The paper by Watanabe and co-authors present simulations of the effects of Earth's orbital variations on climate with a focus on how ozone may affect the ability of models to reproduce the reconstructed climate of the mid-Holocene (MH) and Last Interglacial (LIG) warm periods. The analysis builds on, and in some aspects conflicts with, previous work (Noda et al., 2017) that used an earlier version of the MRI model but only looked at the MH. The authors find some significant and physically consistent changes in ozone, temperature and winds in the stratosphere but the impact in the troposphere and, in particular, on the surface climate are relatively modest.
I was not one of the reviewers for the first submitted version of the paper, nor have I read that version. I have, however, read the comments from the reviewers (after having read the current version) and feel that the revisions to the structure and presentation of the results may have gone some way to address the concerns from reviewer 2 of the first version. I certainly did not have similar concerns about the presentation when reading the paper.
We are grateful to the reviewer for the comments and suggestions, which have helped us to improve the manuscript. As indicated in the responses that follow, we have taken all these comments and suggestions into account during the revision process.

My one significant concern about the discussion of the results centers around the near-surface wind response around the South Pole shown in Figure 9 and discussed starting around line 239. Particularly striking is the seasonal change in the response, with strong positive anomalies for the LIG simulation in JJA-SON, and strong and relatively symmetric weakening in DJF and MAM. I can see the physical explanation for the acceleration of the near-surface winds for JJA and SON being connected to similar signed changes in the stratosphere in a way that is quite analogous with what is seen for ozone depletion since the 1980s. But the changes in DJF and MAM are much harder to connect with the temperature changes that have been presented. The seasonal cycle of temperature change at 3hPa (Figure 3) does not show much of a change in temperature gradient across the mid-to-high latitudes of the Southern Hemisphere for DJF and MAM so I suspect there is some change in temperature that is not apparent in the figures that is driving the weakening of the winds. I would like to see just a little bit more supporting information or discussion to provide an explanation for the strong seasonal behaviour of the impact on the near-surface winds around the Antarctic.
We appreciate the reviewer for pointing this out. As mentioned by the reviewer, the acceleration of the near-surface wind patterns for JJA and SON is associated with the temperature changes in the stratosphere, which is originally driven by the different astronomical forcing. On the other hand, the weakening of the near-surface wind patterns for DJF and MAM originates from the upper troposphere, reflecting the cooling of the troposphere in the tropics (Fig. 5a and 5b). Because this cooling is not as strong as in the mid-high latitude regions in the southern hemisphere (Fig. 7a and 7b), this decreases the meridional temperature gradient in the troposphere, weakening the surface wind in DJF and MAM around Antarctica. We have enriched our discussion as follows:

> *"This wind response would be caused primarily by the decrease of the meridional temperature gradient in the troposphere because of the cooling of the troposphere in the tropics in DJF and MAM (Fig. 5a and 5b), which is stronger than that in the high-latitude region (Fig. 7a and 7b)."*

My other comments are minor and are itemized below.

Line 57 – this study extends previous study of Noda et al., (2017) that used MRI-ESM1 to investigate ozone effects during the MH.

Yes, our study extends the result of Noda et al. (2017) to not only the MH condition but also the LIG condition using the MRI-ESM2.0.

Lines 57 – 62: The section beginning 'This study showed that the positive ozone…' through to '…different astronomical forcing during the MH.' has a few instances of repetition that could be removed to make it a bit easier reading.

We have rewritten the sentence as follows:

> *"This study showed that the positive ozone anomaly in the upper stratosphere around the South Pole during the austral summer of the MH would propagate to the lower stratosphere in the austral winter. This anomaly would increase the air temperature and weaken the southern westerly jet. They further suggested that it may contribute to the retreat of sea ice in the Southern Ocean during the MH. They showed that the positive ozone anomaly with a maximum value of ~0.2 ppm in the stratosphere would be caused by the different astronomical forcing during the MH."*

Lines 89 – 90: The MH and LIG simulations started 'from the steady state obtained for the MH condition submitted to PMIP4.' It is not quite clear what the 'steady state' from the PMIP4 simulation is. I assume the simulations presented here start from the model state after the first 51 years of spin-up for the PMIP4 simulation, but as the authors have explained this is not necessarily a steady state.

The reviewer is correct for pointing this out. We have reworded the sentence as follows:

> *"...starting from the calculation of the MH condition submitted to PMIP4"*

Line 101 – Table 1: In the greenhouse gas column there is reference to NO_2, but I am certain this is a typo and should be N_2O.

Corrected.

Lines 118 – 126: The discussion of the temperature effects on ozone needs to be strengthened. I am most familiar with the temperature effects due to changes in CO_2, for which the impacts on ozone through the temperature sensitivity of ozone loss reactions is well known. Here the irradiance is changing so in addition to the temperature effects on $O_3 + O \rightarrow 2O_2$, there would be changes on O_2 and O_3 photolysis. Given the strong anti-correlation of temperature and ozone changes, I am not doubting that the temperature effect dominates only that the discussion is a bit underdeveloped.

We agree with the reviewer. We have enriched our discussion in the revised manuscript, as follows:

> *"Although the insolation anomaly in the MH and LIG would also affect the photochemical reaction rates of Reactions R2 and R4, the negative correlation indicates that the temperature effect of Reaction R1 would dominate the behavior of stratospheric ozone anomalies in the MH and LIG."*

Line 152 – Figure 4: The presentation of annual mean zonal cross section changes really dilutes the impacts of the changes. The seasonal cycle of changes at 3hPa shown in Figure 3 has the LIG simulation with much larger changes than those seen for the MH, but in the annual average these appear weaker. In particular, the strong positive changes in temperature seen near the north pole in JJA for Figure 3d are now negative in the annual

average shown in Figure 4b. Is the manuscript losing important aspects of the response by presenting annual average changes? Because of the nature of the changes in forcings I can appreciate that it is not as simple as showing JJA and DJF – the insolation anomaly in the MH maximizes in JJA over the northern high latitudes but in SON for high southern latitudes.
The reviewer is correct in pointing out that seasonal changes are important for ozone dynamics in the MH and LIG. This figure was added to illustrate that the comprehensive view of ozone distribution (e.g., the presence of the ozone layer in the stratosphere) remains largely unchanged, even in the MH and LIG, following the previous review comment. Because we have already been extensively discussed in the manuscript, we have retained Figure 4 in the revised manuscript.

Line 170: The use of the word 'transported' in 'This signal was transported into the lower stratosphere.' seems to suggest there was advection of the warm anomaly seen earlier in the year at lower pressure downwards. I think this is difficult to justify. The warm anomaly does seem to follow the ozone anomaly, so I would think it is more of a perturbation of local heating rates.
Because the word "transported" was misleading, we replaced it with the word "propagated", as follows:

> *"This signal propagates into the lower stratosphere. For the case of the LIG, the positive ozone anomaly originated from the upper stratosphere between January and May is present in the lower stratosphere between February and August, whose impact is larger than 1 K. This signal may partly propagate into the troposphere, …"*

Lines 176– 183: The authors might make it clearer that the discussion is focusing on the differences between the differences in the full simulations and the nochem simulations.
We have enriched our explanation in the revised manuscript, as follows:

> *"Comparing the air temperature anomalies obtained for the control experiments and those obtained for the noChem experiments, the impact of the atmospheric ozone changes on the zonally averaged climate was small in both the MH and LIG in the south of 60˚N."*

Lines 233 – 236: I believe I understand the explanation the authors are suggesting for the acceleration of the southern westerly jet. If I do, the authors could make reference to Figure 3c and 3d to show the stronger meridional temperature gradient driven by warming on the equator-ward side of the jet that is responsible for the acceleration.
We have enriched our explanation in the revised manuscript, as follows:

> *"The westerly jet intensity is related to the meridional air temperature gradient in the stratosphere at the mid-latitude regions, which is attributed to the larger meridional temperature anomaly owing to the different insolation forcing (Fig. 3c and 3d)."*

Lines 299 – 300: Similar to the comment on line 170 about the use of 'transported' when discussing temperature anomalies, is it the temperature transported or the ozone anomaly that carries along with it a heating rate anomaly?
Because the word "transported" was misleading, we replaced it with the word "propagated".

Line 301: The statement 'possibly reflecting the modulation of the Arctic Oscillation.' seems unsupported by analysis. And I am not sure what the authors wish to imply with this statement.

The original sentence was misleading, so this sentence was removed in the course of this revision.

Line 303: ‘This ozone-induced expansion of sea ice…’ The caption for Figure 13 states that the dashed line is the sea-ice edge for the PIcontrol, while the solid line is for the MHcontrol or LIGcontrol experiment. When you look closely at Figure 13g, the dashed and solid line seems to suggest that the sea-ice has retreated in the Atlantic side of the Arctic between PIcontrol and LIGcontrol, but the ozone related temperature changes are negative. The idea that the ozone-driven cooling is partly counteracting an insolation-driven warming is supported by Figure 12. But then can it be deduced that the ozone-driven changes are also responsible for counteracting some of the sea-ice retreat?
We appreciate the reviewer for pointing this out. The sea ice edge in the LIG, for example, is smaller than that in the PI in SON, as shown in the figure, reflecting the stronger insolation forcing in summer in the LIG. On the other hand, the impact of changes in ozone distributions works to suppress the insolation-driven reduction in sea ice distribution. Because the original sentence was misleading, we have rewritten the sentence as follows:

> *“While the Arctic sea-ice distributions in the MH and LIG are smaller than in the PI in SON owing to the effect of the different astronomical forcing (black solid lines in Fig.13), the cooling effect from the changes in ozone distributions works to suppress the reduction in sea-ice distribution due to the different astronomical forcing. This ozone-induced net expansion of sea ice would be…”*

Line 311: Figure 12 is for the North pole but the caption has ‘at the tropic region (60–90˚N)’.
Corrected.

Comments from Editor:

Abstract:
L. 15: or "MH and LIG, respectively"?
Corrected.

L. 16: "is" small
Corrected.

Last sentence of abstract needs to be rephrased by making it more specific/less vague.
We have rewritten the sentence as follows:

> *"This is the opposite of the previous finding that implies the importance of ozone in southern hemisphere climates, indicating the need for further assessment of how dynamic ozone variations affect climate and atmospheric structures during past warm interglacial periods using multiple Earth system models."*

L. 35-36:something is missing here. This sentence does not make sense. Maybe you are referring to sea-ice extent?
We appreciate the editor for finding this mistake. We have rewritten the sentence as follows:

> *"Indeed, the minimal extent of sea ice in both Arctic and Antarctic regions would have been smaller than in the PI condition in the MH and LIG owing to the stronger insolation"*

L. 242: Results section should be in present tense.
Corrected throughout the Results section.

L. 243-245 and 249-250: Are you describing your results or those of Noda et al? You might want to move all the comparisons to Noda et al., to the discussion.
Following the suggestion, we have moved these sentences into the Discussion section.

Caption of figure 12: 60N-90N are not the tropics.
Corrected.

Conclusions: I suggest to choose a tense (past or present) and use it throughout the Conclusion.
We have chosen the present tense in the Conclusions.

L. 374: Add "during the MH and LIG".
Done.