

## ***Interactive comment on “Chemical characterisation of iron in Dust and Biomass burning aerosols during AMMA-SOP0/DABEX: implication on iron solubility” by R. Paris et al.***

**R. Paris et al.**

rodolphe.paris@lisa.univ-paris12.fr

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We would like to thank the two anonymous referees for their careful reading and thoughtful comments. We respond to the comments of each referee separately. Their comments are included in bold, followed by our responses. Since some of the referees have some of the same comments, we repeat our responses.

Anonymous Referee #2

I would, however, recommend to split the “results and discussion” section in one “results” and one “discussion” section. This should allow a much clearer “discussion” section. RP: To my opinion it’s difficult to split the Result and Discussion section in 2

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sections, because of direct linking between results and discussion. It is necessary to state the results about dust and discuss them before enunciate the result about BB samples.

Conclusion on P solubility: See table 2: 2 times more soluble P in BB samples, but the results with the PIXE method didn't allow us to calculate more precisely the solubility of this important nutrient. We add the biogeochemical implications because to my opinion iron solubility has an impact on the ocean, though in biogeochemical cycle.

In is present form, table 1 is not interesting. I would recommend having for each sample, the classification (DUST, BB1, and BB2) and the results of the chemical analysis. Due to the very large size of the resulting table, I recommend to had it as supplementary material to this manuscript. The main results could then be resumed in an additional figure. RP: I added for each sample the classification, and the result of Iron solubility, the main subject of the paper. The other chemical analyses are published in different paper (eg. Formenti et al., 2008, Chou et al., 2008). I also reduce the table 1, because it's important to have these informations in the paper.

p. 25024 l. 9: The word "significantly" is not supported by any statistical analysis in the manuscript RP: The word "significantly has been removed from the text.

p. 25024 l. 24-25: Citations on the influence of dust deposition on biogeochemical processes are missing. Add for example Mills et al., 2004 and Moore et al., 2007. RP: I add the reference to the text.

p. 25025 l. 12-13: The estimation of biomass burning solubilities in the study by Luo et al., 2008 is not based on the study by Guieu et al., 2005. Please check this reference. RP: Exactly, the study by Luo et al., 2008 is based on the study by Chuang et al. 2005. I have changed it in the paper.

P. 25027 l. 13-15: The use of this protocol to determine soluble elements should be justified. RP: Two methods are commonly proposed in the literature to determine

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Water Soluble Fraction of collected aerosols: by ultrasonication (Guinot et al., 2006, Karthikeyan et al., 2006, Chang et al. 2005, Wang et al., 2005) or vortex agitation (Falkovitch et al., 2005, Jaffrezo et al., 2005, Decesari et al, 2006). We have chosen the ultrasonication since it is a known and usual method and adapted to aerosol filters (We added in the paper the reference cited above).

p. 25028 l. 12-14: It should be demonstrated if this difference is statistically significant (t-test). RP:  $t = (15.9-7.8)/\sqrt{((116.4/17) + (116.4/36))} = 2.55$ . With a confidence of 95%,  $t = 1.96$  for this number of samples, that means the 2 mean values are statistically different. (116.4 is the common variance for all the samples)

p. 25028 l. 24-26: It is mentioned in the “material and method” section, that the estimation of light elements (like Al) could be underestimated. Is it possible to use a heavier element to estimate the particulate mass on the filter? Give also a reference for the composition of the terrestrial crust. RP: We use Al as a proxy of terrigenous elements. Even if, the estimation could be underestimate we used it to compare the chemical composition of terrigenous element in all the samples, and to confirm that in BB samples are dust particles internally mixed with biomass burning plumes. Composition of the terrestrial crust: I added Mason, 1966.

p. 25029 l. 17-29: Results presented in this section are very interesting. Is it possible to distinguish between the groups BB1 and BB2 by air mass trajectories calculation or altitude? RP: We calculate few air mass trajectories in order to distinguish the 2 groups. But we didn't find any correlation between the group and the origin. To my opinion the difference between the BB1 and BB2 group is the stage of mixing between dust and biomass burning particles. The discussion about the impact of the age or the origin of the air masses has been developed in the text. p8 L9-11: "If we compare our group samples BB1 and BB2 to Formenti et al., 2008, there is no correlation with the aged and fresh biomass burning samples, so the precedent hypothesis shouldn't be the good one."

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p. 25030 l. 2-4: I am not sure if nssK/Fe and excK/Fe can be compared. One is corrected from sea salt and the other from dust influence. RP: In the case of nssK of Guieu et al. (2005) or excK in our paper, the K content is corrected from the major local influence: sea salt for Guieu and dust for us. In consequence, both nssK and excK well represent K issued from biomass burning aerosols.

p. 25030 l. 7-10: This part of the discussion is a little bit poor. The ideas developed here should be more detailed. RP: We detailed this part : "The dust particles that are coarser than combustion particles should be removed by gravitational settling involving a decrease of Fe content in the air mass. We study the number size fraction for different air masses (DUST and BB), measured by PCASP (Osborne et al., 2008). The results show no differences in the distribution. That means that dust particles are removed in the same order of magnitude than BB particles during transport; or the sampling is too close to the source to determine any difference."

p. 25030 l. 14-15: I recommend presenting these results in the form of supplementary material to the manuscript. The word "similar" is subjective in this context. RP: The sentence "Soluble fractions . . . of terrigenous origin . . . are similar in all samples" means that for each element the fractional solubilities in the DUST and BB samples are similar. This sentence has been changed to be clearer by "The soluble fractions of typical elements of terrigenous origin (Al, Fe, Si, and Ti) are lower in DUST samples than in BB samples, but always with values inferior to 3%.". It has been also moved at the end of paragraph since it was not at the right place in the discussion. Moreover, we added a discussion on the behaviour of terrigenous elements and iron about redox state as proposed. p12 L14-17: I added "However, the increase of solubility in BB samples is also observed for the terrigenous elements which are non-redox active (see above). Consequently, a change of oxidation state of iron is probably not significant in enhancing iron solubility in mixed samples."

p. 25030 l. 19: Confidence interval should be given for regression values. RP: The range of R2 is given for the different species and is not a range of R2 for one species.

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p. 25030 l. 25-27: This affirmation is difficult to understand. This part of the discussion should be more detailed. RP: I reformulated this part as follow: "As oxalate has been only determined in biomass burning samples, this indicate that this is the primary source in our samples. Moreover, oxalate concentrations present a negative correlation with K/S ratio ( $R2 = 0.8$ ), indicating that oxalate is formed in the condensation mode during transport."

p. 25031 l. 17: Please better define the meaning of the "zone of transport" RP: The "zone of transport" is the marine atmosphere, where the particles are transported from the source. I added to the text "zone of transport over Ocean" to precise the meaning of "zone of transport".

p. 25031 l. 8-10: It should be mentioned here that the protocol used by Baker at al., 2006 is different than the one used in this study (Different pH of the leaching solution). Comparing these two studies must be done under caution. RP: I added the difference in the leaching protocol to the text.

p. 25031 l. 8-10: The study by Baker et al., 2006 concludes that particle loading in solution as a minor effect on the dissolution of iron. It is not clear here, if the authors mention the effect of particle loading in the leaching solution or particle loading in the atmosphere. RP: I added to the text that we are talking about the loading particles in the atmosphere.

p. 25031 l. 17-20: The recent reference (Buck et al., in press) on the effect of particle size distribution on solubility could be mentioned here. In this study, also, no clear effect of particle size on dissolution has been determined. RP: I add this reference to the text.

p. 25032 l. 5-6: This section on the effect of mineralogy on dissolution is particularly interesting. However, it is not clear to me if the four samples with a different SMg/SFe ratio are clearly originating from other sources of dust. This should be explained with more details by the back trajectories calculations. RP: Except for the three samples

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originate from Bodélé, which are easily identifiable from their Si content and diatoms presence and highly influenced by the Bodélé source due to very intense dust storm, it is very difficult to associate a sample to a dust source by back trajectories. Indeed, since we didn't collect directly at the dust emission source, the dust layers sampled by aircraft are often composed by a mixing of different dust source (see e.g. Formenti et al., 2008). In consequence it is not possible to distinguish one source for one sample, and hence to follow up more precisely the iron solubility variability as a function of source.

p. 25032 l. 20: A sentence to explain briefly the SEVERI dust measurement should be added (or a reference). RP: We added the reference of SEVIRI website in the manuscript ([http://radagast.nerc-essc.ac.uk/Data.htm#SEVIRI\\_data](http://radagast.nerc-essc.ac.uk/Data.htm#SEVIRI_data)) (see p11 L11). SEVIRI is a 50 cm-diameter aperture, line by line scanning radiometer, which provides image data in four Visible and Near-InfraRed (VNIR) channels and eight InfraRed (IR) channels.)

p. 25032 l. 23-24: A reference should be added concerning the transport of diatoms rest in dust originating from the Bodélé depression. RP: I add the reference Washington et al., 2006

p. 25033 l. 1-2: "2 orders of magnitude" may be replaced by "two times". The statistical significance of this difference should also be mentioned. RP: Indeed the difference is approximately a factor of 2, not 2 orders of magnitude. We changed it in the text

p. 25033 l. 9: This sentence is confusing. RP: I rewrite this sentence as follow: "In comparison with dust particles, biomass burning aerosols, in this study, are not a significant direct source of soluble iron."

Technical corrections p. 25024 l. 4-5: The use of the commercial description "Milli-Q" should be avoided in the abstract RP: The term has been removed from the text

p. 25024 l. 4-5: The sentence "Two types of samples are encountered in this period"

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is a somehow confusing: "Aerosols" are encountered in this period but not "samples".  
RP: The sentence has been rewrite: " Two types of aerosol samples are encountered in this period:"

p.25025 l. 25-26: Reformulate "to the supply of iron soluble elements in the atmospheric deposition" RP: The sentence has been reformulate: "we present an estimation of African dust and biomass burning aerosols contribution to the supply of soluble iron to the ocean by atmospheric deposition"

p.25028 l. 22: Reformulate ".. iron is due to mineral dust iron ." RP: I changed to "indicating that even in BB layers, mineral dust is the source of iron, not vegetation combustion."

p. 25030 l.26: Please check if the word "anticorrelation" is adequate. Maybe the right expression is "negative correlation". RP: we change "anti-correlation" to "negative correlation" in the text.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 25023, 2009.

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