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Comment on acp-2021-982

Anonymous Referee #1

Referee comment on "Eddy covariance measurements highlight sources of nitrogen oxide emissions missing from inventories for central London" by Will S. Drysdale et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-982-RC1, 2022

The study by Drysdale et al. investigates NOx eddy covariance flux measurements in context of emission inventories in London. It presents a follow up study on previous campaigns at the same site. A main finding seems to be that NOx emissions are still underestimated by local emission inventories in London. The paper is well written and meets ACP quality criteria for research papers. I suggest publication after the comments below have been addressed adequately.

Line 85: Air was pumped through a 45 m sampling line. Obviously flowrates varied between 2.8 and 26 l/min due to clogged filters. What was the variation of delay time calculated by EddyRe through this line? The dependence of NOx flux vs Reynolds Number suggests a bias up to 50% for the first half of the measurement period. I understand that this would make the discrepancy between measured fluxes and inventory even larger (thus not change the major conclusion of the paper), but I wonder whether this large bias justifies the inclusion of the early campaign data without correction.

Line 100: The EC analysis is done with eddy4R aggregating the flux calculation into 60 min intervals. This is a non-standard flux averaging interval because stationarity can become an issue for longer time averaging intervals. It would be good to present a statistical measure that justifies a 60 min flux calculation interval. Why not simply apply spectral corrections as described by Massman et al. 2002 (doi: 10.1016/S0168-1923(02)00105-3) The cospectral analysis could give a good metric on flux averaging intervals - e.g. how long is long enough?

Line 149: Figure 7 is mentioned before Figure 3,4,5, and 6. It also appears that Figure 5 appears before figure 4 - Figure numbering should be consistent – i.e. in sequential order.

Line 135: This correction formula merits more discussion. For example what was the assumption for the concentration jump of NOx across the PBL?

Line 250: The temporal disaggregation methodology of the yearly emission inventories is not clear. Why would you still scale the LAEI hourly emission data by week and month?

Line 253: It is not clear what the open R package is really used for. The 2D flux footprint should already give you an appropriate weighing function that can be applied to bottom up fluxes. Using the along wind distance to the footprint maximum in conjunction with a polarPlot function seems an unnecessary (and rather semiquantitative) step here.

Section 3.1: NOx fluxes are reported in mg/m2/h – in my opinion molar units would be much more appropriate, since NOx is the sum of two species with different molecular weight. Reporting fluxes in mg/m2/h leads to an important loss of information. This is particularly relevant since most of NOx is emitted as NO from combustion processes. I therefore highly recommend to change from mass to molar units as is done for mixing ratios, which are all reported as ppbv and not ug/m3.

Line 273: It would be informative if the authors expanded their discussion here, comparing their results to NOx flux measurements elsewhere and previous studies at the location. Are NOx fluxes in London quite a bit higher or lower than in other urban areas? e.g. Marr et al., 2013: doi: 10.1021/es303150y; Karl et al., 2017: doi: 10.1038/s41598-017-02699-9; Guidolotti et al., 2016: doi: 10.1016/j.agrformet.2016.11.004; Squires et al., 2020: doi: 10.5194/acp-20-8737-2020; How similar or different are the results to Lee et al.,2015 (doi: 10.1021/es5049072), who published NOx fluxes at the same location? Have fluxes changed since then or rather stayed constant?

Line 280: what is meant be temporal upscaling?