Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2020-1303-RC1, 2021

Comment on acp-2020-1303

Anonymous Referee #1

Referee comment on "The MAPM (Mapping Air Pollution eMissions) method for inferring particulate matter emissions maps at city scale from in situ concentration measurements: description and demonstration of capability" by Brian Nathan et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1303-RC1, 2021

This paper aims to both demonstrate the feasibility of an aerosol sensor network - inversion system and to use that system to quantify aerosol emissions in Christchurch during winter. The paper is interesting and relevant to the ACP readership. I recommend publication after attention to the issues outlined below.

Major comments:

- The description of construction of the background aerosol is difficult to follow. I recommend it be moved to the supplement so as not to disrupt the flow of the paper and expanded with additional figures that would clarify the approach.
- The authors might be saying that the only important aerosol emissions are from wood burning and that this source is the entirety of their prior emission inventory. If this is the case, a clearer statement would help. Also, I found it odd that the prior is unrelated to temperature as I'd expect that heating would be needed more on cold than on warm days (relatively speaking).
- Since the inverse is a spatial map, it would help to provide a spatial map of the prior.
- An analysis of the temperature dependence in posterior emissions would help to indicate that the inverse has produced a reasonable result.
- I suggest that the scope of this paper be narrowed to an explanation of the inversion system and analysis of inversion results. The work concerning OSSEs, while interesting, should either be placed in supplemental information or expanded upon and turned into a separate manuscript. As written it distracts from the paper and is not well developed.

Minor Comments:

While I have many comments, the authors should adopt those they think will improve the paper without excessive additional effort.

- Introduction - Introduction should be more succinct. Since Christchurch is not a megacity, discussion of megacities not irrelevant. Judgements about the correctness of epidemiological studies unnecessary.
- Introduction - The MAPM network is in many ways more similar to BEACO2N than INFLUX. Should cite Turner (2016 and 2020) in addition to Lauvaux.
- Section 2 - More information (in a table) on instrument capability would be helpful. (Instrument precision, size distribution cutoffs at the high and low end, time resolution of data). Comment on whether it matters that the sensors you are using are missing the low end (less than 0.3 microns) of the aerosol size distribution and how much you think this impacts your source inversion.
- Section 2 - Some recent studies have shown that a significant fraction (½) of urban aerosol enhancement can be secondary in nature. Discuss in more detail why these are not relevant (if they are not) to winter in Christchurch.
- Section 3.1 - Figure 2 schematic is disconnected and confusing. Modify to show connections between FLEX/PART - WRF, Prior, and Enhancements and how these are fed into the system to generate posterior.
- Section 3.1 - Provide a brief description of WRF settings, model-measurement meteorological mismatch, etc in the main text and move the bulk of this to a supplement.
- Section 3.1 - 185-214 does not belong in the methods section. Condense and move to introduction, or delete and use as part of a separate paper that includes OSSE experiments.
- Section 3.1 - It would be nice to see overlay of footprints with sensors in Fig 1 or elsewhere.
- Section 3.1 - Put Figure 3 in a supplement. Important information, but not critical to your story arc.
- Section 3.1 - Put Figure 4 in a supplement. Important information, but not critical to your story arc.
- Section 3.2 - 235 - Provide sources for percentages in this paragraph.
- Section 3.2 - 248 - Where does this 75% come from? Why is it reasonable?
- Section 3.3 - Show time series of background against other measurements to persuade the reader that background method makes sense.
- Section 3.3 - The final step, describing the 10 day median is confusing and could use justification. If PM enhancements are synoptic in nature, this step does not make sense.
- Section 3.3 - Put wind direction thresholds in a table, rather than directly in text.
- Section 3.4 - Add a sentence justifying choice of length scale.
- Section 4 - Remove this section from this paper and put in supplement or expand and write a second paper.
- Section 4.1 - Since error is introduced into the real inversion via choice of background value, it seems you are under-representing error by excluding background from OSSE. I suggest finding a way to compensate for this.

- Section 4.1 - Explain error introduced into the "true" time series more clearly. Does 10%/10% mean that for a "true" reading of 25 micrograms/m^3, we are sampling from a normal distribution of sigma 2.5 micrograms/m^3? Does 90%/90% mean that for the same "true" value, we are sampling from a normal distribution with mean 25 and sigma 22.5 micrograms/m^3? I am unclear from the text.
- Section 4.1 - The tradeoff between a large number of high-precision and a small number of low-precision instruments in this type of system is important. However, if my interpretation of 90%/90% is correct, the experiment set up here contrasts relatively good sensors with unrealistically poor sensors. As a result, I do not believe your set-up answers the question in a realistic way. The error you choose in this experiment should be reflective of real sensors (ideally those used in the MAPM project). If you believe the error you have chosen is actually reflective of these sensors, please document this in the text. See also Turner et al 2016.
- Section 4.2 - This is an interesting idea that is worth pursuing, but the results presented here are not adequate for publication. I wonder whether adapting prior based on a longer time average of posteriors would be a better idea.
- Section 5 - Figure 9 - Based on Lauvaux (2020), we expect time series (individual hours/ days) of posterior to show random error, but posterior averaged over a longer time period to be more meaningful. We see this in your timeseries, making it difficult to interpret. Perhaps add a median diel cycle of posterior with error bars to make it easier to see how posterior compares to prior on average.
- Section 5 - 506 - Earlier, you said 75%? Now 70%. Are these different numbers? Not clear from the text.
- Section 5 - Comment briefly on whether you think the whole difference between prior and posterior is due to home heating or whether other sources might play a role. Comment on whether you see temperature dependence in the posterior.
- Section 5 - 506-512 - References needed.
- Section 5 - Figure 11 - Overlay of footprint would be helpful in thinking about the exterior ring.
- Section 5 - 555 - Comment: What is actually in the space that shows higher emissions in posterior than in prior? Is it actually possible that there are unaccounted emissions of the magnitude shown: Are their homes or is it empty space? What are the values in that area in the prior?
- Section 5 - 564 - You comment on the possibility that the MAPM network is picking up hyper-local sources due to height. Use the MAPM time-series data to make an argument one way or another. Do the sensors detect signals propagating through the network? Do enhancements over background at various sites correlate or are they totally unrelated?
- Section 5 - Section 5 - Figure 10 - Time series is too squished. Expand figure horizontally or show us scatter instead.
- Section 6 - Omit discussion of OSSEs.

References

A.J. Turner, J. Kim, H. Fitzmaurice, C. Newman, K. Worthington, K. Chan, P.J. Wooldridge, P. Köhler, C. Frankenberg and R.C. Cohen, Observed impacts of COVID-19 on urban CO2 emissions, Geophys. Res. Lett., https://doi.org/10.1029/2020GL090037, 2020.

A.J. Turner, A.A. Shusterman, B.C. McDonald, V. Teige, R.A. Harley and R.C. Cohen, Network design for quantifying urban CO2 emissions: Assessing tradeoffs between precision and network density Atmos. Chem. Phys, 16, 13465-13475, doi:10.5194/acp-16-13465-2016, 2016.