Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2021-213

Anonymous Referee #2

Referee comment on "Calculating the vertical column density of O_4 during daytime from surface values of pressure, temperature, and relative humidity" by Steffen Beirle et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-213-RC2, 2021

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.

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.

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.

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.

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.

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.

Line 39: "… as the main source…"

Line 42: "The main reason…"

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

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