This revised paper has now been much improved compared to its original version. It is coherently structured making it easy to follow the presentation of major results and clearly stated conclusions. However, I would have liked a more open discussion of the inherent uncertainties in the authors' main derivation, i.e., the modelled OH profiles with and without clouds (sections 3, 5.1 and 6). It appears that the temporal variability of water vapour concentrations over the study region (in connection with high reaching convective activity) as well as lack of knowledge on the mechanisms of ozone loss in the Pacific boundary layer form the major physical/chemical uncertainties in the OH simulations. However, the authors should at least provide estimates of inherent uncertainties resulting from the model itself.

Other than that I have only minor comments (see below). The paper makes a significant contribution to our better understanding of global processes linking oceans, atmosphere, and climate change, and therefore should be published.

<u>Minor comments</u>

p. 2, top line: …in clean tropical air… replace with: in clean tropospheric air

p. 2, lines 68-73: These two sentences as written are irritating and appear contradictive. They should be reworded. The first sentence suggests higher Ox and lower H2O levels in the UT, whereas the next sentence points out low Ox due to higher H2O via convection in the UT in reference to (R2).

p.2, line 100: This sentence can be omitted.

p. 3, line 160: It is not clear right away what is meant by background current, i.e., that this is a technical term, unrelated to some atmospheric "current". I suggest to expand the sentence to: ….based on a correction due to the electrical background current in the ozone sonde measurement cell…

p. 3, Fig. 2, last line and p. 8, line 440: Since OH concentrations are expressed elsewhere in mixing ratios (ppt, which is suitable for this paper), they should also be quoted in ppt on p. 8, and NO levels (detection limit) as well in the Fig. 3 caption. However, if these were calculated for STP conditions this should be stated explicitly.

p. 3, lines 183/184: Correct to: …just missed…, but the results agree well…

p. 4, line 227 and Fig. 4a caption: The symbols are not dots, but open circles. Also, I suggest to choose a different line colour for the circles, such as yellow, for better viewing.

p. 4, line 283: Omit: "large" uncertainty with respect to Tan et al.'s OH measurements <u>or even better</u> (as pointed out above) add a quantitative comparison here of uncertainties in the measured vs. the present calculated OH concentrations !

p. 5, line 372 and Fig. 4h: The meaning of Fig. 4h is not clear. The % per day y-axis label needs to be explained. In addition it should be stated if both photolysis and OH reactions have been considered. Furthermore, the relatively long tropospheric residence times of several tens of days are, at first glance, surprising. Some discussion to illustrate the plausibility of this range would be helpful.

p. 8: It should be clearly stated whether the calculated OH concentrations represent 24 hour averages or daytime averages (with the model assuming zero OH levels at night).