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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Stable isotope measurements confirm volatile organic compound oxidation as a major urban summertime source of carbon monoxide in Indianapolis, USA" *by* Isaac J. Vimont et al.

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We thank the reviewer for taking the time to review our manuscript, and provide us with feedback. We have responded to the individual comments below. We have split the reviewer response into comments, which are denoted by "Comment #" We have provided a secondary comment proposing a revised manuscript, titled "Proposed Revisions to Manuscript".

Anonymous Referee #1 Received and published: 13 August 2018 Vimont et al. present

the analysis of previously published CO mole fraction and isotope measurements at three stations in Indianapolis. The evaluation of summer time indicates that photochemical production of CO from BVOCs is a significant source of CO in summer.

Comment 1:

The scientific content of the paper is low.

Response to Comment 1:

We do not agree with the reviewer's assessment of the overall scientific content of this paper. As stated below in more detail in our response to Comment 3, this paper shows isotopic evidence of a large contribution to urban CO from oxidation of biogenic VOC's. We believe that this isotopic evidence provides confirmation of previously published estimates of urban BVOC produced CO that so far has been lacking. We do feel that the organization of the method description and discussion of the results may have pulled focus from the result. After carefully re-reading the paper, with the reviewers' comments in mind, we see that there is far too much detail included in the manuscript, which should be condensed and moved to a supplemental document. We will correct this in our proposed revised manuscript (please see the additional comment "Proposed Revisions to Manuscript" for more detail).

Comment 2:

At least 3 of the 5 figures (2, 3 and 5) were published previously; one other figure (Figure 1) simply shows three of the INFLUX stations of Figure 2 on a satellite image and has no additional scientific value.

Response to Comment 2:

To address figure 1: this figure was included to show the vegetative ground cover in Indianapolis and the positions of the towers, highlighting that BVOC emissions are likely between the towers. To address figures 2 and 3: Figures 2 and 3 were included in an effort to reduce the reader's need to view multiple papers at the same time. They show the reader visually the locations of the towers and their footprints, rather than simply citing the relevant publications and leaving the reader to find the figures on their own. In the revised manuscript, we will replace figures 1, 2 and 3 with a single map, created using a ground cover model, to more concisely show both the locations of the towers, and dominant foliage type. We will provide a citation to Turnbull et al. (2015) in place of figure 3. To address figure 5, we do not agree that this figure is repetitive due to its previous publication. Without this figure, the readers have no visual representation of the actual isotopic data, and we believe it is inconvenient for the reader to have to look to a different publication to see the actual data. We plan to add an additional plot to this figure, showing the difference in mole fraction between the polluted towers (2 and 3) and the background tower. We will also more clearly define the date periods used in this study.

Comment 3:

Also the entire dataset was already published previously, but then only the winter data were analyzed. The "new" part of the present manuscript is that the summer data were also analyzed, which in terms of the dataset means that simple Miller-Tans plots were produced in Figure 4. It was not a good idea of the authors to split the analysis of one dataset into two papers that now are largely repetitive and both have little scientific value. The main result that there is isotope evidence for photochemical production of CO from BVOC in Indianapolis is valuable, but the paper as a hole has for me too little scientific substance to be published in ACP.

Response to Comment 3:

While it is true that the full Indianapolis data set (Figure 5) was included in a previous publication, it was only included at the request of a reviewer for that manuscript. As this reviewer notes above, we made no attempt to analyze the summer data from Indianapolis in that paper, and to date no one else has either. It is not uncommon for previously published data to be analyzed or re-analyzed in subsequent publications.

While it is true that we used the same method to determine the summertime isotopic signature, we argue that this is the most robust method for doing so, and the two papers had separate goals. The earlier publication mentioned determined the isotopic signature of the wintertime at Indianapolis, and argued that this signature was representative of fossil fuel produced CO. It further showed that this signature was significantly different for the oxygen isotope ratio than previous, mostly European studies, which was attributable to different emission regulations.

This publication seeks to quantify the amount of CO produced from BVOC oxidation, and showed that it plays an important role in the CO budget during the summertime in a US metropolitan center. While the importance of BVOC-produced CO as an urban CO source has been previously suggested by modeling studies and inferred indirectly from other types of observations (e.g. Kanakidou and Crutzen, 1999, Turnbull et al., 2006, and Cheng et al., 2017, referenced in the manuscript), to our knowledge, our study presents the most direct and conclusive evidence of this source. As emission regulations have succeeded in lowering urban CO from traffic, the BVOC-produced CO has become increasingly important. We felt this result was important enough and removed enough from the wintertime conclusions to warrant a separate paper.

Nonetheless, please see the supplemental comment titled "Proposed Revisions to Manuscript" posted in addition to the reviewer final responses. We propose to revise the manuscript, and in doing so, address the concerns the reviewer has expressed in this comment.

Comment 4:

The method description and data analysis is presented in a level of detail that is suitable for a thesis, but in my opinion not for a scientific publication. The analysis presented in Tables 2 in relation to the simplification of the CO budget was already performed in the previous publication by the authors, and is only shown in more detail here. The description of methods is very detailed and contains much material that should be considered general knowledge (e.g. the meaning of a correlation coefficient) or is repeated in too much detail from previous publications. The evaluation of the possible BVOC contribution resulting in table 4 is derived from a simple multiplication of an assumed OH level with rate constants and VOC abundances from the literature. It produces a result that is expected and the discussion then connects results from various previous studies.

Response to Comment 4:

After carefully re-reading the paper, we agree with the reviewer in this comment. Each of these points will be addressed in the revised manuscript, and of course, we will provide a second response with the revised manuscript providing the detailed descriptions of how we have addressed these concerns. We plan to submit a revised manuscript with additional data to constrain the oxidized VOC produced CO isotopic signature. Please see a detailed description of this proposed revision in the additional comment titled "Proposed Revisions to Manuscript". Furthermore, much of the discussion of the reactions and BVOC contributions will be moved to a supplemental document, to improve the readability and flow of the paper.

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