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Interactive comment on "Retrieval techniques for airborne imaging of methane concentrations using high spatial and moderate spectral resolution: application to AVIRIS" *by* A. K. Thorpe et al.

Anonymous Referee #2

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Review of the paper entitled "Retrieval techniques for airborne imaging of methane concentrations using high spatial and moderate spectral resolution: Application to AVIRIS" from Thorpe et al.

This paper concerns the quantitative retrieval of CH4 concentrations in natural and industrial plumes from hyperspectral AVIRIS data.

It addresses an important topic related to air quality, pollution and climate change applications. AVIRIS-like spectro-imaging instruments (with high spatial resolution and moderate spectral resolution) may provide information on anthropogenic emissions on

a local/regional scale and therefore complement data from atmospheric coarse resolution sounders.

The main originality of this paper is from my point of view the use (and the improvement through the SVD) of the IMAP-DOAS method originally developed for very high spectral resolution instruments (sounders).

However, I have several concerns that should be addressed by the authors before the publication of the paper by AMT.

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1) Scientific objective addressed by the paper

What are the precisions in the estimation of CH4 concentrations sought by scientists working on climate change or air pollution? Is the precision obtained here with AVIRIS (or later with AVIRIS-NG) compatible to those needs? If this is not the case, can the authors provide some elements on the spectral resolution and noise levels they consider to be necessary to achieve this goal? Is it realistic to contribute to these studies with hyperspectral imagery?

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2) Two retrieval techniques are presented by the authors: IMAP-DOAS and IMAP-DOAS/SVD

2.1) IMAP-DOAS

* As noted by the authors at the end of part 3, this is the first time that this approach is used with AVIRIS-like data. Also, the corresponding community is not necessarily familiar with the DOAS approach. From my point of view, the authors should give more details about the method and highlight its main principles. As an example, Bovensmann et al., "From radiation fields to atmospheric concentrations - Retrieval of geophysical parameters", in "SCIAMACHY - Exploring the Changing Earth's Atmosphere,"

eds. M. Gottwald and H. Bovensmann, ISBN:978-90-481-9895-5, doi:10.1007/978-90-481-9896-2, Springer, pp. 99-127, 2011, give a nice description of the two main ideas of DOAS.

* First, at the end of part 2, justify the choice of IMAP-DOAS method while many other DOAS methods exist. Why is it more suited to the problem than others?

* Atmospheric scattering is neglected by the authors for the estimation of CH4 because it is performed in the SWIR spectral region. What about industrial plumes that can be composed of both CH4 and aerosols and for which the optical thickness values may lead to non negligible scattering in the SWIR?

* Page 6, lines 30-31: the gas/surface separation will be more difficult with AVIRIS data than with high spectral resolution data, justifying the hybrid SVD approach -> OK

* Taking into account the surface reflectance in the IMAP-DOAS method (parts 3 and 5).

- The authors should clearly state in part 3 that the signal contains a low-frequency component (surface, Rayleigh, Mie) and a high-frequency component (gas). As diffusion is neglected here, the low-frequency component corresponds to the surface reflectance and it is modeled by a polynomial. Indeed, we must expect page 6, line 29 so that the surface is raised while it is for me a crucial parameter in the CH4 estimation procedure.

- One also wonders if the polynomial coefficients are estimated or not. In section 5.2, the state vector does not contain the parameters of the surface, except for lines 1, 2 and 3 on page 10. What is it?

- Why the polynomial corresponding to the surface does not appear in Equation 4 of the direct model so it appears in equation 12 of Frankenberg 2005c? This affects the understanding of the method.

- As there is no diffusion, if we estimate the polynomial do we obtain the reflectance of

the surface? If yes, what is the utility of using the SVD? If not, if the surface is not well known, is it due to the spectral resolution of AVIRIS data?

* Page 9, line 12: "In principle, N2O could be neglected at this spectral resolution but we included it for the sake of completeness." Do the authors mean that it is impossible to estimate N2O with AVIRIS? If so, this is in contradiction with a paper of Thorpe in 2012 that detects N2O with a CTMF filter? Can the authors give some precisions about it?

* H2O appears to be also estimated by the algorithm (the values of the Jacobian is of the same order of magnitude as for CH4). However, the spectral range used here [2218-2457nm] is not well suited. Would it be interesting to include for example the spectral bands around 940nm in the inversion process?

* Overall, for the proposed method, is it preferable to provide the a priori knowledge for H2O in the form of exogenous profiles (which can therefore be quite different from atmospheric conditions during the image acquisition) or in the form of standard profiles scaled with estimated H2O columns in a pixel-by-pixel basis from the image itself (using for example the 940nm AVIRIS bands)?

* Conclusion on IMAP-DOAS. The IMAP-DOAS approach has been developed for high spectral resolution data to separate the high-frequency atmospheric absorption features from the low-frequency surface features. I find the approach quite complex, and, furthermore, it requires some a priori. Also, I am not fully convinced of its interest in hyperspectral imaging compared to conventional methods based on LUTs and smoothness criteria? Can the authors provide in the manuscript some elements of discussion on this point?

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2.2) IMAP-DOAS/SVD (part 7)

* In the same way as for the IMAP-DOAS approach, I find this part difficult to un-

derstand. In particular, the authors should better explain the model equation 9 (see equation 2 of Guanter et al. 2012) and its link with the model equation 4. They should also clearly indicate the parameters to estimate and the cost function to be minimized.

* Is the algorithm sensitive to the choice of the number of eigenvectors retained in Uselect? The authors describe how they determine this number -> OK (4 for the COP image and 9 for the Inglewood image). What is the impact on the estimation of CH4 if we retain an eigenvector more or less?

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3) Results and Discussion

* The contribution of the SVD in the improvement of the results is clearly shown -> OK

* It seems that the results above the vegetation are problematic even with the use of the SVD (see section 10.2 Inglewood, last paragraph). Why? The spectrum of vegetation does not exhibit sharp features in this wavelengths range, is it due to the low reflectance (SNR) only? Can the authors give some precisions about it?

* As indicated by the authors in section 11.1, lines 20 and 21, CTMF provides good results. Is there an interest in coupling CTMF with IMAP-DOAS/SVD? For example: - Estimate CH4 concentrations only for the pixels detected by CTMF - Use the other pixels of the image to improve the characterization of the soil and the background atmosphere - Other perspectives?

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