

***Interactive comment on* “The contribution of anthropogenic bromine emissions to past stratospheric ozone trends: a modelling study” by B.-M. Sinnhuber et al.**

Anonymous Referee #4

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General Comments

This paper addresses the issue of the impact of bromine emissions from both anthropogenic sources and (mostly) natural short-lived compounds on stratospheric ozone. The study uses a classical two-dimensional model, which is acceptable as long as the impact of model deficiencies is clearly discussed.

The model results are compared with satellite measurements, which is good. However, the analysis of the total ozone data for the purpose of this paper can clearly not reach as deep as studies [5, 7, 2] focusing exclusively on the determination of ozone trends.

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Such studies need to be discussed and cited, so that the ‘trends’ shown here can be put into perspective. Most importantly, from Fig. 4 one might conclude that there is a downward trend of total ozone in the tropics which is not the case [7]. Further, I am happy to see that not only column ozone is discussed in the paper but also the vertical profile of ozone. But why is no comparison shown with the observations [6, 7] in this case?

Polar ozone loss is difficult to model with a two-dimensional model and it is not discussed in the paper as it stands. But the export of ozone depleted or activated polar vortex air can make a significant contribution to observed ozone loss at mid-latitude in spring-summer – and in both hemispheres [4, 3, 1]. Therefore, in the model, the mid-latitude trends are not independent from what is happening in the high latitude region. This point needs to be discussed in the paper and some information needs to be given on the simulated ozone loss in the polar regions.

Finally, with all its deficiencies, the employed model has the advantage that it should be easy and cheap to perform a large number of sensitivity and test runs. I believe that this strength of the model has not been used enough in the paper as it stands. One more that one occasion it is stated that “this behaviour is not understood” or “the reason for this is probably”. Why is the model (through more sensitivity runs) not used to better understand its behaviour?

However, the paper has important things to say, in particular the change in the alpha factor and the impact of bromine from very short-lived compounds on simulated ozone trends. I recommend publication after a revision taking into account the comments made in the reviews.

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Detailed points

- I do not understand why the model is run at such a coarse horizontal resolution. Improving the horizontal resolution might help to obtain better results close to the poles.
- Only relative changes in ozone are shown here, but would one not like to know how well the model reproduces the observed values for ozone vertical profiles and for ozone columns?
- I recommend to add information about the observed trend in the vertical profile.
- Tropospheric washout of inorganic bromine was switched off: how realistic is this assumption?
- The finding that there is an 11-year solar cycle in total ozone in the tropics is not new, is it? Add appropriate citations.
- The model underestimates the ozone response to Pinatubo in the NH but not in the SH. This is a good example for a case where more analysis is needed. Is there a chemical reason for this? If yes, are the ClO and BrO fields different in the NH and SH? If it is a transport effect, could this be seen from the behaviour of tracers? Perhaps a comparison with observed HF columns could help?
- It is stated that “the modelled ozone trends agree reasonably well with the observed trend...”, while I would argue that the difference between observations and simulations are mostly greater than those between the different model runs. I suggest to formulate a bit more carefully, after all one would not expect a two-dimensional model to give a too good representation of the reality.

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Minor issues

p. 6499, l. 14: I do not understand why this is the 'converse' question here.

p. 6500, Sec. 2: A bit more information about the model perhaps? What is the vertical coordinate, pressure or potential temperature? Is there a reference for the employed eddy diffusion coefficients?

p. 6502, l. 6: Why is this the most likely value? Is this an assessment of this paper? Then more discussion is necessary.

p. 6508, l. 17: define 'effective alpha column'

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