The authors would like to thank the anonymous reviewers for his/her valuable comments. We also would like to thank the editor for his decision to publish the manuscript, after minor changes (e.g. answering the first comment of reviewer #3).

Although the overall reply to the referee comments is adequate and comprehensive enough, I feel that the first comment of the third reviewer (i.e. why not reporting free-tropospheric columns above the site in addition to local mixing ratios?) would deserve a reply. Please could you comment further on this point and explain the motivation for sticking to vmrs in this study?

It would have been nice if the authors would also have reported free tropospheric trace gas partial columns above the location of the instrument. This complementary information would have been equally unique and relevant and is contained in the MAX-DOAS data set used in this work.

Gomez et al. (2014) proposed a method to retrieve volume mixing ratios of NO2 and O3 by using mountain MAX-DOAS measurements. As the approach of our study is based on their method, we initially were focusing on vmrs of NO2, as they did in their study. In a later step of the analysis, it became clear that the retrieval of HCHO also showed interesting results. Volume mixing ratios of HCHO have so far not being published from such kind of measurements. There was already a large amount of results and we decided to write up everything.

We agree that one drawback of our study is the fact that there are hardly any reasonable measurements available for comparison. However, some studies from the past report on mixing ratios in the proximity of the measurement locations of the instruments used in our study. As we found reasonable agreement between the values reported in those studies and our values, we found that the manuscript was well rounded and that there was no need to do more for this study.

Nevertheless, we agree that it would have been a nice opportunity to also derive free-tropospheric columns of the two trace gases. For this, we could have used satellite measurements for comparison. However, as stated above, the manuscript already had a nice extent and was well rounded. Moreover, a comparison of columns with satellite measurements would have been difficult, as there is a large amount of trace gas concentrations below our MAX-DOAS measurements (e.g. in the boundary layer), which significantly contribute to tropospheric NO2/HCHO in satellite measurements. We only have reasonable data for a few months of the year 2003 at Zugspitze and a multi-year data set at Pico Espejo – but also only for a few months of each year. The only reasonable comparison of columns would have been a comparison of a seasonal cycle. This was nicely performed by Franco et al., (2015). They had a larger amount of data for such an analysis. However, this was not possible in our case. Therefore, we decided not to include an analysis of free-tropospheric columns. Nevertheless, we made a brief reference to this work and even compared our HCHO results at Zugspitze with their results.

In our opinion, the first comment was rather a personal/subjective comment (e.g. “if I would have analyzed these data, I would have retrieved free-tropospheric columns, in addition to vmrs”). After this comment, the following sentence is written:

*In my view, the paper can be published after minor changes **described below**.*

We have taken into account all comments below.