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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Transfer of organic Br and Cl from the Biosphere to the Atmosphere during the Cretaceous/Tertiary Impact: Implications for the stratospheric Ozone Layer" *by* K. Kourtidis

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General Comments

Some similar model runs could be omitted from a revised version, namely runs #3, #5, and perhaps run #4.

Specific comments

Since it appears that with the current text there might be some confusion to the reader, some rephrasing in the revised version according to the referee suggestion could make

clear the distinction between tropospheric organic halide loading and stratospheric active halide loading.

p. 6770, 11: Line 11 of the abstract: "... mixing ratios of active chlorine and bromine..." will be replaced with "...mixing ratios of organic chlorine and bromine..." in the revised version. Also, line 14 of the abstract shall be rephrased to "they could still remain in the <stratosphere> {atmosphere} for many years, {and a substantial part could be transported to the stratosphere, thus} substantially affecting the ozone layer". At other parts of the text, similar changes shall be made in the revised version, to make the distinction between active halides and organic halides (<>:deleted text, {}:added text).

p. 6773, 12: I have here deliberately used the word "flux" for the resulting tropospheric mixing ratio from a given mass flux; since this is confusing, it will be corrected in the revised version.

Ocean model equations: Eq. 1, 3, and 4 can be omitted without loss of clarity.

p. 6780: Lacking a background in biology, I cannot easily address this point. Intuitively I would tend to agree with the reviewer that while ozone loss appears to be A major threat, it might not be THE major threat. Perhaps the major threat would still be the fires and the resulting pyrotoxins and highly acidic wet and dry deposition. The temporal sequence of the events would be firesàpyrotoxins and acidity à enhanced UV radiation. Hence, one could argue that the high post-impact UV levels implied by the present work would affect

1) the species that survived the other two threats, and, perhaps most importantly,

2) the post impact recovery of the biosphere and the resulting species composition during the long-term recovery.

Coming from an atmospheric physics/chemistry background, I was unaware of the very interesting work by Cockell on the UV role in evolution (probably because it was published in biology journals) that the referee mentions; the subject of the present paper

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appeared to me sometimes a bit “esoteric” for the atmospheric sciences and hence I was glad to read the papers by Cockell recently. It appears that there is considerable room for interdisciplinary work on the matter. From an atmospheric sciences viewpoint, ozone destruction from release of organohalogens during lesser impacts is easier to model and understand than ozone loss during such massive impacts as the K/T one. Lesser impacts would not perturb thermally the atmosphere to such an extend; they would also not cause massive plankton die-off; would not alter in such dramatic ways the oxidizing capacity of the atmosphere and would not cause changes in the global structure of the oceans. This might make such impacts more straightforward to model, since the range of the assumptions and uncertainties is much smaller and hence the involved parameters in the air-sea transfer calculations can be better constrained.

The above discussion will be included in the revised version, although detailed modeling about such cases involves an amount of work that might make detailed modeling of such cases the subject of future work.

Technical corrections, typos and language suggestions: I would like to thank the referee for these extensive suggestions; they will all be taken into account in the revision of the manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 6769, 2004.