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Comment on acp-2022-337

Anonymous Referee #1

Referee comment on "Impact of urbanization on gas-phase pollutant concentrations: a regional-scale, model-based analysis of the contributing factors" by Peter Huszar et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-337-RC1>, 2022

The authors investigate the impacts of some key assumptions about representation of urban anthropogenic emissions, dry dep and biogenic emissions, and meteorology from the urban canopy on the fine scale representation of ozone, SO₂, and NO₂ across a part of Europe with a lot of cities using an uncoupled modeling frameworks employing offline meteorology from two different regional climate models. They look at winter and summer averages for SO₂ and NO₂, and summer MDA8 ozone.

This is a useful contribution to the peer reviewed literature, but the framing of the paper and its implications need work to get the paper to be publication ready.

Major issues.

The authors sometimes overly generalize (& in some cases w/ excessively strong statements).

For example,

- I find that the use of the term 'the urban canopy meteorological forcing' is too general. It's hard to imagine how the authors can say that the impact of the urban canopy meteorological forcing *always* decreases primary pollutants but increases ozone. To me, this is equivalent to saying: 'meteorology always increases ozone'.
- The statements "these are the two major drivers of urban air pollution", "the two minor contributors can be neglected" & "it is clear that the main driver affecting..." are quite strong... I don't believe they should be this strong, based on the evidence (or lack thereof) presented by this work. The authors are testing very specific assumptions about different processes.
- The discussion of local versus rural/other cities' influences on air pollution in the Intro seems limited. Some pollutants' distributions and production are very regional scale, so separating sources into local vs. rural doesn't make much sense. Additionally, it seems strong to say that 'air pollution in cities is mainly determined by local sources'... for ozone, background levels in cities should be an important fraction of the total ozone.

There are scattered introductions to urban air pollution and ozone chemistry throughout the manuscript. It would be more helpful if this was concentrated in the Intro.

With respect to the introduction of the simulations in the Intro, it is not clear what the authors are investigating here. Are the authors using a global model? What is the resolution of the model? What is the base simulation? Are the authors assuming a preindustrial like state? Because this is a model study, it seems like this all should be very clear in the Intro.

[Later in the methods, the authors say: "in the first experiment where urban emissions are disregarded, we removed urban emissions only for the 19 cities chosen for the analysis". I think my fundamental question is: how are the authors defining what emissions are "urban"? what exactly are they investigating with this first simulation? This should be clear in the Intro, as well as the methods.]

How is the land use input required for the regional climate models similar/different from the input to the dry dep and biogenic emissions schemes? Seems like the authors need to emphasize that in some of their sensitivity simulations, the deposition/biogenic emissions are forced by meteorology that is decoupled from the land use type impacting the deposition/biogenic emissions.

The approach used by the authors to separate the roles of urban emissions, meteorology, dry dep, and BVOCs (detailed at the end of page 8) is full of assumptions. The effects are not additive; the authors probably should not assume equation (2), although generally looking at the difference between simulations to gauge sensitivity is fine. The authors need to adjust the framing of the work on this front (e.g., "individual components" – I wouldn't characterize as individual components when the components are tightly coupled in real life, and should be in the model).

For ozone, all the cities have very similar observed annual cycles for urban areas, and the model captures them. I'm not sure this is the best way to evaluate ozone for the purposes of this paper. The annual cycle is likely largely driven by regional scale phenomena rather than urban processes. It would be good to evaluate whether the model captures urban-suburban-rural gradients in ozone.

Also, I wouldn't call an uncoupled modeling framework a novel approach.

Minor issues.

There is a fair amount of grammar issues that a closer read or English editing could help (I didn't list these).

Other minor comments.

Intro.

- What do the authors mean an 'artificial' surface?
- Ref for 60% of the population will live in urban areas
- The authors shouldn't start a sentence with 'E.g.'
- Sure, plants are a large sink of many gases, but that doesn't mean other surfaces can't be either. What's known about dry deposition to urban surfaces? Do we as a community know what deposition velocities should be over urban surfaces?
- Again, yes, plants are a large source of BVOCs, but that doesn't mean that the plants that we do have in the cities aren't very efficient producers of BVOCs, or there aren't emissions of BVOCs from consumer products. (In the past couple of years, there has been evidence of both. To be clear, I'm not asking the authors to investigate efficient emissions of BVOCs from urban vegetation or BVOCs from consumer products, rather contextualize and motivate their study accurately/well).
- Use of 'background' as defined as 'without urbanization' is not great, given there is clear definition of 'background ozone' (<https://doi.org/10.1525/elementa.309>)

Methods

- The authors are taking grid-cell average meteorology and feeding the CTM with this? Needs to be super clear.
- What is the point of discussing CLMU in CLM? Is CLM coupled to RegCM4.7? Also, what do the authors mean by 'the traditional urban geometry approach is implemented'?

- It seems like it should be emphasized that the authors are looking at not only two different regional climate models, but additionally two different urban canopy parameterizations.
- Do S&P 1998 and Zhang 2003 give wet deposition parameterizations, or just dry dep? Also, it seems like the authors should elaborate on how urban dry dep is simulated in CAMx and its key sensitivities.
- How do the authors know that the impact of soil NO_x is small?
- What does 'showed that their long-term effect is rather small' mean?
- Clarify that the nested domains are only for one of the regional climate models
- What does 'they found a rather small impact' mean (end of first paragraph of 2.2)?
- What does 'brings some accounting for the uncertainty related to the urban land-cover representation' mean?
- How do they authors grid the irregularly shaped Czech emissions to the model? Is this where FUME comes in? If so, I'm confused why the descriptions are in separate paragraphs.
- I'm confused when the authors are discussing the "city" vs. "non-city" portions of the cross-boundary shapes. Is there a sub-grid distribution of emissions?
- In replacing the urban land with the rural crop land over the entire domain (not just the cities chosen), the authors may be changing background levels of air pollutants, not just local urban levels.

Results

- What's the first difference? I only see a reference to the second difference in the validation section. Also, if a different chemical mechanism is used, then that seems like a big change.
- I don't know what the authors mean by "while all urban and suburban background stations were used from a subset of the analyzed cities". Why a subset? What does background mean here? Which sites were chosen for the evaluation & why? Needs to be clear.
- It seems like there should be some text about the diel cycle scaling of the urban emissions in the methods.

Discussion and conclusion

- Word choice -- the authors selected four contributors based on previous work to investigate, they did not identify them based on their work ...
- A lot of the discussion, especially with respect to model evaluation, is framed as a

comparison to the authors' previous work. This is not too helpful for the reader who is not familiar with that work. Can the authors focus on what knowledge is generated from this work first, and then compare (briefly) to previous work by the same author? In other words, the comparison should be secondary, not primary.

- I'm not sure how the authors can go from "in the case of SO₂, the model is rather unable to correctly resolve the annual cycle of near-surface concentrations" to "in summary, we did not identify substantial model biases" within a couple of sentences...
- I recommend cutting the "hints that the effect of urban emissions is well captured" part
- Why is there stronger dry dep due to higher temperature? If there are meteorological sensitivities to the biogenic emissions and dry dep parameterizations, then they should be spelled out in the descriptions of the models.
- If the piece about increasing in ozone due to suppressed dry dep is not shown in this manuscript, discussion is not merited in the intro. Too speculative.

Table 1.

- I think it would be clearer to say 'crop' rather than 'Nourban'

Table 2.

- For parallel structure, do Landuse (deposition) vs. Landuse (BVOC) as the column names.
- It's confusing to have 'nourban vs urban' for meteorology, and then 'rural vs. urban' for land use. Do the authors want to have different descriptors here? If so, please spell out why in the table footnotes and/or text.

Figure 1.

- Can this map include the model grid? (Also, are the two model grids the same?)

- Make city symbols larger.
- What does the [m] in the title mean?
- Why show terrain rather than land use type?

Figure 2

- Are the observations from the same year as the model? Please specify
- Can the authors put error bars on the observations to represent variability across stations examined?
- Why is there only one model line on here? Shouldn't there be two (the CAMx simulations with different meteorology)?
- I get that some authors like to convert the model values to whatever units the observations are in for model evaluation, but the inconsistency between the units of this plot and the ppbv used elsewhere makes contextualizing the model's limitations challenging.

Figure 3

- The error bars show the maximum and minimum across grid cells, or across days?

Figures 4,5,6 and 8

- Can the Prague panel be the first panel, and then the larger domain ones be the second and third panels? This would make it easier to compare the two larger domain panels...

Figure 10

- Are these changes, or absolute? I think the former because there are some negatives. These changes look huge, but I don't know what the baseline is. Maybe just show the individual simulations (rather than the changes)? This goes back to my saying that you can't really consider the differences in the simulations as additive.

Figure 11

- It might be better to show both DV for urban vs. crop, because it's hard to contextualize the impact without knowing the absolute values (& many readers may be unfamiliar with the magnitude of DV).
- Please convert to mm/s which is more widely used for DV than mm/h & agrees with the units used on the other plot on which you have DV.