***Interactive comment on*** **"Quantifying climate feedbacks in the middle atmosphere using WACCM"** ***by*** **Maartje Sanne Kuilman et al.**

**Anonymous Referee #3**

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I am sorry to say that I am still not happy with this paper in its present form.

First of all, we would like to thank the reviewer for his or her comments. We do point out that almost all of these questions were already addressed in the pre-review for this paper. Large parts of the paper have been rewritten (see also comment on reviewer #2) and we hope that the reviewer is more satisfied with the paper.

The paper is missing a clear message and many statements remain vague.

What we do in this paper is to apply a new method to quantify the temperature responses to different feedback processes that arise in response to changing the $CO_2$-concentration. This is one of the first studies in which it is calculated how much of the temperature change in a specific place in the atmosphere is attributed to which feedback process. The method we applied here can quantify the temperature response, but to provide a complete explanation of all the responses and the exact mechanism behind all the feedback processes is outside the scope of this paper.

For example in the abstract: "feedback processes" (l 51) but which processes?

The "feedback processes" we mean chemical, physical and dynamical processes, which can feedback to the radiation and further change the temperature, in our study these processes arise due to a change in CO2 concentration.

We understand that the formulation in the abstract was maybe not very clear and the abstract has now been rewritten.

*The importance of the middle atmosphere for surface and tropospheric climate is increasingly realized. In this study, we aim at a better understanding of climate feedbacks in response to a doubling of $CO_2$ in the middle atmosphere using the climate feedback response analysis method (CFRAM). This method allows one to calculate the partial temperature changes due to an external forcing and climate feedbacks in the atmosphere. It has the unique feature of additivity, such that these partial temperature changes are linearly addable. We find that the temperature change due to the direct forcing of $CO_2$ increases with increasing height in the middle atmosphere, with the cooling in the upper stratosphere about three times as strong as in the lower stratosphere. The ozone feedback yields a radiative feedback that generally mitigates this cooling, however in the tropical lower stratosphere and in some regions of the mesosphere, the ozone feedback cools these regions further. The temperature response due to dynamical feedbacks is small in global average, although the temperature changes due to the dynamical feedbacks are large locally. The temperature change in the lower stratosphere is influenced by the water vapour feedback and to a lesser degree by the cloud and albedo feedback, while these feedbacks play no role in the upper stratosphere and the mesosphere. We find that* *the effects of the changed SSTs on the middle atmosphere are relatively small as compared to the effects of changing the $CO_2$. However, the changes in SSTs are responsible for large temperature changes as a result of the dynamical feedbacks and the temperature response to the water vapour feedback in the lower stratosphere is almost solely due to changes in the SSTs. As CFRAM has not been applied to the middle atmosphere in this way, this study also serves to investigate the applicability as well as the limitations of this method. This work shows that CFRAM is a very powerful tool to study climate feedbacks in the middle atmosphere. However, it should be noted that there is a relatively large error term associated with the current method in the middle atmosphere, which can be for a large part be explained by the linearization in the method.*

CFRAM (l 54) is not known to me; I am not sure how helpful this statement is without further explanation to the readers of ACP.

Further explanation of the method is now added in the abstract (see above).

I would like to refer to Taylor et al., 2013; Song and Zhang, 2014; Hu et al., 2017; Zheng et al., 2019, who have used CFRAM as a practical diagnostic tool to analyse the role of various forcing and feedback studying surface climate change.

Response to $CO_2$ doubling but at what time has the doubling been reached – would that not be important for the issue of stratospheric ozone? Ozone feedback is mentioned (l 63), but what is assumed for ozone in the upper stratosphere? We know upper stratospheric ozone is "recovering" over the coming decades (WMO, 2018); is this the point here?

We are not speaking here about the changes in $O_3$-concentration due to the ozone hole, but rather changes in ozone concentration that are resulting from changes in the $CO_2$ concentration. The ozone concentration in the control run is for pre-industrial conditions. We change the CO2 and/or the SSTs in the model and compare the two equilibrium states. In runs with the changed CO2 and/or the SSTs the ozone concentration is changed due to the changes in CO2 concentration only. The model has interactive chemistry which calculates the amount of ozone concentration.

And a "warming by 1.5 K, but in which region? Changes in dynamics play a large role (l 66) but which changes, which role? And above 0.1 hPa, which is certainly a region where an ozone feedback is expected. Above 0.1 hpa is above 60 km; this is the mesosphere.

In the earlier version of the paper, average temperature changes over the whole middle atmosphere region were taken. Although this can learn us something, we have now divided Fig. 2 for the regions: lower, upper stratosphere and mesosphere.

Several tropospheric issues are "of minor importance" (l 69), but why is this issue discussed in an abstract of a paper on the "middle atmosphere"?

We investigate the temperature responses to feedback processes. Some of these processes might have an effect in the middle atmosphere, and we see that this is indeed the case for the lower stratosphere, however not for the regions above.

Further comments:

l. 85-90: It should be clearly said that the middle atmosphere is *not* in radiative equilibrium in most regions; downwelling in the polar regions in winter is part of the BD circulation, and not only an "example". See e.g. Dunkerton, JAS, 1978.

This formulation was written similarly in the PhD thesis 'The middle atmosphere and its sensitivity to climate change' by Andreas Jonson, which I read several times while working on this paper (https://www.diva-portal.org/smash/get/diva2:198863/FULLTEXT01.pdf).

I have changed this paragraph to emphasize the role of dynamics for the temperatures in middle atmosphere.

*The circulation in the troposphere is thermally driven, however this is quite different for the middle atmosphere. The air in the middle atmosphere is out of reach for convection and is not in direct contact with the Earth's surface, which means that the middle atmosphere is dynamically stable. In the absence of eddy motions the zonal-mean temperature would relax to a radiatively determined state. However, a wave driven motions of the air drive the flow away from this state of radiative balance and in this way determine the heating and cooling patterns in the middle atmosphere (Shepherd, 2010).*

l 115-122: Ozone is mentioned here, but it is well established that stratospheric ozone responds to changing halogen levels (WMO 2018). This aspect can not be ignored in this study.

While the reaction of ozone to CFCs and the consequent ozone hole are very important and interesting topics in itself, this is not the topic of this study. What we do here is investigate the temperature changes that arise from the direct forcing of CO2 and the feedback processes that result from this increase in CO2. We have not investigated the effect of the depletion/recovery of ozone.

Sec. 2.3: Is this a new formulation of CFRAM or is this section just reiterating a technique used before? It looks like a new description here as there are no references to previous description of CFRAM at the top of sec 2.3.

Partly this is a new formulation. The method is based on the CFRAM method as described in Lu and Cai (2009), however the method needed to be adjusted in order to be applicable on WACCM output data. I referred to Lu and Cai at the end of the section, but have now added an additional reference at the beginning of the section.

l 731-735: No, the ozone concentration is *not* controlled by the Chapman reactions "for a large part". Depending on altitude, NOx and HOx cycles are important; this is well known (check the textbook you are citing). Also chlorine compounds are relevant.

This part has been rewritten:

*Ozone plays a major role in the chemical and radiative budget of the middle atmosphere. The ozone distribution in the mesosphere is maintained by a balance between transport processes and various catalytic cycles involving nitrogen oxides, $HO_x$ and $Cl_x$ radicals. In the upper stratosphere, $NO_x$ and $CL_x$ cycles dominate, while hydrogen species are of most importance in the mesosphere (Cariolle, 1982).*

l 774: The direct influence is only possible where the chemistry not too fast, again check the Brasseur and Solomon textbook.

The section on the changes in the ozone concentration due to the changed $CO_2$ concentration has now been rewritten and significantly shortened, as this is not new and not the main point of our work: we are interested in the temperature response as a result of the changes in the O3 concentration in WACCM.

Sec. 3.5: The feedback for stratospheric H2O is not that simple, see eg. Solomon et al., Science, 2010; Riese et al, JGR, 2012).

The caveat that all models don't consistently reproduce the tropopause temperatures has been added.

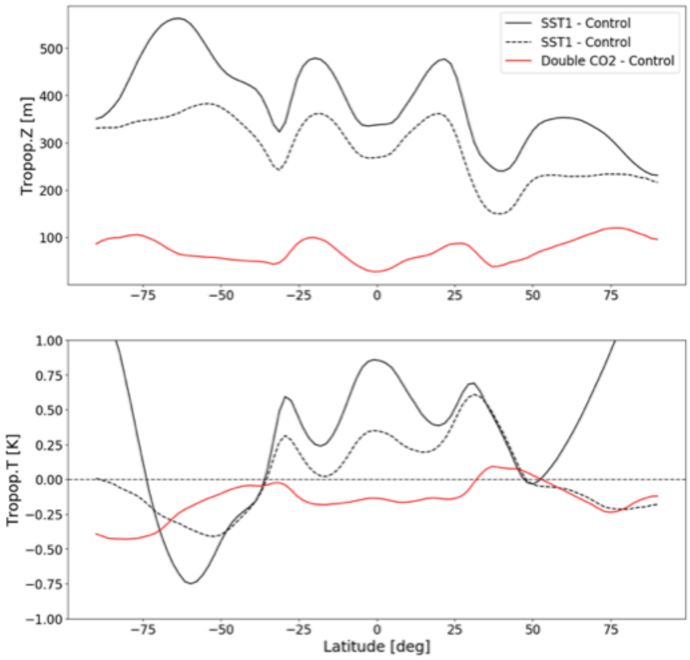
*It can be seen that changing the SSTs leads to an increase in water vapour almost everywhere in the middle atmosphere (Fig. 10c and f). In WACCM, the increase in SSTs is observed to lead a higher and warmer tropopause, which can explain this increase of water vapour. However, it should be noted that models currently have a limited representation of the processes determining the distribution and variability of lower stratospheric water vapour. Minimum tropopause temperatures aren't consistently reproduced by climate models (Solomon et al., 2010; Riese et al., 2012). At the same time, observations are not completely clear about whether there is a persistent positive correlation between the SST and the stratospheric water vapour neither (Solomon et al., 2010).*

Several points are mixed here. There might be a change in stratospheric water vapour based on changing climatic conditions and this change could have an impact on local heating/cooling but also an effect of the surface radiative forcing. These issues should be disentangled.

The CFRAM method here calculates the temperature change on the basis of the radiative changes due to changes in water vapour alone. This method doesn't allow further disentanglement. Our results show that that the regions where there is an increase in the water vapour, there is a cooling, and vice versa. The water vapour feedback as calculated here only takes into account radiative processes, if the water vapour feedbacks on the temperature via dynamical feedbacks this would be shown as a result of the dynamical feedback.

I 820: warmer tropopause: also in the tropics? (where water vapour is entering the stratosphere). Would you not expect higher SSTs causing more wave activity and thus a stronger tropical upwelling? Why is this argument not correct?

Yes, WACCM a warmer tropopause in the tropics as well (the tropopause temperature comes as an output of the model). See here the changes in tropopause temperature for the different latitudes in July.




A study by Deckert and Dameris (2008) indeed shows that higher tropical SSTs can indeed strengthen the tropical upwelling into the stratosphere. However, as explained by Solomon et al. (2010) the transport of the water vapour in the stratosphere is mainly a function of the tropopause temperature. We see that this is changing, which can explain what is seen in the model.

I 815-816: Is this reduction expected from simple water vapour equilibrium (over ice) arguments? Citations?

I assume the reviewer is referring to an earlier version of the manuscript. A suggestion was made, but based on the comments of other reviewers this was taken out.

Sec 3.6 discusses feedbacks that "play only a very small role" for the middle atmosphere. Fig 12 shows that the middle atmosphere is largely grey (zero effect). But the caption tells me that the comparison is to pre-industrial conditions, so the reported "delta" is due to a change relative to pre-industrial? Then one would expect a larger signal – correct? And the discussion starting in I. 810 is not discussing "pre-industrial"; this is confusing.

Yes, the delta T is the temperature change between the run with the changed SSTs and/or $CO_2$ and pre-industrial conditions. Exactly, we see that these feedbacks (albedo and cloud) basically only effect the temperatures in the lower stratosphere and not in the upper stratosphere and the mesosphere. It would have been interesting to see a larger signal higher up in the atmosphere, but it is not there.

I speak about middle atmosphere climate sensitivity which is here used to refer to how much the middle atmosphere will cool or warm after the doubling of $CO_2$ as compared to the pre-industrial state. Figure 12 is no different from Fig. 5 ,7, 9 and 11. It shows the temperature response to the changes in SSTs for the run with high SSTs as compared to pre-industrial conditions.

L 964-972: these lines seem to describe the overall conclusions of the paper; when I read these lines they seem to tell me that CFRAM is okay, but that some refinement is necessary. This is a rather technical statement (which would be more helpful if statements like "some" would be more specific). Most importantly however, the paper promises to talk about "quantifying (!) climate feedbacks in the middle atmosphere" – in my view this has not been achieved in this manuscript.

This is the first study in which it is calculated how much of the temperature change in a specific place in the middle atmosphere is attributed to which feedback process in response to a doubling of CO2. We succeeded in performing the calculations and indeed quantifying these temperature responses. Fig 2, 3, 4, 5, 7, 9, 11 and 12 show the quantification we aimed at achieving. Another goal of the paper was to investigate the applicability of this traditional form of method in the middle atmosphere (contrary to what is done by Zhu et al.). We have learnt that it can be used, but indeed it is not perfect. The linearization is a fundamental part of the method and leads inevitably to errors.

References:

* The current report on stratospheric ozone is WMO 2018, I suggest using this most current information (see above)

This reference is no longer there.

Brasseur and Solomon 2005: this is an excellent text book but might not give the most up to date information required here on upper stratospheric ozone

We think this fundamental relationship between the temperature and water vapor in the middle atmosphere is not out of date. We added further references in the text.