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Atmospheric Measurement Techniques Discussions

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.

Anonymous Referee #2

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Review of amtd-8-2957-2015 Yver-Kwok, C. E., Müller, D., Caldow, C., Lebègue, B., Mønster, J. G., Rella, C. W., Scheutz, C., Schmidt, M., Ramonet, M., Warneke, T., Broquet, G., and Ciais, P.: Methane emission estimates using chamber and tracer release experiments for a municipal waste water treatment plant, Atmos. Meas. Tech. Discuss., 8, 2957-2999, doi:10.5194/amtd-8-2957-2015, 2015.

This paper describes an experiment where various measurement techniques were applied to the same source but with different spatial scales. As such the paper is within the scope of the journal. Overall I find that the paper could still be more compact and that the authors should consider if some of the material could be moved to a supple-

ment (e.g. fig 6, 7 and some of the descriptions there of). On the other hand I realize that the journal is on measurement techniques and I readily admit I am more interested in the results and got a little impatient with the time it took me to get there. I have some comments on section 1 to 5, mostly minor.

I have more serious concerns about section 6 and 7. Here I think the authors make a fundamental error in the comparison with the EDGAR emissions data base. The main point being that they should compare emission factors, not inventories. I will address all points in more detail below and in order of appearance in the paper.

Abstract: The last sentence is true but not very relevant, also the statement relies heavily on the other CH4 sources of Valence that are not investigated (If these sources are over or underestimated the 1.5% would change). It should be removed or replaced by a comment on the derived emission factors (see later comments on section 6 and 7)

Section 3 p 2964, l 6 – Why use a conic shape? If you have boxes and you know the waterlevel, the volume can always easily be calculated.

Section 5.1 p2973, l9 "supports the hypothesis of very local emissions" This is a bit odd as you base this conclusion on a comparison with a background station located about 500 km away. There could be lots of reasons why the local concentrations around Lyon could be different than Paris?

Section 5.3 p2974 l 25 Although I understand the need to recalculate everything to daily fluxes this is not correct when looking at erratic events. You measure for 10 min. and then calculate a flux per day. Strictly for such events you can only calculate an average flux per minute (based on a 10 min measurement) but you have no idea if it would lead to such an average flux per day.

P 2975 l 18-19. This is repetition, you say exactly the same on previous page l25.

P 2977 l20-25 Something is wrong here in the numbers. Degassing basin 1.13 kg/day

and clarification basin 0.8 kg/day. That would never lead to the conclusion that the latter could be neglected. Please recalculate, Fig 9 clearly shows the conclusion that clarification basins are not important – so one of the two figures given here is wrong. Please check also if clarification basin is the correct English term – it sounds a bit strange to me, but might be correct.

Section 6.1, last sentence. The biggest uncertainty for more robust WWTP emission estimates is probably that you measure only 4 days and try to derive an representative flux from this short period. Especially when you aim for more robust estimates, some uncertainties matter little. The results show that the chamber methods can show which basins are important but they cannot provide a good overall WWTP estimate as they miss some of the most important sources (as shown by the tracer method). The uncertainty coming from the short campaign period with little information if this is also representative episode for winter, spring etc. should be mentioned here.

Section 6.2 p 2980 l3 onwards – Here we come to a fundamentally wrong way of comparison. To compare the results of this study a comparison should be made to emission factors (EFs) not inventories like EDGAR. EDGAR is a combination of statistical data and EFs on a national scale and then subsequently spatially distributed using certain maps, e.g with location of WWTPs all over the country. The use of EDGAR for checking an individual grid cell is irrelevant. This also applies to the comparison of line 25 onwards. The reader and /or other scientists learn nothing from a conclusion that for a particular pixel a global emission inventory presents a too high figure. In l20 it says "18 higher" – I assume this is about being a factor 18 higher, so the word factor should be added.

What is relevant than? The comparison of emission factors used in inventories like CITEPA, EDGAR, IPCC. This can be done using your data. First in section 2 you provide key data for Valence: "The station is managed by Veolia France and treats the water for 150 000 inhabitant-equivalents, which represents about 2800m3 h-1 with an exiting BOD of 35 kgm-3 . This can be compared with the Table 6.4 of http://www.ipcc-

nggip.iges.or.jp/public/2006gl/pdf/5_Volume5/V5_6_Ch6_Wastewater.pdf Revised 1996 IPCC Guidelines for National Greenhouse Gas Inventories. From this you can derive whether the Valence station is exceptional or average. If you want to make a comparison with EDGAR (although this reviewer thinks it is pointless) than you would have to make a comparison at the activity data (statistics) level and the EF level. Next the IPCC guidelines say "For domestic wastewater, inventory compilers can compare country-specific values for Bo with the IPCC default value (0.25 kg CH4/kg COD or 0.6 kg CH4/kg BOD). When you take your data you can compare with this IPCC default value and comment on a possibly lower Emission factor being derived for a French WWTP [However, I did not do the comparisons because I am uncertain if the units in Section 2 are all correct.] EDGAR most likely uses the IPCC default factor but again I did not check this – You should be able to find this in the EDGAR documentation. It can also be compared with the EFs that CITEPA uses in their national inventory. This is the relevant discussion and may possibly lead to an argument that an representative EF for France should be derived from new measurements. Again the EDGAR pixel being too high or low will be related to choice of emission factors (+ possibly some redistribution errors of calculated emissions on the national scale). Moreover, the Valence WWTP or Valence city emissions of CH4 are irrelevant at the regional, national or larger scale but the possible implication for revising emission factors are relevant at all these scales. Hence, this is what the wider public and community should be informed about.

So, to conclude this reviewer would like to see a comparison with default emission factors as used by IPCC and possibly CITEPA (EDGAR being optional) and some indicative conclusion from this comparison. Of course the short episode of measuring will prohibit directly replacing EFs in guidebooks but at least it could be a recommendation to repeat such measurements, especially at the plant scale (tracer methods) if the gap appears wide between measured EF and default EF values..

Although this change to comparing EFs instead of inventory grid cells is, in my opinion,

critical, I have advised “accepted with minor revisions” because I think the authors can easily make this adjustment. I trust this will be done accordingly.

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