

Atmos. Meas. Tech. Discuss., referee comment RC1  
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## Comment on amt-2022-167

Anonymous Referee #2

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Referee comment on "Direct measurement of N<sub>2</sub>O<sub>5</sub> heterogeneous uptake coefficients on ambient aerosols via an aerosol flow tube system: design, characterization and performance" by Xiaorui Chen et al., Atmos. Meas. Tech. Discuss.,  
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This paper provides a modification to ambient flow tube systems developed by Bertram et al 2009 and Wang et al 2018. The authors document the increased robustness due to measurements prior and after the flow tube as well as a box model to predict side reactions. While this is an interesting finding, I do not think this work is novel enough for publication in AMT. This work belongs as a technical note. I also noted that the authors need to credit Bertram et al 2009 more for use of their design. My comments are below.

### MAJOR COMMENTS AND CONCERNS

1. Sections 2.1-2.3, the bulk of the flow tube design, are all pretty much the same as (T. H. Bertram et al., 2009) yet the authors do not cite this paper in these sections for the design. A reader who has not read Bertram et al, 2009 might very well think that these aspects of the design are the authors'.
2. The residence time in the flow tube is quoted at 156s, which seems way too short for low values of surface area.
3. The authors measure very high values of gamma, much higher than observed for most ambient studies: (T. H. Bertram et al., 2009; Riedel et al., 2012b) and most flow tube work with the exception of dust particles (Mitroo et al., 2019; Tang et al., 2016), I wonder if the authors have underestimated the particle surface area by not using an APS or if the filter upstream of the CEAS is causing an artificially higher gamma than expected.
4. It is not clear that the box model presented in this work is a significant advance over the box model presented in Bertram et al 2009 and Wang 2018. The authors need to provide evidence that their work is a significant advance over previous work.

## SPECIFIC COMMENTS

### Abstract

1. "newly developed" on line 19. From the paper, this just seems like a slight modification instead.

### Introduction

2. Line 47, also cite (Gaston and Thornton, 2016; Mitroo et al., 2019; Riedel et al., 2012a, 2013)

3. Line 59, also cite (Cosman et al., 2008; Escorcia et al., 2010; Folkers et al., 2003; Gaston et al., 2014)

4. Lines 62-65, also mention particle size as well (Gaston and Thornton, 2016). Also missing papers on organic aerosol (Escorcia et al., 2010; Gaston et al., 2014; Griffiths et al., 2009; Thornton et al., 2003)

5. Lines 71-74 reflect the findings in (Thornton et al., 2003), which should be cited here.

6. Lines 74-77 reflect the findings in (Mitroo et al., 2019; Royer et al., 2021), which should be cited here.

7. Line 105, Mitroo et al 2019 did not use an ambient flow tube.

### Methods

1. Sections 2.1-2.3 are really the design of (T. H. . Bertram et al., 2009; T. H. Bertram et al., 2009), as such, the authors must use appropriate citations here.

2. Lines 215-217, wouldn't the use of a filter upstream of the CEAS cause issues where wet, ambient particles would react with N<sub>2</sub>O<sub>5</sub> going into the CEAS and cause a higher gamma than one would expect? That might explain the very high values of gamma observed in ambient.
3. Lines 287-288, this duty cycle is not that different from (T. H. . Bertram et al., 2009)
4. Lines 305-308, what fraction of VOCs measured had known rate constants that can be used to parameterize the reaction of NO<sub>3</sub> with VOCs?
5. Section 3.2, it's not clear how this box model differs from the previous studies cited.
6. Lines 437-441 is similar to the findings of (T. H. . Bertram et al., 2009)
7. Lines 489-490, 156 s for a residence time is quite short, especially for low surface areas. What is the time required for complete mixing of N<sub>2</sub>O<sub>5</sub> in the flow tube?
8. Lines 635-639, gamma values seem really high. The authors should provide some explanation of how gamma varied as a function of air mass encountered.

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