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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Temporal Evolution of the Bromine Alpha Factor and Equivalent Effective Stratospheric Chlorine in Future Climate Scenarios" *by* J. Eric Klobas et al.

Anonymous Referee #2

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Authors: J. Eric Klobas, Debra K. Weisenstein, Ross J. Salawitch, and David M. Wilmouth.

Title: Temporal Evolution of the Bromine Alpha Factor and Equivalent Effective Stratospheric Chlorine in Future Climate Scenarios.

General Comment: This is a very interesting paper that attempts to modify the Equivalent Effective Stratospheric Chlorine (EESC) definition to include the effect of possible future climate scenarios. This is done by "refactoring" the bromine alpha factor introducing a "climate normalization to a benchmark climate state." This is changing the simple purpose (see WMO definition below) of the EESC to represent a much broader

definition of climate influence on ODS levels in the stratosphere.

As defined by the WMO 2018 assessment executive summary: "EESC is a metric for representing ODS levels in the stratosphere. It is calculated based upon three factors: surface atmospheric concentrations of individual ODSs and their number of chlorine and bromine atoms, the relative efficiency of chlorine and bromine for ozone depletion, and the time required for the substances to reach different stratospheric regions and break down to release their chlorine and bromine atoms. As EESC continues to decrease in response to Montreal Protocol provisions, stratospheric ozone is expected to increase. In this Assessment, EESC does not include chlorine and bromine from very short-lived substances (VSLSs)."

My feeling is that one should not change the EESC definition. However, that being said, I do think there is merit in attempting to include a diagnostic that does address climate impacts on EESC, that is simple, and does not require running a large ensemble of CCMs. Therefore, I wouldn't change the EESC definition above, but would create a new definition. This work is a first step towards this goal. I would recommend that this work be published assuming my comments are addressed below.

Specific Comments:

Line 35: I don't believe (just a suggestion) you need to discuss homogeneous reactions (i.e., like R4) in discussion of lower stratospheric ozone loss. This is a topic that has been explained in hundreds of publications. Just reference the Solomon et al., 1999 review article. You also don't need to summarize the heterogeneous reactions either (i.e., R5-R7).

Lines 135-144: RE: Discussion of Volcanic emission of Cl and Br. I find this discussion topic distracts from the point of this paper. Why go into possible random inputs of these species into a future atmosphere. You might as well discuss the possibility of an ocean surface asteroid impact injecting Cl and Br into the stratosphere. This topic seems like a separate study/discussion to me. I would just focus on the modified "EESC" technique you are proposing.

Lines 152-163: The model description section is very confusing (at first read). One has to have a basic understanding of Daniel et al., 1999 to make sense on where you are going with the scenarios. Evidently you are running time slice experiments (every 10-years, with a duration of 20 years) using constant mole fraction lower boundary conditions for the 20-year period? E.g., Table 1: for “d” superscript you state “informed by Meinshausen et al. (2011) and Watanabe et al. (2011)”. This means you are getting the initial conditions for say year 2020 from Watenabe et al. and the lower boundary mole fraction from Meinshausen et al.? For “c” you are not using the same model, but a 2D model from Fleming et al., (1999)? Why not use the same model for hindcast and future conditions (i.e., MIROC-CHEM-ESM)?

Line 164: You state that you are using the Daniel et al. (1999) approach. Essentially you are using the approach for equation (2) in Daniel et al., correct? [Your equation (2)] This is also why you have three scenarios to derive alpha-Br from a given atmospheric state, correct? I would restate (in your words) the procedure on page 23,874 Daniel et al. (1999). This will greatly help the first-time reader of this work.

Line 218: You probably should define the basic technique of graph-theory.

Line 229: Specified dynamics details are needed here. What are you specifying for the dynamical fields and where did they come from?

Lines 325-330. This is a very interesting result [i.e., better comparison of EESC to 1980 values compared to Dhomse et al. (2018)]. The Dhomse et al. study was an average of many models. Have you looked at one model, say the MIROC-CHEM-ESM, of which was used for the initial condition, for this work?

Lines 329-330. You state that this analysis does not include the “impact of an accelerated BDC, which would hasten the projected recovery”. Since you are using a CCM for your initial state, is part of this process “baked into” the calculation? Certainly, the

temperature affect is; but isn’t it possible that the dynamical state is also influencing the equation 10 result?

NOTE: I would find it very interesting to add an additional figure (like Figure 1) showing the column alpha-Br (latitude vs time) for year 2100. Here I would show four panels, depicting the result for RCP2.6, 4.5, 6.0, and 8.5.

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