Review of Korhale et al.

General Comments

This is a nice study of the factors contributing to ozone levels in Ireland. It illustrates the different trends at urban, rural and coastal sites. The addition of the ozone tagging is helpful to understand the contributing factors to these trends. This will be suitable for publication after addressing some points below.

Some of the descriptions around NOx-saturated vs NOx-limited could do with clarifying, particularly with regard to season. It seems reasonable that urban sites will always be NOx-saturated, but it would be surprising if rural sites in Ireland were NOx-saturated outside winter. It would be useful to compare with other studies in similar latitudes (maybe UK?) to see if NOx saturation has been seen in other rural sites. The patterns in figure 3 show rural sites decreasing throughout the spring and summer (except for Laois) which would be consistent with NOx-limited chemistry, but with very strong increases in February which presumably ends up causing a positive annual trend in table 2. Since February ozone rarely causes exceedances, describing these sites as having an increasing trend might imply pollution is getting worse at these sites when it probably isn't getting worse in the peak seasons. Some discussion of why Laois is behaving more similarly to the urban sites would be useful.

Similarly discussion of the contributions of EU and N. American NOx seem to emphasise the importance of NOx titration which is mostly wintertime rather than the increase springtime production.

The discussions in the supplement should be brought into the main paper if they are important, or removed if not.

Specific comments

Line 66-68: This should comment on whether the NAO increases or decreases ozone levels.

Line 82: Is this 40% increase global, EU or Ireland?

Line 183: It might be useful to give the grid resolution in km over Ireland

Section 3 figures: It would be useful to keep the same order of stations throughout the tables and figures, and to group coastal, rural and urban so that a reader doesn't have to repeatedly refer to table 1.

Figure 2: This figure averages over different time periods for different stations. Given the trend in concentrations over time this can distort the comparison. The same period should be used as far as possible.

Line 212: It is not obvious why a single year (2003) has been highlighted for Valentia when all other values are time period means.

Line 277-278: Figure S2 shows coastal increases in February which is conventionally "late winter" rather than "early spring". And then decreases throughout the spring and summer.

Figure 3 needs error bars. I couldn't see the *** markings.

Figure 5: What years are these trends calculated over? Are they the same or different. As with figure 2 the use of different time periods might distort the comparison.

Line 329 to 347: The percentages changes in figure 6 need some uncertainty analysis – are they at all significant? Could much of the change be due to meteorology in 2020 vs that in 2017-2019? Could there be a circulation change that reduced ozone at the coast but increased it inland? Or could changes in insolation enhance ozone production inland but enhance destruction over the ocean? The stronger change at rural sites rather than urban doesn't support NO2 titration being the major contribution to the 2020 increases. The % changes in rural ozone seem very large compared to the NO2 decrease – it would be good to see this in ppb where presumably the concentration change would be even larger. It is not obvious that there is sufficient decrease in NO2 to explain the ozone increase through titration.

Line 342: This sentence needs to be clearer. Is it referring to urban or rural sites? Average NOx concentrations in each need to be stated and compared with literature studies of NOx-limited vs NOx-saturated conditions.

Line 346: Did Ireland have low-insolation and frequent cloud cover in March, April or May 2020?

Line 356: Figure S5 should be referred to here to illustrate the colocation of the grid and the sites.

Line 428-433: This should discuss that the lifetime of ozone is much longer in winter and hence Ireland can receive transport from further distances. Even in winter insolation and temperatures will still be high enough to produce ozone in South Asia. Discussions of transport should cite HTAP 1 and 2 studies.

Figure 10: It would be useful to see the annual average trends shown too. It looks as if this would be positive for both clean and EU sectors. It looks as if most of the slopes are not significant at the $p<0.05$ level. This should be commented on.

Line 462-463: It is not evident that there is a net chemical sink for ozone over Ireland in spring since ozone decreases with decreasing emissions. A dry deposition sink seems more plausible.

Line 464: Decreasing spring ozone from the EU influenced sector is not consistent with the increases in rural O3 during COVID. This suggests that the COVID ozone change was not due to emissions.

Lines 466-467: The scavenging of ozone via NOx titration is only true in the winter months. In spring and summer the decrease in ozone with decreasing emissions indicates net chemical production in the EU sector.

Lines 472-474: It is not clear what is meant by "there is little O3 advected into Europe from the west in the summer months". Is this just saying that there is no east-west gradient in ozone therefore eastward or westward advection will have no effect?

Line 481: How are the clean and EU sectors separated in the model data?

Line 505: It should be explained why the exceedances are higher for the EU sector even through the mean concentrations are lower – presumably the variance is higher, which would be interesting to discuss.

Line 515: Should clarify that the greater decreasing trend is for the EU sector.

Line 539: Add "VOC" after "dominant".

Line 543: The first clause in this sentence seems incomplete.

Figure 13: Explain how the quantify plotted is derived. Is it the mean ozone above 100 ug/m3 summed for each day exceeded. The units on the y-axis must include a time dimension, presumably ppb days or maybe ug/m3 days if the exceedance criterion is in ug/m3? I suggest thinking about the colours used, it is difficult to see which shade refers to which region.

Lines 558-560 The increased urban ozone is likely to be dominated by the local NOx changes rather than from Europe and North America as suggested here. The exception might be winter, when ozone is low anyway. This study hasn't demonstrated that European and North American NOx are important in urban areas.

Lines 560-562: This claim of correlation of exceedances at coastal sites with years of higher spring maxima is not supported by any of the text in section 3.3. Note that most of the exceedances come from the EU sector which would suggest an EU rather than hemispheric source.