

Answers to the comments of anonymous Referee number # 2 to Gerilowski et al., "MAMAP – a new spectrometer system for column-averaged methane and carbon dioxide observations from aircraft: instrument description and performance assessment." , Atmos. Meas. Tech. Discuss., 3, 3199–3276, 2010

First of all we would like to thank the referee for the detailed, helpful and competent comments. Below we present our response and clarifications to each of these comments. As the reviewer itself provided no numbering for his comments, we introduced a numbering of the referee comments and hope that it reflects the referees view.

Comments:

#1: This paper provides a comprehensive description of the justification and performance of the MAMAP instrument. Overall, this is a good paper, but includes a few apparent misconceptions and could be improved by narrowing its focus to the MAMAP instrument's ability to monitor methane. While the title, abstract, and introduction refer to "CO₂ and CH₄", there is little coverage of CO₂, especially in the introduction. In the rest of the paper, CO₂ is basically being used as a reference gas for the CH₄ measurement, in lieu of O₂.

R#1: We agree. This will be changed in the revised version where also more CO₂ applications will be added as the instrument ability is not only limited to measure CH₄ (especially for geological applications).

#2: The O₂ A-Band is also apparently also measured by the instrument but this channel is barely mentioned here.

R#2: As mentioned by the authors, this paper focuses primarily on the SWIR channel. The XCO₂(O₂) and XCH₄(O₂) performance will be discussed in a separate publication.

#3: The abstract introduces a number of concepts that might be somewhat confusing to some readers. For example, what is a "micro site" (line 6) ?

R#3 Microsites are sites where emissions are estimated on micro scales i.e. 0.1–100 m². See authors response to Referee #1 comment #m2, see also Sachs et al., Environmental controls on CH₄ emission from polygonal tundra on the microsite scale in the Lena river delta, Siberia, Global Change Biology (2010), doi: 10.1111/j.1365-2486.2010.02232.x).

#4 The statement (line 8) "There is a need for measurements of the dry columns of CO₂ and CH₄" is also somewhat confusing. It is impossible to "measure" a "dry column." It is not, however, impossible to retrieve the "column averaged dry air mole fraction" from measurements of CH₄ or CO₂ and O₂.

R#4: We agree. This will be changed in the revised version.

#5: Would it be possible to mention in the Abstract that while MAMAP measures CO₂, CH₄, and O₂, this paper focuses on the CH₄ measurement?

R#5: The paper focuses primarily on CH₄, but the instrument can also be used for CO₂ measurements of local sources / hot-spots as demonstrated in section 6. In the revised version additional examples of CO₂ local and point sources will be added. See also authors responses to referee #2 comments #1 and #9.

#6: Is a 8-page introduction justifying the need for airborne measurements of CH₄ and CO₂ really essential for a 30-page paper? Most of the problems with this paper were encountered in this long introduction. I suggest that it be reduced in length to no more than 2 pages.

R#6: Introduction will be substantially shortened in the revised version.

#7: Pg 3203, near bottom: “The footprint of the Tanso-FTS instrument is 10 km with a gap of about 160km between observations.” The spacing between GOSAT footprints is variable. This number is correct for the nominal 5-point cross-track mode. More recently, to address a problem with their scanner, the GOSAT team has changed to a 3-point cross-track mode with ~320 km between footprints.

R#7: We agree. It will be mentioned that the spacing is variable. It will also be mentioned that the 160 km gap applies only for the nominal 5-point cross-track mode.

#8: Pg 3204, near bottom: “In-situ measurements of ship-based 15 transects show local atmospheric concentration increases of up to 8 ppmv” This is the first place ppmv is mentioned, and the first time that the mixing ratio of CH₄ is quantified in terms of ppmv. The unit “ppm” is not defined until pg 3207, and the background mixing ratio of CH₄ is not quantified. As a minimum, would it be possible to mention that this is ~5 times higher than the background value of ~1.7 ppmv?

R#8: Background concentration will be mentioned in the revised version (if the cited Shakhova et al. paper will not be shortened out in the revised introduction section).

#9: Pg 3205, near bottom: Here we see our first discussion of CO₂ emissions, almost lost between discussions of CH₄ from geological sources (seeps, mud volcanoes) and CH₄ emissions from landfills. Volcanoes emit < 1% as much CO₂ as human activities. Why focus on CO₂ emission from volcanoes rather than fossil fuel, biomass burning, manufacturing, or other sources?

R#9: Other anthropogenic CO₂ sources will be added to the manuscript. Actually airborne in-situ measurements and indirect remote sensing measurements (i.e. remote sensing of SO₂ and utilizing SO₂/CO₂ ratios from groundbased in-situ measurements) are used to study different volcanic CO₂ degassing processes and to monitor CO₂ volcanic emissions. We believe that these indirect remote sensing

methods can be potentially improved by using direct CO₂ remote sensing measurements performed with instruments like MAMAP.

#10: Pg 3206: The effort to quantify CO₂ and CH₄ emissions from the sites is good, but they are difficult to put into context. What fraction of the total global emissions is 61 Mt CH₄/yr or 41 kg CH₄/hr?

R#10: We agree. The fraction of total global emission will be added in the revised version (if the cited paper is not shortened out). The total global emission was estimated by Wuebbles and Hayhoe (2002) to be 503 MtCH₄/Yr.

#11: Pg 3207, near top: “The accuracy of such assessments is questionable.” You might wish to add the qualifier “Based on the uncertainties cited above”

R#11: We agree. It will be changed in the revised version of the manuscript (if the cited part will not be shortened out in the revised introduction section)

#12: Pg 3207, near bottom. DIAL instruments are first mentioned on pg 3206, where their results are highlighted. Given that context, the discussion of DIAL measurements at the bottom of pg 3207 is confusing and contradictory. Why are high altitude observations needed to monitor point sources at the surface? If we are trying to measure a source at the surface, the background CH₄ (and CO₂) in the column is a source of noise. Low-altitude LIDAR measurements from aircraft provide greater sensitivity to surface point sources because they are only sensitive to the CH₄ concentration in the column of air between the aircraft and the target. Passive spectroscopic measurements of reflected sunlight constrain the entire path from the top of the atmosphere to the surface, and back to the aircraft. These measurements are less sensitive to a given mass of CH₄ because that CH₄ makes a smaller contribution to the total column. Observations of the complete column could provide constraints on the regional-scale budget of CH₄, but provide no intrinsic advantage for monitoring point sources. In fact, half way down the following page, in the context of TIR measurements, you acknowledges this, stating “The higher the instrument is flown, the smaller is the sensitivity to the lower parts of the boundary layer”, perhaps without realizing that this also applies to NIR measurements.

R#12: We will try to answer most of the Referee #2 comments in short without going to deep into detail in the comparison of DIAL systems and passive SWIR/TIR systems. As mentioned in the paper, a real and critical comparison between currently available low altitude DIAL systems and MAMAP is difficult to be obtained, because many of the required parameters of the existing DIAL systems are not published and the published values are taken mostly from non-peer-reviewed publications. For a better comparison between both types of systems the parameter [ppm m] was introduced. This parameter is altitude independent. Examining the averaging kernels of MAMAP given in Fig. 6 one can see, that for albedos > 0.1 the MAMAP averaging kernels have their maximum at the lower boundary layer. Thus a SWIR-DIAL system with a given precision of X ppm m for an integration time Y will have the same sensitivity for changes in the lowermost boundary layer as a passive system with the same precision X in ppm m and same integration time Y. Of course a column

variation in the partial column above the aircraft can introduce systematic errors in the retrieved column below the aircraft but will not change the sensitivity/precision (given in ppm m) of the instrument. By flying above the planetary boundary layer (or higher) variations in the partial column over the aircraft can be assumed as smooth in most cases. The averaging kernels in Fig. 6 also show, that the contribution of a total column variations caused by concentration variation of the partial column above the aircraft are smaller in comparison to variations below the aircraft. Thus one can state: smooth variations in the partial column over the aircraft introduce relative accuracy errors if these variations are assumed as constant as done in the current version of the retrieval. To correct for those variations, MAMAP consecutively performs zenith measurements (which are currently not incorporated in the retrieval). The other point not mentioned by Referee #2 is, that for an emission estimate the dry air column (mole fraction) is required also for DIAL instruments. Thus, data from existing low altitude DIAL systems need to be converted from wet columns to dry partial columns (mole fractions) and therefore need to be corrected for dilution by water vapor (where variation can be significant on scales of ~250 m in the lowermost boundary layer), temperature, pressure and associated line broadening.

Concerning the decreased altitude sensitivity: decreasing precision with increasing altitude is inherent for all three types of systems (SWIR-DIAL, passive SWIR and passive TIR) due to different reasons. The main difference between SWIR and TIR systems is that the averaging kernel for the lowermost boundary layer degrades for passive TIR systems with higher altitude while typically staying maximum for SWIR-DIAL and passive SWIR systems. The only exception for passive SWIR systems are measurements over scenes with very low albedos i.e. ~ 0.01 over water surfaces. This drawback can be overcome by the use of solar glint observations in improved versions of the instrument.

Concerning the altitude requirement (comparison of DIAL with passive SWIR): The main difference between DIAL and passive SWIR systems is that actual available DIAL systems are limited to ~ 300 m altitude (while the recommended altitude for most systems is ~ 100 m). As mentioned in the manuscript, development of high altitude DIAL systems is ongoing but currently not available for field applications. An altitude limitation < 300m of current DIAL systems is by far not enough for emission measurements of important hot-spot areas on local scales (i.e. ~ 1-20 km diameter) like for instance entire gas deposits near Yamal/Russia with more than 10 km of diameter (see Jagovkina et al). It can be expected, that for such measurements an aircraft altitude of at least the height of the planetary boundary layer would be required which can reach in summer an altitude of up to 2 km. CTM simulations performed by Jagovkina et. al (2001) show, that CH₄ gradients can occur near the Yamal peninsula in altitudes of up to 7 km as mentioned in the manuscript. The altitude requirement will be changed from 7 km to "at least the height of the boundary layer (up to 2km)" in the revised version of the manuscript. Nevertheless it will be mentioned, that gradients can occur to altitudes up to 7 km for strong hot-spot areas.

#13: Pg 3209: "These must be capable of measuring CH₄ from high altitudes (>7 km) over different surface types at high horizontal resolution (<250m) over areas <10 km up to 200 km and yield a precision and accuracy equal or better than the accuracy achieved by current and planned sensors, i.e. with a precision and accuracy of equal or better than about 1–2% (Bréon and Ciais, 2010). The need for measurements

from >7km is not clearly justified. It is more of a capability than a requirement. Is < 250 m needed for all targets or just some? Why “areas” < 10 up to 200 km? If it is an “area”, why isn’t it km² rather than km? Also on spatial scales ranging from cities to nations, the largest variations in the column averaged CO₂ dry air mole fraction are typically no larger than 1-2 %. While 1-2% is fine for CH₄, and is adequate to detect the strongest sources of CO₂ (medium to large coal-fired power plants), this precision is not adequate for detecting the vast majority of CO₂ sources or for quantifying the CO₂ fluxes from these sources. An order of magnitude greater precision is needed for those applications.

R#13: Concerning the footprint of 250m : The required footprint for different sources can be inferred from the source size and the formulas given in section 5. For estimating the emissions of smaller landfills with a diameter of for instance 500m and emissions < ~ 5 ktCH₄/Yr a footprint of < 250 m would be required for a non imaging instrument like MAMAP. For smaller sources like (smaller) seep fields even smaller footprints are required. Depending on the achievable precision, also footprints > 500 m can be sufficient for stronger sources like big landfills (i.e. emissions >> 5 ktCH₄/Yr, diameter 1-2 km) or for measurements over entire gas fields.

Concerning the required precision: the authors agree, that the requirements of the instrument are inferred primarily for sources of CH₄. Nevertheless such requirements will allow also to constrain CO₂ emissions of strong geologic and anthropogenic local sources. (see also authors response to referee #1 comment #m13 concerning the precision / relative accuracy requirement).

#14: A little further down that page, “The measured data ideally needs to be of an accuracy and precision to yield on inversion the CH₄ emissions from less intense but extensive and larger scale sources and sinks, such as wetlands. As a threshold the accuracy and precision of the data yields on inversion significant constrains on local hot spot emissions to separate them from 25 the less intense but extensive larger scale sources and sinks and thereby allowing an improved estimate of both.” What CH₄ “sinks” are being considered here? Since when are wetlands “sinks” of CH₄? Pg 3212, near middle of page:

R#14: The word "sink" (for CH₄) will be removed from the revised version of the manuscript in this context.

#15: On pg 3209, the concept of Surface Spectral Reflectance (SSR) is introduced (I had to search for it). The quantity “albedo/SSR” is then introduced here, on pg 3212. This is confusing. Why do we care about the albedo? All that seems to be important is the amount of radiation reflected in the direction of the sensor. The albedo and SSR are only equivalent for Lambertian surfaces, but that is never stated here. Pg 3210:

R#15: The albedo is used for the RTM simulations where Lambertian surfaces are assumed.

#16: Finally we are introduced to MAMAP, only 8 pages into the paper. Pg 3211: “One spectrometer system measures in the SWIR over the spectral region at 1590–1690nm to enable simultaneous retrieval of CO₂ (1590–1620 nm) and CH₄ (1630–1750 nm) columns. The second push-broom imaging spectrometer system measures in the NIR over the spectral region between 756–769nm for the detection of the oxygen (O₂) absorption using the O₂-A band. The retrieved O₂ columns can be used to convert the greenhouse gas columns into dry-air column-averaged mixing ratios (see Sect. 3).” Are we to infer from this that while the O₂ channel is a push-broom imaging spectrometer, the CO₂/CH₄ channel is a point spectrometer?

R#16: The SWIR part of the instrument is "non-imaging" as a linear array was used as detector for the SWIR spectrometer channel ("non-imaging" will be added). More details on the instrument concept are given in section 2.2 where the co-boresighting of both spectrometer is explained.

#17: Near the bottom of that paragraph, you note: “Optionally, cosine diffuser plates or a combination of cosine diffuser plates plus glass fibres and collimator optics can be installed on-top of the zenith sky telescopes. In this configuration the instrument can perform direct solar irradiance measurements and diffuse down-welling (ir) radiance measurements in zenith-sky mode.” What is the value of data from a diffuser plate and fibers? While diffuse solar reflectances might be useful for a solar radiation budget, they are far less useful for quantifying trace gas abundance because you will never know the true optical pathlength to 1-2%. Besides this detail, the description of the spectrometer and its measurements is very thorough and informative.

R#17: Direct small field of view occultation measurements using a heliostat system would be best suited to determine the column above the aircraft. However implementing a heliostat system will require a larger zenith aircraft aperture currently not available especially on the smaller aircrafts like the used Cessna 207. Thus we are currently using CH₄/CO₂ column ratios derived from zenith sky irradiance measurements (zenith diffuser measurements primarily dominated by the direct solar irradiance) instead of occultation. Therefore, most of the light path errors introduced by scattering of incoming light cancel out in the ratio like for nadir.

#18: Pg 3216: “The advantage of the first approach is that the mixing ratio of O₂ in dry air is well known (20.95%) and constant up to about 100km and comprises 99.99% of the atmosphere.” The fact that the O₂ mixing ratio is well known is only half the advantage of O₂ measurements. Because of this, simultaneous, co-bore-sited observations of O₂ with CH₄ or CO₂ can help to identify and correct pointing errors, surface pressure uncertainties, and a host of other sources of systematic error that are common to all 3 spectrometers.

R#18: The advantage of the use of the O₂-A band will be published in a separate publication. This publication focuses primarily on the CO₂/CH₄ SWIR channel. In this publication we only mention that the instrument also measures the O₂-A band and has the capability to use other retrieval approaches instead of the presented. An additional notice will be given in the manuscript

#19: Pg 3218: The use of SCIATRAN, a full column model, to retrieve results for an aircraft instrument that only measures partial columns is a significant weakness of this analysis.

R#19: SCIATRAN is not a “full column model” but a radiative transport model able to simulate the radiative transfer through the whole atmosphere for various observation geometries. MAMAP is a passive instrument using reflected and scattered solar radiation and hence its measurements carry in principal total column information. The radiative transfer forward model SCIATRAN takes into account the exact MAMAP measurement geometry including the aircraft's altitude in the atmosphere and solar zenith angle.

#20: Somewhere here, I began to wonder whether there was any effort to calibrate MAMAP against absolute radiometric or spectroscopic standards in the laboratory. Was it calibrated? Did I miss that discussion?

R#20: The instrument is currently not absolutely calibrated, see also authors response to Referee #1 comment #m14.

#21: Pg 3227: Figure 8 is described in the text before Figure 7. Pg 3240: “In order to test the MAMAP sensitivity to score emissions and validate the results obtained “ It is not clear that these experiments, especially the CH₄ measurements constitute a “validation” because there were no comparisons against a recognized measurement standard. At best, the system was “validated” against a model, whose inputs were not well constrained by observations. Complicating factors, such as the presence of cirrus, further compromise the validation. This would be more accurately characterized as a “detection” than a quantitative validation.

R21: We agree. The term validation will be exchanged in the revised version of the manuscript

#22: In spite of these criticisms and suggestions, I believe that that this is a good paper, and could be modified for publication. The introduction is far too long, and is confusing in many places, and would benefit from being shortened substantially.

R#22: We agree. The introduction will be substantially shortened.

#23: In the description of the instrument performance, a little more description of the laboratory calibration (if any)

R#23: The instrument is currently not absolutely calibrated. See authors response to Referee #1 comment #m14.

#24: and a little more care in error analysis and propagation of errors would be helpful

R#24: As both columns for the theoretical analysis are normalized to background concentration (i.e. $a=1$ and $b=1$) error propagation (i.e. Eq (13)) directly follows from $\sqrt{\text{var}(a)/b^2 + \text{var}(b) \cdot a^2/b^4}$. This will be clarified in the revised version (see authors response to Referee #1 comment #m31).