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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Explicit calculation of indirect global warming potentials for halons using atmospheric models" *by* D. Youn et al.

Anonymous Referee #2

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General comments:

This manuscript is the first to use a CTM to estimate the indirect GWPs of any of the halocarbons due to the stratospheric ozone they destroy. The authors have made a good choice to look at the two halons with the greatest atmospheric concentration, as these compounds likely have the most important indirect effects. Indirect GWPs due to the ozone destruction associated with halocarbons have been calculated in the past with a simple parameterization based on equivalent effective stratospheric chlorine. The value for the 100-year indirect GWP of Halon 1301 agrees rather well with the 2007 WMO estimate, which uses the simple parameterization, but the value here is somewhat smaller in magnitude. The indirect value for Halon 1211 is also of smaller magnitude than the WMO value. Nevertheless, both values fall within the single

standard deviation quoted in WMO (2007) and so can be interpreted as confirmation of the simple approach.

The authors should be commended for embarking on this study. There are many simplifying assumptions in the previous approach based on EESC and so it is very useful to have a more thorough calculation with which to compare. Nevertheless, there are a few issues of substance that I believe should be addressed.

First, the authors make a point that their calculated value for the 100-year indirect GWP of Halon 1211 is smaller than the values quoted before using the EESC parameterization. While their value is smaller than the previously-quoted central values, it falls within a single standard deviation of the WMO (2007) value and so is most likely not different. The tone of the manuscript would seem to require change to reflect this.

Second, the majority of the uncertainty associated with the WMO (2007) value is likely due to the assumed ozone radiative forcing from halocarbons and the value of alpha. Without knowing how these values from the models used in the manuscript compare to what was assumed in the WMO assessment, it is impossible for the reader to determine why the values might be different. Specifically, with the high degree of uncertainty in the ozone radiative forcing (see, e.g., IPCC (2007)), it would be helpful if the authors could provide more justification for why the reader should believe the values from these particular models. The error bars quoted in the WMO assessment are quite large, so if the authors were able to reduce the uncertainty in some way, that could be an improvement over our current understanding. This would not be easy, however. In the absence of this, the paper is still useful and should be published. But, it would need to be primarily a confirmation of our current understanding rather than presented as providing a new, different value.

Another question raised by the results here involves the implication of the smaller mean value of the halons when compared to the central values from WMO (2007). If the total ozone forcing due to halocarbons were the same in the models used here as assumed in WMO, this might imply that the indirect forcing of the other gases should be more significant. Or, on the other hand, the total forcing of the ozone lost due to halocarbons may simply be somewhat smaller in magnitude in these models. If the authors were able to address this issue, it would be informative.

The models used here underestimate the lifetime of Halon 1211 compared to WMO (2007). How much does this account for the indirect GWP differences? It would seem to go in the right direction, but not fully account for the difference.

Specific comments: Page Line Comment

Abstract 22-23 Yes, they are smaller, but within the error bars of the previous estimate (WMO, 2007) 15514 13 Provide latest IPCC estimate, which suggests that the net forcing from halocarbons is likely positive 15514 16 While there have been many papers and assessments in the past that have made this statement, it is possible (within 1 error bar, IPCC (2007)) for this indirect forcing to be positive and increase the GWP of the halocarbons. 15514 21 It is exactly linear except for the threshold assumption 15514 22 Another limitation of the EESC approach that should be listed is that the indirect GWP has been calculated assuming that there is a threshold for ozone depletion such that ozone loss ends when EESC falls below 1980 levels 15517 19 can you briefly discuss the advantages/drawback to using these CTMs rather than a GCM? 15518 11 while this model provides some sensitivity results, you have fixed the winds from WACCM and don't allow this feedback; how significant is this limitation 15518 18-19 it seems that you are using a fixed mixing ratio for the Montreal Protocol gases. This would suggest that you would not capture any potential non-linear response between chlorine/bromine levels and ozone. Also, because the EESC approach of calculating the indirect GWP assumes the threshold, it would be useful to say something about the implications of your 1999 fixed assumption. Of course, we do know that alpha changes with chlorine and bromine levels. 15520 7 again 'serious limitations due to the parameterization . . .' seems to overstate the issue since your calculations actually agree with the simple approach 15521 3 it seems the wording should be 'is a key indirect effect'

or 'are key indirect effects' 15521 1-16 I am confused by this justification of using an emission pulse rather than a concentration one; why is the mixing faster using a mixing ratio BC? 15524 22-24 The last phrase of this sentence is confusing to me 15526 10 Doesn't this just arise from the longer lifetime of Halon 1301? This would likely be the case for CFC-11 for an ODP calculation and is certainly the case for the CO_2 forcing in a GWP calculation. 15529 8-11 Please provide more explanation for why this is partially guaranteed. It does not seem obvious. 15536-37 why don't the masses end the run close to or above the starting mass? Is this because you are starting out of steady state? If so, a perturbation figure would be more meaningful. Otherwise, more explanation is needed to interpret this figure. Perhaps the only issue is that you have not included a point at year 0, which would make things more clear.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 15511, 2009.