Dear Referee,

Thank you very much for this detailed review of our manuscript. Your comments helped us to improve the manuscript and we hope that we could address all of your major concerns in the revised manuscript. Please find below our responses to each of your comments. Major changes include:

- We included WACCM in the manuscript title
- We added more information about the NAT parameterization in WACCM
- Some more information about the method how to calculate the max(d) values is provided in Sect. 2.2
- We added two new figures comparing MIPAS HNO_3 with WACCM as (1) a timeseries and (2) a scatter plot, see discussion below and in the revised manuscript

Thanks again for your review and on behalf of all authors,
Michael Weimer

1 General Comments

1.1 The most important point I would like to address is the lack of comparison of the simulated NAT density with observations. Although this is the most important set screw for this study, no attempt is made to constrain the NAT density used in this modeling study with observational data. Satellite-based observations of NAT density are mentioned in the introduction but are not discussed further. The model results are compared to more than 10 years of trace gas observations. I assume that observations of NAT density are also available during this period, at least temporarily.

– and –

The NAT density now recommended as an input parameter differs by a factor of 20 from the previous one. Although the simplifications of the model might not allow direct integration of the measured quantities, it is necessary to relate the model simulations to observations of the NAT density.

The previously used NAT number density of $10^{-2}\,\mathrm{cm}^{-3}$ was based on measurements by CALIOP, but as we show in this study, this value is too large to reflect

an effect of dentitrification comparable to the MIPAS trace gas measurements. We added some details about the NAT parametrization to Sect. 2.1 and also added some of the main simplification of the NAT scheme in WACCM: (1) NAT particles in one $100 \times 100\,\mathrm{km}^2$ grid box are assumed to have one size which is not the case in reality, (2) NAT particles in this scheme are not allowed to grow or decrease over time and (3) the whole amount of supersaturated gaseous HNO3 is transferred to NAT. Due to these simplifications, we think that this value should not be guided by measured values of the NAT number density. Nevertheless, we added some measured values of the NAT number density to put this value into context.

1.2 The manuscript states that reduced NAT density in the model setup leads to better agreement between model simulations and observations. And this seems to be true for HNO3, ClONO2 and ozone. But why is this so? What are the underlying physical and chemical processes that are responsible? At least a rudimentary attempt should be made to explain this plausibly.

A lower NAT density will lead to a vertical redistribution of HNO3 (lower HNO3 at high altitudes and higher HNO3 at lower altitudes). During early spring, HNO3 will be photolyzed and form NO2 which will combine with ClO to form ClONO2. ClO is responsible for the catalytic ozone depletion and deactivated through this previous reaction. Therefore, denitrification will influence HNO3, ClONO2 and ozone. We added this explanation at the start of Sect. 3.

1.3 In general, a more detailed discussion of the simplifications of the model would be helpful to better classify the quality of the presented comparisons between model and observations in this study. For example: the statement that "... so observed NAT particle abundances may therefore not be the best guide for this parameter choice ..." (page 3, line 68) should be discussed in a bit more detail.

We added a more detailed description of the NAT parameterization of WACCM in Sect. 2.1 and also put this statement into this context.

2 Specific comments

2.1 Page 4/line 105: Although the term "maximum difference" might be common in the modeling community, it would be useful for a broader readership to see this concept presented and explained in a bit more detail.

We added the equation how the maximum difference is calculated, now Eq. 1 in the revised manuscript.

2.2 Page 5/line 121: "... Turning off all heterogeneous chemistry except for N2O5 + H2O ..." Why is this reaction not turned off?

We are focusing on the effect of halogen chemistry and denitrification, which are highly temperature dependent. The N2O5+H2O reaction rate is nearly independent of temperature, happens on nearly all aerosols and does not directly affect the halogen chemistry, which is why we kept this reaction for noHetAll.5e-4. We added an explanation to the revised manuscript.

2.3 Figure 1: The labels in the partial figures take up too much space (HetAll ....). This means that the actual content of the figure is not displayed optimally. Since the color coding of the simulations is the same in all subfigures, it could be placed separately next to the figures. The indication "max(d)" should of course remain in the figure.

We made the labels annotations in each panel and put the legend below the figure.

2.4 Figure 1: What does the indication "nprof:...." at the top of the figure mean?

It's the number of profiles used to derive the PDFs of the panel. It depends on the size of the polar vortex and is different for each year and hemisphere. We added this explanation to the caption of Fig. 1.

2.5 Figure 1 to 4: The volume mixing ration of nitric acid in the stratosphere varies with altitude, similar as ozone (Figure 5). In addition, sedimentation of PSC particles leads to a change in the height distribution of HNO3. In Figures 1 through 4, only altitude-independent concentration is given. Please explain briefly how this value is obtained. Would it not be possible, at least for one case of HNO3, to choose a similar representation as for ozone in Figure 5?

We added a new figure showing the timeseries of vortex-averaged HNO3 profiles of MIPAS and all WACCM simulations, now Fig. 1 in the revised manuscript. In addition, we added a scatter plot including all altitudes which shows a concise correlation between WACCM and MIPAS in the case of HetAll.5e-4. This supports that we can use this large pressure range in the PDFs of the next figures.

2.6 Page 5/line 124: "... In HetAll.1e-2, larger HNO3 values are more common in all panels compared to the other simulations..." Can you explain this result? Why does a higher density in the particle phase lead to a higher gas phase concentration of HNO3 and vice versa? What processes underlie this behavior?

We added some statements about this to the revised manuscript: In Sect. 2.1 we explain now that a higher NAT number density leads to smaller particles, which then impacts the sedimentation velocity which is decreased. Therefore, a larger fraction of gaseous HNO3 remains at higher altitudes which can be also seen in the new Fig. 1 in the revised manuscript and which we also discuss there.

2.7 Page 7, line 148: In the manuscript, the discrepancy between model and measurement is attributed to the lower accuracy of the MIPAS instrument. By how much was the accuracy of the measurement reduced compared to the measurements included in Figure 1? Can model processes be excluded for this discrepancy?

We replaced "precision" by "accuracy" and added other possible reasons for the mismatch between MIPAS and WACCM for that year.

2.8 Figure 5: The "interquantile range" is very difficult to see in the figure.

We increased the line width of the interquartile range in this figure and also the figures in the supplement.