

## **Review of Li et al. (2025), “Drivers and implications of declining fossil fuel CO<sub>2</sub> in Chinese cities revealed by radiocarbon measurements”**

I reviewed a previous version of this article for publication in ACP. My major concern was that the estimated trend in  $C_{ff}$  concentration was unreliable because the  $\Delta^{14}C$  observations had been taken at different locations/times of the day, and were compared to different backgrounds. Furthermore, given the high variability in atmospheric mixing, it had not been demonstrated how representative the flask observations with an integration time of 15-20 min are.

In this revised version of the manuscript, the authors have addressed the comments I made. They have provided additional information, such as meteorological data and back-trajectory simulations, to help assess the representativeness of their observations. However, what I'm still missing is to discuss the direct impact of potential differences in atmospheric mixing on the observed concentrations. For instance, the authors present standardized anomalies ( $z$ ) of the planetary boundary layer height (PBLH) during the flask sampling times in Dec 2022. They obtained a  $z$ -value of about 0.2, indicating that the PBLH during the flask sampling period was higher than the average PBLH in winter 2021/22 and the climatological winter mean (see Fig. G1). What implications does this have for the observed concentrations? A higher PBLH indicates a larger mixing volume, resulting in more diluted emissions. Therefore, how much would a  $z$ -value of 0.2 alter the concentrations if emissions remained constant over time? When using the observed trend in  $C_{ff}$  concentration to draw conclusions about changes in emissions, it is essential to carefully discuss which part of the observed  $C_{ff}$  trend can be explained by changes in atmospheric mixing.

What has been improved is the harmonization of the dataset. The authors revised their analysis to make the  $C_{ff}$  estimates more comparable, e.g. by using a consistent  $\Delta^{14}C$  background and observations from the same sites when deducing the trend in  $C_{ff}$  concentration. However, this data harmonization means that there are far fewer observations available for the  $C_{ff}$  trend analysis. If I understand correctly, one of their key statements in the abstract (“We found distinct regional trends: megacities like Guangzhou show significant  $C_{ff}$  declines (34–46 % decrease from 2010 to 2022) along with their source regions”) is now based solely on four winter flask observations in 2022. Given the poor statistics and the high variability in atmospheric mixing, I wonder how reliable and robust this statement is.

Beside that, I'm concerned about the strong impact of the  $\Delta^{14}C$  background. When the “NL air” background is replaced by the “NL tree-ring” background, the  $C_{ff}$  estimate at the GZ7 site changes from 16.8 to 11.6 ppm (see Tab. H2). Why are the “NL air” and the “NL tree-ring”  $\Delta^{14}C$  values so different? This needs to be resolved and understood in order to draw conclusions about the  $C_{ff}$  concentration and its trend.

Overall, one must be very careful when attributing the trends in  $C_{ff}$  concentration to changes in emissions. To reliably estimate trends in emissions, an atmospheric transport model is required, e.g. in combination with inverse modelling. This should really be emphasized throughout the manuscript, including in the title (add “concentration” after “declining fossil

fuel CO<sub>2</sub>”). Therefore, conclusions regarding trends in emissions should be tentative rather than quantitative in this manuscript. As also suggested by the other reviewer, the present study could focus more on trends in the R<sub>CO/CO<sub>2ff</sub></sub> ratios, which are less influenced by atmospheric mixing (see my comment on this in the previous report), and could lead to more robust conclusions.

**Specific comments (mainly focusing on the revised parts of the manuscript):**

I. 185-187: This sentence is slightly misleading because, ultimately, you use the correction estimate from Turnbull et al. (2009), which does not take into account biomass burning. Similar is true for the second sentence in the conclusions (l. 630-632).

I. 194-201: Why don't you use these FLEXPART simulations to assess the impact of transport variability on the trends in the C<sub>ff</sub> concentrations?

I. 226-230: R<sub>CO/CO<sub>2ff</sub></sub> is not an “emission ratio” because it is based on the ΔCO and C<sub>ff</sub> concentrations.

I. 229-230: To avoid any confusion, please state here that this approach is applied for sites/times without Δ<sup>14</sup>C observations.

I. 316-318: What do you mean by “the atmospheric trapping of emissions is higher than local emissions”? It seems that the analysis in this section relies heavily on the assumption that the emission inventory is accurate. What if some sources are missing or at the wrong place in the inventory? I suggest removing this analysis or providing clearer, more quantitative validation for this statement.

I. 361-363: How do the meteorological parameters for the flask samples from 2022 and 2010 compare? In order to make the statement that the 'interannual comparisons mainly reflect emission-driven rather than sampling-driven differences', the meteorological parameters for the 2010 flask samples must also be investigated.

I. 365-366: Again, this statement is not valid. The representativeness of the flask samples from 2010 has not been demonstrated.

I. 382-383: A part of the C<sub>ff</sub> concentration difference between afternoon and evening could maybe also be explained by a diurnal cycle in the emissions?

Fig. 4: Are the scattered measurements from Guangzhou missing in panel (a)? I cannot see them.

Fig. 5: Does the top figure in panel (b) show the C<sub>ff</sub> estimates for the harmonized dataset?

I. 546-552: What impact does the background have on the R<sub>CO/CO<sub>2ff</sub></sub> ratios? When a regional background site is used, the R<sub>CO/CO<sub>2ff</sub></sub> ratio may also be affected by emissions from outside the target city. Could this explain some of the discrepancies compared to the I<sub>CO/CO<sub>2ff</sub></sub> ratios from the emission inventories?

I. 727-728: When a regional wind climatology is used the impact of nuclear contamination might be smoothed out. The nuclear contamination may be higher for individual flask events when the power plant plume hits the observation site directly. It is important to note this, particularly as only a small number of flask samples are used for the  $C_{ff}$  trend analysis in Guangzhou.