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Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

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.

Anonymous Referee #2

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General comments:

The manuscript by Paris et al. is an important contribution in the field of atmospheric chemistry. This manuscript deals with the differences in chemical composition and iron solubility from aerosols particles collected during research flights of the AMMA-SOP0/DABEX experiment. Some new information on factors controlling the chemical characteristics and iron solubility in transported aerosol particles are presented. This is a crucial question in order to better understand the impact of atmospheric iron deposition on marine biogeochemistry. To my opinion the two major results from this manuscript are: (1) the new information on mixing between dust and biomass burning aerosols and (2) the in-situ evidences of a control of iron mineralogical speciation in dust particles on its solubility. The manuscript deserves publication in Atmospheric Chemistry and Physics after significant revisions. My four major concerns are:

- The discussion part of the paper is sometimes poor and some important literature references are missing. These points will be more detailed in the specific comments. 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.

- The final conclusion of the manuscript is not supported by the presented dataset. The scope of the journal is not marine biogeochemistry and I therefore recommend rewriting this part of the conclusion and avoiding the most speculative biogeochemical implications.

- The methods to estimate iron solubility from aerosol particles is subject to large discussion in the marine and atmospheric science community. This should be reflected in the discussion section of this manuscript.

- 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.

Specific comments:

- p. 25024 l. 9: The word “significantly” is not supported by any statistical analysis in the manuscript (See also later comments).

- 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.

- 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

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reference.

- p. 25027 l. 13-15: As mentioned in the “general comments”, the use of this protocol to determine soluble elements should be justified.

- p. 25028 l. 12-14: It should be demonstrated if this difference is statistically significant (t-test).

- 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.

- 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 a airmass trajectories calculation or altitude?

- 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.

- p. 25030 l. 7-10: This part of the discussion is a little bit poor. The ideas developed here should be more detailed.

- p. 25030 l. 14-15: I recommend presenting this results in the form of supplementary material to the manuscript. The word “similar” is subjective in this context.

- p. 25030 l. 19: Confidence interval should be given for regression values.

- p. 25030 l. 24-25: In my version of the manuscript, this sentence is not complete.

- p. 25030 l. 25-27: This affirmation is difficult to understand. This part of the discussion should be more detailed.

- p. 25031 l. l7: Please better define the meaning of the “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.

- 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.

- 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.

- 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.

- p. 25032 l. 20: A sentence to explain briefly the SEVERI dust measurement should be added (or a reference).

- p. 25032 l. 23-24: A reference should be added concerning the transport of diatoms rest in dust originating from the Bodélé depression.

- 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.

- p. 25033 l. 9: This sentence is confusing.

- p. 25034 Conclusion: The last sentence of the conclusion on P inputs by biomass is surprising and is not supported by any data. The paper should be concluded by a less speculative sentence.

Technical corrections

- Title: “Dust” and “Biomass” have a capital letter in the title.

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Interactive Comment

- p. 25024 l. 4-5: The use of the commercial description “Milli-Q” should be avoided in the abstract.

- p. 25024 l. 4-5: The sentence “Two types of samples are encountered in this period” is a somehow confusing: “Aerosols” are encountered in this period but not “samples”.

- p.25025 l. 25-26: Reformulate “..to the supply of iron soluble elements in the atmospheric deposition.”.

- p.25028 l. 22: Reformulate “.. iron is due to mineral dust iron ..”.

- p. 25030 l.26: Please check if the word “anticorrelation” is adequate. Maybe the right expression is “negative correlation”.

References

Buck, C.S.; W. M. Landing and J. A. Resing (in press), Particle size and aerosol iron solubility: A high-resolution analysis of Atlantic aerosols, Marine Chemistry, doi:10.1016/j.marchem.2008.11.002.

Mills, M.M.; Ridame, C.; Davey, M.; La Roche, J. and Geider, R.J. (2004), Iron and phosphorus co-limit nitrogen fixation in the eastern tropical North Atlantic, Nature 429(6989), 292-294.

Moore, J.K.; Doney, S.C.; Glover, D.M. and Fung, I.Y. (2002), Iron cycling and nutrient-limitation patterns in surface waters of the World Ocean, Deep Sea Res., Part II 49(1-3), 463-507.

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