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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Determination of n-alkanes, PAHs and hopanes in atmospheric aerosol: evaluation and comparison of thermal desorption GC-MS and solvent extraction GC-MS approaches" *by* Meng Wang et al.

Anonymous Referee #2

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This manuscript presents results from an intercomparison of two analysis methods for organic aerosol, which mainly differ in the sample preparation procedure, i.e., thermal desorption (TD) and conventional solvent extraction (SE). Both methods were applied to a set of ambient aerosol samples from 4 different cities in China. The resulting chemical speciation data are interesting, but need further evaluation and discussion. As the main focus of the paper is on the comparison of the analysis methods, the conclusion of the study that the TD approach produces comparable results to the tradional SE method is a good finding, especially in light of the fact that the TD method is a

"greener" alternative. Nevetheless, various concenptual and technical issues need to be addressed by the authors prior to publication of this manuscript, as detailed below.

Specific comments:

1. Page 2, Lines 22-22: The authors state that comparison studies between TD and SE methods are still needed, but some have been reported in the literature.

2. Page 3, Lines 13-14: Did the internal standard function as both recovery and co-injection standard?

3. Page 3, Lines 14-16; Page 5, Lines 3-4: Why did the authors not use direct liquid injection for the analysis of the standards? It makes sense to have the same matrix as for actual samples analyses, but for the determination of e.g. the extraction efficiency it might have been better to directly inject the standard solutions into the GC-MS.

4. Page 3, Lines 27-28: Was such long baking time really needed for the TD tubes? And at what temperature was the glass wool baked?

5. Page 4, Lines 27-28: What type of blank was analyzed – filter, trip, field, etc. blank?

6. Page 5, Lines 3-4: Did the authors consider other factors, aside from vapor pressure, which might have influenced the temperature effect? Using n-alkanes to investigate this effect may not adequately represent the temp. effect on other compounds.

7. Page 5, Lines 15-19: The pyrolysis effect mentioned here may not apply to PAHs. In fact, such relatively low TD temperature may not be sufficient to recover higher molecular-weight PAHs. The authors may want to comment on how the temperature effect may be different for different compound classes.

8. Page 6, Lines 11-13: It would be helpful for the reader to see the individual values, which could be placed in the supplementary materials.

9. Page 7, Lines 1-6: This data interpretation is too crude! Why don't the authors report CPI values, as well as Cmax?

10. Page 7, Lines 19-20: Likewise, the conclusions from merely comparing ambient levels of these PAHs are too speculative.

11. Page 7, Lines 25-28: Why do the authors attribute these findings solely to coal combustion? How about combustion of oil? And do the authors have emission factors for coal combustion?

12. Page 8, Lines 12-13: Those high observed levels in Beijing can't be due just to coal combustion, and certainly have a significant contribution from vehicular traffic, as the city has a large vehicle fleet which is rapidly increasing.

Technical corrections:

1. Page 1, Line 32: Change "efficient" to "coefficient".

2. Page 7, Lines 12, 14: Say "PAH" rather than "PAHs".

3. Page 7, Line 32: Please, state whether these concentration numbers are for individual species.

4. Page 16, caption for Figure 2: Change "which" to "with".

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