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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Average versus high surface ozone levels over the continental U.S.A.: Model bias, background influences, and interannual variability" *by* Jean J. Guo et al.

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Average versus high surface ozone levels over the continental U.S.A.: Model bias, background influences, and interannual variability, Jean Guo et al., ACP, (2018) Please see supplemental document for formatted version of the response

Author response to Reviewer #1 This manuscript presents an attempt to derive information about mean maximum daily 8-hour average (MDA8) O3 in the United States, based on ambient measurements and using the global model GEOS-CHEM. Sensitivity simulations examine different sources that affect the 10 highest O3 events and that affect the 10 days with highest model bias against observations for 2004 to 2012 for

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each 10 EPA regions. General comments: The analysis is a valuable contribution to the current understanding of ground level O3 and air quality standard settings. The topic itself is highly relevant and thus will be of interest to the readers of ACP. Discussion of the results and their implications is also scientiïňĄcally sound and the paper includes comprehensive analyses. However, I feel that the paper tried to cover lots of information, which makes it a bit hard for the reader to follow key conclusions from this study. Thus, I recommend that the paper should be published after addressing the following comments.

General

Some comment about day of week effects and model biases in temperature as they relate to the questions raised in the paper seem warranted. There should be comment dramatic changes in the temperature dependence of ozone over this period coincident with the NOx changes. Those changes should have a day of week variation that might appear in the top 10 days.

We have addressed the biases in temperature by adding a comparison to the Global Historical Climatology Network Global Historical Climatology Network (GHCN) and the Climate Anomaly Monitoring System (CAMS). See Supplemental Table 4 and the associated discussion in the text (lines 363-364): “The model monthly mean temperatures in the model (from the MERRA reanalysis) closely match the observed GHCN+CAMS dataset (Supplemental Table 4).”

The request to investigate day of week effects substantially widens the scope of the paper. The general comments suggest that the manuscript is already covering too much information. We feel that tackling day of week effects and its changes over time is a study unto itself and thus outside the scope of this particular paper.

SpeciïňĄc comments: The authors use terms “Baseline O3” and “U.S. background O3”. U.S. background O3 is deïňĄned as “the O3 levels that would exist in the absence of U.S. anthropogenic emissions of precursors” and Baseline O3 is deïňĄned as “tropo-

spheric O3 concentrations that have a negligible iniňĆuence from local anthropogenic emissions". They sound the same, don't they? If yes, please be consistent in the text.

These definitions are not the same. Clarification has been added in lines 105-107. "Baseline O3 is a measurable quantity and differs from background O3 in that it contains some influence from U.S. anthropogenic emissions that were not recently emitted but contributed to the global background." We follow here the definitions of Jaffe et al., 2018 which builds on the 2009 National Academies report "Global Sources of Local Pollution", and the HTAP 2010 report (available at www.htap.org).

Page 4, lines 106-109, Please clarify if the authors apply Schnell et al. (2014)'s interpolation procedure or they use their dataset. Schnell et al. (2014) use surface MDA8 O3 measurements from air quality networks for 2000–2009, while this paper analyzes the data from 2004-2012.

Jordan Schnell is a co-author and provided the dataset that we used here. The interpolation procedure for his dataset is described in his 2014 paper; he provided us with the data for the years since 2009. We have edited the citation to reflect a newer paper in which this extended dataset has been used. See lines 118-120: "we use an available 1° x 1° grid of surface MDA8 O3 measurements that were interpolated from the AQS, CASTNet, and Canadian NAPS networks (Schnell and Prather, 2017)."

A valuable addition would be a statement about the chemistry scheme applied in the version GEOS-Chem at the 2.3 section (GEOS-Chem model simulation). The authors mention issues of isoprene chemistry in last paragraph of Conclusions but a brief description or reference to the speciïňĄc version of the chemistry should be presented before the last paragraph of the paper.

On lines 158-160 we now state: "We use the standard v9_02 chemical mechanism which includes recycling of isoprene nitrates (Mao et al., 2013) in contrast to the mechanisms used in earlier versions of GEOS-Chem (e.g., Zhang et al., 2014 as discussed in Fiore et al., 2014)."

The last paragraph of page 6 needs elaboration where the authors state the sensitivity simulations. The notations for all model simulations should be mentioned and the description of Table 1 should be modiïňĄed so that the Table is read from top to bottom. We have completely rewritten this section with the intent of improving clarity. See paragraph starting from line 183 ("We first perform a base simulation. . .").

Figure 3: Observed O3 concentrations should be represented in a different color to be more visible (maybe black instead of grey) and I would also suggest to plot the curves as an average for 2004-2012 period with associated error bars. Thanks for this suggestion. We have edited the figure (now Figure 2) to show the curves as an average for 2004-2012 period with associated error bars.

Minor comments: The tables start from Table 2 at the manuscript and Table 1 is referenced at Page 8 for the ïňĄrst time. Please ïňĄx ordering of table numbers as they appear in the text. Fixed Page 7, line 195: "a maximum in and" should read "a maximum in summertime and" Thank you. Section has been edited and this sentence was removed.

Author response to Reviewer #2

General:

The paper is very well-written and concerns a topic of considerable interest to air quality planners. However, there are some concerns about the suitability of this particular model conïňĄguration to address some of the stated objectives of the paper (lines 76-79), as discussed below. In general, the paper would be improved if there was greater clarity about the potential connections between the ïňĄndings and possible conïňĄguration concerns. The value of the paper would be enhanced if the conclusions section was bolstered with a "next steps" or "considerations" sentence or two that described how such a global model-based sensitivity study could be improved in the future.

We have attempted to strengthen the paper throughout as suggested by the reviewer.

In particular, we added a sentence in the introduction, (lines 77-80) to highlight the key benefit and drawback of using a coarse resolution model: “Though coarse resolution global models such as GEOS-Chem will mix emissions into the same grid cell that may remain separate in the real atmosphere, a global model is necessary to quantify background O3 transported intercontinentally, including that produced via oxidation of methane.” We added a sentence in the conclusion (lines 476-481) to emphasize a need to confirm our findings with finer scale models: “Future work with high-resolution models (e.g., at the regional scale, ideally with boundary conditions that include source attributions from a global model) is needed, along with observational evidence, to quantify the extent to which biogenic VOC and NOx contribute to the highest observed O3 levels in the warm season. The importance of temperature sensitive sources like biogenic VOC and NOx emissions to background O3 imply that in a warmer climate, these background influences on O3 will play an even more important role in driving up O3 levels.”

In particular, there is concern about the use of a coarse resolution model (2 x 2.5 deg) to investigate contributions of U.S. anthropogenic emissions (O3_USA) given that those contributions originate at scales much smaller than the resolution of the model (i.e., point source emissions, urban area emissions). The paper acknowledges the limitations associated with the coarse modeled resolution in several places (lines 212, 242, 399). The paper may want to revisit these caveats in the conclusion and perhaps provide some thoughts on what alternate global model conïňĄgurations would be better suited for an analysis of source contributions.

Agreed. Please see above.

Kudos to the authors for providing sufiňĄcient detail regarding the performance evaluation to allow readers to interpret the contribution ïňĄndings in light of the model bias/error. However, the ozone overestimations (3-14 ppb in JJA MDA8 top 10 days by region, even worse for JJA all-day averages) suggest caution should be exercised in overinterpreting the contributions. Based on Figure 5 and the associated analyses,

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it appears that the model vastly overestimates ozone on hot days in the late summer, especially in the eastern U.S. (even without consideration of potential additional emissions due to increased power demand on those days). Section 3.3 brieïňĆy summarizes potential causes for this overestimation based on similar studies, but it would be valuable if the paper provided more application-speciïňĄc hypotheses for the underlying cause.

We have added more discussion of the potential for biases in the meteorology (see responses to reviewer #3), as well as in anthropogenic NOx emissions to contribute to the summertime overestimate in the model compared to observations. As our set of sensitivity simulations identifies a potential role for biogenic VOC and soil NOx in contributing to the bias, we have added to the text some discussion calling out the need for better constraints on these biogenic emissions, though we do note that the model nevertheless shows some skill at capturing the observed year-to-year variability, which includes a correlation with OÂň3 produced from natural sources (BVOC and soil NOx), which, like total OÂň3, correlate with temperature (Figure 6). We now state, in lines 441-449 “Our finding that BVOC emissions contribute to the summertime surface O3 biases could reflect poor representation of the emissions (and subsequent oxidation chemistry). Earlier work has noted that MEGAN BVOC emissions are too high over California (Bash et al., 2016), Southeast Texas (Kota et al., 2015), the Ozarks in southern Missouri (Carlton and Baker, 2011), and across much of the U.S.A. (Wang et al., 2017). One recent model study uniformly reduced MEGAN isoprene emissions by 20% (Li et al., ACP 2018), but we did not apply any such scaling here. In regions that are highly NOx-sensitive, additional isoprene should not strongly influence O3, as found over southeast Texas (Kota et al., 2015). While not eliminated entirely, the summertime model bias does lessen in the simulation with BVOC emissions set to zero, suggesting that the O3 bias is indeed exacerbated if BVOC emissions are overestimated in the model.”

FYI, along w/ the possible causes from the Travis research, others have raised con-

cerns about MEGAN biogenic VOC estimates (e.g., Bash et al., 2016; Carlton and Baker, 2011; Kota et al., 2015; Wang et al., 2017). Thank you for pointing these out. We added these references (see previous response). One of the more noteworthy ïňĄndings concerns the modeled trends over the 10-year period (e.g., lines 386-389) where the analysis appears to conïňĄrm previous ïňĄndings that improving trends in U.S. air quality from emissions controls have been tempered by increases in background contributions (and increases in temperature). However, one interesting ïňĄnding here that could use additional explanation is the regional breakout of this "USB vs. USA" tradeoff. Table 5 suggests that the largest increases in high JJA-day O3_USB concentrations between 2004-2006 and 2010-2012 have occurred in the New England and Mid-Atlantic regions, not the western regions where USB concerns are typically greatest. More explanation of the regional differences in modeled USB trends would be beneïňĄcial (e.g., is this just an artifact of the meteorology of the two 3-year periods in these regions).

We agree. The "trends" over such a short period are strongly influenced by fluctuations in temperature. While it may indeed be an 'artifact' of looking at such a short period, it nevertheless suggests that regionally produced background O3 from temperature-sensitive emissions (BVOC and NOx) may grow in importance in the coming decades in light of a warming climate. We have attempted to make this clearer by adding a column to Table 4 that shows the change in temperature between these two 3-year periods in each region. We have edited the accompanying discussion to the main text:

Starting from line 364: "Table 4 shows that regions with O3_USB increases generally experienced rising temperatures over this period, as the 2010-2012 period includes two of the warmest years on record. Figure 6 shows that O3_NAT tracks with. . . "

In response to a comment from Reviewer 1, we have also added Supplementary Table 4 that evaluates monthly mean model temperatures with the Global Historical Climatology Network.

Given model performance ïňĄndings, would the authors see value in revising the “2-step” contribution analysis (assessing contributions on high-bias days, then assessing contributions on high/all observed days) to a “3-step” contribution where as an intermediate step you also investigated contributions on top-10 modeled days? This could be valuable presuming that the subset of days would differ from top 10 highest bias days.

Though we did conduct some exploratory analysis using this 3-step method early on, we did not end up pursuing this method in the paper because the highest model days are less relevant to the “real world” and if this method were used throughout our paper, the number of figures would have doubled. As the paper is already lengthy, we choose to focus on the days in the observations when the O3 NAAQS is most likely to be exceeded.

We have, however added text that clarifies the extent to which there is overlap between the highest 10 days in the model and the 10 days with the highest biases: “There is at most a 2-6 day overlap between the top 10 O3_Base days and the top 10 most biased days in 2004-2012 across all regions, but during most years, the overlap is around 0-2 days. We restrict our analysis to examining the top 10 observed O3 days as these days are most relevant from a policy perspective.” (lines 137-139).

Rather than lumping the Mount Bachelor observations (and subsequent pairs) with surface sites in Region 10, it would be interesting to see how model contributions varied as functions of model performance and observation concentration as a standalone site.

Thanks for this suggestion. We now include a more detailed analysis of Mount Bachelor as a separate standalone section (Section 3.2) that includes new figures (Figure 2 and supplemental figure 4).

SpeciïňĄc:

Line 86: “download” should be “downloaded”. FIXED

Lines 124-127: Would be easier to read, if a new sentence was started w/ “On the days

with . . .". FIXED Line 146: "Avian" should be "Aviation". FIXED

Line 195: Is the word "summer" missing from this sentence . . . "The model, however, has a maximum in [summer] and underestimates springtime baseline O3"? Thank you. Section has been edited and this sentence was removed.

Line 205: Are Travis et al. (2016) conclusions regarding 2011 NEI relevant to a model conïňĄguration based on 2005 NEI w/ annual scalars?

We add a comparison with Travis et al. (2016) in lines 226-230: "Travis et al. (2016) find that the 3.5 Tg N y-1 NEI 2011 estimate for U.S. fuel NOx emissions is too high and contributes to excessive surface O3. Our simulations include even higher U.S. fuel NOx emissions of 4.4 Tg N y-1 during 2010-2012 (Supplemental Table 3), implying that some portion of the model O3 bias reflects excessively high anthropogenic NOx emissions (Travis et al., 2016)."

Line 248: Clarify that these monthly averages are MDA8 O3 (not hourly)? FIXED

Line 367: Move mention of lack of daily variation in emissions to early section? Done. Now at lines 179-183)

Line 396: Same as above, maybe mention this earlier in modeling methodology section? See lines 179-183)

Author response to Reviewer #3

This paper presents a comprehensive modeling analysis of surface ozone and the various factors that contribute to its variability over the United States. By conducting multiple sensitivity simulation removing various sources for the 2004-2012 period, the authors estimate the inïňĆuence of different background sources and of U.S. anthropogenic sources on mean surface O3 and high O3 events as a function of region, season, and year.

Two aspects of the paper that I'd like to see more discussion on are listed below:

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1) The paper is very detailed with many ïňĄgures and tables and is one more study on top of a rich set of published work, including by some of the co-authors. The authors often cite previous work, saying it is consistent with their results, but it would be useful to highlight what are the new key contributions from their speciïňĄc analysis. What new information did the detailed modeling analysis bring to this proliïňĄc ïňĄeld? We added text with the intent of providing stronger motivation to the introduction in which we highlight the use of sensitivity simulations to help us identify which sources contribute most to the summertime bias and to the highest O3 days (lines 70-87). To our knowledge, the finding that increasing O3 production from temperature-sensitive biogenic emissions might be offsetting some of the gains achieved by reducing anthropogenic ozone precursor emissions is new, and potentially of growing importance as record-setting warm years have been increasing. We believe that our finding that the summertime bias is associated with regionally produced ozone – including both U.S. anthropogenic and components of U.S. background – rather than transported background (either internationally or intercontinentally) is also new. We have also rewritten the conclusions to emphasize these points.

2) There isn't much discussion on the causes of the large summer bias over the Eastern US and how this bias affects the interpretation of the results.

To our knowledge, prior studies have not used such a broad set of sensitivity simulations to interpret which sources are contributing most in places and times when the model is most biased against observations. Section 3.3 in the submitted paper is entirely devoted to addressing this point. We thus assume that the reviewer is instead driving at the deeper question of the specific causes of the bias, beyond what we can identify cleanly with the sensitivity simulations. We have added additional discussion in response to reviewer 2 that attempts to address both the causes and how it affects the interpretation of the results.

Specifically, we added

1) a sentence in the introduction, (lines 77-80) to highlight the key benefit and drawback of using a coarse resolution model: “Though coarse resolution global models such as GEOS-Chem will mix emissions into the same grid cell that may remain separate in the real atmosphere, a global model is necessary to quantify background O3 transported intercontinentally, including that produced via oxidation of methane.”

2) We also added a sentence in the conclusion (lines 476-481) to emphasize a need to confirm our findings with finer scale models: “Future work with high-resolution models (e.g., at the regional scale, ideally with boundary conditions that include source attributions from a global model) is needed, along with observational evidence, to quantify the extent to which biogenic VOC and NOx contribute to the highest observed O3 levels in the warm season. The importance of temperature sensitive sources like biogenic VOC and NOx emissions to background O3 imply that in a warmer climate, these background influences on O3 will play an even more important role in driving up OÂň3 levels.”

3) We now state, in lines 441-449 “Our finding that BVOC emissions contribute to the summertime surface O3 biases could reflect poor representation of the emissions (and subsequent oxidation chemistry). Earlier work has noted that MEGAN BVOC emissions are too high over California (Bash et al., 2016), Southeast Texas (Kota et al., 2015), the Ozarks in southern Missouri (Carlton and Baker, 2011), and across much of the U.S.A. (Wang et al., 2017). One recent model study uniformly reduced MEGAN isoprene emissions by 20% (Li et al., ACP 2018), but we did not apply any such scaling here. In regions that are highly NOx-sensitive, additional isoprene should not strongly influence O3, as found over southeast Texas (Kota et al., 2015). While not eliminated entirely, the summertime model bias does lessen in the simulation with BVOC emissions set to zero, suggesting that the O3 bias is indeed exacerbated if BVOC emissions are overestimated in the model.”

Discussing this in more detail would strengthen the paper. The authors have one sentence addressing this by referring to the work of Travis et al. (2016) using a more

recent version of the GEOS-Chem model. They mention potential errors in anthropogenic NOx emission in the NEI inventory, but Travis et al. use the NEI 2011 inventory while the authors use the NEI 2005 inventory. How different are they? If the NEI NOx inventory is indeed too high, how would that affect the calculation of O3_USA?

We now directly compare our NOx emissions to those used in Travis et al., 2016 and include a supplementary table providing the NOx emissions applied in each year within the U.S.A. and globally.

Lines 226-230: “Travis et al. (2016) find that the 3.5 Tg N y-1 NEI 2011 estimate for U.S. fuel NOx emissions is too high and contributes to excessive surface O3. Our simulations include even higher U.S. fuel NOx emissions of 4.4 Tg N y-1 during 2010-2012 (Supplemental Table 3), implying that some portion of the model O3 bias reflects excessively high anthropogenic NOx emissions (Travis et al., 2016).” They mention meteorological factors associated with boundary layer mixing and cloud cover which would affect the vertical distribution of O3, but Travis et al. used different meteorological ïňĄelds (GEOS-FP) compared to the MERRA ïňĄelds used by the authors. It is unclear whether these potential explanations apply in this case. If MERRA meteorology is indeed biased, then that would certainly affect the validity of the relative inïňĆuence of various sources on the “most-biased” days analysis and on the average MDA8 O3 levels. A discussion of this would be valuable.

Thanks for this suggestion. We have attempted to address this point by including more discussion of published evaluations of MERRA meteorology:

1) Lines 152-157: “MERRA meteorology captures summer mean surface temperatures to within 1-2 K across U.S. regions and precipitation to within 0.5 mm d-1 except for over the Northern Great Plains where a positive bias exceeds 1 mm d-1, but the variance in summer mean precipitation is lower than observed in some regions (Bosilovich, 2013). While interannual variability in cloudiness observed at weather stations is largely captured by MERRA, the reanalysis generally underestimates cloud cover and thus over-

estimates observed downward surface shortwave fluxes (Free et al., 2016)."

2) Lines 230-232: "The low bias in cloud cover in the MERRA meteorology and associated overestimate in downward shortwave surface radiation (Free et al., 2016) may also contribute to excessive O3 production in the model."

3) We also added our own evaluation of surface temperature over the U.S.A. in the MERRA fields (Supplemental Table 4).

Minor comments:

Line 154. "Anthropogenic emissions. . . are scaled each year on the basis of economic data". It would be useful to have a bit more discussion on how anthropogenic emissions are scaled over the continental U.S. which uses 2005 as the baseline. By how much do NOx emissions change over the time period of the simulation 2004-2012.

Supplemental Table 3 was added to provide the NOx emissions within each year, both globally and within the U.S.A. (Lines 178-183)

Are these scaling factors taken from the NEI trends report (https://www.epa.gov/air-emissions-inventories/air-pollutant-emissions-trends-data) itself or was independent estimate done?

The van Donkelaar et al., 2008 describes the standard GEOS-Chem emissions scaling reference. The scale factors use government statistics where available.

Edited this sentence (lines 169-170) to include "provided by individual countries, where available" Line 195. "a maximum in and underestimate springtime. . ." is "summer" missing after maximum?

Yes. Thank you

Line 196. While the authors talk about potential causes for the springtime underestimate (stratospheric intrusions), they do not talk about the summertime overestimate, which is quite large.

See response to general comments above and our additions above regarding anthropogenic NOx emissions and citations of prior work evaluating MERRA meteorology (temperature, precipitation and cloud cover).

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-115/acp-2018-115-AC1-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-115, 2018.

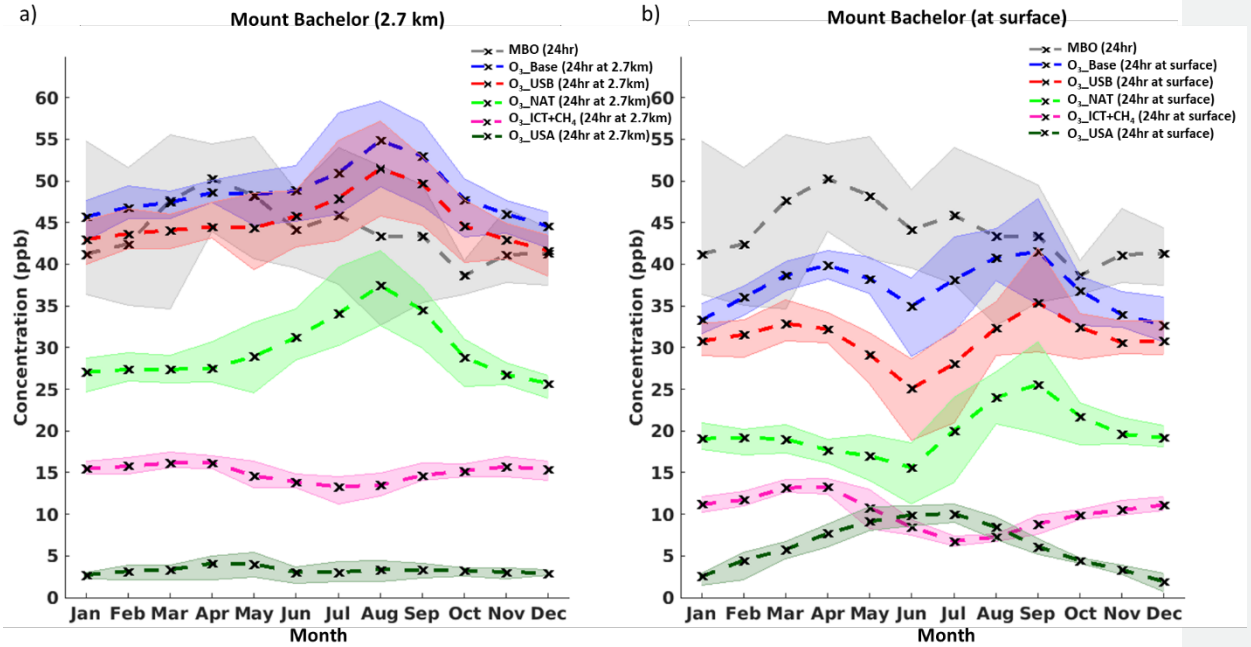

Fig. 1. Monthly 2004-2012 average 24-hour O3 concentrations at Mount Bachelor Observatory. Observations (grey) are the same in both panels. Simulations from the GEOS-Chem model are sampled in the grid cell co

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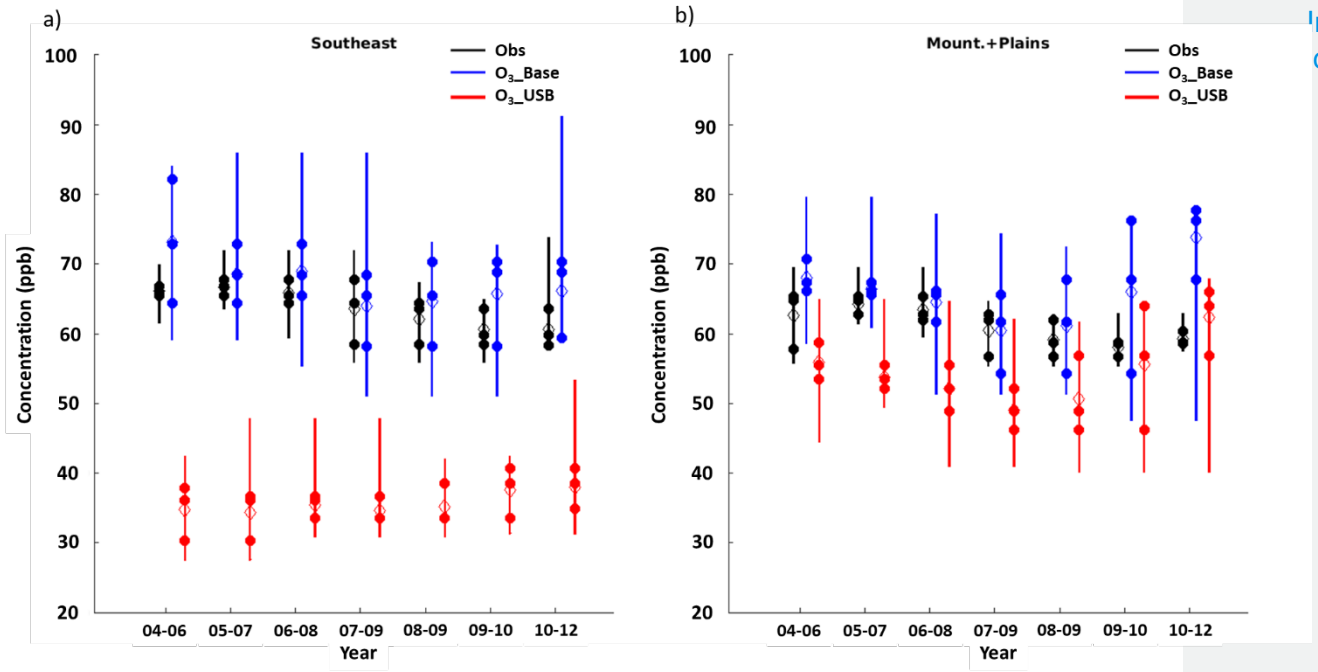

Fig. 2. The three 4th highest days in each year (solid dots) that went into the calculation of the three-year average of the 4th highest MDA8 O3 day (hollow diamond). Error bars show the range between the hig

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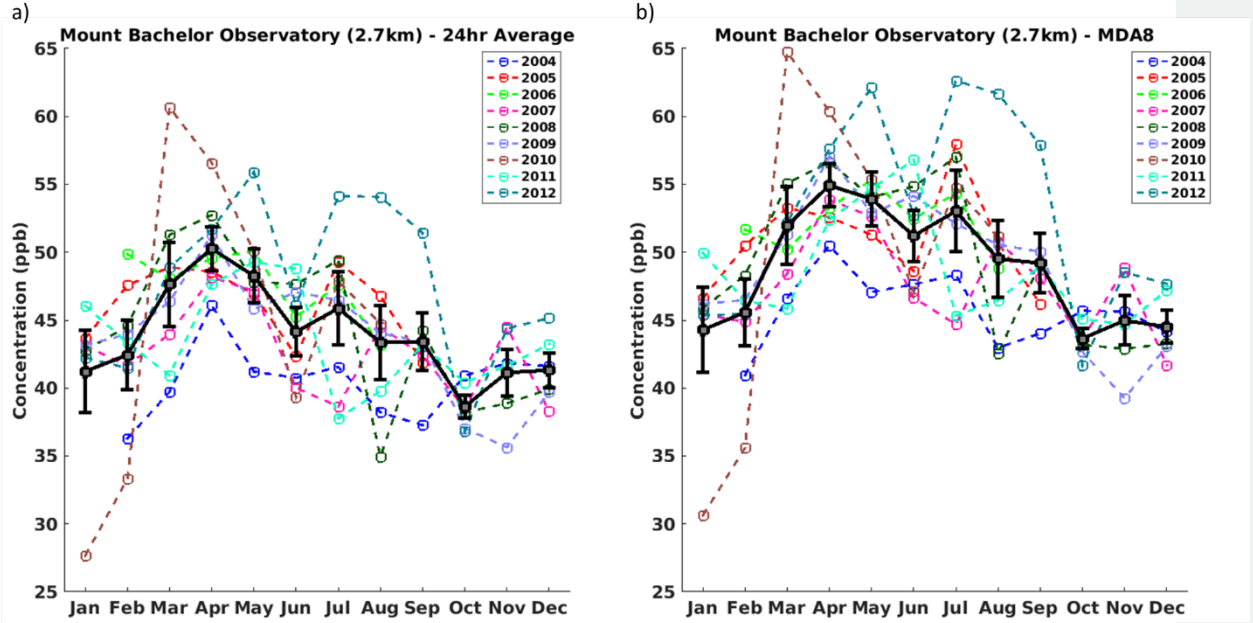

Fig. 3. Monthly average of observed (a) daily 24-hour and (b) MDA8 O3 concentrations averaged across 2004-2012 at Mount Bachelor Observatory. Black line shows the average of each month from 2004-2012. Error b

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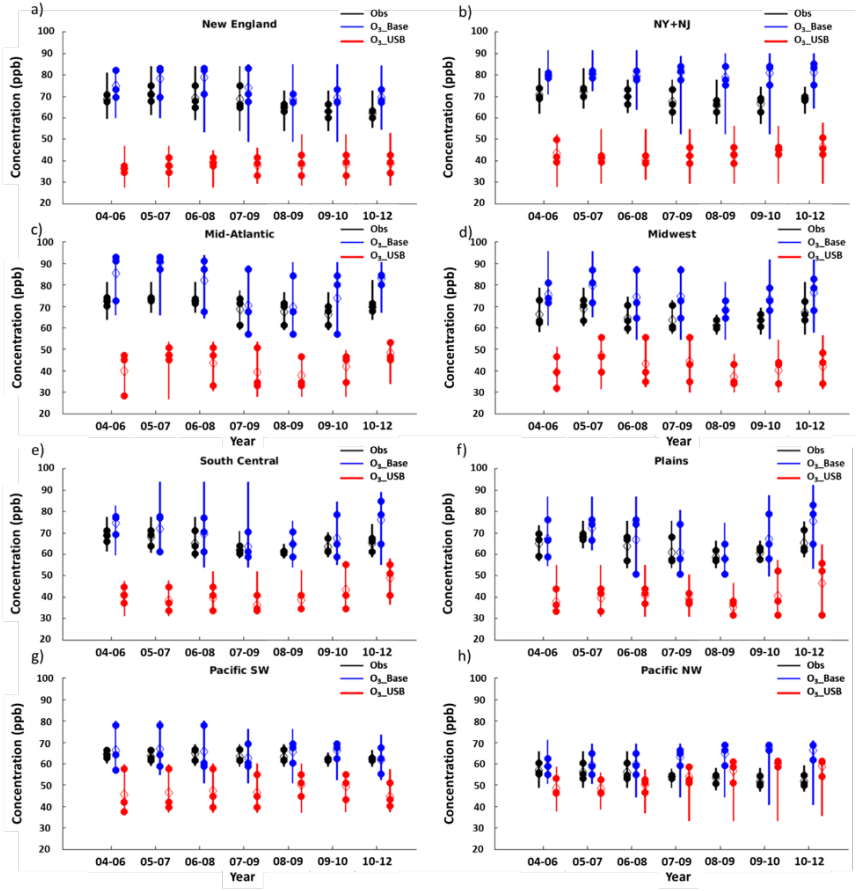

Fig. 4. Summary information for each region showing the three 4th highest days in each year (solid dots) that went into the calculation of the three-year average of the 4th highest MDA8 O3 day (hollow diamond

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	Emissions	2004	2005	2006	2007	2008	2009	2010	2011	2012
Global	*Anthropogenic NO with biofuels (Tg N)*	30.3	30.2	30.1	29.9	29.5	29.0	28.8	28.8	28.8
	Biomass burning (Tg N)	4.5	4.7	4.6	4.6	3.9	3.5	5.0	3.7	3.7
	Soil (Tg N)	9.0	9.1	8.8	8.5	8.4	8.6	8.4	8.6	9.2
	Lightning (Tg N)	5.5	6.1	6.2	6.4	6.9	7.3	7.2	7.1	7.2
	Isoprene (Tg C)	493.0	499.3	471.5	453.6	435.3	455.4	466.0	453.3	467.3
US	*Anthropogenic NO with biofuels (Tg N)*	6.32	6.04	5.75	5.44	5.13	4.63	4.36	4.36	4.36
	Biomass burning (Tg N)	0.02	0.06	0.06	0.07	0.04	0.04	0.05	0.12	0.12
	Soil (Tg N)	0.78	0.86	1.02	0.92	0.82	0.79	0.77	0.95	1.10
	Lightning (Tg N)	0.86	0.86	0.77	0.75	1.10	1.13	1.13	1.28	1.31
	Isoprene (Tg C)	18.1	21.5	21.9	22.0	19.3	18.3	20.2	22.0	22.4

Fig. 5. Global and US emissions totals for 2004-2012.

	Model Temperature (C)			GHCN Temperature (C)			Model Temp. Bias		
Region	*MAM*	*JJA*	*SON*	*MAM*	*JJA*	*SON*	*MAM*	*JJA*	*SON*
New England	7	19	11	8	20	11	0	-1	1
NY+NJ	9	21	12	9	21	12	0	0	1
Mid-Atlantic	12	23	14	12	23	14	0	0	0
Southeast	17	26	18	17	26	18	0	0	0
Midwest	10	22	12	10	22	12	0	0	0
South Central	18	27	19	19	28	20	-1	-1	-1
Plains	13	25	13	13	25	13	0	0	0
Mountains+Plains	7	20	9	7	19	8	0	1	1
Pacific SW	14	23	17	14	22	17	-1	0	0
Pacific NW	8	17	10	8	17	9	0	0	1

Fig. 6. Monthly average temperature across all days in each season (average of 2004-2012) in (1) GEOS-Chem, in (2) the Global Historical Climatology Network (GHCN) and the Climate Anomaly Monitoring System (C

	Correlation	
Region	**O_3 Base and O_3 USB**	**O_3 Base and O_3 USA**
New England	0.28	0.64
NY+NJ	0.50	0.58
Mid-Atlantic	0.54	0.70
Southeast	0.66	0.59
Midwest	0.75	0.76
South Central	0.71	0.72
Plains	0.80	0.75
Mountains + Plains	0.95	0.64
Pacific SW	0.72	0.28
Pacific NW	0.98	0.05

Fig. 7. Correlation between (1) O3_Base and O3_USB and (2) O3_Base and O3_USA on the average of O3_top10obs_JJA days from 2004-2012 in each region.

	Obs	O_3_Base	O_3_USB	O_3_USA	Temperature (C)
New England	-6	-4	6	-10	2
NY+NJ	-2	-4	3	-7	1
Mid-Atlantic	0	-3	4	-7	1
Southeast	-4	-5	2	-7	1
Midwest	-2	-4	2	-6	0
South Central	-6	-2	5	-7	1
Plains	-1	-2	4	-5	1
Mountains + Plains	-4	-1	1	-2	-1
Pacific SW	-3	-4	0	-4	-1
Pacific NW	-7	-5	-4	-1	-1
Average	*-3*	*-3*	*2*	*-6*	*0*

Fig. 8. Change in MDA8 O3 concentrations from 2004-2006 to 2010-2012 on O3_top10obs_JJA days in the observations, O3_Base, O3_USB, O3_USA, and temperature.

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Region	Range	Obs	O_3_Base	O_3_USB
New England	4th highest day	15	16	10
	3-year average 4th highest day	9	10	3
	Difference	-6	-6	-7
NY+NJ	4th highest day	11	10	12
	3-year average 4th highest day	6	2	6
	Difference	-5	-8	-6
Mid-Atlantic	4th highest day	13	36	25
	3-year average 4th highest day	7	21	10
	Difference	-6	-15	-15
Southeast	4th highest day	9	24	10
	3-year average 4th highest day	6	9	4
	Difference	-3	-15	-7
Midwest	4th highest day	13	22	24
	3-year average 4th highest day	8	11	10
	Difference	-6	-11	-14
South Central	4th highest day	11	26	22
	3-year average 4th highest day	8	13	13
	Difference	-3	-13	-9
Plains	4th highest day	14	32	24
	3-year average 4th highest day	9	18	11
	Difference	-5	-15	-13
Mountains + Plains	4th highest day	9	23	20
	3-year average 4th highest day	6	13	13
	Difference	-2	-10	-7
Pacific SW	4th highest day	5	23	20
	3-year average 4th highest day	3	5	5
	Difference	-2	-18	-15
Pacific NW	4th highest day	11	14	15
	3-year average 4th highest day	5	9	12
	Difference	-5	-5	-3

Fig. 9. Summary information for each region. The first row next to each region reports the range across 2004-2012 of the 4th highest values from each of the 9 individual years for the observations, O3_Base, a