The study improved the simulation of aerosol formation processes in the NPF-explicit WRF/Chem model. The amount of work done was impressive. The results are meaningful to aerosol modeling. Overall, the manuscript is well written. However, some details are missing and some expressions need to be revised. I recommend the publication of this work after the following concerns are addressed.

Major comments:

(1) As stated by the authors, new particle formation (NPF) and subsequent particle growth are important sources of condensation nuclei (CN) and cloud condensation nuclei (CCN). NPF is an objective phenomenon and therefore its contribution to CN and CCN is definitely positive. "The nucleation resulted in decreased CCN" is only based on the simulation analysis. The authors should be cautious about the discussions relevant to "negative contribution".

(2) When using empirical formulation to calculate nucleation, the overestimation of CN could not be simply attributed to the high prefactor/coefficient in formulation. The empirical formulation has physical flaws because it could not consider the influence of other factors besides sulfuric acid, e.g., temperature and condensation sink.

(3) The model bias in this study is only based on the comparison of simulation against measurements at one costal site. Some discussions may be not applicable to other regions. I suggest that the authors can do some work based on more measurements if possible. At least, discussions on existing modeling studies in China or the influence of factors affecting model performance in other regions of China should be added.

(4) The comparison of the simulated mass concentration of aerosol components and gaseous precursors against observations is encouraged to present before evaluating the model performance in CN.

(5) The method of calculating CCN is necessary in Sect.2.

(6) How is the emission of IVOC considered?

(7) Is Knudsen effect considered when calculating the condensation of gases onto

nano-size particles?

(8) The simplified SOA simulation in this study did not consider the multi-stage oxidation of organic species, which can affect the microphysical properties of organic species in particle growth processes. Discussions on this issue is suggested.

(9) The model resolution is not high enough to well resolve the urban pollution. In discussing the model performance, this factor should be taken into account.

(10) Fig.3 shows a different temporal variation of CN_{10-40} from that of CN_{40-100} which shares a similar pattern with that of $CN_{100-1000}$. Primary emission may be a dominant factor contributing to model bias in CN_{40-100}. Is the diurnal variation of emission considered in emission data? Correlation coefficient between the simulation results and the observations is recommended to show.

Technical suggestions:

L191 and L193, organic aerosols -> organic matters

L269, NFP-explicit -> NPF-explicit

In Fig.2 and Fig.3, horizontal label bar is better

Higher and narrower Fig.4 with horizontal label bar is better.

In fig.5, a unified color bar for fig.5a, b, and c would be better.

The following papers are for reference:

Wang et al., 2015. Connection of organics to atmospheric new particle formation and growth at an urban site of Beijing. Atmospheric Environment.

Zhu et al., 2022. Airborne particle number concentrations in China: A critical review. Science of the Total Environment.

Chen et al., 2021. Global-regional nested simulation of particle number concentration by combing microphysical processes with an evolving organic aerosol module, Atmospheric Chemistry and Physics.

Yu et al., 2010. Spatial distributions of particle number concentrations in the global troposphere: Simulations, observations, and implications for nucleation mechanisms, Journal of Geophysical Research.

Spracklen et al., 2008. Contribution of particle formation to global cloud condensation nuclei concentrations. Geophysical Research Letters.