Interactive comment on "How sensitive is the recovery of stratospheric ozone to changes in concentrations of very short lived bromocarbons?" *by* X. Yang et al.

Anonymous Referee #1

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General Comments

The work by Yang et al. presents a simple modeling experiment using a complex 3-D climatic model to give an estimation of the changes in the ozone depletion due to a hypothetical increase of VSL bromine stratospheric injection. The model experiments are designed to represent different time-slices for the present and past/future scenarios of background chlorine and bromine levels, which affect the halogen-driven ozone destruction. Even when an original and interesting topic is introduced, and a simple as well as appropriate method is used to address its relevance, the experiments and results are not presented clearly. Then, before its final publication, I suggest the au-

thors to introduce several improvements on the manuscript describing the experimental setup as well as on presenting results more rigorously.

Mayor Comments

The description of the experiments and the results obtained is not presented in an easy lecture format. The descriptions are not rigorous enough for another reader to visualize unambiguously how the sensitivity simulations were setup, making difficult both the traceability/reproduction and the interpretation of results. There is a lack of formalism on the following points:

I) referencing previous publications for the proposed scenarios: The authors highlight the importance of considering different inorganic bromine and chlorine backgrounds in their simulations. They clearly define the range of stratospheric bromine values published (focusing on VSLS contribution) and cite at list a dozen of papers related to that field. But besides WMO report, there are no other references supporting the inorganic chlorine levels they used for the simulations. See for example the following sentences related to inorganic chlorine. page 9731, line 20 (present day burden) page 9732, line 30 (evolution from past to present) page 9734, line 12 (model setup) Even when the work is mostly based on bromine-mediated ozone losses, I suggest the authors to introduce proper references related to inorganic chlorine burden and evolution.

What would usually be a minor comment is that the authors cite (Eyring et al., 2010) and the correspondent bibliography is missing in the reference section. The omission in this case is not minor as they point out to a specific table on that publication, which is used to compute an ozone recovery rate of 1.4 DU yr-1. Please introduce the missing reference and make clear how you reached that value using the data from Table 4 of Eyring et al., ACP, 2010.

II) Describing the details of the experimental setup for each run. The concept of O3 changes between the pairs of simulations [2xVSL (10 ppt) - 1xVSL (5ppt)] is very

simple, but it is not clear the total Bry background between the 1xVSL and 2xVSL scenarios (10, 15 and 24 ppt). For example for the 10 ppt experiment:

a. Does it mean that 5 ppt are VSL and 5 ppt are Long-Lived (LL)? If yes, for the 2xVSL experiment, it means there are 10 ppt VSL and 5 ppt LL? Do the authors consider this a possible scenario? **b.** Does it mean there is a constant background of 10 ppt from LL, and the 1xVSL considers 15 ppt (10 LL + 5 VSL) and the 2xVSL considers 20 ppt (10 LL and 10 VSL)?

Only after looking at Fig. 2 I realized option a. was the case, but this should be clear for any reader at first instance. I suggest to describe the possible scenarios in a more clear way. The inclusion of a small table including the LL and VSL contribution for each simulation might be of help.

III) Presenting clearly the results when comparing a pair of runs.

Figure 1: Even when the work is focused on stratospheric ozone changes, the authors state that "It is interesting to see that the ozone loss is significant in most of the troposphere" and they present percentage differences. But do not describe whether those differences depend on the Cly and Bry background, or on the doubling of VSLs. Also they recognize that "Near the tropical tropopause, ozone losses of 2–4

Figure 2: What is the porpoise of the linear fitting for each Cly experiment? The authors do not discuss at all why this relationship is linear. The authors also mention a "black" line (i.e. line 21 (page 9736), and somewhere else), but the figure only includes "blue" and "red" lines.

Figure 3: I am not sure if the procedure used for averaging simulations in Figure 3 is useful, or instead, it makes difficult the interpretation and may be misleading. Why have the authors averaged simulations with different background stratospheric Bry and identical Cly? Even when they mention that it reduces noise in the middle and upper stratosphere, most of the O3 column changes between 1xVSL and 2xVSL occur in

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the lower stratosphere, where the O3 loss is different for each of the Bry background scenarios (Fig. 1). I suggest describing the averaging method in a more clear way, as well as introducing in the Figure a caption/label indicating if the lines correspond to a specific pair of simulations (background Bry), or to the average of them. Also, the sentence following "...if we similarly average the left and right columns of Fig. 1..." is confusing or not well explained, confusing the reader on the procedure applied to generate Figure 3.

The authors also state that "The Southern Hemisphere sees the largest reductions, with an annual average around 8 DU". It Is in not clear how they reach that value looking at Figure 3? Which latitudinal range within the SH are the authors pointing at? For which Cly and Bry backgrounds? Also, as the averaging procedure is not well justified, the final conclusion ("crude estimate") reached at the end of the paper on the delay of antarctic ozone recovery is quite uncertain, and should be further justified.

Minor comments:

Even when the 10, 15 and 24 ppt values can be chosen "somewhat arbitrary", the authors should indicate the reasons/references that lead them to restrict the sensitivity study to that specific range.

As main results and conclusions are expected to be included in the abstract, the use of "for example" in this context is not appropriate. Additionally, what do you mean by compound in the following sentence of the abstract (line18): "Although bromine plays an important role in destroying ozone, inorganic chlorine is the dominant halogen compound."

Avoid using the word "perhaps" or "of course". In any case, cite a corresponding reference, or explain why this would be the case (i.e., page 9732, line 17; page 9736 line 12).

The sentence starting at page 9733 line 4 is very long and should be re-written. What

do you mean by "against" in the first sentence of Section 3, Results and Discussions?

Page 9735, line 8. The complete sentence can not be included into a parenthesis. Please consider re-writting: "(We plot the $2\times$VSLS$-1\times$VSLS ozone differences.)"

Typo errors:

There is a "space" missing in the abstract: line 18. a (space) pre-industrial There is no sense on defining acronyms for NH and SH if they are not used later in the abstract. The authors should unify the usage of pptv and ppt between text and figures.

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