

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## **Comment on acp-2021-369**

Anonymous Referee #1

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Referee comment on "Response of atmospheric composition to COVID-19 lockdown measures during spring in the Paris region (France)" by Jean-Eudes Petit et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-369-RC1>, 2021

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The subject manuscript reports on trends in atmospheric composition during the COVID-19 lockdown in Paris, as compared with meteorologically similar days in previous years. The authors develop and apply a 3-layer analog approach for identifying meteorologically similar days in different years; the layers include synoptic (sea-level pressure), regional (air mass trajectory), and local (temperature and relative humidity). Unlike some of the early studies of COVID-19 on air quality, the study includes analysis of secondary pollutants, including secondary organic aerosol, and composition and origin of particulate matter. The focus on the role of meteorology in interpreting air quality measurements is a strength of the paper, and the A3Q methodology can be widely applicable provided data are available. For the manuscript to be of broad interest and of high impact, some revisions are needed. The results presented in section 4 are clear, but this may be of limited interest to broader ACP readership, and more appropriate for a Measurement Report. However, with better characterization of the limitations of existing approaches to account for differences in meteorology (see scientific merit below) and editing to remove unclear language (see presentation quality below), the manuscript may be suitable for publication in ACP.

Scientific Merit:

Significant changes in meteorology would be expected in the transition from winter to spring (i.e., pre-lockdown period and lockdown period) and even from early to late spring (i.e., beginning of lockdown period to end of lockdown period). Thus, the pre-lockdown and lockdown comparisons in pollutant concentrations (Fig. 1) and air mass trajectories (Fig. 2) are important for illustrating the point, but should not be a main focus for justifying the A3Q approach. For example, in lines 171-173 it is stated that the continental sector is under-represented and the oceanic sector over-represented when comparing the lockdown period with other reference periods. This is clear for the pre-lockdown period; however, within the measurement uncertainty there appears to be no statistically significant difference for the continental sector across the other reference years (over the same date range) and statistically different but (potentially) small differences for the oceanic sector in three of the four periods. Similar comparisons for temperature and precipitation for the LP and references periods would be valuable. Minor comment-the date range listed in 161 does not match any of those in Fig. 1.

The authors need to better demonstrate that the A3Q approach provides a significantly better solution than comparing with a range or ranges of previous years over the same days (i.e., see Parker et al. GRL, 2020). It would be useful to see some of the pollutant concentrations presented in Fig. 1 with the results using the different analogs (Fig. 7).

presentation quality:

Clarity of the manuscript could be improved with organizational changes and additional editing. The language in many places is quite challenging, and the main points are obscured or unclear.

The introduction, as written, includes one paragraph about COVID-19 restrictions, perturbations in human activity, and associated opportunities to better understand atmospheric composition and chemistry. Indeed, there have been a large number of studies published on air quality during COVID-19, some of which are cited by the authors. It is suggested that the introduction be reorganized such that these two pieces (motivation and current published studies) be combined as a single paragraph. The introduction could then be expanded to describe the current state-of-the-science regarding air quality in France/Europe and the importance of considering meteorology in air quality studies, which are both relevant to the conclusions and only cursorily described in the second paragraph of the introduction. A more concise discussion of secondary

chemistry and PM composition would also strengthen the introduction and the manuscript as a whole.

There are several statements that are unclear as written. A few examples follow, but this is not an exhaustive list. It is recommended that the manuscript be reviewed for these unclear or ambiguous statements.

Lines 49-51: "Without climatologically representative values, comparisons of concentrations observed during and outside the lockdown periods shall thus free themselves from differences in weather." The authors are making the valid point that care must be taken when comparing air quality measurements over different time periods because much of the variability can be driven by meteorology. However, this specific sentence seems to contradict that point, though it is very difficult to interpret.

Lines 55-58: The authors state- "Air quality shall not be restrained to NO<sub>x</sub>, O<sub>3</sub>, and PM<sub>x</sub> only, and limited number of studies so far has treated air quality as a whole, notably taking PM chemistry into account." The sentences before and after suggest that the authors are referencing the need to consider speciation of PM. Depending on the objectives of a study, this may be a critical aspect, but isn't necessarily a requirement for air quality studies.

Lines 176-177: "...it may also explain why these methodologies are associated to an increase of eg NO<sub>3</sub>, SO<sub>4</sub> and OOA, due to an underestimation of business-as-usual concentrations for LP2020 meteorological conditions." It seems the authors here are suggesting that if the air masses for LP2020 were correctly represented/compared, and if there was no lockdown, then the NO<sub>3</sub>, SO<sub>4</sub>, and OAA concentrations in the reference periods may not have been higher than business-as-usual LP2020. However, there was no business-as-usual LP2020. Maybe the authors mean to suggest that the differences in concentrations, without accounting for meteorology/air mass origin, are not only due to changes in human activity and are likely exaggerated due to fewer air masses of continental origin in prior years.

minor comments:

lines 79-83: The latter part of this sentence is unclear-it is clear that the measured fractions were corrected for collection and ionization efficiency, but it is not clear what it meant by "successfully participated in" intercomparison. Is this for the measurements being reported or is this something that has been done previously?

line 140: Please provide some additional details regarding ZeFir.

line 186: How is "sunny" quantified?

line 227: Why would wetter days lead to enhanced condensation of semi-volatile compounds?

lines 230-231: Annotation (brackets) is unclear (also in Table 2).

section 3.2.4-The presentation of the sensitivity tests is confusing as written, with both "S" used to indicate a scenario and also "scenario".

line 295: "specie" should be "species"