Reply to reviewer #2

We provide a point-by-point reply to the issues raised by reviewer 2 below. The original review is included in grey. Text changes in the manuscript are indicated in italic font.

Please also note the modifications made in the revised manuscript that are specified in the Corrigendum.

The manuscript by Beirle et al. presents a parameterization of O_4 vertical column densities (VCD) based on surface observations of temperature (T), pressure (p), and, ultimately, relative humidity (RH). A first parameterization that only consider p and T is derived based on first principles and performs reasonably well when compared to "true" O_4 VCDs calculated from WRF, ECMWF, and radiosonde data. The authors use a modified version of the first-principles parameterization and the true O_4 VCD to develop an empirical parameterization that also include surface RH. This empirical parameterization improved the O_4 VCD calculations to below a 2% uncertainty that is needed for MAX-DOAS based inversions. The authors identify several instances in which the parameterization is less accurate, such as condition with surface inversions and mountainous regions.
Overall, this is a well-written manuscript that present a new method to improve O_4 VCD calculations. The presented parameterizations will be useful to the MAX-DOAS community, which needs these VCDs for their retrievals. There are some parts of the manuscript that could be further strengthened, as I will outline below, and a few minor text/language issues. The manuscript fits well into AMT, and I recommend its publication after minor revisions.
We thank the reviewer for the positive assessment. Below we reply to the raised issues point by point.

Detailed comments:
Line 44-45: It seems unlikely that a lapse rates close to 0 can be achieved due to condensation alone. There likely some dynamic reason as well. It may be worth citing books/manuscripts from the meteorological literature that give an overview of potential atmospheric lapse rates here.
This is a misunderstanding; we do not want to claim that the lapse rate becomes 0 by condensation. Instead, the addendum "closer to 0" is meant to indicate the direction of the change; as the dry lapse rate is negative, it is lower than the moist lapse rate, but its absolute is higher. In order to avoid confusion, we do not use "lower/higher" here. We modified the respective sentence as follows and hope that this avoids misunderstandings:
… parts of the oceans with weaker (i.e. closer to zero) lapse rates due to condensation.

Section 3.4: Please provide some more information on the time frame over which the sondes were flown. It also seems that some of the locations had very few sondes, thus making the statistical interpretation challenging. It may also be a good idea to add the number of sondes for each location to Table 3.
We thank the reviewer for this comment. We have added information on the time coverage of the analysed sonde launches to Appendix E. By comparison with table E1, we then noticed that the number of sonde launches did not match. Actually, table E1 lists the number of all GRUAN profiles rather than just those for SZA < 85°. We have corrected this in the revised manuscript.
We agree that for several stations, statistics are quite limited (even more for the corrected number of available profiles). However, if stations with few profiles would be skipped, some conditions (e.g. tropics) would not be included any more. We still consider the limited information content of these stations to be valuable for this study, as none of the stations shows any exceptional behaviour.
In response to the comments of reviewer 1, we have decided to present the data of table 3 in a new figure in the revised document. In order to indicate low statistics, the results for stations with few profiles are marked by lighter color.

Section 5.4 and 5.5: These sections present some interesting ideas. However, the proposed formulas are not backed up by any data or detailed analysis. I also found these sections rather distracting from the main point of the paper. They should either be expanded by showing that the calculation of lapse rates yields reasonable results by comparing them to the meteorological data the authors have already used in the manuscript or, which would be my recommendation, be moved into their own publication.

We understand that sections 5.4 and 5.5 of the discussion paper could be considered distracting. We have thus revised the manuscript as follows:

- Concerning Sect. 5.4, the effective lapse rate is now already defined in the formalism section (2.3). We now also present a comparison of the effective lapse rate to the 5 km lapse rate from ECMWF profiles in Sect. 4.
- Concerning Sect. 5.5, this is so far not more than an idea for a future application, which might indeed become a separate publication as soon as substantiated by measurements. Nevertheless, we would like to mention this idea already in this manuscript. In order not to distract the logical flow of the discussion, we moved this subsection into a new subsection of Appendix A, where the ratio of effective heights is discussed.

Lines 257 – 259: This is such a central part of the manuscript that I would recommend expanding it to provide the reader with more information on how the parameters a and b were derived. Maybe add a figure of the data and the fitting line. In addition, please provide uncertainties and R^2 of the fit.

We agree that the description of the fit was lacking for detail.
In the revised manuscript, we have clarified the fitting procedure in section 2.5 (formalism).
The fit parameters (with uncertainties) are now derived in section 4.2, which also includes a new figure showing the data, correlation coefficient and the fitted line.

Line 329 – 330 (and other places in manuscript): I believe this could be generalized in stating that the parameterization loses accuracy when surface temperature inversions are present, i.e. in the morning and evening, in the Arctic, etc.

We agree. In the revised manuscript, we have added the following footnote to the introduction:
Note that, for this approach, as well as for the parameterizations presented in this study, temperature inversions are problematic. As MAX-DOAS applications require daylight, however, night-time inversion layers are irrelevant for this study. The remaining temperature inversions at daytime, mostly occurring in early morning hours and over cold water and ice surfaces, will be discussed in Sect. 5.2.
We discuss the effect of temperature inversions in detail in a new section (5.2) in the discussion, including a map of surface temperature inversions in ECMW data on 18 June 2018 that clearly illustrates that for these conditions higher deviations are found.

Line 39: "… as the main source…"
Done.

Line 42: "The main reason…"
Done.

Line 159: introduce SZA here by spelling out "solar zenith angle"
Done.

Line 350-351: change to "Obviously, other factors would probably also have to be…."
Done.