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Interactive comment

Interactive comment on "UK surface NO_2 levels dropped by 42 % during the COVID-19 lockdown: impact on surface O_3" *by* James Lee et al.

Anonymous Referee #2

Received and published: 9 September 2020

The authors address the impacts of the COVID-19 lockdown to NO2 emission reductions in the UK, and the possible implications to surface O3 levels. More specifically, they present measurements from 128 urban monitoring stations and compare the 2020 lockdown period, to the 2020 pre-lockdown period, and the same periods from 2015-2019. They follow an approach to deseasonalise and linearly detrend the 2020 data based on the previous years to show that NO2 levels have dropped for various UK cities while O3 has increased.

Although the authors discuss the implications of meteorology to the NO2 concentration reductions these effects are not carefully taken into account. They present meteorological differences between these periods that show higher wind speeds during the lockdown and many times from different directions. A characteristic example is e.g.

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Cardiff that showed increased wind speeds (Fig. 3) and the highest NO2 reductions (Fig. 4) that are currently fully attributed to the lockdown. I would recommend that the authors perform a more detailed analysis of the meteorological conditions and only include the cities that had similar wind speeds, wind directions, and exclude the ones that did not.

When moving to the O3 trends things become more challenging since O3 is strongly affected by meteorology as well as NOx and VOC emissions (the lifetimes of the latter also affected by meteorology). Although O3 formation is complicated the authors seem to oversimplify it and often promote a link between O3 formation and NOx reductions that is not supported at all by the observations. On the contrary, observations promote differences in the UV levels that could drastically increase O3 compared to previous years. Overall, the manuscript would be suited for ACP after (1) a careful exclusion of cities that had different meteorological conditions from 2020 to 2015-2019, and (2) more honest and precise conclusions regarding the increased O3 levels.

Specific comments

Page 1, lines 17-18: "... suggesting the majority of this change can be attributed to photochemical repartitioning due to the reduction in NOx.". The authors did not quantify the effect of meteorology and NOx reductions to be able to conclude this. Please rephrase.

Page 1, line 21-22: Can the authors make this statement without looking in more detail the meteorological differences between the studied years?

Page 1, line 37-39: Where is the remaining 16% NOx coming from? Please provide in parenthesis the variability as $\pm$ XX%. Also, there is no contribution of biomass burning to NOx which especially in the wintertime could play a role.

Section 2.3, line 133-134: "... we first linearly detrend and deseasonalise NO2 data at each AURN site based on the climatology of the previous five years". Please, elaborate

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more and show characteristic examples of data before and after deseasonalising in the main text or SI. It was not clear to me what is shown in Fig. 2 and I had to spend a long time before understanding the de-seasonalisation approach (not 100% sure I still do). This is an important step for this study and is only very briefly discussed. This also includes the associated uncertainties.

Section 2.3, line 147: It is surprising to me that this sudden drop in January-February is suggested to be only due to emerging crises in nearby European cities. The authors later discuss that meteorology is significantly different for these months compared to March-May but still not that drastically different compared to the same months from previous years (Figure 3). I consider it important to understand where this drastic drop in concentration before the lockdown even started, is coming from. This rapid change not related to the pandemic is strong proof that this approach may not work since the needed weight to meteorology or other factors is not accounted for. If differences in meteorology between the 2015-2019 pre-lockdown, and the 2020 pre-lockdown are the reason for this drop in NO2 concentrations (which I suppose mostly is as also discussed in section 3.1) then similar differences during the lockdown (e.g. Cardiff) could play a crucial role in reduced NO2 concentrations.

Section 3.2, line 186-189: The authors already showed how strong influence meteorology could have on the trends based on the pre-lockdown period. If a comparison for the different years was made it should be followed (and weighted) by a comparison of wind direction, wind speeds. For example, Cardiff that has higher wind speeds in 2020 compared to other years (Figure 3) has the highest drop in NO2 which is not due to the lockdown alone. Also, it would be great to see the bars in Figure 4 colored based on the concentrations observed at each site, and with error bars.

Line 209: Is this the mean of all 4 years from 2015-2019? I wonder whether it would make more sense to compare only to 2019. More detailed sensitivity analysis and discussion will improve the presented results here and show whether uncertainties are higher than the observed trends.

Line 227-230: What is the contribution of biomass burning to NOx? The increase in the later hours promotes the possible effects of residential heating. Please discuss the contribution of other emission sources further in the main text.

Section 3.3, line 250: Photochemistry is a key driver for O3 production. However, the authors here don't address the possible effect of yearly variations in photochemistry. Comparing j-NO2 for the different years during these periods would be essential to answering this.

Line 288-311: Aren't the authors suggesting here that the increased O3 is mostly due to meteorology? Please emphasize this more and de-emphasize the O3 increase due to NOx reductions since there is no trend to support this.

Line 313-331: Various sources of VOCs and oxygenated VOCs are not discussed here, e.g. biomass burning, volatile chemical products, industry, that can play a crucial role in determining the total VOCs and total reactivity, and therefore understanding O3 formation. Presented here is not the total VOCs or total reactivity since the discussed VOCs are predominantly related to combustion/traffic emissions. In general, please emphasize more the variability of VOC emissions and that to understand O3 formation NOx and VOC emissions are equally important.

Line 341: Nothing is clear based on the presented results. The authors have no proof that O3 increased due to changes in NOx or changes in meteorology or VOCs. Please rephrase.

Figure 9 is since January although the lockdown was not in effect. How many exceedances happen during the pre-lockdown period? Please separate the two periods and further discuss them if necessary.

Line 427: The increase in Ox can be due to differences in UV levels that will increase OH and O3 levels as mentioned by the authors in the main text. Please rephrase.

Line 436-438: This is a stretch when there is no quantification of the factors affecting

O_3 formation. Please rephrase.

Line 441-443: Strong wording. Please rephrase.

Technical comments:

Page 2, line 49: O_3 is the main pollutant for urban pollution too. Please rephrase.

Page 2, line 78: Change “has” to “could have”.

Page 3, line 96: correct to “levels are”.

Page 4, line 121: delete “and”. Also, an error is provided for the PM2.5 measurements but there is no mention of the type of instrumentation used. Since PM2.5 is not used at all in this study the authors could completely skip this.

Line 202: correct to “increase”.

Line 213: Do you mean “Observed variations in O_3 will also reflect changes in precursor VOC emissions”? Even then, how would that happen? Please rephrase.

Line 240: correct “O_3”.

Line 278: delete “however”.

Figures comments:

Please improve the quality of the figures in the main text and supplement. Also, include uncertainties/error bars to the figures.

Figure 2: Could the authors add the 25th and 75th percentile? Also, could the authors present the results for urban and background environments in the SI for cases where this approach works and cases where this approach is more challenging?

Figure 6: x-axis label is missing.

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