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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Strategy for high-accuracy-and-precision retrieval of atmospheric methane from the mid-infrared FTIR network" *by* R. Sussmann et al.

**C. Frankenberg (Referee)**

christian.frankenberg@jpl.nasa.gov

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The manuscript "Strategy for high-accuracy-and-precision retrieval of atmospheric methane from the mid-infrared FTIR network" by Suessmann et al. suggest an accurate and precise total column retrieval using mid-infrared spectra.

The paper is in general well written and shows that the current retrieval scheme can be improved using the updated method. Frank Hase posted a very valid and detailed comment and his concerns should be taken into consideration and discussed in a revised version. However, this paper should be treated as a purely scientific publication



and by no means as a strict recommendation for the entire NDACC network. The decision whether or not this scheme is to be applied as operational NDACC scheme should be treated independently from this manuscript (the manuscript in conjunction with Frank Hase's remark, for instance, would serve well as a guideline).

I do have some minor remarks and 2 more major ones, which I ask to take into account in a revised version. Minor:

- What is the actual motivation to use narrow microwindows? Wouldn't larger windows (as TCCON does) result in less interference errors if proper lines of the interference species are retrieved alongside?
- Interference error: It looks like H2O and HDO have real spectral features only in MW5. In the other windows, it looks more like a broad-band change in the continuum. I assume polynomial are fitted for the baseline and if yes, does the degree of the polynomial change the outcome? In these very small microwindows, it actually might.
- Page 2967, line 3: There are many SCIAMACHY scientific data version around, it would be more correct to just state "agrees well with the WFM-DOAS v2.0 (Schneising et al...) scientific XCH4 retrieval product"
- page 2967, line 21: Bousquet may not be the best reference for the loss term. Please look at more original literature for the OH-destruction (e.g. Lelieveld)
- Page 2980, TCCON: Please cite the TCCON overview paper by Wunch et al, 2011, Phil. Trans. R. Soc. A. The authors provide an overview of errors that are being reduced by using O2 and clouds appear to be a minor concern. Many instrument effectively cancel out when using O2 measurements in the same band (e.g. zero-level offset, pointing error).

- Page 2985, pressure: Do you use on-ground pressure sensors? Interpolating NCEP to a high altitude station might cause some errors (how large do you estimate them to be?).

Major:

- Regularization method: I am not entirely sure about the choice of the Tikhonov constraint $\alpha$. As far as I understood, the authors empirically change $\alpha$ in order to optimize the RMS of retrieved XCH4 values during the day. The optimized $\alpha$ has roughly a DOF of 2. In figure 4, we see that there is a seasonality of DOF, varying from 1.6 to 2.2. Is $\alpha$ fixed once and for all or does it depend on station and time of year?
- $\alpha$: The author claim (page 2978, line 9) that the new scheme better integrates the measured absorption-line profile. In the current version, this claim is unsubstantiated by evidence. To prove this, a plot of $\alpha$ vs. reduced $^2$ of the fit would tell us whether or not the true fit quality really improves by fitting a profile. This curve (in principle, an L-curve), would also enable a more obective choice of the regularization parameter $\alpha$ as it would enable you to find the optimal choice (weak enough in order to improve DOF but only as weak as it reduces true spectral residuals). The authors might want to consider a Tikhonov L-curve scheme (widely published), especially because the choice of $\alpha$ may depend on FTS SNR, spectral resolution and so forth. This scheme may also be more universal to all different sites. It may be that you will find the same answer for $\alpha$ but the L-curve method is a proven and robust technique.
- Quality selection: RMS is typically a bad indicator of the goodness of the fit. Why don't the authors use a reduced $^2$ measure? This should eliminate some of the seasonal variability. My question is also: As you see a seasonal cycle in RMS, you probably also see a diurnal cycle (if airmass is the trigger). What



SZA's do you consider each day? Is SZA dominating your SNR variations (causing the RMS variability) or is it that systematic residuals are more apparent at higher airmass (also causing the RMS variability but for a very different reason). If you have a good noise estimate for each and every spectrum (from out-of-band RMS?), you can easily calculate the reduced $^2$ instead of relying on the division by DOF, which may seem innovative but is lacking theoretical foundation

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