

Interactive comment on “Technical Note: In-situ derivatization thermal desorption GC-TOFMS for direct analysis of particle-bound non-polar and polar organic species” by J. Orasche et al.

Anonymous Referee #2

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General comments:

The manuscript of Orasche et al. presents a new technique in which in-situ derivatization is added to an existing direct thermo desorption GC-TOFMS system. This technique is an interesting approach since it provides the opportunity to switch efficiently with relative low time and effort between measurements of polar and non-polar compounds. The manuscript shows detailed comparison of the IDTD-GC-TOFMS measurements with results from SE GC-MS method of urban dust reference material and ambient aerosol samples. This study is of interest for the readership of ACP and I recommend publication after the following questions/comments are addressed.

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Occasionally the author use terms like “...quite good...” and “...somewhat...”. (e.g. page 15268, line 16-19 and 26-27, page 15271, line 12, 16, 22-23, 26). Please be more precise. In some cases when these statements are used the manuscript would benefit if the findings would also be compared to typical values found in other studies in the literature. E.g. can the authors compare their precisions achieved for their SE, IDTD measurements compare to similar conducted studies using DTD and/or SE for this range of compounds? This would allow the reader to better assess how well the novell IDTD technique works. Also especially in section 3.4 on some occasions terms/combinations of “somewhat lower/higher correlation coefficient/variations” are used. Can these variations be quantified to provide the reader with numbers to better evaluate the comparison of the methods?

Specific Comments:

page 15256, line 1-3. In the first sentence the author state that the IDTD-GC-TOFMS was developed for determination of polar organic compounds. However the title of the manuscript suggests a study of “non-polar and polar organic species”. This is confusing. Please revise.

page 15256, line 9 (see also page 15260, line 12-13 and page 15265, line 25-26). Does the derivatization speed/rate really increase by heating to 300C? If possible measurements should be provided to back up the authors statement.

page 15257, line 12-13 What does the authors mean with “particle fractions”? To my knowledge the TAG instrument samples and collects aerosol samples (using a PM2.5 cyclone) not fractions of it. Please explain.

page 15258, line 9-13 The author should discuss briefly why they think it is “essential” to have in general derivatization steps. Common techniques exists (e.g. GC and 2d-GC using columns with a highly polar mobile phase, HP-LC etc.) which can detected reliably and quantifiable many polar compounds. The pros and cons of these techniques compared to derivatization should briefly be mentioned. What are the benefits

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of IDTD compared to these methods?

page 15260, line 7-9. Could the author explain briefly how the moisturizing of the filter is done? Is also the saturated carrier gas used to stream over the filter using a low injector temperature or is this a separated automated introduction of MSTFA through different means? This is also interesting to know in conjunction with my comment below (page 15266, line 8-11).

page 152663, line 1-11. The last paragraph of section 2.4 and section 2.5 are almost identical. Please revise these two paragraphs.

page 15266, line 8-11. This might be an interesting question to discuss. Did the author tested if polar compounds degrade before they are derivatized using only the MSTFA saturated carrier gas stream to flush it over the sample while heating the injector? It would be very interesting to know if high derivatization yields could be achieved using only a saturated carrier gas stream starting with a cold injector for some time and then subsequently heat it up.

page 15266, line 12-14. This manuscript doesn't seem to provide data which actually shows that the MSTFA saturated carrier gas flow protects the derivatized compounds. This statement should be supported by data.

page 15271, line 1-5. The explanation of a correlation plot is not necessary. The part should be deleted.

page 15271, line 15-17. The author state "Retene exhibited a good comparability but a somewhat lower correlation coefficient. Experiences indicate higher variations of retene when analysed by thermal desorption methods." Please provide a reference to back up this statement.

page 15271, line 12. I assume it should read "IDTD" instead "DTD"

page 15272, line 14-17. It would be instructive to briefly add information how LOD (e.g. S/N greater than 5) and LOQ (e.g. 10xLOD) was derived.

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page 15272, line 17-21. I'm wondering why the correlation for mannosan (0.826) is as bad as galactosan (0.83) with mannosan having 10 times more signal. The authors state this is due sterical hindrance resulting in lower derivatization yields for galactosan and mannosan. However I would have assumed that with a signal ten times higher the effect would have been less for mannosan resulting in a better correlation coefficient. It would be good if the authors could provide additional discussion to explain their statement further.

Figure 1, injector blow-up. The text describing the different parts of the injector should be larger. Please also indicate where the MSTFA inlet is.

Figure 2. All peak annotations, axis descriptions and values are almost not readable. Please revise.

Figure 5a,b,c: Please say something about errors of the measurement shown and add error bars to the graph or give an error bar cross in a corner of each graph. This allows the reader to better assess how well the data points scatter around the line through the origin.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 15255, 2011.

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