Reply to Editor

Thank you for your considerate handling of our manuscript. According to the referee's comments, we added the requested descriptions to the revised manuscript (P7, L197−198, L222−225). We also added typical IO averaging kernel in Figure S1 as suggested by the referee. We also added some references in Table 2 because one of the co-authors was suggested to referee the original works from which these rates were obtained.

One reviewer recommends minor revisions prior to publication. Please follow the recommendation closely in the revised manuscript.

In addition, the other reviewer made the following suggestions in the event of minor revisions:
1) This sentence from the response "We might not need a brand new flux mechanism but rather a good parameterization of the traditional organoiodine fluxes (including their photolysis) over the region." or something to the same effect would still be useful in the text.

2) The quoted degrees of freedom make clear that the near surface IO might have some interference from the column. If the authors could offer more detail on the vertical distributions of information content it would be better.

Reply to Referee #2

We thank the reviewer very much for the constructive comments.

In response to my concerns about the retrievals, the authors have added one line to the manuscript detailing the DOFs of the data set which average slightly above 1. In the response and further in the manuscript, they further explain this piece of information is a surface mixing ratio and information in higher layers is reflective of a priori selection rather than measurement data. This point would be improved with an exemplar averaging kernel showing surface sensitivity (could be in supplement). I'm concerned that the authors still have not presented the uncertainties associated with the retrieved IO mixing ratios. It sounds like the retrieved quantities heavily depend on a priori selection, which raises the question of why the authors are doing a retrieval at all? Presumably the low elevation slant columns would be sufficient to show the relationships described in the manuscript. I understand a mixing ratio is needed for the model comparisons, but if the uncertainty on the retrieved quantity is to high to be useful, one questions the utility of this comparison to begin with.

We added a typical averaging kernel for IO in Figure S1. To test the sensitivity of vertical profile shapes on the retrieved surface IO mixing ratio, a different a priori profile was tested where an exponentially decreasing profile with a scale height of 1000 m (same as Großman et al, 2013), instead of constant below 500 m with an exponentially decreasing profile above 500 m; the IO mixing ratio changed from ~0.8 to ~0.6 pptv. Considering the 3-D model simulation outputs, the tested profile here might be already off the likely range and thus the mixing ratio change should be regarded as maximum. Note that as described in the original manuscript, day-to-day variations near the surface were unaffected by the choice of profile. As such, as the reviewer pointed out, discussion on the basis of DSCD would be preferred, but we used both DSCD and mixing ratio for the comparison with the mixing ratios derived from our 0-D model, with caution.

This information from the response should be incorporated into the manuscript. "The correlation coefficient between SST and IO was 0.39. That between SST and O3 was −0.51. That between wind speed and IO was −0.45. Also, that between wind speed and O3 was 0.59. It is noteworthy that the correlation coefficient between IO and O3 was −0.75, which is much higher than others, and thus being the dominant feature. "

We added a related description to the revised manuscript (P7, L222−225).

Großmann, K., Frieß, U., Peters, E., Wittrock, F., Lampel, J., Yilmaz, S., Tschritter, J., Sommariva, R., von Glasow, R., Quack, B., Krüger, K., Pfeilsticker, K., and Platt, U.: Iodine monoxide in the Western Pacific marine boundary layer, Atmos Chem Phys, 13, 3363-3378, 10.5194/acp-13-3363-2013, 2013.