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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The impact of traffic on air quality in Ireland: insights from simultaneous kerbside and sub-urban monitoring of submicron aerosols" *by* Chunshui Lin et al.

Anonymous Referee #2

Received and published: 7 April 2020

Lin et al. (2020) in this study measured the chemical composition of submicron aerosol (PM1) in two locations in Dublin (kerbside and residential sites) from 4 September to 9 November 2018. Measurements were performed by using an Aerodyne Aerosol Chemical Speciation Monitor (ACSM) and an Aethalometer (AE-16 and AE-33). PMF analysis was also implemented on the data.

In general, the manuscript is well written though the results presented in the main manuscript do not correspond to the entire period of 4 September to 9 November 2018, but to only two weeks of measurements. The authors chose two periods of the available data, with the first one corresponding to a rather clean period (P1) with

low mass concentration of PM1 and another one rather polluted (P2) by local sources (heating purposes).

The study presented is very rich in data and analysis. However, these information are not very clear in the main manuscript or not even included (they are left on the supplementary) making the manuscript weak and less readable, especially in the results section. Additionally, the reading might be challenging as OA sources and changes in concentrations are vaguely discussed and not really explained. Moreover, figures need reshaping as labels are not well positioned or data are not clear due to high values of the axis.

Specific comments:

Line 26: "solid fuel burning..." it should be explained for which period is referred to. Solid fuel burning was not present in both periods.

Line 77: the word "monitor" is missing.

Line 107: The 1 hour interval that was set for ACSM in the residential site is really high. This might lead to the lack of data originating from local sources and may affect the corresponding diurnal profiles of OA sources. Authors chose this in order to reduce uncertainty though in Fig. S1b the measured PM1 mass concentration is quite high reaching values greater than the Kerbside site.

Line 110: How much the collection efficiency was on average?

Line 111: The Rathimnes site is mentioned. It was not described before in the manuscript and is referred also afterwards without any information. Please provide some info.

Line 118: CO measurements are available for kerbside but they are not presented anywhere in the main manuscript.

Line 137: Figure S1 deserves to be in the main manuscript. The overall (2 months period) chemical composition of submicron aerosol (PM1) should also be included for both sites and discussed.

Line 151: Please specify Fig.1 and Fig.2 with Fig.1a, Fig.2a.

Line 168: Fig.3a is mentioned before Fig. 1b, c,d and Fig. 2b, c, d. Please fix this.

Line 173: “(Fig. 1)” Please define to which letter of Fig. 1 the text is referring to.

Line 195: “197.3 μg m-3” This value is not consistent for Fig.1 c and Fig. S1a. Please provide the correct figure/ value.

Line 207: Nitrate can be also present in higher values due to organonitrates (ON). Have the authors checked the fraction of ON to nitrate?

Line 215: “(Fig. S1)” should change to Fig. S1c

Line 226: Fig. 5a is referred before Fig. 4b, c, d.

Line 234: Higher COA mass concentrations could be related to the decrease of boundary layer.

Line 235: Fig. S7 does not correspond to the OOA profile spectrum. Fig. S6a, b deserves to be in the main manuscript instead of Fig. 5a.

Line 235: The authors state that OOA resembles of the less volatile OOA (LV-OOA). What criteria lead to this statement?

Line 243: The authors have constrained with ME-2 tool, 4 out of 5 factors with a relative strict α-value. This leads to two factors (wood and coal) contributing fairly low to OA (4% and 8%, respectively), making the results less robust. How confident are the authors about this constraint? It seems that solution was forced to an erroneous solution, comparing the data to the unconstrained ones, where biomass burning factor contributed for approx..20% of OA.

Line 244: Fig. 4c and Fig. 5a are referred before Fig. 4b and Fig. 5b.

Line 245: There is no such high peak in HOA around 23:00, according to Fig. S11b. It seems that on Fig. 5 the 3rd quartile of the data has been used instead of the average values. This is consistent to the average values given in the manuscript. Again, Fig. S11 deserves to be in the main text instead of Fig. 5c.

Line 253: peat, coal and wood factors contribute one third to the total OA during P2 in kerbside. However, only a line is describing their existence.

Line 254-255: Could this higher OOA levels be transported by another area and not due to SVOCs?

Section 3.3.2: Lacks of discussion.

Line 258: A peat and a HOA factor were constrained with ME-2. Why the authors chose a peat factor instead of a BBOA one from literature? Does this change the results significantly? Also, a COA factor is not present. Have the authors tried to constrain it? In this case how much was its contribution to the OA? Again Fig. S14a,b should be included in the main text.

Line 265: Again 4 out of 5 factors have been constrained with ME-2 tool. The unconstrained solution of this period (P2) for the residential site is not given in the supplementary so no direct comparisons can be made. How confident are the authors about this solution? Does peat contribute that high (30%of OA) in previous studies? Usually, BBOA accounts for that high percentages.

Line 290: Please define R2.

Line 315: The percentage of OA to PM1 is not consistent to the rest of the text.

Section 4. The authors repeat the results as in the abstract and main results with no clear conclusion and “take home message” made by this work.

Tables 1 and 2. NOx should be mentioned in ppb.

Figure 1. Figure should be replaced by Fig. S1 (without Rathimnes station). Fig. 1c

seems to be wrong in comparison to Fig. S1 that includes all the measured data. Labels of elements seem wrong placed, making them hard to be read.

Figure 2. All chart pies should have the same size, the (a), (b), (c), (d) labels should be re-arranged accordingly to the main text. SO4 should be written as SO4 -2. The same applies for NH4, and NO3 (for the entire manuscript).

Figure 3. The (a), (b), (c), (d) should be re-arranged. Current Fig. 3b should go up to 5 μg m-3, so that the trend of PM1 components will be visible. SO4, NH4 and NO3 should be changed according to the previous comment for Fig. 2.

Figure 4. Pie charts should have all the same size. Labels of factors should have some space between each other. The (a), (b), (c), (d) should be re-arranged accordingly to the main text.

Figure 5. The values seem like the 3rd quartile of the data and not the actual average values. Please replace the whole figure according to the above suggestions.

Figure 7. Define SEAI, SCO. Does this figure correspond to data related to your measurements? Labels of sources should have some space between each other.

Major Comments:

Having read the paper I am still not sure what the authors want a reader to learn.

To my understanding, the authors want to empathize to traffic and vehicular emissions and more precisely to the importance of the diesel ones in the kerbside site. The authors claim that “in residential site, traffic emissions were found to have a minor impact on air quality”. Yet, the HOA contribution to OA is the same (18%) for kerbside and residential sites for P2 period, with its mass average concentration to be 1.2 μg m-3 for kerbside and 1.6 μg m-3 for residential, something in contrast to their final statement.

The possession of the aethalometer AE-33 at the kerbside site is not taken into ad-

vantage by the authors. How much of the measured BC is related to traffic and how much to biomass burning? This could help interpreting better the results of the PMF but also the HOA/BC ratio in section 3.5. Have the authors tried to split BC according to its origins and then conclude to resemblance or not compared to other studies?

Peat, coal and wood factors play a major role (33-50% of OA) during P2 in both sites, yet the authors do not discuss their significance or any related mechanisms. The authors should pay attention to combustion sources, since they seem to be of greater importance than traffic.

Also, the PMF solutions for P2 are too constrained. Have the authors tried to constrain less the solution? What are the results?

How oxidized the OA factors are? Could the authors calculate the O:C ratio of each factor? This would be really helpful to the scientific community.

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