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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric radiocarbon measurements to quantify CO_2 emissions in the UK from 2014 to 2015" *by* Angelina Wenger et al.

Anonymous Referee #2

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This paper makes interesting use of measurements of atmospheric 14C in CO2 in order to attempt to estimate fossil fuel emissions from the United Kingdom. This is an interesting and potentially useful approach and the publication of the data would certainly be beneficial. As far as I can gather, the methodology appears to be relatively thorough and robust. Whilst it is disappointing that the measurement uncertainly appears to prohibit a thorough understanding of the emissions, I still feel that it merits documentation. However, the manuscript in its current state feels disorganised and lacking in detail. It includes a number of vague or confusing sentences that do little to clarify the reasoning of the authors, and the reader is forced to work quite hard in order to follow the science. Unfortunately, it therefore needs a substantial rewrite before it

can be accepted for publication.

Whilst I have described some of the more significant problems below, I should reiterate that much of the paper is hard to follow and more detail is needed in most sections. This is a particular problem where equations have been included. Many of these are difficult put into context, and some do not even use the same variables for what I assume to be the same parameters. I found myself having to refer to the supplied references to understand the context of these equations. The jump from equation 9 to equation 10 is particularly jarring. The reader is forced to work hard to follow the logic here and so much more care is needed.

Some parts of the paper feel a little rushed and the number of mistakes included lead me to wonder if the text has been properly proof-read. For example, Figure 4 is never mentioned in the main text although it appears to be a fairly important model-observation comparison. Some sentences, such as the first sentence of Section 3.3.1, do not make any sense. The values discussed in the first paragraph of Section 4.1 do not seem to correlate with those shown in Figure 2. The locations of TAC and MHD are not displayed anywhere in the main paper!

Finally, I'd suggest that just a little more justification for some of the authors' decisions are necessary. For example, why is the 15th percentile used in order to estimate the background 12CO2 concentration? Why is the CO and a concentration ratio used as a proxy for ffCO2 in the forward NAME runs in the final section instead of using the EDGAR inventory to directly simulate CO2? Does a consistent 40m cut-off for the boundary layer (BL) in NAME model affect the results, or would a BL that varies with the time/season produce different footprints?

I'd stress that the work in this manuscript appears to be good, but more care and time is necessary before it is ready for publication. I'd recommend that the authors make major revisions to the text of the manuscript, but that the paper could be accepted if these are carried out.

Brief suggestions:

Slightly more detail about fractionation in Section 3.2. What exactly is it and why is it a problem?

Include a figure showing an example NAME footprint for the site, and also examples of the emission distributions used with these footprints to create the simulated mole fractions.

Section 3.3 should be expanded as it is currently too brief and confusing. Also, a more detailed description of why the biospheric and nuclear corrections are necessary and how they are applied.

Is Figure 4 unnecessary due to the inclusion of Figure 3? If so, remove it!

For Figure 5, it might be clearer to colour the winter measurements differently from those made during the rest of the year, as these are specifically referred to in the text.

All equations to be checked for consistency and fully explained.

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