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Atmospheric Measurement Techniques Discussions

Interactive comment on "Measurements of methane emissions from natural gas gathering facilities and processing plants: measurement methods" *by* J. R. Roscioli et al.

G. Allen (Referee)

grant.allen@manchester.ac.uk

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Summary:

This study by Roscioli et al presents a very interesting and rigorous case study of fugitive methane emissions from a natural gas G&P facility. The approach to deriving emissions is to use a dual tracer release system consisting of simultaneous but separate nitrous oxide and acetylene release points. The paper is a very thorough discussion of the pros and cons of positioning the release points for optimal sampling downwind while also considering the nuances of on-site source profiles and point source types.

Tracer releases for flux estimation are a well-established method and they are currently (and increasingly) being used to derive fluxes of greenhouse gases from hotspots such as oil and gas and landfill sites. Mobile approaches are also increasingly common for urban pollution sources and the like. C2H2 and N2O are recent trace gases of choice for this as they are not as environmentally damaging as PFCs and SF6 (the latter of which is now useless as an inert tracer because of over-use). However, N2O suffers from some problems as it does not always have a stable background in certain environments.

I found the discussion of how to treat the overlap (or separation) of the two tracer plumes as a function of distance, while using them also to diagnose uncertainties and on-site differentiation of sources, very interesting in terms of the detail offered. There is a careful discussion of the various emission points on this site specifically and how these might affect the downwind results (e.g. high stacks). This type of consideration would be (and will be) a useful resource and guidance for those considering tracer release systems as flux systems for complex sites such as this.

I have no doubt that the data are of very good quality (I have experience with Aerodyne GHG QCLs with much praise) and have been interpreted to the highest standards by the Aerodyne team themselves. There is clear evidence of attention to detail on calibration etc.

As this paper is a useful technical resource with guidance for others pursuing this very topical area of measurement-led science, it is well suited to AMT. The paper is well-written and well presented (just one typo that I found) and figures are of good quality. I would strongly recommend publication. However, there may be a few suggestions that I think should be considered. I list these below.

Specific Comments:

1/ From the beginning to the end I was looking for some results for the flux (aka emissions). They must have been calculated but they seem to be absent. I can only imagine

this is by design? Section 6 mentions repeatedly that “emissions” are presented in the plots and there is discussion of relative emissions from different source types in the text. However, “emissions” results are actually presented as gas concentrations (often spatio-temporal plots of concentrations along a plume at ground level) and not a mass per unit time. Is the intention that some powder is being kept dry for the companion Mitchell paper submitted to Env. Sci. Tech? If so, then I looked for that paper but it is not yet accessible so I could not see results there. From what I read in this text, it seems that the Mitchell paper will be an attempt to scale up these measurements to the national scale (actually a bottom-up approach but based on local top-down measurement here). The absence of fluxes is an obvious flaw in the current manuscript and any reader would want to see those results here. I strongly recommend those are added here to do this paper the justice it deserves. It is the obvious and necessary place for those results. I see no reason how that would tread on the toes of a paper that tries to scale up, which is very separate. And to try to separate the flux results would be an inefficient way to present the data.

2/ Where was this site? Do you have an anonymity agreement with the site/company? If so, I would accept that it isn’t necessary to name it. But if not, it would be useful to know where the measurements were so readers of the companion paper (when accessible) can assess how representative it is for the purposes of scaling up to a national average.

3/ The discussion of the problems with e.g. PBL convection and topography-induced turbulence as invalidators of the Gaussian plume assumption was interesting. Is there any way you can add more to the discussion of how these effects may influence the results of this case study (or more generally) as guidance for future studies? Is it something that could be folded into an error budget; and if so, how? More generally, what I think you could add here (as it is a technical guidance paper) would be some details of how to construct a robust and conservative error budget for tracer release flux calculation that others could follow. I don’t think this is out of scope of this paper

and it would really add to its impact.

4/ Can you suggest any ways that the tracer system fluxes could be validated against another method? I realise that wasn’t done here but how much need for this do you feel there is and could you suggest any ways to do this, e.g. eddy covariance, Lagrangian mass balancing, airborne measurement etc? A brief concluding discussion on this could be a nice addition.

Technical corrections:

1/ Change “emphases” to “emphasis” in the abstract.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 12357, 2014.