

Atmos. Chem. Phys. Discuss., referee comment RC1
<https://doi.org/10.5194/acp-2022-512-RC1>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on acp-2022-512

Anonymous Referee #1

Referee comment on "Characterization of ultrafine particles and the occurrence of new particle formation events in an urban and coastal site of the Mediterranean area" by Adelaide Dinoi et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-512-RC1>, 2022

The MS deals with the properties of NPF events and particle number concentrations in various size fractions at an urban background site in Lecce and at a coastal location in central Mediterranean, southern Italy. It presents valuable results and conclusions and contributes to the growing knowledge on the atmospheric nucleation and consecutive particle growth phenomenon in this larger region. However, the MS could be and should be improved substantially in several ways before deciding whether it is acceptable or not for publication in the ACP. The corrections can hopefully be accomplished by a very careful and thorough revision of the present version.

Major concerns

- The MS is too much of the descriptive character. Lots of simple statistical results are just supplied without interpreting them or putting them into appropriate frameworks or formulating clear conclusions or messages from them. Examples could be large parts of Sect. 3.2, lines 221–228 or Table 3. Further possibilities for improved interpretations could involve e.g. explaining and better comparing the seasonality of NPF events and diurnal concentration patterns of various particle number size fractions. Further important references on urban NPF could also be added to this purpose.
- The SO₂ is often used in the existing interpretations (e.g. in Sect. 3.3). Despite the fact that 1) its photochemical oxidation to the key nucleating vapour of H₂SO₄ was shown to be slow, complex and of less direct influence on the NPF, and 2) the authors possess all necessary properties and variables for deriving the proximity value of H₂SO₄ (which is more directly connected to the process) either by the classical method of Petäjä et al. (ACP, 2009) without the scaling factor or by its improved estimation proposed in Dada et al. (ACP, 2020). The authors may want to amend this part, which could contribute to the improved overall quality of the final MS.
- It was shown in several publications that the size range below 10 nm is crucial for identifying and characterising NPF events in particular in cities (e.g. Nieminen et al., ACP, 2018). The authors are asked to discuss how they avoided the limitations imposed

by their relatively large measurable diameter of 10 nm in Lecce. For instance, how did this fact influence the share of the undefined days.

- The frequency of missing days were relatively large around 22% at both sites. It is wondered how these days were distributed over the years or the measurement campaign since the NPF occurrence frequency showed a remarkable seasonal dependency, which could possibly impact on the representativity of the remaining days.
- The reader can have the feeling at several places (e.g. in lines 154–160) that the nucleation or NPF processes and the particle growth process are not clearly distinguished. For instance, it could be clarified what the authors meant by “the temporal evolution of the events”.
- It is not described properly how some important properties were obtained. An example could be the lines 222–224 where the CS is mentioned only very briefly. By the way, this (in a more detailed extent) should be shifted from the section Results and discussion to e.g. Sect. 2.3 since this is not their result. In this respect, it is also mentioned that the original NSF in Salma et al., ACP, 2017 was further developed, and its NSF_{GEN} and NSF_{NUCL} are more informative than the original form and should be used or at least mentioned. It is not clear (lines 225–228) how the start and end times of the growth events and more importantly, the geometric diameters $Dp1$ and $Dp2$ were derived and whether the latter were modal median diameters or something else.
- Figure 4 (and possibly some others as well) contains too many lines and it is difficult to follow. In addition, it should be discussed whether a local time involving the daylight-saving time (clock change in the EU) or UTC+1 or else were used as the time scale. This could be related to the shift in the positions of the diurnal peaks in different seasons.
- Line 239 and further: the phenomenon or process is somewhat more sophisticated. The authors perhaps want to include and discuss the NPF occurrence with respect to the ratio of sources and sinks of low-volatility vapours and not just the amount of CS alone.

Minor comments

- Line 33: use either primary or emission (source).
- The references should be ordered chronologically and not alphabetically, e.g. lines 36–37.
- Some abbreviations are not explained, e.g. line 101: MPSS, or Table 3 W, Sp, S and A.
- Line 105: is TSI Inc. really based in Rome, Italy?
- Line 239: replace "discouraged" by not favoured or something similar.
- What is the advantage of using a CNR4 net radiometer which measures the energy balance between incoming short-wave and long-wave far infrared radiation versus surface-reflected short-wave and outgoing long-wave radiation to measuring global or direct solar radiations.