In this paper the authors hypothesize that aerosols produced by biomass burning and urban pollution contribute to the occurrence of extreme ice crystal events (EIE). They assemble data sets from a diverse array of independent sources and demonstrate correlation between elevated aerosol amounts and clouds with ice number concentrations greater than 5000 L^{-1}. The paper is logically organized and clearly written. Analysis methods appear to be robust. The topic has significance for climate change research, weather forecasting, and aviation safety. I recommend that the paper be published with revisions as suggested below.

General Comments

The authors have shown qualitative correlation between several independent data sources and the IAGOS incidence of Extreme Ice Events. But their suggestion that high aerosol concentrations are a cause of EIE is still circumstantial. It would be interesting to somehow quantify typical values of AOD, CO anomalies, FRP and other indicators for the EIE vs. non-EIE samples, and to test the statistical significance of the differences between the two sample populations. Short of this, I believe the authors can clarify and streamline their arguments to present a more convincing case. At the same time, they should acknowledge more prominently that convective processes play a large role in determining the location of high ice water content regions.

Specific Comments

1. Introduction
    a. The beginning of the paper seems abrupt as the authors immediately reference prior publications by other researchers rather than introducing and motivating the topic of this paper. An introductory paragraph with some general information describing ice clouds, what is known about their interactions with aerosols, and/or why it matters would set-up the current study more effectively before reviewing past literature.
    b. Lines 75-79. Could the authors please explain why they chose to use N_{ice} and define a new term (Extreme Ice Events) rather than using IWC? Some of the papers cited give existing thresholds for elevated ice conditions based on IWC (i.e., HIWC), so why not be consistent? If there is a valid reason for defining an EIE threshold rather than using existing HIWC thresholds, how do the two thresholds compare?
    c. Line 109. Baumgardner et al. (2004) is cited here, but not listed in the Reference section.
    d. Figure 1. Some of the black stars indicating megacities are obscured by the EIE symbols.

2. Measurement and Analysis Methodology
    a. Line 204 describes the geographic domain of the study as extending from -50^o to +180^o longitude, though Figure 1 indicates EIE outside of these boundaries and many of the subsequent figures (e.g., AOD, FRP) extend to a larger range of longitudes. It's not clear why the authors have limited their analysis to this region, or for that matter, why they didn't include events outside of the ±30^o latitude range. Some explanation is in order.

3. Results
    a. Figures 6 and associated discussion. Do the authors have an explanation for why the measurement-derived median CO anomalies are ~5 times larger than the modeled CO anomalies? Also, I'm confused by the assertion that, "The frequency distributions do

suggest that emissions from UP sources are potentially a larger source of nucleating particles in the ice clouds, in general." Can you elaborate on how Figure 6 demonstrates this result?

4. Discussion
    a. Figure 10. The density of EIE events in SE Asia obscures the values of FRP in that region.
    b. Lines 364-66. The following statement suggests that the evidence presented thus far proves that aerosol particles are responsible for EIE: "However, the maps suggest that these BB emissions are the source of some, but not all of the particles that lead to EIE." While the authors have shown spatial and seasonal correlation between aerosol presence and ice crystal concentration, I think it is overstating to say that the particles cause EIE. Correlation is not causation, and as the authors discuss in later sections, other processes contribute to EIE.
    c. Line 372-373. Please explain why only certain aerosols are considered relevant. Would sea salt not be of interest, for example.
    d. Lines 378-380. The authors state that Figures 11 and 12 show the high AOD over northern Africa in July is collocated with high dust concentrations and EIE in this area. Maybe I'm misreading the small map in Figure 12, but the high dust concentration and cluster of EIE points appears to coincide with a relative minimum in the AOD distribution for July (top of Figure 11). Can the authors please clarify?
    e. Section 4.4. The authors nicely link their work to previous studies in this subsection. A key point in their argument is that the ice clouds observed in the IAGOS data set are likely liquid in origin. I'm confused about how the authors know this, and how it relates to their statement (in the abstract) that droplets are lofted and freeze heterogeneously. Please clarify.

5. Conclusions
    a. Lines 505-506. Regarding the qualitative comparison of ICI events and EIE, wouldn't a positive correlation be expected given that both phenomena are based on high amounts of ice crystals? This again raises my earlier question about why new terminology and associated threshold for high amounts of ice crystals is introduced for this analysis.