

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

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The paper by Checa-Garcia et al deals with a very important topic, namely the impact of spectroscopic uncertainties in H₂O for global space-borne methane retrievals in the short-wave infrared. Given the upcoming TROPOMI launch, this manuscript is timely, relevant and well suited for AMT. I do have some general remarks that should be considered before publication in AMT though, as outlined below and followed by a short list of small issues:

Major remarks: How was the FTS fit performed? I can see that the CH₄ profile was

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fitted but was the same done for H₂O? Given the wide range of lower state energies for H₂O transitions, its T-dependence can be very large and a total column scaling fit may produce overly pessimistic spectral residuals (i.e. flaw 2). Please explain how H₂O was fitted, both in the FTS but also in the satellite case, it may make a big difference as the profile assumption is a hard constrained, forcing the fit to push deviations from the profile into other state vector variables, presumably CH₄.

The other question to me is the following: How can we potentially mitigate this problem? Yes, more spectroscopic studies are needed but experience shows that these are time-consuming and most likely won't have resolved the problem before the TROPOMI launch. Are there are partial solutions that could reduce this problem? What if you exclude the regions with the highest deltaT values in the spectral fit (or de-weight these regions)? How would results look like and can this be done without harming the CH₄ retrieval itself? Right now, I have the impression that more could be done to mitigate the problem at hand and it would strengthen this paper by including such an additional analysis.

Also: You propagate a delta-T from 2 FTS stations to the globe, only taking the transmission into account. As I mentioned earlier, the H₂O line intensities are very temperature dependent, hence transmission errors at similar H₂O columns can actually vary substantially. The more “freedom” you provide the spectral fit to fit these features (e.g. fitting a H₂O profile without stringent shape constraints), the less the cross-talk into CH₄ will be. Ideally, I would like to see this analysis be repeated with a full H₂O profile fit, both for the FTS but also the satellite retrievals. All atmospheric layers for the profile fit could be fitted independently, which can introduce jack-knifing but may help minimize the H₂O impact.

Figure 1: It covers a wider wavelength range than the final fits using surrogate satellite data. How do spectroscopic errors look like if you shorten the window? The residuals appear rather large, » than 1%. These seem substantially larger than results in Frankenberg et al, where Jenouvrier et al H₂O spectroscopy was used. This needs

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to be double-checked. Also, as shown in Frankenberg et al, the Q-branch was most sensitive; it would be very worthwhile to check the sensitivity after exclusion of the Q-branch in the spectral fit.

Review, some language checks needed. Page 1336, line 19: “strives” → describes Page 1336, line 26: “chained” into the state. Is this a new “technical term” denoting a state vector element with really stringent prior constraints? Page 1337, line 19: does not dispose → does not include the SWIR. . . Page 1338, lines 5ff: The parameters are not extrapolated, the shape of the far wing is not well constrained so that the theoretical line-shape is not entirely certain. What does “only a small part of the lines was . . .” mean in that context here? Not clear, part of a line (near the center) or not all transitions of a band? Please be more accurate.

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