Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-838-RC1, 2020

Atmospheric Chemistry and Physics Discussions

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 #1

Received and published: 6 September 2020

General Comments:

Lee et al. analyze NOx and O3 data at a large set of routine air quality monitoring sites across the UK to assess changes in emissions and chemistry associated with the COVID-19 lockdowns. Their analysis runs through the spring of 2020, so not through the end of the lockdown period, but through the largest reductions in mobility. They compare NO2 and O3 during the lockdown to the historical average from the 5 previous years, and to a projection for the lockdown period based on a fit to the trend in the 5 year data set. They quantify changes in NO2, O3 and Ox based on this analysis, and show that large decreases in NO2 were offset by large increases in O3 at relatively constant Ox at sites across the UK. Further analysis of trends in UV radiation, temperature and isoprene (at only limited sites) showed that any apparent changes in total Ox were

Printer-friendly version

Discussion paper

likely related to these variables than to a response to emissions reductions.

Overall the paper will be of interest to ACP and should be published with revisions. The major comments that the authors should address are as follows.

1. The authors make reference to the influence of meteorology and invoke it to explain aspects of the data set qualitatively, but they refrain from a quantitative assessment of the role of meteorology in the main conclusions. The analysis requires either that they look at the relationships to meteorology in a more quantitative sense, or that they provide some quantitative set of uncertainties associated with neglecting the influence of meteorological variability. There are more comments to this effect below.

2. In several instances (e.g., abstract, NO2 reduced by 42%), quantitative measures of the changes in air pollutants are quoted without error estimates or even measures of variability. Such estimates are required, especially if the work is to be compared to the large body of developing literature using different methodologies on this topic. It is unlikely that the numbers quoted here are exact. Again, there are more comments to this effect below.

3. In a number of instances, either the figures or the conclusions drawn from them are not clear. See comments below to improve readability and robustness of conclusions.

In addition to these comments, the authors should address the following more specific comments.

Specific Comments:

Line 41-42: What is the recent trend in NOx concentrations?

Line 48: The definition is for diameter rather than radius

Line 49-50: Check sentence grammar

Line 52-55: Statement may apply to urban centers, but it is not broadly true. O3 has decreased with decreasing NOx emissions in many locations. See, for example:

Printer-friendly version

Discussion paper

Strode, S.A., J.M. Rodriguez, J.A. Logan, O.R. Cooper, J.C. Witte, L.N. Lamsal, M. Damon, B. Van Aartsen, S.D. Steenrod, and S.E. Strahan, Trends and variability in surface ozone over the United States. Journal of Geophysical Research: Atmospheres, 2015. 120(17): p. 9020-9042.

Cooper, O.R., D.D. Parrish, J. Ziemke, N.V. Balashov, M. Cupeiro, I.E. Galbally, S. Gilge, L. Horowitz, N.R. Jensen, J.-F. Lamarque, V. Naik, S. Oltmans, J. Schwab, D.T. Shindell, A.M. Thompson, V. Thouret, Y. Wang, and R.M. Zbinden, Global distribution and trends of tropospheric ozone: An observation-based review. Elem. Sci. Anth., 2014. 2: p. 29.

Cooper, O.R., R.-S. Gao, D. Tarasick, T. Leblanc, and C. Sweeney, Long-term ozone trends at rural ozone monitoring sites across the United States, 1990–2010. J. Geophys. Res., 2012. 117(D22): p. D22307.

Line 65, Figure 1: The notation in the insets is quite difficult to read and will not be legible on a printed page (readability required >200% magnification on my screen).

Line 66: Figure 1 uses Google mobility data, which are qualitative at best and have shown different reductions relative to other markers, such as traffic counts, in different regions. Can the authors provide another data set, such as traffic counts, or else some statement of uncertainty in the Google mobility data? If not, a caveat should appear re: the use of these data and their uncertainty as a proxy for actual traffic counts. Google and Apple mobility data are easy to obtain but not necessarily the best measure.

Line 115-116: It is useful to have the description of the NOx measurements in this paper and acknowledgement of the potential for interference on Mo converters – several recent analyses of COVID impacts have not addressed this issue at all. For this last statement, however, it is apparent that many of the sites in the network are not urban. What is the bias in using Mo converter NOx for these sites? Would this really be consistent with the 15% accuracy quoted below? How much does this affect the analysis of Ox later in the manuscript? As with other comments, this uncertainty should be propagated through the quantitative measures given later.

Line 133-137: What variables were used for the detrending? Was this purely a seasonal trend, or were standard meteorological and day of week variables used? How well did this fit the 5 year data? The statement regarding emissions changes lacking importance during the 5 year period does not seem consistent with the two noted changes in 2017 and 2019. Would large step changes in emissions be expected, especially if new control measures were implemented in 2019, the year before the lockdowns?

Line 139, Figure 2: What is the measurement frequency or averaging period for the NO2 and O3 data? These appear to be approximately monthly averages? If data are heavily averaged, the figure should also show a variability? Such variability would represent both the variability in averaging to a monthly (?) value at a given site, and the averaging of all sites. It is also not clear that averaging this collection of sites together makes sense, since there are likely large, systematic difference between sites? A relative, rather than absolute, y axis would seem to be more appropriate in this case.

Line 180-181: "Care must be taken" in the comparison due to the meteorological variability. However, there does not appear to be an effort in this paper to correct the data for meteorological variability during the lockdown period. Perhaps this will appear in a later section, but some estimate, at least, of the effect of meteorological differences on the uncertainty of the NOx and O3 changes quoted in the paper is needed.

Line 184-185: Figure S2 is quite difficult to read, even with magnification. The color legends are not legible, and the red lines are nearly invisible. So despite the statement here that the figure shows clear changes, it is not of sufficient quality to do so.

Line 189-194: Were there any obvious differences in meteorology (e.g., just T for example) on the days that were above and below the long-term average?

Line 206 and Figure 4: What is the error bar on the 42%? See comments above – this should be shown in the figure and represent the site to site variability together with the

Printer-friendly version

Discussion paper

uncertainties in the measurements and potential meteorological artifacts.

Line 211: Is it a t-test or a z-test? Use consistent notation and give a definition. If the NO2 data are not normally distributed (which is likely), is the test appropriate?

Lines 214-220: The discussion here is somewhat unsatisfying, again due to the qualitative use of meteorology, which is rather arbitrarily applied to provide justification of a difference when one is not expected, but is not given any weight in the case where a difference is expected due to the lockdown.

Line 239, Figure S4: Same comment as above for Figure S2.

Line 242: Reference is likely intended to Figure 5 rather than 4.

Line 247-250: The previous discussion cites only the effect of titration by NO emission in regulating the response of O3. Here photochemistry is invoked. Is the seasonal photochemistry expected to be strong in this season in the UK? For example, does O3 exceed its background values at this time of year? The discussion re: petrochemical emissions seems quite speculative in the absence of measurement or modeling.

Line 273: Same comment as above re: the use of Google mobility data as a proxy for traffic. Caveats or uncertainties required, but more reliable data sets, such as actual traffic counts, would be preferable.

Line 288-311 and Figures 7 and S7: A 5% increase in Ox in southern UK cities is cited here. Similar to comments above, there is no statement of the uncertainty or variability in this estimate, but the authors should provide one. The changes in Ox appear to be well correlated with changes in UVA for 2020 – in other words, that the increased Ox is plausibly not attributable to reduced NOx emissions. This would also be consistent with the trend shown in Figure 7a, at least qualitatively. Is that the conclusion of this paragraph? It is not clear what is being said here. Finally, the T correlation in Figure 7b is difficult to interpret. What is the line? Is this a fit? If so, it does not appear to represent the data. The T data appear to be more related to the isoprene data in the following section, and so perhaps should be presented with that figure.

Line 321-323: Statement might be true, but it would really depend on the dominant source for VOCs. For example, if VOC were mainly from traffic emission, as NOx emissions are stated to be for this region, then one might expect similar changes in the two. Absent some statement of an inventory and major VOC sources, this statement does not appear justified.

Line 333, Figure 8: Why is isoprene apparently exactly zero in the historical average at Marylebone Rd. for most of the record? Is this a measurement artifact?

Section 4.3: The literature review given here is useful, but it more likely introductory material than it is a conclusion. Suggest moving to the introduction.

Line 436-438: The "warning" regarding O3 is an important statement, but it is not clear that it is justified. The major influence of NOx, if I understand the conclusion of the paper correctly, is in the change in O3 titration, but not in photochemistry (at least not in the winter-spring season studied here). Thus, O3 would go to its background value in the absence of NO titration, while photochemistry would not be strongly affected. Does this scenario really constitute a "warning" that would need to be taken into account to inform emissions reduction policy? The finding is quite different from that referenced in the following sentence regarding O3 in China, where changes are clearly attributable to photochemical processes. The paper should not conflate the two regions, which are clearly quite different.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-838, 2020.

Printer-friendly version

Discussion paper