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Atmospheric Chemistry and Physics Discussions

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

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Reply to reviewer's comments:

We appreciate the careful evaluation and many important comments from the reviewers. We made additional analyses and major revisions to the paper, including the following: 1) Uncertainty analysis for observation instruments and significance test 2) The impact of the biological (vegetation) sink. 3) We rewrote and reorganized the on-road CO2 and enhancement results and statement.

Below, we describe these changes in detail and address comments and suggestions point-by-point.

Please see the supplement to this comment for figure details and blue font response.

Reply to Reviewer 2:

The authors adopt the idea of investigating urban CO_2 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 CO_2 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. Thank you for your suggestion. The purpose of this paper is to provide evidence of on-road CO_2 concentration reductions in Beijing influenced by traffic emissions that were substantially decreased due to the lockdown and stay-at-home order. Using budgetary analysis (such as the mass balance method) to calculate CO_2 emissions is beyond the scope of this manuscript. In addition, the budgetary analysis (such as the mass balance method) is good at calculating the emissions for an entire city, not only ground transportation emissions. The trajectories of these mobile observations (such as aircraft) should be around the whole city and far away from emission sources, to obtain the downwind CO_2 concentration and to further calculate the emissions for the whole city. However, the mobile observations in this study were conducted on roads, and the results largely reflected the on-road emissions information. Therefore, it is not suitable to use budgetary analysis (such as the mass balance method) in this study. Moreover, we did not have enough weather data to implement budgetary analysis (such as the mass balance method) right now. Thanks again.

A key uncertainty of the presented analysis is that the obtained enhancement comparisons during the lockdown are quite unspecific for quantifying changes of urban

CO_2 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 CO_2 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 CO_2 decrease during the lockdown. The finding that CO_2 changes during a lockdown is not really something new for China. While qualitatively it makes sense that traffic reduced urban CO_2 emissions, a quantitative value cannot 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. Thank you for your suggestion. The aim of this study is to capture the on-road observation CO_2 decrease signal induced by ground transportation reduction due to COVID-19 restrictions. We agree with you that this article is a more qualitative rather than quantitative study. However, the article is still of great value. Because the main contribution and significance of this paper is that we observed a clear CO_2 concentration decrease induced by ground transportation emission reduction due to COVID-19 restrictions. As we discussed in the Introduction section, although the global emission reduction due to COVID-19 restrictions is very huge, it is very difficult to observe the CO_2 concentration decrease from ground-based CO_2 concentration monitoring (Kutsch et al., 2020; Ott et al., 2020). Therefore, choosing a suitable research target is the key to observing the CO_2 decrease signal.

We agree with you that the on-road observations were inevitably affected by other emissions (e.g. commercial and residential) along the road. However, it is true that ground transportation emissions are the most important signal in on-road observations. Additionally, the enhancement method adopted in this study would reduce the background CO2 concentration fluctuations. “The lack of a weekend weekday effect seems to corroborate this concern”. We believe that the lack of a weekend/weekday effect is a reflection of the COVID-19 restrictions and work times (9:00 – 17:00 LST). This is because of the limitation of ground transportation emissions rather than other emissions (e.g., commercial and residential emissions). During DC evening rush hours, the enchantments were as high as those during BC evening rush hours (Figure 5A, 5B and 5D, 2nd Ring Road). We do not have enough weather data to support us do advection analysis. According to Cheng et al., (2018), the monthly average CO2 concentrations from the 8 m and 280 m height levels differ by a relatively consistent amount. Because there are no strong emission sources, such as factories, near the IAP tower, the CO2 concentration difference is mainly caused by diffusion. Specific comments: Line 19: what about the biological sink? Thank you for your careful review. We added “biological sink” in Line 19 (clean version document). Line 24: Grammar: means ? - ‘the’ onroad. . .. Thank you for your careful review. Revised accordingly. 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. Thank you for your suggestions. We have changed “eliminate” to “reduce”. It indeed does not completely eliminate impacts from weather; however, it could reduce the influence. 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). Thank you for your careful review. We used the WHO COVID-19 situation reports. Line 45: change to ‘industrial production’ Thank you for your careful review. Revised accordingly.

Line 57: change to ‘urban areas’ . Thank you for your careful review. Revised accordingly.

Line 57: specify what you mean by weather changes. . . Thank you for your suggestions, and we added the explanation as ‘for example, high wind speed accelerates the mixing and diffusion of CO2’ in Line60-61 (clean version document). 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. Thank you for your suggestions. We mean “stable conditions”, and we added “in which the planetary boundary layer heights (PBLH) were only ~600 m” in Line68 (clean version document) to back up our opinion. Line 66: change to ‘global emission reductions’. . .. and . . . ‘Despite global emission reductions. . .’ Thank you for your careful review. Revised accordingly. Line 75: what means transects and communities? Thank you for your careful review. We added the detailed explanations for the transects in line80 (clean version document) as ‘for instance, on-road CO2 concentration distributions were presented as transects in urban areas along routes’. In addition, we removed the ambiguous word ‘communities’. Line 77: what do you mean by diffusion condition? Thank you for your careful review. We added the detailed explanation in line81 (clean version document), as ‘for example, wind speed which is directly associated with CO2 mixing and dilution’. Additionally, the ambiguous word ‘diffusion condition’ was removed. 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. Thank you for your suggestions. We added a discussion about the “influence of vegetation sinks”. To understand the CO2 variability impacted by natural sinks (especially for vegetation), we used the dynamic vegetation and terrestrial carbon cycle model VEGAS (Zeng et al., 2014) to simulate the terrestrial biosphere-atmosphere flux (Fta) in Beijing during 2000-2020 (SFigure 3). The model was run at a 2.5×2.5-degree resolution from 1901 to June 2020, forced by observed climate variables, including monthly precipitation and hourly temperature. Although precipitation and temperature in 2020 were higher than the climatology (average of last 20 years), the difference between the Fta in 2020 and the average was within one standard deviation. This suggests that the Fta in 2020 was not obviously unusual compared to that over the last 20 years. We also analysed the CO2 concentration at the Shangdianzi station in the Beijing rural region, which is one of the three WMO/GAW regional stations in China, to determine the CO2 background variation (Fang et al., 2016). The results (SFigure 4) showed that the background CO2 concentration variation mainly induced by natural factors from February to May was only approximately 5 ppm.

In addition, the IAP tower and on-road observations were conducted in urban areas, and vegetation in urban areas is much less than that in rural areas. Therefore, the impacts of vegetation sinks could be reduced by using enhancement. 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. Thank you for your careful review. Figure1 shows the background for the on-road observations, and the data were collected from online news and publications (Liu et al., 2020). We believe that it is not appropriate to move this to the Result section. In addition, we rearranged this figure in the Supplementary Material. Thank you. 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? Thank you for your careful review. At that time, we considered the feasibility of the experiment, including the observation instruments, urban traffic restrictions and personnel arrangements. We have only this one observation after the lockdown. This is indeed a shortcoming of our experiment, and we will improve on this in the future, which is also written in our conclusion. Line 108: what are reality photos? Thank you for your careful review. We changed reality photos to “Real-time panoramic photographs”.

Line 143: The reference should actually read Sun et al., (2019) and not Sun et al. (Sun et al., 2019) Thank you for pointing this out! This was a technical error on our side: it happened when we used Endnote to automatically generate citations in MS word document, we have carefully checked and revised the manuscript. 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. Thank you for your careful review. The uncertainty from the measurement instruments was ~0.1 ppm (from Picarro G2301/G2401), ~1 ppm (from LI-COR LI-7810) and less than 5 ppm (from the low-cost sensor, K30; also there was a validation of 3.6 ppm in method sector). We added the uncertainty for each observed value in the MS. We also added this information to the Uncertainty analysis section. We also conducted a significance test (Table 4): “The CO2 enhancement for BC was also significantly different from that for DC ($p< 0.05$); however, the difference between the AC and BC enhancements was not significant. This suggests that the decreased CO2 enhancement observed during the COVID-19 restrictions was significantly different from those before and after the COVID-19 restrictions.” Four instruments were used in this study and are described in the methods and data sections: 1. Picarro (G2301) in the IAP tower: the precision was ±0.1 ppm, and it was calibrated with standard gas (traced to the World Meteorological Organization, WMO) 4 times each day to ensure that its accuracy was ±0.1 ppm. 2. Picarro (G2401) for on-road observation: the precision was ±0.1 ppm, and it was calibrated with standard gas before departure to ensure that its accuracy was ±0.1 ppm. 3. LI-

COR LI-7810 for on-road observation: the precision was ±1 ppm, and it was calibrated with standard gas before departure to ensure its accuracy was ±1 ppm. 4. Low-cost sensor for on-road observation: the precision was no more than 5 ppm after calibration and environmental correction, and it was calibrated with standard gas before departure to ensure that its accuracy was no more than 5 ppm. We also verified the low-cost sensor, and its data were consistent with Picarro's (RMSE of 3.6 ppm). We added the natural vegetation flux (flux from the terrestrial to atmospheric compartments) variation to the Supplementary Materials to show that the vegetation variation in 2020 was not significantly different from that in the last 20 years. We also adopted the CO2 concentration at the Shangdianzi station, which is one of the three WMO/GAW regional stations in China, to indicate the CO2 background variation (Fang et al., 2016). The CO2 concentration variation induced by natural changes from February to May were approximately 5 ppm. However, these two factors (vegetation flux and natural changes) both indicated an area far larger than that of urban Beijing. Because the location of the IAP tower and the path of the on-road observations are both in urban Beijing, when we used the enhancement method, these factors were reduced.

Table 3: There is no statistical analysis presented here. Thank you for your careful review. We performed a detailed statistical analysis (Table 4), which includes the mean and one standard deviation for different periods and different roads (including a whole trip). Table 3, which was extracted from Table4, presents the main conclusions of this study and is simplified here. We added the instrumental uncertainty in Table 3. 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. Thank you for your careful review. We added a statistical significance test, and found that "After a statistical significance test, we found that the CO2 enhancement difference between working times and rush hours for all trips was significant ($p < 0.02$, assuming that α=0.05). The CO2 enhancement for BC was also significantly different from that for DC ($p< 0.05$);

however, the difference between the AC and BC enhancements was not significant. This suggests that the decreased CO2 enhancement observed during the COVID-19 restrictions was significantly different from those before and after the COVID-19 restrictions." We believe that the deviation listed in Table 4 is very large, which may mislead you and readers. Considering that the instrumental uncertainties in BC/DC/AC are ∼0.2/∼1.1/ less than 6.1 ppm (added in the MS/Uncertainty analysis sector), the deviation here mainly included the on-road CO2 concentration variation. For example, when there is a traffic jam, a large CO2 concentration variation would result in a large deviation. Line 379 cc (conclusion): the sentence is incomplete and/or grammatically wrong. Thank you for your careful review. Revised accordingly. 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. Thank you for your careful review. Revised accordingly. We added the traffic flow map during the COVID-19 restrictions for both WE and WD (SFigure 2). During working hours, all ring roads both WE and WD were smooth.

References: Cheng, X. L., Liu, X. M., Liu, Y. J., and Hu, F.: Characteristics of CO2 Concentration and Flux in the Beijing Urban Area, Journal of Geophysical Research-Atmospheres, 123, 1785-1801, 10.1002/2017jd027409, 2018. Fang, S. X., Tans, P. P., Dong, F., Zhou, H., and Luan, T.: Characteristics of atmospheric CO2 and CH4 at the Shangdianzi regional background station in China. Atmospheric Environment, 131, 1-8, 2016. Kutsch, W., Vermeulen, A., Karstens, U., 2020. Finding a hair in the swimming pool: the signal of changed fossil emissions in the atmosphere. https://www.icos-cp.eu/event/917. Liu, Z., Ciais, P., Deng, Z., Lei, R., Davis, S. J., Feng, S., Zheng, B., Cui, D., Dou, X., Zhu, B., Guo, R., Ke, P., Sun, T., Lu, C., He, P., Wang, Y., Yue, X., Wang, Y., Lei, Y., Zhou, H., Cai, Z., Wu, Y., Guo, R., Han, T., Xue, J., Boucher, O., Boucher, E., Chevallier, F., Tanaka, K., Wei, Y., Zhong, H., Kang, C., Zhang, N., Chen, B., Xi, F., Liu, M., Bréon, F. M., Lu, Y., Zhang, Q., Guan, D., Gong, P., Kammen, D. M., He, K., Schellnhuber, H. J.: Near-real-time monitoring of global CO2 emissions reveals the effects of the COVID-19 pandemic. Nat Commun. 2020 Oct 14;11(1):5172. Ott, L., Peters, G., Meyer, A., 2020. Special virtual panel: Covid-19 and its impact on global carbon emissions. https://carbon.nasa.gov/policy_speaker_28052020.html. Zeng, N., Zhao, F., Collatz, G. J., Kalnay, E., Salawitch, R. J., West, T. O., and Guanter, L.: Agricultural Green Revolution as a driver of increasing atmospheric CO2 seasonal amplitude, Nature, 515, 394-+, 10.1038/nature13893, 2014.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2020-966/acp-2020-966-AC2-supplement.pdf

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