Response to interactive comment on "Estimation of continuous anthropogenic CO_2 using CO_2, CO, $\delta_{13}C(CO_2)$ and $\Delta_{14}C(CO_2)$" by anonymous referee #2

We would like to thank the anonymous referee for his/her helpful input. We have revised the manuscript respectively and describe the changes in the following.

This manuscript by Vardag et al. presents a modelling study of anthropogenic CO2 using simulated CO2 and CO mole fractions, as well as simulated δ13C(CO2) and Δ14C(CO2) isotope measurements, at three conceptual measurement sites representing urban, polluted and rural environments. Overall, the manuscript is well-written, andpresents a thorough analysis of the sensitivity of different types of theoretical measurement site (e.g. rural, polluted, and urban) to anthropogenic CO2. The authors also assess the potential detection of anthropogenic CO2 from various sources at each type of measurement site using different combinations of CO2 and related tracers.
This work will be useful to the atmospheric community, is well suited to the remit of ACP, and following some minor revisions is recommended for publication in ACP.

General Comments:

The title could be improved so that it is explicit that this is a modelling study.

We have changed the title to: Continuous estimation of anthropogenic CO2: model-based evaluation of CO2, CO, δ13C (CO2) and Δ14C(CO2) tracer methods.

This manuscript would benefit from either changing the site type descriptions 'urban' and 'polluted' to terms that are more dissimilar, or a more detailed description of these terms, since it is currently not clear what the difference between these two are, or which measurement site is expected to 'see' more anthropogenic CO2.

We introduced the three stages of pollution in the introduction and clarified which station is rural, urban and polluted in the revised manuscript.

There are a few sentences (e.g. 3rd paragraph of section 2, and lines 24 - 25 of page 20190) where the authors state that a number of fluxes and/or processes have been excluded in the modelling analysis; some extra text justifying the exclusion of some fluxes, and the expected impact of these exclusions on the analysis is recommended.
There are some sections of the text, particularly in the results section, that are difficult to follow, and would benefit from greater clarity.

We have elaborated the effect of neglecting these fluxes in more detail, especially in Section 3.4. We also tried to clarify the text to make it easier to follow.

The authors state that it is not currently feasible to determine fuel CO_2 at rural sites, owing to the high uncertainty to signal ratio typically found at such sites, however, the definition of 'rural' is somewhat subjective. It might be more helpful to provide a minimum detection limit of fuel CO_2, since some measurement sites might be classified as rural, and yet might still detect fuel CO_2 above the detection limit. The authors should therefore exercise caution in their recommendation of revising atmospheric network designs that aim to quantify fuel CO_2, partly because some rural stations might still be suitable if they are located down-wind of large population centres, but also because improvements in understanding/quantification of fuel emission ratios in the near future and improved methods for determining fuel CO_2 may nullify this issue by reducing the uncertainty of fuel CO_2 quantification.

The "signal to noise" ratio depends strongly on the absolute signal at a site since the measurement precision ("noise") is equal for all sites. We have now carefully defined rural, urban and polluted sites.
We agree that in certain cases during pollution events, we can use also rural sites for fuel CO_2 monitoring. We added this comment in the manuscript. We nevertheless want to point out that the current measurement network design (in ICOS, designated to monitor ecosystem signals) and the current measurement precision may prevent a high-resolution monitoring of fuel CO_2 contributions using tracers. We have therefore revised the wording of our recommendations for clarity. In the previous version of the manuscript as well as in the revised manuscript, we emphasize that a reduction of measurement uncertainty would lead to an improvement of precision of fuel CO_2 estimate.

Specific Comments:
The introduction section mentions the current limitations of verifying anthropogenic CO_2 emissions from inventories, however, the authors do not mention atmospheric transport modelling uncertainties, which also contribute to anthropogenic CO_2 emissions uncertainties in 'top-down' verification studies.

We agree and mention the effect of transport model uncertainty for inverse model approaches in the introduction and in the discussion of the revised manuscript.

The time period over which RF is averaged should be stated, as this is not currently clear from the text on lines 16 – 17, page 20185.

In the initial configuration, we look at monthly averaged RF values. We have added this in the text here.

Line 5 of page 20189, section 2, states that 100 particles are released within STILT. This is rather low – has the potential bias of using so few particles been investigated? Is there justification for using so few particles?

We have rerun the model ten times with 100 particles to see how results differ for total CO_2 offset and found a relative standard deviation from model run to model run of about 15%. However, for our model study the accurate representation of the footprint is not important since we are not interested in knowing the true value, but we just want to create a realistic setting. Also, the uncertainty due to a limited ensemble size is random and therefore for longer time series there will be no unidirectional bias. We have chosen to use 100 particles and not more since it saves computation time (proportional to particle number). But, the reviewer correctly points out, a lower number of particles leads to higher uncertainty (increases with square root of particle number). Nevertheless, for the given reasons, we leave the particle number unchanged.

The description of the term ‘footprint’ on lines 8 – 9 of page 20189, section 2, could be improved.

We have elaborated the description in the revised manuscript.

The fact that Δ14C(CO2) is not sensitive to biofuel contributions (lines 10 – 14, page 20193) might be advantageous, if one wishes to determine fossil fuel only anthropogenic CO2. Similarly, the fact that the CO method is insensitive to biofuel might be disadvantageous for some studies that wish to only determine fossil fuel CO2. This point of view should be acknowledged in the manuscript, since many readers will be interested in determining fossil fuel CO2 only, rather than all fuel CO2.

We agree and elaborate this point in more detail in the introduction.

The abstract text does not currently accurately reflect all the key findings/conclusions of the manuscript.

We have assessed the abstract and made small modifications of the structure. We think that the abstract now reflects the key findings of the manuscript.

Technical corrections:
The term ‘short-cycle carbon’ is ambiguous.

We have avoided the term “short-cycle carbon” in the revised manuscript and defined the biofuel CO2 in detail in the introduction.

There are several grammatical errors in the introduction section that should be rectified for greater clarity, e.g. lines 23 – 27 of page 20185, lines 1 – 2 of page 20186, etc. Typing error on line 13 of page 20194.

We have carefully reread the entire manuscript and corrected all grammatical or typing errors, which we could find, including the ones mentioned.