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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling chemistry over the Dead Sea: bromine and ozone chemistry" *by* L. Smoydzin and R. von Glasow

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Coexistence of different approaches

Congratulations to the authors for their study about Reactive Bromine Species (RBS) activity in the unique area of the Dead Sea. Herein, we would like only to comment about few claims of the authors relating the study of Tas et al. (2006):

The authors claim incorrectly that Tas et al. (2006) stated that the only source for the high bromine levels is the Bromine Explosion reaction taking place within sea salt aerosols. This is defiantly wrong, and must be corrected in the text. It was explicitly stated by Tas et al. 2006 that: In the present simulation study, it was assumed that Br2 was released solely from the sea salt aerosols. This is not necessarily true, since it

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is also possible that part of the Br2 is released directly from the water or salt surfaces (Sect. 3.1.2), or from sulfate aerosols (Von Glasow et al., 2002; Fan and Jacob, 1992).

Therefore we kindly ask the authors to exclude or change their statement that (P. 4527, lines 18-21: A recent model study by Tas et al. (2006) claims that the only source for the high bromine levels is the bromine explosion reaction taking place within sea salt aerosols which are emitted from the Dead Sea water and the related release of bromine species out of these aerosol particles.

The authors claim that Tas et al. (2006) chose the rate of this reaction in a way that it will fit better to the measurements. It was demonstrated by Tas et al. (2006) that the model results were not sensitive to changes in time of the rate of Br2 production by this reaction. Furthermore, it was shown that the exclusion of this reaction did not influence the diurnal profiles of BrO, in terms of correlation with the measured BrO. Only the levels of BrO were changed but not the diurnal profile shape. This reaction was included in our model simulation only in order to supply the realistic amount of Br2, which, in fact, could be contributed also by additional sources, as explicitly stated by Tas et al. (2006), and also in order to investigate what is the release rate of bromine into the gas phase that can account for the RBS activity at the Dead Sea.

Therefore we kindly ask the authors to exclude or change their statement that (P.4550, lines 14-17) : In general, it has to be questioned that, choosing this reaction rate as the only degree of freedom in a numerical model, as done by Tas et al. (2006), and varying this rate until model simulations agree best with measurements is an appropriate approach to explain observations.

The authors claim that Tas et al. (2006) chose an unrealistic value for the Bromine Explosion mechanism. This is not correct, because this value was not chosen, but was found to be most appropriate according the measurements and model simulations, in order to account for the realistic release rate of bromine to the gas phase. As mentioned above, the release mechanism, anyhow, can be different.

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Therefore we kindly ask the authors to exclude or change their statement that (P. 4550 , lines 11-12: Tas et al. (2006) chose an unrealistically high reaction rate of 1.35×10^{-6} nmol/s.

It should be also remarked that the average value used in the model is in a good agreement with other published values (e.g., Michalowski et al., 2000).

The authors claim that (P. 4551, lines 4-7): Tas et al. (2006) chose the strength of fluxes into the model domain such, that their simulations agree best with measurements. This is defiantly not precise. All fluxes in the model, except for O3, were set based on measurements concluded at the measurement site, on days for which there were no signs for RBS activity and definitely not in order to achieve agreement between measurements and simulations (Tas et al., 2006). The input of O3 fluxes was based on O3 measurements at the measurement site. The contribution of Tas et al. (2006) was in pointing out for the first time that these O3 fluxes are crucial in order to achieve agreement with measured BrO profile. However, the input timing of O3 fluxes was determined only based on the measured O3 at the measurement site, rather than on the measured or simulated BrO.

Therefore we kindly ask the authors to exclude or change their statement that: Tas et al. (2006) chose the strength of fluxes into the model domain such, that their simulations agree best with measurements.

(P. 4551, lines 6-12) - It should be further noted that the advection of air mass was controlled by wind speed, however the O3 flux was controlled by the spatial gradient of reactive bromine and O3 itself, at the measurement site, while the wind direction was quite stable for the specific date of simulation. It is very wrong to assume that O3 will be transported over the evaporation ponds without being depleted by the reactive bromine over the ponds, and will be dominated by advection.

(P. 4527, lines 21-24) - The authors claim that the model used by Tas et al. (2006) is constrained for a large extent. This is not correct. The model simulations were highly

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supported by intensive measurement campaign performed at the evaporation ponds. This makes the simulations more realistic and reliable, however the simulations were defiantly not constrained for a large extent.

Therefore we kindly ask the authors to exclude or change their statement that (P. 4527, lines 22-24): However, as Tas et al. (2006) have chosen a set up constraining their model to a large extent and as several of their assumptions are strongly simplified the conclusions drawn from this study have to be thoroughly reconsidered.

Finally, we believe that including more detailed multi-phase mechanism in model simulations as was applied by the authors is crucial for a valuable research of RBS activity in the unique area of the Dead Sea. As was stated in Tas et al. (2006), our study aimed at providing only a basic and preliminary description of the RBS activity at the Dead Sea, however, we dont agree with the overcriticizing line acted by the authors in their publication. Indeed, by applying a rather simplified approach and by achieving good agreement with measurements, by using based-measurements (but not prescribed) simulations, we could point out several factors and processes that are crucial for a better understanding of the RBS activity at the Dead Sea. We explicitly stated in the paper that: 8220;Future studies of RBS chemistry should certainly investigate additional heterogeneous processes in order to describe the RBS activity at the Dead Sea area in greater detail. Therefore, we dont agree with the general overcriticizing of the authors about the simplified approach applied by us in few cases that were not mentioned in this short comment. We expect the authors to consider this point in any newer version of the manuscript.

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