Reply to Reviewer #1, received and published 26 September

We would like to thank the reviewer for their helpful comments.

General Comments. Overall this is a very straightforward study that addresses the impact that future CH_4 and N_2O increases will have on ozone. The simulations are clearly defined and the results are clearly shown. This paper is a useful addition to understanding the role that CH_4 and N_2O independently play in affecting stratospheric ozone. I have a few specific comments that I would like addressed (see below).

Specific Comments:

Page 1, Abstract:

You should state, "Overall however, increasing the CH_4 concentrations lead to an increase in global-mean total column ozone." You should follow this sentence up with why this happens. (You did state "why" for the N_2O increases).

We have included a sentence stating why increasing CH_4 leads to ozone increases.

Page 2, discussion 16-20. Please summarize the mechanism where "…its effectiveness as an ODS is mitigated by increases in CH_4 and CO_2". I believe you are talking about the temperature dependence of $N+O_2$? E.g., Rosenfield et al. 2002, describes the mechanism in the following manner: "A reduction in NO_y with cooling has been shown [Rosenfield and Douglass, 1998] to arise from the very large temperature dependence of the reaction $N + O_2 \rightarrow NO + O$ (k ~exp[-3600/T]). The abundance of N is increased with cooling, leading to an increase in the loss of NO_y which is controlled by the reaction $N + NO \rightarrow N_2 + O$. This decrease in NO_y contributes to the reduction of the ozone loss frequency due to the NO_x cycle."

The reviewer is correct, and we have extended this discussion to include the mechanism described by Rosenfield and Douglass [1998].

Page 2, line 22, 23. List catalytic cycle. E.g., $H + O_3 \Rightarrow OH + O_2$, followed by $OH + O \Rightarrow H + O_2$.

We have listed the dominant HO_x-catalyzed ozone loss cycle for the upper stratosphere.

Page 3, lines 6 through 9. The authors state that "Furthermore, in the troposphere and lower stratosphere, where the concentrations of CO is sufficiently large (Lanzendorf et al., 2001), increased HO_x causes an increase in the rates of reactions (R2) – (R5), therefore leading to ozone production." In Lanzendorf et al., the CO abundance was coming from observations. In the CCM simulations, in this paper, I don't believe the tropospheric mechanism includes oxidation of NMHCs to form CO, therefore, the abundance of CO will be significantly less than what observations would suggest (in the troposphere and lower stratosphere). Therefore, based on this discussion, the ozone production process would be underestimated. This "apparent" lack of detailed tropospheric chemistry should be commented on if you are going to discuss photochemical smog processes in the lower stratosphere.

The reviewer is correct in saying that NIWA-SOCOL does not include NMHC oxidation. We have commented on this in the manuscript.

Page 4, lines 1 through 3. Please define here if you only include CH4 and its oxidation products (i.e., no representation of NMHCs).

See response above.

Page 6 discussion of Figure 4. It would be very interesting to also see a figure that showed the change in reactive odd nitrogen (NO_x) consistent with the "N_2O-8.5 minus N_2O-2.6" case, both in absolute change and percentage. Since the main production region for NO_x ($O(^1D) + N_2O$) is in the tropics, near 5-10hPa, this would show where the peak loading of NO_y is occurring. It would also

show that there is significant loading in the lower stratosphere causing the ozone change depicted in figure 4a. This is just a suggestion.
This is a good suggestion and we have included such a plot in the paper. We see enhanced NO_x loading particularly in the Antarctic lower stratosphere, which explains why the ozone decreases seen there (in Fig 4a) are more severe.

Page 6, lines 11-20. You could make a similar figure showing the change in reactive odd hydrogen (HOx) to complement figure 5. Again, this is just a suggestion to add more quantitative details on how the ozone precursors are changing.
While we agree that adding this additional figure would be interesting, unfortunately the model output files required to make the figure are not available. While the simulation could be repeated and these files generated, we feel that this would constitute only a very small incremental improvement to the paper. The additional figure would also be tangential to the main message of the paper. To avoid ‘mission creep’ on this paper, we have included only those figures which are key to communicating the main message of the paper, irrespective of how interesting they may be in their own right.