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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Consumption of atmospheric O_2 in an Urban Area of Tokyo, Japan derived from continuous observations of O_2 and CO_2 concentrations and CO_2 flux" *by* Shigeyuki Ishidoya et al.

Anonymous Referee #3

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In this paper, the authors use ∼18 months of continuous atmospheric CO2 and O2 measurements from an urban site near Tokyo in Japan. Using co-located CO2 eddy flux measurements to obtain a vertical diffusion coefficient and the CO2 and O2 concentration measurements from two heights, they obtain estimates of the local CO2 and O2 fluxes, and combine these to obtain the oxidative ratios of local fluxes (ORF) and the wider regional air masses (ORATM). They find ratios that are consistent with gaseous and liquid combustion fuel sources, with seasonal and diurnal variability consistent with wintertime heating and traffic signals respectively.

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Discussion paper

The use of CO_2 eddy flux measurements with concentration measurements of CO_2 and O_2 to look at ORs in a forest environment has been already published by this group, however, this work is novel owing to the urban setting.

On the whole, the paper is well-written and well-presented, and the data appear to be of high quality, which is a worthy achievement for atmospheric O_2. My main concern is the lack of met-related filtering of the atmospheric CO_2 and O_2 data prior to deriving the fluxes. I feel that the data handling as it is currently presented is perhaps too simplistic and should be taken further. I would like to see: a) filtering of the data to exclude periods that are highly influenced by regional not local fluxes (i.e. using associated met data, other tracers, or the concentration measurements themselves); b) more robust quantification of the ORs. While I can see the authors have attempted some robustness by calculating the ORs over two different time horizons (1-day and 1-week), I think this approach is not the best. Usually ORs are most robust during the onset of an atmospheric 'event' but not during the recovery phase when atmospheric conditions are unstable. So I would recommend only calculating ORs during the onset of atmospheric events. Also, a more robust approach to calculating ORs might consider other factors such as wind direction. This might also yield a more in-depth analysis of the OR results. I would also caution the authors about ascribing variations they see in the atmospheric ORs to changes in local fluxes, unless they can discount the influence of seasonal/diurnal atmospheric dynamic effects.

My specific comments are as follows:

Several times in the introduction, the authors mention that ORs can be used to separate out the contribution of different sources to the observed CO_2 flux. I cannot think of a way this would work in reality without additional information (i.e. from bottom up inventories) unless one has a very idealised case with discrete sources coming from very different wind directions, for example. But for most cities, the sources are mixed. Ultimately, the measured OR will be a mixture of all the sources in the footprint, so it could be used to 'check' modelled OR estimates (although two 'wrongs' can also make a ‘right’), but it cannot be used in itself to distinguish CO_2 fluxes from different sources.

Several times, the flux footprint vegetated area is stated at 9% in summer and 2% in winter. The authors should state how these values are derived. They also seem too low, based on the images given in figure 1. If there is a strong seasonal difference in the footprint of the measurements between summer and winter, how are the authors sure that the OR results they obtain are related to changing flux patterns/behaviour and not simply caused by the changing footprint?

Lines 73-74: How did the authors subsample from such a high flowrate without using a tee and causing fractionation of O_2 wrt N_2? Please clarify, since this is an important technical point.

Lines 91-96: I’m not sure that the logic is valid here. Since ORs are calculated from regressing two sets of data, they are most sensitive to inaccuracies at the high/low ends of the scale. So I think the authors might find the uncertainties in OR are larger than the 1% uncertainties at the high end of the CO_2 scale. The easiest way to check is to recalculate some ORs using a 1% difference in the high CO_2 values and see how large the difference in OR is. My guess would be it’s more like 10%.

Lines 115-116: two things here. Firstly, I would caution against attributing changes in the atmospheric data to changes in fuel usage without very strong evidence, ideally from multiple sources. Such changes can sometimes be caused by seasonal changes in atmospheric dynamics or changing footprint, see my comment above. Secondly, winter is usually associated with more boundary layer turbulence, not more stratification. If the authors disagree, please can they provide a citation to back up this statement, which seems to me to be erroneous.

Lines 177-178: I think the authors state here that there was no coal fluxes observed because no ORs were 1.17? If so, I would strongly advise the authors retract this statement, since it is very possible that a mixture of coal and gas could give a ratio that looks like liquid fuel, and yet perhaps there was no liquid fuel burnt at the time. If there is independent evidence for expecting no or very little coal (such as from an inventory) then please provide this here.

Line 194: “on the other hand” used twice in same paragraph. Suggest to rewrite one of them. Or better still to omit entirely, since this is rather colloquial language for such a publication.

Line 202: Suggest to rewrite “seasonal “climatological” diurnal cycles” as I am not sure what the authors mean. I think what is meant is the average diurnal cycle in different seasons.

Lines 219, 221: I would advise caution again here, unless there is independent evidence to back these statements up. It would also be nice to see how much diurnal variation there is in the site footprint, in addition to the seasonal variation.

Lines 237-247 and corresponding text in conclusions: I do not see the value in this paragraph or it’s relevance to the rest of the paper. The authors state that the O2 urban fluxes are very large compared to the global mean O2 fluxes, but the global O2 decrease accounts for O2 fluxes from all urban regions, so I’m not sure what the point of the comparison is. And as the authors themselves state, it is unrealistic that urban O2 depletion would lead to atmospheric O2 falling to levels that are dangerous for human health (perhaps this is possible for isolated indoor environments, but not in the free atmosphere – this has been debunked many times now by many people). I would recommend the authors remove this paragraph and focus solely on the OR analyses.

Figure 2: It is hard to see the seasonal difference of delta O2 and delta CO2 with the current y-axis scaling.

Figure 3: please separate the O2 and CO2 grey data points with more white space so the two time series datasets can be viewed independently/more easily.

Figure 4: do these regression fits account for the difference in measurement precision between CO2 and O2? Also, please state whether the fits account for both x and y

uncertainties.

Figure 6: what are the open circles? The evening peak seems to occur too late in the day to be accounted for by traffic alone (especially in winter). Also, this peak is much broader than the morning peak, suggesting there is a net flux of traffic out of the region over time (whereas presumably this is not the case). I think some more in-depth analysis into these patterns would be useful here.

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