Dear Editor,
We agree with the referee that uncertainties in our model calculation need to be thoroughly and openly discussed in the manuscript. We do think that our most recent version of the paper does a fair job in doing so and we have made further adjustments according to the referee's comment. These are described below.

Measurements of OH are not available for the region of the world that is discussed in our paper. So the model results we present are the best available estimates we have. Despite the lack of direct observations of OH we find the results important and think they need to be reported in the literature. We hope they will motivate the considerable effort that will be required to measure OH in this region of the world. We agree that eventually such measurements will be needed to make robust statements about the level of OH in that air and we say so in the paper. But without such model based estimates in the literature it will be very difficult to convince funding agencies to provide the considerable resources that will be required to carry out such measurements.

In our paper we try to describe this situation in a very open way. In the abstract we say that we have observed an ozone minimum but point out that these measurements *suggest* (line 8) the existence of an OH minimum. We think this is a fairly careful way to present our model results.

In Section 3 we point out the lack of direct observations of OH and in the revised manuscript we have added a new paragraph (lines 264–288) with an even broader discussion of the uncertainties of such model based estimates of OH and conclude that direct measurements of OH are required. In the third paragraph of the revised conclusions we reiterate the uncertainties and the need for direct observations.

It would be desireable to robustly quantify the uncertainties of our model results. However, for global model studies this is not possible and it is the state of the art in global modelling to assess the validity of model results by evaluating the model in comparisons with available observations rather than by propagating potential sources of uncertainties through the model and come up with a bottom-up quantification of the uncertainties. We have carried out a comprehensive evaluation of the model run, which is mostly described in Ridder et al. and which is referred to in our paper. We have also compared our results for OH with all available measurements of OH with very encouraging results (lines 294–300 and section 2.2).

A bottom-up quantification of uncertainties would require to propagate dozens of potential uncertainties in chemical kinetic data and from uncertain parameterizations for convective transport, microphysics etc. through the model. The only way to do that is in a Monte Carlo approach, similar to the one described in Kawa et al., ACP, 9, 8651–8660, 2009. This requires thousands of model realizations. While this can be done for a zero dimensional box model for a very limited set of chemistry (c.f. Kawa et al.), it is absolutely impossible to do that with a global 3d model. Therefore this has never been done in the large body of literature that reports results from such models. Although desireable, we have to accept that the state of the art does not allow such a rigorous quantification of uncertainties in 3d model studies.

Any attempt to just isolate some uncertainties and look at the sensitivity of the results on the main drivers of uncertainties are very problematic. As the referee mentioned, water vapour and clouds are major drivers of uncertainty in our calculations. The parameterization for convective transport is another. But we cannot simply vary these things in the calculations and assess the sensitivity of the resulting OH field. For example, if we just vary water vapour, the model radiation (and hence the model vertical transport), the model convective transport and the model clouds will all be inconsistent with the water vapour field. For example we would have clouds (and henced reduced radiation) in undersaturated air or cloud free areas in supersaturation, which would both result in very unrealistic chemistry in that air. Also patterns of vertical transport, which are closely related to water vapour would not be consistent with the water vapour field in such model runs. Very similar inconsistency and unrealistic chemistry would occur in model runs in which any of the other paramters are varied in isolation.

The only way to vary water vapour or clouds in the model is to change the parameterizations for convection and microphysics in the underlying dynamical core of the model. This is the GEOS-5 Earth System Model and changing any parametrization in this model requires to adjust a large number of free parameters in the model setup. This would be a major multi-year project well beyond the scope of our paper.

Best regards,
Markus Rex