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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Recent progress in understanding physical and chemical properties of mineral dust" *by* P. Formenti et al.

Anonymous Referee #2

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This paper reviews the recent data on the physical and chemical characterization of mineral dust. The information provided in this paper would be of interest to the atmospherid dust community as well as wider science community. The figures on the potential dust source regions seem to be useful.

However, there are a number of problems in the present manuscript need to be addressed before the paper is acceptable for publication. The reviewer suggest a major revision, particularly the abstract, the synthesis and recommendations, and the tables. The reasons are given below.

1. Abstract: This paper did not provide strong evidence why future research should focus on the evoluation of dust properties during transport (which hints that not in the source regions). As mentioned below (and in this paper), although there are significant

progress in the characterizing the dust properties in or close to the source regions, there are still major uncertainties. For example, the regional variaiblity of the dust properties are still not well known. The authors argued that there are limited data on the iron mineralogy in the Asian dust source region. The size distribution of the chemical and mineralgical compositions of the dust areosols from different source regions are still poorly known. Therefore, the reviewer believes that it is important to understand the evoluation the properties of dust during transport as well as to characterize dust in the source region .

2. Abstract: The claim that "large particles should be important for deposition-based studies such as those on ocean productivity" is based on the authors' speculation rather than evidence. Throughout the manuscript, there are no evidence to support this argument. Indeed, this argument can only be true if substantial amounts (mass) of >20um particles were transported to the open ocean and/or the solubility of the iron in such particles are substantially higher than smaller dust particles. Both are questaionable. On the othe hand, the super-large particles can serve as giant CCN and therefore may play some role in the cloud formatin and precipitation (Levin et al. 2005; Rosenfeld et al., 2001). This may indirectly affect the radiation budget. The other possibility is that some of such large particles may have been formed during atmospheric transport (e.g., by cloud processing and coagulation) rather than emitted directly from the dust source regions.

3. It appears that this paper was prepared for a long time (since 2008) and therefore some more recent studies are not included in this review. The reviewer urge the authors to have another review of the more recently published papers and included the relevant progress in this review. The reviewer noticed that even the more recent papers by some of the authors of this review are not included, e.g., Hand et al., 2010, JGR; Tobo et al. 2010, PNAS.

4. Australian dust is one of the important sources of mineral dust in the world. Although less is known, it is unreasonable to complicately ignore the works by Australian

researchers. If the authors choose to do so, the title might need to be constrained with Asian and African dust.

5. Page 31193, line 11: what is the definition of a dust storm? What is the reference? Even 500 ug m-3 is an extremely high concentration. Haze is defined as “An aerosol that impedes vision and may consist of a combinatin of water droplets, pllutants, and dust; Dp<1um) (Seinfeld and Pandis, 2006). Haze and dust are completely different.

6. Page 31207, line 28: Lafon et al. 2004, 2006 did not conduct any EM observations.

7. Page 31207 last line-31208, line 1: the so-called Fe oxides particles here were defined as Fe>0.15 and Si/Fe<1 and Ti/Fe<1.33 (e.g., Kandler et al., 2007). They could only be regarded as Fe-rich particles rather than Fe oxides particles.

8. Table 1: Composition should also have an impact on the ice nucleation (IN) ability. There are many new papers in this regards. What is mass number distribution? Is it mass/number distribution? What is surface number distribution? Roughness (related to surface area) may affect heterogeneous reaction.

9. Due to its significance in cloud formation and precipitation and therefore the climate system, it is suggested that the IN ability and hygroscopicity of dust is discussed somewhere in this review. They are related to dust aging and chemical composition. The reviewer believes this area is one of the important progress that has been made in the last few years.

10. Table 2: There are some major problems in this table. The first is how the authors identify a or a group of dust aerosol samples collected on the ground to be from a particular source region. It seems that the authors ascertain the source region of an aerosol sample when the air mass passes that region. If it is true, this needs to be specificially mentioned as a notation below the table because this analysis does not gurantee an accurate identification of source region. It can at most tell you that the air plume passed over a source region.

In case soil samples are collected, the number of samples and the spatial distribution (or sample frequency) of the samples should be indicated in the notation. This will allow the readers to evaluate the robustness of each tracer listed here. The uncertainy should also be mentioned in the text. For example, if there is only one or a few data point, it should be clearly mentioned.

Second, there is a real danger to use mineralogical or elemental compositions from one or a few aerosol samples or maybe soil samples to represent the extremely large area (hundreds to thousands of km2) of a particular potential source region (they look very small on the figure!). This methodology may be valid only when the data are availalbe at a longer time scale and/or are verified by several relevant papers. A longer term measurements on the mineralogical and elemental compositions of atmospherid dust aerosols are needed to confirm whether such heterogenenity is significant or not in the dust aerosols produced from each particular source region. Although the reviewer believes that such compliation worth doing, the potential uncertainty needs to be clearly identified in the manuscript.

11. Table 2: The reviewer does not check other areas. But it seems wrong to say that the carbonate content is very low in Bodele depression. Washington et al. (2009) and Moreno et al. (2006) showed small amounts of carbonate (less than 2% of CaO, therefore less than 4% of carbonate) in the soil samples collected from selected sites from the Bodele. However, Bristow et al. (2010) showed the dust from Bodele are much more variable and are generally high in Ca content (Ca up to 4%, CaCO3 up to 10%). Also the salt content can also be very high. The reviewer believes this difference is due to the the heterogeneity of mineralogical compostion in the Bodele depression. In some part of the the old Chad lake, there are still diatomite deposits but in the margins the deposits may come from the Tibesti Mountains or Sahel by flash floods. The mineralogical compositions would therefore be very different.

This example clearly illustrates the heterogenity of dust properties in the same source region and therefore the uncertainty complied in this table. If there are only one or two samples (particularly soil samples) from a particular site from a dust source region, the representativeness of such datasets should be robustly evaluated. Accordingly, in section 3.1, the conclusions should be made with caution.

The above mentioned authors also provided the ratio of Fe/Al and other elemental ratios which should be included in the Table.

12. Table 2b, The reviewer did not find the reference Zhang et al., 1996. Why there is a "*" in Wang et al., 2005*? I also did not find this referece in the reference list. How many other references are missing from the reference lists?

13. Table 2: Are the elemental ratios mass based? The data from Zhang et al. (2003b) is based on electron microscopy-EDS which could only be regarded as semi-quantitative. In addition, the ratio provided in the Zhang et al. (2003a, b) is atomic number based. Has this atomic number based ratio be re-calculated to mass based ratio? Yes or no, this needs to be mentioned. Indeed, for integrity, it is suggested that the methodology for all the data presented in the Table should be briefly listed below the table.

14. Li et al. (2007), Shi et al., (2005), and Shao et al. (2008) reported extensive datasets on the carbonate content in the Asian dust. Li et al., 2009 (Geology, Natural and anthropogenic sources of East Asian dust) also anayzed the isotope ratio of Asian dust.

15. Page 31206, line 20: why weathering lead to a higher Fe/Al ratio? Reference? It appears that Fe/Al ratio is largely dependent on the parent rock rather than weathering.

16. Page 31206, line 1: Li et al. (2007) have some useful data for discussion here.

17. Page 31206, line 16 onwards: The opposite trend in Kandler et al. (2007) is another evidence of the heterogenity of dust properties in the same dust source region. The higher Ca/Al ratio shown in Table 2 may be related to the limited datasets in Africa and/or Asia. Recent data from Bristow et al. (2010) showed that Ca/Al ratio is highly

variable (even in the Bodele Depression itself) but generally below 1. The large variability of these ratios in the potential source region makes it very difficult to use them as a tracer.

18. Section 3.1.2: The paper by one of the authors (Tobo et al., 2010, PNAS) reported the effect of chlorite and nitrate uptake on Asian dust chemical and physical properties. This should be included in the review.

19. Page 31211, line 10: The paper by one of the authors in JGR by Hand et al. (2010) showed the internal mixing of soot with dust particles. Such mixing is also reported in Arimoto et al. (2006, EPSL).

20. Page 31218, line 11: Uptake of organic is not only important in areas where dust and biomass burning mix (see Sullivan et al., 2007b). VOC is everywhere and its concentration is high in anthropogenic plume as well. In addition, ATOFMS can identify some of the organic acids althout it cannot quatify them. So it is not accurate to say "no techniques is yet available . . ..".

21. Table 3: Please explain explicitely the parameters below the table, for example, CMD, GSD, VMD, BL, FT, a.g.l, a.s.l etc.

22. Fig. 2: Zhang et al. (2003)? Can't find it in reference list?

23. Fig. 1 and Fig. 2: There are areas that most of the papers agree but there are also areas which do not overlap. It would be better if a description of such uncertainty is given. Is the shaded area defined as the potential source area in this paper? If yes, what are the standards used to draw the boundaries. If not, what are the shaded area?

24. Fig. 1: it is said that the figure is based on Brooks and Legrand (2000), Schepanski et al. (2009) etc. But in the figure, no line or area is drawn for these two papers. How the results of these two studies are considered in this figure?

25. Radhi et al. (2010a) have reviewed the Fe/Al in literature. It seems that the ratios are much less variable than reviewed in Table 2. How representative are the

data presented in this paper? The references of Bergametti et al. (1989a, b) are missing from reference list again. The reviewer understand the difficulty of writing a comprehensive review like this paper, but the missing of key references again and again is not acceptable. This caused lots of trouble for the reviewer to evaluate the quality of the data chosen for review and to provide suggestions for revision.

The reviewer checked Bergametti et al. (1989, JGR) and again found that there are only case studies based on aerosol collections. This should be mentioned in the Table notations. Again, it need to be careful to use a single or a couple of data points to represent the geochemical and mineralogical properties of dusts from a particular source area.

26. Section 4 is difficult to follow. The reviewer suggests a re-organization of this section. It would be better if the synthesis or summary is presented first and then followed by recommendations. It would be ideal if the recommendations are numbered. The first several paragraphs in this section concentrates on the limitations of the methodologies, rather than progresses.

27. The recent progresses are not well summerized. The important progress in dust aging as described in this paper and their impacts on IN and CCN activity is not summerized in this section. The uptake mechanisms of nitrate, chlorite (Zhang and Iwasaka, 2001; Sullivan et al., 2009; Tobo et al., 2010; Shi et al., 2008; Laskin et al., 2005) and sulfate (Manktelow et al., 2010; Krueger et al., 2003; Laskin et al., 2005a; Shi et al., 2008) on dust is a major finding in the last few years in dust chemistry. But in page 31217, line 17: only recommendations are provided. This may give readers an impression that no progress has been made at all in this regard. The progress in fingerprinting dust source regios including isotopic ratios appear to be an important progress as well (this is why, the reviewer guess, so much attention has been made to address this issue in the paper) but is also not mentioned in the summary.

28. Considering the number of experienced researchers involved in this review, it is

striking to find that there are numerous technical errors throughout the manuscript. AAAA et al., 1996) should be AAAA et al. (1996). Sulfate or sulphate, please be consistent.

Some references which are mentioned above: 1. Levin, Z., Teller, A., Ganor, E., and Yin, Y.: On the interactions of mineral dust, sea-salt particles, and clouds: A measurement and modeling study from the Mediterranean Israeli Dust Experiment campaign, J. Geophys. Res.-Atmos., 110, D20202, doi:10.1029/2005JD005810, 2005.

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