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Atmospheric Chemistry and Physics Discussions

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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.

D. Rosenfeld et al.

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Following are our responses to the comments of Referee 3, which are replicated here in *italic text.*

Comment: *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.*

Response: Indeed, the objective of this paper is a comprehensive understanding of this well studied storm, on all its observed aspects. This has not been done before in such detail to any pyro-Cb. Other papers complement it with simulations. M. Fromm documented so far only the effluents from the storm top, and not the storm itself.

Comment: *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.*

Response: The purpose of the paper is to provide the best description possible of the dynamical, microphysical and electrical operating mechanisms, based on the observations and their interpretation.
The paper is not aimed to promote any specific hypothesis, but it does provide hypotheses that are supported by the observations, as it should.

Comment: *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 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?

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Response: Even though the energy release from the fire is enormous, model simulations suggest that the energy released from condensation and freezing of water in the convective cloud dominates the overall energy budget of the pyro-convection (Trentmann et al., 2006; Luderer et al., 2006). Therefore, microphysical processes can not be considered irrelevant for the ejection of air into high altitudes. However, we agree with the reviewer that there is a positive correlation between the energy release from the fire and the injection height as found from observations (Lavoue et al., 2000), which is explained by a positive feedback between sensible heat flux and latent heat release (see Luderer et al., 2006).
The delayed formation of precipitation is critical for the release of the smoke into higher altitudes. Generally, wash-out of the smoke in the convective cloud operates through the scavenging of the smoke and subsequent sedimentation with precipitation-size droplets. In the case of delayed onset of precipitation, washout of the smoke is reduced and smoke injection at higher elevations is more effective. Through this mechanism (reduced wash-out by reduced formation of precipitation) the delayed formation of precipitation is important to address the question of smoke transport into higher elevations in the atmosphere, and needs to be discussed in this context. Note that the physical mechanism that leads to the reduced efficiency of precipitation formation can not be easily determined from these observations alone.

Comment: *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.*

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Response: If the speculation of the reviewer that it is a case of “*breaking waves taking gulps out of the convective core*”, it still means detraining cloud and smoke particles from the convective cloud into that level in the stratosphere If so, the suggestion of the reviewer that "the nature of the plumes will be primarily stratospheric and smoke-free" is not valid. For the reviewer to be correct, the feature should not be physically connected to the anvil, as simulated by Wang. But in this case the newly nucleated cloud ice particles formed there should have had larger reflectance in 3.7 micron than the main body of the anvil, as has been documented by Setvak and colleagues. This is not the case here. In addition, if it was a cloud that does not contain smoke, its visible brightness should have been larger than that of the main anvil. But quantitative comparison of the reflectance of the visibly flat surfaces revealed equal mean values. Therefore, the filament is likely detrained from an overshooting updraft, and in such case, explains the detrainment height of the smoke, as previously documented by Fromm. Parts of this discussion will be added to the manuscript.

Comment: *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...*

Response: That paper states: "The small crystals produced by aerosol freezing have the largest impact on cloud-top ice concentration for convective clouds with strong updrafts but relatively low aerosol concentrations". This is not applicable to a pyro-Cb, which has extremely large concentrations of aerosols in the updraft.

Comment: *...and "Evidence for the Predominance of Mid-tropospheric Aerosols as Subtropical Anvil Cloud Nuclei" by Fridlind et al. in Science.*

Response: This paper is also not applicable here, because in the Chisholm pyro-Cb case there were large amounts of aerosols coming within the cloud from the low levels, with no reason to expect high aerosol concentrations at the ambient atmosphere at mid level, as in the case quoted by the reviewer.

Comment: *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 nucleate 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*

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in a paper on Florida anvil cirrus evolution by Garrett et al. in JAS in 2005.

Response: According to the paper of Jensen and Ackerman (GRL, 2006) that was quoted by the reviewer, the nucleation of new ice crystals on entrained aerosols would be important only if the concentration of aerosols within the convective cloud would be relatively low. This is clearly not the case for a pyro-Cb.
To prove this point, the pyro-Cb in areas 2 and 3 of Figure 11 are not nearly as vigorous as the Chisholm case, but have similarly small cloud top particle effective radius, whereas the ambient cloud in Area 1 has a much greater effective radius. In the case that the entrainment of ambient aerosol through the side of the convection would be the determining process that leads to the creation of small ice crystals at the cloud top, we would expect similarly sized ice crystals in the pyroCbs (areas 2 and 3) to those in the ambient cloud (area 1). Since this is not the case, we have to suspect that other mechanisms in pyroCbs are responsible for the dominance of small ice crystals at the tops if pyroCbs.
Many more such comparisons are available, and will be incorporated in the paper if the reviewer thinks that they are necessary to support this point.

Comment: *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?*

Response: The reflectance values of all the pixels of the two patches are compared, and they differ from each other with very high statistical significance. The two histograms will be added, and that will eliminate the perceived ambiguity.

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Comment: *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?*

Response: Point S in Figure 8 corresponds to the source point of the smoke in Figure 7, which is denoted by the point of the middle V that is annotated on the bottom panels of Figure 7. Common annotation will be added to Figures 7 and 8 to indicate the echoes that are pure smoke.

Comment: *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?*

Response: Such weak reflectivities cannot be a significant factor in the attenuation of a 5 cm radar. Furthermore, there are no intervening echoes between the target cloud and the radar.

Comment: *In summary, this paper does not present convincing evidence that aerosol play a significant role in modulating the ejection of aerosol into the stratosphere.*

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Response: This has not been the objective of this paper. The paper merely intends to describe the various aspects of the observations of this pyro-Cb and discussion of their physical significance. This will be stated more clearly in the revised version of the manuscript.

Comment: *All microphysical 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.*

Response: We agree that no final conclusions on the relative importance of the high updraft velocities at cloud base and the smoke aerosol in explaining the presented observations can be easily made based on the observations of this paper alone. However, a companion paper by Luderer et al., titled "Modeling of biomass smoke injection into the lower stratosphere by a large forest fire (Part II): Sensitivity studies" (ACPD, 6, 6081-6124, 2006) simulated this storm with and without aerosol emissions from the fire. Their results suggest that the size of the small droplets indeed is mainly modified by the aerosol and not by the dynamics (compare their Figures 6a and 6b). Without the smoke aerosols the size of the small hydrometeors is substantially larger. The effect for the larger hydrometeors, however, is not as straight forward. Spectral bin models are required in order to provide more accurate insights in the future.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 9877, 2006.

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