

Interactive
Comment

Interactive comment on “High-frequency urban measurements of hydrogen and carbon monoxide in the UK” by A. Grant et al.

Anonymous Referee #3

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The manuscript "High-frequency urban measurements of hydrogen and carbon monoxide in the UK" by A. Grant et al. presents a three months time series of H₂ and CO mixing ratios measured in an urban environment. The manuscript focuses on determining the H₂/CO emission ratio (from traffic) and on investigating the soil sinks of both traces gases. Although the paper tackles questions relevant for a better understanding of the continental hydrogen and carbon monoxide cycles, I cannot recommend publication in ACP because the manuscript is seriously flawed and presents findings not evidently supported by the data. My most important criticism is that the authors ignore fundamental atmospheric transport processes which are most essential for a correct quantitative interpretation of atmospheric trace gas variations, and in particular for estimating fluxes of these gases.

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Below I will give a few examples substantiating my criticism:

(a) When discussing the observed diurnal features, the authors neglect the influence of atmospheric mixing. This leads to the doubtful speculation that night-life traffic on Fridays (but not on Saturdays !?) is a huge source for H₂ and CO or that school transport is a substantial source in the urban H₂ and CO budget (p.1172, l.3-13) without backing up their assumptions by independent datasets (e.g. traffic counts).

(b) One key aspect of this manuscript is the investigation of the urban H₂/CO concentration ratio. However, the most unusual feature in the data, two distinct regimes of H₂/CO ratios, is hardly discussed at all. Not enough effort is made to elucidate this finding; instead they focus on aviation emissions which do not significantly differ from the well understood H₂/CO ratio of combustion processes. Moreover, the discussion of the transport emission ratio lacks any statement (or estimate) concerning the influence of the H₂ and CO soil sink (the latter being further down estimated as considerably larger than found in other studies). In fact, several recent publications (Aalto et al. 2009, Hammer et al. 2009, Yver et al. 2009) have pointed out that the influence of the regional soil sink must not be neglected when estimating reliable H₂/CO emission ratios.

(c) When attempting to estimate the soil sink, the authors again attribute the evaluated concentration changes (decrease during the night) to the sinks only, i.e. by evaluating the evening CO and H₂ decrease during times when CO is still largely perturbed by traffic emissions during the evening rush-hour. The decrease of the rush-hour peak by atmospheric dilution is completely neglected and this decrease instead is solely attributed to the soil sink. A sensitivity study to investigate the effect of the time window used to calculate the CO sink strength would be the appropriate way to go.

Besides these scientific omissions/errors, the manuscript is also flawed concerning technical details, and often lacks adequate scientific diligence. Handling uncertainties is only one example, i.e. the authors do not report any uncertainties of their key findings

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(e.g. mean soil sinks) nor do they show any error bars in the figures. Moreover, at those rare occasions when error propagation is performed, the results are more than doubtful and not replicable (e.g. p1176 I9-12). Basic information on the numbers calculated is not given (e.g. for regressions). The lack of care is apparent by the disagreement between values stated in the text and the figures (e. g. p.1172; l. 17-18). Copying whole sections from other papers (e.g. analytical methods) should be carried out with great care, so that Mace Head is not placed to California and the instrument does not suddenly change from a PP1 to a RGA3 within two sections (p.1172 l.12-17).

Recommendation:

If submission of a completely re-written manuscript were envisaged, I would strongly urge the authors to provide a thorough investigation of the different processes that affect concentration changes, refrain from any speculations, and do not leave the reader with more questions than answers. A more thorough pre-review of the manuscript would be helpful in this case.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 1167, 2010.

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