Atmos. Meas. Tech. Discuss., 6, C3750–C3756, 2013
www.atmos-meas-tech-discuss.net/6/C3750/2013/

Atmospheric Measurement Techniques Discussions

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.

A. K. Thorpe et al.

akthorpe@geog.ucsb.edu

Received and published: 18 December 2013

Response to Anonymous Referee #1 (A revised version of the manuscript clearly indicating all changes is provided as a supplement).

The manuscript of Thorpe et al., submitted for publication in AMT, addresses a relatively new and important topic, namely to detect and quantify elevated atmospheric methane concentrations due to emissions by localized sources using airborne remote sensing. The manuscript is very well written, it contains new material and the topic is appropriate for AMT. I therefore recommend its publication after the items listed below

have been considered by the authors.

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

Section 1: Page 8545, line 16: please explain MMT.

Response: The abbreviation MMT has been replaced with the unit million metric tons.

Section 2: Page 8546, line 12 following: As shown in Krings et al., 2013, using MAMAP it was already possible to quantify local methane emissions, i.e., much more has been achieved than "offer the potential to measure local emissions" and "detect elevated CH4 concentrations". The text needs to be modified to consider this.

Response: The text has been modified to emphasize that MAMAP results permitted an inversion estimate that agrees closely with emission rates reported from mine operators (see supplement, page 3).

Page 8546, line 18 following: Concerning: "However, increased spatial resolution requires reduced spectral resolution". This is not true but depends on the technology used. For example it is planned to extend MAMAP with a 2D CCD detector which would result in increased spatial sampling (same resolution) but would maintain the spectral resolution. The text needs to be adjusted to consider that the current statement refers to existing instruments but not to a fundamental limitation / tradeoff. The same comment applies to page 8566, line 18 following.

Response: This is a good point and this statement has been removed (see supplement, page 3). Clarification has also been provided for the text (see supplement, page 22) to emphasize that imaging spectrometers like MAMAP are currently limited to flying transects across local plumes. The inclusion of the word "currently" does not exclude the possibility that sensors like MAMAP might one day provide increased spatial sampling.

Section 3: Please improve the explanations of the equations: e.g., (i) Eq.(1): this equation appears to be valid for a single absorber and not, as stated in the text, to multiple absorbers, (ii) what is an ideal instrument?, (iii) why is tau the measured differential (!?) optical density (Eq. (2)) ?

Response: As suggested for (i) the text has been modified to be consistent with Equation 1 and valid for a single absorber (see supplement, page 5). Regarding (ii), we clarified this in the text (see supplement, page 5). For (iii), the reviewer is correct to point out this error, it is indeed optical density, not differential optical density (see supplement, page 5).

Page 8549, line 14: The listed SCIAMACHY numbers are valid for spectral resolution, not for spectral sampling intervals.

Response: This change has been made (see supplement, page 6).

Page 8549, line 15-19: Order of citations: Gerilowki et al., 2011, needs to be cited first (for MAMAP instrument details and mission goals) followed by Krings et al., 2011, for CO2 emissions from power plants, followed by Krings et al., 2013, for methane emissions.

Response: This citation order has been corrected (see supplement, page 6). Thank you for pointing this out.

Page 8549, line 24 following: Please add a reference or more details to justify the statement that scattering can be ignored here.

Response: This point was also raised by Reviewer 2 and we have added a few background references regarding scattering in the SWIR (Buchwitz and Burrows, 2003; Dufour and Breon, 2003) and descriptive text to strengthen this section (see supplement, page 5). 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 certainly be low. 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%).”

Section 4: Page 8550, lines 20 and 27: Please also give local time (in addition to UTC) as this is more relevant in the context of this study (meteorology, etc.).

Response: This is a good suggestion. The local times have been added to make it more intuitive (see supplement, page 8).

Section 5: Page 8551, line 16: Please add the spatial and temporal resolution of the used NCEP data.

Response: The NCEP data has spatial coverage of a 2.5 degree latitude x 2.5 degree longitude global grid, vertical resolution of 17 pressure levels between 10 and 1,000 mb, and temporal resolution of 4-times daily. This useful information has been added to the manuscript (see supplement, page 8).

Page 8553, line 12: “each n layer”: n is not the number of layers but the number of atmospheric state vector elements, or ?

Response: The reviewer is correct to point out this mistake, which has been corrected in Section 5.2 of the manuscript (see supplement, page 10).

Section 7: Page 8556, line 11: Here “c” is mentioned for the first time and the reader may wonder where “c” is coming from. This is explained (much) later but it would be nice to at least shortly mention here that later it will be explained how the “optimum” c is being determined.

Response: To improve clarity, the optimal selection of c has been added with a reference indicating that further details of the selection of c is subsequently described in this section (see supplement, page 13).

Section 9: Page 8558, line 25: “sensor saturation”? What does this mean? Totally useless spectra as the maximum possible values (ADC output) are exceeded or values that are above a given threshold, e.g., defined by detector linearity, i.e., no “true saturation”. Please add more details on this.

Response: This point has been clarified in the manuscript (see supplement, page 15). This refers only to a small portion of the scene where the full well of the detector is saturated for multiple channels in the SWIR.

Page 8559, line 1 but also various other places: “uW“ ? What does this mean? Microwatt ? If yes, please use a different (more common) notation (e.g., greek letter) or at least give clear definition / explanation when used for the first time.

Response: The suggested clarification has been made (see supplement, page 15).

Section 10: Page 8561, line 26: What does “0.0075” mean? Is this the relative signal variation (i.e., 0.75%) or are some other units involved here ?

Response: The SVD is performed on standardized radiance (unitless), so the standard deviation of the residual for each image pixel shown in Fig. 12c is also unitless. Because this is unclear, it has been clarified in the text (see supplement, page 17) and captions for Fig. 11 and Fig. 12.

Section 11: Page 8563, line 4: 2x typo “threshold”.

Response: These two typos have been corrected (see supplement, page 19).

Page 8563, line 8: I recommend to replace “validated against” by “compared with”.

Response: The authors agree that the suggested language is a better choice and it has been incorporated (see supplement, page 19).

Same paragraph: It would be nice of a more detailed explanation of the CTMF method would be given.

Response: A detailed description of the CTMF was not provided here given the considerable length of the manuscript. However, some additional information on the CTMF was added for completeness (see supplement, page 19). It was also noted that additional algorithm details and CTMF results from both the COP and Inglewood scenes are available in Thorpe et al., 2013 (Remote Sensing of Environment).

Caption Fig. 1: Replace “spectral sampling” interval by “spectral resolution” (as this is relevant for convolution).

Response: The term “spectral sampling interval” has been replaced by “spectral resolution” throughout the entire document (see supplement, page 30).

Fig. 15: I guess that an offset has been added to the images. If yes, please list which offsets have been added. xy-plot bottom right: The green and blue color are hard to distinguish in a printout. I recommend to replace “light blue” by “dark blue”.

Response: No offsets were applied to Fig. 15. a, b, or c. However, an offset was applied to the transect data shown in d. The CTMF results shown in red, which correspond to the axis “CH4 CTMF score,” is offset for clarity from the IMAP-DOAS (green) and SVD (cyan) results, which share the axis “CH4 (ppm relative to background)”. The descriptive text corresponding to Fig. 14 as well as captions for Fig. 14 and Fig. 15 has been changed to clarify this (see supplement, pages 20, 40, and 41). The choice of the colors was deliberate and made consistent between the four panels of Fig. 14 and Fig. 15. The choice of darker blue for the transect shown in Fig. 15 b would be difficult to see against the black background. There are two different vertical axes in Fig. 15 d, and the SVD and IMAP-DOAS results share one vertical axis, which is labeled in cyan. While the green may be a bit difficult to distinguish from the cyan in a printout, it is much clearer in the digital pdf. The original color choice will be maintained, however additional descriptive text has been added for clarity in the captions for Fig. 14 and Fig. 15 (see supplement, pages 40 and 41).

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/6/C3750/2013/amtd-6-C3750-2013-supplement.pdf

---

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