*** Blue color indicates the change being made in response to the comments in the text.**
*** Red color indicates question or comment.**
*** Black color texts show the Responses to the reviewer.**
*** Yellow highlight show action header**

1) Response
2) before – text before correction
3) after – text after correction
4) added – text is added to the original article

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

Received and published: 29 January 2011

Response:
To anonymous Referee#1:

Thank you for the valuable comments. The comments are constructive and valuable. It reflects the dedication of the referee (apparently you have spent numerous hours on it). We are very pleased to receive such detailed comments. We have according to the comments and made necessarily changes to the original text.

Regards,
Yun Lam/Joshua Fu

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.

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.

Response: The publications suggested by the referee have been added to the reference list and have been incorporated into the discussion in the paper. Those publications give audience more information on the effects of global climate change on ozone and aerosol concentrations due to the change of biogenic emissions. It adds value to overall paper.

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.

Response: Clear definition of downscaling/downscaled has been added to the text. Moreover, additional information on downscaling methodology has been added to give more descriptive information. For detailed downscaling methodology, reader can refer to the publication we have cited. This paper was not focus the methodology of the downscaling, instead, we focused on the results of the air quality.

Added:

(pg 2 ln 9-11)
The term "downscale/downscaling" refers to the technique for enquiring global climate/chemistry model output (usually with coarse resolution) as the input for regional climate/air quality model (usually with fine resolution) to study regional/local phenomena.

(pg 5 ln 14-20)
For GISS downscaling, the GISS GCM III outputs were interpreted and interpolated into the format accepted by the MM5 preprocessor, REGRID, to provide meteorological initial and boundary conditions for MM5. For GEOS-Chem downscaling, the GEOS-Chem outputs were undergone time-step interpolation, vertical and horizontal interpolations, chemical species conversion, appending chemical species, and unit conversion to achieve CMAQ model-ready initial and boundary conditions. Details of the downscaling methodology are described in (Lam and Fu, 2009; 2010).

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?

Response: The term "erroneous" was used by mistake. It should be "important" I am totally agree that it is important to have the consistent emissions input. So, the outputs from global and regional models are on the comparable base.

Before,

Their study revives the erroneous notion that consistent emissions input between the global and the regional models should be used when model comparisons are performed.

After,

(pg 2 ln 38-40)
Their study revives the important notion that consistent emission inputs for the global and the regional models should be used when model comparisons are performed.

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.

Response:

1) The term "positive feedback" should be "indirect effect".

2) The abbreviations for BEIS and MEGAN in the text have been modified to BEIS3 and MEGAN2 to provide additional information on the version it used. This removes the confusion raised related to the biogenic models.

Before,

Although the effect of climate change on temperature and stagnant air flow would encourage the formation of ozone, most researchers have found that the anticipated emissions reduction from IPCC cases (i.e. A1B) in the United States tends to compensate for the effect of climate change on ozone formation with or without considering the positive feedback from Biogenic Volatile Organic Compounds (BVOC) and yields an overall ozone reduction of -4 to -15% in 2050.

After,

(pg 3 ln 10-12)
Although the effect of climate change on temperature and stagnant air flow would encourage the formation of ozone, most researchers have found that the anticipated emissions reduction from IPCC cases (i.e. A1B) in the United States tends to compensate for the effect of climate change on ozone formation with or without considering the change of Biogenic Volatile Organic Compounds (BVOC) in future. The overall ozone reduction is projected to be -4 to -15% in 2050.

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?

Response: The text was not writing really clear on this section. We have removed "36km resolution" from the text.

In this study, we intended to study three different issues: 1) the effect of using different biogenic models for the climate change study, 2) the air quality effect from climate change in Eastern United States, and 3) spatial resolution effect on climate change study.

In order to minimize number of CMAQ simulations required for the study, we have integrated all three scenarios. First, we ran one present year (2000) and one future year (2050) of CMAQ simulations on CONUS 36 km domain for each biogenic model (MEGAN and BEIS) to investigate the effects of using different biogenic models in the climate change study. Second, we ran one present year and one future year of simulation on the VISTAS 12 km with MEGAN emissions. Now we would have a total of two years of CMAQ outputs (one year from present climate and one year from future climate) on both 36 km and 12 km domains to investigate the effects of the spatial resolution

Finally, we ran additional two years of present climate (1999 and 2001) and two years of future climate (2049 and 2051) of CMAQ simulations using MEGAN emissions at 36 km. Now, we have a total of 6 years of CMAQ outputs from the MEGAN emission scenario to investigate the effect of climate change on air quality.

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.

Response:

Before,

Figure 1. The CONUS domain with the selected study areas boxed in red.

After,

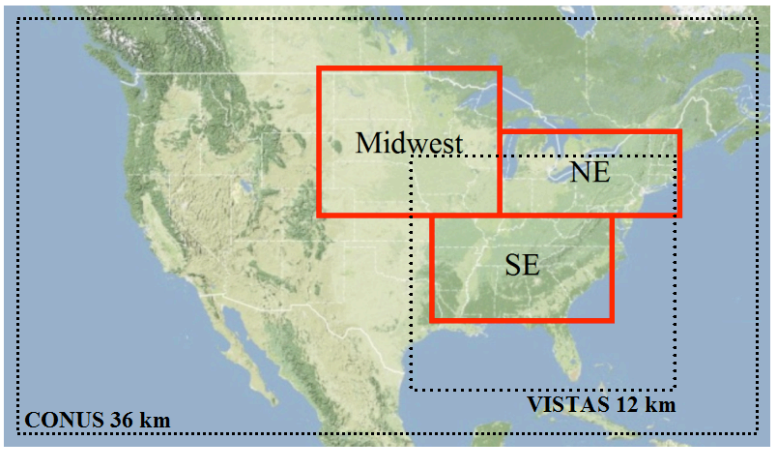


Fig. 1. The CONUS 36 km and VISTAS 12 km domains with selected study areas boxed in red (the dark green color on the map indicates locations of vegetation).

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?

Response:

The appendix A from Lam et al. (2010)

Lam, Y. F., and Fu, J. S.: Corrigendum to "A novel downscaling technique for the linkage of global and regional air quality modeling" published in Atmos. Chem. Phys., 9, 9169-9185, 2009, Atmos. Chem. Phys., 10, 4013-4031, doi:10.5194/acp-10-4013-2010, 2010.

Appendix A

GEOS-Chem to CMAQ IC/BC species mapping table

CMAQ CB-IV species	GEOS-CHEM species
[NO_2]	[NO_x]
[O_3]	[O_x]-[NO_x]
[N_2O_5]	[N_2O_5]
[HNO_3]	[HNO_3]
[PNA]	[HNO_4]
[H_2O_2]	[H_2O_2]
[CO]	[CO]
[PAN]	[PAN] + [PMN] + [PPN]
[MGLY]	[MP]
[ISPD]	[MVK] + [MACR]
[NTR]	[R_4N_2]
[FORM]	[CH_2O]
[ALD2]	1/2[ALD2] + [RCHO]
[PAR]	[ALK4] + [C_2H_6] + [C_3H_8] + [ACET] + [MEK] + 1/2 [PRPE]
[OLE]	1/2 [PRPE]
[ISOP]	1/5 [ISOP]
[SO_2]	[SO_2]
[NH_3]	[NH_3]
[ASO_4J]	[SO_4]
[ANH_4J]	[NH_4]
[ANO_3J]	[NIT] + [NITs]
[AECJ]	[BCPI] + [BCPO]
[AORGPAJ]	[OCPI] + [OCPO]
[AORGBJ]	[SOA1]+[SOA2]+[SOA3]+ [SOA4]

Due to the lack of all species from GEOS-Chem, profile boundary information were used as supplement for missing species. Profile boundary condition contains average concentrations on all required CMAQ species.

Added:

(pg 5 ln 16-20)

For GEOS-Chem downscaling, the GEOS-Chem outputs were undergone time-step interpolation, vertical and horizontal interpolations, chemical species conversion, appending chemical species, and unit conversion to achieve CMAQ model-ready initial and boundary conditions. Details of the downscaling methodology were described in (Lam and Fu).

Page 2192:

1)Biogenic emissions models: As I mentioned earlier, the authors need to be explicit about which version of BEIS that was used.

2) 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? How were fires included in the simulations? How were increases in fire emissions determined for the future? This should be more clear- since this is a big component of the future simulations.

3) 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.

Response:

1)The abbreviations for BEIS and MEGAN in the text has been modified to BEIS3 and MEGAN2 to provide additional information on the version it used. This removes the confusion raised related to the biogenic models.

2)For biomass burning, the IPCC considers only those emissions which are directly related to human activities. It does not include any natural wildfire. For example, if forest burning is purposely executed for expanding agricultural land (*managed forest* in IPCC terminology), then it is included in IPCC. Emission projection from biomass burning in IPCC A1B is certainly decreased. However, for this study, we have used the IMAGE *managed forest* projections with supplemented estimates of wildfire emissions from the *mature forest* projections. We assumed that the increase in *mature forest* in future is proportional to increase in wildfire emissions. Therefore, the future biomass burning is increased from the projection.

3)The reason for under predicting the peak percentile of the PM distribution is due to our emissions are based on the year 2000. However, the observational data is from 1998-2002. There are several incidents of large fires in United States (i.e., 1998 and 2002). Therefore, our output compares well with the mean of the data, but fails to compare with peak percentile.

Added:

(pg 6 ln 35-38)

The growth of these emissions was mainly contributed by the IMAGE managed forest projections, where supplemented estimates of wildfire emissions from the mature forest were used.

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 the trees. For example, which algorithms were used (from Guenther et al., 2006) and which emission factor maps were used, etc.

Response: I totally agree what the comment with the referee. However, the comparison of those two models has been documented elsewhere. The fact that we have obtained our MEGAN model v2.02 from Sakulyanontvittaya et al, we have modify the text to explicitly reference his publication in the text. We are not planning to discuss all biogenic processes involved in those two models in details, but we will certainly to incorporate some of the discussions of LAI into the text. Since LAI is a very important variable in the models.

Here is the table from Sakulyanontvittaya et al

Sakulyanontvittaya, T., Duhl, T., Wiedinmyer, C., Helmig, D., Matsunaga, S., Potosnak, M., Milford, J., and Guenther, A.: Monoterpene and sesquiterpene emission estimates for the United States, Environ. Sci. Technol., 42, 1623-1629, 2008.

TABLE 1. Mean and Standard Deviation of Emissions Factors (EF) by PFT, for SQT and MT compounds in EF-S06 and EF-D06[a]

emission cases	BT EF (μg m^{-2} h^{-1})	NT EF (μg m^{-2} h^{-1})	SB EF (μg m^{-2} h^{-1})	GC EF (μg m^{-2} h^{-1})
		Sesquiterpenes		
EF-S06	(37)	(40)	(18)	(18)
α-farnesene	22.2 ± 46.3	14.2 ± 18.6	1.8 ± 4.2	21.2 ± 40.4
β-caryophyllene	18.6 ± 38.8	12.1 ± 15.8	4.2 ± 9.7	25.4 ± 48.3
other sesquiterpenes	46.7 ± 97.5	55.0 ± 71.7	21.5 ± 49.8	55.1 ± 104.8
total sesquiterpenes	87.5	81.3	27.6	101.8
EF-D06	(12)	(1)	(1)	(9)
α-farnesene	76.1	78.7	20.0	20.9
β-caryophyllene	63.7	67.0	45.7	25.0
other sesquiterpenes	160.2	304.2	234.3	54.1
total sesquiterpenes	300.0	450.0	300.0	100.0
		Monoterpenes		
EF-S06 and EF-D06	(43)	(35)	(12)	(18)
myrcene	22.1 ± 46.6	85.8 ± 112.0	20.9 ± 35.5	5.6 ± 18.2
sabinene	14.3 ± 30.2	41.9 ± 54.6	17.3 ± 29.4	8.0 ± 26.1
limonene	40.7 ± 85.7	98.9 ± 129.0	173.9 ± 295.6	41.5 ± 135.4
3-carene	5.0 ± 10.6	43.5 ± 56.8	6.1 ± 10.4	17.2 ± 56.1
trans- and β-ocimene	134.4 ± 283.3	3.9 ± 5.1	103.0 ± 175.1	14.3 ± 46.6
β-pinene	40.6 ± 85.6	91.7 ± 119.6	45.0 ± 76.6	21.9 ± 71.6
α-pinene	36.1 ± 76.0	225.9 ± 294.8	51.2 ± 87.0	57.2 ± 186.9
other monoterpenes	155.9 ± 328.7	281.2 ± 366.9	318.3 ± 541.2	158.1 ± 516.1
total monoterpenes	449.2	872.6	735.8	323.7
biomass density (gdw m^{-2})	500	750	500	500

[a] Values in parentheses are the number of samples. BT, NT, SB, and GC represent broadleaf tree, needle leaf tree, shrub-bush, and grass-crop categories, respectively.

Added:

(pg 7 ln 36-40)

These emission differences are mainly resulted from the differences in methodology, PTF, LAI and emissions factors used in the models. Arneth et al. (2007) suggested that the BEIS3 isoprene emission factor was significantly low which caused the underestimation of total isoprene emission in BEIS3.

---

1)What about increases in future LAI? CO2 inhibition? Was any of this considered?

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

Response:

1)Here is from the text: "We assumed the same land use and vegetation patterns as 2000 on all years and all scenarios. "

We didn't consider neither increases in future LAI nor CO2 inhibition.

Added:

(pg 7 ln 14-15)

We assumed the same land use and vegetation patterns as 2000 on all years and all scenarios. These include the same leaf area index (LAI) and plant functional type (PFT) as well.

Response:

2) Yes, it is important to clarify that at the beginning of the discussion.

Before,

In response to the change of greenhouse gases, the future mean surface temperature was predicted to increase 1.0–2.0 K when compared to the present, as shown in Table 5.

After,

In response to the change of greenhouse gases, the results from regional MM5 show that the future mean surface temperature was predicted to increase 1.0–2.0 K when compared to the present, as shown in Table 5.

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)?

Response:

From Aw et al. He stated that

"The direct effect of intraannual temperature variability on ozone and PM2.5 concentrations at the urban scale was simulated using a high-resolution air quality model that tracks the temperature-dependant formation of secondary organic and inorganic aerosol components. Calculations show that the concentration of ozone and non-volatile secondary particulate matter will generally increase at higher temperatures due to increased gas-phase reaction rates. The concentration of semi-volatile reaction products also will increase at higher temperatures, but the amount of this material that partitions to the particle-phase may decrease as equilibrium vapor pressures rise."

Response:

The CASTNET data was selected due to the consideration of the regional analysis for average regional impact. Since the purpose of CASTNET data is to determine trends in regional atmospheric nitrogen, sulfur, and ozone concentrations (primary goal for CASTNET), it is more appropriate to use CASTNET.

In fact, the AQS data contains both rural and urban observational points. It is purposely sampled to assess air quality, assist in Attainment/Non-Attainment designations, evaluate State Implementation Plans for Non-Attainment Areas, perform modeling for permit review analysis, and other air quality management functions, which may be less suitable for our purpose.

The main difference between CASTNET and AQS is the urban observational sites. For example, AQS may have an observational site downstream from an industrial plan, which is mainly used for permitting purpose. It is a fact that the CASTNET data is lower than the AQS data, as shown in Zhang et al. (2008). The difference is about 3–5 ppbv on average for O_3.

Table 2. Observed and Simulated Variables for the Current-Climate Summer[a]

Variables	Network[b]	Sample Number	Mean Observation[c] (μg m^{-3})	Mean Simulation[c] (μg m^{-3})
Max 1-h O_3	CASTNET	13807	59.9	55.5
	AIRS-AQS	199695	61.3	60.0
Max 8-h O_3	CASTNET	13616	53.9	52.5
	AIRS-AQS	198598	53.4	55.8
$PM_{2.5}$	IMPROVE	7314	8.6	10.2
	STN	5249	14.8	18.5
NH_4^+	IMPROVE	159	2.0	2.7
	STN	6389	2.4	2.0
	CASTNET	1802	1.4	1.7
NO_3^-	IMPROVE	7333	0.3	0.1
	STN	6389	1.9	0.3
	CASTNET	1802	0.4	0.1
SO_4^{2-}	IMPROVE	7346	2.5	4.0
	STN	6389	5.3	8.2
	CASTNET	1802	4.8	7.2
BC	IMPROVE	6919	0.3	0.3
OM	IMPROVE	6938	2.6	3.2
DCV_Mie[d]	IMPROVE	6778	15.1	13.7
DCV_Recon[d]	IMPROVE	6778	15.1	13.9
EXT_Mie[d]	IMPROVE	6778	48.1	56.8
EXT_Recon[d]	IMPROVE	6778	48.1	59.2
Precipitation	NADP	3172	22.5	20.6
Wet Depo NH_4^+	NADP	2434	0.5	0.9
Wet Depo NO_3^-	NADP	2434	1.8	1.2
Wet Depo SO_4^{2-}	NADP	2434	1.9	4.9

[a]Current-climate summer is June, July, and August (JJA). The values for the current-climate summer are the average values of JJA 2001 (C01_Base) and JJA2002 (C02_Base).

[b]CASTNET, the Clean Air Status and Trends Network; AIRS-AQS, the Aerometric Information Retrieval System–Air Quality System; IMPROVE, the Interagency Monitoring of Protected Visual Environments; STN, the Speciation Trends Network; and NADP, the National Acid Deposition Program.

[c]The units are ppb for O_3 mixing ratios, μg m^{-3} for $PM_{2.5}$ and its composition, deciview for haziness, M m^{-1} for extinction coefficient, mm for precipitation, and g ha^{-1} for wet deposition.

[d]Haziness (DEV) and extinction coefficient (EXT) are calculated in CMAQ on the basis of two methods: an approximation to Mie theory and a mass reconstruction technique, referred to as DEV_Mie and EXT_Mie, and DEV_recon and EXT_recon, respectively.

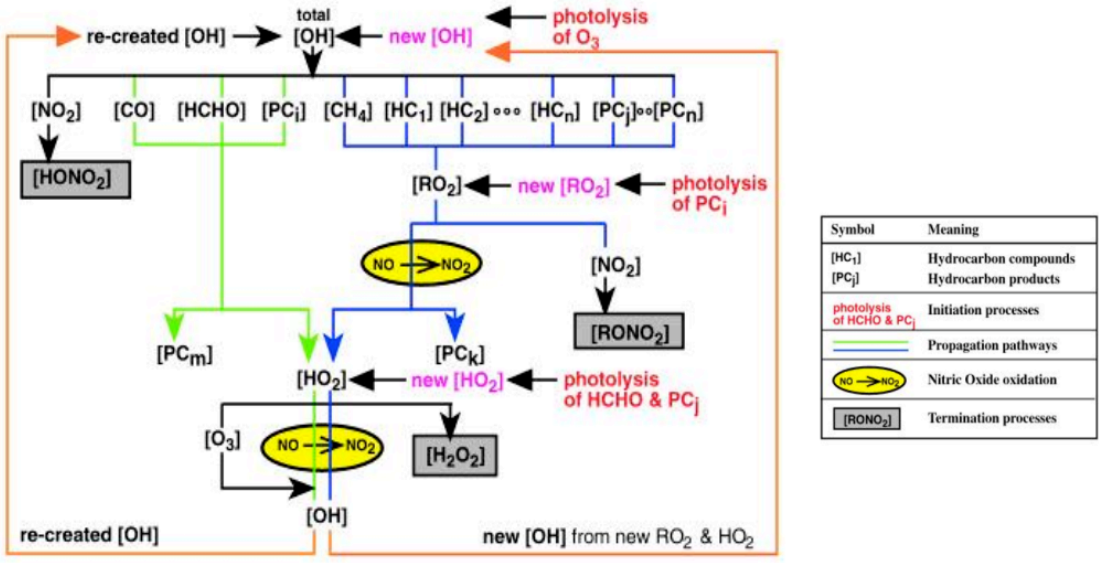
Zhang, Y., Hu, X.-M., Leung, L. R., and Gustafson Jr, W. I.: Impacts of regional climate change on biogenic emissions and air quality, J. Geophys. Res.-Atoms., 113, D18310, doi:10.1029/2008JD009965, 2008.

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).

Response:

The NOx and CO has large impacts on regional ozone due to the influential role of affecting the concentration of hydroxyl radical. The figure below shows the relationship between O_3, NOx, CO and OH in CMAQ. If the concentration of NOx and CO are not right in the model, it would not give you sufficient ozone. Similar statement is also cited by Zhang et al. (2008).

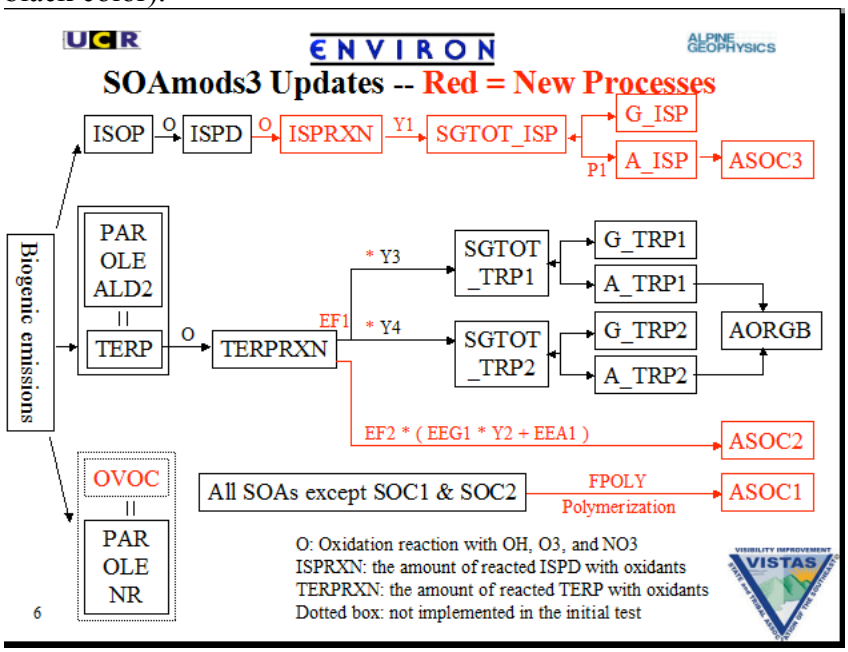
Chemical Cycle of O_3 and OH



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 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.

Response: We have rephrased that and have been removed the term "As expected" in the text.

In this study, we discussed two air pollutants, ozone and $PM_{2.5}$. In fact, a large portion of the discussion in the biogenic emission was dedicated to the discussion of increase of isoprene in the future and the differences in isoprene emission between BEIS3 and MEGAN2. The increase of isoprene affects the future ozone concentration, particularly in the Northern US where the VOC is the limited factor for increasing ozone. For $PM_{2.5}$, the majority discussion was focused on the biogenic terprene emissions and the reduction of the future SO_2 emission. The isoprene was not discussed a lot and was not blamed for increasing $PM_{2.5}$ in the Southeastern US. Since the version of CMAQ used in this study did not include isoprene aerosol mechanism, as shown below (Our version is the one with black color).



source: VISTAS

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.

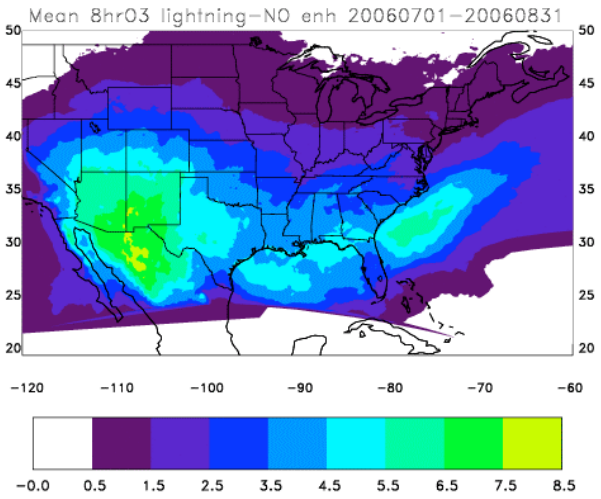
Response:

I agree with the referee that many parameters can affect the differences between two models:

1) meteorological parameters (i.e., PBL, wind speed, etc.)
2) chemical mechanism
3) sensitivity of ozone under the coarse resolution
4) lightening NOx (CMAQ does not have lightning NOx in the simulation)

In GEOS-Chem, the additional NOx produced at the upper layer from lightning has increased the average concentration of NOx in the upper layer, which eventually affects the NOx concentration at the surface. Research has been showed that with or without the effects of lightning NOx from CMAQ can be up to 8.5 ppbv for MDA8. (Allen et al., 2010)

Allen, D., Pickering, K., Pinder, R., Henderson, B., Koshak, W., and Pierce, T.: Impact of Lightning-NO Emissions on Eastern United States Photochemistry During the Summer of 2006 as Determined Using the CMAQ Model, The 9th Annual CMAS Conference, Chapel Hill, NC, 11-13 October, 2010.



In the text, we said "these discrepancies may be partially related to the lack of lightning emissions in CMAQ since we did not implement lightning as a source of NOx in the upper troposphere for either the present or future climate condition" The term *partially* has been used. We think it is a reasonable statement for the difference in maximum ozone.

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1) Page 2206, line 26: why are future climate and air quality predictions in the Southeast controversial? References? Examples? Section 3.5: What do the authors mean that the GEOS-Chem did not include "sufficient" PM2.5 species at the moment?

2) 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?

3) 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?

Response:

1) "Nevertheless, the future climate and air quality predictions in the Southeast have been controversial" has been removed from the text.

The reason for including it in the text was because temperature predictions among different global climate models have shown inconsistent. I can't find the publication which states that. So, I have removed this sentence from the text.

All the outputs are based on 24-h average $PM_{2.5}$.

Response:

2) The table from Lam et al. (2010) show the mapping table between CMAQ and GEOS-Chem for GEOS-Chem v8-03-01. The last species, which is AORGBJ, was missing at the GEOS-Chem v7.03.06.

Appendix A

GEOS-Chem to CMAQ IC/BC species mapping table

CMAQ CB-IV species	GEOS-CHEM species
[NO_2]	[NO_x]
[O_3]	[O_x]-[NO_x]
[N_2O_5]	[N_2O_5]
[HNO_3]	[HNO_3]
[PNA]	[HNO_4]
[H_2O_2]	[H_2O_2]
[CO]	[CO]
[PAN]	[PAN] + [PMN] + [PPN]
[MGLY]	[MP]
[ISPD]	[MVK] + [MACR]
[NTR]	[R_4N_2]
[FORM]	[CH_2O]
[ALD2]	1/2[ALD2] + [RCHO]
[PAR]	[ALK4] + [C_2H_6] + [C_3H_8] + [ACET] + [MEK] + 1/2 [PRPE]
[OLE]	1/2 [PRPE]
[ISOP]	1/5 [ISOP]
[SO_2]	[SO_2]
[NH_3]	[NH_3]
[ASO_4J]	[SO_4]
[ANH_4J]	[NH_4]
[ANO_3J]	[NIT] + [NITs]
[AECJ]	[BCPI] + [BCPO]
[AORGPAJ]	[OCPI] + [OCPO]
[AORGBJ]	[SOA1]+[SOA2]+[SOA3]+ [SOA4]

This is what it has changed in the text

Before,

For $PM_{2.5}$, since the GEOS-Chem v7.03.06 did not incorporate sufficient $PM_{2.5}$ species and did not provide separation between $PM_{2.5}$ and $PM_{2.5\ to\ 10}$ at the moment, no $PM_{2.5}$ comparison between GEOS-Chem and CMAQ will be presented. The maximum and average $PM_{2.5}$ from CMAQ were ranged from 96.1 to 127 $\mu g/m^3$ and 4.5 to 11.7 $\mu g/m^3$, respectively.

After,

(pg 15 ln 36-40)

For $PM_{2.5}$, no comparison between GEOS-Chem and CMAQ will be presented since parts of the secondary organic species and the breakdown of $PM_{2.5}$ and $PM_{2.5\ to\ 10}$ were missing from GEOS-Chem v7.03.06. In CMAQ, the 1-h maximum and annual average $PM_{2.5}$ among different domains and scenarios were ranged from 96.1 to 127 $\mu g/m^3$ and 4.5 to 11.7 $\mu g/m^3$, respectively.

Response:

3) This table shown the summary of $PM_{2.5}$ concentrations. Max – hourly maxima and Avg – annual average.

PM2.5 type	Domain	2000M_2000E	2000M_2050E	2050M_2000E	2050M_2050E
Max	MW	127	130.2	96.1	102
	NE	119.3	116.8	116.6	117.7
	SE-36km	131.4	133.9	107.5	108.5
	SE-12km	115.7	104	105.3	102.5
Avg	MW	8.2	4.7	8.7	5
	NE	11.6	7.1	11.7	6.7
	SE-36km	11	6.1	10.9	5.4
	SE-12km	10.4	5.7	10.5	5.1

It is a combine effect. More VOC emissions (i.e., BVOC) were produced by the increase of temperature. Therefore, more $PM_{2.5}$ was generated. On the other hand, $PM_{2.5}$ may also be vaporized or broken down and further react with NO_x to form ozone. The result from these effects was "more $PM_{2.5}$ was produced."

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.

Response: The changes have been incorporated into the text.

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

Response: Fixed

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

Response: Yes, it is surface temperature.

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

Response: It has been modified to "the IPCC A1Fi (i.e., fossil intensive) and the B1 (i.e., clean technology intensive) scenarios."

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

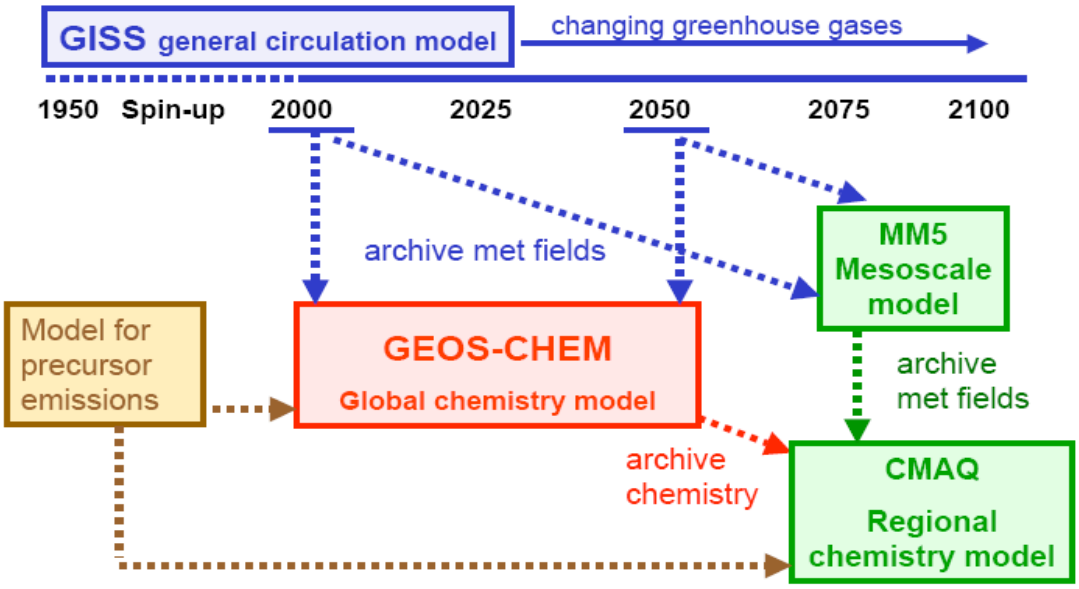
Response: SARMAP air quality model. Reference has been added to the text and SARMAP is also incorporated into the text.

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

Response: Fixed

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

Response:



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

Response: Fixed

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 theradiation scheme? Please be more explicit.

Response:

The term "fully coupled" has been changed to "coupled". It is a chemistry coupling between NOx-VOC chemistry and aerosols, not meteorological coupling. If it is a chemistry coupled with the meteorology, it usually refers to "two-way coupling".

"GEOS-Chem (v7.03.06) was used, which includes a coupled treatment on tropospheric ozone-NOx-VOC chemistry and aerosols."

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

Response: Fixed

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

Response: Fixed

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

Response: Fixed

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

Response: Fixed. See Table 2 for correction.

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

Response: Fixed.

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

Response: Fixed.

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

Response: Fixed.

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

Response: Fixed.

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

Response: Fixed.

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

Response: Fixed.

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

Response: Fixed.

The reference Xiaoyan et al., 2008 should be Jiang et al. (2008). Please check the reference for Zhang et al. (2008)

Response: The reference has been changed to:

Jiang, X., Wiedinmyer, C., Chen, F., Yang, Z.-L., and Lo, J. C. F.: Predicted impacts of climate and land use change on surface ozone in the Houston, Texas, area, J. Geophys. Res.-Atoms., 113, D20312, doi:10.1029/2008JD009820, 2008.

Zhang, Y., Hu, X.-M., Leung, L. R., and Gustafson, W. I., Jr.: Impacts of regional climate change on biogenic emissions and air quality, J. Geophys. Res.-Atoms., 113, D18310, doi:10.1029/2008JD009965, 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?

Response: The original table contains shaded areas and a dot frame. It was removed after editorial page prove (see below table).

In this study, we intended to study three different issues: 1) the effect of using different biogenic models for the climate change study, 2) the air quality effect from climate change in Eastern United States, and 3) spatial resolution effect on climate change study.

In order to minimize number of CMAQ simulations required for the study, we have integrated all three scenarios.

First, we ran one present year (2000) and one future year (2050) of CMAQ simulations on CONUS 36 km domain for each biogenic model (MEGAN and BEIS) to investigate the effects of using different biogenic models in the climate change study.

Second, we ran one present year and one future year of simulation on the VISTAS 12 km with MEGAN emissions. Now we would have a total of two years of CMAQ outputs (one year from present climate and one year from future climate) for both 36 km and 12 km domains to investigate the effects of the spatial resolution.

Finally, we ran additional two years of present climate (1999 and 2001) and two years of future climate (2049 and 2051) of CMAQ simulations using MEGAN emissions. Now, we would have a total of 6 years of CMAQ outputs from the MEGAN emission scenario to investigate the effect of climate change on air quality.

Scenario			Model			Scenario Index
Meteorology	Anthrop. Emission	Bio. Emission	GEOS-Chem	CMAQ		
1999-2001	2000	MEGAN (1999-2001)	4° x 5°	36km x 36km	12km x 12km*	2000M_2000E_M
1999-2001	2050	MEGAN (1999-2001)	4° x 5°	36km x 36km	12km x 12km*	2000M_2050E_M
2049-2051	2000	MEGAN (2049-2051)	4° x 5°	36km x 36km	12km x 12km*	2050M_2000E_M
2049-2051	2050	MEGAN (2049-2051)	4° x 5°	36km x 36km	12km x 12km*	2050M_2050E_M
2000	2000	BEIS (2000)	-	36km x 36km	-	2000M_2000E_B
2000	2050	BEIS (2000)	-	36km x 36km	-	2000M_2050E_B
2050	2000	BEIS (2050)	-	36km x 36km	-	2050M_2000E_B
2050	2050	BEIS (2050)	-	36km x 36km	-	2050M_2050E_B

* Only 2000 and 2050 cases were simulated

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

Response: Dash boxes have been drawn for CONUS 36 km and VISTAS 12km domains to give more descriptive information in Fig. 1. The title of the figure has also been changed.

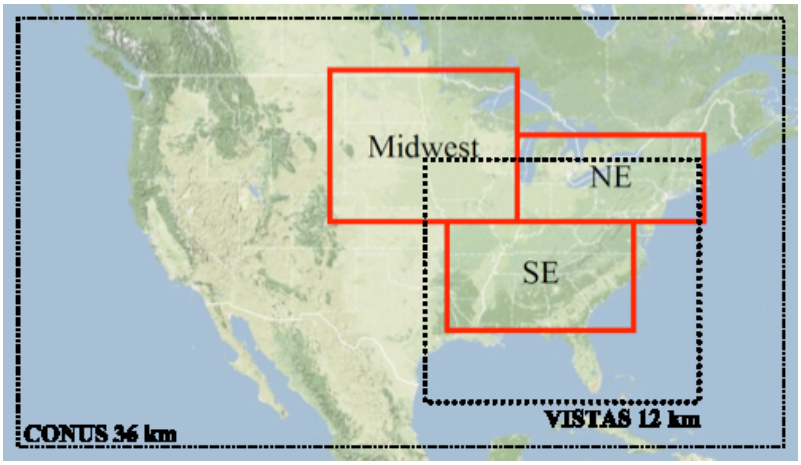


Fig. 1. The CONUS 36 km and VISTAS 12 km domains with selected study areas boxed in red (the dark green color on the map indicates locations of vegetation).

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

Response: Two things have been added in the manuscript. First, we added a unit for the PBL height. It should be in "meter" or "m" (circled in red in the Fig. 3 below). Moreover, "Average change" has been modified to "Average seasonal change" in Fig. 3.

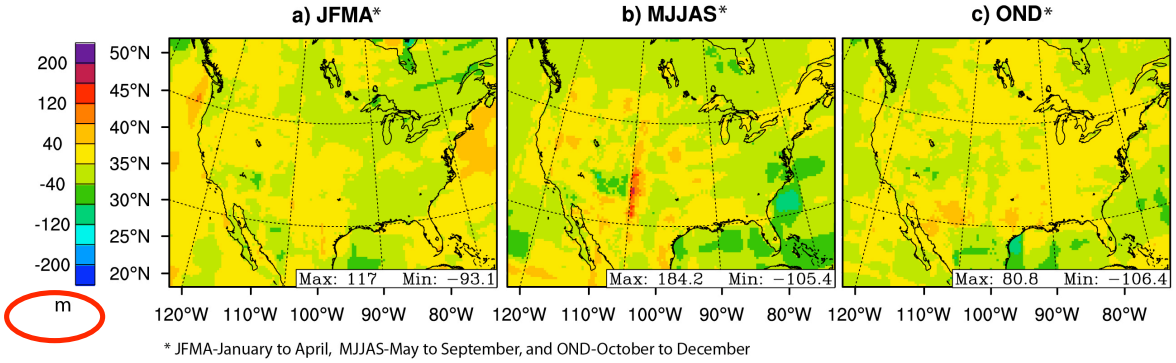


Fig. 3. Average seasonal change in PBL height difference between 2049-2051 and 1999-2001 from MM5 outputs: **(a)** JFMA, **(b)** MJJAS, and **(c)** OND.