Authors' response to the comments of Referee #3 on “In-operation Field of view Retrieval (IFR) for satellite and ground-based DOAS-type instruments applying coincident high-resolution imager data” by H. Sihler *et al.*

We would like to thank Referee #3 for the review of our submission to AMTD and for contributing helpful comments and suggestions to improve the quality and clarity of our manuscript.

For reference, the original Referee comments below are typeset in black, our responses in blue. Modifications of the original manuscript (green) are indicated in red.

This paper presents a method to in-operation retrieve the Filed of View (FOVs) of an instrument based on the correlation of a high resolution instrument. The authors applied their method on GOME2 and AVHHR, OMI and OMI and MAX-DOAS and a SO2 camera.

This paper is well-written and interesting. These results will be helpful for the remote sensing retrieval and validation communities. I recommend to publish after the authors have some minor revisions.

General comments.

1, OMI pixels influenced by the row anomaly were included in the FOV retrievals. I was wondering whether and how the row anomaly influences the FOV retrievals.

We were wondering that, too, and hoped to contribute more quantitatively to the specification of the row anomaly. Strangely, there seems to be no apparent difference between IFR results of regular OMI pixels and those affected by the row anomaly as noted in p.7, l.3 and shown in the Supplement. Possible explanations for this finding are:

Firstly, there are different types of row anomalies which appeared and disappeared during phases. Not all types of the row anomaly could be covered here, and, were avoided since no clear signal was found using data potentially affected by different kinds of row anomalies.

Secondly, the applied linear model for the characterisation of the FOV is not sufficient to represent anything more distorting than a constant offset between LR and HR measurements. So shielding of the entrance slit alone would lead to different IFR results c (the plots in the manuscript show normalized results), but higher order effects like light dilution due to scattered solar light depending on orbital parameters are not covered.

It may, however, be possible to characterise the row anomaly more analytically using a more complex model function for OMI and run the analysis again, but this was not within the scope of this paper. This outlook is added to the discussion of OMI results after p.27, l.30:

In order to investigate the effect of row anomalies in the future, second order effects may be added to the linear IFR model presented in this paper.

2, I agree with Reviewer 1. There is no error analysis for the regularized solution used for OMI and the SO2 camera. The error analysis for the regularized problem should be included in the conclusion.

Actually, it is not possible to perform such an error analysis for the regularised problem.

See also answers to RC1 for applied changes of the manuscript.

Specific comments:

P2, L12-15. It would be better to include de Graff's paper in this introduction.

In the first paragraph of the introduction (at the specified location), we state how pixel boundaries are most frequently treated in practice, i.e. while gridding the data for visualisation purposes (p.2 l. 32). It is true that de Graaf et al. in fact applies a more complex model, but not for the purpose of visualising satellite data in the first place. We therefore believe that it is adequate to cite the mentioned work by de Graaf et al. at p.3 l.4. Furthermore, the revised manuscript now contains more details from the de Graaf paper as described in our answers to RC1.

P3,L30. Please move this paragraph to Introduction section.

At the specified location, the contents within the subsections of the second section "Method" are listed, whereas the structure of the entire paper is presented in the last paragraph of the introduction. We therefore still think it is more clear to leave structure as is.

P12, L10. Again, what if the LR radiance is influenced by the row anomaly?

The presented method description is based on an idealised model for the sake of simplicity. The row anomaly would require further terms, which are neglected here.

We add at the end of p.12, l.9:

assuming an idealised linear response for both instruments. Second order effects necessary to model the effects of the row anomaly on OMI measurements are neglected.

P19,L5. Would 20m/s wind-speed threshold work?

Yes and no. We conducted experiments using different thresholds t_w from 5m/s to 30 m/s and found a reasonable trade-off between image noise (decreasing with t_w) and FOV dilution due to cloud movement (increasing with t_w). Hence, increasing v_th to 20m/s would increase the width of the fitted FOV parameterisation due to increased scene changes between overpasses.

Changes to the Method sections (p.8, l.11)

Scene measurements in which the interpolated maximum wind-speed between 1 and 3 km altitude exceeds the wind-speed threshold t_w=15 m/s were filtered.

Measurements of scenes in which the interpolated maximum wind-speed between 1 and 3 km altitude exceeds a certain wind-speed threshold were discarded in order to decrease the effect of cloud movement between overpasses.

Changes to the Results section (p.19, l.4):

Therefore, the initial set of 10^5 combined measurements was filtered using a wind-speed threshold t_w=15 m/s which proved to increase SNR of the FOV retrieval significantly. The application of a more strict threshold, e. g. 10 m/s, however, led to an SNR reduction due to the reduced statistics (cf. Fig. A2).

Therefore, the initial set of 10^5 combined measurements was filtered using a wind-speed threshold t_w=15 m/s which proved to increase SNR of the FOV retrieval significantly. The choice of t_w was determined in preceding tests and presents a trade-off between discarding too many measurements, which would decrease SNR, and not filtering enough measurement necessary to reduce the smearing effect of scene changes due to cloud movement. The results can be considered robust as they only weakly depend on t_w.

See also answers to RC2.

Technical comments:

P15, Figure 7. Please use the same color bar.

Done. The colorscales in a and b are now the same. The 1D-graphs now have scales and units. See also answers to RC2.

P22, Figure 16. Please use the same color bar.

Done. The colorscales in a and b are now the same. The 1D-graphs now have scales and units. See also answers to RC2.

Reference:
de Graaf, M., Sihler, H., Tilstra, L. G., and Stammes, P.: How big is an OMI pixel?, Atmos. Meas. Tech., 9, 3607-3618, doi:10.5194/amt-9-3607-2016, 2016.

The reference is now updated from AMTD to AMT in the new manuscript.