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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 #1

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In this paper, the authors document the results of model experiments analyzing the influence of ENSO on interannual variability of carbon monoxide, tropospheric ozone, hydroxyl radical and resulting impact on radiative effects and methane variability. The paper is generally well-organized and addresses scientific questions within the scope of ACP. My two main concerns are: a) the ability of the model to capture observations during ENSO years has not been explicitly demonstrated and b) results from previous studies (both model and observational) on the influence of ENSO on tropospheric ozone have not been considered (though studies on IAV of CO have been considered). These issues are highlighted below with specific suggestions on further improving the manuscript.

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Discussion paper

Specific Comments:

Abstract: To me, the first sentence of the abstract gives the impression that the focus of this study is the analysis of methane trends and variability which is currently being intensely debated in the literature (Turner et al., 2019; Nisbet et al., 2019). Unless I misunderstood, the paper is geared towards analyzing the impact of ENSO on carbon monoxide, tropospheric ozone, hydroxyl radical as well as methane. The influence of ENSO on methane variability is discussed here but a full analysis of the methane growth rate is not performed in this study. Lines 45-47 better reflect the analyses performed here. The first couple of sentences should be revised so that they accurately represent the focus of this study which I understand to be ENSO driven changes in atmospheric composition and their impacts rather than just methane growth rate.

Section 2.1: How does the model calculate biogenic VOC emissions? Given that VOC oxidation is an important source of CO (about 15% according to Duncan et al. 2007), I would imagine that variability in VOC emissions (driven by variations in meteorology, radiation, land-use, CO_2) (Lathiere et al., 2005) would have some impact on CO IAV.

L133-134: Emissions of "all source of methane have been included in the model." Could you please elaborate on which sources of methane emissions have been included in the model?

L135: Replace nitrous oxide (which is N_2O) with nitrogen oxide.

L137-139: What emissions does JULES simulate? Wetlands? Agriculture? Please clarify.

Section 3: Given that the model is being used to analyse the impact of El Nino, in addition to the climatological evaluation discussed in this section, a more focused evaluation against measurements in El Nino years would more appropriately build confidence in the model's ability to capture features unique to conditions in these years.

L167-168: Are averaging kernels applied to model output for evaluation against satellite observations. Please clarify for MOPITT CO and OMI ozone.

L190-191: It is stated on L139-140 that surface methane concentrations are scaled in the model to observed values. It is therefore not surprising that the model performs well near the surface for methane. I think this sentence should be caveated.

L191-192: It looks like the model O3 is a factor or two too low compared with aircraft observations over southern Africa and off the coast over southern Atlantic. Please elaborate on the possible reasons for this bias.

L195-196: It would be helpful to have a quantitative estimate (e.g., bias, error, correlation) of how well the model captures the observations.

L196-197: By how much are the simulated concentrations of NOx lower than the observations? What processes (emissions, chemistry or meteorology) are likely responsible for these biases? Do these biases in NOx have implications for the simulation of ozone?

Section 3.2: It would be useful to clarify that the OH evaluation is performed for year 2000. How is the tropopause determined to calculate tropospheric OH concentrations? How does the model tropospheric methane lifetime (due to OH reaction only) compare against that derived from observational estimates by Prather et al. (2012)?

L215-216: What caused the lowering of OH in this version of TOMCAT versus that described by Monks et al (2017)?

L218-219: As I understand, the authors choose to calculate the chemical lifetime rather than the atmospheric lifetime of methane from the model because not all loss processes affecting the atmospheric lifetime are considered in the model (e.g., soil uptake, stratospheric loss, tropospheric loss due to chlorine). And not “Due to the long lifetime of CH4”. Please rephrase this sentence.

L246-247: I think this sentence should be placed before describing the Voulgarakis et al (2015) results.

L250-252: Is the simulated IAV in tropospheric CO concentrations driven by biomass burning emissions similar to the IAV in the imposed biomass burning emissions? I would imagine that the IAV in GFED4 CO emissions would be similar to that for CO simulated by the model. Would be useful to confirm this. This then begs the question - what is driving the interannual variability in biomass burning emissions - is it changes in area burnt, biomass available for burning, or meteorological conditions or all of these?

L297-298: I find this sentence confusing - is it that including CO results in a decreasing trend in OH? Though, this is not evident from figure 6.

L316-317: Given that the reaction rate constant of CH4+OH is strongly sensitive to temperature, approximately 2 % K -1 (John et al., 2012), what is the impact of assuming constant temperature in the box model on the results discussed here?

L332-L333: The authors mention that they compared the early mean period (1997-2001) with the end period but Figure 7 shows the early period as 1999-2003 without the influence of El Nino. Please revise this sentence.

L335: Remove “a”.

L337-338: The influence of shift in ozone precursor emissions from the mid-latitudes to the tropics has been demonstrated by Zhang et al. (2016).

L344: This sentence is confusing. From figure 7b, it looks like India becomes “both-limited” towards the end of the simulation.

L347-349: Do observations also indicate this significant alteration of the vertical distribution of tropospheric ozone? How does this interpretation of the model results compare with the analysis of Chandra et al (1998) and Ziemke and Chandra (1999)?

L385-L387: These are broad-brush statements which then make it easy to question the model’s ability to simulate chemical composition during El Nino years and derived interpretations. As I mentioned above, a more focused evaluation would be helpful to reveal model strengths and weaknesses building confidence in the results.

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References:

Duncan et al. 2007, https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2007JD008459. Chandra et al. 1998, https://agupubs.onlinelibrary.wiley.com/doi/10.1029/98GL02695. John et al., 2012, https://www.atmos-chem-phys.net/12/12021/2012/. Lathiere et al., 2006, https://www.atmos-chem-phys.net/6/2129/2006/acp-6-2129-2006.html. Nisbet et al., 2019, https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2018GB006009. Turner et al., 2019, https://www.pnas.org/content/116/8/2805 Zhang et al., 2016, https://www.nature.com/articles/ngeo2827. Ziemke and Chandra, 1999, https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/1999JD900277.

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