Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-966-RC2, 2020

Atmospheric Chemistry and Physics Discussions
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Interactive comment on "Observed decreases in on-road CO_2 concentrations in Beijing during COVID-19" *by* Di Liu et al.

Anonymous Referee #2

Received and published: 10 December 2020

The authors adopt the idea of investigating urban CO2 enhancements, by reducing the concept of a flux budget approach to a horizontal concentration gradient measurement that typically enters advection flux calculations. The approach would be more meaningful if it was at least combined with a wind field analysis (see: doi: 10.1016/j.atmosenv.2010.02.026, doi: 10.5194/amt-8-3745-2015) and analysis of horizontal advection, rather than using the quite qualitative and subjective argument of 'similar weather patterns'. As it stands, my concern is that the presented findings are rather qualitative. It would be expected that CO2 emissions during the lockdown should drop due to reduced traffic loads. This has been observed and documented before for China. I could see the methodology combined with a more in depth budgetary analysis appropriate for ACP (see references above), but concerns outlined below should be addressed.

A key uncertainty of the presented analysis is that the obtained enhancement comparisons during the lockdown are quite unspecific for quantifying changes of urban CO2 emissions. They are certainly not representative of a city scale change, because measurements are biased towards road traffic. On the other hand the analysis can not tease out exclusive changes due to road traffic either, because these measurements will almost certainly be influenced by other urban CO2 combustion sources (e.g. in the residential, public and commercial sectors) that might have changed quite differently during the lockdown period (e.g. doi: 10.1038/s41558-020-0797-x; doi: 10.1038/s41467-020-18922-7). As such it leaves the reader with a rather vague number for the obtained CO2 decrease during the lockdown. The finding that CO2 changes during a lockdown is not really something new for China. While qualitatively it makes sense that traffic reduced urban CO2 emissions, a quantitative value can not be easily justified, because the observed changes are also influenced by other processes, that are only poorly constrained by the analysis. In particular what is the influence of urban heating devices and other combustion sources, where some studies have suggested increased demand, others have suggested decreased demand during the lockdown? The lack of a weekend weekday effect seems to corroborate this concern. The comparison before, during and after lockdown is qualitative at best, and it is not clear why this hasn't been combined with a more thorough analysis of advection.

Specific comments: Line 19: what about the biological sink?

Line 24: Grammar: means ? - 'the' onroad....

Line 31-32: The enhancement ratio per se does not eliminate the impact of weather. Furthermore even under similar meteorological conditions, turbulent transport in the street canyon could significantly influence enhancement ratios and ambient concentrations. This statement is therefore not substantiated by the presented approach.

Line 38: The cited reference of le Quere does not investigate the dynamics of the pandemic. I suggest to use a more appropriate reference (see WHO literature on that).

Line 45: change to ‘industrial production’

Line 57: change to ‘urban areas’ .

Line 57: specify what you mean by weather changes. . .

Line 64: be more specific about turbulence here – do you mean turbulent mixing and stable conditions? For a statement like that you should specify quantitative parameters such as the Monin Obukhov length or similar parameters to backup your opinion.

Line 66: change to ‘global emission reductions’. . .. and . . . ‘Despite global emission reductions. . .’

Line 75: what means transects and communities?

Line 77: what do you mean by diffusion condition?

Line 80 cc: While enhancement ratios could reduce background CO2 variability there are many more factors in urban areas that could influence the quantitative change of CO2. For example vegetation can reduce CO2 concentrations. I acknowledge that this might not be a huge factor in winter, but the generalization made here is rather bold and certainly an oversimplification of the problem. If the lockdown in Beijing occurred in summer there would definitely be a big influence from the vegetative sink of CO2.

Line 84 cc and figure 1: you start discussing results including a figure in the introduction without introducing the methodology before. This paragraph should be rearranged and moved to the results section.

Line 100: May 9th 2020 serves as a post lockdown reference day, but it was a Saturday – so what justification is there to use this day for a post lockdown reference day?

Line 108: what are reality photos?

Line 143: The reference should actually read Sun et al., (2019) and not Sun et al. (Sun

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et al., 2019)

Line 205-235: It is not clear what statistics was used to determine whether the results are statistically significant. A proper error propagation and uncertainty analysis should be outlined. It is unclear why no error bars are provided here. When I see a value of 477 ppm I wonder how accurate this number really is, given, that some of the data are based on cheap air quality sensor measurements. This is a major weakness of the present form of the manuscript. A rigorous scientific analysis of errors (systematic and random) needs to be included in order to validate the presented results. It would also be good to see the magnitude of natural variability; how much of the observed variability is due to instrument noise and detection limits, and how much is natural variability? This is a fundamentally missing piece of the analysis. Without such an analysis I have doubts that the presented conclusions are justifiable.

Table 3: There is no statistical analysis presented here.

Table 4: there could be a statistical significance test applied to these data. In this context the section on uncertainty analysis is somewhat apart from the rest, and fails to apply a rigorous mathematical approach to analyzing such kind of data in a statistical sense. I would strongly encourage to improve this part of the manuscript.

Line 379 cc (conclusion): the sentence is incomplete and/or grammatically wrong.

Line 385: there is no significant WE/WD effect during COVID, which is a surprise – how different were traffic flow patterns between WE and WD? I suggest to look at typical WE/WD traffic count data and compare these with CO2 results. If indeed there was no WE/WD effect for traffic count data, one would accept this finding. Otherwise it suggests that one needs to be cautious when extrapolating CO2 enhancement measurements along roads to traffic related changes alone.

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