The authors would like to thank the reviewer for the useful comments which help to improve the manuscript. Our replies to the comments are given below, with the original comments in black, and our response in blue. We have revised the manuscript accordingly. All changes made to the manuscript have been marked with Track-Change tool in one of submitted files.

Anonymous Referee #2

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..

We would like to clarify that the lack of organic nucleation was based on model simulations, not assumptions. The model calculates biogenic emissions based on MEGAN (see Section 2.1). Yes, we have revised the relevant sentences as suggested to weaken the language.

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).

This is a valid point. We have deleted this sentence from the abstract.

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

Yes, we have revised the statements to include the results of the 2017 paper.

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

Added as suggested.

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

Added as suggested.

L177: [NH3] *partitioning* is calculated with ISOROPIA II
Modified as suggested.

L215: The abstract said >85% for the surface
The value given here is for the two specific sites (PSP and APP) while >85% given in the abstract is for the whole NEUS region.

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.
We have changed CCN0.4 due to SP to CCN0.4 due to PP.

L240: "Apparently" doesn't seem like the right word here. It makes this seem like the CCN-CDNC connection was not expected.
This word has been deleted from the sentence.

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.
We have changed "better" to "proper".