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Atmospheric Chemistry and Physics Discussions

Interactive comment on "MAX-DOAS measurements of NO_2, SO_2, HCHO and BrO at the Mt. Waliguan WMO/GAW global baseline station in the Tibetan Plateau" *by* Jianzhong Ma et al.

Anonymous Referee #2

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In their manuscript "MAX-DOAS measurements of NO2, SO2, HCHO and BrO at the Mt. Waliguan WMO/GAW global baseline station in the Tibetan Plateau", the authors report on three years of MAX-DOAS measurements at the high altitude station of Waliguan. They provide a detailed description of the technical steps taken in the evaluation of the spectra, describe the radiative transfer calculations performed and discuss connections between the measurement results and meteorological parameters.

Measurements of high altitude background concentrations of atmospheric trace gases and aerosols are an interesting topic, and MAX-DOAS measurements are one sensitive measurement technique to obtain long-term data sets of such quantities.

Unfortunately, this manuscript in mainly a long and very detailed description of the technical aspects of the analysis and the meteorology at the station and provides very little results which are of general interest. I can therefore not recommend this manuscript for publication. I'd suggest that the authors work on the manuscript by removing all unnecessary parts, tightening the technical discussions to what is really needed, and focus more on the results and what we can learn from them. Such a shortened and more focused study could then be re-submitted to ACP or another journal.

Major comments

- The manuscript introduces and discusses many meteorological parameters and their correlations with the measurements, but all correlations are very small and this whole part could be summarised in a single sentence: No significant correlation was found between any of the measured quantities and meteorological parameters. Removing all the unnecessary figures and descriptions on meteorology would already considerably reduce the length of the manuscript.
- Most of the manuscript deals with technical aspects of the retrieval and in particular in the supplemental material, sensitivity studies are described in great detail. While thorough documentation of the methods used is a good thing, most of what is described is state of the art and could have been summarised in a few short sections.
- The one point where the authors introduce a better approach than earlier studies, namely accounting for the topography along the line of sight is unfortunately not expanded upon at all – there is no discussion of what the difference to a standard analysis using a representative surface altitude and a 1d retrieval would have been, how these 2d effects vary with season, snow cover and aerosol optical depth and if this approach results in more accurate results considering that due to computational time limitations, only two scenarios could be computed.

- Unfortunately, the measurements were performed with a simple, not very sensitive instrument and are therefore not of high quality. Because the shading tube was missing, zenith measurements could not be used which leads to a further reduction in sensitivity. The fact that elevation calibration appears to be off by 4° further increases uncertainties, in particular as this implies that the authors cannot be sure about the exact pointing of the horizontal measurements they use. This uncertainty needs to be evaluated and included in the error budget.
- There is literally no discussion of the results in terms of comparison to other measurements or model results or what we can learn from the three years of measurements which are presented.

Minor comments

- I believe that the excessive listing of references at the top of page 4 makes no sense as these references have no connection to the topic of this manuscript.
- I'm not so sure about the discussion of straylight on page 8. My guess is, that the straylight correction does not primarily correct for more straylight in the reddish direct sun spectra (cloudy sky spectra should have a similar wavelength distribution as direct sun spectra) but that it corrects for the change in wavelength dependence of Fraunhofer filling in.
- It should at least be mentioned that zenith measurements which are used here for the CI were earlier discarded for trace gas retrievals because of suspected direct sun impact
- Figure 8: Are these realistic stratospheric BrO profiles? This would imply a factor of 4 variability in stratospheric BrO columns – has this been observed in measurements?

- Figure 10: Title of figures not correct
- Figure 11: t does not become clear if all directions are included in the statistics and if so why. My understanding is that only 1° measurements were used in a quantitative way.
- Conclusions, line 22: Enhanced trace gas mixing ratios – enhanced in comparison to what?
- I'm surprised that no reference is made to the paper

  Gomez, L., Navarro-Comas, M., Puentedura, O., Gonzalez, Y., Cuevas, E. and Gil-Ojeda, M.: Long-path averaged mixing ratios of O3 and NO2 in the free troposphere from mountain MAX-DOAS, Atmos. Meas. Tech., 7(10), 3373–3386, doi:10.5194/amt-7-3373-2014, 2014.

  which uses a similar method for a related topic.

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