

## ***Interactive comment on “Observed decreases in on-road CO<sub>2</sub> concentrations in Beijing during COVID-19” by Di Liu et al.***

### **Anonymous Referee #1**

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The MS mainly deals with atmospheric CO<sub>2</sub> concentrations measured during 6 on-road observation trips in Beijing, China using mobile platforms before (1 trip), during (3 trips) and after (2 trips) the local COVID-19 restrictions. The topic belongs to the scope of the ACP, it is timely and of research interest. The general levels of the evaluations and discussion, however, should be largely improved. Due to this severe contradiction, which is documented in the comments listed below as examples, the evaluation of the MS cannot be performed unambiguously.

#### Major comments

1. To reduce the weather and background impacts on the atmospheric concentrations, the authors selected the days which were similar to each other as far as the local weather is concerned. They used reality photos collected from the IAP tower, PM<sub>2.5</sub>

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mass concentrations and WS data for this purpose. Looking at the photos and PM<sub>2.5</sub> data, it seems, however, that some days were rather different from the others. The PM<sub>2.5</sub> mass concentrations, for instance, changed from 6 to 169 microg m<sup>-3</sup>. Do these conditions really represent similar weather? (Furthermore, can the latter case indeed be classified as “Light polluted day”?) In addition, one can only wonder why the authors did not use visibility data (possibly available from the AP tower as well) instead of photos, which are demonstrative character only.

2. More importantly, the planetary boundary layer height (PBLH) - which is an important property that can affect the actual concentration of pollutants emitted from surface sources - was not taken into consideration and discussed. The same arguments partially hold for precipitation (and for vegetation activity over the months). All these should be included and addressed in detail in the revised MS.

3. The number of trips (1/3/2) was rather limited. The authors should discuss the representativity of their results and conclusions. Of the 6 trips, there were 5 trips performed on weekdays and 1 trip on weekend. The authors should clarify their statement that “During COVID-19, there was no significant difference between weekdays and weekends.” Could this sentence be made more specific or is the number of trips sufficient for the conclusion.

4. L78-80: The authors state “. . . the enhancement, which calculates the difference in the CO<sub>2</sub> concentration between urban and rural background observations, could effectively reduce the influence of background CO<sub>2</sub> concentration fluctuations to analyze CO<sub>2</sub> concentration characteristics in urban areas. . .”. The IAP tower is, however, located in the city, and there are no arguments why it should be considered as the background environment. The authors refer to its values only as baseline concentrations, which were obtained at the lower or surface levels. One can wonder if the area of the trips and the site of the tower (in particular at lower levels) are influenced by the same environmental conditions. In addition, what is the prevailing wind direction in the area?

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#### Minor comments

5. The MS is extremely difficult to read which detracts from its values. It should be better organized, some strange citation practice (e.g. L58: . . . from Le Quere et al.(Le Quere et al., 2020)), the rounding off strategy (e.g. L51: . . . emissions dropped abruptly by 53.4%), oversophisticated formulations or non-consistent presentations (e.g. Fig. 1, panels C and D: color coding/line representation reversed, it contains dashed and not dotted lines as specified), spelling mistakes (L61: . . . difficult to detect a decrease in the urban CO<sub>2</sub> concentration decrease directly) and frequent redundant repetitions should be carefully revisited and corrected. This all implies that the authors should have paid more attention to finalizing their MS.

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