The authors thank the referee for his/her constructive comments which help to enhance the manuscript. Our point-to-point replies (in blue) to the comments are given below (the original comments are copied here in *Italic*). The manuscript has been revised accordingly. All the changes to the manuscript have been highlighted using the Microsoft word "track-changes" tool in one version of the submitted revised manuscript.

Anonymous Referee #2

This manuscript for the first simulates the global distribution of DMA/methylamines concentrations, and discusses its impact on new particle formation. The work is novel and can be published in ACP, while i have a few major comments for the authors to consider, as appended below:

1) It is not clear to me, that why DMA is selected as the model amine. is it because there is relatively enough info available in the literature for DMA than other amines?

We choose DMA partially because of recent CLOUD chamber study showing the impact of DMA on nucleation. Following the referee's suggestion (below), we have carried out simulations for monomethylamine (MMA) and trimethylamine (TMA) and expanded the manuscript to include MMA and TMA.

2) The discussion based on the simulated results is somehow weak at its present form, can be expanded accordingly.

After adding results for MMA and TMA as well as budget information, we have substantially expanded the discussions on the simulated results.

3) The simulation uses the spatial distribution and seasonable variations of ammonia for the DMA, due to the lack of info. for amines. This is reasonable, as the emission sources of amines are indeed similar to ammonia, although with different emission fluxes. However, Is there any way to evaluate this assumption, for example, by some sensitivity tests upon changing the spatial and seasonal variations in the model simulations?

This is good point. However, we don't have any information with regard to the possible spatial and seasonal variations of amines emissions. The suggested sensitivity tests are more meaningful when more high quality measurements of amines become available. The added MMA and TMA results in the revised manuscript provide some useful insights on how emission fluxes may change the simulated concentrations.

Also, as the distributions of ammonia can also be used for other methylamines (MMA or TMA), and the estimated fluxes, uptake coefficients, etc., are also available for MMA and TMA, this simulation can be conducted on them too. In this regard, i believe FIgure 5 can be modified. I suggest the authors to do so, the modeling results with more methylamines should make the paper more scientifically sound and valuable for other future studies.

This is a great point. We agree that "*the modeling results with more methylamines should make the paper more scientifically sound and valuable for other future studies*". Our original plan was

to simulate MMA and TMA in future studies. Following the referee's suggestion, we have carried out simulations for MMA and TMA and expanded the manuscript to include MMA and TMA. Simulated MMA and TMA concentrations have also been compared with available measurements. We have substantially modified the results and conclusions to reflect the additional results incorporated.

*Other specific comments: 1) Both in the abstract and methods, the authors talked about "amines", while the results are actually only for DMA. Some clarifications are necessary. Just one example, in P17732-line 20, the reaction coefficient 6.54*10-11 is for DMA or for what amines?*

In addition to DMA, the revised manuscript also includes MMA and TMA. The reaction coefficients for three different amines (from literature) are different and given in the text.

2) P17729-line 21: "several others" to "several other studies"

Done.

3) tile of section 2.1, can be changed to "Sources and fluxes" as you also mentioned the emission fluxes.

Done.