

Interactive comment on “Sulfuric acid-amine nucleation in urban Beijing” by Runlong Cai et al.

Anonymous Referee #2

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The H₂SO₄-amine nucleation is a potentially important NPF pathway in the polluted boundary layer. While the importance of this mechanism has been shown in chamber studies and in certain megacities, whether this mechanism plays an important role in cities with a relatively low amine concentration and high existing aerosol concentration like Beijing remains unclear. This study combines long-term measurements at an urban site in Beijing and kinetic modeling to show that the H₂SO₄-amine nucleation is a dominant mechanism to initialize NPF in Beijing. The governing factors for H₂SO₄-amine nucleation are also elucidated. This work is meaningful for improving our understanding of NPF mechanism in polluted environments. The paper is generally well written. I think it can be accepted for publication after revisions to address the following (mostly minor) comments and suggestions.

(1) You tried to exclude organic nucleation as a main NPF pathway in Beijing (Line 198-202, Figs. S5 and S6). However, you only considered pure organic nucleation.

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A potentially important pathway in polluted environments is the nucleation of organics with H_2SO_4 . I think some calculations are needed to explore whether this mechanism could play a role that is comparable to the H_2SO_4 -amine mechanism.

(2) In Fig. S5, only ELVOCs are used in the calculation of pure organic nucleation rate. I think Kirkby et al. (2016)'s equation was based on HOMs. What adjustment was made to the original equation?

(3) The opening sentence of Results and Discussion gives a major conclusion of this study. However, the relationships between this conclusion and the supporting evidence detailed below are not very clear. For example, how do the results presented in Figs. 1 and 2 support this conclusion? Although I know the underlying logic, it is unfortunately not explained in the paper. The connections between the evidence and the conclusion should be described directly and clearly.

(4) The field measurements in Beijing were conducted from January 2018 to March 2019. However, it is not clear which parts of these data are used in the results shown in Figs. 1-4. Did you use measurements on all days or NPF days only? Is every NPF day between January 2018 and March 2019 included in these figures?

(5) I think the kinetic simulations are only done at selected conditions based on the description in Line 165. However, the measurements cover a wide range of conditions which, as you show in the paper, have a large impact. How do you make sure that you are doing an apple-to-apple comparison between modeling and measurements in Figs. 1 and 2?

(6) How are the results in Fig. 4 derived exactly? From the kinetic model or a combination of model and measurements?

(7) Line 167: The sentence is vague. How does the dimer concentration contribute to understanding the reaction pathways?

(8) Line 195-197: The simulated particle formation rates using these previous models

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and evaporation rates are orders of magnitude lower than the measured particle formation rates in urban Beijing. Why does this happen? Does this affect the robustness of your conclusion?

(9) Line 251-253: You may want to directly give the saturation concentration of amines here.

(10) Line 288-290: What are the main reasons for the much higher amine concentration in Shanghai than that in Beijing?

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