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Atmospheric
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Interactive comment on “Collision dynamics and uptake of water on alcohol-covered ice” *by* E. S. Thomson et al.

Anonymous Referee #1

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The manuscript “collision dynamics and uptake of water on alcohol-covered ice” by Thomson et al. provides data from measurements of the scattering and uptake of heavy water on ice covered by methanol and butanol. The authors show that a monolayer of butanol reduces water uptake by around 20% while a monolayer of methanol does not result in a measurable decrease. Considering the potential impacts of surfactants on aerosol processes, this work is certainly relevant.

In general the paper is well written and the data are appropriate for ACP. The authors should address the following points in a revised submission.

Specific comments:

1. Line 10 of 27640: “there is evidence of incomplete mixing [of water and methanol]

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at microscopic levels." And line 6: "suggests incomplete mixing on a molecular scale." Since the formation of a methanol monolayer on water ice is central to this work, I would like a more definitive statement to this effect. Later in section 3.1, you "experimentally verify" monolayer coverage. If no one else has definitively shown that methanol forms a monolayer on water-ice, do your measurements do so? If the authors cannot make a more definitive statement on the monolayer formation of methanol on water ice, I would then appreciate a discussion of what experimental result would be expected if the assumption of monolayer formation is not borne out.

2. Page 27644 line 5-8. I found this section confusing. Why does k have to be forced to be $>>$10^3 s? That is, why doesn't this arise naturally?

3. Also concerning section 2.2, the authors state that there are five free parameters. Have the authors done any study to demonstrate that the fitted parameters are indeed a global best fit, and not one of many possibilities (as would be the case if two or more parameters are compensating one another)?

4. I appreciate the careful error analysis that the authors performed, but Fig 2 presents some difficulties. For example, the butanol data below 170 K make it "impossible to rule out temperature dependent uptake" (p. 27651, line 13. Why not make the direct, positive statement? Your data show a very strong trend compared to the size of the error bars in that region.

5. Similarly in Figure 2, why are some error bars very large while others are small? In particular, there are two red circles at around 172 K which have small error bars but are in disagreement with one another by around 0.2. This implies that either (i) the (statistical) error bars are underestimated or (ii) an unaddressed systematic error occurred.

6. In Fig. 5, do the "higher temperatures" at which decay is too fast correspond to

175 K $< T <$ 200 K? Or should we interpret this as a statement on the reliability of the higher-temperature data displayed in this plot?

7. In Fig. 5, two fits are offered, one of which removes the two lowest temperature points. The authors suggest [pg 27650 line 15-16] that a change of kinetic behavior may have occurred. More justification is needed here for excluding these points. Why sort of kinetic change could cause such a strong shift in kinetic behavior over just a few K?

Minor comments:

Ensure vs. Insure. I believe you should rather say that you set vapor fluxes to **e**nsure steady state conditions. This happens in several places.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 27637, 2012.

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