Review of: "Revisiting the challenges of ozone depletion from a prospective LCA perspective"

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Potential competing interests: No potential competing interests to declare.

First, I will admit my expertise is not in the domain of life cycle assessment so I will restrict my comments to the ozone science aspects of the manuscript.

In Section 3.2 there is a discussion of the use of time horizons, reference and return years. As I understand it, there is not a reference year for the calculation of ODPs in the way there is a reference year for ozone recovery. The most widely used ODP is the steady-state ODP, defined as the time integrated change in global ozone due to a pulse emission of a unit mass of a particular ODS, relative to the time integrated change in ozone from a unit mass pulse emission of CFC-11. The time integral should be over the complete length of time both the ODS and the reference CFC-11 remains in the atmosphere – typically on the order of 100 or more years. ODPs can also be calculated from the steady-state change in global ozone due to a sustained emission of a unit mass of a particular ODS relative to the sustained emission of a unit mass of CFC-11. The two calculation methods can be shown to be mathematically equivalent.

There is also a time-dependent ODP, that integrates the effects on ozone over a limited time period in a similar way to Global Warming Potential can be defined over a particular time horizon. For a short-lived ODS, the time-dependent ODP would be large for a short time horizon of, say, 20 years but would become smaller at longer time horizons as the longer-lived CFC-11 continues to effect ozone while the ODS in question no longer has an effect. To the best of my knowledge, the time-dependent ODP is used less than the steady-state ODP.

From the discussion in Section 3.2 it is not clear which type of ODP is being used in the calculation and whether the discussion of integrated effects between 1980 and 2044 for the ReCiPe model is related to the use of a time-dependent ODP. This should be clarified.

Minor Comments:

I have always found the evolutionary nature of the Montreal Protocol to be an important aspect, one that offers important lessons for current efforts to reduce GHG emissions. So I worry the statement ‘After the adoption of the Montreal Protocol [6] in 1987,…’ gives the impression that the process of developing a framework for controlling ODSs was completed with the initial adoption of the Protocol. As shown by the attached graphic, there were numerous amendments to the Protocol that were quite important for changing the evolution of ODSs and I think the statement should give some impression of the evolutionary nature of the controls.

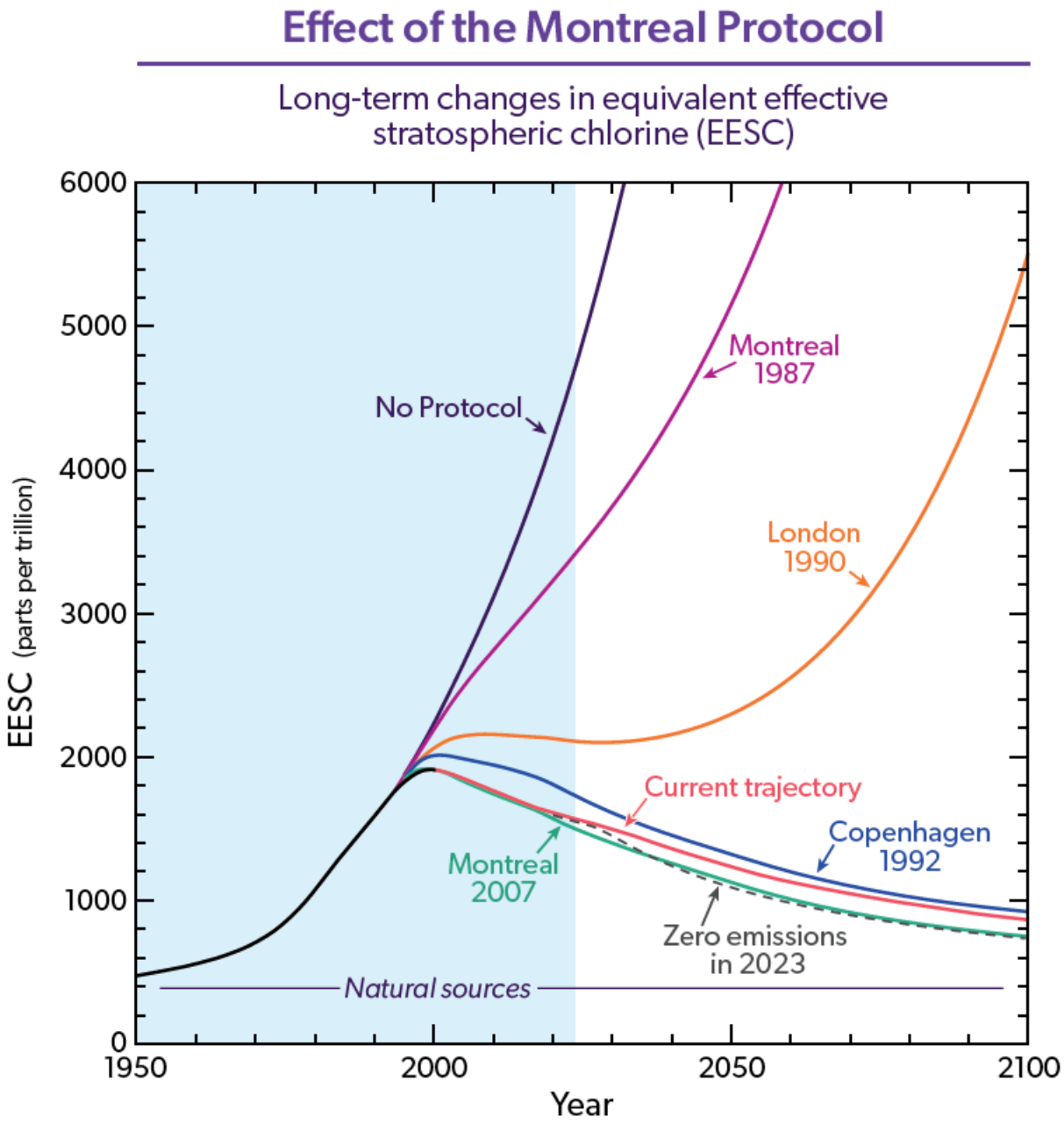

Figure Q14-1 from Ross J. Salawitch (Lead Author), Laura A. McBride, Chelsea R. Thompson, Eric L. Fleming, Richard L. McKenzie, Karen H.Rosenlof, Sarah J. Doherty, David W. Fahey, Twenty Questions and Answers About the Ozone Layer: 2022 Update, Scientific Assessment of Ozone Depletion: 2022, 75 pp., World Meteorological Organization, Geneva, Switzerland, 2023.

In Section 3.4 there is the following statement:

> *Climate change also accelerates the Brewer-Dobson circulation and stratosphere-to-troposphere transport of ozone. This is the most likely explanation as to why no significant ozone layer recovery has been observed at mid-latitudes, despite the decrease in ODS emissions.'*

The acceleration of the Brewer-Dobson circulation is expected to cause ozone in the tropics to decrease, while ozone in

the mid-latitudes increases – the result of increased transport from the tropics to the mid-latitudes of both hemispheres. While there is increasing evidence that the acceleration of the Brewer-Dobson circulation may be responsible for observed decreases in the lower stratosphere in the tropics, the lack of an associated increase in the mid-latitudes is a mystery. I believe the statement as written needs to be revised.