

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.

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Response to Anonymous Referee #2 (A revised version of the manuscript clearly indicating all changes is provided as a supplement).

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.

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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.

Response: The authors appreciate the thorough assessment of the manuscript and useful comments. A detailed response for each comment is listed below. Please refer to the revised version of the manuscript clearly indicating all changes, which is provided as a supplement.

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?

Response: This is a good point, and we thought it best to address this concern in three locations (i, ii, and iii below). (i) Section 8 first discussed the sensitivity study by referencing Figure 6 (see supplement, page 14). For an AVIRIS-like sensor (Fig. 6, black line), the expected error is 0.31 to 0.61 ppm CH₄ over the lowest atmospheric layer (up to 1.04 km) for a SNR between 100 and 200 (a conservative estimate based on low albedo surfaces). Towards the end of Section 8, we acknowledge that these errors are considerable (see supplement, page 15). (ii) In Section 11.2, we speak in greater detail about sensors like AVIRISng that should have errors approximately half

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that of AVIRIS. In this section the authors acknowledge that significant errors are still expected with sensors like AVIRISng (see supplement, page 21). (iii) In the Conclusion, we emphasize that imaging spectrometers like AVIRISng (high spatial resolution, higher expected errors) will be complementary to the higher precision, low spatial resolution sensors like MAMAP. We have added text to emphasize that there is great value in the ability of imaging spectrometers to map CH₄ plumes in high resolution, directly attribute emissions to individual point sources, and provide estimated concentrations despite the considerable errors (see supplement, page 23). This is particularly true given the large uncertainties in fugitive CH₄ emissions from the oil and gas industry (Petron et al., 2012; EPA, 2013; Allen et al., 2013) and projected increase in these types of emissions (EPA, 2006). In this section we state that further reducing errors in retrieved subcolumn concentrations is of concern, and would likely require developing an imaging spectrometer built exclusively for trace gas mapping and quantification. In addition, we provide the example of a potential imaging spectrometer with 0.05 nm spectral sampling interval and 0.15 nm FWHM would have an error around 18 times smaller than AVIRIS.

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.

Response: The authors agree that additional background information will be helpful to

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readers unfamiliar with DOAS and have used the suggested reference to supplement the text (see supplement, pages 4 and 5). This includes ground-based, satellite, and airborne examples as well as the main principle of DOAS. Finally, the authors indicate in which sections of the manuscript the polynomial function is discussed in greater detail.

* 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?

Response: For strongly absorbing trace gases measured at moderate spectral resolution, it was found that the classical DOAS approach is not suitable as the relationship between measured optical thickness and number concentration of trace gases become non-linear (Frankenberg et al., 2005). The 2300 nm range includes very strong and saturated (in the line core) absorption features of both CH₄ and H₂O, requiring the use of an iterative DOAS scheme such as IMAP-DOAS. This has been clarified in the revised version (see supplement, page 6). In addition, the authors agree that a more explicit statement for the choice of IMAP-DOAS should be included. The authors thought it best to add this statement in Section 3 because there is a more detailed description of the IMAP-DOAS and WFM-DOAS techniques in this location (see supplement, page 7).

* 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?

Response: Reviewer 1 also touched on this point. For the two examples presented in this study, a natural CH₄ seep and a CH₄ plume from hydrocarbon storage tanks, scattering due to aerosols would be low. However, we have added a few background references regarding scattering in the SWIR to strengthen this section (Buchwitz and Burrows, 2003; Dufour and Breon, 2003). Further, we have added an additional state-

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ment to acknowledge in some examples scattering could be considered non-negligible for industrial plumes that contain heavy aerosol loading or dark surfaces with low SZA (see supplement, page 5). The authors also point out that scattering is discussed in more detail in the second to the last paragraph of Section 3 (see supplement, page 7), "For the 2,300 nm range, where Rayleigh scattering can be ignored and aerosol optical depths are low, this assumption is valid given errors induced by neglected scattering in AVIRIS scene are typically much lower (0 to 2%) than precision errors in retrieved column estimates (>3%)."

* 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.

Response: This is related to a previous comment and a description of the importance of distinguishing between the high-frequency (gas) and low-frequency components has been added to the first paragraph of Section 3 (see supplement, page 5).

- 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?

Response: This is true and was unclear. For simplicity, we only included the trace gas related aspect of the equation and omitted the polynomial term in the original

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manuscript. In the revised document, the equation has been changed by adding the polynomial term outside of the exponential expression as a factor (similar to Eq. 12 in Frankenberg et al., 2005) (see supplement, page 10). An improved description of the polynomial coefficients has also been added to the last paragraph of Section 3 (see supplement, page 7) and in Section 5.2 (see supplement, page 10).

- 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.

Response: Please see the previous response.

- 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?

Response: The main reason for using the SVD approach is that we found surface feature interference errors using the IMAP-DOAS approach. This is most likely caused by the fact that relatively large spectral windows are used, where simple polynomial expressions might be unsuitable to account for some of the reflectance variability of the surface. The SVD approach basically creates orthogonal functions of the reflectances in the scene, avoiding this complication. Other methods (such as using Legendre polynomials for the surface) could be tried, but this is beyond the scope of the current study.

* 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?

Response: The reviewer is correct to point out that this statement appears to contradict

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results published in the referenced SPIE paper (Thorpe et al., 2012). For the work presented in this AMT manuscript, CH₄, H₂O, and N₂O profiles were used and the statement quoted above by the reviewer has been removed to avoid confusion (see supplement, page 10). The N₂O absorption features are in fact quite weak, as shown in Figure 4 of this manuscript and Figure 1 of Thorpe et al., 2012. Detection of N₂O with AVIRIS is challenging, but the CTMF algorithm did detect N₂O anomalies at a wastewater treatment facility in Los Angeles where elevated N₂O concentrations would be expected (Thorpe et al., 2012).

* 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?

Response: This is a very interesting idea, however it is outside the scope of this paper. In the future, we are interested in modifying the spectral range used for the CH₄ retrieval window, including the weaker CH₄ absorption present at 1700 nm. We will also investigate the potential use of a dual window approach using the 2218-2457 nm range and a window centered at 1700 nm. The reviewer brings up a good point regarding strong water vapour absorptions at shorter wavelengths and the authors will consider using techniques described in this manuscript for water vapour retrievals around 940 nm.

* 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)?

Response: The reviewer suggests that a per pixel H₂O column estimate could be first generated (using the 940 nm AVIRIS band) and used to improve the CH₄ retrievals

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presented in this manuscript, which rely on exogeneous profiles derived from volume mixing ratio profiles (NCEP/NCAR). This possibility is of interest to the authors and will be investigated in future work. Robust water vapor estimates are certainly relevant given much of the scientific research performed using AVIRIS/AVIRISng data is performed on reflectance retrievals that require accurate estimates of water vapour. Further, comparing results obtained using exogenous H₂O profiles to those obtained using a per pixel H₂O column estimate could be particularly instructive.

* 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?

Response: The IMAP-DOAS technique is less complex than it appears and very time efficient. It requires some a priori knowledge, but strictly speaking not more than LUT approaches. LUT approaches also depend on profiles of temperature, pressure, and H₂O/CH₄/N₂O volume mixing ratios. Prior to this research, two of the authors were involved in developing techniques that used lookup libraries of simulated radiances using Modtran. To do so, for each scene a new lookup library had to be generated that took into account the ground elevation, flight altitude, time, as well as additional Modtran parameters that are not easily modified or readily documented, such as preset temperature and VMR profiles. In contrast, the IMAP-DOAS technique presented in this manuscript uses an atmospheric model that can be easily adjusted to account for ground elevation, flight altitude, and time. In addition, IMAP-DOAS greatly reduces the complexity by using a non-scattering radiative transfer setup. In previous work, lookup libraries were generated for simulated at sensor radiance in the SWIR for combinations of albedo (0% to 60% at 1% intervals), column water vapor (1.0 cm to 4.0 cm at 0.5 cm

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increments), and CH₄ concentration (0 ppm to 200 ppm at 2 ppm intervals) averaged over the lower 1 km of the atmosphere, which resulted in a lookup library of over 43,000 synthetic spectra. For this example, the intervals are quite coarse (for example a 2 ppm CH₄ interval), so an adequate lookup library would be considerably larger. Modtran calculations are quite slow, so generating a sufficient lookup library would take a very long time.

2.2) IMAP-DOAS/SVD (part 7) * In the same way as for the IMAP-DOAS approach, I find this part difficult to understand. 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.

Response: Following the example of Equation 2 in Guanter et al., 2012, we have included an additional equation (Equation 10) to emphasize the forward model is composed of the background and CH₄ component of the radiance. In addition, we have clearly described the corresponding fit coefficients (see supplement, page 13).

* 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?

Response: The reviewer brings up a good point regarding the effect of the choice of Uselect on the retrieved CH₄ concentrations. The authors agree that this needs to be discussed and thought it would be best to do so towards the end of Section 11.1 in the context of the transects shown in Figure 15 (see supplement, pages 20 and 21). Mean concentrations for the profile were calculated using 8, 9, and 10 columns of Uecon (used to generate Uselect) to illustrate that concentrations are influenced by the choice of Uselect.

3) Results and Discussion * The contribution of the SVD in the improvement of the

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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?

Response: In discussing the IMAP-DOAS results for Inglewood (Section 9.2), high standard deviation of the residuals were observed for dark pixels. In Fig. 9d, dark pixels less than $0.1 \text{ uWcm}^{-2}\text{sr}^{-1}\text{nm}^{-1}$ in the fitting window were excluded. For the SVD discussed in Section 10.2 and the results shown in Figure 12, the vegetated regions also prove problematic. As noted in the existing text, the results shown in Figure 12d exclude pixels with greater than 0.0075 standard deviation of the residual, corresponding with vegetated surfaces present in the scene. The authors have calculated the average radiance at 2278 nm for those pixels with greater than 0.0075 standard deviation of the residual ($0.1368 \text{ uWcm}^{-2}\text{sr}^{-1}\text{nm}^{-1}$) as well as the average radiance for the remaining pixels in the scene ($0.3129 \text{ uWcm}^{-2}\text{sr}^{-1}\text{nm}^{-1}$), which is considerably brighter (see supplement, page 18). The authors have included a discussion of these points in Section 10.2 to emphasize that vegetation has low reflectance in the SWIR and the low SNR is the primary cause of the problematic results shown in Figure 12.

* 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?

Response: The authors have considered the possibility of coupling the CTMF and the hybrid approach using IMAP-DOAS and SVD. As noted by the reviewer, this could utilize the strengths of the CTMF in detecting the diffuse portions of plumes while suppressing the background, and the ability of the hybrid approach to estimate CH₄

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concentrations. In addition, the authors are interested in investigating the relationship between CTMF scores and gas concentrations as well as the potential for improved results by excluding portions of the scene containing the plume prior to running the CTMF and/or hybrid approach.

Please also note the supplement to this comment:

<http://www.atmos-meas-tech-discuss.net/6/C3757/2013/amtd-6-C3757-2013-supplement.pdf>

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 8543, 2013.

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