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Interactive comment on "Background Heterogeneity and Other Uncertainties in Estimating Urban Methane Flux: Results from the Indianapolis Flux (INFLUX) Experiment" *by* Nikolay V. Balashov et al.

Anonymous Referee #2

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Balashov et al., investigate components of an inverse modelling framework and how these may affect estimates of methane emissions. This is a well written paper with clearly described methods and results. The conclusions drawn are reasonable and are highly applicable to those working on city-scale and mesoscale inversions of methane and other tracers.

Methane concentrations are on the rise, and there is still a great deal of uncertainty about where these emissions are coming from. It is important to investigate how sensitive estimates of methane emissions are to the method and assumptions, and investigate which approaches are most reasonable. The INFLUX experiment resulted in a high density of in situ measurements, both tower and flight measurements, which provides a unique platform to test approaches for determining city-level emissions of methane through methods such as inverse modelling. Specifying the spatial domain, the approach to dealing with background concentrations, the periodicity of the measurements and temporal variability in these measurements, and unknown sources of methane missing from the bottom-up approaches are all important considerations in determining city-level emissions.

There are a few sections which may require some clarification, and it may be helpful to the reader if the authors reordered some of the content.

The order of the subsections in section 2 (Methods) does not follow the order that the four components of the inverse modelling framework are listed in the abstract or discussion section. It may make it easier for the reader to follow from the methods through to the results if there is an explicit methods section for each of the four sensitivity analyses.

Section 2.4 lines 172 to 185: Could the authors explain what the percentages are referring to and provide the overall quantity? Does this come from an inventory analysis?

The section following, starting at line 187, is also labelled as Section 2.4.

Section 2.5 line 218: Should this be a subsection of the previous background section (2.4)? Or should this subsection be called Variability in Background Concentration?

Equation 3 line 250: This equation and notation are taken from Hanna et al. 1982. It may be useful to the reader to explain the terms more thoroughly. Most of the explanation for terms is taken from Chapter 9 of this text, but some of the terms are explained more fully in earlier chapters. I was particularly distracted by the zi until I realised that it was conventional for the zi to denote the boundary layer height of the box, rather than i as an indicator for height at time step i (or distance i). Do you take zi to be the average

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boundary layer height during the hour?

In the text the units of Qa are described to be in mass per unit time per unit area, and the units of C are described to be in mole fraction in the first instance. Should the concentration not be converted to moles per volume? Later on from line 263 this does appear to be the case.

Section 3.1 lines 292 to 307: This information seems to be related to methods, and should perhaps be in Section 2, probably before the methods section on Background concentrations to be consistent with the abstract and discussion sections.

Line 350: I think it would be useful to justify why you needed to use the bootstrapping approach.

Lines 350 to 363 and Figure 6: I would propose that the analysis for assessing the number of measurements required to obtain a reliable background concentration estimate should focus on the standard deviation rather than on the standard error.

It is unsurprising that the standard error (standard deviation of the mean) reduces as the sample size is increased, as you state in line 359. This is a property of the standard error. If you're interested in the stability of the background concentration difference estimate, you should rather be looking at the standard deviation of the concentration differences (which you can do in the same way, except instead of looking at the standard deviation of the mean values for the 5000 samples, you look at the mean of the variances for the 5000 samples – which can then be converted to a standard deviation). The standard deviation will provide information about the uncertainty in the background concentration. The plots in Figure 6 are slightly different for each wind direction because the standard deviation of the background concentration from these directions differs and because the bias for each direction differs. At some optimum sample size the standard deviation of the concentration differences will stabilize for each direction. If you assume that under normal circumstances (where you don't have two competing domains or multiple background sites) you would be able to obtain the uncertainty in the background concentration from historical data and this uncertainty remains stable over time, you would want to ensure that the background concentration estimate is obtained from a sufficiently large sample size so that you know what the uncertainty in this estimate is. Therefore, if you have multiple background sites where you can assess at which sample size the standard deviation of differences between background sites stabilizes, you may want to determine for each wind direction what this sample size is and what the stabilized standard deviation is. Basing the background concentration on a sample of this size or larger should provide an estimate with a predictable uncertainty, which is now independent of the sample size. If you know what the standard deviation is, then it follows what sample size is required to obtain a background concentration with the required standard error (precision) (if you can use SE = SD/sqrt(n)).

If you decide to stick with the standard error plots, I think you should show on each plot at which sample size each direction reaches the required precision, say 3 ppb, as this differs for each wind direction.

A point that should be discussed is that the measurements you obtain for the background site are taken at different times, and as the number of measurements increases, so too does the averaging period, which changes the interpretation of this average. There's a danger that if the averaging period is too long, the background concentration measurements may be representative of different synoptic periods.

Line 453-454: "Background random error is a function of sample size and decreases as a number of independent samples increase". As mentioned earlier, I don't think that this is the interesting part of the temporal variability analysis and is already a property of standard error. I think it would be more interesting to discuss how different the standard deviation in the background concentration differences are between wind directions. It would be interesting to know if the sample size at which the standard deviation of the background concentrations stabilizes is similar for all directions. This would be a helpful number if it can be assumed to be generalizable. Basing the minimum sample size on the standard error is less generalizable as the required precision may differ, and the variability in background concentrations would differ between regions.

Figure 9: This caption should be expanded in order to make each figure stand-alone.

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