General Comments

The authors have done a commendable job of addressing my points and those of other reviewers. I still very much appreciate this study overall, and wish to see it formally in the literature. I also appreciate the effort made to more fully characterize aerosol compositions involved in the classification of cases, to clean up the equations and make more precise explanation, and to add methodological details. While the aerosol chemistry does clearly indicate differences between the cases, and the statistical differences of some of the INP populations is supported, my opinion is that it is no clearer that such categorizations tag reliably specific INPs or makes them any more meaningful for categorization in models. This is acknowledged in the paper. Nevertheless, I think it is entirely possible that in follow-ups to this study, the authors or other authors will use these categories or consider the polluted or high black carbon or high dust cases as representative of more specific INPs categories in numerical models. I think that would be in error, as more effort is needed in future research to get to the point of a true "closure" study. This regardless of the apparently noble efforts modelers are making to deal with imperfect observational data, to paraphrase one of the more ludicrous responses I have seen (i.e., modelers not having the "luxury" to wait). It is well known that biological/biogenic INPs play a significant role in the atmosphere at temperatures >-20C, and also that they are the most difficult category to quantify and pin down. Over a land location especially, these must be present at some level always, and they cannot simply be characterized by total biological particle concentrations from a given sensor. INPs are always more specific, even within some broad categories, and this is likely especially true for the most efficient ice nucleators that are active at modest to moderate supercooling. In the end, I think that readers will focus most on the quantification and possible generalization of time dependence for ambient INPs (i.e., the focus of the title and what is in the abstract), and will focus less on the classifications of cases that represent some unknown mix of different INP types in all cases. I list a few specific points below for possible attention.

Specific Comments

1) In the new discussion circa line 942, is it necessary only to point out the utility of findings only for INP measurements that have short residence times (i.e., real-time measurements like a CFDC)? One imagines that it means that slow cooling or isothermal measurements, while helpful for validating results such as presented in this paper, are overall not necessary even for classical immersion freezing measurements.

2) The authors note that they found weaker time dependence across cases than found in the literature, and they reference Herbert et al. (2014) as supporting that different INP types should show little difference. That is not a result highlighted in Herbert et al., as far as I read that paper, and it is also the case that it was heavily focused on inorganic materials, especially minerals. Also, Wright et al. (2013; doi:10.1002/jgrd.50365), Fig. 3, shows that some types of INPs vary in time dependent character when isolated. But this all leaves me to wonder, again, if there is any reason to think that the different cases are representative for future application to specific INP scenarios, versus a more generalized time dependence to use for any INP category (at least to the extent that data at -15C characterizes things across temperature of relevance to mixed-phase clouds)? I know that categorization to fit model categories was an imagined goal of this paper, but it remains the one that has the least clear support. I would even say that the careful effort put

in to attempting to categorize aerosols and air mass characteristics as related to INPs stands as testament to how extremely difficult it is to characterize INP scenarios, since INPs are but a limited fraction of the entire aerosol population. The INP data are certainly representative for the region, but parsing them out to different sources is not possible from the data collected. The story on time dependence is the true feature result here.

3) Regarding new Fig. 5 and discussion around it (much appreciated), background is important whether in the temperature regime where it is “rare” or where the frozen fraction gets very high. The question I have, and which needs a clear statement, is if any corrections are actually applied, and if the rare occurrences at higher temperatures are simply ignored. I am not judging, just saying that it needs to be said. Some would average all background cases and do corrections, but one at least needs to say what was decided.

4) The data availability statement is not up to current standards and expectations, in my opinion.