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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.

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This paper presents an interesting study on the change of the relative efficiency of Bromine with respect to Chlorine Atoms in destroying ozone under varying atmospheric conditions and of the changing impact of halogens on ozone in future climates. The title has its emphasis on Bromine, but actually the changing efficiency of Chlorine is also studies, so the authors might consider adjusting the title to take this into account. The subject is well in the scope of ACP and worth studying. However, I believe that the authors are extending the EESC concept to something it is not: a proxy for the effect of halogens on ozone even under changing climate and chemical conditions and for

projection of ozone recovery. It is discussed in the last WMO assessment that EESC is a proxy for the inorganic halogen content and its recovery should not be considered to be a recovery of the ozone layer. Therefore, while I find the new concept useful, I suggest to not mix it with the concept of EESC. In addition, an extremely important further driver (namely the change in Brewer-Dobson circulation) is not included in the concept. Therefore, this new concept extends EESC into the direction of an ozone proxy, yet it only takes into account a part of the expected climate-related changes, namely those arising from different temperatures and those from different chemical regimes. Dynamical changes (which are highly uncertain but may be very large drivers) are not incorporated, which makes this new concept difficult to position: it is neither a halogen proxy under otherwise unchanged conditions (which EESC is), nor is it a real proxy for ozone recovery (as it lacks the dynamical changes). I also have some more specific comments, which are listed below. I can recommend the paper for publication after revisions as to my specific comments and also after a more thorough discussion what this proxy really represents and after considering whether this new proxy should still be called EESC (I believe it should not be called EESC).

Specific comments

Major comments

(i) As explained above, I believe that the authors should give some more consideration to what the new proxy really is and if it should still be named EESC, in particular as not only the α value for bromine becomes a variably, but also the effectiveness of chlorine to destroy ozone (η) becomes a variable. This was not considered in EESC so far as EESC so far was not really an ozone recovery proxy (see e.g. the discussion in box 1-4 of WMO 2018), even though it has often been used as such. I would consider naming this differently, maybe something like "Equivalent Ozone-effective stratospheric chlorine (EOESC)" or something along this line. Further, the nice thing about EESC as it is used now is that it can be easily calculated and applied to a new scenario, without the need for a model. This advantage is lost with the new concept, unless a method to

parametrise the η factors needed in eq. (9) is given.

(ii) The study is performed with a 2D model. How is the climate state taken into account, i.e. the change in temperature and the changes in dynamics due to the expected accelerated BDC? While the changes in temperature and chemical environment can be simulated in a 2D model, the change in the Brewer-Dobson circulation which is projected by the 3D climate models is most probably not included. Under such changing climatic conditions, the new fractional release factor formulation by Ostermöller et al., (2017) is independent of the trend of the species but it does depend on the state of the atmosphere, in particular it is expected to change with time for a given mean age value due to dynamical changes (accelerating BDC).

(iii) Unfortunately, the explanation of the model experiments is rather unclear and difficult to follow. The paper lacks a clear explanation of which model runs have been performed, and exactly how they have been forced. In particular, the dynamical forcing is not described and it is unclear if changes in the Brewer-Dobson circulation are included in the simulations from the description in section 2. Only on p.15, l.329 it is clearly stated that changes in the BDC are not included. A clearer description is required here to ensure that the results can be understood and reproduced.

(iv) In section 3.1. it would be important to describe more clearly the physical meaning of the new EESC formulation (9). The definition of the η values for chlorine and bromine is always relative to the Ozone sensitivity with respect to Cl in the reference state. Therefore, EESC defined in (9) is also referenced to dO3/dCl in that reference state. It would be good to explain this concept more clearly and give a more intuitive explanation of this quantity. In my understanding the new formulation in eq. 9 describes the 1980-equivalent stratospheric chlorine impact on ozone, adjusted for changing stratospheric temperature and changing chemical environment, but not for changing dynamics.

Minor comments:

General: the term background is used in many places (e.g. l. 148: inorganic halo-

gen background). A background is a state against which something is referenced. I suppose level or content might be more appropriate.

l. 15.: what is meant by inorganic halogen precursor compounds? I suppose this is the source gases? Then I would term this the precursor compounds for inorganic halogen.

l. 45: the use of “unlike” is unclear to me: in the absence of chlorine, also Br would require the oxygen atom and there are also other Cl-recycling reactions.

l. 49: please specify what you mean by lower stratosphere here.

l. 54 (and other places): please be more specific with respect to the WMO 2018 citations: Usually the respective chapter should be cited in order to allow the reader to find the information.

l. 86: what do you mean by chemoclimatic?

l. 107: see for example discussion in box 1-4 of WMO 2018: EESC should really not be used as an ozone recovery proxy. It is a halogen recovery proxy. See also major comment above.

l. 114-125: the projected super recovery of stratospheric ozone is mainly due to changes in dynamics, not changes in chemistry. This section reads like the chemical influences are dominating.

Section 2: In this section a clearer discussion of the model set-up is required, in particular how the dynamics (and possibly changing dynamics) have been incorporated.

l. 171ff: The concept of the perturbation experiments should be clearer explained.

l. 202-204: A clearer description should be given specifying that both sensitivities are given relative to the sensitivity of ozone to chlorine in the benchmark chemical-climate state.

Section 3.2.: have perturbations in T and in chemistry been performed independently?

i.e. can it be distinguished between an effect due to increased CH4 and increased HOx with respect to an effect due to increased T?

l. 235: I suggest using the term temporal evolution or temporal development instead of trajectory, as trajectory has a different meaning in atmospheric sciences.

l. 243: please give the percentage increases relative to what? Also monotonic and percentual do not go very well together. I suppose what is meant is a linear trend resulting in an increase of xx eq. (11): which time series is used here? If I understand correctly, the model is run for 20 years into equilibrium. In this case, the temporal trend of the trace gas in the integral would be equal to the (constant) mixing ratio and the whole integral would become the (constant) mixing ratio.

l. 312: The values in Table 1 in Engel et al. (2018) are trend-independent. Fractional release factors are expected to change for different climate states.

l. 314 and Figure 5: the grey used here looked very “blue-gray” on my printout. I suggest to use a clearer grey colour for better distinction.

l. 317.: why does the EESC formulation according to Engel et al. show differences for different RCP scenarios at all? Should EESC not be independent of RCP in this formulation?

l. 329.: This information should come much earlier and be discussed in section 2.

l. 344: Can the dominance by geological perturbations (I supposes volcanoes) be substantiated by a reference?

l.345: processing rates of what? I suppose ozone?

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