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Climate of the Past
Discussions
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Interactive comment on "Role of the stratospheric chemistry-climate interactions in the hot climate conditions of the Eocene" *by* Sophie Szopa et al.

Anonymous Referee #2

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The role of stratospheric ozone changes in Eocene climate conditions is studied using coupled chemistry-climate simulations. These hot climate conditions are simulated by setting CO2 to 4 time its preindustrial (PI) control value, as well as setting tropospheric CH4 and N2O to significantly higher values compared to PI control. Much of the qualitative atmospheric temperature and ozone response resembles that of 4xCO2 experiments, which are well documented in the literature: tropospheric warming and stratospheric cooling, increased upper stratospheric ozone and decrease ozone in the lowermost tropical stratosphere indicative of an accelerated Brewer-Dobson circulation (BDC). Radiative feedbacks due to the modified ozone distribution are then argued to potentially play an important role in Eocene-like climates, of similar importance as non-CO2 boundary conditions.

Climate feedbacks due to stratospheric ozone have recently received some attention with different studies coming to different conclusions - compare Dietmueller et al. (2014), Nowack et al. (2015), Marsh et al. (2016) - e.g., summarized in Chiodo et al. (2018). This study could provide a valuable contribution to this line of research and extend it to past climates. However, as presented I see a number of issues that need to be dealt with before publication. Perhaps this is possible within a major revision, but it may require a more substantial effort.

Major issues

The main motivation appears to be the climate sensitivity to stratospheric ozone, but this cannot be studied with the model setup used: because SSTs and other lower boundary conditions are prescribed, surface climate cannot respond to atmospheric changes. In fact, the prescribed SSTs, which come from a low-resolution coupled climate model (FOAM, which as far as I understand does not include interactive chemistry), are likely inconsistent with the ozone feedback that would result from the interactive chemistry simulations with LMDz.

Given this issue, another motivation is to study the stratospheric response (ozone, circulation) to the prescribed Eocene conditions. This would be fine, but in the current presentation in the manuscript this appears to be poorly conceived:

a) Only annual mean cross sections are shown, even though the essential dynamics take place in each hemisphere's winter and spring seasons. I think that seasonal-mean plots are necessary to substantiate the results and interpretations related to the stratospheric response.

b) Changes to tropospheric CH_4 and N_2O are applied but no results are presented that show what the effects of these changes are (N_2O should lead to modified chemistry, as noted by authors; CH_4 could lead to changes in stratospheric H_2O). If these changes are deemed to not be so important then why not simply perform simulations with $4xCO_2$, which would also have the advantage of providing better comparability to previous results with similar forcing? If above changes of tropospheric species are deemed to be important then this calls for corresponding analyses and results to be presented.

c) How well does this model (LMDz) simulate the stratospheric circulation compared to other state-of-the-art chemistry-climate models? The CCMVal-2 activity included a version of this model, which indicates it is performing well in terms of several diagnostics, but also has some issues (see SPARC CCMVal report referenced): e.g., huge warm bias in upper stratosphere, where the radiative scheme seems to behave questionably, bias in surface energy balance, large cold bias in SH leading to strongest ozone hole out of all compared models etc. The only place in the literature where I could find a plot of the model's overturning streamfunction (i.e., its BDC) is Dietmueller et al. 2018 (Fig. 1 therein): it looks completely off, questioning the model's ability to simulate stratospheric transport (despite the fact that its AoA distribution looks okay) . . . in all fairness, Dietmueller et al. note that this may be a diagnostic, rather than an actual model problem. In any case, the authors should include information about the basic model performance in regards to stratospheric dynamics, transport, and climate, and convince the reader that this is a suitable model for the purposes of the study.

d) Other studies on ozone changes due to 4xCO2 (see references listed above) have highlighted the crucial role of changes in stratospheric H2O, which come about due changes in tropical tropopause temperature, but also due to ozone-temperature feedbacks near the tropical tropopause. This type of sensitivity could be important in order to understand the climate response to 4xCO2 and should be included in the results and discussion.

e) At face value, the presented results indicating both an accelerating BDC and stronger polar vortex seem to contradict each other, since a stronger BDC should be associated with stronger wave drag, which would be consistent with a weaker vortex. This is not discussed in the paper but seems important to understand the stratospheric changes. My guess is that this can be explained by the seasonality in the changes (cf.

Figs. 5, 6): the wave forcing seems indeed weaker in early winter when the vortex is much stronger (and I would expect a weaker BDC during that part of the season, but this should be checked and potentially included in the presented results). During late winter and spring the wave forcing is much enhanced consistent with an accelerated BDC - again this should be checked based on residual circulation diagnostics.

f) It is claimed that the stratospheric cooling due to higher CO_2 levels explains the changes in polar vortex strength, but why would the CO_2 cooling affect the meridional temperature gradient rather than lead to a meridionally uniform cooling, which would not affect the polar vortex? With the presented results the cause of the strengthened vortex remains confusing.

Minor Comments:

Fig. 1: why not present AoA similar to panels a, b (difference as color shading with PI control as black contours)? Also: what are the units for the presented PI O_3?

page 4, section 2.1: it would help to include some information about how the model compares to other chemistry-climate models (see major comment)

page 4, line 31: please also provide the model top

page 5, line 3: “snapshots” - do you mean “time slices”?

page 5, line 17: please explain “LPJ”

page 5, line 24: please provide justification / motivation for why you choose a CO_2 value at the low end of what’s recommended

page 6, line 1-2: please provide more detailed explanation for why radiative effect due to enhanced CH_4 and N_2O levels would be accounted for by enhanced CO_2?

page 6, line 10: “80s” - you mean the 1980’s? Is this meant to represent an “ozone-hole climate”? Why not simply use the O_3 field from, e.g., a CCMVal-2 chemistry-climate simulation with your model?

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page 6, line 25: please discuss the temperature changes a bit more, e.g.: is the Antarctic amplification (largest temperature response over Antarctica) a well-known response for these types of simulations? Why is there no corresponding Arctic amplification as happens for current climate change and happens for pure 4xCO2 runs?

page 7, line 13: "total ozone column" - here and elsewhere: usually this is referred to as "total column ozone (TCO)" and I'd recommend nomenclature consistent with other literature

page 8, line 3, Fig. 4: you already showed an indication that the winter season matters most, so why not show DJF and JJA changes instead of the annual mean (see major comments)?

page 9, line 1: "... drives the strength of the zonal wind" - 1) thermal wind balance doesn't tell you about cause and effect, so "drives" is misleading, 2) it's a relation between the meridional temperature gradient and the vertical zonal wind gradient (not the wind itself), so you wouldn't necessarily expect the temperature gradient at 10 hPa to correspond to the wind at 10 hPa ...

page 9, line 2: the heat flux is a proxy for the vertical Eliassen-Palm (∼wave activity) flux, which more accurately also involves the vertical temperature gradient and the background vorticity; given that you compare two very different climates, I wonder whether the heat flux is a sufficiently accurate measure of wave activity flux, since both background temperature and vorticity structures might contribute?

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