

## ***Interactive comment on “Formation of secondary organic aerosol from isoprene oxidation over Europe” by M. Karl et al.***

### **Anonymous Referee #1**

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Just some short comments to the author’s reply:

I am still not convinced by the arguments that sulfate is treated in a reasonable way. The authors are still applying fairly standard partitioning theory, as widely used for organic partitioning. Such theory was not intended for mixtures of inorganics, organics, polar and non-polar compounds. Where are the effects of ions in the proposed methodology? Or phase-partitioning? The authors cite Bowman and Karamalegor (2002) to defend their approach, but that paper explicitly deals only with miscible organic-water systems, not with inorganic ions. There is a big difference! Many papers discuss the complexities of inorganic/organic mixtures, e.g. Clegg et al (ACP, 2008), Erdakos et al (Atmos. Env., 2006), Zuend et al. (ACP, 2008), and all highlight the complexities.

Further, the authors suggest that somehow this test is simulating organosulfate formation. Now the arguments are being mixed, since the Wilson equation is really not intended for such chemical reaction steps!

It is fine to use simplified expressions, but only when there is a good physical basis for the simplifications. Do the authors have any experimental evidence that sulfate can be handled in this way? The experiments of Kleindienst et al. (Atmos. Env., 1999) suggested that standard organic partitioning theory worked for the organic phase only, even in mixed sulfate-organic aerosols? The cited works of Kroll involve other processes than sulfate being part of an absorbing pool. If the authors can provide experimental evidence that their sensitivity test (which is one concerning absorption, not reactions) has any physical basis I can accept it. If not, I see little point in repeating this type of test.

Concerning the no-evaporation tests, then I suggest the authors find a different terminology, or explain more clearly what is going on. The methodology is explained in Tsigaridis+Kanakidou, but this method should not be called a no-evaporation method. What they are really doing is assigning a lifetime (absolute, not  $1/e$ ) to the semivolatile compound which is equal to the model's timestep. This also seems very unphysical, but is at least simple to explain. Make it clear what this timestep is though.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2855, 2009.

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