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Atmospheric Chemistry and Physics Discussions

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. The paper is missing a clear message and many statements remain vague. For example in the abstract: "feedback processes" (l 51) but which processes? 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. Response to CO2 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? 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

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is above 60 km; this is the 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"?

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.

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.

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.

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.

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

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

l 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?

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

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 l. 810 is not discussing "pre-industrial"; this is confusing.

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.

References:

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

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

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