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Interactive Comment

Interactive comment on "The Chisholm firestorm: observed microstructure, precipitation and lightning activity of a pyro-Cb" *by* D. Rosenfeld et al.

Anonymous Referee #3

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This paper describes a particularly strong pyro-cumulus event in northern Canada. Previous studies of this event by one of the co-authors have described this event in detail, including injection of combustion aerosol into the stratosphere, and subsequent global transport. Given this was such a singular event, with such long reach, it is a worthwhile goal to understand the mechanisms responsible.

The paper provides much remote sensing analysis that by itself can be considered a worthwhile addition to the literature. What is the physical nature of extremely vigorous convection? Comparisons with more typical terrestrial convection would make an interesting topic, and this paper contains elements of these comparisons. Unfortunately, this paper does not hold together in its current conception. It is not clear from its organization what is its intent, nor how it adds meaningfully to what has already been contributed by M. Fromm on this case. Many of the “conclusions” are too speculative, and not well supported by the observations. Some of the physical mechanisms provided as explanation for the observations need to be weighed objectively against other, perhaps more likely, explanations.

In general I would recommend restructuring the paper to focus solely on the microphysical and dynamic aspects of the storm, and compare its features to storms elsewhere, leaving out extended discussion of reduced precipitation scavenging, which is much too speculative. Highlight only the physics that can be solidly supported. That would be a nice paper.

In particular, the following questions and concerns should be addressed if the paper is to be considered further.

1. Is the purpose of the paper to provide observations of a case, or to further the hypothesis that the smoke suppressed precipitation development in the pyro-Cb, and hence favored ejection of smoke into the Stratosphere? If the former, the paper should be organized to focus primarily on observations of the case, with some brief discussion of likely explanations of the observations saved to the later parts of the paper. If the latter, Section 5 on electrical activity is out of place, and should be omitted from the paper, and the title should be changed to more accurately reflect the intent of the paper. Based on the speculative nature of the conclusions provided, the paper might be best organized by focusing on the observations alone.

2. On page 9881, the comparison with nuclear weapons is interesting, and illuminating. If the Chisholm firestorm was equivalent to 3 to 5 Hiroshima type bombs per minute in sensible heat release at the surface, aren’t all any other factors irrelevant insofar as ejection of air to high altitudes is concerned? Any factors related

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to precipitation suppression should be entirely secondary. Since it is a central premise of this paper that precipitation suppression significantly contributes, a much better case needs to be made as to why. The reason this is important is that it has already been stated in previous work that a vigorous firestorm was responsible for ejection into the stratosphere. What justification is there for adding unnecessarily complicating factors? In the nuclear winter literature is there any reference to precipitation suppression within nuclear convection? Should there be?

3. The plume on top of the convection between B and D in the transect shown in Figure 4 is highly reminiscent of the plume features described by Setvak and colleagues on top vigorous deep convection. Brief discussion is made of this feature in the conclusion, but, since it stands out in the multi-spectral imagery, it deserves more discussion. Papers by P. K. Wang suggest this is likely a result of gravity wave breaking taking "gulps" out of the convective core. If so, this isn't specifically convective detrainment in the stratosphere; the nature of the plumes will be primarily stratospheric and smoke-free, which may account for their observed warm temperatures.

4. On p. 9886 there is discussion that the small size of ice crystals observed in the anvil is a result of homogeneous freezing of small water droplets. I recommend the authors read "Homogeneous aerosol freezing in the tops of high-altitude tropical cumulonimbus clouds" by Jensen and Ackerman in GRL and "Evidence for the Predominance of Mid-Tropospheric Aerosols as Subtropical Anvil Cloud Nuclei" by Fridlind et al. in Science. The size of anvil ice crystals is most likely determined primarily by the strength of the updraft velocity in the convection, and has very little to do with the aerosol concentration in the boundary layer. The reason is that deep convection is turbulent. Small aerosols in the *clean* free tropospheric environment are entrained into the deep convection, where they freeze homogeneously. At very high updraft velocities, the number of aerosol that nu-

cleate is very large, so consequently the size of the resultant ice crystals is very small (see Karcher and Lohmann, 2002 in JGR). Even if CCN concentrations in the Free-trop are low, total aerosol concentrations can nonetheless be very high, and aerosol of any size can nucleate at sufficiently high humidities. Note that there are really *two* mechanisms of homogeneous nucleation that operate in deep convection. The first is freezing of pre-existing cloud droplets in the convective core, which is referred to here. The second is freezing of entrained haze aerosol through the side of convection. This latter process is most important for creating small ice crystals and determining the optical properties of the anvil cirrus that would be viewed in the satellite imagery described here. Evidence for the resulting bi-modal size distributions described by Jensen and Ackerman is discussed in detail in a paper on Florida anvil cirrus evolution by Garrett et al. in JAS in 2005.

5. The comparison in Fig. 7 is unconvincing. The variability within clouds is much greater than the variability between clouds. How do we know we aren't simply seeing shadows in the variations in darkness?

6. On p. 9887 it is not clear what the radar is seeing. Is it ash, cloud, or precipitation? Presumably some combination. But how is it determined which contributes most based on the echo magnitude? This needs to be clarified before conclusions can be made about any differences in precipitation. To what extent is radar attenuation a consideration in the signal retrieved from the deep convective cloud?

In summary, this paper does not present convincing evidence that aerosol play a significant role in modulating the ejection of aerosol into the stratosphere. All micropysical and dynamic processes described here are most easily explained by appealing to the large sensible heat release at the surface, and subsequent high updraft velocities in the convection.

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