Interactive comment on “A Fabry–Perot interferometer based camera for two-dimensional mapping of SO_2 distributions” by J. Kuhn et al.

Response to the comments of R. Campion (Referee) (Robin.Campion@ulb.ac.be)
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We like to thank the reviewer for his valuable comments, which definitely helped to improve our manuscript. In the following we repeat the comments of Robin Campion and add our comments in italic face.

I’ve read with great interest the mauscript of Kuhn et al. entitled “A Fabry-Perot interferometer based camera for two-dimensional mapping of SO2 distributions” The paper presents a novel theoritcal concept for measuring SO2 by UV spectrometry. It falls completely in the scope covered by AMT. Although being at this stage a purely theorical concept, the study and calculation of the optimal parameters of the future instrument are scientificall sound, and I’m convinced that this research group (arguably one of th 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 recommand the publication of the manuscript once the following point have been addressed 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.

The following sentences were included in the introduction:

“The FPI technique introduced here is in general similar to the COSPEC method, which has already been successfully applied at various volcanoes for decades (Millan et al., 2008). However, the opto-mechanic system is replaced by interferometer optics, resulting in a smaller, more robust and cost-efficient design, which can record one- or two-dimensional data with high temporal resolution.”

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 5.1018molec/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 e the absorbance at the “through” wavelengths continue to increase)

We expanded the simulations to SO2 column densities of up to $1x10^{19}$ molec/cm^2, a value that may only be observed close to the volcanic vent of very strong emitters. The updated figures (5 (a) and 5 (b)) show a saturation effect at higher column densities due to the large

absorption at lower wavelengths. However, this saturation only leads to a reduced sensitivity at very high SO2 absorptions and may be dealt with by carefully calibrating the apparent absorbance. (We added this explanation in section 3.1)
In addition, when measuring at volcanoes with very high SO2 emissions a band-pass filter at higher wavelengths which blocks the wavelengths of saturated SO2 absorption could be employed.
While a saturation effect can be observed, we also see that the interferences of aerosol extinction and ozone are still not influencing the FPI device. The saturation of the device is simply a question of adaption of the device to the specific volcano.

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.

The absorption cross section of ozone changes between the wavelength ranges of filter A (310nm) and B (330nm) by approximately $1x10^{-19}cm^2$. A 100 DU change is equivalent to a change in ozone column density of $2.7x10^{18}$ molec/cm^2.
The SO2 absorption cross section in the same wavelength range changes by approximately $2.5x10^{-19}cm^2$. This means that a 100DU change in ozone background would cause a higher instrument response (AA) than a SO2 column of $1x10^{18}$ molec/cm^2.
100 DU were chosen in order to demonstrate and compare the impact on the two instrument types.
In SO2 camera measurements the clear sky is often used as a correction for changes in the illumination of the sky (see e.g. Equation 15 – 20 in Lübcke et al., 2013). When not applying these correction factors we can observe variations of the magnitude mentioned above in SO2 camera measurements. While correction factors largely diminish the effects of a changing solar zenith angle, the FPI device is clearly advantageous in situations where no area of clear sky can be identified or the ozone background changes inhomogeneously across the field of view.

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 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.

We thank the referee for this valid and valuable advice. While the FPI method is advantageous with regard to some interferences that negatively influence the SO2 camera evaluation, it is nevertheless influenced by radiative transfer issues in a similar manner as most other remote-sensing techniques in the UV (this includes the COSPEC, the SO2 camera and DOAS measurements). However, combining an FPI device with DOAS measurements (which allows to assess radiative transfer issues, see e.g., Kern et al. 2010 and 2012) might help with these problems.

The following sentences were added at the end of section 3.1:

“In this simple calculation we only considered aerosol extinction. This approximation holds for low plume AOD. Radiative transfer effects like light dilution and multiple scattering in the plume (e.g., Kern et al., 2010a; Millan, 1980) will still affect the FPI method in a similar manner as almost all passive UV absorption measurements. The FPI approach only removes errors of the traditional SO2 camera introduced by measuring at different wavelength ranges because both FPI signals (A and B) are obtained at nearly the same wavelength range. Radiative transfer calculations remain necessary to fully assess and possibly correct other error sources.”

Technical/Minor corrections

A definition should be given for the etendue

We added the following sentence as a footnote to section 4.1:

“The etendue of an instrument is a measure for its maximum possible light throughput and is determined as the product of the limiting beam solid angle and receiving area.”

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)

We thank the reviewer for this suggestion. Oppenheimer et al. (1998) is indeed inappropriate at this point. We added the reference to Mc Gonigle et al. (2004) and also the recent publication by Beirle et al., 2014 who found the volcanic SO2 lifetime to be on the order of 1-2 days.

p,5126, ln 29 change singal for signal

We corrected this typo.

p.5128 ln 2, change pixle for pixel

We corrected this typo.

p.5128 ln 15, change shiftet for shifted

We corrected this typo.

p.5128 ln 14, add a coma after moreover

We added the comma.

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

We added the comma.

ln 265-275, specify which value of single scattering albedo was assigned to the aerosol

We used in our simulation an Angstrom exponent which was measured by Spinetti and Buongiorno (2007) and characterizes a typical wavelength dependency of volcanic aerosol extinction. No statement about a distinction of aerosol scattering and absorption is made in this application of the Angstrom exponent.

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.

Figure 3 (a) shows the optical density seen through a FPI set-up for varying optical distance between the two reflecting surfaces. In Figure 1, we plotted the FPI transmission as stated by the Airy Function (Eq. 9). We used this ideal model (no absorption, light incidence parallel to optical axis) for the first theoretical study, where the transmission is indeed 1 for constructive interference.

Even though we changed the label to 'relative transmission', since the band pass filter's absolute maximum transmission is not 1 in our model.