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Interactive comment on “A Tropical West Pacific OH minimum and implications for stratospheric composition” by M. Rex et al.

Anonymous Referee #1

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This paper reports very interesting observations but falls short in data analysis and proper discussion. As such I reject it for publication in its present format. My detailed reasons are as follows: Although this paper is intended to be part of a special issue, in my view it should nevertheless constitute a full paper in its own right, with a broader Introduction to the basic climatology and its recent history of the study region, the relevant chemistry of the atmospheric halogens considered to be important, and an extensive Discussion of the results in light of previous similar investigations in order to establish well-founded conclusions which otherwise, as presently, may not be more than mere speculation (as, for example, the brief “hint” at the modification of the stratospheric sulfate layer being sustained by moderate volcanic activity and increasing SO₂ emissions in the region and its relation to global climate, i.e., the Solomon et al. paper

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and WMO study). Frankly, with less than four core text pages I can see its current format representing not more than a “brief outline” of a paper and by no means living up to the entitlement of justifiable conclusions other than stating the - nevertheless unique observations - made in the study area. In short, the paper needs a lot more work!

It lacks in comprehensiveness and fails to set the observations in a proper modelling context, both historically and in comparison with previous field and model studies for the area. There is no real “Conclusions” section. And why are there five appendices? Are these considered to be “less” important, although they include four critical figures and key details on data sources, measurements, and modelling ?

The abstract mentions halogen emissions from kelp and seaweed farming as potentially important sources for reactive halogens in the stratosphere. Nothing about this statement is further substantiated in the text, not even maps of primary production or chl-a are included. And what about seaweed farming? Any data from the study area or a survey of worldwide seaweed farming growth and related halogen emission estimates? Negative. Likewise, no tabulated data on “moderate” volcanic activities and anthropogenic SO₂ emission trends in the East Asia/West Pacific region are provided. I am also missing a thorough analysis in relation to the “warm pool” climatology of the region, and in particular its potential relation to ENSO (2009 was an El Nino year, although anomalous SST were not reaching the study area (see, e.g., Kim et al., *Geophys. Res. Lett.*, 10 AUG 2011 | DOI: 10.1029/2011GL048521). Nevertheless, the related large-scale atmospheric circulation patterns developing in ENSO causing advection of low O₃, low OH air masses to the western Pacific mentioned in the paper should be thoroughly investigated ! Is this a persistent feature throughout each year, or occurring seasonally, or just in relation with ENSO events, etc. ?

One previous model study comprising a large data set from multiple NASA field experiments over the Pacific, specifically PEM-West and PEM-Tropics, also including direct airborne measurements of OH in this region, should certainly be referenced and discussed for comparison of model results: Liang et al., *ACP*, 10, 2269–2286, 2010.

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<http://www.atmos-chem-phys.net/10/2269/2010/acp-10-2269-2010.pdf>

With regard to the figures included, in particular Fig. 4, I really don't know how anyone without big goggles can be expected to "read" these pictures, unfortunately a negligence often encountered in modelling papers. In contrast, Fig. A2 is too simplistic (no proper y-axis marking, no caption explanations on the number of data and vertical bars).

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 28869, 2013.

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