

Interactive comment on “Impact of El Niño Southern Oscillation on the interannual variability of methane and tropospheric ozone” by Matthew J. Rowlinson et al.

Anonymous Referee #2

Received and published: 23 April 2019

General comment:

The manuscript “Impact of El Niño Southern Oscillation on the interannual variability of methane and tropospheric ozone” written by Matthew J. Rowlinson describes the impact of meteorological variability and forest fires associated with El Niño Southern Oscillation (ENSO) on methane and tropospheric ozone. The manuscript contains novel investigation to quantitatively isolate the impact of forest fire emissions and meteorological variability on methane lifetime and growth rate using modeling approach. While many modeling studies on the impact of ENSO on tropospheric ozone have been conducted, this is an interesting work that deduced spatial variations in radiative effects of

[Printer-friendly version](#)

[Discussion paper](#)



tropospheric ozone changes during El Niño. The authors used appropriate model simulations to use for this science problem. Overall this manuscript is well written and easy to follow. I would like to consider the publication of this manuscript from Atmospheric Chemistry and Physics after minor revision. Please see the following comments.

Specific comments:

1. Model evaluation

In Section 3, the authors presented the model evaluation of mean concentration fields of O₃, CO, CH₄, and NO_x with satellite, aircraft, and ozonesonde observations, whereas the authors do not conduct model evaluation of inter-annual variations in these concentration fields during El Niño. The model evaluation would also be helpful to support validity of the model simulations used in this study.

2. Impact of ENSO on OH and methane

In Section 4.2, the authors quantified the impact of forest fire emissions on CH₄ growth rate; however, the authors do not compare it with the observed CH₄ growth rate, even though the authors concluded that “This effect, combined with concurrent direct CH₄ emission from fires explain the observed changes to CH₄ growth rate during the 1997 El Niño” (P. 15, L. 391–392) in Section 6. I would like to recommend to add the comparison of the observed and simulated CH₄ growth rate in Section 4.2.

The authors also presented the impacts of forest fire emissions and meteorological variability on OH concentrations in Figure 6. Although the impacts are comparable except during the 1997–1998 El Niño, the authors hardly mentioned the impact of meteorology. A more detailed analysis (e.g., how do specific humidity, cloud, and lightning NO_x affect inter-annual variations in OH?) would also be interesting, though this may be beyond the scope of the current manuscript.

P. 2, L. 57: Tropospheric O₃ changes related to meteorology were attributed by both transport and O₃ loss with water vapor in Section 4.3; Why do the authors mention

Printer-friendly version

Discussion paper



only atmospheric transport changes in abstract?

P. 4, L. 139–140: Why is CH₄ concentrations scaled to the observations even though the CH₄ emission inventory is used in the model?

P. 8, L. 242–243: What is definition of El Niño periods used in this study? Please clarify the definition.

P. 8, L. 253–255: I would like to recommend to add the percentage number of the increase during the 2002–2003, 2006, and 2009–2010 El Niño events to compare them with the extreme El Niño event in 1997–1998 quantitatively.

P. 9, L. 271–272: Why does fire emissions have small impact on CO IAV?

P. 11, L. 300–302: Do you have possible explanation of the difference between this study and Butler et al. (2005)? Did you compare increases in CO emissions in GFED4 with Butler et al. (2005)?

P. 13, L. 327: Please clarify the reason why you conducted this analysis in Section 4.3.

P. 14, L. 346–362: Many previous works have been done with different models and satellite observations with regard to ENSO impacts on tropospheric ozone. It would be beneficial to see more discussion of how the results presented here compare to previous studies (e.g., Stevenson et al., 2005; Zeng and Pyle, 2005; Doherty et al., 2006; Koumoutsaris et al., 2008; Nassar et al., 2009; Ziemke et al., 2010; Sekiya and Sudo, 2012; Oman et al., 2013; Neu et al., 2014; Inness et al., 2015).

P. 14, L. 364–365: Do the authors used TES O₃ radiative kernel? Please clarify what data the authors used in this study.

Technical corrections:

P. 2, L. 58: typo for nitrogen oxides?

P. 4, L. 135: typo for nitrogen oxides?

Printer-friendly version

Discussion paper



P. 13, L. 332: Which is correct, “1997-2001” or “1999-2003”? The “1999-2003” period would be appropriate to obtain the mean fields.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-222>, 2019.

ACPD

Interactive
comment

Printer-friendly version

Discussion paper

