Second review of "Ice crystal number concentration estimates from lidar-radar satellite remote sensing. Part 2: Controls on the ice crystal number concentration" by E. Gryspeerdt et al.

The authors have generally done a good job of responding to the comments from the first round of reviews and the revisions to the manuscript are generally appropriate. However, I still have a few concerns as described below.

1. Response to comment and text revisions with regard to the peak in $N_{i(top)}^{5\mu m}$ at T just below -35C: I still think this feature is somewhat exaggerated by the discussion in the text. Looking again at Figure 2, it seems to me that the the peak is only apparent in the upper envelope of the frequency distributions. No peak (or even change in slope) is visible in the most frequent (red) occurrences of $N_{i(top)}^{5\mu m}$ versus temperature. Also, I still think any feature associated with homogeneous freezing of pure droplets should be smeared downward by ice sedimentation rather than extending to lower temperatures (higher altitudes). Except in strong convective updrafts, the ice crystals are typically large enough to fall relative to the vertical uplift driving the cloud formation. The cloud top may rise with time as the updraft continues and ice nucleation propagates upward to lower temperatures, but this would not shift down the temperature at which there is a change in the mode of ice nucleation.

2. Page 20, line 10: It is plausible that an increased dominance of heterogeneous nucleation over homogeneous freezing can reduce ice concentrations. However, I think it is a bit misleading to call it a "negative Twomey effect". The ice concentration is not decreasing in response to the overall aerosol abundance. Rather it is a response to a change in the abundance of a small subset of the aerosol population.

Discussion of co-varying meteorology: The authors have added appropriate caveats about the possibility that the correlations between MACC aerosol product and retrieved ice concentrations may be a result of covariances between meteorology and aerosol processes. They suggest that investigation of such covariances is not possible with this analysis. I would think the extensive statistics available with the satellite retrievals would make such a study possible. I'm fine with the authors stating that such an analysis is beyond the scope of this paper. Perhaps they could suggest that the covariance analysis should be undertaken in a future study.