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Atmospheric Measurement Techniques Discussions

Interactive comment on "Estimation of waste water treatment plant methane emissions: methodology and results from a short campaign" *by* C. E. Yver-Kwok et al.

W. Eugster (Referee)

werner.eugster@usys.ethz.ch

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The authors present an interesting methodological paper to assess the methane emissions from a waste water treatment plant in France near Valence. The field experiment uses an impressive combination of state-of-the-art gas analyzers, includes the natural ^{222}Rn tracer and the artificial C_2H_2 tracer applied to the water tank. In principle, they also measured CO_2 and $\delta^{13}C$ in CO_2 but did not analyse these gas emissions.

The most critical point is the short duration of the measurements in combination with the gappy ^{222}Rn data from the FTIR, which makes all quantitative statements rather

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problematic, as the authors state themselves. A possible scaling error with natural ^{222}Rn flux estimates, and the somewhat arbitrary approach to estimate extreme emissions (see my detailed comments below) reduce the scientific quality of the overall manuscript.

However, the presented material has a relevant scientific value for advancing the methodology to measure CH_4 fluxes from waste water treatment plants and thus the authors should be given a chance to revise and improve their manuscript for AMT.

Details

9184/1: "at all scales" – this is not possible. Please use more specific wording (you do not work on the atomic scales and at interstellar scales, for example). Maybe simply write "at all relevant scales" if you do not want to be more specific.

9185/6: CH_4 should not be subscript to kg, it is kg CH_4

9187/7: write "and" between the two references.

9188/3: how can you be sure that it is only the temperature variablity that matters for the reproducibility? Are you sure that the 2°C higher temperature does not matter, only the variability?

9188/4: put a period after the closing parenthesis

9191/21: atm is on the list of unacceptable units (see http://physics.nist.gov/cuu/pdf/sp811.pdf). Please rectify and use SI units only throughout the manuscript!

9193/6: on 9192/21 you defined H as "a well-mixed layer of height H", whereas here you call it the "boundary-layer height". This is confusing since the boundary layer is only well mixed during unstable stratification, i.e. during the day, but not at night.

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However, you implicitly pretend Eq. (5) can be used at any time, which is not quite correct. Please (a) be consistent in the wording, and (b) clarify the issue with non well-mixed conditions (since you do not limit your analysis to well-mixed conditions). Under nocturnal conditions Eq. (5) would need to be expanded to account for the different diffusivities of ^{222}Rn and CH_4.

9193/23: this is not unproblematic: you use a specific, local measurement for everything, but a very coarse, generalized ^{222}Rn flux map. Hence, all errors in the estimate of the true ^{222}Rn flux at your locality translates to errors in CH_4 flux estimates. I am not convinced that 25% uncertainty is a realistic estimate for your environment. It is the problem that the uncertainty of a mean values is definitely lower (i.e. 25% in your estimate) than that of a specific timepoint as it is used in Eq. (5).

9193/22 and elsewhere: Bq is s^{-1} in SI base units, so your units are definitely wrong here – Bq already has the time reference it it, multiplying Bq with h^{-1} gives an acceleration, which is nonesense. Please rectify, check all numbers. The value in the map under http://radon.unibas.ch is on the order of 1 Bq cm^{-1}, so for 1 m^{-1} it should be 10000 Bq m^{-1}, I guess. What you wanted to do with the addition of h^{-1} remains obscure. Please rectify.

9194/9–10: the selection of a threshold correlation of 0.6 seems rather arbitrary. How did you chose this threshold, how can it be justified?

9198/14 and elsewhere: when reporting mean $\pm$ uncertainty you must show the same number of digits for both, **and** you must only show the significant digits. In addition to that I wonder: if you actually use non-SI units to express your flux (day is an accepted non-SI unit), why not use SI-derived mass units to have more easily readable numbers, e.g. 6.0 ± 0.4 g d^{-1}.

9199/18–20: you call this a “very conservative upper limit”, but from only a few days of measurement I am not sure you have the full insight into the true variability of the system. Why are you not using a (more robust) extreme values distribution approach

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(as e.g. the Gumbel approach, or a Pareto approach) to make a sound estimate for the extremes? We tried the Gumbel approach with eddy flux data (see Eugster et al. 2010) and this should also be a sound approach for your estimates.

9202/7: normally the linear function is fit to the data and not vice-versa! Please reword.

9203/2: use ISO 8601 date and times throughout your manuscript (http://en.wikipedia.org/wiki/ISO_8601, http://www.iso.org/iso/home/standards/iso8601.htm)

9203/22–26: this is the weakest aspect of the manuscript: you realized that 4 days is not enough to make a sound estimate. But since this manuscript was submitted to a journal that rather focuses on the method than on the results obtained with this method, I see the value of the manuscript to be published after revisions. However, as a reviewer, I am not supportive of this least-publishable-unit approach.

9204/5-7: There is another confusion here: if three quarters of total emissions are from the plant and the other quarter is 0.077 kg yr^{-1} per inhabitant then I would have guessed that 100% is roughly 0.308 kg yr^{-1} per inhabitant. But then you claim that 106.102 kg yr^{-1} per inhabitant is "close to the last value" (which was not even 1/1000 as big), which cannot possibly be correct. What are the correct numbers? Please rectify and clarify the wording.

Table 2: significant digits only, and same number of digits for mean and uncertainty! Use g d^{-1}.

Table 3: significant digits only!

Table 4: (3): 2 digits only; (2) same number of digits, please; Station: significant digits only (which means: no digits here); Surrounding: remove two insignificant digits

Fig. 3: the word is "deployment" not "employment"

Fig. 4: it is obvious that CH_4 and ^{222}Rn have very different time constants here, hence a correlation approach is not unproblematic (serial autocorrelation and serial cross-

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correlation problem). In my view Rn is to a great extent the inverse of the wind speed, which indicates the relevance of mixing during conditions where the atmosphere is stably stratified. It is also obvious that CH_4 responds more strongly to abrupt changes in wind direction than Rn. I think the main problem in your approach is that you use Eq. (5) also for conditions when the atmosphere is not well mixed at all.

Fig. 5: do not connect points across data gaps. It is unclear what the lines denote, and what the symbols represent.

Fig. 6: time labels must use natural inervals, so if you want to show 10-minute intervals the labels must be 14:50, 15:00, 15:10 etc. (not 14:49, 14:59, etc.). Better remove some labels, or increase the interval to 15-minute intervals. The rule of thumb is still that no more than 10 labels should be shown. Also do not place the legend prominently in the center of the graph area. Legends to go to one of the corners. There is no need for a frame around the legend.

Fig. 7: there should not be any symbols on the frame around the plot area. Increase the plot area to allow points to be inside the plot area.

Fig. 8: increase plot area to allow points to be inside that area (not sitting on the frame).

Fig. 10: increase plot area to allow curves to be inside that area (not sitting on the frame). Make sure the tick labels are not overlaying each other. Use ISO 8601 dates. Also here, natural time intervals would be preferred (which would also reduce the problem with overlay over the y-axis tick labels).

Fig. 11: use ISO 8601 dates

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Reference

Eugster, W.; Moffat, A.; Ceschia, E.; Aubinet, M.; Ammann, C.; Osborne, B.; Davis, P. A.; Smith, P.; Jacobs, C.; Moors, E.; Dantec, V. L.; Béziat, P.; Saunders, M.; Jans, W.; Grünwald, T.; Rebmann, C.; Kutsch, W.; CzernÃ¡, R.; Janouš, D.; Moureaux, C.; Dufranne, D.; Carrara, A.; Magliulo, V.; Tommasi, P. D.; Olesen, J E.; Schelde, K.; Olioso, A.; Bernhofer, C.; Cellier, P.; Larmanou, E.; Loubet, B.; Wattenbach, M.; Marloie, O.; Sanz, M.-J.; Søgaard, H. & Buchmann, N. (2010) Management effects on European cropland respiration. *Agriculture, Ecosystems and Environment*, **139**, 346–362

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 9181, 2013.