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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climate forced air-quality modeling at urban scale: sensitivity to model resolution, emissions and meteorology" *by* K. Markakis et al.

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Reviewer 1

My overall rating for this paper is "minor revisions"; there are a few places where the text should be clarified. They are however important clarifications – the authors' title implies that a 4km resolution CTM and 0.11 degree/12.2km resolution driving meteorological model (or even a 4km resolution meteorological model) captures urban meteorology and chemistry. This may not be the case, from some recent work they reference. I wouldn't say that this invalidates any of the authors' results, but their conclusions and

discussion need to acknowledge that some important meteorological circulation effects may only be captured at finer scales than employed in their study. There are three aspects to this concern, which I outline below, all of which can be addressed by adding appropriate caveats to their text. There are also a number of smaller concerns and spelling/grammar issues which follow, which would clarify and improve the readability of the paper.

Larger issues:

(1) I think it's important that the authors acknowledge that at least some of these results may be specific to the region they have studied (Ile de France). For example, one of their findings was that model vertical and horizontal resolution (both in the driving meteorology and the resolution of the CTM that made use of that meteorology) had a relatively minor effect on O3. Meanwhile, I'm aware of studies in coastal environments (where the city is located along a large lake or ocean coastline) where the resolution can have a profound effect on the ability of the meteorological model to capture the land-sea (or land-lake) breeze, and that in turn has a significant impact on ozone predictions. Similarly, though the authors note themselves (second sentence of section 2.1) that the region under study is far from the coast and has limited topography, these factors if present could significantly increase the relative impact of horizontal and vertical resolution. In that respect, some caveats should be placed within the text acknowledging the potential limitations of their study towards similar resolution studies in other domains (e.g. in the Abstract, page 4768, line 17, change "are of little effect." To "are of little effect for the regional/urban domain and models studied here."

Response: We acknowledge the fact that some of our results related to model resolution are representative to the specific characteristics of our study area. The abstract was revised according to the reviewer's suggestion: "The air quality model horizontal and vertical resolution are of little effect at least for the urban domain and the models' resolution refinements attempted here." We also insert the following text on page. 4784, line 27: "Never the less this result could reflect the local area's characteristics

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(flat terrain, away from the coast) confirming previous studies (Menut et al., 2005; Valari and Menut, 2008). In regions with more complex topography or close to the coast the resolution of the meteorological input could have a profound effect in the simulated meteorological conditions (Leroyer et al., 2014)."

(2) The authors also need to discuss the accuracy of their meteorological model in the context of resolution – is the lack of a resolution impact of the meteorological model on chemical predictions reflective of resolution not being important (the authors' conclusion, I think) or reflective of a meteorological model whose performance has not improved in going to higher resolutions? They later (page 4776, lines 18-19) reference previous work that shows that the flat homogeneous topography of the region results in little benefit in going to higher resolution. But what was the highest resolution attempted in the referenced work by Menut et al, 2005, and Valari and Menut, 2008)? The highest resolution of the input meteorology used in the authors' current paper is 0.11 degrees or about 12.2km – this is inappropriately coarse to resolve much of the urban circulation (cf. also line 4782, line 2 regarding the CTM itself – I agree, this is very likely, so caveats in the abstract and conclusions need to be added to that effect). In the Leoyer et al paper quoted by the authors, substantial changes in vertical and horizontal transport in an urban environment for a meteorological model occurred mostly in the transition from 2.5km to 1km to even higher horizontal resolutions (e.g. 250m). The resolution impact in the IdF region could thus be substantial, but just not yet seen on the still relatively coarse resolutions used by the authors in this set of tests. 12.2 km meteorology inputted into a 4km CTM won't see much if anything of the urban circulation, and a 4km resolution met model might not see much of that circulation either. The authors' conclusion should be modified to include caveats to that effect, with the message going from, e.g., "horizontal and vertical resolution are not important" to "input horizontal and vertical resolution and CTM resolution were not important for the regional/urban domains and model resolutions studied here. The relative importance of resolution may however increase with further reductions in grid size in both the driving meteorological model (c.f. Leoyer et al, 2014), and the CTM making use of that

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meteorology."

Response: We agree. The increase of the meteorological model's resolution from 0.5o to 0.1o might not be enough for the chemical model to produce noticeable concentration responses. In Menut et al., 2005 the increase is from 0.5o to 3km and in Valari and Menut, 2008 from 0.5o to 5km. Based on the Leroyer et al., 2014 results this might not be sufficient. Never the less according to Leroyer et al. 2014: "The necessity of using subkilometer meteorological numerical systems remains, however, questionable. The improvement in forecasting obtained by going to subkilometer modeling systems, with the use of a detailed surface description and of a more physically based (less parameterized) representation of atmospheric processes, still has to be rigorously demonstrated". Despite that it is useful to include such discussion in the text e.g. page. 4784, line 27: "We should also take into account that the meteorological model's resolution refinement attempted here (from 0.5o to 0.1o) might not be sufficient for the CTM to simulate noticeable concentration responses. For example Leroyer et al. (2014) (see also references therein) observed that substantial changes in vertical and horizontal transport in an urban environment occurred mostly in the transition from resolutions of 2.5km to 1km and even higher (250m)." We also revise a sentence in the conclusions section (page 4795, line 4): "We also note the weak sensitivity of modeled concentrations to the increase in the CTM's and the meteorological model's horizontal resolution at least for the area studied here (characterized by low topography and situated far from the coast) and the resolution refinement attempted (50km to 4km for the CTM and 0.5o to 0.1o for the meteorological model)."

(3) Page 4785, section 4.3. I'm assuming here that this sensitivity test looks at the vertical resolution of the CTM, but there is no corresponding increase in vertical resolution of the driving meteorological data (this should be clarified - mentioned in the first sentence)? This needs to be explicitly made clear in the text. This is another case where the impact of "vertical resolution" needs to be split conceptually in the text between "vertical resolution of the driving meteorology" and "vertical resolution of the

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CTM". My point here is that a meteorological model with a higher vertical resolution will almost certainly generate different vertical diffusion coefficients, than one with a lower vertical resolution. Increasing the resolution of the CTM may not capture this effect. A caveat to that effect should in the conclusions and the abstract with regards to the vertical resolution issue. The authors should avoid the use of phrasing like "of the model's vertical resolution" and instead be using "of the CTM's vertical resolution" or "of the driving meteorological model's vertical resolution".

Response: The output meteorology from WRF is allocated in a vertical mesh of 31 layers thus it is sufficiently resolved (this is added now in the description). When CHIMERE runs it interpolates the WRF meteorology to the CTM's vertical grid. Because the WRF meteo is highly resolved the increase of the CTM's resolution from 8 to 12 layers does include refinement of the inputted meteorology. The refinement of the vertical layering of the meteo model is not something necessarily desirable by the authors even if it was possible to elaborate on that effect. That would be a separate sensitivity. In this section we do intend to isolate the effect of CTM vertical resolution. The revised statement of the abstract e.g. "The air quality model horizontal and vertical resolution are of little effect at least for the urban domain and models' resolution refinements attempted here" covers the caveat of not increasing the vertical resolution enough in order to be as close as possible to the original 31 layering of the meteorological model output. Also across the manuscript we use the phrase CTM's vertical resolution removing the more ambiguous "model vertical resolution."

Minor issues:

Abstract and conclusions: needs to be clarified with regards to the resolution of the input meteorology versus the resolution of the CTM used for the modelling.

Response: These additions were implemented, please refer to the responses in the "major issues" section.

Page 4768, line 14: is that supposed to be "meteorological model input resolution"?

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Response: No this is the CTM's horizontal resolution. We have revised.

Page 4769, line 20: another reference here: Kelly et al, 2012, 5367-5390, 2012.

Response: Added.

Page 4770, line 19 to line 24: the sentence "In Markakis (2014)...NOx-limited conditions." Could use a little clarification: presumably each didn't work for the other condition, as well?

Response: The reviewer's question is a bit confusing to the authors. Both "resolutions" provided results of chemical regimes at both urban, rural areas. In rural areas they agreed as to the NOx-limited character whereas in urban areas gave opposite environment.

Page 4771, first line: using RCP6, Kelly et al did this – a laborious process of linking the RCP recommendations with specific industries.

Response: This is indeed an oversight from our part. We revise: "This is because long-term projections are very constrained by the evolution of energy supply and demand, which is a large scale issue (an effort to link global and regional scale projections was attempted in Kelly et al. (2012))."

Page 4772, line 13: The authors should explain why this particular RCP was used in their work, rather than the other RCP scenarios available.

Response: This is related to the availability of simulation on the larger scale used to provide the boundaries to our simulations.

Page 4774, line 1: might be worth mentioning here that the database in question does not explicitly consider point sources.

Response: We revise "Present-time emissions (as areas sources) are compiled. . ."

Page 4775, line 25: not clear why the same metrics were not being used for both O3

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and PM2.5. Explain.

Response: We use MFB and MFE which are considered more suitable for fine particles evaluation based on EPA guidelines. In Page 4775, lines 6-9 we already include the EPA, 2007 reference.

Page 4788, lines 24-25: This was rather a surprise to me, though I may be used to more detailed emissions inventories in North America. When you say "no major point sources can be found within the urban area" does this mean that "no major point sources exist within the urban area of Paris" or "no major point sources are explicitly included within this inventory within the urban area of Paris"? In contrast, North American emissions inventories in Canada and the USA include tens of thousands of point sources (to the extent that one has to choose criteria for deciding which ones will be selected for plume rise calculations and which have minor enough emissions to be treated as area sources). So a line or two explaining whether the issue here is a lack of data in this inventory for this region, a lack of data in any inventory for this region, or if there really are no major point sources in this large city.

Response: This is the first case as the local emission inventory includes tenths of point source (Page 4774, line 13-15). But naturally the larger and most emitting are not inside the urban area but in industrial areas outside the city. We revise: "Following the AIRPARIF post-processing (ANN) all urban emissions are released in the surface layer because according to the local point source emission database no major industrial units are found within the urban area."

Page 4789, lines 10-13 and section 4.5 in general. You might also be interested in having a look at Makar et al, Geoscientific Model Development, 7, 1001-1002, 2014, since there are several points of overlap between the authors' paper and that one: the reference looks at several stages of emissions improvement and how two different off-line CTMs responded to those changes. Both temporal and spatial changes in emissions were evaluated and the impacts on O3 and PM2.5 predictions evaluated.

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Response: The paper is very interesting and we have added a sentence in Page 4778, line 13: “Makar et al. (2014) investigated the modeled concentration response to the refinement of the spatial and temporal allocation of the inputted emissions and found the model as sensitive as to the improvements in the parameterization of vertical mixing. Also they conclude that the temporal distribution of emissions in particular, could be very important in stable urban atmospheres and that this sensitivity is reduced with increased mixing conditions.” We also add at the end of section 4.5: “We must note that recent work (Makar et al., 2014) has pointed out the sensitivity of modeled concentration to the choice of model used; the signal of the refinement of the spatiotemporal parameters used to derive the input emissions in two different CTMs was very much dependent on the model despite the fact that the same inventory -and improvements- were applied to both models.”

Page 4791, line 15-16; see the above discussion; I think that this last sentence currently ending “especially taking into account the large increase of model resolution from 50 to 4km” should be “for the range of meteorological and CTM horizontal resolutions attempted here. A stronger impact of resolution may occur at even higher resolutions (c.f. Leroyer et al, 2014).”

Response: This sentence is under the “effect of CTM’s resolution” section therefore we revise the statement according to that feature alone e.g.: “We may conclude that the benefit of increasing the CTM’s resolution is insignificant for both ozone and PM2.5 especially taking into account the large refinement attempted here (0.5o to 4km).” The reviewer’s suggestion as to the possible effect of refining the meteorological model resolution is already added in the text in accordance to the comment number 2 of the “major issues” section.

Page 4792, lines 25 through 27 seems to be saying “getting annual emissions totals right has a big impact on O3 results” while lines 12 through 14 seem to be saying “getting the annual emissions totals right has a very minimal impact on O3 results”. This needs to be clarified.

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Response: The statement in lines 12-14 regards only urban ozone. This is clarified: “Considering the discrepancies in the inventorying methodologies used to compile the ECLIPSE and the AIRPARIF datasets (top-down vs. bottom-up), it is very interesting that the least influential factor to the urban ozone responses is the annual emissions totals.”

Page 4793, lines 10 through 12: the authors may wish to consider and discuss the potential for compensating errors in this regard. That is, (1) the PM2.5 levels in the urban regions are likely mostly controlled by primary emissions; (2) increasing the emissions inventory resolution will defacto concentrate the PM2.5 emissions into a smaller spatial extent of the urban area (the reverse side of the artificial dilution issue that the authors have already discussed); (3) if the emissions totals are themselves biased high, then the resulting error will only become apparent at higher resolution. That is, the conclusion should not necessarily be “the emissions resolution makes the PM2.5 worse”, but “the emissions resolution may be showing us that the emissions totals are too high, and this only becomes apparent at high resolutions”.

Response: “the emissions resolution makes the PM2.5 worse”: this was not in any case the message we intent to convey. Table 6 only shows how the REG application would respond if selectively incorporated features of the local application. The message is: “if the REG application keeps the coarse inventory totals but adopts a higher resolution for the modeling and refining spatially its emissions this would only result in high overestimation of concentrations” and that “the REG application has to adopt all emissions related local features”. We have revised this part in the manuscript but we are not sure if the reviewers suggestion in the same line of thought.

Minor spelling/grammar mistakes to be corrected:

Response: Spelling mistakes were corrected in the text.

Page 4768, first sentence of abstract starts with a preposition, better to use “Previous research helped to....spatial scale effect, but our knowledge is limited....

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Page 4768, line 21: “(same improvement” should be “(the same improvement”

Page 4768, line 23,24: “bias on” should be “bias of”, and “associated to” should be “associated with”

Page 4769, line 2: “at urban scale” should be “at the urban scale”, ditto, line 24.

Page 4770, line 2: “By principle” should be “In principle”.

Page 4770, line 27: “to higher” should be “in higher”. Reductions of what? O3?

Page 4771, lines 3 to 5: sentence is unclear.

Page 4771, line 7: “demands and that” should be “demand. This“

Page 4771, last line: “f)” should be “(f)”, ditto for “g)” should be “(g)” on the next page.

Page 4773, line 5: “sulfates, nitrates” should be “sulfate, nitrate”.

Page 4773, line 14: “vertical” misspelled.

Page 4773, line 19: “gasses” should be “gases”, I think. Might be British versus American spelling conventions, here.

Page 4774, line 5: “are available” should be “are also available”

Page 4776, line 9: “speed was the” should be “speed were the”

Page 4777, line 9-10: “due to the surface emissions, ozone concentrations in the afternoon peak hour had the second largest sensitivity after meteorology.” Should be “the sensitivity of ozone concentrations in the afternoon peak hour was the second largest after the sensitivity associated with meteorology.”

Page 4778, line 6: “scale” should be “scales”

Page 4784, line 3: “to modeled precipitation” should be “to the accuracy of modelled precipitation.”

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Page 4784, line 20: “of the meteorological grid” should be “of the input meteorological grid” (I think).

Page 4787, line 5: “is in the “ should be “is on the”.

Page 4790, line 6: “model processes” should be “model process”. Line 18: “Fine particles” should be “Fine particle”

Page 4791, line 8: “is very little sensitive” should be “has relatively low sensitivity” Page 4791, line 24: “same source” should be “same sources”

Page 4792, line 5: end the sentence with a question mark and put quotations around “what are the main...or at least reduced?”

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 4767, 2015.