We thank the **reviewer #2** for his/her comments, which will lead to a clearer manuscript notably regarding the seasonality of the changes in the stratospheric circulation.

Hereafter, we explain how we are able to improve the paper regarding the issues mentioned by the reviewer #2 in blue italic (actions taken on the manuscript are preceded by an arrow).

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 inter- active chemistry simulations with LMDz.

Our main motivation is not the climate sensitivity to stratospheric ozone. The first focus is the stratospheric ozone changes themselves in the Eocene and then the associated first-order climate effects. If the study had been about climate sensitivity to stratospheric ozone, we would have ran 4xCO2 simulations with other boundary conditions being set at present-day conditions (to be comparable to most other studies on the topic) and if focused on climate sensitivity we would have tried to use a fully-coupled ocean-atmosphere model (as recommended by the reviewer hereafter). Since it is the first study on stratospheric ozone changes for Eocene, it makes sense to use an atmospheric climate model forced by SST to make a first estimation of ozone distribution changes, to explore the drivers (easier to do with a fixed-SST configuration than in a fully coupled configuration) and assess the potential climate forcing. Such configuration has been applied many time for such paleoclimate investigations [e.g. Botsyun et al. 2019, Ladant et al., 2014; Ladant et al. 2016; Licht et al., 2014; Pohl et al. 2016; Porada et al. 2016]. It is more reasonable to launch into fully coupled long simulations only if ozone changes and first-order effects are found to be potentially significant in the fixed-SST configuration.

Our ultimate objective here is to estimate the first-order climate signal that can be missed in a typical warm paleoclimate simulation when the response of stratospheric ozone to Eocene conditions and associated dynamical feedbacks are ignored. This first-order impact is the ozone-driven changes in atmospheric dynamics, temperature and radiative balance. As noted by rev#2, our ocean is not interactive, so we missed the effect on sea-surface temperatures, and the associated potential feedbacks. We consider that it is not necessary to include the ocean feedback, which requires a far more complex model setting and longer computation times, for an estimation of first-order effects.

Ref:

Botsyun S., P. Sepulchre, Y. Donnadieu, C. Risi, A. Licht, J. K. Caves Rugenstein. "Revised paleoaltimetry data show low Tibetan plateau elevation during the Eocene.", Science (2019), *in press*.

Ladant, J. B., Y. Donnadieu, V. Lefebvre, and C. Dumas (2014), The respective role of atmospheric carbon dioxide and orbital parameters on ice sheet evolution at the Eocene-Oligocene transition, Paleoceanography, 29, 810–823, doi:10.1002/2013PA002593.

Ladant, J.-B. & Donnadieu, Y. Palaeogeographic regulation of glacial events during the Cretaceous supergreenhouse. Nat. Commun. 7:12771, doi: 10.1038/ncomms12771 (2016).

Licht, A., et al. (2014), Asian monsoons in a late Eocene greenhouse world, Nature, 513(7519), 501–506.

Pohl, A., Y. Donnadieu, G. Le Hir,J.-B. Ladant, C. Dumas, J. Alvarez-Solas,and T. R. A. Vandenbroucke (2016), Glacial onset predated Late Ordovician climate cooling,Paleoceanography,31, 800–821,doi:10.1002/2016PA002928.

Porada, P. et al. High potential for weathering and climate effects of non-vascular vegetation in the Late Ordovician. Nat. Commun. 7:12113 doi: 10.1038/ncomms12113 (2016).

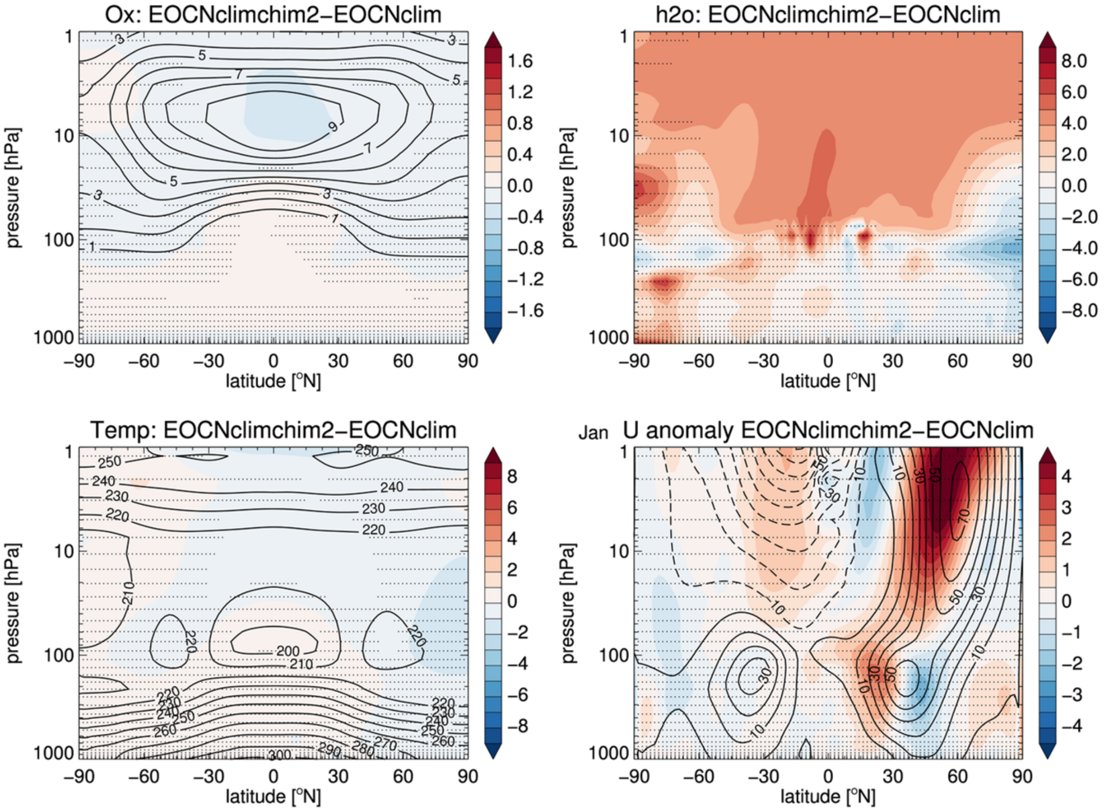
Given this issue, another motivation is to study the stratospheric response (ozone, circulation) to the prescribed Eocene conditions. This would be fine, but in the current presentation in the manuscript this appears to be poorly conceived:

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.

As pertinently pointed out by the reviewer, in the stratosphere, essential dynamics take place in hemisphere's winter season, when the polar vortex dominates the large scale zonal circulation at mid-and-high latitudes. Initially, the annual mean plots were meant to provide a general overview of the major stratospheric changes on ozone and circulation in a standard way. In addition, timeseries in key sectors (e.g. zonal-mean zonal wind in the climatological center of the NH polar night jet (60°N,10hPa), meridional eddy heat flux at 100 hPa and 45-75°N) provided valuable information on the seasonal evolution of the stratospheric dynamics, essentially to better characterize and understand the total column ozone (TCO) gradient which appears peculiar in our study in comparison with other studies where present-day climate 4xCO_2 experiments are analyzed. Nonetheless, we agree that providing additional analysis of the seasonal evolution of the stratosphere dynamics using monthly or seasonal mean cross-section can help elucidate mechanisms, notably and help to (i) understand the apparent contradiction noticed by the reviewer of a faster Brewer-Dobson circulation despite a stronger polar vortex, and (ii) understand better the TCO gradient in the Northern Hemisphere. Therefore, the analysis is now extended by adding Northern Hemisphere's (where changes are the strongest) monthly mean zonal cross sections of the zonal-mean wind, transformed eulerian mean diagnostics, etc, for preindustrial conditions and the anomalies associated with Eocene conditions. Please find more details in our response to comment e).

=> In the revised version of the manuscript, an entirely new analysis of the seasonal stratosphere dynamics - based on seasonal mean cross section in Northern Hemisphere winter as suggested by the reviewer – has been performed and presented section 3.2 which has been renamed "Seasonal evolution of the Northern Hemisphere stratospheric polar vortex in Eocene conditions". Figures 5 and 6 have been removed and replaced by the three Figures shown in our response to comment e). Figure 5 has been moved in the supplementary material as we believe that it illustrates very clearly the seasonal changes of the polar vortex.

b) Changes to tropospheric CH4 and N2O are applied but no results are presented that show what the effects of these changes are (N2O should lead to modified chemistry, as noted by authors; CH4 could lead to changes in stratospheric H2O). If these changes are deemed to not be so important then why not simply perform simulations with 4xCO2, 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.



Rev2 Figure 1. Changes in zonal-mean O_3 (in ppmv), H_2O (in%), temperature (in K) and zonal wind (in m/s) associated with the changes of N_2O and CH_4 under Eocene conditions. Dotted region indicates that the anomalies are not statistically significant at the 5% level.

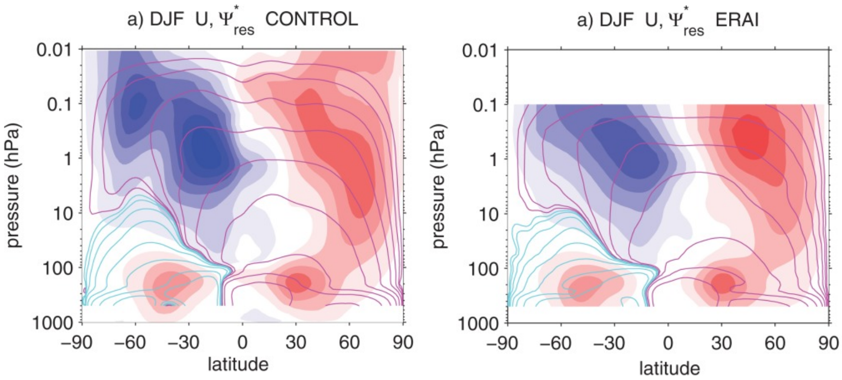
As shown above (Rev2 Figure 1), changes in CH_4 and N_2O lead to changes in H_2O and O_3 that are small but consistent with the expected response (less than 5% increase of stratospheric water vapour due to increasing methane and a similar max ~5% decrease of ozone due to increase N_2O). The associated changes in dynamics appear also to be small (see zonal mean annual temperature and January zonal mean zonal wind on Figure 1). In the case of the zonal wind, the changes are insignificant for the January month. Other months show some patches of statistical significance but longer runs would be required to assess the robustness of these dynamical changes. In summary, these changes are small compared to the anomalies that are initially discussed in the paper; i.e. EOCN-PREIND and anomalies related to ozone mis-specification.

=> In the revised version of the manuscript, we now put more emphasis on the above mentioned anomalies (which are more important for the climate community), rather than adding an extra discussion on the role of CH_4 and N_2O that appear to be of secondary importance in light of the results presented above and whose effects are not very important, particularly with regard to the uncertainties of the climate 55 Ma years ago. Nonetheless, the effect of N_2O and CH_4 will be mentioned and included in the associated plots as supplementary material. We modified the manuscript as follows:

"Note that in comparison with a standard $4xCO_2$ simulation, including a 17% increase of N_2O in our Eocene simulations leads to a slight decrease of ozone which reaches a maximum of 3% in the equatorial upper stratosphere (5 hPa) (see supplementary material). Although the N_2O increase influence on ozone is statistically significant, its impact appears to be small compared to the 40% upper stratosphere ozone increase due increasing CO_2."

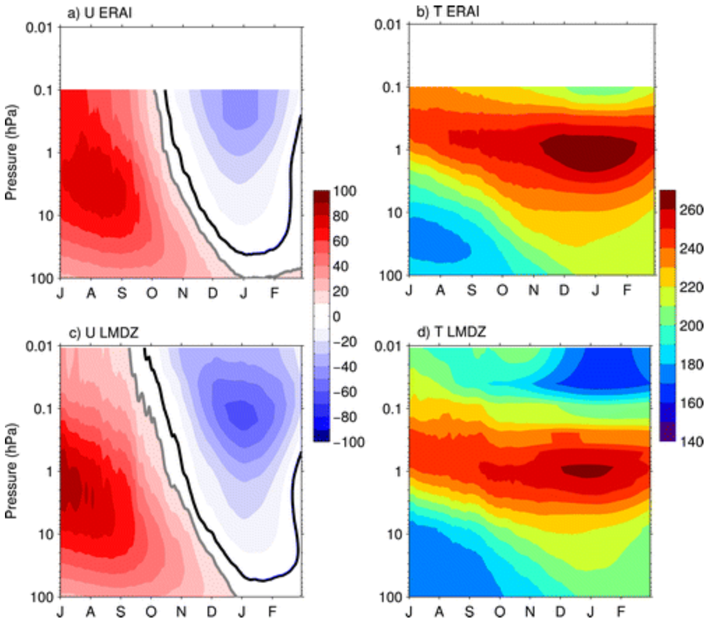
c) How well does this model (LMDz) simulate the stratospheric circulation compared to other state-of-the-art chemistry-climate models? The CCMVal-2 activity included a version of this model, which *indicates it is performing well in terms of several diagnos- tics, but also has some issues (see SPARC 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.*

The model has been involved in a range of studies, model inter-comparisons and evaluation. The overall conclusion is that LMDz is not the best or worst chemistry-climate model, it all depends on the chosen diagnostics and the selected regions. On a more general level, is there anything to gain in having all of us running the same model with the same set up, unless we want to end up with the same results? Below, we show several results of recent literature where the middle atmosphere dynamics of LMDz (or LMDz-Reprobus) have been evaluated against various reanalysis and other models in the frame of CCMI and other projects (e.g. CMIP). This should give an overview of how LMDz performs. It appears from the first example (Rev2 Figure 2 taken from de la Camara et al. [2016a]) that the stream function and zonal-mean zonal wind (here shown in DJF) – despite some differences - are overall highly consistent in LMDz and ERA-I reanalysis. In this case, LMDz is far from being completely off.

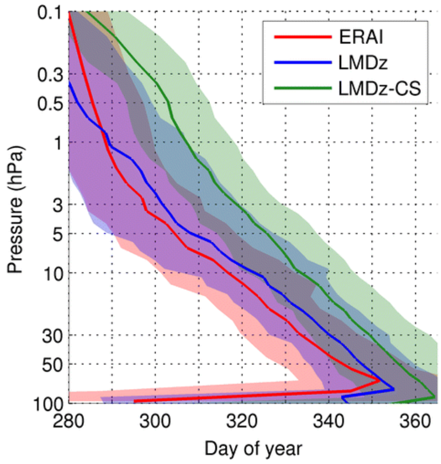


Rev2 Figure 2. Zonally averaged zonal wind (in m .s^{-1}, shaded), and stream function of the residual mean meridional circulation (contours) in LMDz. Magenta contours represent positive values (i.e., clockwise circulation), and cyan contours represent negative values (i.e., counter-clockwise circulation). From de la Camara et al. (2016a)

Both figures below (Figure 3, taken from de la Camara et al. [2016b]) give an overview of the seasonal evolution of the Southern Hemisphere polar vortex in LMDz in comparison with ERA-I. Again, despite some differences, the model appears to perform reasonably well, also near the stratopause and above. Interestingly, the final warming date (onset of the Southern Hemisphere polar vortex break-up) appears particularly well represented (comparing the blue and red vertical profile on Figure 4) in LMDz. LMDz shows only few days delay in the climatological final warming date; this cold bias appears to be particularly small with regard to the large interannual variability. Note that this reduced cold bias in the Southern Hemisphere is, to a large extent, due to the advanced non-orographic wave parameterization, which, if inaccurately specified (see de la Camara et al. [2016b] for more details) leads to an actual polar vortex break-up delay of ~15 days.

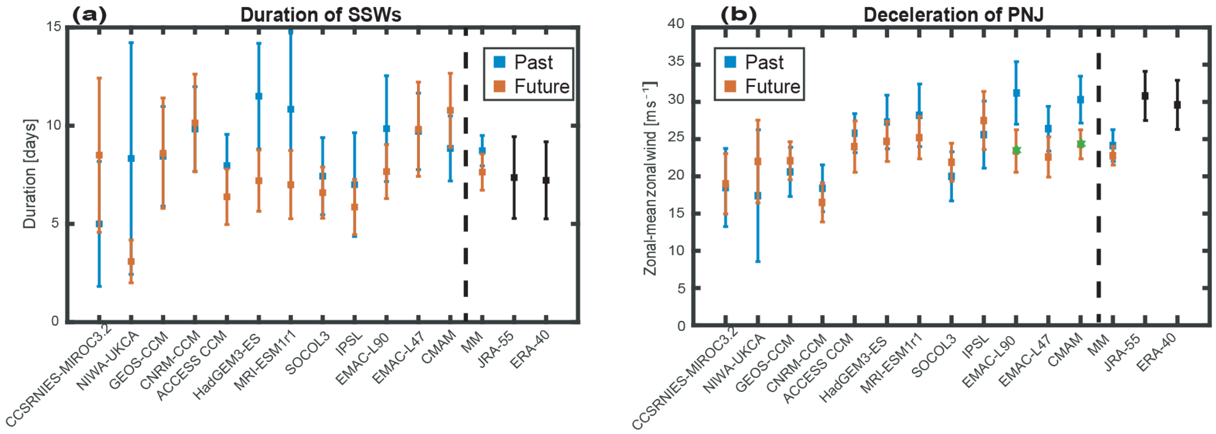


Rev2 Figure 3. Time–height evolution of (a) zonal-mean zonal wind (m s^{-1}) averaged over 70°–50°S and (b) temperature (K) averaged over 85°–60°S during the southern winter and spring for ERAI. (c),(d) As in (a) and (b), respectively, but for LMDZ. From de la Camara et al. (2016b)



Rev2 Figure 4. Final warming dates as a function of pressure level in ERAI (1992–2011; red), LMDZ (blue), and LMDZ-CS (green). The climatological means are given by the solid lines, and the shaded areas represent plus or minus one standard deviation. From de la Camara et al. (2016b).

Finally, the Northern Hemisphere stratospheric winter time variability also appears to be of good quality as revealed by the results of Ayarzagüena et al. [2018]. In their study, they showed that, first, the Sudden Stratospheric Warmings (SSWs) mean frequency in CCMI LMDz-REPROBUS is close to the one derived from reanalysis. Among CCMI models, LMDz further appears to be one of the closest to the real world. As shown below (Rev2 Figure 5), the mean duration (partly radiatively driven) and deceleration of the PNJ and their standard error in LMDz are particularly consistent with reanalysis. LMDz appears to perform well compared to other models that participated to CCMI.



Rev2 Figure 5. (a) Duration of SSWs (in days) and (b) deceleration of the PNJ associated with SSWs (in m s^{-1}) in each model for both periods of study. Bars denote ±1.5 standard error, and green stars indicate future values that are statistically significantly different from the past ones at the 95 % confidence level. From Ayarzagüena et al. (2018).

These various results demonstrate that the LMDz model simulates the middle atmosphere dynamics and circulation decently. Of course, the comparison with reanalysis is not perfect, but never completely off. Furthermore, these results demonstrate that LMDz performs reasonably well in comparison with other models of the same kind. In the revised version of the manuscript, we now recall this by citing relevant examples.

The focus by the reviewer on the Dietmueller et al. 2018 reference is very unfortunate and rather selective. How is it possible to reconcile the "completely off" model's overturning streamfunction (i.e., its BDC) shown in Dietmueller et al. 2018 with all the other results (and not only the AoA distribution)? Dietmueller et al. concluded: " The reason for these additional circulation cells is unknown. However, as the model shows a reasonable AoA, there might be a diagnostic problem in the residual circulation data". They are right. The reviewer also seems to have serious doubts about this plot. Unfortunately, Dietmueller et al. had contacted the wrong person for our CCMI runs, instead of the CCMI PIs (S. Bekki and M. Marchand), contrary to the CCMI guidelines, and no LMDz people are co-authors of this study. We are now in touch with Dietmüller et al. to sort it out, perhaps including a correction to the publication. At this stage and "in all fairness", the LMDz performances should be assessed in the light of all the other studies, including CCMI and CMIP inter-comparisons

=> In the revised version, additional information on model performances are now included in section 2.1.

Ref:

de la Cámara, A., Lott, F., and Abalos, M.:Climatology of the middle atmosphere in LMDz: Impact of source-related parameterizations of gravity wave drag, J. Adv. Model. Earth Sys., 8, 1507– 1525, https://doi.org/10.1002/2016MS000753, 2016a.

de la Cámara, A., F. Lott, V. Jewtoukoff, R. Plougonven, and A. Hertzog (2016b), On the gravity wave forcing during the southern stratospheric final warming in LMDz, J. Atmos. Sci., 73, 3213–3226, doi:10.1175/JAS-D-15-0377.1

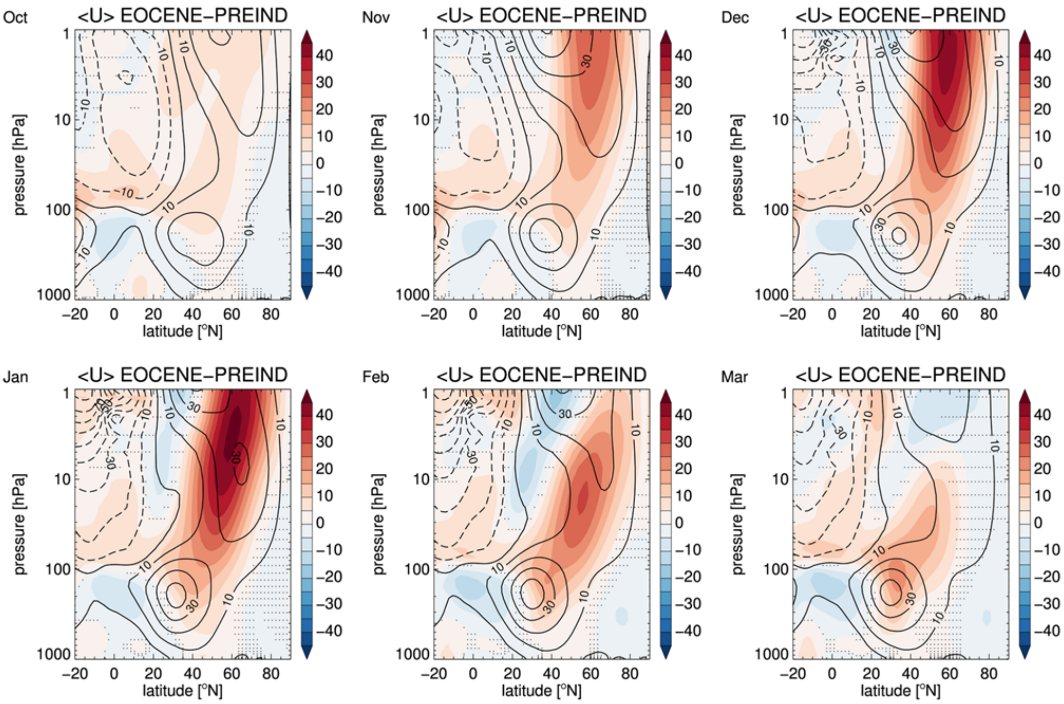
Ayarzagüena, B., Polvani, L. M., Langematz, U., Akiyoshi, H., Bekki, S., Butchart, N., Dameris, M., Deushi, M., Hardiman, S. C., Jöckel, P., Klekociuk, A., Marchand, M., Michou, M., Morgenstern, O., O'Connor, F. M., Oman, L. D., Plummer, D. A., Revell, L., Rozanov, E., Saint-Martin, D., Scinocca, J., Stenke, A., Stone, K., Yamashita, Y., Yoshida, K., and Zeng, G.: No robust evidence of future changes in major stratospheric sudden warmings: a multi-model assessment from CCMI, Atmos. Chem. Phys., 18, 11277-11287, https://doi.org/10.5194/acp-18-11277-2018, 2018.

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.*

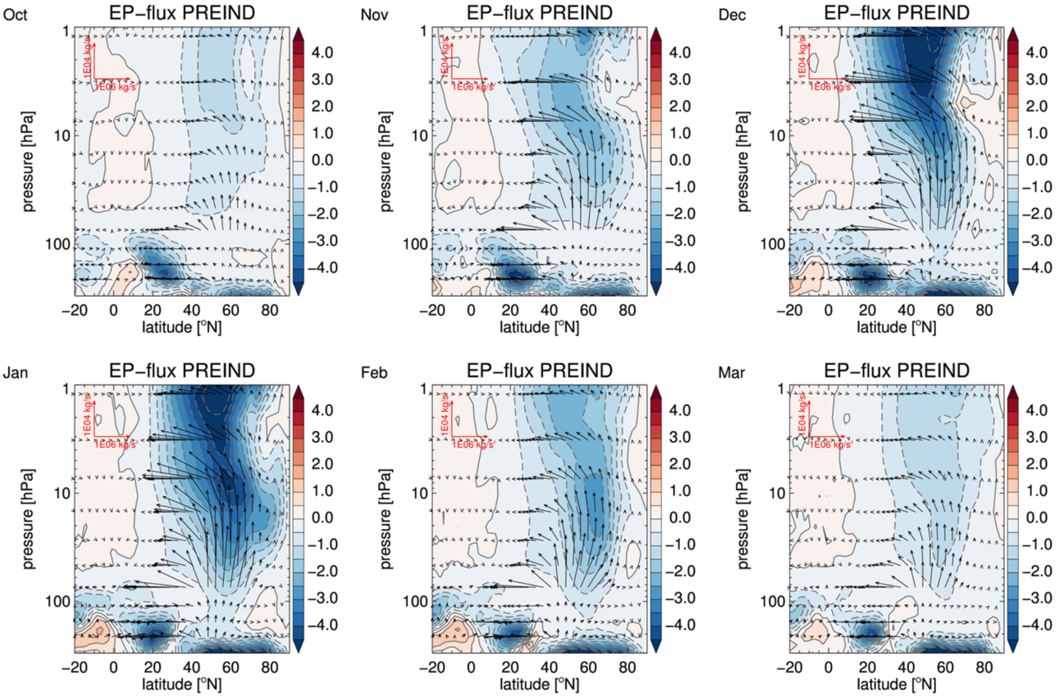
We agree that stratospheric H_2O changes could play a significant role in the climate feedbacks and help to understand the climate response to 4xCO_2. Note that it is not only stratospheric H_2O but also changes in high altitude clouds (cirrus). These water cycle feedbacks may explain some of the large model and scenario dependency of climate impacts associated with ozone changes (Nowack et al., JGR, 2018). However, we think the issue of the water cycle changes should be explored in a less constrained configuration (i.e. coupled atmosphere-ocean framework). In addition, tackling this issue is a paper in itself and is beyond the scope of the present paper.

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.

We thank the reviewer for pointing out this apparent contradiction and his/her very relevant insights. As shown on Figure 6 below (which is now inserted in the revised version of the manuscript and which is consistent with the zonal-mean zonal wind seasonal evolution shown a 10 hPa/60°N in the former version of the manuscript – Figure 5), a much stronger stratospheric polar vortex develops in early winter (Nov-December) under the Eocene climate compared to pre-industrial conditions. The strength of the polar night jet is doubled over the full depth of the stratosphere. By late winter (starting in January), the anomalies progressively reverse from the upper part of the stratosphere. In March, the stronger polar vortex anomalies in the middle atmosphere is no longer significant. As noticed by the reviewer, such a strong polar vortex anomaly seems at first glance in contradiction with a faster Brewer-Dobson circulation. Analysis of the wave activity and its interaction with the mean flow (i.e. engine of the brewer Dobson circulation or extratropical pump) allows removing this apparent contradiction.

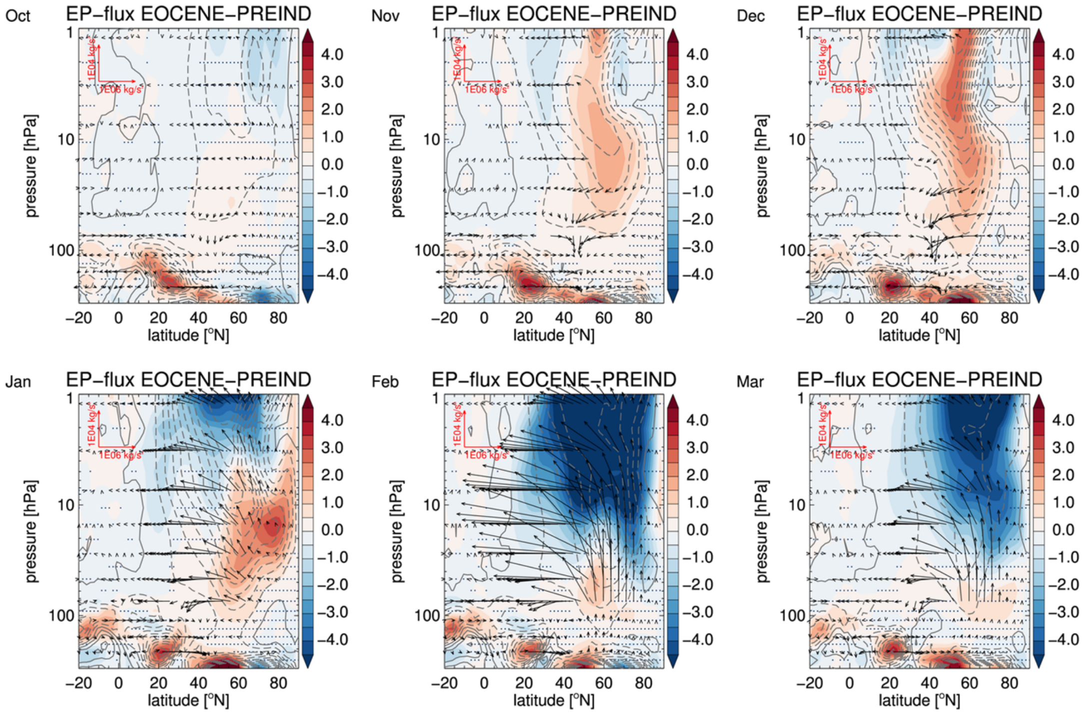


Rev2 Figure 6. Seasonal evolution (Oct to Mar) of the zonal mean zonal wind differences between the Eocene and preindustrial conditions. Shaded contours indicate that anomalies are significant at the 5% levels according to a t-test. Black isocontour shows the preindustrial run climatology.



Rev2 Figure 7. Seasonal evolution (Oct to Mar) of the Eliassen-Palm Flux (vectors) and its divergence (contours, in m/s/d) under preindustrial conditions.

The preindustrial climatology of the planetary wave propagation (EP-flux) and its interaction with the mean flow (EP-Flux divergence) shows that, permanently in winter, the wave activity penetrates in the stratosphere and the breaking of planetary waves lead to westward momentum drag which maximize near the location of the southern flank of the polar night jet (Rev2 Figure 7). This contributes to erode and weaken the polar vortex, to a warming of the polar stratosphere and lead to a net poleward residual mass transport (which drives the Brewer-Dobson circulation to a large extent). The wave activity and its interaction with the mean flow peaks in January but is already large in November (which can eventually lead to SSW [e.g. de la Camara et al., 2016]). Note that these model results are very consistent with reanalysis (see also response to comment c)) and therefore indicate that the representation of the stratosphere dynamics and circulation in LMDz-Reprobus is of an overall good quality.



Rev2 Figure 8. Seasonal evolution (Oct to Mar) of the differences between the Eocene and preindustrial conditions of the Eliassen-Palm Flux and its divergence. Shaded contours indicate that anomalies are significant at the 5% levels according to a t-test. Preindustrial climatology is shown with dashed contours.

Under Eocene conditions (Rev2 Figure 8), it appears that the planetary wave penetrating the stratosphere in early winter (Nov-Dec) is significantly reduced and deflected equatorward as revealed by the downward and equatorward pointing of the EP-Flux vector in the lower mid-latitude stratosphere (see also Figure 6b showing a lower eddy heat flux at 100 hPa in the former version of the manuscript). This is associated with an anomalous positive E-P flux divergence throughout the depth of the stratospheric polar night jet (near 60°N), which indicates a reduced westward momentum forcing by waves and hence allows a stronger development of the polar vortex in early winter. In contrast, by January, the wave activity becomes significantly larger (see also Figure 6b), the westward forcing appears strongly amplified in the upper stratosphere and this momentum forcing anomaly progressively propagates downward. This is consistent with the reversal of the zonal mean zonal wind anomaly in the upper stratosphere, but also with the overall extremely rapid deceleration of the polar vortex strength seen on Figure 5. Relatively, the Brewer-Dobson circulation will hence be less reduced in early winter than accelerated in late winter (where the differences in the wave forcing are much stronger), which results in a net acceleration of the Brewer-Dobson under Eocene conditions compared to preindustrial conditions as revealed by the younger age of air. Note that we also examined the residual circulation velocities v* and w* which confirms the seasonal changes in the Brewer-Dobson circulation strength (not shown).

=> This analysis is now included in the revised version of the paper in section 3.2. Accordingly, section 3.2 has been renamed "Seasonal evolution of the Northern Hemisphere stratospheric polar vortex in Eocene conditions". Figures 5 and 6 have been removed and replaced by the three Figures shown in our response to comment e). Figure 5 has been moved in the supplementary material as we believe that it illustrates very clearly the seasonal changes of the polar vortex.

Ref:

de la Cámara,A., Lott, F., and Abalos, M.:Climatology of the middle atmosphere in LMDz: Impact of source-related parameterizations of gravity wave drag, J. Adv. Model. Earth Sys., 8, 1507– 1525, https://doi.org/10.1002/2016MS000753, 2016.

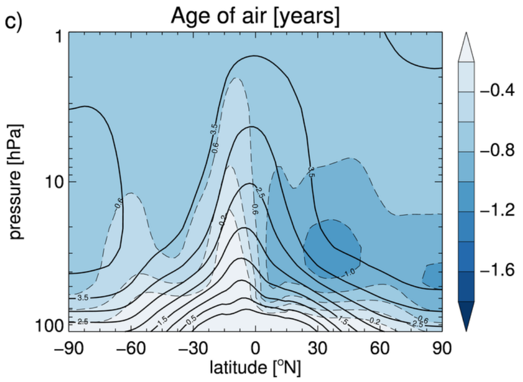
f) It is claimed that the stratospheric cooling due to higher CO2 levels explains the changes in polar vortex strength, but why would the CO2 cooling affect the meridional temperature gradient rather than lead to a meridionally uniform cooling, which would not affect the polar vortex? With the presented results the cause of the strengthened vortex remains confusing.

We thank the reviewer for noticing this flawed explanation. This comment has prompted us into analyzing more thoroughly the mechanism. Indeed, the CO_2 cooling in the stratosphere should be uniform and hence should therefore not affect the meridional temperature gradient. As shown on Figure 1b of the former manuscript, however, the increase of ozone in the upper stratosphere could inflect further the shortwave heating rates gradient on the winter hemisphere. This is indeed what we found with an overall more negative meridional SW heating gradient throughout the depth of the stratosphere which maximizes at the stratopause. This effect is however balanced by the fact that equatorial temperatures are decreasing at a faster rate than the polar temperatures in the Eocene simulation. So the net difference is slightly positive in the middle stratosphere and negative at the stratopause level. Radiative effects on the polar vortex appear hence to be modest compared to the changes in the wave activity described in the previous comment. The manuscript has been revised accordingly.

=> confusing statements regarding CO_2 radiative effect is now removed in the revised version of the manuscript (section 3.2 and conclusion) and a discussion related to changes in the wave activity and the stratospheric wave drag (based on the diagnostics shown in the response to comment e)) is now added.

Minor Comments:

Fig. 1: why not present AoA similar to panels a, b (difference as color shading with PI control as black contours)?



Rev2 Figure 9. Age of air (contour lines) calculated after 20 years of simulations by taking as a reference entry point the equatorial lowermost stratosphere, slightly above the tropopause (i.e. pressure level corresponding to 74 hPa). Shaded contour shows the difference between the Eocene and preindustrial experiments.

Interestingly, it appears that the Brewer-Dobson acceleration is more intense in the Northern Hemisphere, consistently with the wave-mean flow interaction diagnostics.

=> This age of air figure now replaces Figure 1c of the former manuscript.

Also: what are the units for the presented PI O3?

=> This is now clarified.

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)

=> Some elements have been added in the revised version of manuscript based on the discussion of the major comment c).

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

=> We now mention the model top (~70 km) *"15 levels above 20 km and around 24 above 10km and a lid height at ~70 km"*.

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

=> yes, this is now modified in the revised version

page 5, line 17: please explain "LPJ"

=> LPJ is the Lund-Potsdam-Jena vegetation model. This is now clarified.

page 5, line 24: please provide justification / motivation for why you choose a CO2 value at the low end of what's recommended

As explained earlier, we used an existing protocol for Eocene to show for the first time the potential importance of the stratospheric ozone feedback for past climate simulations. We will continue to investigate this issue with more complete set of simulations in the future (which will arise from the DeepMIP protocol).

page 6, line 1-2: please provide more detailed explanation for why radiative effect due to enhanced CH4 and N2O levels would be accounted for by enhanced CO2?

Even if inferred from proxies, the temperature changes for Eocene are better known than the greenhouse gases content (for which only CO_2 level can be inferred but with large uncertainties). For this reason, paleoclimate modellers have investigated the Eocene climate running simulations with various CO_2 covering a large range of concentrations with the aim of reproducing the amplitude of surface temperature changes. They do not change the level of other GHGs because no data are available on them. However, as only CO_2 is adjusted to match the temperature difference it means that the CO_2 perturbation implicitly represents the sum of all the GHG perturbations. This methodology is the one recommended by Lunt et al. 2017 for the DeepMIP project. For that reason we only perturb CO_2 in FOAM and LMDz for Eocene simulations.

Thus, In the LMDz-REPROBUS chemistry-climate modelling, fixed CO_2, CH_4 and N_2O are used as inputs to the radiative scheme. As a result, only ozone changes influence the climate during a preindustrial or Eocene simulation. That way, the effect of ozone changes on middle atmospheric climate can be isolated and quantified. Obviously, ozone chemistry is also affected by changes in N_2O and CH_4, two key stratospheric source gases. To account for this effect, there are CH_4 and N_2O chemically active tracers (i.e. modified by the transport and chemistry schemes) and whose surface concentrations are taken from the modelling study of Beerling et al. 2011. Their global distributions change with time during a simulation, but they are not used as inputs to the radiative scheme and hence their changes do not affect the climate, only ozone changes do.

=> we have now clarified this point in section 2. 2 and in the Table 1

page 6, line 10: "80s" - you mean the 1980's? Is this meant to represent an "ozone-hole climate"? Why not simply use the O3 field from, e.g., a CCMVal-2 chemistry-climate simulation with your model?

Indeed, we mean 1980's, it is now clarified in the text. We describe here what is available in our atmospheric model.

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?

In present climate conditions, the polar amplification is well more pronounced in the Arctic than in the Antarctic. This has been the subject of many studies and several key processes have been identified; i.e. the surface albedo feedback [e.g. *Serreze and Francis*, 2006] (increase in surface absorption of solar radiation due to snow and ice retreat), temperature feedbacks [e.g. *Pithan and Mauritsen*, 2014] or changes in poleward heat transport [e.g. *Graversen et al*.,2008], etc. Large uncertainties however remain in quantifying the contribution of these various processes. Under Eocene climate, EOMIP simulations from various Earth-System model analyzed by *Lunt et al*. [2012] revealed a stronger Antarctic than Arctic amplification, consistently with proxy records. The greatest warming in the Antarctic region is due to the lower topography (see Figure below) via the lapse rate effect and the change in albedo. The role of topography on Antarctic amplification was further demonstrated by *Sazlmann* [2017].

=> A short mention of this has been added in section 3.1 of the revised version of the manuscript, when Figure 1 is described.

Ref:

Serreze, M. C., and J. A. Francis (2006), The arctic amplification debate, Clim. Change, 76, 241–264.

Pithan F and Mauritsen T (2014), Arctic amplification dominated by temperature feedbacks in contemporary climate models, Nat. Geosci. 7 181–4

Graversen, R. G., Mauritsen, T., Tjernstrom, M., Kallen, E., and Svensson, G.: Vertical structure of recent Arctic warming, Nature, 451, 53–56, 2008.

Lunt, D. J., Dunkley Jones, T., Heinemann, M., Huber, M., LeGrande, A., Winguth, A., Loptson, C., Marotzke, J., Roberts, C. D., Tindall, J., Valdes, P., and Winguth, C.: A model–data comparison for a multi-model ensemble of early Eocene atmosphere–ocean simulations: EoMIP, Clim. Past, 8, 1717-1736, https://doi.org/10.5194/cp-8-1717-2012, 2012.

Salzmann, M.: The polar amplification asymmetry: role of Antarctic surface height, Earth Syst. Dynam., 8, 323-336, https://doi.org/10.5194/esd-8-323-2017, 2017

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

=> done

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)?

=> section 3.2 of the manuscript has been revised accordingly (and deeply). See also response to 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 . . .

We agree, this is misleading.

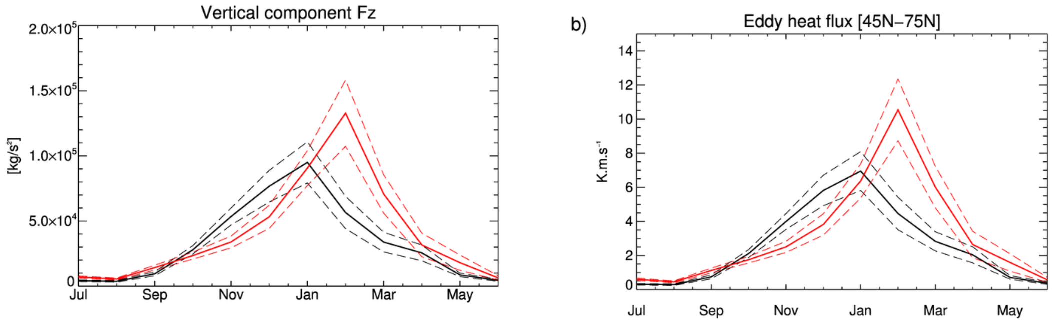
=> The part on this has been removed in the revised version and section 3.2 is now entirely revised.

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?

The vertical component of the EP flux is expressed as

$$F_z = \rho_0 a \cos\Phi\, f \frac{\overline{v'\theta'}}{\partial\bar{\theta}/\partial z}$$

where ρ_0, a, Φ, f and Θ are the air density, earth radius, latitude, Coriolis parameter and potential temperature, respectively. Under Eocene conditions, the planetary vorticity could have been slightly different as the Earth was rotating faster (the length of the day would have been less than an hour less corresponding to less than 4% difference), but this is not accounted for in our simulations. The vertical gradient of the potential temperature in the stratosphere increases as a result of higher CO_2 but the difference is small and almost null at 100 hPa, where we calculated it. The difference is hence expected to be modest. As shown by the comparison on Rev2 Figure 10 below, the heat flux appears to be sufficiently accurate (note that the eddy heat flux is multiplied by cosΦ). Finally, note that the meridional eddy heat flux term is, per definition, a quantification of the departure from the zonal mean which characterizes the wave itself, so this appears to be the major term in the measure of the wave activity.



Rev2 Figure 10. (left) vertical component of the Eliassen-palm flux at 100 hPa and averaged in the 45-75°N latitude band for (black) Preindustrial and (red) Eocene experiments. (right) same as left but for the meridional eddy heat flux.