

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2022-173

Anonymous Referee #1

Referee comment on "Sources of surface O₃ in the UK: tagging O₃ within WRF-Chem" by Johana Romero-Alvarez et al., Atmos. Chem. Phys. Discuss.,
<https://doi.org/10.5194/acp-2022-173-RC1>, 2022

This paper describes a modeling study to investigate the sources of ozone over the UK in the spring-summer period in 2015 using a tagged approach. It is a competent study using an established technique, and while the results are not unexpected, they provide a valuable quantification of source contributions that constitute one of the first available in the literature. In particular, the study highlights the importance of sources outside the region in influencing ozone, and provides a thorough quantification of local and regional contributions across different parts of the UK. The finding that different measures would need to be taken to address ozone as represented by the MDA8 and AOT40 metrics is interesting, and this finding could be exploited better in the paper. It also feels as though model evaluation has been skipped over lightly, and inclusion of a brief assessment to convince the reader of the quality of the model simulations would strengthen the paper. Once these issues have been addressed, along with the points below, I feel that the manuscript would make a valuable addition to the literature and would be suitable for publication in ACP.

General Comments

An original aspect of this study is consideration of impacts over different parts of the UK and using a number of different ozone metrics. Neither of these aspects is fully exploited in the results/discussion section, however. Which regions matter most from a population exposure perspective, for example? Which regions are currently close to regulatory limits? The exploration of different metrics is interesting, but how sensitive are the results likely to be to the meteorology in 2015? Sources in BEL/LUX/NET/GER may be more important than FRA in other years. Some consideration of these issues is needed.

Evaluation of the model simulation is consigned to the supplement, but I feel that something is needed in the paper to convince the reader that the model is up to the task, particularly given that "a good representation of O₃ in the European domain" is expressly stated in the conclusions. Please adapt the existing section 2.4 to provide a more quantitative summary of the model performance, particularly for O₃ and NO_x.

It would also be useful to show a 4-month timeseries of ozone at at least one location to demonstrate the seasonal and diurnal variability (this could be hourly ozone or alternatively daily MDA8). This is important to show the relative importance of episodes, which are investigated in the latter part of the study.

While the manuscript presents a case study from 2015, it would be valuable to speculate on how general the results are likely to be for other years.

Specific Comments

Line 40: narrow concentration window: this might be rephrased, as three orders of magnitude isn't particularly narrow.

Line 49: "European" -> "UK and European"

Line 56: Reductions in European NO_x emissions would be expected to give a reduction in rural ozone concentrations in the UK, as this is far from the source region.

Line 65: As stated, tagged-ozone methods are better than perturbation approaches for attribution studies quantifying the contribution of different sources at a given place/time. However, they are less well suited for quantifying the effect of emission controls which involve changing sources (which is how this concept was introduced in line 60). Some rephrasing is needed to avoid undermining the approach adopted here.

Line 75: Is the tagged ozone mechanism used here existing or new? Please make any novel aspects of the current study clear.

Line 107-8: It would be helpful to add a sentence here to suggest why nudging led to poorer simulations.

Line 126: "The method used here is based on...." Is the Lupascu and Butler approach used here directly or are there any developments or changes in implementation? It is important to be clear about the scientific contributions of the present study. Is any element of this new?

Line 136: How important is reentry of ozone into the model domain likely to be?

Line 151: This sentence does not describe how the contribution of tagged O3 to AOT40 was calculated, it just describes how AOT40 is calculated.

Line 156: Equation 1 is incorrect: $\max(O_3-40, 0)$
Note that this is summed over specific hours, not all hours

Figures 7 and 8 show the same variable (O3 chemical production) and it would be helpful to combine them so that they can be compared more easily.

Figures 9-12: It is not clear that all four figures are required; presenting results for two contrasting months would be sufficient, with the others placed in the supplement. Note that use of contrasting color palettes would allow the reader to separate the inset pie chart more easily, and that separating the legend into two sections would make interpretation of the charts easier.

Figures 13-15 could also be presented a lot more clearly, ideally with the panels arranged in a more geographically-intuitive layout. Flipping x and y axes would make the figures easier to read (so key sectors LB and UK are first rather than bottom of the list), truncating the O3 axis at 25 or 30 would make values more readable, and coloring bars consistent with Figs 9-12 would make contributions stand out better.

Typos and minor issues

Line 88: is -> are

Line 94: citation error "G. a."

Line 100: citation format for Mar et al.

Line 147: established (also exceeds -> exceed)

Line 151: remove "concentration of"

Line 169: units needed for the mean bias

Line 201: remove "from"

Line 260: Remove subsection, as there is no 3.1.2

Line 321: Units on ozone mixing ratios

Line 385: positive and negative bias in what/where?

Line 506: The Romero-Alvarez reference is out of sequence

The coastlines in Fig 1a are drawn at very low resolution, and the figure would look tidier if the resolution was improved. Consider adding the model grid to give the reader an indication of the model resolution.

Fig 6 caption: Closed up -> Close up

Data availability: key output data should be made available through a publicly accessible repository such as CEDA

Author contributions: A clearer statement of author contributions is needed.

Several entries in the reference list refer to discussion papers that are now published (e.g., Lupascu and Butler; Kuik et al.). Please update these.

Lines 798, 818: number not indicated in header, remove comment?

Supplement:

S1.1: Person -> Pearson

p.5: particulatly -> particularly

p.6: Fig 5S-> Fig S5, Fig 4S -> Fig S4

Most of the figures in the supplement are not of publication quality, and the timeseries in particular need to be larger and more clearly labelled so that the comparison of measured and observed concentrations is clearer. In the spatial maps (Figs S6-S8) the results would be much clearer if a more appropriate color scale was used for the difference plots (ideally dichromatic).

I do not find the composition comparison very convincing. While the analysis points to a number of model weaknesses, the causes remain unclear, so the comparison does not lend confidence in the performance of the model. While derived metrics, particularly those based on thresholds, are challenging to match well, I would have expected diurnal variation in NO, NO₂ and O₃ to be represented better.