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Atmospheric Measurement Techniques Discussions

Interactive comment on "Towards imaging of atmospheric trace gases using Fabry Perot Interferometer Correlation Spectroscopy in the UV and visible spectral range" *by* Jonas Kuhn et al.

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Dear Editor,

You gave me the opportunity to review the manuscript by Mr Jonas Kuhn et al. about the progresses made in the development of a new type of instrument dedicated to the remote sensing of UV-VIS absorbing species. My general appreciation is that this work constitutes a significant milestone towards the realization of new instruments capable of following the emission and the dispersion of important trace species (such as SO_2, BrO, and NO_2) in the atmosphere with a high spatial and temporal resolution. Compared to a previous concept paper by the same author (Kuhn et al, Atmos. Meas. Tech.

7, 2014), the new results described here provide some confidence about the sensitivity of the future instrument thanks to realistic simulations, and field experiments with a non-imaging prototype.

I only have a small number of comments and questions which will, would they be addressed by the author, improve in my view the quality of the manuscript further. In addition I listed some typos.

Specific comments/questions:

- It is true that the concept of aligning the transmission comb of the FPI with the periodic structures of the absorption cross-section of species has been described by the author in an earlier paper. However, I would have liked to see a bit more description here as well. For instance, the radiometric model of the measurements described in section 2.2 should make clearer that the measured signals I_A or I_B are made out of photons captured by each transmission peak of the FPI at the same time. Equation (5) could emphasize this by expressing I as the result of this summation process. Alternatively, one could bring equation (6) into section 2.2.

- Figures 2, 3, and 4 show a possible setting of the FPI targetting a different species respectively. One can imagine that the performance of the measurement method is strongly determined by the capability of finding the best "position" of the FPI comb in order to maximize the correlation. I found that this could have been pointed out and discussed in more details, by showing for instance how evolves this correlation as a function of a spectral displacement of the comb. For instance, by looking at figure 4 (NO2), one observes that one of the FPI peaks of the off-channel actually captures a relatively high absorption. This sounds like a sub-optimal configuration whose impact could have been dealt with.

- The determination of the CD eventually depends on the knowledge of the effec-

tive differential cross-section as shown in eq. (5). However, the way to determine this quantity is not discussed. With the actual instrument, will there be sufficient knowledge of the FPI transmission curve and of the BPF to compute it from a high resolution cross-section dataset? Or will it be necessary to measure it in the lab?

- The field experiment shows encouraging results, although it does not go up to comparing SCDs retrieved by the FPI-based instrument with the ones obtained from the classical grating-based instrument. As a reader, one immediately wonders why it is like that. Is it because there is no a priori knowledge of the effective differential cross-section (see point before), or because no clear sky measurement could be made at the time of the measurements?
- In the discussion of the results of the field experiment, there is time spent on the interference by O3. However, in absence of any attempt to retrieve the SO2 CD from the FPI-based measurements, it is difficult to adhere to the conclusions of the author about a relative insensitivity to O3. On the other side, if this sensitivity is more robustly confirmed, this aspect is an important asset compared to the widespread SO2 cameras, and emphasized further.
- In section 3, there are some inconsistancies between the text and Table 1 which it is referring to. Furthermore, the paragraph is not making it completely clear that the selected CDs are actually detection limits. In particular, for NO2, a value of 1e16 is clearly not the maximum that can be observed above a smokestack, hence it must be a detection limit... This is however better stated in the caption of Table 1.

Technical corrections:

- p.2,L.21: repeated "by column"

- p.2,L.22: replace "rather high" by "rather large" or "rather long"
- p.2,L.27: add a comma after "imaging tachniques"
- p.3,L.5: replace "Due to" by "Thanks to"
- p.3,L.11: add commas before and after "for the three gases"
- p.3,L.12: replace "by around" by "about"
- p.4,L.1: remove "a stable"
- p.5,L.28: "calculated" is misspelled
- p.7,L.15: remove the comma after "this illustrates"
- p.10,L.6-7: the end of the sentence is very clear.

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