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Discussions

Interactive comment on “Wintertime New Particle Formation and Its Contribution to Cloud Condensation Nuclei in the Northeastern United States” *by* Fangqun Yu et al.

Anonymous Referee #2

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This manuscript investigates the contribution of nucleation to particle number and CCN in the eastern US using WRF-Chem-APM. The simulations show the majority of the BL number and around half of CCN0.4% from nucleation. The simulated CN10 and some gases were evaluated against measurements at 2 sites. I'm in favor of publication once some issues have been addressed.

Abstract and other places: There are statements about how the nucleation is entirely inorganic because of low biogenic emissions in winter. However, while this is a sound hypothesis, it was not explicitly tested. Please weaken the language to make it clear that the lack of organic nucleation was assumed, not a finding.

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Discussion paper

Abstract and throughout (e.g. L60-61): Please add more statements of “The model shows. . .” or “We predict. . .” etc. The current writing style likely has these statements implied, but there is a risk of this sentiment being missed by some readers, and they may think this was more than a model finding..

L25-27: This sentence is strange. What is the changing paradigm of wintertime precip? This isn’t discussed in the paper other than maybe one sentence at the end of the intro (L61-65, though it doesn’t refer to a changing paradigm).

L54-56: The statement seems incomplete. I believe the conclusion of Yu et al. (2015) was that the ion-mediated scheme they used did not have a temperature dependence, which caused it to overpredict in the summer. Yu et al. (2017) estimates a correction for the temperature dependence that may prevent the overprediction in the summer. The current statement should explain the findings better.

Not that Fangqun won’t know the reference, but for completeness: Yu, F., Luo, G., Nadykto, A. B., and Herb, J.: Impact of temperature dependence on the possible contribution of organics to new particle formation in the atmosphere, Atmos. Chem. Phys., 17, 4997-5005, https://doi.org/10.5194/acp-17-4997-2017, 2017

L132-134: Please add the specific instruments from which data was used here.

Figure 2d: It would be useful to show the NH_3 values from the model averaged over the times of the AMoN site.

L177: [NH_3] *partitioning* is calculated with ISOROPIA II

L215: The abstract said >85% for the surface

Figure 3: It’s confusing that there is a line for CN10 due to primary particles and CCN0.4 due to secondary particles. Please make them either both primary or both secondary for consistency.

L240: “Apparently” doesn’t seem like the right word here. It makes this seem like the

CCN-CDNC connection was not expected.

L242: Why does it highlight the need for *better* representation. Has this paper found deficiencies in representation? I don't think this paper has evaluated this.

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