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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying the large-scale electrification equilibrium effects in dust storms using field observations at Qingtu Lake Observatory" *by* Huan Zhang and Xiaojing Zheng

Anonymous Referee #2

Received and published: 20 September 2018

This is a well-organized study of natural dust storm electrification, with novel analysis and new findings. The English has been meticulously prepared. Some improvements are in order pertaining to the physical interpretation and the real evidence for equilibrium effects. A number of substantive issues are worth addressing by the authors in the preparation of their final manuscript. These issues are followed by detailed edits/comments on the text.

Summary: Publish after appropriate revision

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Discussion paper

Substantive Issues:

(1) Physical origin of dust events

The physical/meteorological basis for the events with other extensive documentation in this work is not elaborated. Lines 110-111 suggest a role for straight line winds. Are the cold downdrafts from thunderstorms/squall lines important for these events, as was the case in Niger in a study by Williams et al. (Atmos. Res., 2009). (We are aware of earlier thunderstorm studies in the Lanzhou area of China by other investigatorsâĂŤS. Liu for example.)

(2) Physical hypotheses for “equilibrium effects”

First of all, the physical quantity “equilibrium charge” introduced in lines 78 needs to be better defined there. Are you talking about charge or space charge density or space charge density per unit mass of dust? It is made clear later in the paper what you are measuring but this needs to be clarified in the Introduction, given the importance of the equilibrium concept throughout the work. Regarding hypotheses for equilibrium charge, the Introduction gives nothing and lines 195-196 gives nothing. Only late in the paper (Section 4.2) is any discussion provided. If this came in the Introduction, the reader would have a better idea where you were heading in the overall work.

Regarding one working hypothesis: dielectric breakdown, there is an important observational test: Corona discharge is a form of dielectric breakdown and furthermore, this process is a source of light. With a sensitive video camera operating in nighttime conditions (with better signal-to-noise ratio), one could look for light intensification as a signature for equilibrium. Have the authors tried this?

(3) Physical units

The authors should be clear about physical units for rho, M10, mu, lambda and ACD, all linked with equation (1) and (2) (where rho has standard MKS units of C/mˆ3.) It should also be made clear what ACD actually stands for. This may be Chinese, but

in any case needs to be spelled out because in my experience this is non-standard usage.

(4) Sign convention on Ez and polarity of space charge

Important missing information in this study is the sign convention on Ez and the predominant polarity of the space charge density. Figure 2 can't be interpreted without this information. (See again Williams et al., 2009)). This issue is also related to physical mechanisms for macroscopic dust particle charging and two prominent ones are as follows:

(i) Collisions between large and small particles in the cloud with selective charge transfer and then separation of the large and small (oppositely charged) particles by gravity. Result: a bipolar dust cloud

(ii) Lofting of fine dust particles by wind-driven saltation. Result: a unipolar cloud.

What can the authors offer up to distinguish these two mechanisms?

It is worth commenting further on findings by the reviewer that went beyond the published findings in Williams et al. (2009) and which are also based on work in Niger. This evidence came from a single day characterized by very gusty straight line winds, but of insufficient strength and persistence to form a deep opaque dust cloud. But with every strong gust, large perturbations (many kV/m and as a large as during the large haboob events) in the surface electric field were noted. This we take as evidence for mechanism (ii) above. The very find dust (clay) is charged with negative polarity during saltation. But in the context of the present work with emphasis on mass loading, please note Figure 6 in Williams et al. (2009) that does show some (weak) positive correlation between maximum E field and (inferred) mass loading. More analysis of this kind is needed in the present work to shed further light on physical mechanisms of dust charging.

(5) Puzzlements about Table 1

Table 1 is a reliable compilation of numbers for the ten documented cases, but would benefit from ACD values and maximum Ez values. But in light of claims that larger RH increased the charge transfer (contrary to this reviewer's intuition and experience in Niger where slightly more moisture and humidity served to suppress the dust and particularly the fine dust). I looked at extreme cases in Table 1. Case #2 has the largest RH and the largest rho, and Case #9 has the lowest RH and the largest rho. These findings are in keeping with my intuition. But then in studying in more detail the multi-regression and the evidence in Figures 4 and 5 I became confused. Sometimes the signs of the derivatives are positive and sometimes negative. The work should strive to go beyond regression to address physical explanations for behavior, whenever that is possible. And regarding regression alone, unless the coefficients are provided in equation (5), the reader does not have a quantitative result.

(6) Evidence for equilibrium effects

The equilibrium charge is a key concept in the paper. But when all is said and done, what exactly are the authors pointing to in support of such an effect? For example, in Figure 4, the space charge density is increasing monotonically with mass loading throughout the range, with no evidence for saturation. There are also no signs of asymptoting in Figure 5. What then is the real evidence for equilibrium charge?

Detailed edits/comments on the text:

Page 2 Line 24 Why is 10 min an important time scale?

Line 28 A little confusing as you never measure the charge on one dust particle in the paper.

Page 3

Line 40 "electrical charge" Lines 41-42 This is not shown nor discussed later in the paper. Please explain why it is important? (It could be another explanation for the equilibrium charge, for example.)

Line 50 “of the electric field”

Line 64 “influence”

Page 4

Line 71 “using a Faraday cage” It is not clear how you are measuring this quantity with a Faraday cage.

Line 80 “in the quantification of particle electrification”; “such an electrification equilibrium exists under. . .”

Line 84 “such as the ambient”

Line 86 change “such as” to “and especially”

Line 90 The authors do it with multi-regression but do not do it physically.

Page 5

Line 110 What is a prevailing wind route?

Line 113 Why is this? I don’t follow the argument.

Line 117 , 118 Vertical gradients in what quantity?

Page 6 Line 123 could add “at centrally-located S9”

Line 125 “by a solar panel system”

Figure 1 should make it clear that Ex and Ey are non-zero because you are measuring them in altitude above the surface

Line 140 “can be determined”

Page 7

Line 143 It is not clear how you do your calibrations with instruments at this height

Line 158 You should give the sampling frequency.

Line 162 “The PM10 mass concentration. . .”

Line 166 “a sand particle”

Line 167 “a temperature-humidity sensor”

Line 170 Tell the scale over which the visibility measurement is made

Line 171 Presumably the Ez measurements are more frequent than 1 Hz.

Line 195 How did the SLR model show equilibrium effects?

Page 9

Line 199 See Williams et al. (2009)

Line 213-214 Authors should make it clear that the derivatives will be shown to be both positive and negative.

Line 228 This is a HUGE field to have near the ground, and I would expect lots of corona light from ground features.

Figure 2 Reader needs the convention for Ez polarity to get the polarity of the dust cloud. Line 11

Please add the suggested quantities to Table 1. Visibility numbers are also shown in Williams et al. (2009)

Line 157 It is difficult to see the arrow directions on these plots. Page 13

Line 266 It is not clear to the reviewer that a constant ACD value is evidence for equilibrium charge unless that constant shows up in all cases. Has this been shown? And where has it been shown that ACD is independent of wind speed?

Page 14

This figure 4 shows evidence that rho is increasing with RH. This runs contrary to my intuition. Page 15

Lines 312-313 This is not the scale that I got in looking at the figure. Those scales are larger.

Line 323 What lab studies show this?

Page 16

Lines 347-349 What is evidence for this in the paper?

Page 17

Line 366 Where do I see this finding in plots in the paper?

Page 19

Lines 423-424 Where is this shown in the paper?

References

Suggest adding Williams et al. (2009) and studying it.

End review

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