Answers to Reviewer 2

We thank the reviewer for useful comments which we tried to address fully in the following.

1. Abstract: This paper did not provide strong evidence why future research should focus on the evaluation 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 variability of the dust properties is 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 mineralogical compositions of the dust aerosols from different source regions is still poorly known. Therefore, the reviewer believes that it is important to understand the evaluation 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 is 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 questionable. On the other hand, the super-large particles can serve as giant CCN and therefore may play some role in the cloud formation 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.

We agree with both statements. The abstract has been rewritten to take them into account.
This now reads "This paper presents a review of recently acquired knowledge on the physico-chemical properties of mineral dust from Africa and Asia based on data presented and discussed during the Third International Dust Workshop, held in Leipzig (Germany) in September 2008.
Various regional field experiments have been conducted in the last few years, mostly close to source regions or after short-range transport. Although significant progress has been made in characterising the regional variability of dust properties close to source regions, in particular the mineralogy of iron and the description of particle shape and mixing state, difficulties remain in estimating the range of variability of those properties within one given source region. As consequence, the impact of these parameters on aerosol properties like optical properties, solubility, hygroscopicity, etc. – determining the dust impact on climate – is only partly understood. Long-term datasets in remote regions such as the dust source regions remain a major desideratum.
Future work should also focus on the evolution of dust properties during transport. In particular, the prediction of the mineral dust size distribution at emission and their evolution during transport should be considered as a high-priority.
From the methodological point of view, a critical assessment and standardisation of the experimental and analytical techniques is highly recommended. Techniques to characterize the internal state of mixing of dust particles, particularly with organic material, should be further developed."

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 urges 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.

When relevant, this more recent literature has been added.

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

We change the title to “Recent progress in understanding physical and chemical properties of African and Asian mineral 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 combination of water droplets, pollutants, and dust; Dp<1um) (Seinfeld and Pandis, 2006). Haze and dust are completely different.

We replaced “haze” by dust emission events

6. Page 31207, line 28: Lafon et al. 2004, 2006 did not conduct any EM observations.
The reviewer is actually right. Those observations are provided in Lafon, 2004.
Lafon, S. (2004), Les oxydes de fer dans l'aérosol désertique en relation avec ses propriétés optiques: caractérisation physico-chimique de poussières minérales générées en soufflerie, Ph.D. thesis, 325 pp., Univ. Paris 12, Val-de-Marne, Créteil, France.
This has now been corrected.
We also added Greenland, D. J., J. M. Oades, and T. W. Sherwin (1968), Electron microscope observations of iron oxides in some red soils, J. Soil Sci., 19, 123– 126.

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.

This has now been corrected.

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.

This has now been corrected.

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.

The following paragraph was added to the introduction “Mineral dust particles, mostly clay minerals as kaolinite, illite or montmorillonite are thought to dominate the atmospheric nucleation of ice at temperatures below -12 ℃ (e.g., Mason 1971; Eastwood et al., 2008; Zimmermann et al., 2008). At higher temperatures, biological IN may play an important role as the highest temperatures of ice nucleation were reported for specific bacteria (e.g., Schnell and Vali, 1972; Möhler et al., 2007). Elevated IN concentrations were found in Saharan dust plumes transported over long distances to the eastern U.S. (DeMott et al.,2003) and to Europe (Klein et al., 2010). A robust correlation was found between the number concentrations of IN and of the particles above 0.5 µm diameter, which are often dominated by mineral dust (DeMott et al., 2010). Recent space observations support this by showing that the fraction of supercooled clouds (at the -20℃ isotherm) and the coincident dus t aerosol frequency are negatively correlated on the planetary scale, due to glaciation by dust (Choi et al., 2010). In the Amazon forest, IN were almost entirely composed of local biological aerosols and some imported Saharan dust (Prenni et al., 2009)”.

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 specifically 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 uncertainty 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 available at a longer time scale and/or are verified by several relevant papers. A longer term measurements on the mineralogical and elemental compositions of atmospheric dust aerosols are needed to confirm whether such heterogeneity is significant or not in the dust aerosols produced from each particular source region. Although the reviewer believes that such compilation worth doing, the potential uncertainty needs to be clearly identified in the manuscript.

The reviewer claims that we "ascertain the source region of an aerosol sample when the air mass passes that region“. However, as the submitted manuscript represents a review we only compiled the existing literature and consequently have to rely on the published data and authors statements. Of course, we are aware of the problems associated with source determination for example by means of back-trajectory analysis, but it is in most cases impossible to evaluate the quality of the source determination in the considered papers.
We agree with the reviewer, that the number of investigated samples plays an important role in the evaluation of the reliability of the data. Hence, we have added a supplementary table in which the sampling site, the number and type of samples, and the method of source determination are described. However, for consideration of the scientific quality, it will be always necessary to refer to the original work. We don’t want to have the reader to skip this step. A robust error quantification of the compositional data is partly hampered by the fact, that some of the presented compositional data has been extracted from graphs from the original literature. Therefore we decided to present the compositional data as a range of values and not as for example arithmetic mean plus standard deviation.
We also agree with the reviewer that it could be dangerous to deduce the mineralogical, isotope, or chemical composition of a potential source area on the basis of a few samples. But again we have to claim that the manuscript represents a compilation and only reviews the available data. If the data density isn´t better, we cannot do anything. However, the additional table for the supplement (see above) should at least yield an impression of the presented data quality.
Longer time series would be definitely useful. Nonetheless, it is important to evaluate the relative effort in obtaining those time series with respect to the benefit in using them to determine the dust impacts on climate. There is some evidence from the AMMA and the GERBILS programs that small-scale differences in mineralogical composition are not relevant to calculate optical properties and that as soon as one day after emission, which is the relevant time scale for regional and global observations. A comment on this has been added to the recommendation section (paragraph 4).

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 heterogeneity of mineralogical composition in the Bodele depression. In some part of 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 heterogeneity 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.

The reviewer claims that it is wrong to say that the carbonate content is very low in the Bodélé depression, and cites the geochemical studies of Washington et al. 2009, Moreno et al. 2006, and especially Bristow et al. 2010. Firstly, the above mentioned studies were not considered in Table 2, because in the present manuscript we mainly concentrated on chemical data given as mass concentrations (µg/m3) for an easier comparison with

Asian dust. Furthermore, the representativeness of the study of Bristow et al. (2010) may be questioned because their samples were taken in a comparatively small area. To our knowledge, the mineralogical study of Mounkaila et al. (2006) with more than 70 analyzed sediment samples from the area of the Bodélé depression still represents the most comprehensive mineralogical data set for this region. Of these 72 samples only 4 samples exhibit elevated calcite content, all other samples contain no calcite. Accordingly we changed the carbonate content from "very low" to "low" in Table 2. Thirdly, simply transferring elemental data (Ca) into mineralogy (calcium carbonate) as the reviewer proposed is not justified because Ca may also be incorporated in other minerals such as calcium sulfate or plagioclase (as discussed by Bristow et al., 2010, by the way). Finally, we are completely aware that the proposed source regions are heterogeneous with respect to composition of source sediments and emitted mineral dusts. However, we are convinced that Table 2 reports some important compositional trends that should be confirmed (or discarded) by future studies.
A sentence was added to explain this. "However, in potential source areas where a high number of mineralogical or chemical analyses is available, a significant compositional heterogeneity in one source region may be recognized. For example, the data sets of Mounkaila et al. (2006) and Bristow et al. (2010) reveal both a variety in mineralogical and chemical composition for the potential source sediments of the Bodélé depression in Northern Africa. In a few examples, even compositional trends in single source areas may be revealed (e.g., W-E trend of εNd(0) values in PSA EAS5 in Eastern Asia). "

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 reference in the reference list. How many other references are missing from the reference lists?

The missing reference were added
Wang, Y.Q., Zhang, X.Y., Arimoto, R., Cao J.J., and Shen Z.X.: Characteristics of carbonate content and carbon and oxygen isotopic composition of northern China soil and dust aerosol and its application to tracing dust sources, Atmospheric Environment 39, 14 2631–2642, 2005.
Zhang, X.Y., Zhang, G.Y., Zhu, G.H., Zhang, D., An, Z.S., Chen, T., and Huang, X.P.: Elemental tracers for Chinese source dust, Sci. China, Ser. D 39, 5, 512–521, 1996.
The "*" in Wang et al., 2005 was explained in the heading of the column of the table and means "analyzed by titration". This was intended to show the different analytical methods used for the determination of the CaCO3 content (XRD versus titration). However, we skipped the "*" to avoid confusion.

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 semiquantitative. 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.

Unfortunately, in Table 2 there is some confusion with the numerous Zhang et al. (2003) references. In Table 2, the references Zhang et al. 2003 a and b are now correctly cited as Zhang et al. 2003 c and d, respectively. They represent the following papers:

Zhang, X. Y., Gong, S. L., Arimoto, R., Shen, Z. X., Mei, F. M., Wang, D., and Cheng, Y.: Characterization and temporal variation of Asian dust aerosol from a site in the Northern Chinese deserts, J. Atmos. Chem., 44, 241–257, 2003c.

Zhang, X. Y., Gong, S. L., Shen, Z. X., Mei, F. M., Xi, X. X., Liu, L. C., Zhou, Z. J., Wang, D., Wang, Y. Q., and Cheng, Y.: Characterization of soil dust aerosol in China and its transport and distribution during 2001 ACE-Asia: 1. Network observations, J. Geophys. Res., 108, 4261, doi:10.1029/2002jd002632, 2003d.

In Table 2 all elemental ratios are based on mass concentration data (µg/m3). As mentioned in the table heading all chemical analyses that are given in weight % are not considered.

The heading of the table was changed to the following "Table 2a. Selected mineralogical (weight %), elemental (µg/m3 / µg/m3), and isotopic parameters for African mineral dusts and source sediments. Note: Chemical analyses given as mass fraction (weight %) were discarded, because they are very scarce for Asian dusts and are not directly comparable to atmospheric mass concentrations. Note also that carbonate contents were analyzed by different methods (e.g., XRD, titration). For X-ray diffraction analyses carbonate contents are given as the sum of the calcite and dolomite content. The following classification was used for the carbonate content: very low (0-1 wt.%), low (1-10 wt.%), intermediate (10-20 wt.%), high (> 20 wt.%)."

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.

The reference Li et al. 2007 is now considered in Table 2. The references Shi et al. 2007 and Shao et al. 2008 weren´t incorporated in Table 2 because samples were taken in Beijing and no potential source areas were given by the authors.

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.

We have re-written this paragraph. The sentence "The K/Al ratio shows little variability from one source area to the other. One could infer from the intense weathering in the sub-tropics that the Fe/Al ratio would increase from North to South over the region. Surprisingly, this is not an observed trend of the recent data-set." Was removed and replaced by "The Fe/Al and K/Al ratios show little variability from one source area to the other. However, Kandler et al. (2007)…."

16. Page 31206, line 1: Li et al. (2007) have some useful data for discussion here.
We added a sentence considering the paper of Li et al. (2007). "Dolomite (CaMg(CO3)2) has also been suggested as a possible source tracer for Asian dust (Li et al. 2007), offering the possibility to discriminate between dolomite-bearing dusts originated in PSA EAS1, PSA EAS3, and the western part of PSA EAS5 (Badain Jaran Desert) and dusts uplifted in other source regions (e.g., PSA EAS2, eastern part PSA EAS3 and PSA EAS6) without dolomite."

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.

As stated above, for a better comparison of the data we only considered chemical analyses of mass concentrations (µg/m^3) for the calculation of the elemental ratios given in Table 2. Consequently, the Ca/Al ratios from Bristow et al. (2010) calculated from analyses in ppm are difficult to compare with the data set presented in Table 2.
We also added a paragraph on the potential compositional heterogeneity of single source regions (see above).

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.
This has been done. See comment at point 27.

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).

We added a short paragraph that includes some additional references for the mixtures of soot and dust. The paper of Arimoto et al. (2006) just claims finding but doesn't show anything. The paragraph reads "Only few publications show the presence of some internal mixtures of soot and mineral dust in connection with intensive burning processes and high sample loadings (Parungo et al., 1992; 1994; Hand et al., 2010). For other situations, there is no report on internal mixtures of soot and mineral dust, while external mixtures of dust and organic carbon may be more frequent (Falkovich et al., 2004; Kandler et al., 2009; Leaitch et al., 2009; Deboudt et al., 2010; Matsuki et al., 2010). The presence of microorganisms on dust particles has been confirmed at various sites for Asian dust (Iwasaka et al., 2009), which is not surprising for surface soil particles."

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 although it cannot quantify them. So it is not accurate to say "no techniques is yet available . . ..".

We rewrote this sentence in the following way " In this field of research, we recommend that further attention is given to the deposition of organic acids on dust particles (Hatch et al., 2008). This process could increase the ability of the particles to absorb water vapour (Hatch et al., 2008). Note however that no technique is yet available to quantify organic acids in individual particles."

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

This has now been done

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

This has now been corrected. reference in Fig. 2 is now Zhang et al. (2003e): Zhang, X. Y., Gong, S. L., Zhao, T. L., Arimoto, R., Wang, Y. Q., and Zhou Z. J.: Sources of Asian dust and role of climate change versus desertification in Asian dust emission. Geophys. Res. Letter, 30, 24, 2272, doi:10.1029/2003GL018206, 2003e.

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?

The shaded areas in Fig. 1 and Fig. 2 represent indeed our recommendation. Outlines of the shaded areas are drawn by hand. This is now explained in the captions of Fig. 1 and 2. In our opinion, the heterogeneity of the data (especially for Africa) prevents a robust statistical approach.
The Figure caption have been changed to improve clarity.
Fig. 1. Potential source areas in Northern Africa (PSA NAF) based on work by Brooks and Legrand (2000) (not shown), Caquineau et al. (2002), Prospero et al. (2002), Israelevich et al. (2002), Goudie (2003), and Schepanski et al. (2009). Map of fraction of dust source activations (DSA) per day by Schepanski et al. (2009) (their Fig. 1) were transferred to isolines by hand. Outlines of potential source areas (shaded areas) are also drawn by hand.
PSA NAF-1: Zone of chotts in Tunisia and Northern Algeria; PSA NAF-2: Foothills of Atlas mountains (PSA NAF-2a) and western coastal region (PSA NAF-2b; Western Sahara, Western Mauritania); PSA NAF-3: Mali-Algerian border region; PSA NAF-4: Central Libya; PSA NAF-5: Bodélé depression (Western Chad); PSA NAF-6: Southern Egypt, Northern Sudan.
Fig. 2. Potential source areas in Eastern Asia based on work by Xuan et al. (2004), Laurent et al. (2006), Shao & Dong (2006), Kim et al. (2007), Wang et al. (2008), and Zhang et al. (2003e). Outlines of potential source areas (shaded areas) are drawn by hand.
PSA EAS-1: Taklamakan; PSA EAS-2: Gurbantunggut; PSA EAS-3: Kumtaq, Qaidam, Hexi corridor; PSA EAS-4: Mongolian (Northern Gobi) deserts; PSA EAS-5: Inner Mongolian (Southern Gobi) deserts: Badain Jaran and Tengger (PSA EAS-5a), Ulan Buh, Hobq, Mu Us (PSA EAS-5b); PSA EAS-6: north-eastern deserts (Otindag Sandy Land, Horquin Sandy Land, Hulun Buir Sandy Land).

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?

The data of Brooks and Legrand (2000) was also considered to approximate the outlines of the potential source areas in Figure 1. For sake of clarity, we decided to not include their data (their Figure 2.a) in Figure 1. This is now mentioned in the figure caption. The map of fraction of dust source activations (DSA) per day by Schepanski et al. (2009) (their Fig. 1) has now been incorporated in Figure 1. Again for the sake of clarity, we transferred their graph into isolines by hand. This is also explained in the caption of Figure 1 (see above).

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 understands 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.

We can´t follow the reviewer at this point. Radhi et al. (2010a) has not reviewed the Fe/Al in literature at all. Their list (see their Table 5) only exhibit the Fe/Al ratios of 3 sampling sites in eastern Asia and 3 sampling sites in Northern Africa. Furthermore, Fe/Al ratios by Lafon et al. (2006) are based on mass percentages and not on mass concentrations (µg/m^3) as in our Table 2.
However, from the data of Chiapello et al. (1997) (their table 1) we now also calculated the Fe/Al and K/Al ratios (as Radhi et al. 2010a) and included them in Table 2. We also included the data of Sun et al. (2005). The reviewer claims that the Fe/Al ratios presented by Radhi et al. (2010a) are less variable than values in our Table 2. Firstly, a large spread of values is also visible in the table of Radhi et al. (see for example Fe/Al ratios for "Chinese Desert Regions"). Secondly, we included much more references in our table, and hence we are convinced that the data set of Table 2 is much more comprehensive and representative than Table 2 of Radhi et al. (2010a).
We apologize that we missed some references in our reference list. The references Bergametti et al. (1989a and b) are added to the reference list.
Bergametti, G., Gomes, L., Coudé-Gaussen, G., Rognon, P., and Le Coustumer, M.-N.: African dust observed over Canary Islands: source-regions identification and transport pattern for summer situations. J. Geophys. Res. 94, D12, 14855-14864, 1989a.
Bergametti, G., Gomes, L., Remoudaki, E., Desbois, M., Martin, D., and Buat-Ménard, P.: Present transport and deposition patterns of African dusts to the north-western Mediterranean. In: Leinen, M. & Sarnthein, M. (eds.): Palaeoclimatology and Palaeometeorology: Modern and Past Patterns of Global Atmospheric Transport, NATO ASI Series, C, 282, 227–252, 1989b.
For a better evaluation of the data in Table 2, we added a supplementary table with references including the number of samples and the method of source determination.

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.

The section was reorganized to take into account the reviewer's comments

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 regions 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.

The progress of studies on the uptake of sulfate, nitrate and chloride was summarized in pages 31209 and 31210, except that the most recent result by Tobo et al. (2010) is not included. We do not think it is necessary to repeat those descriptions. In the revision, the following sentence is added before 'ACE-Asia ...' in line 13 on page 31210 to show the major result of Tobo et al. (2010). "They further showed that elevated concentrations of HCl in the remote marine boundary layer were sufficient to modify Ca-rich particles and could play a more important role in forming a deliquescent layer on the particle surfaces (Tobo et al. 2010)."
The following reference is added in the reference list in the revision: Tobo, Y., Zhang, D., Matsuki, A., and Iwasaka, Y.: Asian dust particles converted into aqueous droplets under remote marine atmospheric conditions, Proc. Natl. Acad. Sci. USA, 107 (42) 17905-17910;, doi:10.1073/pnas.1008235107, 2010.