Dear Andreas Richter,

please find below our reactions and responses (in bold) to your comments and suggestions (in italic) for the manuscript acp-2022-416, dated from Dec. 12, 2022.

Dear Flora Kluge,

I'm pleased to accept your revised manuscript "Airborne glyoxal measurements in the marine and continental atmosphere: Comparison with TROPOMI observations and EMAC simulations" for publication in ACP subject to minor revisions as listed below.

Most of the concerns raised by the reviewers have been addressed in the replies, and some clear improvements have been implemented in the revised manuscript. Please consider the following minor before submitting the final version of your manuscript:

Thank you for the overall positive assessment of our reactions to the previous reviews of the manuscript.

• In several places of the manuscript, you mention that the mini-DOAS measurements are validated by the satellite observations. I don't think that the satellite observations, in particular at low glyoxal levels can and should be used to validate your observations. If anything, it should be the other way round.

We agree and accordingly changed the wording to 'cross validation' of the air- and spaceborne measurements, wherever necessary in the manuscript (lines 107, 113, and 609)**.**

• On page 5, 153 you state "the instrument measures the atmospheric column density of the targeted gases below the aircraft..". I do not think that is correct (see your Figure 2). I suggest to reformulate to "the instrument receives light from the surface and atmosphere below the aircraft ..:" or a similar formulation.

We accordingly changed the text to '…In the Nadir observation mode, the instrument receives light from the surface and atmosphere below the flight altitude (Fig. 2). It thus preferably measures glyoxal below the aircraft with a rectangular foot print of …'

• On page 8, first lines you provide the detection limit for the column. As this is still in the slant column section, I assume that you are providing slant column detection limits, and these are independent of the air mass factor.

We concur and accordingly changed the text to '…the typical SCD detection limit is….' in order to make more clear what is meant.

• In the caption of Figure 2, please add the information on what you assumed in terms of clouds (I assume none) and aerosols (I don't know what you assumed here).

In order to make clear what is assumed in the radiative transfer simulations, we added the following sentence to the legend of Figure 2: For the radiative transfer simulations, no clouds, but aerosol profiles as described in the text are assumed (see sect. 2.1.4).

• On page 12, line 290 you state that you have used an "all-sky albedo of 0.3". Is that really the case? This value appears excessively large to me. If this is meant to correct for the effect of residual clouds,

this would depend strongly on the glyoxal vertical profile and the cloud altitude – assuming a large albedo increases the AMF while a cloud above the boundary layer should lead to a smaller AMF.

Essentially we following your comment, but fortunately the sensitivity of the AMFs on the assumed all sky albedo for the actual flight altitudes is relatively moderate but distinctively larger for altitudes below the aircraft, as the RT simulations in Figure 2 indicate. In order substantiate our conclusions regarding the RT related uncertainties on the inferred VCDs, we additionally compare the inferred VCDs for clear (A = 0.1), cloudy (A = 0.6) and all skies (A = 0.3) for the glyoxal profiles and flight altitudes shown in Figure 2 (and mention them in the text at the end of section 2.1).

• *On page 31, line 717 you state that negative columns are omitted in the comparison to the model data. As the second reviewer, I believe that this will introduce a bias in your comparisons. This bias may be small, but removing negative values in such a comparison is not mathematically correct. As you have already evaluated the effect in your reply to the reviewer, I would suggest to replace the figures and numbers with the version using all measurement data.*

We agree and adjusted Figure 12 as well as the text accordingly (i.e. removal of lines 716ff: 'Since for low glyoxal concentrations the measurement noise occasionally leads to negative data (see Fig. 6 and Fig. 11), and the model does not reproduce such measurement noise, inferred negative glyoxal observations are omitted from the comparison.').

• *In the conclusions, please repeat the point that your profiles should not be used as a climatology as the sampling is strongly biased to polluted and coastal scenes. While this is clearly stated in the text, I worry that some readers will directly skip to using the profiles as "typical" for the respective regions, which would not be good.*

We accordingly changed the sentence in line 855 (cont.) to:
'The combined airborne Nadir and Limb data set is the first of its kind and may offer new information into the fate of atmospheric glyoxal in the global atmosphere. The Limb measurements further allow us to infer and investigate different glyoxal profiles in the troposphere over different regions of the world. They are, however, not representative for climatological glyoxal studies due to their limited coverage in space and time.'