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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Stratospheric ozone depletion from future nitrous oxide increases" *by* W. Wang et al.

Anonymous Referee #3

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The goal of this work is to quantify the radiative and chemical effects of N2O increases separately.

Weakest part of the paper has to do with the abstract/introduction discussion of ODP. There are two reasons for this. First – the ODP is not defined in this work, and as I see it the definition most people use is not being followed. I realize that ODP has a bit of a slippery definition, but it is pretty clear from the early work that the ODP itself addresses perturbations in various chlorofluorcarbons relative to F11, without explicitly stating the presumption that errors in the 1D model of the era would cancel out since the area of interest was the upper stratosphere and chlorine would not care where it came from. The big uncertainty for HCFCs had to do with how much would be destroyed in the troposphere. Later work uses the same definition, without regard loss of cancellation

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of errors when processes involved were different. In ALL cases, the ODP has been used for a single base atmosphere – thus you might calculate the ozone change due to a perturbation in N2O in 2000 and then the same thing for a perturbation in N2O in 2050, with a cooler stratosphere. Using the classic definition, the ODP would be nearly the same (as shown by Ravishanka's paper and also by Fleming). Clearly this paper has a strong sense of the other processes that are neglected by this approach, and I think they are important. However, this paper in the introduction uses ODP in a somewhat different matter, implicitly including impacts of cooling on the NOy/N2O as part of the ODP. This part of the paper requires substantial revision. Clarification will also lead to some changes in organization which will help the overall structure.

Presentation could be organized much more cleanly – even in the abstract, the percents are difficult to understand (increases of N2O 50%/100% cause ozone reductions, but then TCO still increases but N2O causes a 2%/6% decreases). This needs careful construction to avoid confusion. Author intent becomes clear when reading the whole paper but the abstract should be understandable on its own.

SPECIFIC COMMENTS

Abstract: As discussed above, the ozone depletion potential for N2O in a future climate does not depend on temperature or tropospheric N2O level using the 'classic' definition of ODP, even though as stated earlier in the abstract the NOy per N2O yield is a function of temperature.

In my opinion this points up the inadequacy of the classic ODP to address this problem, but at the same time the authors need to take care to use terms like ODP in the same way that they are used by others in the community, most importantly Ravishankara et al. (2009), Portman et al. (2012) and Fleming et al. (2011).

Last sentence in the abstract – it is very surprising that the dynamical effects of N2O increases can be large locally anywhere. p. 29453 L 20 "Even though the yield of NOx from N2O decreases in a cooler stratosphere, the efficiency of ozone loss due to NOy

ACPD
13, C10547–C10550, 2013

increases so that N2O increases in run E3 still causes an increase in ozone loss due to the increasing NOy in an atmosphere with decreasing halogens." This point needs explanation.

p. 29453 L 24 – inconsistent use of ODP compared with previous authors. This needs discussion earlier in the paper.

29454 L 20 Specifics of the changes in vertical transport are not completely robust across all 3D models (CCMVal report) – sign change in the tropics is the same, but the rate of change differs and the latitude dependence of downward transport increase in the extratropics differs. This is important for the context of this discussion.

29455 L 15 ff - The discussion of the lack of increase in tropical w* (causing an ozone decrease) is weak. 'perhaps it didn't change in our simulation?' - that is a big difference compared with WACCM results in CCMVal and almost certainly happens because of some other change compared with prior simulations. This is a hot topic and must be addressed. The mention of SSTs later (p 29457) is not really sufficient – why use something different, and why is there little trend in the SSTs? If it is due to using SSTs without trend, then you need to reference the papers that talk about the influence of SST trend on w*.

29455 L 25 – slow down ozone recovery – this is jargon! - and imprecise at that. Some segment of the community thinks of 'recovery' simply as 'ozone increase to prior levels' but another segment thinks of 'recovery' as 'no more loss due to anthropogenic chlorine'. N2O can affect one definition of 'recovery' but not the other.

29456 L 20 PSCs jump out of nowhere. If this is an important part of the feedback, many aspects of PSC formation and parameterization need to be discussed.

29458 L 10 why does cooling cause the water vapor decrease in the stratosphere (away from the troposphere).

29459 L 3 To support confidence in the projected change in PSC area – how do sim-

ACPD
13, C10547–C10550, 2013

ulations for present compare with the PSC climatology developed from CALIPSO? Is there also an increase in HNO3 that would affect PSC formation (and not just cooling associated with N2O? If CO2 is increasing, isn't the effect of N2O cooling in the noise? You do get to the NOy effect as the last sentence of the paragraph – confusing presentation.

29460 L4 ff MLR is a wonderful tool for a lot of things, but it does not separate mechanisms unless they are sufficiently orthogonal – discussed by Oman et al. (2013) concerning QBO and ENSO during the Aura period and also by Stolarski et al. (2010) talk about needing a long enough time record for signals to have sufficient orthogonality to separate. How do you think your proxies behave? You need to at least talk about this, and how the coefficients are less certain when the proxies are less orthogonal.

29461 l17 – 'may not be separated' – you have a foundation to say something about this if you have taken the steps of discussing what you need to have a definitive answer from MLR.

29462 L 7 Curious that increasing N2O increases the BDC whereas in prior discussion the change in upwelling was smaller than expected due to lack of trend in the SSTs. This bears discussion.

Table 1 – not clear that the N2O increases are percents relative to A1b (although that is clear in the discussion/examples). I think a plot would be much more clear than the table.

Grammar

29455 L 14 'simulated in simulations' There are other grammatical issues but because suggested revisions are substantial I do not take the time to identify more of them.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 29447, 2013.

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13, C10547–C10550, 2013

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