Brus et al. measured the RH and temperature dependence of the diffusion coefficients of gaseous H_2SO_4, using a flow tube, and the performance of the experimental set-up is supported by CFD simulations. A much stronger effect of temperature, compared to theories, has been reported, and cluster kinetics modeling has been used to explain and interpret their experimental data. Diffusion coefficients of H_2SO_4 are of great importance in atmospheric chemistry, and the results reported by this work are interesting. Nevertheless, I do have a few major concerns which the authors should address before I can recommend this manuscript for final publication:

1) Line 384-386: I do not believe that the CFD simulation can confirm the assumption that the wall of the flow tube is an infinite sink. Actually this is a critical assumption already made in the model (line 166). The modeling result that $[H_2SO_4]$ on the flow tube wall is less than 6% of $[H_2SO_4]_0$ is determined by the spatial resolution used in the model; if a higher resolution is used, $[H_2SO_4]$ on the flow tube wall will be closer to 0. A proper way to confirm this assumption is to measure the diffusion coefficients as a function of pressure in the flow tube, as described in previous work (Fickert et al., 1999; Liu et al., 2009). Though I believe that wall used by Brus et al. should be an infinite sink for H_2SO_4, these incorrect statements need to be changed and the proper way to confirm this assumption should be mentioned.

2) I disagree with their proposed temperature dependence of H_2SO_4 diffusion coefficients (D). Examination of the experimental D at different T and RH shown in Figure 3 reveals that for the same RH, within the experimental uncertainties there is no significant difference between D measured at 278 K and at these measured at 288 K. The very strong dependence of D on T suggested by the authors are based on three data points at i) 298 K and 4% RH, ii) 288 K and 8% RH, and iii) 278 K and 26% RH (line 290-296). From i) to iii), T decreases and RH increases, both very likely leading to the decrease in D; in addition, Figure 3 shows that for RH below 15%, RH has a strong effect. One may conclude that instead of temperature, the change of RH can play the major role; therefore, the strong temperature dependence suggested by Brus et al. is not convincing if not wrong.

3) Even if their proposed temperature dependence is correct and can be justified, I feel this study is not complete or does not provide much insight with broad implications. A strong T dependence was found and can be explained by clustering with amines. However, the possible presence of amines is an experimental artifact. This manuscript has not yet answered the key question, i.e. the true dependence of $D(H_2SO_4)$ on T, and thus currently may not be suitable for publication by ACP which requires studies with general implications for atmospheric science.

Reference

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