

Atmos. Chem. Phys. Discuss., referee comment RC2 https://doi.org/10.5194/acp-2022-436-RC2, 2022 © Author(s) 2022. This work is distributed under the Creative Commons Attribution 4.0 License.

Comment on acp-2022-436

Anonymous Referee #2

Referee comment on "Measurement report: Atmospheric new particle formation at a periurban site in Lille, northern France" by Suzanne Crumeyrolle et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-436-RC2, 2022

This work reports long-term (4 years) measurements of particle number size distribution at an urban site in Lille, North of France. This study aims to better understand the environmental factors favoring/disfavoring atmospheric new particle formation in this urban environment. These studies allow to reduce the lack of knowledge that still exist on the process of new particle formation and their subsequent growth. It is a complex and extended dataset and analysis and the results will fit within the scope of ACP, being of interest for the international research community. However, I would suggest some aspects to be considered in order to improve the manuscript and/or strengthen its impact before it is published in ACP.

Major comments

The dataset presented is of interest for the international community and combine a large number analysis. However, the manuscript is mainly descriptive, the results of each section are not analyzed/discussed in deep and not big conclusions are reached. I could suggest the authors to include more discussion about GR (maybe the contribution of H2SO4 to GRs and/or improve the discussion of possible precursors -comment below-), include Formation Rate analysis, the differences between event and non event days (in deep analysis) and try to investigate the differences on the CS. In this sense, since 1) the introduction is mainly focus on urban areas and CS effect on NPF events and 2) ATOLL has some measurements of aerosol chemical composition (with ACSM), I would also suggest to look if there is some relationship with the chemical composition of pre-existing particles acting as CS (two recent reads about CS efficiency Du et al. (2022) and Marten et al. (2022)). The ACSM measures from ~80nm and it's a good estimation of the CS chemical composition. This links with the fact that BCwb is high during non-event days, and recent study (Yus-Díez et al., 2022) have shown the impact

of secondary aerosol on this quantity.

Minor comments

L36 – "highly // significantly" contribute.

L63 – I would use "NPF event" instead of "nucleation event", nucleation is just the process of formation.

L71 – I would include reference (Dada et al. 2007 fits well).

L94 – Indicate altitude

L97 – This reference is not included in the reference list. Include at least title?

L99 - Rose et al. 2021 maybe fits better?

L104 – volcanic plumes affect the surface levels?

L115 – 3082 is not a DMA, is the classifier. Maybe, DMA would be 3081A?

L123 – reference format and rephrase.

L129 – Which are the properties of the CS? The same properties than the vapour? The loss rate will depend on the molecular properties but also the CS chemical composition. Same line, "which" underlined.

L131,132 – The CS only indicate the loss by condensation, no by coagulation.

L133, 137 – Please rephrase and indicates the meaning of GR15-30 (or don't use it and just indicate that the GR will always refer to the growth rate of 15-30nm).

144,145 – "L.min" change by "L·min". "Wood Burning" not necessarily capital letters.

L148, 149 – indicate what is each instrument measuring and how you use it. About the solar radiation, only global seems to appear in the results sections.

L158 – section is titled "NPF event frequency and Growth rate" but the growth rate is not included in this section.

L169, 172 – I would not compare the undefined frequency with the boreal forest, the environments are pretty different. Have the authors other explanation? Maybe the NPF doesn't growth enough (cut off Dp is 15.3nm)? Similarly for the whole text, the authors compare with pristine boreal forest in many sections, I would recommend compare with other urban environments when possible.

L197 – I would recommend "Aerosol number size distribution", and I would also recommend to remove the "dry" term for the whole text (if you follow ACTRIS guidelines, it is assumed to be dry and you don't have another "wet" smps)

Figure 3 – These figures don't have the time resolution of the instrument. Please indicate in the text the average you have use for the data or provide the plots with the instrument time resolution.

L217 – I would suggest "NPF starting time..." and "growth" instead of "Growth"

L220 – fewer events "starting" in the early morning?

L221 – Authors sometimes use GR and others GR15-30, please use only one (figures included). As I mentioned before, I would recommend to use always GR and state in the methods section that it always refers to 15-30 nm size range.

L231 – "presence of availability"?

Figure 4 – the number of cases for the GR is the same than for the starting time figure?

Lines missing in page 13, but "nm.h-1" again change "." by ".". Temperature "dependence". Finally, in this same page, the authors points to the importance of biogenic emissions, however, the measurements were done in an urban environment probably where higher influence of anthropogenic organic compounds are expected? As I pointed in my major comment, what is the contribution of H2SO4? Should be minimal I guess (even more at this size range).

Figure 6 – I would recommend showing same periods for figures a) and b), if not the reader cannot use both figures information. In this sense, include two boxplot figures (one for each period) or combine solar radiation figures on only one. In addition, here the authors use UTC, and before have been using Local Time, please use always the same and indicate it (e.g. L238). Finally, figure caption is not really clear, I would add c) instead of "b) top and bottom".

L255 – space before reference

L256, 261 – I don't see the link of these reasons with the data shown on Fig 7. RH<40%, these relative humidity values are not observed at your site. High RH limit some VOC ozonolysis, but what about H2SO4–water nucleation? The authors linked in previous section the GR with the increase of temperature, what about the other parameters?

Figure 7 – please use same scale for both periods. Again UTC.

L273, 279 – It is not clear what are you comparing. For event days are larger, maybe yes, but I can not see the overall differences, just the diurnal evolution. Provide the mean or median values, the period of time that you are averaging, ...

L334 – Where can we see that?

L335, 338 – all the undefined cases show a growth that is stopped? What happen the rest of the percentage?

L341, 342 – almost have not talk about HYSPLIT before, I would introduce here why you use it and the objective. Time, UTC or Local?

L347, 351 – I am not totally agree with this. CS seems that doesn't play an important role to inhibit the formation of new particles and probably is more the absence of precursor vapors and/or photochemistry (polluted Beijing: Kulmala et al. 2017, Du et al. 2022 or even strong dust events: Nie et al. 2014, Casquero-Vera et al 2020). Are you comparing only clear sky days (no clouds)?

L373 - NSF6-100 use subindex.

Section 3.7 – have the authors look the nucleation strength factor for the 50-100 nm size range? It could maybe be an estimation of the increase of CCN due to NPF...??

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