

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-995

Anonymous Referee #1

Referee comment on "PM₁₀ variation, composition, and source analysis in Tuscany (Italy) following the COVID-19 lockdown restrictions" by Fabio Giardi et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-995-RC1>, 2022

Regarding the manuscript entitled "PM₁₀ variation, composition, and source analysis in Tuscany (Italy) following the COVID-19 lockdown restrictions". The manuscript presents new results for the effects of the lockdown period in the particulate-related (mainly) pollution of a region in Italy. Generally, the manuscript is well structured, interesting, and should be considered for publication, but it is in need of major revisions. Some of the decisions regarding the source apportionment analysis need to be discussed more in the manuscript. By saying that, I do not imply that I disagree with the decisions the authors made, but I strongly believe that the readers would benefit if such a discussion were included. Additionally, I believe that sections 3.5 and especially 3.4 need to be thoroughly revised. Section 3.4 is the heart of the manuscript, and it appears extremely rushed, with little to no discussion and just a couple of references to support it. This section needs to be re-written and the results should be compared with that of other covid lockdown period publications. I would also suggest increasing the discussion and comparison to other studies in section 3.3, but that is just a suggestion. I really appreciate the amount of work that was put into the study, regarding the different types of analysis that were done and the data collection, and I feel that it is a pity not to showcase that in the discussion of the results.

Line 86: Why was the pre-lockdown period preferred as the standard for the comparison compared to, for example, the same time period of the previous year? Could the results be affected by the seasonal effect on source emissions (winter to spring)?

Line 122: Why was this protocol used instead of EUSAAR2? Is there a certain advantage?

Lines 134-135: When samples from several sites are pooled together, the assumption that is made is that the number and type of sources is the same at all sites. Even though this is a valid approach that has been proven effective on many occasions in the past, in my opinion, a short discussion of why this is a reasonable assumption for the sites of the study would be very useful to the readers. For example, another approach could be (even though the limited number of samples here makes it hard to implement) to separate the sites into traffic and urban background and do two different SA analyses since traffic-

related source profiles might be different in the two regions. Another option could be to separate the samples as pre and during lockdown and examine the differences in the profiles as well as the contributions. Generally, I am not suggesting that the option used was not good, but I believe it would be good to have a discussion here why it was preferred since there are other options that appear valid and may offer some advantages.

Lines 136-138: Has the rotational ambiguity and the stability of the solution been evaluated?

Line 268: How much uncertainty was assigned to PM?

Line 274: Swaps are observed for displacement only. For BS it would be useful to report the number of BS runs and how many of them correlated to the base case and what was the correlation threshold used. Which species have been actively displaced during DIS ("good" species)? Generally please include the mathematical indicators that prove the goodness of the fit.

Line 280: Please rephrase. Secondary species are formed in the atmosphere, their precursors can originate from a source, not them directly.

Lines 280-282: This statement is unclear. Please explain/rephrase

Lines 287-290: No references are provided here.

Lines 290-292: Could you please explain the presence of Ca and S to the factor? Generally, the discussion here appears rushed, with little to no discussion and no references. The use of the English language is also not on par with the previous sections.

Lines 295-296: Could you please justify that decision? In theory, sulfate is preferable to sulfur since it consists a higher percentage of PM mass.

Line 311: urban or urban background? 99% seems an extremely drastic decrease even for a lockdown! Can this be an effect/artifact of the analysis and particularly due to the fact that the samples from all sites were combined? Providing the time series of the source contributions would help to assess how good the solution is.

Lines 312-314: Please rephrase

Lines 324-327: A 99% decrease is not consistent with a 75% decrease in vehicular volume. As mentioned before the 99% is hard to support and appears to be an artifact of the analysis.

Lines 328-329: This is the reason that comparing the same time period of 2019 to that of the lockdown period of 2020 might have worked better. In this case, you need to take into account the seasonal effects as well.

Lines 335-336: This explanation is not enough. Please try to provide a better explanation for this, or/and compare it to other studies from the same time period in different regions. In the entire section 3.4, which is by far the most important of the entire study there are only 3 references.

Lines 343-344: What does this mean exactly? What could the local natural sources of soil dust be, and why are they different in areas with such close proximity? The source of dust is practically one, resuspension (other such as volcanoes are possible, but it is not relevant to the area). Resuspension can be other natural (wind-related) or can also be induced by vehicular movement. As said before, the discussion in this section appears very rushed, and the explanations provided for the observations are vague.

Lines 357-365: How do the source profiles compare? Any differences there? Comparing the source profiles can be much more interesting than comparing the source contributions.