Anonymous Referee #2

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The manuscript describes the test of an amine permeation source and the measurement of gas-phase amines from two field campaigns. This is certainly an interesting topic given the importance of low molecular weight amines in new particle formation and the lack of field measurements. Accurate quantification of gas-phase amines is a challenging analytical task because these species are very sticky and typically present at ppt level. Therefore, the present paper will be a useful addition to the literature. My judgment is that it does, however, require substantial revision and major modification (see comments below) before it could be considered for publication in AMT.

Major comments:

1. Quality control is missing in this manuscript, which in my opinion C1422 is very important info especially for quantifying sticky species at low concentration. The authors should include an estimate for reproducibility and accuracy of both the amine permeation source and the AmPMS measurement, as well as the limits of detection for AmPMS.

We will add a few sentences at the end of section 3.1 regarding PT variability. The Supplemental will be made more clear on this issue. Detection limits are now presented in the Supplemental which addresses in part the general comment that 'quality control is missing'. We feel this comment has been addressed also in our revised presentation of the stickiness issues.

2. The wall loss: a very long sampling line was used in this study (30-100 cm for laboratory study and up to 6 m in the field). Given amines are notoriously sticky compounds, significant wall loss may occur in the sampling line, instrument inlet, ionization source etc. The extent of wall loss may also be affected by the temperature and relative humidity. More important, those amine lost to the wall may randomly re-emit back to the sampled air leading to cross-interference. I wonder if the authors consider this issue. Why did they use such long sampling line? What material is the sampling line made of? The authors should clarify these points.

We have worried about this issue. However, the ambient data shows single digit detection of amines, we cannot escape that. Furthermore, we have worked on the Supplemental to emphasize strongly that the delays seen in the data are primarily from surfaces in the instrument and not due to the inlet. We have shown further that delays due to the inlet can be significant for methyl amine but trimethyl amine does not seem to be affected (at two different levels: 2000 pptv and 400 pptv.) We assume that delays are not dependent on amine level and this will be clearly stated in section 3.3.

3. The manuscript is not well organized, and the results are not discussed logically and properly. For example, 1) the 'title' does not properly reflect the main work discussed in the manuscript; Title will be changed (as per ref. 1) 2) in the 'abstract', the major focus should be amines instead of e.g., DMSO; We will eliminate the second mention of DMSO in the abstract. 3) in the 'introduction', the authors should at least discuss briefly AmPMS; 4) in the 'experimental', a separate part describing AmPMS and the field campaign is definitely needed. A paragraph with a short description of AmPMS will be added to the experimental section 2.2 and a longer paragraph with more details in the Supplement.

Specific comments:

1. Page 3836, line 26, 'short'!highly. Will do.
2. Page 3837, line 4, 'apparati'!apparatus. Will do.
3. In the introduction part, I am not sure if the authors would like to focus more on the permeation generator? But I think a brief summary of the measurement techniques including AmPMS would certainly be useful for the readers to understand why they

develop this approach. We will add a sentence citing Hanson et al. 2011 where amine mass spectrometer techniques and the origins of AmPMS are discussed.
4. Page 3838, line 8-12, the amines are very volatile and sticky. How constant are
C1423 the amine calibration vapors generated without careful temperature control? (They were generated at room temperature. I think temperature changes from daytime to nighttime will significantly change the equilibrium of amines in the permeation device and therefore the permeation/diffusion rate. Once the equilibrium is interrupted, it may take hours to reach new equilibrium).

We have stated in the Supplemental that a +/-25 % variability was found without temperature control. We will add this sentence also in the text as per this referee's 1st major comment.

5. Page 3841-3842, from what I understand, the major focus of this work is about amines. However, the authors mislead the readers by spending too many efforts on other species. For example, Figure 1 and 2 discuss NH3, why not amines??
We will change Figure 1 to include both NH3 and DMA titration results.
6. Page 3844-3846, for field measurements, did the authors calibrate their AmPMS in the field with the permeation sources? The influence of RH on the field measurements should also be included. Some calibrations were done in the field as shown in Table 2. Our previous paper showed that little RH dependence is expected and, while not explicitly controlled for in the calibrations, none has been observed.
7. Page 3859, figure 3, 'before, during and after addition..' should be clearly marked in the figure. I assumed this temporal plot was from laboratory study, this should be clear in the figure caption. We will add these to the figure.
8. Page 3860-3861, figure 4 and 5, these two figures are not readable at all.
We apologize. They do certainly look better on a full sheet of paper. We will certainly present these data in a better format.
Interactive comment on Atmos. Meas. Tech. Discuss., 7, 3835, 2014.