

Interactive comment on “MAMAP – a new spectrometer system for column-averaged methane and carbon dioxide observations from aircraft: instrument description and performance assessment” by K. Gerilowski et al.

Anonymous Referee #2

Received and published: 28 September 2010

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. The O₂ A-Band is also apparently also measured by the in-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



strument but this channel is barely mentioned here. The abstract introduces a number of concepts that might be somewhat confusing to some readers. For example, what is a “micro site” (line 6)? 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. Would it be possible to mention in the Abstract that while MAMAP measures CO₂, CH₄, and O₂, this paper focuses on the CH₄ measurement? 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. 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. 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? 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? 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? Pg 3207, near top: “The accuracy of such assessments is questionable.” You might wish to add the qualifier “Based on the uncer-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

tainties cited above, . . .” 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. 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% (Br’eon 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 pre-

Interactive
Comment[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

cision is needed for those applications. 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: 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: 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? 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 de-

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



Interactive
Comment

scription of the spectrometer and its measurements is very thorough and informative. 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. 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. 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? 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. 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. In the description of the instrument performance, a little more description of the laboratory calibration (if any) and a little more care in error analysis and propagation of errors would be helpful.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 3199, 2010.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)