

Interactive comment on “A Fabry–Perot interferometer based camera for two-dimensional mapping of SO₂ distributions” by J. Kuhn et al.

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I've read with great interest the manuscript of Kuhn et al. entitled “A Fabry-Perot interferometer based camera for two-dimensional mapping of SO₂ distributions” The paper presents a novel theoretical concept for measuring SO₂ by UV spectrometry. It falls completely in the scope covered by AMT. Although being at this stage a purely theoretical concept, the study and calculation of the optimal parameters of the future instrument are scientifically sound, and I'm convinced that this research group (arguably one of the most experimented in this research area) will soon be able to produce a research prototype of an SO₂ imaging system based on the presented concept. So I recommend the publication of the manuscript once the following points have been addressed

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and changed in the manuscript. This is equivalent to moderate revisions. 1) Overall, the wavelength selection and the spectral response of the proposed instrument is very similar to the COSPEC instrument, except that the selection mechanism is different (interferometer here, opto-mechanic system for the COSPEC). Therefore I believe more references should be made to this instrument. I suggest adding the following two references in the manuscript. Millán, M. (2008) Absorption correlation spectrometry. In: Williams-Jones, G., Stix, J. & Hickson, C. (eds.) The COSPEC Cookbook: Making SO₂ Measurements at Active Volcanoes. IAVCEI, Methods in Volcanology, 1, 1-62. and Moffat A.J. and Millán M.M. (1971) The application of optical correlation techniques to the remote sensing of SO₂ plumes using skylight. Atmospheric Environment, v. 5, p. 677-690.

2) The author should expand the simulations of the instrumental response towards higher slant column amounts of SO₂. SO₂ camera are commonly applied to very young proximal plumes, whose SCAs often exceeds 5.10¹⁸ molec/cm². I expect that for these highly concentrated plume the apparent absorbance could start showing a significant saturation effect (i.e. the absorbance at the “peak” wavelength does not increase anymore due to a close to 1 optical thickness while the absorbance at the “through” wavelengths continue to increase) 3) From their simulations of the ozone response of conventional SO₂ cameras, the authors conclude that the change of the apparent absorbance caused by an increase of 100DU (equivalent of a SZA change from 30 to 48°) is superior by 110% to the AA caused by 1 10¹⁸ molec/cm² of SO₂. If we do observe an instrumental drift with SZA in the “real world”, it is not of such a high magnitude, it might be worthwhile checking the simulations again. 4) I have the general feeling that the paper is maybe too optimistic about the insensitivity of the FPI-camera towards radiative transfer effect. The authors convincingly demonstrate it is the case for wavelength-dependent extinction due to Mie-scattering, but unless I'm wrong, the other issues (such as light dilution and non-ideal light paths through the plume) remains. They could possibly be enhanced in the proposed set-up because the shortest wavelengths of the UV spectrum (which are the most affected) are included in

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the sensitivity range of the FPI-camera instrument. The authors should clarify this point or may be present some simulations of these effects on the FPI-camera's response.

Technical/Minor corrections

A definition should be given for the etendue

Citing Oppenheimer et al. (1998) might be inappropriate in this context, since the main result of this study is that the lifetime of SO₂ is of the order of a few hours. Further investigations have questioned this result (eg. Mc Gonigle et al. 2004, Nadeau et al., 2008)

p.5126, ln 29 change singal for signal

p.5128 ln 2, change pixle for pixel

p.5128 ln 15, change shiftet for shifted

p.5128 ln 14, add a coma after moreover

ln 270, add a coma after "for the second set of lines

ln 265-275, specify which value of single scattering albedo was assigned to the aerosol fig.1. It seems from comparison with fig, 3 that the peaks of the FPI transmission should not reach 1 as suggested in figure 1. Would it be more appropriate to talk about normalized transmission? Or to change the graph ordinates for showing the absolute transmission.

Please also note the supplement to this comment:

<http://www.atmos-meas-tech-discuss.net/7/C1845/2014/amtd-7-C1845-2014-supplement.pdf>

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 5117, 2014.

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