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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurement-based modeling of bromine chemistry in the boundary layer: 1. Bromine chemistry at the Dead Sea" *by* E. Tas et al.

Anonymous Referee #1

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I am still not convinced that the model approach the authors use is adequate for their purposes and still think that the paper needs a thorough revision both in contents and style. The following is regarded as response to the author's comments and does not replace my first review.

Methodical problems:

1. I agree that the two investigated reactions are necessary to reconstruct the BrO time-series at the Dead Sea. However, I doubt that the model is suitable to proof that these two processes are sufficient because no other heterogeneous processes are included and the model is too strongly constrained (e.g. by prescribed fluxes).

2. I still do not understand how equation 4 (which provides ambient seasalt concentration) is exactly used to calculate the reaction rate of H2.

3. The authors prescribe NOx and hydrocarbon fluxes in order to match modeled and measured time series. If the concept of contrained modeling is used, what is the advantage of determining fluxes instead of constraining concentrations directly? Are the measurements vertically resolved? If not, how is the vertical profile as input for the column model determined?

4. If the ozone concentration cannot be captured quantitatively by their model, at least sensitivity studies should be performed to show that the absolute magnitude in ozone concentrations is unimportant above the threshold of 1-2 ppb. This is an essential information. Reagarding the inert species ‘X’: I still do not see the advantage of this concept compared to simpler budget calculation. What physical properties does species X have (which are necessary to calculate transport/deposition)?

5. Details regarding the vertical model should definitely be provided in the new manuscript version. Also, the fact that results are only shown for ground level must at least be mentioned.

Further major points:

1. I do not doubt that the described chemical cycles are important for the chemistry going on in the Dead Sea area, and that effects of this chemistry may differ from findings in other areas. However, the described chemistry itself is not new and should not be part of the results chapter.

2. Information on how the threshold value of 1-2 ppb was determined should be included into the new manuscript version.

3. O.k., then thorough re-wording of the respective parts is necessary.

4. The morning peak in BrO should be discussed and the explanation for the strong structure in Fig. 3 c,d, should be added to the text. The unrealistically high Br2 flux in

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some cases has to be discussed.

6. Figure 8: “BrOx production due to reactions H1 and H2 is max. 0.25 ppt/min, i.e., 15 ppt/h. This production rate is not high enough to explain BrO levels of 120 ppt that are formed within 2 h” (taken from my first review). What is wrong with this interpretation of the Figure?

Minor points:

3./4. In the new text version, it should be clearly distinguished between model results and guesses that are inferred from the results.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 4929, 2006.

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