

The manuscript has improved a lot and been corrected according to the comments. All the issues raised by the reviewers has been addressed point by point. I recommend this paper can be accepted after the minor revisions as given below:

1. The authors have fully discussed the limitation and uncertainties of the model result in this study. as you mentioned the H₂SO₄-DMA nucleation is probably the major path way of nucleation in Beijing (Yan et al., 2021; Cai et al.,2021,2022), and in your work, TIMN scheme simulation agree well with the nucleation. So how is the atmospheric implication of TIMN in Beijing? And have evaluated the contribution by “ion-mediated process” in Beijing?
2. Line 17, to be favorable for
3. Line 55-56, please add the references to support the first sentence in this paragraph.
4. Line 60-63, can you give the suitable conditions for each nucleation mechanism, as you mentioned HIO₃ nucleation is dominant for coastal areas.
5. Line 188-189, I don't think the resolution of sulfuric acid can be 1 molecule/cm³, I recommend the digitals are given as $(6.1 \pm 3.1) \times 10^5$
6. Line 207, governed by aerosol Fuchs surface area, which is a representative parameter of coagulation scavenging (Cai et al., 2017a).
7. Line 216, necessary condition for NPF occurrence in the atmosphere

8. Line 329, can not
9. Line 420-425, does the APM model consider the nucleated particle growth process when simulate the PM mass concentration? If it is possible to evaluated the contribution of NPF to particle mass?