

Atmos. Chem. Phys. Discuss., referee comment RC2
<https://doi.org/10.5194/acp-2020-1256-RC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on acp-2020-1256

Grant Allen (Referee)

Referee comment on "Background conditions for an urban greenhouse gas network in the Washington, DC, and Baltimore metropolitan region" by Anna Karion et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1256-RC2>, 2021

Review of Karion et al., 2021 - Background conditions for an urban greenhouse gas network in the Washington, D.C. and Baltimore metropolitan region

Summary:

The paper is a very thorough examination of various model-and-measurement-based approaches to establishing upwind urban-background mole fractions of methane and carbon dioxide for use in Lagrangian (anthropogenic) flux calculations using urban measurement network measurements. It rightly highlights the significant challenges that such methods are typically (if not always) subject to, which include PBL mixing/dilution, biospheric attenuation, significant problems with inventories/priors, global model backgrounds, and model transport error. Some key conclusions of the paper are that an upwind column method appears to provide optimal backgrounds, but with caveats that summertime presents (expected) challenges concerning temporal PBL development and potential negative biases due to biospheric influences. These conclusions are not at all surprising, but they are very useful to others following this work and attempting to conduct urban GHG flux closure. The results are highly specific to the Washington/Baltimore area but the authors are very upfront about that and rightly suggest that conditions need to be assessed on a case-by-case basis. I would also assume that the uncertainties presented here are highly specific to the design of the measurement network/sampling (and this environment) and cannot be taken to be more broadly representative for other areas – and the paper does point this out.

The paper is generally very well written and well-presented. It represents incremental scientific advancement that is very important to others attempting similar important work. I recommend publication after only small potential modifications and perhaps some thought to the specific comments below which may help increase the impact of the paper for others.

Specific comments:

1/ I think the conclusions section could be clearer than it is. There is no central guidance in the conclusions section on what method(s) – I.e I think the upwind column method? – is/are optimal, and which are highly sub-optimal. Summarising some of what was concluded in the discussions section would be very useful, and arguably more important than discussing the RMSE and bias errors, which are highly specific to this singular environment and case study. Moreover, I wasn't entirely clear after reading the paper whether the UCF might be optimal in all conditions/seasons, or whether other methods may be better under specific conditions?

2/ The paper could offer more guidance on what the authors consider might be an optimal network design in future, especially concerning how to place upwind measurement stations. Given that the central conclusion here is that a measurement-based background is optimal (I hope I've read that correctly?), can you go any further here to talk about whether model-based backgrounds should ever be trusted/useful, and/or whether towers with measurements at more/various heights might aid background, especially considering the biospheric problem where sinks are obviously land-based – for example, could a mix of surface sites and towers go some way to addressing the biospheric problem? It seems to me that upwind surface measurements may be more important than anything else here. Residual layers at higher altitudes are of course also important, but I would imagine that after ventilation from the day before, upwind surface measurements and free tropospheric knowledge may be more important than vertically-resolved measurements all through the background PBL?

3/ Many of the biospheric problems discussed here exist simply because of the way the goal is defined, which is to deconvolve anthropogenic flux components. However... this problem could be turned around if the goal is about understanding "net" urban GHG emissions. Given the growing agenda on Net Zero carbon, urban biospheres are an integral part of the equation/solution. You could argue that understanding net emissions are an important result in and of itself. It might be interesting to discuss the pros and cons of this. The surrounding biosphere creates a problem for establishing representative upwind backgrounds (fully recognised and discussed), but the urban biosphere also creates a problem for deconvolving anthropogenic urban emissions – I don't think the latter problem is recognised or mentioned in the paper, but there are also good reasons why we might want to know what it is in a "net" sense.

4/ In the conclusions section, a suggestion is posed on using "ensembles" of the different methods/models as a proxy for error/uncertainty. I would disagree with this. This would not really be an ensemble, as each method/model is systematically entirely different. Ensembles usually represent variations (e.g. monte carlo simulations or parameter space) of a systematically-consistent approach (e.g. perturbing winds, prior uncertainty space etc in one model/method). You wouldn't really get a statistically-relevant ensemble by comparing apples with pears this way, and it's a very different approach to e.g. comparing outputs of different climate models (which the IPCC would call an ensemble). I'm not even convinced it would give a max/min range of uncertainty that could be useful. I can see why it's attractive to comment on how uncertainty may be better defined but I'm far from convinced that the above would be fit-for-task. I would recommend removing this, or if

not, then to discuss the above caveats or suggest alternative guidance on how to establish uncertainty. But this is just a suggestion.

5/ The uncertainties are presented in concentration space (for background). But nothing is given in terms of how this may manifest as flux error. I guess SNR is a proxy for this to some extent and I guess this paper's scope is on background evaluation, so I don't strongly suggest that flux error should be included, but if there is anything you can say about that, it may be helpful.

Technical corrections: None – thank you.