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Interactive comment on "Air-quality in the mid-21st century for the city of Paris under two climate scenarios; from regional to local scale" *by* K. Markakis et al.

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Overall I thought that this was a good paper and its topic material and the issues raised are suitable for publication in ACP. The authors have shown that regional scale simulations of air-quality under future climate conditions may not adequately represent the physical and chemical conditions in urban environments. The policy advice that may result from regional simulations may therefore differ significantly from the advice provided in a (presumably) more accurate higher resolution model. This is an important contribution, in that the impacts of climate change mitigated air-quality on the human population will be in the cities, where the human population tends to be concentrated.

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Interactive Discussion

Discussion Paper

High resolution urban modelling, such as carried out by the authors, is therefore necessary to provide improved estimates of the impacts of climate change as well as projections of emissions changes on human health. The authors also found that these effects are limited to the urban regions themselves – with implication that the regional models and scenarios perform adequately for predicting regional impacts, and that higher resolutions in that context are unnecessary, which is also valuable information for the scientific and policy communities.

I had three specific issues with regards to the methodology used in the paper before I can recommend publication. I'm hoping that these can be resolved through clarifications in the model text as opposed to repeating model simulations, hence I've listed the paper as "minor revisions". However, the first of these ((1) in the list below), is a potentially serious problem if it not a matter of a poorly worded sentence that should be clarified - and if temporally invariant emissions profiles were actually used, that is a serious issue which needs to fixed (amounts to major revisions). I also have a number of smaller issues that definitely are minor in that they can be resolved just through improvements to the existing text, figures and/or tables.

Specific issues:

(1) On page 103, line 14, the authors mention that emissions were kept constant during the control run, and that only the vertical distribution of point source emissions across model layers varies in time. This statement can be interpreted in two ways. The first of these is that the emissions are time-invariant at all time scales; the emission rates at every hour of every day are the same. The other is that the annual total emissions do not change between the different years of the climate run; no attempt is made to try to take into account changes in emissions levels that might take place between one year and the next. The second interpretation, of a constant annual total, makes sense in terms of trying to determine a climatological response of the model to its emissions: the annual emissions should be held constant at mean values, to eliminate emissions variability as a cause of differences, at least as a first stage in the process.

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The first interpretation, that the emissions do NOT change from one hour to the next using typical hourly time splitting profiles, would have a serious negative impact on the model accuracy. Many of the emitting activities have a very strong diurnal signal, varying from between near-zero levels at night to relatively high levels during the day. For example, emissions from the domestic "mobile" sector have a nighttime minimum, and two daytime maxima, corresponding to morning and evening rush-hours (cf. Zhang et al, http://www.epa.gov/ttn/chief/conference/ei20/session1/jzhang.pdf). The extent to which these variations are accurately simulated can have a critical effect on model chemistry (Makar et al, GMDD, 6, 5595-5644, 2014). I'm hoping that this is a matter of a sentence that needs to be reworded to indicate that emissions are changing on an hourly basis, but annual totals are the same. This seems to be likely given that later, on page 107, lines 14-16, they mention that monthly temporal profiles have been used to distribute the residential heating. Presumably hourly temporal profiles are also used for at least some of the emissions (mobile sector, residential sector, etc.), and this is just a matter of clarifying a single sentence. This needs to be made clear – if there is no hourly variation, the model is not accurately representing "real" emission activities, and model reruns with these variations will be needed.

(2) I am a bit concerned about the use of a 10km resolution meteorological model to drive a 4km resolution air-quality model, particularly in an urban context. The authors' work explores the extent to which changes in resolution of emissions affect the resulting chemistry, but there is also a large body of work that suggests that changes in resolution of the driving meteorological model can have a significant impact on model accuracy, particularly for complex urban environments (cf. ACP special issue on the BAQSMet study, Hayden et al, Makar et al, and other papers therein, papers by Jonathan Pleim et al elsewhere, Flagg and Taylor, 2011, LeRoyer et al, 2014). WRF has been used at 4km resolution in the past – so why wasn't it used in that mode here? I would expect that the urban heat island would be better resolved, temperatures and wind velocities would be impacted, etc. The authors need to justify this in the text, or at least discuss the possible impact of using lower resolution meteorology on their

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results, given that they risk missing meteorological effects that could significantly affect their results. A good recent example of the impact of resolution on model meteorological predictions for urban regions can be found in LeRoyer et al, J App Met and Clim, doi: 10.1175/JAMC-D-13-0202-2013. Another: Flagg, D.D., and Taylor, P.A., ACP 11, 2954-2972, 2011.

(3) Given the relatively small size of the meteorological and air-quality model domains, more description is needed for the downscaling and the potential impact of boundary conditions. Some questions that come to mind: For the 50km resolution simulations, were the same emission data used as for the 4km resolution simulations? What boundary conditions were used for the outer 50km simulation, and where did they originate (if these were in the global coupled runs, was the model speciation the same or were there issues with matching them)? The authors mention that a coupled global model was used for the initial meterological runs – what IPCC/RCP / other emission scenarios were used for that simulation, and how did they compare to the emissions in the model simulations used here? A one-paragraph description of the CHIMERE model is needed for readers unfamiliar with the model (as opposed to just the reference) – what are the gas and particle phase components of the model and references for them, for example – just enough so that the reader has an idea as to how this model compares to others in the same class.

Other (more minor) issues with the manuscript:

(4) The authors use mean daytime surface O3 and and Ox and the daily maximum of 8 hour running mean O3 as indicators of O3 performance and state that the model reproduces sufficiently well both urban titration (page 105, section 3.4) and photochemical formation. These metrics will test the extent to which the model performs well for daytime urban titration and photochemical formation, but will not test how well the model performs when the titration is the dominant process, i.e., at night. The authors should also include statistics for hourly O3, or, better, O3 binned according to local hour, before stating that performance of titration is accurate.

(5) Some aspects of the REF versus MIT scenarios and the relationship between the regional emissions inventories and those used for high resolution were difficult to follow. If I've understood correctly from page 105, the national-level changes were used to derive local level scalings for the IdF (which makes sense, since local spatial allocation of emissions is likely to be higher resolution). However, what was done for the driving 50 km resolution simulation? This is not necessarily a trivial question – one concern I have is the extent to which the changes in the high resolution domain are due to local emissions changes versus changes in boundary conditions from the 50km model, or both. Were the emissions downscaling procedures used for the driving 50km model as well as the 4km model? This is not mentioned in the text (or I've missed it). Also, Figure 4 suggest that the relative impact of local emissions is much greater in the MIT scenario than the REF scenario, but this is masked by the colour scales chosen. Figures 4(b) and 4(c) should be repeated (4d, 4e) with the same colour scale for both figures and same upper and lower limits for that scale, and no more than 10 colour bars on the scale, to show this difference. The gradient between urban and rural O3 has greatly increased in the MIT scenario and this is worth pointing out, since it shows that the local emissions become much more important for some emissions scenarios than others.

(6) The discussion on page 112 needs to be linked back to the earlier discussion on emissions downscaling. Are the changes discussed on lines 1 through 6 of page 112 the result of changes to absolute emissions levels and/or background emissions changes or local emissions changes? When the authors conclude at the bottom of page 112 that the regional scale emissions fail to distinguish between rural and urban chemistry, the reader is left wondering whether this is due to the downscaling procedure for the emissions, the absolute emissions levels used in the different scenarios, or both.

The use of Sillman type indicators on page 113 is a very good idea, and shows the differences nicely – as is the use of EKMA type on figure 5. The axes on Figure 5

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should be labeled “Relative NOx”, “Relative NMVOC” to underscore that these are sensitivity runs off of a starting point. Were these scalings on a mass or mole basis (I’m guessing the latter if units were ppbv, but this should be mentioned in the text.

Smaller issues:

(a) Page 92, last line: another paper to add to the list of regional AQ+climate models: Kelly et al, ACP, 12, 5367-5390, 2012. Blatant plug for our own work, but it does deal with the impact of climate versus emissions that the authors mention is beyond the scope of their own work.

(b) The authors state (page 98, line 10) that regional-scale emission inventories fail to represent the plethora of emission sources at large cities. I think this needs to be qualified through some additional explanatory sentences. This may also depend on the geopolitical jurisdiction for which the emissions data have been collected, and the accuracy of spatial as well as temporal allocation data. The emissions data used for current-time regional air quality models for short-term policy predictions or air-pollution forecasting are often very detailed, and emissions processing systems such as SMOKE allow county-level data to be spatially allocated on a very local scale. Having said that, the details of those spatial and temporal allocations may be inaccurate (cf. Makar et al, GMDD, 2014) and this may lead to inaccurate model predictions. I suspect that the authors here mean to refer to “Regional scale emission inventories for future climate change scenarios”, such as the RCP scenarios described thereafter in the paragraph: those extra five words would make this clear and avoid any confusion with current-climate emissions inventories (which are much more detailed than the RCP scenarios, etc).

(c) Page 99, line 17, “(iii) two mid-21st century. . .”. A few sentences describing these scenarios and placing them in the context of RCP scenarios, etc., are needed here.

(d) Page 101, line 5, a comment: Actually, a 10 year run may not be that bad. The authors could check this by calculating statistics using sub-sets of their 10 year period

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to see the effect on the comparisons to observations, or check the standard deviations of the statistics with different numbers of years out of the total they have.

(e) Page 102, lines 14 – 20. The authors seem to expect poor performance for a climate model simulating a current climate, but this should not be the case if the variables are being compared to observations on a climatological time scale (e.g. annual or 10 year averages) for both the air-quality forecast model and the air-quality climate model (e.g. Kelly et al had about the same performance for O3 for climatological and meteorological simulations with the same off-line AQ code, though worse performance for PM2.5 in the climatological simulation).

(f) Page 104 lines 8 through 14: it would be useful to the reader to have a table added which gives the IdF emissions totals for major pollutants (e.g. NOx, SO2, VOCs, CO, PM2.5) for these different scenarios, to allow the reader to see the potential impact in the urban region of the different sources of information.

(g) Page 105: is the high bias of wind speed improved when WRF is run at higher resolution for urban regions? Given the LeRoyer et al and Flagg papers referenced above, they probably would be. See earlier comment on the resolution of the meteorological model simulations carried out.

(h) Page 106, line 21: I don't follow the reasoning that short term meteorology would fail to result in 95th percentile peaks being simulated. If this is a climatological comparison, if the model is doing a good job and the time period is sufficiently long to be representative of the climatology, the 95th percentile peaks should be showing up in the model results (just not at the specific times in the observation record). Could the authors please clarify their argument?

(i) Page 107, line 17: what is the PM2.5 speciation of CHIMERE (this should appear in the model description section), and what proportion of the PM2.5 in the IdF is primary versus secondary in origin, both in the observations and the model? Is SOA a large part of the (observed) SOA? i.e. what evidence is there to suggest that each of the

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factors suggested by the authors might lead to the deficit in PM2.5 noted? The authors go on (line 20) to state that the summertime emissions inventory might be at fault – how well does the model simulate the PBL height, vertical diffusion, etc.? These can also have a large influence on the model results.

(j) Page 108, line 13, "It is beyond the scope". The Hogrefe reference already quoted and Kelly et al (ACP 12, 5367-5390, 2012) deal with the issue of climate versus emissions changes.

(k) Page 111, line 18: The authors need to define in a sentence what they mean by "in-plume" chemistry here, and how they extracted it from the model. I'm assuming this means that the downwind side of the model plume from the IdF was used to generate the "in-plume" results from the rest of the paper, but this is not clear from the text.

(l) Page 114, section 4.2.3. The metrics being used here (Nd130, MTDM, SOMO35) may not be familiar to some readers – a short sentence or two with a reference describing their relevance as health metrics should be provided.

(m) There are several cases where there are minor errors in the use of English:

a. Page 96, line 4: "High-resolution" should be "A high resolution"..."further extended to year" should be "further extended to the year"

b. Page 102, line 6: "metrics is used" should be "metrics are used".

c. Page 108, line 19: "compare to present" should be "compare to the present".

d. Page 109, line 10: "Actually modeled" – unsure of what's meant here, "Modeled" would probably make more sense in the context of the sentence.

e. Page 109, line 14: "3.6 times" should be "by a factor of 3.6".

f. Page 109, line 24: "Projections show increase" should be "projections show an increase".

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g. Page 110, line 12: “opposed to our” should be “in contrast to our”.

h. Page 110, line 27: “dimensional” misspelled.

i. Page 111, line 5: “then run” should be “then ran”

j. Page 111, line 20: “increase” should be “increases”

k. Page 111, line 27: “proper” is not appropriate here – just leave it out.

l. Page 112, line 4: “Titration process” should be “The titration process”

m. Page 113, line 6: “Paris city” should be “city of Paris”

n. Page 113, line 25: “decrease” should be “decreases”.

o. Page 116, lines 20, 21: “such extend” should be “such an extent”. “ozone increase instead” should be “ozone increases instead”.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 95, 2014.

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