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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Delayed Recovery of mid-latitude lower stratospheric Halogen Loading" *by* Andreas Engel et al.

Anonymous Referee #3

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The paper studies the combined effect of chemical loss and transport for the formulation of a simplified index describing the ozone depleting capacity due to halogens in the atmosphere. Usually, for that purpose the quantity EESC is used (for example in the WMO ozone assessments). EESC experienced several changes in its definition already in the past, when the correction for transport times was refined including its impact on release factors and the impact of transport time distribution when correcting for a non-linear trend has been included. Following Newman et al. (2007), the authors here present a further refinement of this concept.

Generally, I find this paper a very nice exercise to better understand how chemical loss and transport work together, here applied to describe the halogen loading in the atmosphere. The value of the paper is to remove inconsistencies in the formulation of

the traditional EESC concept, but still use the same simplified approach. The paper is clearly written, references are given as necessary, the assumptions are discussed in detail and possible consequences for the ozone recovery description are discussed.

My main obejection is the title of the paper which is in my opinion totally misleading. The paper is about a refinement of the EESC concept. In this concept, parameters can be derived like the time of recovery. This parameters has a descriptional character, and can be used to compare different scenarios of Halogen loading, for example. Modifiying the formulation of the concept will change the value of a parameter, but not the scenario, and therefore also not its "real" Halogen loading. In addition, as the authors state by themselves, the concept does not include deviations of the implicit assumed stationarity of the dynamics which is not even true for the past.

My second concern is somewhat related: as the EESC concept describes only one of the main driving processes for additional ozone depletion I would ask the authors therefore to put their results in the context of model studies where the effect of the accelerating Brewer Dobson circulation has been analysed.

The derivation of the concept is somewhat lengthy in my opinion and can be combined, for example for equs. (11)-(14). Stationarity means that at the end that the combined history of an air parcel (including its mean photochemical dose) is only a function of the position in the atmosphere. So equation (19) is not a surprise. Much more subtle is the transition from equ. (19) to (20). This is valid only under the specific condition that the distribution G# is determined by its first moment only which may be not generally true. Here I would ask the authors to discuss the assumptions in more detail.

Finally, to be more than just an interesting exercise, the paper would strongly improve if the authors could show that using their new formulation would yield a more concise ozone trend analyses, at least in one example.

Minor points

p2l9: transport within

p4l17: you mean averaged over the seasons == annual mean

p4l18: eliminate "it is expected"

p4l20: the typical path from the tropical tropopause through the stratosphere back to the troposphere at higher latitudes will not yield this shorter lifetime at the end of the path. You mean strictly in the stratosphere.

p4l21: f will be a function of r, too. This does not harm the derivation.

p5l31: what are the three? I see the trend and and the chemical loss only.

p6l2: the exponential chemical loss term is only used here and can be left out.

p11l9 remove "classical"

Typos:

parameterization/parameterization should be typed in one version only

p3l9: the first moment has a lower value

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