**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Impacts of future climate change and effects of biogenic emissions on surface ozone and particulate matter concentrations in US" *by* Y. F. Lam et al.

Anonymous Referee #1

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This manuscript describes a modeling study that investigates the impacts of climate and emissions changes on predictions of ozone and particulate matter. The authors use several different models to evaluate the impact of model grid resolution on the predictions, which I think is very valuable. I believe that the overall study is worthwhile and applicable for publication in Atmospheric Chemistry & Physics. However, I believe that substantial revisions should be made before the paper is acceptable for publication.

There has also been quite a number of studies lately investigating future emissions and climate on air quality. Although the authors do cite quite a few, I feel as though they have not recognized studies that are directly relevant to this one. For example:

Heald, C. L., Henze, D. K., Horowitz, L. W., Feddema, J., Lamarque., J-F., Guenther, A., Hess, P. G., Vitt, F., Seinfield, J. H., Goldstein, A. H., Fung, I.,: Predicted change in global secondary organic aerosol concentrations in response to future climate, emissions, and land use change, J. Geophys. Res., 113, D05211, doi:10.1029/2007JD009092, 2008.

Jiang, X., Z.-L. Yang, H. Liao, C. Wiedinmyer. (2010) Sensitivity of Biogenic Secondary Organic Aerosols to Future Climate Change at Regional Scales: An Online Coupled Simulation. Atmospheric Environment, 44, 4891-4907, doi:10.1016/j.atmosenv.2010.08.032.

and

Avise, J., Chen, J., Lamb, B., Wiedinmyer, C., Guenther, A., Salathé, E., and Mass, C.: Attribution of projected changes in summertime US ozone and PM2.5 concentrations to global changes, Atmos. Chem. Phys., 9, 1111-1124, 2009. This project is particularly unique because it attempts to provide information about the value in model grid resolution, which is very important. However, I believe that the authors provide stronger reason(s) why this study is different than others.

Page 2186 and elsewhere: The authors talk about a downscaling method. Although they say that it is described in another publication, a quick (e.g., 1 sentence) description should be included here. In addition, the definition of "downscaling" should be made clear right from the beginning.

Page 2186, line 20: why is it wrong ("erroneous") for modeling studies to use consistent emissions input when comparing models? Are the authors saying that the emissions should Not be consistent? Page 2187, line 10: what is the positive feedback from BVOC emissions? Throughout the paper, the authors make statements like this that are not justified/explained. The authors discuss BEIS. Which versions? This makes a big difference, since different versions of BEIS produce very different estimates of biogenic emissions. The authors need to be explicit about this.

Pages 2187-2188: the authors discuss a study that showed that 12km resolution is much better at predicting regional air quality and 30 km resolution is too coarse. Yet, in the next paragraph, the authors state that they are using 36km resolution? Can they justify this? Why? It seems contradictory. Why not do all of the study with 12km?

At the end of the introduction, there should be more justification for the study and more information about the overall benefit of the results.

Figure 1 does not include the VISTAS domain, as mentioned on page 2190. Please define VISTAS when it is first mentioned in the paper and put it on the figure (if it is relevant). Figure 1 doesn't show the different domains- only the analysis boxes.

How are the species between the GEOS-chem and the CB-IV mechanisms mapped? So that the GEOS-chem output can be used to drive the boundary conditions of the CMAQ model simulations?

Page 2192: The authors say that there is a large increase in ALD2 and FORM in the future emissions due to biomass burning. Where do these future biomass burning emissions come from? The authors later state that the PM concentrations of the model underpredict the maximums because no special events are included, like fire, are included in the simulations (Section 3.2, paragraph 2). This seems contradictory. How were fires included in the simulations? How were increases in fire emissions determined for the future? Are all of the factors in Table 2 from Streets et al. (2004)? This should be more clear- since this is a big component of the future simulations.

Biogenic emissions models: As I mentioned earlier, the authors need to be explicit about which version of BEIS that was used. Also, the emission factors are not the only difference between the BEIS and the MEGAN models. The light algorithms in both models (for isoprene) cause very different emission estimates. And the authors don't mention anything about the leaf area index inputs for each model. Were they the same? The LAI can drive the seasonal dependence of the isoprene emissions, since many isoprene emitters are deciduous and will not emit when there are no leaves on

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the trees. The description of the biogenic emissions models is very poor and should include much more explicit information. Particularly with the description of MEGAN. For example, which algorithms were used (from Guenther et al., 2006) and which emission factor maps were used, etc. MEGAN has evolved much in the past few years, and it is really necessary for users to write explicitly what was used.

Page 2195 describes the differences between MEGANv2.02 and BEISv?, now and in the future simulations. Can the greater increase in MEGAN isoprene be the result of the light? What about increases in future LAI? CO2 inhibition? Was any of this considered?

The authors need to be clear whether they are talking about global or regional changes climate/temperature. (e.g., Page 2197, lines 3-4).

Page 2197: The authors are repetitive. For example, on Pages 2196-2197, the authors state that higher temperatures could cause increases in ozone and PM formation (page 2196, lines 23-24, Page 2197, lines 1-2, page 2197 lines 15-17, page 2197 line 26).

Page 2198, line 12: How can the authors "conclude" that wind speed has a minor effect on PM and ozone. There isn't evidence for this.

Why did the authors not use the EPA AQS data for the measurement/model evaluation? (or, is this the same as CASTNET)?

Page 2200, lines 23-24: How can the authors say that CO and NOx emissions used in this study were close to actual measured emissions – just because there is good agreement with O3 concentrations? I see no evidence for this. There are MANY reasons why O3 is right (or wrong).

Page 2202, line 26: The authors state "As expected, the changes in PM2.5 concentration(S) are limited to only organic aerosol from the biogenic VOC". I am confused by this statement. Does this mean that in the two simulations with BEIS and MEGAN emissions, only the organic component of the PM2.5 changes? Is this true? Carlton

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et al., ES&T, 2010 show that changes in isoprene emissions can have a dramatic impact on sulfate and other aerosol components by altering the OH that is available for reaction with SO2. I would remove the clause "As expected" since I don't believe that this is true. And in the next statement the authors state: The impacts of changes in PM2.5 in the southeast is larger than other regions because of the isoprene emissions of the two models. Can the authors explain this? (Later they mention that there is no isoprene SOA in the models). What is going on here?

In several places throughout the manuscript, the authors use statements like "It is believed that...". It would be more useful to show proof of why they "believe" the statement put forth.

Page 2205: The authors describe reasons for differences between the CMAQ and GEOS-chem results. However, I don't think this analysis is very robust. Why is lightning NOx suggested to be a reason for difference in the min/max ozone of the two models? What about vertical mixing? How does the PBL differ between the models. I think the authors need a much more robust analysis. This comparison is very valuable, but the evaluation is not detailed enough.

Page 2206, line 26: why are future climate and air quality predictions in the Southeast controversial? References? Examples?

Again, the conclusions provided in Section 3.4 is confusing and poorly written.

Section 3.5: What do the authors mean that the GEOS-Chem did not include "sufficient" PM2.5 species at the moment? What PM species does GEOS-chem have? The authors provide a range for the maximum and average PM2.5 concentrations from CMAQ. Are these daily? Annual? How do you get a range? Also, in this section, the authors note that an increase in SOA is observed in the future simulations, despite increases in temperature which would lead to semi-volatile species remaining in the gas phase. Could this be the result of increases in VOC emissions? Or in PM emissions?

Overall, this paper is awkward and could be edited to be more clear, flow better, and provide more relevant information. I am including some specific editorial suggestions below as examples, but I am recommending that this paper be rewritten and edited throughout. Throughout the paper, the authors start sentences with prepositions. For example, "In the area of regional climate change and air quality", "In the regional model perspective", etc. These introductions should be removed and incorporated within the sentences (or just removed all together).

Minor comments: Throughout the paper, the authors put a period at the end of the sentence and then add the references/citations. I think that the references should come before the period.

Page 2185, line 11: What is "the" regional model? Should it be "regional models"? This entire sentence does not make sense.

Page 2186, line2 1-2: Is it surface temperatures? This could be reworded as "Predicted a 1-3oC temperature increase for 2050"

Page 2186, line 10: What is dirtiness and clearness scenarios?

Page 2186, line 16: what is RAQM? Please include a reference.

Page 2187, lines 26- ; This sentence is very long and difficult to follow. And "i.e." can be removed.

Page 2188, line 14: What is the GCAP framework?

Page 2190, line 2 (and elsewhere): The authors use the term "climate event". The word "event" should be removed.

Page 2190, line 7: Edit: "In this study, GEOS-Chem (v7.03.06) was used. This model includes fully coupled treatment...." Also, what do the authors mean by "fully coupled"? Is the chemistry coupled with the meteorology? Do the aerosols feed back to the radiation scheme? Please be more explicit.

Page 2190, line 13: The emissions scenarios are described below (not "were described" – they haven't been described yet).

Page 2190, line 17: It should be "Details of the global models' set-up are described by Wu et al. (2007)."

Page 2192, line 7-8: edit to "...above has been applied for simulations with both GEOS-chem and CMAQ..." (And again, as mentioned earlier, it is "erroneous" to have consistent emissions? This is confusing).

Page 2194, line 26: the authors should define the chemical species (also in Table 2).

Page 2196, line 18: Reword the sentence. For example: "Several climatic variables were selected for evaluation since they impact air quality: ground temperature, …."

Page 2196, line 25: Remove "low-volatile"

Page 2199, line 3: Should be "air quality". And what is "early findings"? From where?

Page 2199, line 9: Should be "future climate conditions"

Page 2199, line 21: Remove "As expected" (unless you explain why this is expected)

Section 3.1: This whole section is wordy and awkwardly written.

Page 2200, line 27: Replace "O3 result" with "The simulated O3 concentrations were about …"

Page 2201, line 9-10: Remove "For the underestimate of PM2.5, "

End of Section 3.2: There are MANY reasons for the underprediction of aerosol concentration in the models, beyond that which is mentioned here. (Many more reasons beyond isoprene SOA).

Section 3.3: Isoprene emissions are less in winter, not only because of lower temperatures, but also because the majority of isoprene-emitting trees are deciduous and don't have leaves in the winter.

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Page 2205, line 23: This should be "Inconsistencies in the GEOS-chem and CMAQ MDA8 O3 results in the southeast domain were observed."

The reference Xiaoyan et al., 2008 should be Jiang et al. (2008).

Please check the reference for Zhang et al. (2008)

Table 4 is confusing. I thought three years were run for every case? Were only one year of simulations with BEIS performed just for sensitivity? And what does the dotted line around the top 4 simulations represent?

Figure 1: where is the VISTA domain? Is this the entire 36 km domain? What is the color of the map?

Figure 3: the % change in PBL height could be more informative. Also, is this the average over each season?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 2183, 2011.