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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.

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We would like to thank the reviewer for the improvement brought upon the manuscript from the insightful comments of this review.

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.

Reply: The authors believe that the fact that the two test cases implement different model chains is overemphasized in the manuscript therefore we find it appropriate to revise the phrasing “a different chain of models was used for the two case studies” as well as a possible misleading phrase “therefore particularly interesting to compare” in page 27044, line 28. A careful discussion of the implications due to that particular choice would be relevant in a cross-city comparison. This is not the case here as results are presented separately for the two cities and at the final stage (section 5.5) the cities are linked but still not compared; they are used as illustrative examples (Stockholm is dominated by regional pollution while IdF exhibits less regional influence) to discuss possible policy misclassification issues. It is commonplace that biases are indeed model-specific but without an intercomparison experiment or ensemble modelling within each case study more conclusions cannot be drawn. These types of experiments are not within the scope of this study as in any other single case, single model study. We add “We should note that the range of uncertainty in the results presented here is probably underestimated due to the choice of a single model chain for each case study.”

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,

ACPD
15, C11341–C11348, 2016

however, there are major differences, the authors need to explore these and interpret their results in light of these differences.

Reply: Indeed, the simulations on both domains were performed based on the standard practice and similarly to the reviewer we expect small differences. We have performed a linearity check for the Stockholm simulations and confirmed the linear relationship. We added in parenthesis “the linearity of this relationship was confirmed for the Stockholm simulations and assumed for the IdF simulations”.

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?

Reply: We have removed the percentages from the manuscript and revised that part. We simply state that this assumption for the local scale emissions is in-line with the European perspective. In the previous section it is clearly stated that the mid-21st century ECLIPSE CLE scenario assumes full enforcement of all legislated control technologies until 2030 and no climate policy thereafter. Never the less in section 5.3 (Local air quality at 2050 due to emission reductions) we add a paragraph that addresses the issue of potential implications of that assumption: “We have assumed unchanged local-scale emissions for the 2030-2050 period. Never the less, the projected concentration change in the Stockholm region is mostly affected by regional emission mitigation that according to the CLE emission scenario is weak. Therefore, further mitigation of local scale emissions would not strongly affect the future concentration change in the Stockholm domain. In contrast additional emission mitigation in the IdF scale would result in further improvement of domain-wide ozone and PM2.5-related air-quality at the mid-21st century horizon. However, due to highly non-linear ozone chemistry over Paris it is difficult to make firm assumptions on the nature of ozone projected changes under additional mitigation of ozone precursor emissions in the 2030-2050 period.”

ACPD
15, C11341–C11348, 2016

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 their previous work (e.g. Megaritis 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?

Reply: We have expanded the discussion in the corresponding paragraph to provide more information regarding error compensation. Indeed, in Fig. 3c the annual unbiased estimates are due to an overestimation during autumn and underestimation during winter.

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.

Reply: This section along with Table 6 were removed from the manuscript.

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 needs to 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,

ACPD
15, C11341–C11348, 2016

ARTEMIS, CHIMERE, 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.

Reply: We have expanded the acronyms in the text and in Table 1, when appropriate (e.g., CHIMERE, MELCHIOR and ISORROPIA are not abbreviations).

Technical Comments/Corrections:

Page 27044, Line 16: Expand “yr” to “year” here and elsewhere in manuscript.

Reply: Corrected.

Page 27046, Line 5: Expand “ca”

Reply: “ca.” has been removed.

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

Reply: “a.s.l” is changed to above sea level.

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

Reply: changed to monoterpenes.

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

Reply: Instead of providing an external reference which will necessitate further reading from the non-European audience we choose to revise the statement to “The 2030 emission projection for the IdF region includes gradual renewal of the vehicle fleet according to the latest emission standards (Euro VI)”.

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?

ACPD

15, C11341–C11348, 2016

Interactive Comment

Reply: There are no available suburban or rural measurement sites (and only one urban background site) in the finer scale (1km) resolution grid used for the evaluation presented in Figure 3. We do use measurements from two rural sites, Norr Malma and Aspvreten that are both located at the 12km domain. These are used for the evaluation of other products. The Aspvreten site for OC, EC and sea salt and the Norr Malma site for the evaluation of the regional contribution. We have revised some parts of the text to provide with more clear information.

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

Reply: We have identified a possible confusion in the evaluation section that is related to the use of the term "Paris" and "urban". The areas defined in geographical terms are different from the classification of stations defined by the local environmental agency that operates them. "Urban stations" besides those located in Paris are sited in some other heavily populated areas. So in the evaluation section it is preferable to refer to "urban stations". In this particular paragraph we revise: "Fig. 3a shows that over the urban stations of IdF, CHIMERE overestimates daily ozone (overall bias=10%) mostly at the urban sites outside the city center; focusing on downtown monitoring sites the model bias is only 3.7% (not shown)."

Page 27054, Section 4, 5.1, and 5.2: It 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.

Reply: We have revised according to suggestions, section 4 is the climate projections, section 5.1 is the present-time air quality and onwards the future air-quality analysis.

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?

ACPD

15, C11341–C11348, 2016

Reply: Indeed, we might have caused some confusion with the references to Table 4 and Figures 4 and 5. Table 4 shows both mean and MD8hr while in the maps only the mean. We have revised the various statements in the manuscript and in the table, figure captions to lift any confusion.

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

Reply: We have seen this in previous work already sited at the end of the paragraph (Markakis et al., 2014). In fact, the Markakis et al. (2014) paper was the first to document long-term projections of air-quality at urban scale. Therefore, for the 2050 horizon we are the first to show this result for IdF. There is another paper that only projects road traffic emissions and up to 2020, showing the same trend over Paris:

Roustan, Y., Pausader, M. and Seigneur, C.: Estimating the effect of on-road vehicle emission control on future air quality in Paris, France, Atmos. Environ., 45, 6828-6836, 2011.

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.

Reply: For the regime analysis we extract MD8hr values for all chemical compounds involved. This is because regimes can be more easily distinguished at the time of the local maximum when the phenomena of production/loss are stronger. A common practice is to use the 1h maximum (if it can be easily identifiable) or take the average of several hours around the local maximum. Since there is no convention on this we arbi-

ACPD
15, C11341–C11348, 2016

trarily use MD8hr in our analysis. In fact, the analysis was also performed implementing the daily averages but results remained the same. We have revised the caption of Fig. 6 to make clear that all values correspond to MD8hr.

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

ACPD

15, C11341–C11348, 2016

Interactive Comment