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Atmospheric Measurement Techniques Discussions

Interactive comment on "Performance of a low-cost methane sensor for ambient concentration measurements in preliminary studies" *by* W. Eugster and G. W. Kling

Anonymous Referee #1

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General Comments

This manuscript describes the evaluation of a low cost, low performance sensor for methane to determine the suitability for its use in a survey application. The intention is to identify some means of surveying extended greenhouse gas emission sources, such as arctic tundra. A network of low cost sensors could be designed to cover the areal extent of the source efficiently. Higher performance (and therefore higher cost) sensors could then be deployed to characterize the most representative points of the source. The manuscript evaluates the performance of a low cost, solid state, metal oxide (MO)-based sensor for methane. Two such sensors were deployed for a season

on a floating platform in an Alaskan lake, along with a tunable diode laser (TDL)-based high performance sensor. The MO sensor output is known to have a dependence on both ambient temperature and humidity, and cross sensitivities to other potentially important species, such as CO. The authors have presented their arguments clearly. The supporting figures and tables are sufficient and the language is clear and concise.

The paper provides a somewhat rare look at some of the issues inherent in designing a network to measure extended natural emission sources. This includes the perspective of determining if less capable and less expensive sensors have any value in surveying for the best sites to locate more capable sensors. The characterization of the individual sensor capabilities in the field is quite useful and the analysis of the initial field data is valuable. In this effort though, there seems to be a need for definition of the metrics for usefulness. For example, in general terms, how much performance can be sacrificed and still identify major trends; can diurnal excursions be measured with what signal-to-noise ratio? The performance of the MO sensors were evaluated with respect to one site and not generalized. It might also be useful to determine the economic boundary conditions of the problem for a given site. . .for example. . .how many less-capable sensors can one afford in order to cover the emission source ? Can the required survey measurements still be made even with inexpensive sensors?

The authors may want to include mention of other highly capable but still economical, laser-based sensors coming available. For example, S. So, A. A. Sani, L. Zhong, F. Tittel, G. Wysocki, "Laser Spectroscopic Trace-Gas Sensor Networks for Atmospheric Monitoring Applications", The 8th ACM/IEEE International Conference on Information Processing in Sensor Networks, Proc. of ACM, vol. ESSA 2009, (2009) as well as others.

Specific Comments

There are several points in the manuscript that the authors could provide some additional clarification: *Page 2570, line 24. . . Cross sensitivities . . ..Are there cross sensi-

tivities to any hydrocarbons or other volatile organics that might also be emitted from the lake or tundra wetlands? *Page 2574, line 24. . .Does the sensor drift in one direction only so that initial and final calibration measurements will provide a linear interpolation? What is the drift mechanism? *Page 2575, line 12 The correlation coefficients obtained for the two MO sensors are quite low compared to that obtained from the manufacturer's data plot. Is there any understanding of why this is the case? *Page 2576, line 17 The authors state they found no consistent lag between the MO and TDL sensors but reproducible lags seem to have been observed with the diurnal data trend (Fig 5). A little more discussion here might help. *Page 2576, lines 19-22 The magnitude of the observed MO sensor drift is twice that of the seasonal change. What is the smallest seasonal change that can reliably be recovered? *Page 2577, lines 19-20 The R2 values obtained for the concentration data of the MO sensors with respect to the TDL sensor data were poor. . .supposedly because of time lags. . ..any idea as to mechanism for the lag? If the lag could somehow be removed, the R2 values would presumably increase. Could a correlation calculation be done that would identify a constant time lag that could then be removed? *Page 2579, lines 12-13 CO contamination from forest fires could be well above normal ambient, depending on the distance of the observation point from the fires

Technical Corrections

*Page 2572, line 2 RL should be Rs *Page 2575, line 10 R2 should be Rs

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Interactive comment on Atmos. Meas. Tech. Discuss., 5, 2567, 2012.