1 ton. This makes interpretation difficult.
- Lines 657-660: This paragraph appears to be unnecessary.
- Line 661: What do the authors mean by “spatially averaged maximum emission values”?
- Line 665: Does this mean the highest emissions within Helsinki and Copenhagen, or across the 4 urban areas?
- Line 712-713: This sentence is unclear: “The concentration distributions from RWC ...”
- Figure 6: Labels are missing on panels. On the color scale, why are some intervals 1 ug/m3 and others 0.5 ug/m3? As mentioned in the major comments, maps of RWC-derived PM2.5 would be more informative than these for total PM2.5.
- Line 740-741: Support this statement with references.
- Figure 7: Panels should be labeled. Again, the color bar is misleading by using varying interval sizes.
- Line 773: Define “spatial ranges”

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- Figure 8: The quality of this figure would be greatly improved by using a box plot (or another visualization of distributions) instead of the overly simple bar plot included. Titles on axes are unclear.
- Line 795: The second reason mentioned does not appear to be identified.
- Line 862: Why are annual average values reported instead of seasonal values if RWC appears to be largely concentrated into colder seasons?

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

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