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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimation of the fossil-fuel component in atmospheric CO based on radiocarbon measurements at the Beromünster tall tower, Switzerland" *by* Tesfaye A. Berhanu et al.

Anonymous Referee #2

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General comments

Berhanu et al. report co-located observations of continuous CO2, CO and 14CO2 from grab samples at the Beromunster tower (Switzerland) for 2013-2016. The variability of the mixing ratio gradients relative to the high-alpine research station Jungfraujoch is discussed, especially focussing on the CO2 offset, which is demonstrably affected by both biogenic and fossil fuel CO2 fluxes. Seasonal, episodic and diurnal variations of DCO2,ff and DCO2, bio are interpreted as well as RCO, which is compared to reported emission ratios for Switzerland. The paper is well written, soundly structured and the

experimental methods are well described. The tools used to interpret the data are commonly used in this field and the results are presented clearly. Unfortunately, the interpretation of the reported results falls short at several occasions. Although plausible, more care has to be given to substantiate the interpretations.

1.) At several occasions local temperatures are given as likely cause of e.g. large positive DCO2, bio due to high temperatures (L341) or high RCO due to unusually cold conditions (L371). If T is such a strong predictor it should be added to the figures or a regression analysis added to the manuscript. The potential impact of PBL variations on reported mixing ratio gradients is also mentioned, but no thorough analysis is performed.

2.) Changes in the area of influence (footprint) are also mentioned as likely causes for specific excursions of the time series, but footprints are not given in the manuscript (or as a supplement).

3.) More broadly, the influence of a (changing) footprint seems to be ignored when the observational RCO is compared to RCO reported in the Swiss emission inventory. Before suggesting that the Swiss emission inventory potentially under-reports CO/CO2 emission ratios, the author need to demonstrate if the observed RCO is representative of average Swiss emissions or how much (and when) RCO is indeed affected by CO ad CO2 emissions from other regions (as mentioned in e.g. L371) plus photochemistry during the trajectory. Overall, the topic is of interest and novel data that could help constrain biogenic CO2 fluxes for (central) Europe can be expected to be of some interest for the ACP readership, if the major comments are addressed.

Specific comments:

L69: The 14C produced in the lower stratosphere is definitely of great interest as it is most easily transported into the troposphere, but 14C is (also/mostly) produced in other altitudes

L165: How did you ensure that samples taken before the leakage was detected were not (slightly) contaminated? Leak sometimes slowly increase over time before they are noticed or was there an abrupt change in mixing ratios or an identifiable mechanical failure?

L167: Please correct to “replaced”

L203: Please note in the text that equation 2 is only an approximation. For the correct mass-balance for 14CO2 small deltas need to be used (big delta includes an isotopic correction term based on small delta 13C)

L222: Here you mention that the correction for 14CO2, bio used cannot (fully) account for short-term respiration changes, yet the daily cycle of CO2, bio is discussed (see Figure 6). Please include a comment to which degree the choice of a simple correction could alter the retrieved CO2,bio in the results/discussions section

L256: Please expand why the 2015 14C emissions Benzau emissions were assumed to be 0 during the shut-down period. The production of radionuclides should be smaller during maintenance, but more possibilities of contamination or release might exist (depending on reactor type and maintenance/intervention)

L274: Why is precision of 10-min aggregates reported for JFJ, while long-term reproducibility was reported for Beromunster observations? How are those two quantities combined into one uncertainty for DCO?

L329: Please consider highlighting periods with southeastern European air masses in Figure 2.

L349: Please clarify: if the reported uncertainties of RCO (summer and winter) in L347 and L348 are correct there is no significant seasonal difference. Hence, the authors should discuss why there is no difference rather than discuss a reason for a non-existing seasonality.

L355: Consider changing to: “The value obtained this way is statistically not different

. . .” This could also be discussed more in terms of its implications.

L371: The authors mention that cold conditions and mass transport from Eastern Europe are likely causes, yet no meteorological data is shown in this paper. Please consider adding a supplement with the key information that you have based this analysis on.

L380: The ratios do indeed differ significantly, but you need to establish why Beromunster-JFJ based RCO should be representative for Switzerland (only). See general comment #3

L407: Colder temperatures are mentioned a main cause, again. If temperature is such a good predictor of DCO2ff a simple scatter plot should suffice to strengthen your argument. Likely an analysis of the impact of the PBL would be useful.

L425: Please consider adding visual aides to highlight the seasonal cycle in Figure 5. It seems not too clear in the printed version.

Table 1: An overall of 45 RCO values is reported, while the study is supposedly based on a 3-years long time series (L451) of bi-weekly samples (i.e. 78). Seven samples were accounted for due to the leakage problem reported in L167. What caused the other 26 to be missing here? Were they excluded, not samples, etc?

Figure 2: Do the dashed lines in 2c and 2d both denote the averages of is the dashed line in 2d just y=0ppm

Figure 5: see comment L425

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