

Interactive comment on “Methane emission estimates using chamber and tracer release experiments for a municipal waste water treatment plant” by C. E. Yver-Kwok et al.

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Dear Editor,

thanks for the explanation. It would have been good to know this information as a reviewer. The similarity report provided in the online system only gives a similarity index of 43%, which (as noted in my previous assessment) would indicate, that clearly more than 57% is new.

What concerns me more is that our document checker finds more than 81% agreement – which clearly biases my perception of the issue.

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My interpretation of the open peer review so far was that a discussion paper has undergone full peer review, but then the Editor makes the decision that no final publication is foreseen. From your information I deduce that reviewing a discussion paper is **not** a full journal peer review. A clearer formulation thus would be “or discussion papers that were not accepted for a final paper in AMT”, this would have made your point much clearer.

So as already noted in my online assessment, I find the paper interesting; I take note that the authors have been able to address the main critique from the previous round (I considered the short duration in combination with gappy ^{222}Rn data from the FTIR a major flow, hence recommended major revisions). Thus, the remaining details to address for the final version are listed below.

I think the Editor has provided the insight why this paper – although I interpreted it as self-plagiarism – is within the journal’s rule set and hence the manuscript should get accepted after minor revisions.

Minor Issues

2961: 6, 8 (and elsewhere): use comma as the separator in numbers exceeding thousands (18,600, not 18 600)

2964: 11: please give a reference to clarify the Gaussian error propagation. I came across Lo (2005) Ecological Monographs, 75(4), 451–466 who claims that this technique is not well known and rarely used in ecology, thus maybe other readers would also profit from seeing a reference used to understand the details of the concept.

2966: 1: to calculate the unknown flux you need a **regression** not a correlation here (or I misunderstood your approach)

2966: 24: how independent are data in the time series averaged at 1-s intervals? I assume that there is a high autocorrelation. Maybe add a statement how much over-

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sampling this means, or clarify that the system really provides serially uncorrelated data at this resolution (which I however doubt).

2966: 29: 500 km is too much – add m for meters after 500 to avoid misinterpretation.

2967: 10: if → though

2969: 1: it would not hurt to mention the year of the field study again, e.g. “took place in year 2012 from . . .”

2973: 6: fast → quick

2973: 22–24: I disagree with the statement “We can then reasonably expect that if we had calibrated the CRDS instrument more often, we would reach the recommended goal even for polluted air masses”. These instruments are very sensitive to changing temperatures and pressure conditions, because their control loops are tuned to optimize minute drifts as in a nicely thermostatised laboratory environment. In my view more frequent calibration does not really solve the issue, such instruments always have a lower performance in outdoor real-world applications than in a perfect laboratory. We accept this, and are fine with this, but I am not convinced at all that this simply can be overcome by more frequent calibration. Most likely a better insulation and temperature and pressure control will have a much stronger (positive) effect on performance. At least that is my experience. You may want to modify the wording to be less categorical with this statement.

2976: 5–7: This is an interesting statement!

Fig. 4: you have a turn in wind direction most likely via North late on 18 September, but with points connected with lines it looks like the clockwise turn has stopped and jumped to NE via S. Probably using dots (as in panel a) without connecting lines better represents conditions.

Fig. 6: use a–h to label panels (this is also easier to refer to in the text where I first struggled across this)

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Fig. 10: there are only three tracer release episodes shown here, you removed one but should also reflect this in the first line of the caption.

Final Statement

I still don't understand why the journal editors did not want such a major revision in the first place for the 2013 version. Editors always have to make the difficult decision to want a paper or to reject a paper. In my view, something like this was what I expected as a major revision of the previous paper (where I suggested major revisions). For a new submission I still think there is a need for improving things with the Copernicus journals. On their web page http://publications.copernicus.org/services/public_peer_review.html it still says: “In the second stage, the peer-review process is completed and, if accepted, the final revised papers are published in the journal” – so for me the full journal peer review is **finished** when a paper is **not** accepted for the journal, no matter at which stage this happens. And hence I understood that the status of such a paper is as published, but published in the gray literature.

It would be helpful if you could encourage the Copernicus authorities to update the “General terms” to reflect what actually is meant by “discussion papers that have not undergone full journal peer review”, this would help the next authors running into the same issue – and their reviewers.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 2957, 2015.

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