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Comment

***Interactive comment on “An episode of extremely high PM concentrations over Central Europe caused by dust emitted over the southern Ukraine” by W. Birmili et al.***

**Anonymous Referee #1**

Received and published: 2 October 2007

Review on the manuscript by Birmili et al. ("An episode of extremely high PM concentrations over Central Europe ...")

**GENERAL COMMENTS**

The manuscript by Birmili et al. discusses observations of an apparently exceptional event of soil dust being transported in the boundary layer from the source region in the Ukraine over central Europe. The authors present extensive experimental data, mainly originating from a network of ground stations providing a number of aerosol property measurements (aerosol mass, chemistry, AOD, size distribution), but also including satellite data and, for one observation site, lidar data. Discussion of the

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results is focused on identifying the source region of the dust plume and continues on the processes and temporal development of aridification in this region. Overall, the presentation of methods and experimental data is well done and sound. The discussion and conclusions do not arrive at exciting new scientific insights, but the value of the manuscript lies in the thorough description of the particular dust event.

In my opinion, the manuscript should be well suited for publication in ACP if a number of points get addressed by the authors. Minor revisions are certainly required. Most important, the manuscript is quite long and could be easily shortened without losing relevant information for the reader. This holds true as well for the very long appendix B.

### SPECIFIC COMMENTS

The manuscript is good to read and in general well structured, but contains quite a few of redundant bits which should be eliminated. Partly, this can be easily corrected if some discussion is moved from Sections 3 and 4 to be presented in its entirety in Section 5. See also further comments below.

Section 1.3, page 12236, line 5. "Mass balance deficit" - please explain.

Section 2.4, page 12240, 1st paragraph. State briefly how the inversion of the SMPS data was done for this instrument.

Section 2.4, page 12240, 2nd paragraph. Method of the Grimm EDM is described in many details. Why? Could be shortened. But what I would rather like to know in this context: Has a size calibration been done for this instrument? Do the particle optical properties (refractive index) significantly effect the size calibration? I expect they do, in particular if the soil dust contains absorbing components. This should be taken into account.

Section 2.6, page 12241. Which meteorological data fields did enter into the trajectory calculations? Explain "sigma surface". Explain more clearly, how the vertical settling

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velocities were entering the trajectory calculations. I am not sure I understand what the authors have done here.

Next page, same paragraph, last sentence. I did not understand this. What is the relation between mean values of pressure etc. and the "local contact frequency"? Are results of this shown in the manuscript?

Section 3.2, page 12243, 2nd paragraph. "The low level dust cloud is pictured by yellow and green colors." - This is not clear. The dust plume has not yet been introduced at this point. The colors of the trajectories represent meteorological parameters, not dust.

3rd paragraph. "Transport only possible if gravitational settling is taken into account" - What is the meaning of this statement? Transport is independent of gravitational settling. Do you want to say that the Saharan air did not enter the BL air over Europe and only gravitational settling of large Saharan dust particles could lead to some mixing of Saharan dust with the soil dust plume? Would this not rather belong into the discussion section?

Section 3.3. Should this section not appear first within the entire Section 3? The observations by satellite, showing actually the dust plume forming over the Ukraine, set the motivation for all the transport considerations.

Section 3.3, page 12245, lines 7-8. This sounds speculative. Can it not be checked if there was precipitation or not?

And in general on in this paragraph: Again, I would suggest moving this aspect of possible Saharan dust contribution to the discussion section. This aspect appears quite redundantly in the manuscript at various places. I am quite convinced that the Saharan dust contribution is minor or not existent from the arguments presented. I would cover it only once and it should be part of the discussion chapter 5.

Section 4.1.1, page 12246, 2nd paragraph. The statement (last sentence) on the dust event being outstanding can actually only be made by checking the long-term data

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records of the various stations. This is being done later in Section 5.

Section 4.1.3, page 12248, last two paragraphs. The vertical distribution is crucial to the estimate of the total dust mass in the plume. Here I would like to see some more observational evidence illustrating the average 1400 m layer thickness. More importantly, regarding uncertainties in the area covered by the plume (horizontally and vertically), and variability of observed PM10 data, an uncertainty range for the 60 Gg of dust should be presented.

Page 12250, lines 10-11. But this is to be expected, isn't it? Surface wind speed is always lower than wind speed (air mass advection) aloft. So is this an important statement at all? Can be removed.

Page 12250, discussion in last paragraph. Comment on why mixing with FT air is not possible would be of interest here. Is that due to synoptical/meteorological conditions - or due to the dust plume itself and its possible influence on atmospheric stability?

Page 12251, last two paragraphs. This part I find rather lengthy and I don't see the relevance. Should be more focused or skipped.

Section 4.4.1 and later on in many instances. The usage of the term "non-volatile" with respect to the inlet lines being heated to "only" 50 °C I find very misleading. Often much higher temperatures are used in aerosol research to describe the non-volatile particle fraction.

Section 4.4.1, page 12252, last sentence. "Grimm EDM provides correct mass concentrations" - but this can be only true if there is a dominating coarse particle mode!?

Section 4.4.2, last paragraph. Is this important for this study? Can be skipped in my opinion.

Section 4.4.3. and 4.4.4. Overall I find these two sections too lengthy. Given that the plume originates obviously from one single source and that mass concentrations agree well within the plume, it is not surprising that the chemical composition agrees as well.

And as for the discussion of the Fe results towards the end of Section 4.4.4, I think this can be skipped as well, because it does not contain new information on the properties of the soil dust investigated in this paper.

Section 5.2. Essentially, this is not really a discussion but a summary of what has been said earlier in the manuscript. Author's should decide where the discussion of this aspect should go. It should occur only once. (And, as mentioned, an uncertainty range should be given.)

Section 5.3. This is an interesting section, dealing with the important information on how exceptional the dust event actually was, but I am wondering why only the data of the Melpitz station have been addressed here. What about the data records of the many other ground stations used in this study. Do they confirm the conclusion?

Furthermore, if this event was so extremely exceptional, it kind of takes the ground away of the discussion following later on in Section 5.4 and 5.5 (desertification trends). How do the authors rate the relevance of such a phenomenon, if it is so unusual.

Section 5.3, page 12257, line 8. Unusual stability - that's interesting, but it was not explicitly explained in the manuscript. Are there data on this?

Sections 5.4 and 5.5 make interesting reading, but are not clearly connected to experimental data (or model predictions). Therefore, I feel this can be somewhat shortened as well.

Conclusions section in general. This is rather a summary or abstract and I don't find new conclusions here. Many statements are redundant to what has been said earlier. I would recommend shortening this considerably and highlighting only the most important conclusions on the observed dust plume, its aerosol properties and its impact.

In general: I would prefer "dust plume" versus "dust cloud".

Appendix A (and table). Can the sources of the data be stated in more detail? Are the data publicly available?

Appendix B. Discussion here is by far too long. Just describe the essential information needed to understand how the inversion of the data was done.

Figure 1. Too small and hardly decipherable. Figure caption gives wrong date for the b) case.

Figure 3. Too small. Is there an essential difference between pressure and height plots? If not, skip one of the panels (and make the others larger). Last sentence of Figure caption should rather go into the discussion in the text.

Figure 8. Again, please enlarge. (Figure 10 could be smaller.)

Figure 9. Remove the extrapolated near-surface part of the lidar profile (or represent it with a dotted line).

Figure 12. What are the black numbers on top of each panel and what are the red numbers in brackets?

#### TECHNICAL COMMENTS

Section 2, page 12236, line 20. Insert "on" after "particles". Replace "wavelength" by "radiation". Page 12240, line 5. Isn't it "Grimm GmbH" or similar, not "Grimm Inc."? Please check.

Page 12242, line 14. Insert "March".

Section 3.3, page 12244, line 24. Convert Kn to km/h.

Page 12245, line 3. Remove "theoretically".

Section 4.1.1, page 12246, line 6. Concentrations "increased" (in stead of "augmented").

Page 12249, line 13. " - " missing.

Next line: Remove "at".

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Page 12249, line 26. Remove " a " .

Page 12250, line 22. Add " and extinction coefficients".

Section 5.1, page 12256, line 8-9. "On top" is misleading in this context as it does not refer to the atmospheric layering.

Section 5.5, page 12259, line 6. "comparison"

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12231, 2007.

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