Referee #2

General: This paper analyses the various ways in which increases in N_2O affect ozone and stratospheric temperature, by conducting sensitivity simulations using a chemistry climate model. I think the general approach to this problem is fine, and I don't doubt the results presented by the authors. Embedding the results in the existing literature can be improved. I agree with reviewer 3 that the term "ozone depletion potential" has been used – rightly so – several times in the paper but has neither been defined nor quantified. This could be changed – Ravishankara et al. (2009) have done so. The total ODP of N_2O should be determined, and the various contributions to it (chemical, radiative, dynamical) can be quantified. Also the impact of N_2O increases has been modelled before; the findings of this study should be compared to these earlier studies. The language of the paper is mostly adequate expect for a few instances when the formulations come across as overly complicated, see below.

This is a good point! Due to the ambiguous definition of the ODP, we dropped out the term 'ODP' at most places in the revised text, introduced the 'classic' definition of ODP in the Introduction section and added some discussions on the N_2O ODP at the end of Section 4. Also see details in the response to the comments from reviewer #3.

Specific comments:

P29448L16: Replace "reverse" with "opposite".

Corrected.

P29449L10: Please also discuss the papers by Revell et al. (2012) in GRL and ACP. Both contain results that have to be compared to what is found here.

Thanks for the suggestion. The following text is added in the revised text:

'This result is supported by Portmann et al. (2012) and Revell et al. (2012a, 2012b), which discussed the importance of the nitrous oxide to ozone depletion through the 21st century.'

After P29450 L10 in the Introduction:

'The 'classic' definition of Ozone Depletion Potential (ODP) is defined as the time-integrated global ozone depletion induced by a unit of mass emission of gas X relative to a reference gas, i.e., CFC-11 (Ravishanka et al., 2009). Based on the 'classic' definition of ODP, the ODP of N_2O is a fixed value in different radiative and chemical environments (Ravishanka et al., 2009; WMO 2010). However, Portmann et al. (2012) pointed out that nonlinear interactions between N_2O and other gases may cause uncertainties in the ODP of N_2O. Revel et al. (2012a) further found that the effects of N_2O on ozone depletion depend both on the CO_2 induced cooling and the chemical effects of CH_4. '

P29L21: "ODS" (singular)

Corrected.

P29450L7: "a series"

Corrected.

P29451: In describing your scenarios, please avoid use of terms such as "1%/year". This would imply exponential growth when you mean linear growth. How about "In run E1, N_2O is increasing linearly from 344 ppbv in the year 2000 to 517 in 2050"?

Thanks for the suggestion. The sentence is rephrased as:

'In runs E1 and E2, the surface vmrs of N_2O were increased linearly from 344 ppbv in the year 2000 to 517 and 688 ppbv in 2050, respectively, which is 50% and 100% more than the N_2O value in the year 2050 in A1b scenario.'

P29453R4 and R5: Please provide references for the reaction rates.

The reaction rates for R4 and R5 are from Sander et al. (2006). The reference is added in the revised text.

P29453L26: Replace "reversed" with "opposite".

Corrected.

P29454L8: Break sentence into two.

The sentence is rephrased as:

'Meanwhile, projected decreases in Cly and Bry tend to tie up less NOy in $ClONO_2$ and $BrONO_2$, and increase the efficiency of N_2O in ozone destruction processes (e.g. Ravishankara et al., 2009; Daniel et al., 2010). However, Fig. 1 suggests that this effect is outweighed by the effect of N_2O increases.'

P29455L18: "increasing". The question of whether tropical total column ozone will decrease in the future depends on the GHG scenario; in your case, the prescribed SSTs may well be the dominant factor in determining this.

Here, we discuss the long-term trend of tropical ozone in the lower stratosphere. From previous studies (e.g., Xie et al., 2008; Oberländer et al., 2013), both GHGs and SSTs can impact on the tropical upwelling and SST is the dominant one. Previous studies also provided evidence that an enhanced tropical upwelling tends to cause ozone decreases in the tropical lower stratosphere (e.g., SPARC-CCMVal, 2010).

P29456L10: I can't discern from figure 4 that in the Arctic, ClOx is increasing in the beginning of the simulation despite total Cl coming down.

It is indeed misleading here. The sentences are rephrased as:

'However, the relative rates of ClOx decline between experiments are different due to the increased N_2O. In the Arctic stratosphere, ClOx in runs E1 and E2 tends to increase compared with that in the control run E0 in the first 10 to 20 years of the simulations, and then reverses in the following years. In the

Antarctic middle stratosphere, N_2O increases have no significant effects on ClOx, which shows an evident decreasing trend with marginal differences between the four experiments. Accordingly, the ozone time series in the Antarctic middle stratosphere show persistent increasing trends in all four experiments. In the tropics, the increases of N_2O reduce the stratospheric ClOx and lead to stronger declines of ClOx until 2050.'

P29457L18: replace "short of significant" with "insignificant".

Corrected.

P29458L12ff: The discussion of feedbacks involving CH_4 and H_2O appear out of context and unmotivated. You probably need to discuss what's understood about these in the introduction. I suspect that CH_4 changes found in your simulations are useful as tracers / indicators of changes in transport but their chemical and radiative importance is likely very small. Please quantify or state so. If indeed these effects are minor compared to the other ways in which N_2O is affecting ozone and climate, you could choose to drop the entire paragraph, shorten it considerably, or state that the changes are indicative of differences in transport. H_2O changes may well be dominated by trends in the cold-point temperature caused by changes in upwelling and ozone. If that's the case, please state so.

This is a good point. Figure 5d-5i and the related discussions are deleted in the revised text.

P29460 eq (1): Please state that the coefficients c_j are determined through a least square

s minimization of ε; this involves introducing a metric which you should mention. Also the goodness of the fit needs to be considered: If ε is substantial, or has a systematic component to it, this would suggest that your model is not ideal. For example, coupled effects, involving products of the explanatory variables, do not figure in the expansion. You mention this at P29461L16 without quantifying it.

The following text is added in the revised text:

'The coefficients c_j are determined through a least square minimization of ε. The regressions are applied to each model levels from 200 to 1 hPa. The residuals through the regressions are close to zero at all levels. The confidence level for the fit are all over 95% at all levels, and are over 99% at most of the levels from 200 to 1 hPa.'

And, yes, our model is not ideal and not for the purpose of prediction. We mainly focus on understanding the relative importance those factors in ozone depletion processes. Also see replies to Referee #3.

P29463L17: "less halogen"

Corrected.

Figure 1: Use of colour would make the figure more intelligible. Same for the other contour plots.

Yes, using color would make the figures better. For the purpose of saving publishing expenses, we use white-black style since the figure is clear enough.

Figure 3: Consider using smoothing in the right column to make the different lines easier to identify and distinguish. Same for figure 4.

Thanks for the suggestion. Figures 3, 4 are replotted.

Figure 5: Some of these patterns are quite complicated. I wonder how robust they would are. If you performed ensembles of simulations, this could be determined.

Yes, we agree that some of these patterns are quite complicated, which indicate the complicated processes between NOx and HOx chemical interactions as well as the radiative and dynamical responses. We agree that ensembles of simulations and more simulations from other CCMs are necessary in future.

References:

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