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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Decoupling of urban CO_2 and air pollutant emission reductions during the European SARS-CoV2 lockdown" *by* Christian Lamprecht et al.

Anonymous Referee #1

Received and published: 25 November 2020

This paper presents an analysis of NOx, CO2 and NMHC data from 2020 in Innsbruck, Austria, including during the lockdown introduced to mitigate the spread of the SARTS-CoV2 virus. It uses direct flux measurements of the pollutants and comparison to a business as usual model (calculated by boosted regression tree analysis) to provide information on the split of sources of NOx and CO2 between traffic and residential / commercial energy emissions. The major finding is that, at least in the Austrian emission inventory, emissions of NOx in the residential, commercial and public sector are overestimated (21% estimation compared to 2% from this study). This result has significant implications for air pollution abatement policy, with the emphasis needing to be mainly on the transport sector for NOx emissions.

This is a very nice study, well written and concise and will be of great interest to the air pollution science and policy community. I recommend its publication in ACOP subject to the authors answering the following minor comments.

General:

The methods section is very short. I realise that the instrumental techniques are largely described elsewhere but I feel the readers of this ACP paper would benefit from a more enhanced description. Two things really stand out as missing. Firstly there is no mention of how NO2 is converted to NO for measurement in the 2 channel chemiluminescence instrument. There should be at least a brief description of the type of converter and discussion of any potential interferences. Secondly, there is no discussion of the uncertainty of the NOx, CO2 or NMHC measurement. Please could the authors add a short discussion on this?

There is also very little discussion of the eddy covariance flux methodology. Again I realise this is discussed at length in other publications, however I feel there are certain things specific to this study that should be described. For example, what filtering methods were used, how much of the data passed the filtering, did this cause any bias, were any corrections made for flux storage? Also, some mention should be made of the uncertainty of the calculated fluxes and how this carries through to the final results.

On line 267 it is stated that average traffic loads in Innsbruck decreased by 60%. Could the authors provide more detail on this number? For example how was it measured? Is there any information on the change of fleet composition?

From 301, there is a discussion on the changes in time spent in residential (20% increase) and commercial / public sector (30% decrease) buildings. Could the authors expand this discussion to take into account how each of these sectors is heated. My thought would be that the residential sector is largely heated by biomass burning (either natural gas or solid fuels), whereas the commercial and public buildings are largely heated by electricity. Is this correct for Innsbruck and if so how does this split affect the

findings.

Minor:

Line 103: The authors could also add Squires et al., ACP, 2020 which reports NOx and CO fluxes from an urban site in Beijing, China.

Line 88: probably should be a new paragraph.

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