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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Can a global model reproduce observed trends in summertime surface ozone levels?" *by* S. Koumoutsaris and I. Bey

Anonymous Referee #1

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Review of "Can a global model reproduce..." by Koumoutsaris and Bey

This paper seeks to answer the question posed in the title. But I'm not sure the question is particularly well posed or that the tools have been well chosen. Ordinarily a negative result (hypothesis proved wrong) would be just as important as a positive one (hypothesis correct). However in this case, I think there are a number of simplifications that make the analysis less then insightful. The major ones are:

1. Highly variable trends in observations from site to site (for Europe) combined with relatively low model resolution (2x2.5)

2. Ignoring changes in the spatial distribution of emissions (very important for the US);

3. Ignoring changes in biomass burning emissions.

4. Analysis only goes thru 2005. Five additional years would add a lot of insight.

While I recognize that it would be a major undertaking to implement these changes, this study would be improved substantially by addressing the points above.

In summary, the model appears to capture O3 trends in regions where there are robust, spatially broad trends from numerous sites. This mainly applies to the Eastern US, where emissions are known to be decreasing. In Europe, it seems the trends are more variable. As such, we would not expect a global model to be able to reproduce these well at all. In the western US, anthropogenic emissions are probably not decreasing (in contrast to your presumed distribution) and biomass burning emissions may be increasing. There is at least one paper that claims a summer increase in O3 due to increasing biomass emissions in the western US.

So while there are some useful results here, the analysis is not nearly as useful as it could be. My detailed comments below are meant to be helpful should the authors decide to submit a revised manuscript.

Pg. 2026, line 15: The authors imply that long-range transport is thought to be an important source of O3. There is ample evidence of this for spring, but most studies suggest the impact is minimal in summer (see for example US NAS 2009).

2027, line 7: There are much more recent references for surface O3 trends. While recent, the Cooper (2010) paper focuses on free trop O3, not surface. Jaffe (2008) claims that increasing O3 in the western US trends in summer is linked to increasing biomass burning.

Line 12: There are also suggestions that CH4 may be important in the changing background O3.

Line 21: There have been other changes since 2005. Why do you constrain yourself to this period. The data are readily available?

2028, line 22: “Interannual varying. . .” In the discussion below, you mostly focus on trends in emissions.

2029: line 6: Uniform scaling across the US??? This is an important mistake for the US.

Line 8: Unclear if Mexico and Canada are still only 10%. US NOx emissions coming down, Mexican emissions going up.

2030, line 1: I am unclear how biomass burning emissions through 2005 are included. Text says interannual variations, but cites papers from 1999 and 2003. This is important for summer O3.

2031, line 2: While a seasonal average of June, July and August is commonly used, it should be noted that June MDA8s are typically much higher than August.

2032, line 1-10: To what extent is the model-observation correlation driven by seasonal cycle?

2033, line 25: “As these data are not available. . .” Really?

2034, lines 1-8: This discussion is confusing. How can the sites be rural, yet have high NOx/VOC. Seems contradictory.

2037: “5.1 Long-range transport from Asia” Unclear why the authors focused on this. Past work has shown very little impact on surface O3 in summer, with some impact on free trop. Much greater impacts demonstrated in spring. Using more or less the same type of model, why do you expect a different result?

Line 15-16: GEOS-Chem has done well on long-range transport in the past. Many examples of this (eg Jaegle et al 2003; Zhang et al 2008).

2040, lines 25-27: Not clear what this is referring to. The free tropospheric increase documented by Cooper 2010 is only for spring. The sentence seems to mix modeled and observed results so it is not clear what the sentence is saying.

Figure 2: Too many lines.

Figure 5: I find this figure confusing. There is an inconsistency between number of lines and caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 2025, 2012.