

## ***Interactive comment on “Bridging the gap between bromocarbon oceanic emissions and upper air concentrations” by S. Tegtmeier et al.***

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We would like to thank Ross Salawitch for his valuable comments and for opening the discussion. We appreciate in particular that he has signed his review and are certainly open to a fair scientific debate. In order to address the two major concerns raised by the reviewer quickly we would like to provide this short author comment, while we will post a more detailed answer at a later point.

Overall we think that one of the reviewer's concerns could be taken into account fully in a revision of the paper while the other major point of criticism does not apply to the full extent and could be addressed mainly through a better description of the method used.

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1) The reviewer points out that we do not cite and represent recently published measurement papers. Due to the nature of our derived profiles, which are based on simulations during a particular time period, we have decided to not compare to individual VLSL profiles derived during specific measurement campaigns during other periods and in other regions but rather to compare them to a global mean estimate as it was provided by the latest WMO Ozone Assessment. However, it is true that we also should discuss recent studies which were not included in the WMO estimate, certainly to the extent that these new data deviate from the data on which the WMO estimate was based on. We definitely agree that the papers mentioned by Ross Salawitch are important for our study and, thus, they would be included by us in a revised version of the paper.

2) The second comment was questioning our method and asking whether we consider heterogeneous chemical reactions of inorganic bromine species. Although our model does not explicitly include heterogeneous reactions for Bry, pTOMCAT has included a detailed heterogeneous reaction scheme for Bry on both atmospheric aerosols and cloud particles (see Yang et al., 2005; Yang et al., 2010). The heterogeneous reactions result in elevated BrO/Bry ratio (Yang et al., 2010) and the direct usage of pTOMCAT Bry partitioning in our model for HBr and HOBr wet removal means these aerosol effects have been taken into consideration, though not explicitly described in our model. We cannot see any problem here regarding the methodology, but agree with the reviewer that the method should be better described, the key papers would need to be cited and the uncertainties would have to be clearly addressed in a revised paper version.

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