Interactive comment on “Mapping spectroscopic uncertainties into prospective methane retrieval errors from Sentinel-5 and its precursor” by R. Checa-Garcia et al.

Anonymous Referee #2
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The study of Checa-Garcia et al., which estimates the propagation of spectroscopic errors on the XCH4 retrieval errors obtained from S5 and S5P-like spectra, is very relevant for AMT. The paper is very well structured and clear. Therefore, I recommend the publication of this paper in AMT, after a few remarks have been addressed.

1) If the goal of S5 and S5P is a total accuracy of better than 2% (or 30 ppb) for XCH4, then I would say that the -20 / + 20 ppb XCH4 errors obtained for SW3, even if not perfect, is good enough to achieve the goal, considering that the other main systematic error source has been estimated to less than 1% (Butz et al., 2012). Can the authors explain why they conclude differently? Are there other expected error sources which, added to the spectroscopic ones, would make the 2% aimed accuracy not possible?

2) In agreement with the referee C. Frankenberg, I suggest to investigate the role of the H2O profile on the residuals. Only a scaling factor is applied to the H2O a priori profile used in the retrievals. The H2O a priori profile is taken from a model ECHAM5-HAM, but it is not said if this a priori profile is calculated daily or hourly or interpolated to the time of measurement? Do it improve the fit to, either make a preliminary retrieval of the H2O profile and use it as a priori in the CH4 retrieval, either make a simultaneous profile fit of H2O? Even if only a scaling of H2O will also be used in the “real” S5/S5P retrievals, this test on ground-based data will help to determine if only the spectroscopy is responsible for the bad residuals.

3) The conclusions are different than the study of Galli et al. (2012). This study used 6-hourly H2O profiles from ECMWF as a priori. Could this be one of the reasons for the weakest dependence of CH4 retrievals on H2O found in this study (which also only scales the H2O a priori profile)? What else could explain the different conclusions?

4) The regularization for the CH4 profile retrievals is chosen to obtain 1 Degree-of-freedom (p.1339). Is it the expected maximum DOFS that can be obtained (i.e. no profile information can be derived; a “real” a priori covariance matrix would also lead to 1 DOF)? If not, can a less strict constraint improve the residuals and the XCH4 errors?

5) Fig.8: can the authors provide some explanation for the different correlations observed at different humidity regimes, which is very pronounced in the case of SW3 (especially, for the decreasing interference for very humid cases)? Is this behavior true at a regional / local scale (to remove the influence of very specific surface properties, such as the albedo, e.g Amazonia or the height, e.g Himalaya)?

Minor or technical comments:
- p.1335, l.22-26: You could add the information that the 20% overestimation of the tropical CH4 source was due to an overestimation of CH4 in the tropics up to 60 ppb, just to give to the reader the order of magnitude of the spectroscopic error found in this previous study compared to the present one.

-p.1341, l.18: remove “approximately”.

- p.1344 (Sect.4): I would prefer a more quantitative assessment of the correlation: e.g. it would be good to provide the correlation coefficients between the XCH4 errors and the AMF for each window configuration, and a similar figure than Fig. 8 for AMF. And for Fig. 8, the correlation coefficients could be provided.

p.1345, l.3: “mid-latitudes”

p.1355: Explain alpha_gb in the legend of the Table.

P1356-1357: Figs 1 and 2 could be plotted in the same figure (a and b) in AMT. I would use the same scale for the residuals in dry and humid cases.

p. 1363: Fig. 8: Enhance the font size, please.