Interactive comment on "Sources of Airborne Ultrafine Particle Number and Mass Concentrations in California" by Xin Yu et al.

Summary:

This work demonstrates the source appointment of ultrafine particle number and mass concentrations in California using the UCD/CIT chemical transport model. The manuscript fits well to the scope of ACP. However, I am worried about the method used for retrieving PN source and corresponding conclusions. This paper is worth to be published, but not in its current form. Thus I recommend it to be resubmitted after the following major comments listed below have been adequately addressed.

Comments:

1. Maybe MFE and MFB are very useful to present the model performance. But I would suggest the authors also provide the correlation between the predicted and measured results, which is more straightforward.

2. Page 8, line 230: I am confused why the author raise the value of 8% (N_{7-10}/N_{7-1000}) here? Did you use it to correct model results? If so, then this value is measured at Fresno supersite, which is located near roadways with moderate traffic. So could it be used for all the cases? Also, the particle number concentration has a significant diurnal variation, especially during the nucleation event days. But the authors only compare the daily average, this might be problematic. Concerning the particle number simulation, the number size distribution is also very important. Do they have any number size distribution measurements on the sites? I think it might be worth to compare.

3. Page 10，equation 1: The method used to convert mass contribution to number contribution is questionable. First, which mass (mass size distribution or total mass) do you use in eq. (1)? It is not clear how you define the Dp. Second, the nucleation is a major source of particle number, but it won't contribute a lot to the mass concentration, so if you use the mass size distribution in eq. (1), then it is better to check the number size distribution of nucleation source to evaluate the method. Also, condensation is an important process for the growth of nucleation mode particles. So the change of density can not be ignore.

4. Page 12, line 268: which method was used to measure $PM_{0.1}$ in Xue's paper? In figures 2-3, the authors only compare the data in 2015 and 2016. I guess there was no measurement in 2012. But in Figures 7-9, you only show the time series of $PM_{0.1}$ in 2012? This selective comparison is also shown for particle number concentration (figures 4-6). I would suggest the authors should also show the time series data in 2015 and 2016, which contain both measurement and modelling results. Additionally, what is the time resolution in Figures 4-9? It seems the x-axis in Figures 4 and 7 is not regular.

5. In Figures 2-3, there is no nucleation source, if the authors use eq.1 to convert mass contribution to number contribution, then wow did the authors define "nucleation" source?

6. Nucleation is a major source of particle number concentration. I would suggest the authors also show the modelling results only for nucleation days. If you put it in the average data (figures 12-14), then more information might be covered. And why you only show the average data from August and December.