

Interactive
Comment

***Interactive comment on* “Options to accelerate ozone recovery: ozone and climate benefits” by J. S. Daniel et al.**

J. S. Daniel et al.

john.s.daniel@noaa.gov

Received and published: 19 June 2010

Below, we have included specific responses to the comments of both reviewers. We thank them for spending time carefully reading our manuscript and for their comments. The comments were constructive and helped make the manuscript better.

Reviewer 1:

Line 27: We agree. We are certainly not claiming that EESC explains all variations in total ozone. We have rewording this sentence as follows: “The ability of EESC to describe total ozone changes arising from ODS changes is also quantified.”

Lines 34-35: We agree that the suggested wording is preferable.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Line 50: That is indeed what we meant. Since our wording did not make this clear, we have adopted the reviewer's suggestion since that wording does seem clear.

Lines 304-306: Because the 'background case' includes changes in CH₄ and CO₂, but not in ODSs, ozone in this scenario is actually higher than 1950 levels. We have reworded the sentence in an attempt to be clearer: "For example, if ozone column levels are still depleted due to the presence of ODSs, but they are higher than the determined globally-averaged natural level because of increases in CH₄ and CO₂, is ozone depletion still a concern?"

Line 311: This is a good question. Global data do make it seem that depletion begins around 1980. Jackman et al. (1996) shows a comparison of ozone anomalies between model and observations. This supports the thought that ODSs might be contributing to depletion before 1980. Our calculations (Figure 2) support this as well. It is possible that this depletion is hidden by solar cycle effects or some other natural variability. We have altered the sentence referred to and the one after it. They now state: "The year 1980 has frequently been used as a reference year to evaluate progress towards ozone recovery, but it likely does not mark the onset of global ozone depletion (Jackman et al., 1996) (also see Figure 2). If impacts are no longer considered after total EESC returns to 1980 levels, a value judgment is made to neglect potential longer-term O₃ impacts."

Line 219: Yes. Thank you for catching this.

Reviewer 2:

1. We agree that it is important to make clear that we have used mixing ratio boundary conditions (BCs). We do discuss where the mixing ratios come from, but we have now added a sentence clarifying that we use these BCs when we introduce the models. The reviewer is also correct that the use of mixing ratio BCs leads to implied emissions that will most likely differ among models. The Douglass et al. (2008) paper does a nice job describing this; they even find implied negative emissions (into the ground) in some

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

situations due to the lifetime mismatch between the model and the mixing ratio BCs. The use of mixing ratio BCs can sweep some model imperfections under the rug. For example, the Cly will appear to decay properly (i.e., in a manner similar to the source gas decay) even if the model lifetimes are inaccurate. However, there are a few points that make the use of mixing ratio BCs acceptable in our case. First, both models have been compared in detail against observations of transport-sensitive features such as N₂O, with the comparison suggesting accurate transport calculations for the purposes of this manuscript. Also, because a goal of this work is not to infer emissions, the level of mismatch between modeled lifetimes and lifetimes assumed in the calculation of the BCs should not be detrimental to our analysis. Although our BCs imply that we will miss some of the impacts of the slight change in lifetimes of source gases and Cly due to climate change, this is also not critical to our conclusions. We have added a paragraph after the discussion of the models stating this information and pointing out some of the drawbacks of using this approach. We agree that ideally, assuming we had a model that accurately accounted for all atmospheric and surface ODS losses, emissions BCs would be preferable. However, current models are likely not accurate enough in their loss process calculations to make the results from using emission BCs better than results from using mixing ratio BCs. We also note that none of the 3-D models used in CCMVal-2 used emission BCs.

2. We agree that there needs to be additional discussion justifying our different treatment of N₂O compared with CO₂ and CH₄. Since we have submitted this manuscript, it has come to our attention that our use of the term “ODS” is interpreted by some people involved with policy to signify that we are suggesting that N₂O should be regulated by the Montreal Protocol. We are not trying to suggest that, as that is not solely a science question. Therefore, to avoid confusion, we do not identify N₂O as an ODS here. We have added text explaining this stance. This also means that we don’t have to decide whether to call CH₄ an ODS. We have added some discussion in the introduction of CH₄ and CO₂ to explain the different treatment and to highlight the potentially extremely important impacts of these two gases on future ozone levels.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

3. This is a very good idea. Thank you. We have added a figure showing how selected ODS and N₂O concentrations change in the scenarios described.

4. We agree that this was confusing. We also agree that there is benefit to looking at the results when using the best age spectrum from each model. We have now added text describing that the EESC used in generating figures 4 and 5 are the 3-year mean age with 1.5-year width. We also discuss the differences and the importance to the conclusion when the best-fit age spectrum from each model is used.

5. Because our focus here is on globally averaged ozone, we feel that getting into distributions is not needed. However, we like the idea of including ozone data very much. We have superimposed observed global ozone anomalies onto the first panel of figure 2 to show how the models compare with that data.

Minor Comments

Pg 10841: We have added a reference for this statement. We have also added a reference for the CH₃Br QPS and critical use exemption statement and the CCl₄ co-production statements. These are not in the refereed literature, but we agree with the reviewer that it is important to cite the source of the information.

Pg 10842: After discussing the situation with the author of the study that provided the new fractional chlorine release, we jointly decided that at the time the measurements were made for that study, the growth rates were so large that we felt using older model results was the preferable approach. This issue needs to be resolved with further work before a satisfying resolution can be achieved. However, for this work, this is not a very important matter because the two HCFC gases in question contribute a small amount of chlorine compared to the most abundant HCFC, HCFC-22. Of course, it doesn't affect the model results at all, only the EESC calculations.

3rd minor comment: "The discussion in the first part of Section 3 is rather confusing...": We have now clarified this discussion. We no longer refer to 1950.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

We do not want to get into a discussion of what ozone level is unperturbed by human activity, so we now refer to this level as the “natural ozone level”.

Figure 1: We prefer to keep this scaling statement only in the caption. It is very small and only affects the visual comparison of the 2 models in the middle panel. It has no impact on the conclusions. However, we do clarify our meaning of the 3% scaling.

Figure 2: Yes, we agree this warrants discussion in the text, so we have added some.

Figure 3: Thanks for catching this. We now state that the figure shows the GSFC results. We initially were going to include the NOCAR results in the same panel, but there were simply too many curves and no particularly useful additional information. We also clarify that the EESC is calculated with the average age of 3 years.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 10839, 2010.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)