

Interactive comment on “The atmospheric chemistry of sulphuryl fluoride, SO₂F₂” by T. J. Dillon et al.

T. J. Dillon et al.

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We thank reviewer 1 for his/her useful comments.

Comment The one drawback to this study, and it does not preclude it in any way from deserving to be published, is that the most likely loss process for sulfuranyl fluoride is found to be VUV photolysis in the stratosphere and that process was not studied as part of this work.

Reply The focus of this work was to quantify as yet unknown loss processes for SO₂F₂. Photolysis lifetimes in the stratosphere could already be calculated using previous measurements of the absorption cross sections. Our calculation of a lifetime of circa 500 years will have uncertainty related to measurement of cross sections of a stable compound and are unlikely to be significantly wrong.

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Comment p. 15218 - I am assuming that measuring the concentration of SO₂F₂ by manometric methods involves putting a flow of a mixture of SO₂F₂ through a flow meter. Is this flow meter heated? Is there any chance that SO₂F₂ could be lost inside the flow meter?

Reply SO₂F₂ is stable both thermally and with respect to surface reactions. It is not conceivable that it will be lost within the short residence time in a slightly warm flow meter.

Comment p. 15219, line 26 - is "about 8 s" the most precise that this time can be reported for this time?

Reply The number presented gives the reader an idea of the approximate, maximum gas residence time. The accurate contact times were of course used in the analysis and are shown in Figure 8. In the revised manuscript, the reader is referred to Figure 8.

Comment p. 15222, line 27 - it would be helpful to list the recommended values from Sander et al. and Atkinson et al. for k₁₁ so that the reader can make his/her own evaluation of what is "good agreement"

Reply Rather than listing 4 extra rate coefficients (none of which were the focus of this study) to make the comparison in detail, we now state that the results agree with IUPAC and NASA evaluations within the combined uncertainties.

Comment p. 15233, line 2 - a reference should be provided for the 10710;6 kg/year number

Reply The value of 1e6 kg per year refers to emissions from the state of California only and is referenced in the introduction. We now clarify this.

Comment p. 15239, Table 1 - I applaud the authors for including this table that has a number of experimental parameters listed in it; I would encourage the authors to include even more experimental details (pressure, H-donor concentrations, laser flu-

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ence, etc.) so that it is easier for future experimentalists who wish to repeat these experiments to do so

Reply As we state in the text, the other experimental parameters (pressure, laser fluence and concentration of H-atom donor) were not varied in each experiment. All experiments were carried out at pressures of close to 60 mbar and the laser fluence was typically 12 mJ / cm²/ pulse. The concentration of the H-atom donor was also held constant at 61627; 2e13 molecule / cm³. We will revise the text (experimental section and footnote to Table 1) to clarify this.

Technical corrections p. 15219, line 15 - remove word "about" in describing pressure range p. 15226, line 18 - superscript for k1 rate coefficient should be "-10" instead of "-11" p. 15230, line 12 - upper limit should not be reported as "= 1 x 10⁻⁷", but rather "< 1 x 10⁻⁷"

Reply These technical corrections will be taken care of in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 15213, 2007.

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