Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-1093-RC1, 2016

Atmospheric Chemistry and Physics Discussions
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Interactive comment

Interactive comment on "US surface ozone trends and extremes from 1980–2014: Quantifying the roles of rising Asian emissions, domestic controls, wildfires, and climate" *by* Meiyun Lin et al.

Anonymous Referee #1

Received and published: 12 December 2016

The manuscript on US surface ozone trends and extremes by Lin et al. is clearly one of the best modelling studies I have read in my career. It covers an important scientific topic with political relevance and provides an in-depth analysis of US surface ozone and its drivers to the extent that this can be achieved with a global model. It contains a careful and insightful analysis of observations and model results including a well-designed set of sensitivity experiments to attribute ozone trends and variability to various factors. The text is well structured and very well written. All arguments are clearly presented and justified; there is an adequate recognition of previous work. The figures are also very well designed and clear and readable. This would have almost

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been the first manuscript which I would recommend to “publish as is”, except that I do have a few very minor comments and suggestions how the text could be even further improved. In short, it was a real pleasure to review this manuscript.

Introduction: start with at least one general sentence about ozone being an important air pollutant which has been of relevance to the US for a long time

Page 2, lines 7-10: explicitly mention methane here (part of climate effects?)

Page 2, lines 33/34: this result is based on a previous study with the same model. Don’t state it as undisputed fact. Please write “Previous model simulations indicate . . .” or similar.

Page 3, line 2: not only precursor trends, but also inter-annual (meteorological) variability make this difficult if not impossible

Page 3, line 14: you may also want to mention that models have difficulties in simulating the seasonal cycle at baseline sites correctly (see recent papers by Parrish et al., Derwent et al.)

Section 2: please provide at least one general statement about the GFDL model with a reference to the model description paper before describing the experiments.

Page 4, line 22: please provide a reference to the dry deposition climatology

Page 5, line 22: awkward grammar: “a number of studies (Hiboll).”

Page 6, lines 7-10: statement misleading: there are thousands of long-term monitoring sites from AQS and several hundred “rural” stations. Add “selected”?

Page 6, lines 15-17: Please state if trend was derived from daily MDA8 values or monthly values and how you tested for the appropriateness of a linear trend model.

Page 9, line 8 vs. Caption Figure 6: Lee et al. once cited as 2013, and once as 2014.

Page 10, line 11: Please add a quantitative summary statement how well the Asian

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trends are captured. Figure 6 indicates within 10-20%, Mt. Happo is within 37%.

Page 11, lines 9-20: I recall from earlier discussions on USNE surface ozone that a large change occurred around 2001 when NOx scrubbers in power plants were activated. Is this worth mentioning here? Could this have an impact on the observed trends and/or the relation between spring and summer trends?

Page 13, line 4 vs. 20 ff: perhaps the rising isoprene discussion could be merged in one place? It is slightly confusing to see this in two places.

Page 14, lines 11ff: Figure caption (Figure 13) uses “NAB” as abbreviation for “Background” run. This should be made consistent (also the font of “NAB” in the legend differs from the other legend entries).

Page 15, line 1: Does the statement “can explain 50-65%...” assume linear additivity of the factors controlling surface ozone? Would the impacts be the same if you applied linear regression on the differences between the model simulations (instead of subtracting the linear trend estimates from each other)? Perhaps, Table 2 would be easier to digest if the individual contributions were listed (i.e. the differences) instead of the regression results themselves?

Page 15, line 38: please add a note how Asian emissions will decrease after 2030 according to RCP8.5. For example, will they reach year 2000 or year 1990 levels?

Page 16, lines 33/34: “consistent with the seasonality of pollution transport from Asia.” Isn’t this also the influence of the Asian summer monsoon in July/August which reduces surface ozone over Asia itself?

Page 20, lines 22-27: if possible, the argument about dry deposition influencing the high end of ozone distributions during the 1988 heatwave should be substantiated by an additional (1-year or only summer months) model simulation where dry deposition could be turned off (or reduced).

Page 21, lines 1-2: how about “plume chemistry” as another explanation for the overall bias? There are strong NOx gradients also in the horizontal, and ozone production efficiency is higher in the medium-NOx range than in the high NOx range.

Conclusions: the conclusions are more a summary than real conclusions. I suggest to shorten this summary of results and instead try to go one step further in assessing the possible consequences of this study. For example: even though methane hasn't played a major role in the past, will it become more important in the future if, as suggested by the RCPs, Asian NOx emissions will decrease again? Or: what do we expect from future NOx emissions in the NEUS? In relation to climate change: could there be a greater role of biogenic VOC and would this lead to more or less severe ozone episodes?

Figure 20: why are the observed trends not included in this figure?

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