

Interactive comment on “Seasonal influences on surface ozone variability in continental South Africa and implications for air quality” by Tracey Leah Laban et al.

Anonymous Referee #1

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The paper describes observations of ozone and other relevant gas phase species made over a number of years in central South Africa and attempts to evaluate the ozone production regime. The paper describes the sites used and the methodology for assessment.

There are few reported observations of air pollution in Africa and this paper provides a useful description of the regional background conditions present in South Africa. In general, the publication is suitable for publication. However, I have a couple of suggestions which might improve the usefulness of the publication. I outline my major comments and then identify some minor issues.

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Source of the ozone. The authors are trying to make the case that CO plays a significant or dominant role in the production of O₃ over south Africa - 'Abstract: It was indicated that the appropriate emission control strategy should be CO (and VOC) reduction associated with household combustion and regional open biomass burning to effectively reduce O₃ pollution in continental South Africa.' They do this through Figure 6 which shows the that trajectories arriving with high CO are the same trajectories with high O₃. They back this up with the arguments from their calculation of ozone production which is essentially ranks the local O₃ production for the different VOCs / CO by their OH reactivity. The difficulty with the trajectories argument is that it provides evidence of a common source (biomass burning) but doesn't necessarily show that the CO is leading to the ozone. Biomass burning is known to emit significant quantities of VOCs and NO_x into an air mass, which overall leads to O₃ production. Attributing the ozone production to the CO specifically from the biomass burning is difficult and probably requires a more detailed analysis than that provided here. Similarly, the local reactivity calculation shows that CO is a significant player in the reactivity, but there are not many datapoints in Figure A2 where the CO is the dominant source of the reactivity. For much of the time it appears that the aromatics, presumably from local industrial activities would outweigh the CO.

Given this, I think that the strength of the comments about the role of CO should probably be toned down. CO is obviously playing a large role here and this is surprising as CO is generally not seen as really leading to regional O₃ production. However, I think a policy of reducing both the CO and the VOCs together is likely the story here rather than an emphasis on CO alone. This would probably change the emphasis of sources from those for CO alone (domestic burning, biomass burning) to include some industrial component which would presumably be the source of the aromatic compounds.

Observed concentrations. It would be useful to provide a basic time series of concentration for the key compounds measured at the 4 sites (O₃, CO, NO, NO₂ etc). The summary plots (Figure 3 and Figure 4) are fine in themselves but it would be useful to

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see the full dataset as this would show the scope of the observations and boost the confidence in the quality of the dataset and the subsequent analysis.

The abstract says that much of region is above 40 ppbv of ozone, whereas the corresponding text (Page 13 lines 15) says that this is the case only in the spring time. Figure 3 would suggest that the observational sites are rarely above 40 ppbv. Can this all be clarified? The color scale on Figure 2 makes it almost impossible to define the color for 40 ppbv. Could this be improved and the color scale on Figure 2 lengthened so that the relationship between colors and concentrations is easier to understand?

Reactivity calculation The reactivity calculation is based on the measured CO and the measured VOCs. It is therefore a lower limit. This should be more explicitly explained. There are some obvious missing compounds in this calculation methane, alkanes, alkenes etc. Their concentrations could be estimated. Would they change the perspective offered on whether the site is VOC or NO_x limited? Presumably not and it would only have a slight tendency to move the data-points in figure 11 upwards but not very much? It would be better to make some comments about this head on rather than ignoring it.

Minor comments. The abstract is rather long. Could this be shortened?

The explanation of ozone production at the top of page 3 is a little confused. It starts of saying that the only way to produce ozone is through NO₂ photolysis but then says that this doesn't make ozone. Can this be re-phrased to be clearer?

Page 4 line 30. It would be useful to explain what the South Africa AQ standard for O₃ is here. It is mentioned in a couple of places in the text but it take us a bit of time to find out what these values are.

The resolution of Figure 7 is rather low. The country names are not clear at the output resolution.

The text at the start of section 3.4 is a little confused. The first sentence says that there

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is an absence of VOC data. The next sentence talks about a two-year dataset. It's not obvious what the first sentence therefore means.

I'm not sure that Figure A1 is necessary. Its comes out of the calculation but its isn't really needed for the calculation of P(O₃) which essentially just uses the reactivity. Just stating the campaign average value and the variability is enough to show that the calculation is giving a reasonable number.

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