

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

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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:

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- 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 α_{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.

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