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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

**Anonymous Referee #2**

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I would like to express my delight to the authors who make another effort to explain the interesting and special atmospheric processes at the Dead Sea. The paper stands out due to the fact that meteorological parameter are tried to be implemented.

From an overall point of view, after an appropriate introduction the two following sections seem to be somewhat imprecise (see below). During the model part of the paper, the outline is clearer. In the discussion section, the tone of the authors facing the work of E. Tas has been mentioned already in other comments and should be revised.

Generally spoken, it is surprising that iodine species are missing in the model. Iodide concentration is increased in the Dead Sea brine and Iodine Oxide has been measured repeatedly and up to significant mixing rations of 10 ppt [Zingler and Platt, 2005]. Iodine

chemistry should be implemented in order to draw a complete image of halogen chemistry, or it should be discussed why iodine chemistry does not influence bromine and ozone chemistry, which would be in contradiction to the sensitivity runs in [von Glasow et al, 2002], where the inclusion of iodine chemistry resulted in quicker Br activation and greater ozone loss.

Further on I wonder if orography is taken into account in MISTRA. The model contains meteorology and uses meteorological parameters (relative humidity) as decision maker for the activation of aerosol-chemistry (P4531). The strong orographic structure of the Dead Sea area significantly influences meteorology and therefore also microphysics and chemical composition of the atmosphere.

A remark should be made if UTC or Local Time is used for time reference. This is related to photochemistry which is somewhat missing in section 6. However, it is mentioned on P4544 last paragraph, but with a relation to ozone which is not observed by measurements. The authors find that ozone mixing ratios start to decrease immediately after sunrise. Measurements show that ozone mixing ratios generally do not decrease before 6:00 UT which is about 3 hours after sunrise (in summer). During the same measurements, BrO also appeared only with a 3 hours delay after sunrise. This unexpected reality is not reflected in the model runs of this publication.

The authors aim to find an explanation for measurements taken along the Dead Sea (P4528 L1). Thus, real conditions should be carefully addressed for model input. For reasons of simplicity, the following comments are given in listed style.

On P4528 L18 the authors write without giving any citation: "The wind speed over the Dead Sea is very low." This statement is generally not true. Wind speed at the Dead Sea is rather variable, up to stormy.

On P4528 L19f it is stated (again without any citation) that "... the relative humidity along the Dead Sea valley is very low". This is not in agreement with measurements which show a rather strong diurnal variation of relative humidity.



**ACPD**

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P4528 L24f: "... the ambient relative humidity can be as low as 40% at an altitude of only 10 m above the water surface." Where did the authors get this information from?

P4529f L23ff: I agree with the authors that the topography at the Dead Sea strongly influences the wind regime, and that the local flow pattern is insufficiently understood. However, the remarks on prevailing wind directions are in contradiction to literature and observation. The authors write that "the main wind direction is roughly South to North", and later on that due to a lake breeze the wind at the northern shore is almost always southerly. In reality, a very complex wind system of overlaying valley winds, cross valley winds, lake breezes and synoptic influences needs to be addressed, which causes a large variability of the wind direction. [Sirkes et al, 1997] show a comparison of several stations along the Dead Sea including one at the very northern end (Bet HaArava).

P4530 L7 and P4550 L6: the fact that "pers. comm. M. Piot" is cited appears a bit strange. By chance it is known to the referee that M. Piot was only accompanying during measurements at the Dead Sea. Maybe it would be good practice to cite the person responsible for measurements referred to.

P4530 L19: The authors have choosen an aerosol size distribution for rural areas. It might have been more appropriate to put in an aerosol size distribution for arid areas or the choice for rural should be justified.

P4534 L21ff: It might be interesting to add not only O3 and NO2 advective fluxes, but also SO2. In the lower boundary layer, SO2 is generally not observed in significant amounts. But it could be of importance in higher levels of the boundary layer.

P4547 L13: It might enrich the paper if the results of the modified EXLIM scenario (exchange of Br2 and BrCl allowed) would be shown.

The absolute maximum values of BrO tend to be overestimated compared to measurements. Therefore, further modification of the model (including the implementation of other species such as iodine chemistry) is probably needed.



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Minor comments:

P4529 L20: Citation incorrect: Niemi et al. are the editors of the monograph. The article's author should be given.

P4553 L4: Might it be less ambiguous to write "Austral. Meteorol. Mag."; instead of "Austr. Meteorol. Mag."?

References:

Zingler and Platt, 2005, Iodine oxide in the Dead Sea Valley: Evidence for inorganic sources of boundary layer IO, J. Geophys. Res., 110, D07307, doi: 10.1029/2004JD004993

von Glasow et al., 2002, Modeling halogen chemistry in the marine boundary layer 1. Cloud-free MBL, J. Geophys. Res., 107(D17), 4341, doi: 10.1029/2001JD000942

Sirkes et al., 1997, Surface currents and seiches in the Dead Sea. In: Niemi et al. (eds.), The Dead Sea, Oxford Monographs on Geology and Geophysics, 36, p. 104-113

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 4525, 2009.