Response of the reviewers' comments on " *How COVID-19 related policies reshaped organic aerosol source contributions in central London"* by Gang I. Chen et al.

We thank all the constructive comments from two reviewers. The following texts are the response to the reviewers with normal italic font is original review comments, green font for authors' responses, and the *blue italic font for changes in the revised version.*

Review comments

Authors' response

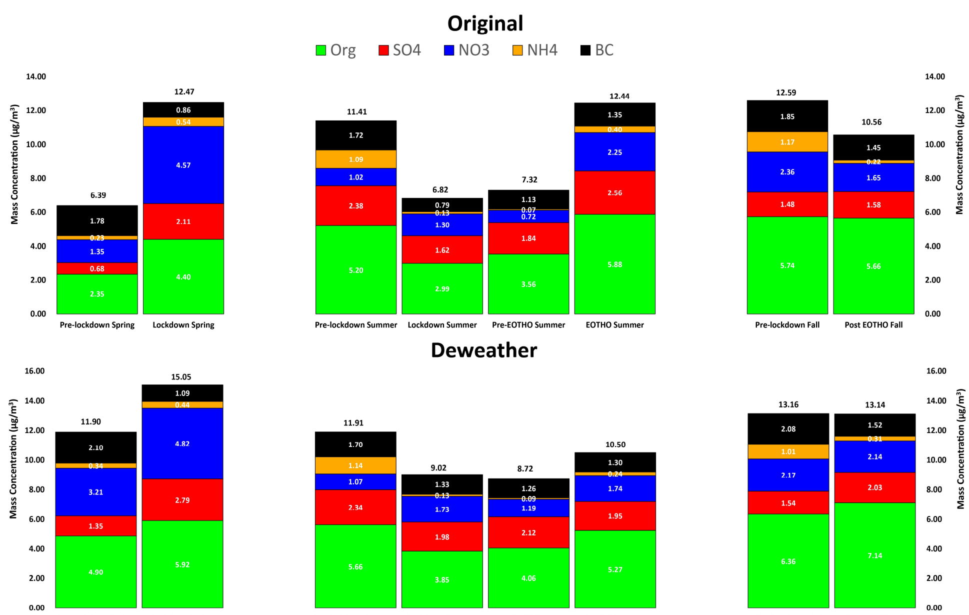
Texts from the revised version

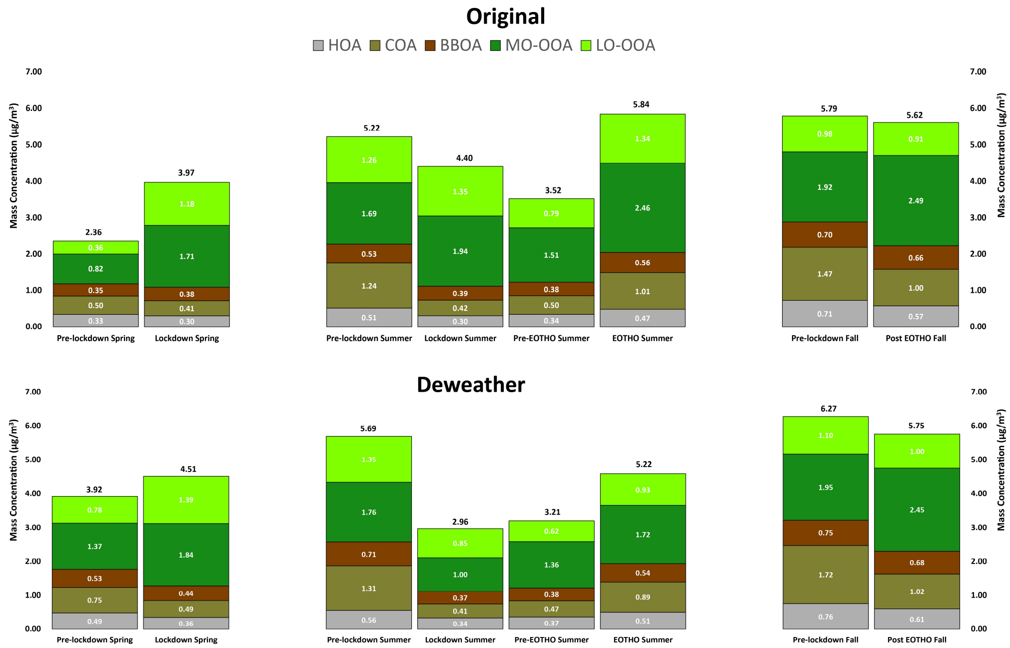
Reviewer #2

Chen et al. present an original study on the impact of COVID-19 related social distancing policies in London, UK on the composition of atmospheric aerosols. Aerosol mass spectrometer measurements combined with source apportionment analysis allowed a dedicated focus on the different organic fractions. They highlight a sharp decrease in primary organic aerosols (eg traffic and biomass burning) during lockdown period (LD), but a significant increase of cooking organic aerosols during Eat Out To Help Out (EOTHO) policies.

In this study, the impacts of COVID-19 policies have been estimated by comparing LD and EOTHO periods to pre-periods, supposed to be representative of business as usual concentrations. After all the flourishing literature on this kind of study, I am rather concerned that the authors didn't take into account (nor at least discuss) the main limitation of this kind of analysis : the variability induced by meteorology. Did the authors check that their "business as usual" periods are representative, meteorologically speaking ? As examples, I don't think the pre-LD period is representative of the meteorological conditions during LD; conditons in June may also be different than in August. Precipitation, temperature, relative humidity, wind speed & direction, BLH (among others) can have a direct impact on the variability and the concentrations measured at a given site, and none of these are presented. This is especially dangerous when stating that component X has increased/decreased by Y% during lockdown (/EOTHO), because these results are very important for stakeholders, and can also be easily understood by non-expert citizens. To this regard, methodology is a critical aspect of the work, and must be as robust as possible. I am afraid that this is not quite the case here. As a main major revision, I suggest the authors to : investigate the meteorological representativeness of the different periods, and discuss the potential impacts on the results. If representativeness is not achieved, I suggest either to change methodology (by taking meteorology into account), or reshape the presentation of the results, by avoiding as much as possible to present numbered decrease or increase results.

We appreciate reviewer's positive comments on this manuscript. We also agree that the meteorological conditions can have significant impacts on the concentrations of PM/OA species. Therefore, we have conducted deweathering analysis using "worldmet" R package (Carslaw, 2025) to minimize the meteorological effects (i.e., RH, air temperature, wind speed, and wind direction). It generally it showed moderate effects for most of periods, except for the pre-lockdown spring period. More importantly, the effects of lockdown decreasing the primary organic aerosols (POA) and black carbon were even more evident for the deweathering analysis compared with the original dataset. Similarly, the EOTHO increased the COA by 88% instead of 100% after deweathering analysis. In addition, given that the data included business as usual seasons without lockdown and EOTHO policies, we have made figures below to isolate the effects from seasonality of the PM/OA species for both original and deweathered timeseries. Again, it demonstrated the evidence of how COVID-related policies had significant effects on both PM and POA mass concentrations as elaborated in the manuscript. Therefore, this study will present the original results by acknowledge the effects of meteorological effects via showing the deweathered results in SI.





Other major concerns :

- Introduction is not well structured. For instance, I don't know why the authors talk about aerosol mass spectrometers and source apportionment here, which is, in that case, more "material & methods" rather than an element of context justifying the interest of the study.

We appreciate reviewer's comment on the structure of the introduction. This part has been moved to Methodology section.

- Section 2.4 is way too long. We don't need a general description of how ME-2 works, the authors need instead to provide all valuable information showing how the final PMF solution was obtained. Additionnally, BBOA has a rather constant contribution to OA throughout the different seasons (around 10-12%), even in summer. I am guessing that this may priorily come from the use of the rolling PMF rather than barbecue-ing or meat cooking. Plus, it is not clear if the authors constrained BBOA in their previous "summer" PMF. Did the authors also unambiguously find a solution with BBOA during summer only (with a profile ressembling to winter BBOA ?) ? The authors may also check BC data (and Angstrom exponent) to support the discussion.

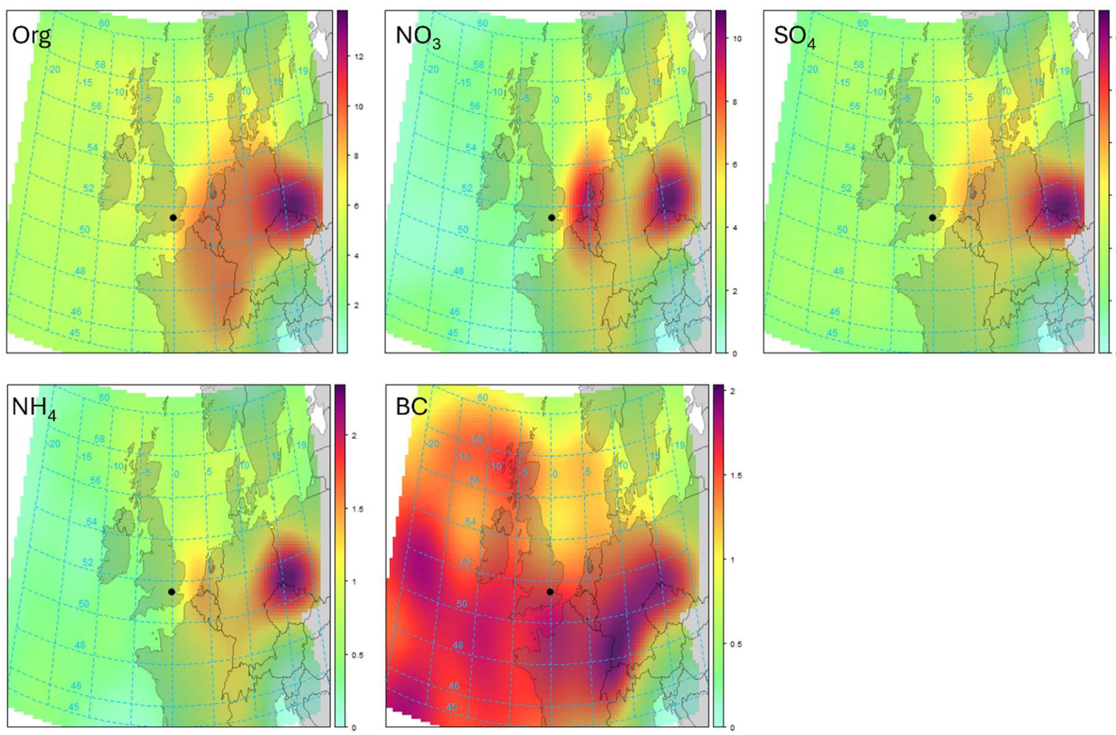
Agreed, general information about PMF, ME-2, etc have been removed now and more details in terms of how source apportionment was conducted have been added in this section.

BBOA was also found in summer PMF in a seasonal PMF analysis in August 2019 as well as the summer 2023 dataset in the same site. It showed a similar diurnal variation as COA since there is evidence showing restaurants in the UK do use solid fuel to cook to meet customers' demands in flavours as a Defra's report suggests (Defra, 2023). With sufficient proof from the Defra report, we think additional analysis on BC data (i.e., Angstrom exponent) is not necessary to support our statement. In addition, in a roadside site, like Marylebone Road, the Aethalometer model to resolve BCwb is highly uncertain when BC is predominantly coming from traffic emissions.

Minor comments:

- Figure S3 (b) and (c) don't quite support long range transport. Maybe trajectory analysis (CWT, PSCF or cluster) would better help. Much more discussion is anyway needed concerning NO3, because it may not only arise from long range transport. Trajectory analysis may also help to look at the occurence of air masses (through eg cluster) throughout the different periods of the study. It would contribute to appreciate their meteorological representativeness.

The CWT of Org, NO3, SO4, NH4, and BC are provided in SI, which confirmed our statement that NH4, NO3, and SO4 are mainly originated from long-range transport. The CWT of Chl is not included as the non-refractory chloride in the atmosphere in Europe is often low (Bressi et al., 2021), therefore, the Cl measurement by ACSM is around detection limit with high uncertainties.



- Can the authors elaborate more on Figure S1, and especially how the different slopes obtained over time may impact the presented results. Is it an issue of ACSM calibration? or FIDAS measurements?

The ACSM RIE calibration factors during the measurement periods were averaged and then applied across the whole period, therefore, these discrepancies were not caused by calibrations, but rather different measurement techniques for FIDAS and ACSM. Therefore, the slope is often composition dependent. For instance, the low ACSM concentration but high FIDAS is caused by high fractions of refractory aerosols (e.g., sea salt) which cannot be measured effectively by ACSM. Also, the high ACSM+BC periods might be caused by the high fractions of ultra fine particles, which cannot be measured properly by the FIDAS. Therefore, the ACSM mass closure practice is a sanity check with independent measurements rather than fully trust one instrument over the other.

- Comparison with literature and previous results elsewhere is clearly missing in almost all result sections.

In introduction, there is comprehensive discussions of general phenomena of the influences from COVID lockdown in previous studies. We do agree, there is little comparison with previous studies in the results section. Therefore, we have added some sentences in section 3.2.2 to mention the similarity and dissimilarity from current study and literature

References

Bressi, M., Cavalli, F., Putaud, J. P., Fröhlich, R., Petit, J. E., Aas, W., Äijälä, M., Alastuey, A., Allan, J. D., Aurela, M., Berico, M., Bougiatioti, A., Bukowiecki, N., Canonaco, F., Crenn, V., Dusanter, S., Ehn, M., Elsasser, M., Flentje, H., … Prevot, A. S. H. (2021). A European aerosol phenomenology - 7: High-time resolution chemical characteristics of submicron particulate matter across Europe. *Atmospheric Environment: X*, *10*, 100108. https://doi.org/10.1016/J.AEAOA.2021.100108

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