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Interactive comment on "New method to determine the instrument spectral response function, applied to TROPOMI-SWIR" *by* Richard M. van Hees et al.

Anonymous Referee #2

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############# General comments #############

The paper "New method to determine the instrument spectral response function, applied to TROPOMI-SWIR" by R. M. van Hees et al. addresses the determination of the Sentinel-5p / TROPOMI instrument spectral response function (ISRF) in the SWIR spectral region. The authors claim that the accuracy of the derived ISRF is well within the requirements for accurate trace-gas retrievals, which is stated to be known with an accuracy of 1% of its maximum where the ISRF is greater than 1% of its minimum.

The paper addresses an important topic, as accurate knowledge of the ISRF shape and FWHM is essential to avoid systematic errors in trace gas retrievals, especially for

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Discussion paper

missions with stringent requirements on spall systematic errors, e.g. greenhouse gas missions such as OCO-2/3 MicroCarb, GeoCarb, and the upcoming future European CO2 monitoring missions. The paper describes an iterative approach to accurately retrieve the ISRF shape from a series of measurements performed with an optical parametric oscillator (OPO) during the TROPOMI on ground spectral calibration measurements at CSL. The topic is in general of high interest to be published in AMT. However, my impression is, that this paper resembles in large parts a technical document or an ATBD describing the applied mathematical algorithm without explanation or deeper analysis of the applied steps. I agree in this regard with anonymous Referee #1 that this Manuscript should only be published in AMT after substantial revision. As a very comprehensive review is already given by Referee #1 addressing most of the issues I found in this paper, I will only briefly address some additional issues and my major points of concern.

To avoid further confusion, I will use in the following review the terms ISRF and ISSF as defined in this manuscript.

- The authors claims to introduce a new method for ISRF characterization without giving any evidence for the case. To underline the issue, the authors should perform a more comprehensive literature review on the topic and should cite for instance previous literature like K. Sun et al. 2017, R.A.M. Lee et al. 2017, J.O. Day et al. 2011, Beirle et al. 2017, Liu et al. 2015, Dirksen et al. 2006 and others.

- The authors are representing an iterative approach to derive the high resolution ISRF from a series of ISSF measurements, claiming that the high resolution ISRF could not be measured directly.

However this is only true, if the spectral accuracy, linewidth and “intensity” of the used optical stimulus (in this case the OPO) is insufficiently known. I have the impression, that this the case for the used OPO setup as the authors stated in two cases: “During the on-ground calibration measurements, the absolute wavelength of the source is not measured accurately enough" and "The ISRF parameters cannot be retrieved directly from the measurements, because the wavelength and intensity of the signal are unknown and have to be determined via the ISSF". If this is the case, the authors should clearly state in the introduction section of the manuscript, that the iterative approach presented in this paper is required due the insufficient accuracy of the used spectral calibration equipment and is therefore the novelty of the described procedure. However a carefully design of the calibration stimulus should be able to overcome this problem and should allow a direct high resolution measurement of the ISRF for each detector pixel. Nevertheless, construction of the ISSF from such measurements could be tricky, as in addition, detector issues, as for instance differences in pixel to pixel crosstalk, PRNU etc. needed to be considered.

- The authors fail to justify, why for instance the Pearson VII distribution is used or why the given iterative approach is chosen. There is no comparison with other possible distributions, see for instance Beirle et al. 2017. Also the use of filter parameters is not sufficiently justified. For instance rms filtering of ISRF fits with an rms larger than 0.0065 is applied. Why not 0.005 or 0.008?

- The authors claim in the abstract and the conclusion that "The accuracy of the derived ISRF is well within the requirement for accurate trace-gas retrievals". However, the method described in this paper presents only a fit procedure able to fit the measurements with a given accuracy. The authors lack to provide a comprehensive error budget, including effects on PRNU, detector non linearity and other mostly detector related effects to underline that claim. The paper also lacks to provide an independent verification for that claim, see for instance Frankenberg et al. 2015 (doi:10.5194/amt-8-301-2015) for comparison.

- The use of the terms ISRF and ISSF as defined by the authors is confusing and not consisted with the paper of Hasekamp et al. 2016. cited for justification of the ISRF knowledge requirements. Typically the (derived) ISRF function is used to convolute a theoretical high resolution RTM spectrum to lower spectral resolution in the retrieval. The (optical) ISRF is typically defined as the spectrometer response to a uniform monochromatic stimulus and approximated as the convolution of the slit image (typically represented by a boxcar function) with the PSF (or more accurately, the LSF) if the detector properties are neglected, see for comparison also Caron et al. 2017. This definition of the ISRF function is the mirror function of the ISRF function as defined in this paper, when assuming the changes of the (optical) ISRF are smooth over the image plane.

- Section 5 needs a deeper analysis to justify statements given in this section. For instance statements as: "However, it is believed the method is sensitive enough to be used on board for long-term monitoring, being able to distinguish between changes in the real instrument ISRF and changes in the speckle pattern" needs to be justified by analysis or removed.

############# Specific comments #############

Section 2, P.3 L.1-6: (calibration measurements): Is a Wavemeter and a (spectral response and linearity) calibrated monitoring detector used in the setup ? Is there any other type of direct laser wavelength monitoring integrated in the setup. If yes, what is the accuracy? What is the laser linewidth?

P.2 L.23-24: "using the wavelength assignment derived from an independent wavelength calibration measurement." - State how accurate the independent wavelength measurements are, as this has impact on the accuracy of the derived ISRF shape and how is it done? Is a different setup used?

P.3 L.25: convolution of block distribution should be exchanged by convolution with a boxcar function in the entire manuscript.

P.3 L.26: The optics is "blurring" the image by the spectrometer PSF (LSF) which could be asymmetric and often has also oscillations in the wings. Using a normal distribution for approximation of the PSF/LSF is only a first order approximation.

P.4 L.21: The measurement data is corrected for background, PRNU and straylight. Why is the data not corrected for detector non linearity as the used MCT detector can have nonlinearity in the % range over the dynamic range ?

P.5 L.2-3: Wavelength and intensity of the signal are unknown: are they really completely unknown? What is the wavelength accuracy/knowledge and stability of the used OPO ?

P.5 L.11-12: The laser wavelength scan is not regular -> why ?

P.5 L.17: larger than 6 % - > justify 6%, why not 5% or 7% ?

P.5 L.21-23: "It is expected that the fit parameters that define the local ISRF vary only smoothly over the surface of the detector as this is determined by the spectrometer optics. Therefore, a bivariate polynomial fitting is used to smooth and to interpolate the ISRF fit parameters." – This can only be expected for the optical system in case additional detector effects are neglected. However in a real life scenario, the effective ISRF is additionally compromised by insufficiently corrected detector effects. I guess, if all detector effects could be corrected to the required level, the resulting derived ISRF would be a smooth function. Therefore this statement contradicts the claim by the authors that the accuracy of the derived ISRF is well within the requirement for accurate trace-gas retrievals. Also the statement that most of the outliers are at same wavelengths and are caused by laser artefacts / scan imperfections contradicts that claim, as laser artefacts and scan imperfections need to be considered in the total error budget of the ISRF. See also discussion on P9. L.10-13. For a better understanding it would be helpful to show ISRF cross sections measurements from compromised rows, where the fit procedure fails.

P.9 L.3-7: A sketch of the radiance and irradiance calibration setup would be very helpful. Is the on-board diffuser illuminated via the integrating sphere during irradiance measurements or directly by the OPO ? I would expect from the text and as a ND filter is used in front of the OPO (P3,L10), that the OPO is directly illuminating the on-board diffuser. This would result in a better SNR as more light is entering the instrument but also in more spectral structures introduced by the laser + on board diffuser combination. The authors state that the integration sphere in combination with the spinning mirror is used to avoid speckles. So please justify, why the irradiance measurement which obviously should have more spectral structures is used for the key-data. This furthermore triggers the discussion what is physically more meaningful, a smaller spread in the data as observed in figure 8 and also stated on P.9 L9-10 for the radiance measurements or a better RMS of the fit as observed for the irradiance measurements, which can for instance be caused by a larger number of fit parameters defining the degree of freedom of the fit.

P.9 L.14: How is it judged that the observed difference is attributed to the non-optimal scanning of the laser? What is an optimal scan? In fact, the wavelength accuracy and intensity of the laser are imperfectly known as previously stated.

P.9 L.15: Is a difference of the block width between radiance and irradiance measurements of $\sim$ 5 % as shown in Fig 8 c for the left side of the detector really negligible in comparison to the requirements ?

P.9 L.29: Would taking the median over an entire row not imply the assumption, that there could not be a relative ISRF change along the row ?

P.10 L.1: The statement is contradicting to the previous statement. In fact, if the laser can be used to recalibrate the ISRF for a significant part of the detector, they can be used for trace gas retrievals.

P. 13 Figure 1 (a): it should be added to the axis caption that “column” is the detector spectral direction and wavelength is the wavelength derived for each ISSF measurement by the fitting procedure.

P.17 Figure 5: The figure caption should be clearer. For instance “white areas on top and bottom of the detector blocked by the slit for stray light correction and DC monitoring” or something similar.

P.22 Table 3: How meaningful is the fit of the skew parameter s with > 100 % error ?

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