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Atmospheric Chemistry and Physics Discussions

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 #2

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This paper reports four sets of surface ozone measurements in South Africa to explore the spatio-temporal variations as well as the major processes affecting surface ozone variability. Although the measurement data are quite valuable and can enrich the global tropospheric ozone observation database, the current manuscript cannot merit for publication at a high quality journal like ACP. The authors are encouraged to revise the manuscript and submit to another localized journal. I have the following concerns and comments for the author's reference.

Major Concerns:

On the significance of this study: the current manuscript looks more like a report other

than an academic paper. Almost all the results and findings regarding the ozone variations and processes are already well known, except for that the data are newly acquired from South Africa (actually some of the data had been reported in previous studies). The authors need shorten the general description and interpretation of the results and elaborate more about the new findings and significance of the present study.

On the writing of the paper: although the organization and writing of the paper is overall fair, the manuscript is too long and contains a lot of very basic information which I presume the readership of the journal has already known. Some discussions are redundant with each other. For example, the abstract and conclusions are very long and should be largely shortened. The second paragraph in the Introduction (Page 3) describes the ozone formation principles which are very familiar with the community. Seasonal variations of ozone were discussed in Sections 3.1.2 (Fig. 3), 3.2 (Fig. 4), and 3.3 (Fig. 5). The authors are encouraged to remove/shorten such general description and focus on the main findings, and write the paper more concisely.

On the calculation of the ozone production rate: the authors should carefully evaluate if this empirical method is applicable to the environmental conditions in the present study. From the equation in the paper, the P(O3) was calculated as the double reaction rates of VOCs with OH. This assumption may only work to some degree for the high NOx and low VOC conditions. And even under such conditions, the ozone production rate might be also largely underestimated as the contributions of the VOC oxidation products to ozone formation are ignored. Furthermore, the empirical calculation of OH concentrations should be also only applicable to rural atmospheres where ozone photolysis is the dominant OH source, and may be subject to large uncertainty in polluted areas where other radical sources such as HONO and OVOCs photolysis become more important. Therefore, the calculation of P(O3) in this study may be subject to large uncertainty that the authors have to address.

On the "CO-limited ozone formation regime": the authors concluded from the O3-NOx-CO relationship analysis that CO played a significant role in O3 formation in South

Africa (or the so-called “CO-limited O3 formation regime”). I highly suspect that this should be not true. In general, CO is less important than VOCs for ozone formation even though it contributes to a significant fraction of OH reactivity. This is because that the contributions of VOCs can be magnified by not only the ROx radical cycle but also the further reactions of their oxidation intermediates and products. The authors are strongly encouraged to utilize the available data of VOCs, NOx, CO and O3 to perform a photochemical modeling analysis to examine the detailed O3 formation regimes.

Other comments:

Section 2.2: it would be better to provide the detection limit and measurement accuracy of the individual measurements. The traditional NO2 measurements may be subject to positive interference from the catalytic conversion, especially in rural and remote areas. The authors need elaborate more about their NOx measurements.

Figure 2: it would be better to highlight the four measurement sites in the present study in the map, and indicate the prevailing wind directions.

Page 13, Line 1: “Marikana” is a typo?

Section 3.1.2 and Fig. 3: it would be much helpful if the measurement results in East Asia can be also compared to obtain a wider spatial coverage.

Section 3.2: on the interpretation of the late winter and early spring ozone maximum, what are the meteorological conditions (e.g. temperature, solar radiation, etc.) during this period?

Page 19, Lines 1-15: the authors attributed the lower ozone concentrations at Elandsfontein to the high-stack emissions. However, the surface ozone in the industrialized areas can be also titrated by the freshly emitted NOx. It would be helpful for the authors to examine the Ox (Ox=O3+NO2) levels to exclude the effect of NO titration.

Page 28, Lines 11-13: from Fig. 11, most the data points fall in the NOx-limited regime zone. This doesn’t support the statement that large part of the regional background of

continental South Africa can be considered VOC-limited.

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