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Atmospheric Measurement Techniques Discussions
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Interactive comment on "Quantitative imaging of volcanic SO_2 plumes with Fabry Pérot Interferometer Correlation Spectroscopy" *by* Christopher Fuchs et al.

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1 General evaluation:

The manuscript describes a major development step for this very elegant atmospheric measurement technique already initiated several years ago. The idea of matching the transmission comb of a Fabry-Pérot interferometer (FPI) with the regular structures present in the absorption spectrum of the target atmospheric species finds a very convincing application here with the remote sensing of volcanic SO2. The instrument concept, and the field campaign are well described, and my feeling is that the overall quality (both in terms of content and language) is already quite high with this initial submission. Though, I have a few points of concerns which I would like to raise. They are discussed below.

1.1 The abstract.

Although the abstract is a good summary of the manuscript (high level description of the instrument concept, and the experimental results achieved), I think it is slightly exaggerating the demonstrated capabilities of the instrument. For instance, it is claimed that the instrument does the job for SO2, BrO, and NO2, whereas only the first species is addressed. I understand that the prototype was designed to correlate with the SO2 structures, but therefore, at least a theoretical simulation of performance for the other species should have been presented. In absence of this, the BrO and NO2 capabilities should only be referred to as potential future applications. The same goes for the statement that the instrument allows to determine gas fluxes, while this aspect is also not discussed in the paper. The factual performance of the prototype is also a bit misleading: the claimed integration time of 1s is, as far as I could understand, the integration time of a single image, not yet the temporal resolution of the geophysical product (presumably closer to 5 seconds) as it seems currently suggested. Hence, I would recommend to rework a bit the abstract such that undemonstrated, though potentially achievable goals are not presented as conlusions of the work.

1.2 The instrumental model.

The mathematics describing the measurements have been carefully developed, and the reader will appreciate the author's will to integrate all the meaningful aspects of the model (in particular the splitting of the instrument transfer function into different multiplicative terms). However, I have two remarks regarding this section:

1. Less experienced readers might be lost in this section because it lacks a drawing representing the light paths involved. Supporting the mathematical description with a figure showing that eq.(2) refers to the light path originating from the Sun and going up to the point of scattering into the instrument line of sight would already be helpful. Having two rays illustrating the difference between I_i, and $I_{0,i}$ would also be appreciated.

2. Recalling the reader about the fundamental FPI equation is valuable. However, eq.(7) appears to be a step too far, especially that the weighting function term N remains mysterious at the end. I believe that the discussion on the effective transmission spectrum of the FPI is an important point. But because the reader will anyway not be able to reproduce your model (because of the undetermined term N), it is better to illustrate the effect of increasing the acceptance angle (or the tilt angle) on the FPI transmission with the help of a figure (a bit like fig.(1), but emphasizing the change of T_{FPI} as a function of these angles). Also, I found it not so clearly explained that the way the comb of the FPI is shifted (to go from setting A to B and back) is by rotating the FPI axis. A few words about the different means of performing this shift with nowadays FPI technologies (e.g. MEMS, piezo), and the trade off which led to the selection of the tilting approach would be appreciated.

1.3 Minor comments.

- p.2,l.26: The NO2 camera, presented in Dekemper et al. 2016, has a spectral resolution of 0.6nm at 440nm... The statement that native spectral imagers have a "strongly reduced spectral resolution" is therefore not correct. It is not because the classical filter-based SO2 cameras have a poor spectral resolution that all other spectral imagers have the same drawback, especially when the filter technology is completely different.

- I was wondering if the tilting of the FPI in order to go from setting A to B was introducing a shift of the respective images onto the detector? Is there a re-alignment step needed in the pre-processing of the data? If yes, then this is worth a couple of sentences addressing this aspect.
- Section 3.2: Your forward model uses a geometric air mass factor to estimate the SCD of O3. The model was validated for a relatively small SZA with the two gas cells. However, your field measurements were performed with a much larger SZA of almost 80°. Don't you expect a bias coming from the geometric AMF in that circumstances?
- p.10,l.203: How did you estimate the background SO2? Your method relies on using the background signal in order to determine the CD in the plume. Which I0 did you use for the determination of the background SO2?

1.4 Typos.

- p.4,l.80: stratosperhic -> stratospheric
- p.7,l.145: describe -> described
- p.7,l.151: add a comma after "model"
- p.7,l.154: add a comma after "quality"
- p.7,l.157: including -> include
- p.8,l.173: add a comma after the first "model"
- On several occasions, the form "I. e." is used at the beginning of a sentence (like on p.8, line 177). I don't understand this abbreviation.

- p.8,l.179: remove the comma after "Note"
- p.10,l.199: start a new paragraph with "An evaluated ..."
- p.11,l.229: "increases selectivity" -> "increases the selectivity"

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