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Comment

Interactive comment on “Accelerated dissolution of iron oxides in ice” by D. Jeong et al.

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I would like to highlight some issues and ask a few related questions regarding this interesting report of the effect of the frozen phase and binding of organic species on the dissolution of different forms of iron oxide particles.

1) Although the BET SAs for the iron oxide samples are quoted, there seems to be no indication of particle size or size dispersion which presumably is the key controlling factor for consideration of the dissolution rates/trends reported. The SAs are quoted with surprising precision i.e. single values, which would suggest monodisperse samples – this surely is not the case? To my knowledge, the commercial products stated usually come with a range of quoted values (and/or mean particle size) which reflect

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the inherent size dispersion. Page 5, line 24 states particles are ‘nanosized’, but can the authors define this further as 100s of nm or 10s of nm or a dispersion of both dimensions? Presumably it is the latter as deposition of particles was observed (page 4, line 24). Would it not be better to report dissolution rates (microM per hour) from Figure 2 normalised to the respective BET SA? In these terms, goethite has the lowest rate and magnetite the highest. How representative are the particle sizes/SAs used to ‘real’ iron-containing dust/soil samples?

2) In the abstract (lines 9-10) and summary (page 10, lines 15-16), the authors state that there is a compositional/structural effect for the reported dissolution trends. I see no definitive evidence for this in the paper and, following from my first point, suggest the dissolution trends for experiments without organic binding to be predominantly a consequence of the varying particle sizes within samples of the different oxides. The trend of the oxide with the highest SA (i.e. contains the larger particles) having the highest dissolution rate, the one with the lowest SA, the lowest rate and those for which similar SAs are quoted have similar dissolution rates (bottom of page 6 / top of page 7) seems to indicate that, at best, the form of oxide plays only a relatively minor role in the dissolution behaviour compared to particle size.

3) In both aqueous and frozen phases, another factor to be taken into consideration is that the particle dissolution rate is affected by the presence of surface defects (i.e. pits, cracks etc – see Jeschke and Dreybrodt, 2002; *Geochim. Cosmochim. Acta*, 66, 3055-), in which water can freeze and ions concentrate? These are likely to be more prevalent in the larger particles within samples. The role of such an ‘inhomogeneous’ dissolution process (even more important for ‘real’ dust particles which will have greater surface roughness than synthesised, commercial samples) would be more realistic than an idealised homogeneous dissolution at smooth outer surfaces as seemingly implicitly assumed by the authors. Maybe, any planned future studies could include TEM/SEM particle imaging of ‘before and after’ samples to answer this point.

4) I think the previous points indicate the importance of studies on monodisperse samples if possible in future which would prevent any such uncertainties on the role of

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particle size dispersion. Is it not possible to grind/filter the initial samples accurately enough to produce monodisperse samples or at least reduce the likely size dispersion of sample prior to dissolution? Maybe the authors could try to produce their own samples in future as sol-gel synthesis of monodisperse iron oxide nanoparticles is well described in the literature (e.g. Matijevic, 1993; Chem. Mater., 5, 412-)?

5) I'm sure spelling errors will be sorted out in the review process but I would highlight the inconsistent spelling of 'organic' i.e. page 7, line 6 and line 9.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 20113, 2012.

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