Reply to Referee #2

We thank the reviewer very much for the careful review and constructive comments. All changes are highlighted in the track-changes manuscript. A detailed description of our revisions is presented below.

Section 2.1: An explanation of the a priori choice is needed. Is it appropriate to use an identical a priori given the wide range of regions covered? Similarly, explanations for the choice of aerosol single scattering albedo, asymmetry parameter, and surface albedo should be given, preferably with references.

For this study, the single scattering albedo, asymmetry parameter, and surface albedo were identical to those reported by Großman et al. (2013). We added a corresponding description to the revised manuscript (P4, L113−114, track-changes manuscript).

Here, the single scattering albedo, asymmetry factor, and surface albedo were used similarly to work presented by Großmann et al. (2013).

The authors characterize the experimental uncertainty for the dSCDs, but then do not provide any comparable information for the retrieved IO VCDs or mixing ratios in the lowest 200m. The reader needs this information to interpret the level of support provided by the data for the conclusions. Similarly, given the importance placed on the near surface mixing ratios, the authors need to demonstrate the retrieval is sensitive to the near surface mixing ratio particularly given later comments suggesting that a priori selections play a large role in determining the retrieved surface mixing ratios. How large of an effect is this? If it is minimal compared to daily variations, that statement needs more support. This comment suggests the retrieval does not reflect the true atmospheric state, which raises questions about the ability of the authors to quantify the amount of IO present beyond a dSCD. These questions can be answered by showing averaging kernels that reflect the ability to retrieve the IO mixing ratio in the lowest layer (Peak near 1 near the surface with minimal values in other layers). To summarize over the entire data set the authors should provide statistics on the total DOFS for the retrieval as well as the DOFS in the near surface layer. This information will give the reader confidence that the IO values being presented are meaningful, and the papers conclusions are well supported.

The DOFs for the NO_2 profile during the MR14-06 (leg1) cruise were 1–1.4. We added a related description of DOFs (P4, L114−115, track-changes manuscript).

The degrees of freedom (DOFs) for the IO retrieval for MR14-06 (leg1) were 1–1.4.

Because the vertical profile information was insufficient, the IO concentration near the surface depends on the shape of the *a priori* profile used for the retrieval, as described in the original manuscript (P6, L165−166). However, day-to-day variations near the surface or

correlation between ozone and IO were unaffected by the choice of profile (for example, we retrieved the IO profile using a priori profiles constructed as an exponentially decreasing profile with scale height of 1000 m, as did Großman et al. (2013). In this case, the IO content near surface is lower, but clear negative correlation with ozone was obtained).

Section 2.4

A characterization of the uncertainties associated with the in-situ ozone and CO measurements should be added to this section.

We added a description about uncertainties to the revised manuscript (P5, L150−153, track-changes manuscript).

The O_3 instrument was calibrated twice per year in the laboratory, before and after deployment, using a primary standard O_3 generator. The CO instrument was calibrated on board twice per year, on embarking and disembarking of the instrument, using a premixed standard gas. The reproducibility of the calibration was to within 1% for O_3 and 3% for CO (Kanaya et al., 2019).

Line 157. The authors mention insufficient data to show diurnal variations. This statement needs more clarification. You have a lot of data, more than most folks trying to measure IO, why do you not feel good about showing diurnal variations?

We added a figure of diurnal variation (Figure S4). No clear diurnal variation was observed, but clear day-to-day variation was observed as shown in Figure 7.

Line 207: Figure 6 doesn't support the statement of no correlation by itself. I just see timeseries of wind speed and SST, with no attempt to relate these quantities to IO or ozone.

We added the median and 1σ values to the wind speed and SST similarly to CO in Figure 6 to clarify their mutual correlation. The correlation coefficient between SST and IO was 0.39. That between SST and O_3 was −0.51. That between wind speed and IO was −0.45. Also, that between wind speed and O_3 was 0.59. It is noteworthy that the correlation coefficient between IO and O_3 was −0.75, which is much higher than others, and thus being the dominant feature.

Section 4: Why is it important that IO was detected at low latitudes?

High IO content was observed at low latitude around Indonesia (near the coast; not remote ocean) as well as western tropical Pacific. This sentence might be confusing: we deleted that.

Figure 3: This figure needs error bars on the IO and ozone measurements to show the spread over the data set. I'm also unclear why the linear fit is calculated/shown. I didn't see a reference to it in the text, unless the goal is simply to show anti-correlation, in which case showing an R value makes more sense then the linear fit equation. If there is something important about the fit equation, it would be helpful to know what type of linear fit was done, particularly since the temporal variability of the IO measurements and ozone measurements are not necessarily linked.

As described above, we added an explanation about the uncertainties of O_3. We suppose that it would be a natural choice to select a linear fitting line in Figure 4 to show the dominant feature. In accordance with the referee's comment, we added the correlation coefficient to Figure 3 ($R = -0.75$).

Figure 4: Why plot dSCDs rather than VCDs or the surface mixing ratio? dSCDs don't really have much meaning to folks outside the DOAS community. While I find this figure very helpful for showing the cruise tracks and overall spatial extent of the data set, I find myself also wanting to be able to see each cruise plotted individually so I can examine the dataset for each cruise individually. Right now it seems like there are a lot of data points plotted on top of each other. Can you put plots for each cruise in the supplement for the curious reader?

In this study, because we were unable to obtain sufficient vertical information from DSCDs, we showed IO in DSCDs. During the MR14-06 cruise, positive correlation was found between IO VMR and IO DSCDs. Following your comment, we added the DSCDs plots for each cruise in the supplement (Figure S6).

Why are figures 5-7 only shown for 1 cruise?

We specifically intend to examine IO and O_3 variations at high SST area in this study and thus chose the MR14-06 Leg 1 cruise here

Data availability: I don't see a data availability statement showing where the data underlying this paper can be obtained, which I believe is a requirement for publication in ACP, and also a generally helpful thing to do for the broader scientific community.

Thank you for your comments. We created a section explaining data availability in the revised manuscript.

Line 34: regiona006C to regional?

This was corrected to regional.

References

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.