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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Mid-21st century air quality at the urban scale under the influence of changed climate and emissions: case studies for Paris and Stockholm" *by* K. Markakis et al.

Anonymous Referee #1

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General Comments:

The authors have conducted a relevant and valuable case study highlighting the need for high-resolution emission inventories and high-resolution modeling capabilities when examining air quality (both ozone and PM) in and around urban centers. The focus on two major European cities, Paris and Stockholm, demonstrates the complexity in a multi-scale modeling approach and convincingly highlights problems that may result if urban air quality policies are based solely on coarse-scale modeling efforts.

This manuscript is largely ready for publication in ACP. Below I point out some minor,

but largely answerable, questions that I feel the authors need to address prior to publication, and then more minor/technical questions and modifications that I feel need to be made to the manuscript. There are many sources of data incorporated into this paper, each with their own set of citations and acronyms. At times this can be overwhelming, and I have made some suggestions below to help improve the clarity and readability of the manuscript.

Specific Comments:

Page 27046, Line 15 The authors state that "a different chain of models was implemented for each case study," which in itself is fine. I feel, however, that the authors need to spend some more time exploring the potential uncertainties that may result from such a choice. Primarily, what would happen if a different chain of models were used instead? To some degree the selection of models is a subjective choice, and it is critical to consider potential differences in the results if a different set of choices were made. This manuscript does a good job demonstrating that choices regarding resolution, emission inventories, and meteorology impact the model results, but I feel that Section 2.1 could be expanded to address this, as well as perhaps an expanded discussion regarding potential implications in Section 6.

Page 27047, Lines 15-16 The authors state that the "signal of emission mitigation alone can be subsequently derived from the concertation [sic] difference between the two aforementioned runs." Was this linearity simply assumed, or did the authors perform some sort of non-linearity check? I realize this is common practice when forecasting air quality into the future, but I still feel that at the very least this should be verified. If differences are indeed minor, which I expect, then the authors should mention this. If, however, there are major differences, the authors need to explore these and interpret their results in light of these differences.

Page 27051, Lines 21-24 Can we assume that the urban-scale changes in NOx, NMVOCs, and PM between 2030 and 2050 match the European scale emissions?

Where do the percentage differences in this paragraph come from? There needs to be a citation. I realize these are small differences but what are the potential implications of assumption that emissions are constant between 2030 – 2050?

Page 27052, Lines 23-26 The authors explore one possibly way in which compensation among model errors is occurring, but I do not feel that this possibility is explored sufficiently. I am not (nor will many readers) be familiar with there previous work (e.g. Magaritis et al., 2014) so the authors need to offer some more evidence for why they are confident that some of the model results that match observations are not due to some compensation of model errors. In addition, in Figure 3c, why does the annual average look to perfectly match the observations when the summer average underestimate compared to the observations? I would think that the annual average would be biased low as somewhere between the winter and summer bias? Unless the spring or autumn biases are high?

Page 27058, Lines 22-25 I don't see enough evidence that the Paris and Stockholm examples suggest that the SOMO35 metric may be misleading. I assume that the authors have good reasons for stating this, and believe these reasons should be included in this section. I'm not sure the two paragraphs examining SOMO35 adds to the paper, and think it should either be removed or expanded.

Page 27059, Section 5.5 These are interesting results, but I feel like one paragraph isn't sufficient to describe what's going on. This could be expanded.

Finally, throughout the manuscript acronyms needsto be expanded. I realize there are many in this paper, but it would be helpful for readers not familiar with the various models and inventories to see their expanded titles in addition to their acronym and appropriate citation. Some are expanded (e.g. WRF, PREV-AIR) while many others are not (e.g. IPSL-CM5A-MR, CORDEx, AIRPARIF, EC_EARTH, LMDZ-OR-INCA, ARTEMIS, CHMERE, MATCH, SMHI, MELCHIOR, ISORPOPIA)

I feel that either a summary table in the document, or perhaps in the supplement is

necessary to help readers navigate through the wide variety of abbreviations.

Technical Comments/Corrections:

Page 27044, Line 16 Expand "yr" to "year" here and elsewhere in manuscript.

Page 27046, Line 5 Expand "ca"

Page 27046, Line 7 Define m.a.s.l.

Page 27048, 19-21 The abbreviation MT is unneeded as it is only used here. Just use monoterpenes

Page 27050, Line 24 Please provide some citation for Euro VI. Non-European readers are probably not familiar with this.

Page 27052, Line 3 Why is there no suburban or regional comparisons for Stockholm? Is one urban site sufficient to understand what's happening in the city?

Page 27052, Line 13 Is the Paris bias not shown? Isn't that what the REF_urban shows?

Page 27054, Section 4, 5.1, and 5.2 It's feels a little disorienting to flip from Figure 4 in Section 4, then to Table 3 in Section 5.1, then back to Figure 4 and 5 in Section 5.2. Consider starting with the climate projections/met data (Section 4), then proceeding to the results with Figures 4 and 5.

Page 27056, Line 2 Please indicate what are shown or plotted and which are not, and please be specific. For example, Table 4 only has MD8hr ozone. What is shown in Figures 4 and 5? Daily? Or MD8hr ozone?

Page 27057, Lines 7-9 This is an interesting result. Does anyone else show this for IdF?

Page 27059, Lines 6-9 Why do you show MD8hr for NOy? I haven't seen that particular metric used before. Throughout the manuscript the term "MD8hr" is used to mean

“MD8hr ozone” so the sudden switch to MD8hr NOy can lead to confusion. To improve clarity, please address. For instance, does Figure 6 plot MD8hr ozone and MD8hr NOy? Or daily? What about for the ratios?

Please be careful and specific with these. This analysis is interesting and useful, and others may make their own plots for comparison. You need to be very specific so that others can reproduce this analysis. For example, Page 27063, Lines 16-20 are very clear.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 27041, 2015.