Observations and modelling of glyoxal in the tropical Atlantic marine boundary layer – Response to Anonymous Referee #2

We thank the reviewer for their positive and supportive comments on our manuscript. Below we respond to each specific comment in turn. Reviewer comments are shown in italic text, and our responses in normal text. Where we have added new text to the manuscript, this is summarised in bold text. Line numbers refer to the previously submitted original manuscript.

Page 1, lines 13-14: The modeled glyoxal seems rather insensitive to aerosol effects, especially compared to the effects of acetaldehyde or sesquiterpenes. Later in the paper the authors say this (page 21, line33-34): "…the sensitivity of the modelled glyoxal to changes in the rate of aerosol uptake is not sufficient to reconcile the model with the observations." I would suggest changing the language in the abstract to be more consistent with the later text.

This is a good point, and we have modified the abstract text, replacing this sentence with:

"**The model showed limited sensitivity to changes in deposition rates of model intermediates and the uptake of glyoxal onto aerosol compared with sensitivity to uncertainties in chemical precursors.**"

Page 3 line 18-19: It would be good to also cite Lerot et al. 2021, who report glyoxal retrievals from TROPOMI, which like the other satellites also sees enhanced glyoxal over remote tropical oceans. The authors discuss several reasons why this might be the case.

Thanks for drawing this to our attention. We have included a citation to this study, which we had previously missed in preparation of the manuscript:

"The largest mixing ratios have been measured near coasts, and these measurements show less evidence for enhancement of glyoxal over remote tropical oceans as suggested by some satellite measurements (Vrekoussis et al., 2009; Lerot et al., 2010; 2021). Limited measurements of glyoxal in the free troposphere by airborne MAX-DOAS (Volkamer et al., 2015) may be consistent with the satellite-observed enhancements, although caution may be needed in interpretation of glyoxal satellite retrievals due to spectral inteferences (Lerot et al., 2021)."

Page 3, line 34-35: While 1.5e14 is the number from Lawson et al. (2015), it is a little confusing to compare a column measurement with an in situ measurement without also discussing the assumptions used to convert the in situ mixing ratio into a VCD. Stating that the satellite columns indicated higher levels of glyoxal than the in situ measurements would be fine.

We agree with this point, and have modified the text to more explicitly state how the in situ and satellite data were compared by Lawson et al., (2015):
"Assuming that all satellite-observed glyoxal resides in a well-mixed marine boundary layer of depth 850 m allowed a direct comparison of the in situ observations with GOME-2 vertical columns. This comparison suggested that the satellite observations exceeded the is situ observations by more than 1.5×10^{14} molecule cm^{-2} at both sites. However, this neglects the possibility of further glyoxal enhancements aloft, revealed by airborne measurements noted above."

Page 14, line 8: Are there any measurement of aerosol composition, either at Cape Verde Atmospheric Observatory or from the ATom campaign, that could be used to better inform

the model? Several of the references for the glyoxal uptake value (e.g. Volkamer 2007) are from studies in urban areas, where I would expect the aerosol to be mostly organic. I'm not sure what effect the different ions in sea spray aerosol would have (e.g. Waxman et al. 2015), and a "real" number is better than a made up on, but it should be noted that an uptake coefficient for urban aerosol may not be representative of marine aerosol.

While there is uncertainty in the aerosol uptake coefficient, which may be dependent on aerosol composition, we note that the model results are relatively insensitive to aerosol uptake (as also pointed out by the Reviewer above). We therefore do not feel that it would be worthwhile incorporating more information on aerosol composition in the model. Our sensitivity test to aerosol uptake (Fig. S10) also allows some measure of the magnitude of influence that may be expected, which is small. Nevertheless, we do agree with the reviewer that it is important to note that the aerosol uptake coefficient assumed is taken from data on continental aerosol, and may not be representative of marine aerosol. We have added a sentence to this effect (Page 14, line 8):
"This value of γ for glyoxal uptake is based on studies of continental aerosol, and so may not be representative for aerosol in the remote marine atmosphere. However, we find small sensitivity of simulated glyoxal in the model to the assumed value of γ."

The yellow font, and to a lesser extent the yellow traces, used in Figures 10, 12, and S10 is rather hard to read. A darker shade of yellow for at least the legend would help.

We apologise for the unclear yellow colour used in these figures. We have changed this to a more readable darker shade in these figures.

I'm not sure what the ACP style guide says, but in Tables 1 and 2 I would use a lowercase "i" and "n" to abbreviate iso-butane and n-butane (and the other VOCs where this applies), to avoid confusion with nitrogen and iodine.

Thanks for noting this. We have decided to change these prefixes to lower case to avoid confusion.

Figures 5, 7, S1, and S2: Figure 5 uses day of month, while the other figures use Julian Day. It would be easier for the reader if a consistent date format, preferably that which was used in Figure 5, was used for all these figures. Alternatively, dashed vertical lines on the Julian Day plots to indicate the first day of each month would work.

All figures now use a consistent day of month time axis.