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.
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.


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

Figure 2d: It would be useful to show the NH3 values from the model averaged over the times of the AMoN site.

L177: [NH3] *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.