Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-276-AC2, 2020

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.

J. Eric Klobas et al.

klobas@huarp.harvard.edu

Received and published: 5 June 2020

We thank the anonymous referee for his or her thoughtful criticism which has resulted in changes that have improved the quality of our manuscript. We provide point-by-point responses to the referee's comments (**bold text**) in plain text below.

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.

Per the recommendation of both referees regarding changing the variable name, we have opted to call this proxy Equivalent Effective Stratospheric Benchmark-normalized Chlorine (EESBnC) and will refer to it as such in our responses here.

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).

We have revised the introduction to eliminate them.

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.

We have removed this paragraph and the associated statement in the conclusion.

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.?

We have revised the model and experiment description sections to better communicate the procedure we followed. The referee's interpretation is correct. We employ 20-year time slice experiments for each decade spanning 1980 - 2100. Temperature fields are obtained from RCP scenario realizations of the MIROC-CHEM-ESM from the CMIP5 archive (Watanabe et al., 2011), while chemical boundary conditions for our model evaluation are obtained from RCP scenario specifications (Meinshausen et al., 2011).

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)?

For 'c', historical past simulations were evaluated using climatological and temperature fields prepared previously. We employ these climatological conditions because (1) these climatological fields were used in previous studies and facilitated validation of model performance, and (2) the climatological fields prepared from MIROC-CHEM-ESM for the present do not significantly differ with the climatological fields we employ.

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.

The reviewer is correct on all counts. We have reworded the procedure such that it is more clearly communicated to a reader who does not have prior knowledge of Daniel et al. (1999).

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

We note that the technique is described immediately following this line.

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

We now provide the information the referee requests in Section 2, model description.

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?

In response to the referee's question, we performed a quick investigation and found that MIROC-CHEM-ESM RCP / HISTORICAL experiments from CMIP5 show a similar qualitative trend to our results (extrapolar RCP8.5 recovering to 1980 ozone layer thickness sooner and extrapolar RCP 2.6 recovering to 1980 ozone layer thickness later). The dates of recovery from these experiments are not exactly the same as the dates we derive from our EESBnC treatment in Table 4, which is not unexpected given that our prognostication is based upon halocarbon inventories and our eta parameters are derived using a model with different chemical/aerosol/transport schema.

We note that Eyring et al., 2013 explored ozone layer recovery to 1980 thickness and the authors included MIROC-CHEM-ESM in their model ensemble.

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?

It's true that certain parameters such as scale height will be dependent on the imposed temperature structure, such that the dynamics deviate slightly between model realizations. If we review vertical profiles of ozone for the control runs in our temperature sensitivity studies, we find very little variation between midlatitude ozone profiles in the

lower stratosphere where ozone is subject to dynamical control, especially with regard to circulation-induced ozone super-recovery. However, regions where photochemistry dominates the ozone steady-state do exhibit variation in ozone as a function of temperature boundary conditions.

From these comparisons, we infer that any variation in ozone due to dynamics baked-in to our boundary conditions are insignificant to photochemical changes in ozone as a result of the temperature perturbation.

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.

We agree that such a plot is quite interesting, but we are reserving this type of analysis for a future manuscript involving a method which is more sensitive to PSC response to climate changes in the polar regions.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-276, 2020.