

## ***Interactive comment on “Do organic surface films on sea salt aerosols influence atmospheric chemistry? – A model study” by L. Smoydzin and R. von Glasow***

### **Anonymous Referee #1**

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The effect of film forming compounds (FFC) on gas phase and aqueous phase chemistry in the marine boundary layer is modeled. This is a valid and important question, but modeling it suffers from substantial lack of detailed information about the nature of the organic component of sea salt aerosols and on the detailed processes involved. Because of the lack of information, the authors have decided to choose oleic acid as a model for the marine FFC. I agree with the previous referee comments published here and would like to add a few others that may help the authors in their modeling approach.

The authors conclude that the key parameter in affecting any process here is the film

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lifetime. The choice of pure oleic acid as representative compound maybe poor, as also concluded in the paper due to several factors. However, there are various processes that have already been pointed out in the literature that should be included or at least considered in the model in order to at least attempt to make it more realistic:

1. Even the use of a pure monolayer of oleic acid requires a different modeling approach, namely adopting Langmuir-Hinshelwood. It has been shown (Ammann, et al., 1998; Kwamena, et al., 2006; Poschl, et al., 2001; Poschl, et al., 2005) that the reactive uptake coefficient decreases with time due to depletion of the reactive sites, and can change due to competitive adsorption by water and due to matrix effects. Some of these processes may reduce the reactivity of ozone towards the layer substantially.

2. Recent studies have shown that the layer composition and phase may strongly affect the reactivity of oleic acid in aerosol particles (Hearn, et al., 2005; Hearn and Smith, 2005; Knopf, et al., 2005). Such processes may play a role in this system as well, and will also increase the lifetime of the layer.

3. Oleic acid has been studied by many groups as model system and as probe for understanding aerosol phase processes. It has to be remembered that there is a limit to how much one can take the simplified model systems and assume that that the atmospheric system behaves as compact and ordered organic monolayer. This should be stated in the paper clearly, as has been often been remarked by the groups who conducted oleic acid studies.

Other factors that have been overlooked: 1. There are other measurements on the uptake of  $\text{N}_2\text{O}_5$  by coated aerosols, showing a decrease due to a thick coating by about an order of magnitude (Folkers, et al., 2003). There is also evidence that the diffusion of ammonia through a monolayer may be slowed (Daumer, et al., 1992).

2. It is possible that the products of the oxidation process actually dissolve in the aerosol phase. Increased dissolved multifunctional organic matter may affect the aqueous phase chemistry of the radicals (Exner, et al., 1994; Hermann, et al., 1995; Moise,

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et al., 2005; Neta, et al., 1988). How would that affect the chemistry? 3. Reactive uptake of NO<sub>3</sub> (Knopf, et al., 2006) and of halogen atoms (Moise and Rudich, 2001) as well may affect the rate of loss of the organic layer. This should be at least mentioned and discussed in the paper. 4. How would the monolayer composition and structure affect mass transfer from the aerosol phase to the gas phase, and has that been modeled here?

Therefore, I suggest that the authors will add caveats to their model, try to test the effect of a better kinetic scheme on the monolayer lifetime, and discuss these issues in the revised paper.

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