We thank the referees for their comments and suggestions and the careful reviewing of our manuscript. They have helped us to improve greatly our draft. We have followed the most of their suggestions and we have changed the text of the manuscript accordingly for a better understanding of our work.

Our reply to your questions/comments is in blue.

Referee #2,

Major comments

I was confused in several places in Section 1.2. First, the authors state, "it is assumed that for both geometries the scattering altitude is the same and is close to the station level." I cannot accept this assumption. Additionally, Fig. 2 does not support the assumption; it indicates that (1) the station level is h(s), which is different from the scattering altitude of h(s) + h for observations at an elevation angle of zero degrees and (2) the direct sunlight measurement is represented for an elevation angle of 90 degrees, while the MAX-DOAS should measure the scattered sunlight. The authors might claim that Fig. 2 is a schematic diagram, nevertheless the definition of the scattering heights in Fig. 2 indicates that for both geometries the scattering altitudes are not the same and differ from the station level by h.

The assumption must be supported by a radiative transfer (RT) model. Indeed, the authors have included discussion using the SDISORT RT model in Section 2. However, their approach using the differential box-AMF is not appropriate for discussing the scattering altitude. They argue that the scattering takes place below 3 km for elevation angles of both zero and 90 degrees, as the differential box-AMF is very small above 3 km. If this logic is correct, how do they explain the differential box-AMF for other sets of elevation angles (e.g., for 1 and 90 degrees), which show significant values of differential box-AMF at least up to 10 km?

If the authors want to discuss the scattering altitude, the box-AMF is a better parameter than the differential box-AMF. However, I suggest that they use the most direct parameter, which is the scattering altitude estimated using a RT model. I understand that the authors wanted to utilize a unique feature of the differential box-AMF vertical profile for elevation angles of zero and 90 degrees. Please interpret the feature properly, if the assumption "for both geometries the scattering altitude is the same and is close to the station level" is unnecessary.

We apologize for the confusion caused by the way in which the manuscript was written.

The method relies in the assumption that slant paths contribution of both geometries (zenith and horizontal) cancel out, and only the tracer contained in the horizontal path is measured (see modified figure 1). The zenith spectrum is used as a reference to analyze the horizontal one, thus, only the contribution of the horizontal rays remain. The approach is valid in a near-Rayleigh atmosphere and if measurements are simultaneous. Free atmosphere is generally clean and it is particularly the case In Tenerife. Mean AOD is below 0.05 being 0.02 a common value, out of Saharan outbreaks. On the other hand small differences in time due to motor movement are taken into account and corrected.

In summary, it is not a required condition that the scattering takes place at the level of the station that in fact, as the reviewer has clearly stated, is not the case. We have reformulated the text to make it clearer.

As concern the sketch in figure 1, it has been redrawn to avoid the misunderstanding mainly due to the amplification of the Earth curvature. In fact, the value of 'h' in our case is of only few hundred of meters, at maximum.

Second, the authors have used correction factors f and f' to account for a SZA difference. I understand that this correction is valid for the column density of target gas above the scattering altitude. While this study targets the column density below the scattering altitude, it is unclear how this correction functions. Please clarify this issue in the manuscript.

The methods presented in this work are based in the fact that all what happens above the station is cancelled. If measurements at IEA = 0° and 90° are not simultaneous, the total AMF for both situation is different, then the optical path through the layers above the station is also different, and so it is not cancelled. Since a delay exists between the measurement taken at 0° and the one taken at 90° due to the time required by the stepping motor to complete the cycle, a correction is required to refer both measurements to the same sza. Factors f and f' account for this effect.

Third, in order for Equation 5 to be valid, we need to assume that the box-AMF (not the differential box-AMF) is constant at altitudes above the scattering altitude. Is this supported by a RT simulation?

Eq. 5 is valid also if the thickness of all layers (Δz) is the same, what is the case of our calculations, and if the effective optical path is close to a straight line above the scattering altitude (see figure 3). Then, AMF= $\Delta s(c1+c2+..+cn)/\Delta z(c1+c2+..+cn) = \Delta s/\Delta z = n\Delta s/n\Delta z = s/H$ in the equation. This point has been further explained in the text.

Additionally, for the calculation of d' with the RT model, a constant concentration has been assumed from the top of the atmosphere to the station level, but this is obviously invalid for O3 and NO2.

In no case a constant concentration with height has been assumed. Only inside each model layer the concentrations has been assumed constant, as usual. Profiles of tracers are taken from the US Standard atmosphere for tropical latitudes (see page 8246, lines 10-11, and Table 2)

In the case of NO2, profiles depending on the sza from SLIMCAT box-model were used to account for the photochemical changes along the day (see page 8248 lines 12-22).

As described in Section 1.3, O3 was retrieved from the 430-500 nm wavelength region. To my knowledge, this region is poor for the O3 retrieval. Please discuss the quality of O3 retrieval using this wavelength region. Specifically, is there any interference with other fitting parameters (DSCDs for other gases or polynomial coefficients)?

The NDACC recommendation for the O3, routine analysis in the Chappuis region is 450 to 550 nm. Our standard analysis covers the 450-520 nm in which O3 can be accurately retrieved. As explained in the text, the 500-520 nm region has been removed to avoid strong misalignment due to the large H_2O absorption in the horizontal path as compared to the zenith one and the well known features due to saturation lines. The extension to shorter wavelengths (toward 430 nm) was decided for NO2 simultaneous evaluation. In summary, the selected range cover the highest NO2 structures in the 430-450 nm range and two well structured O3 absorption bands in the 450-500 nm range. An error paragraph has now been added and further information on detection limits is given. Additionally a plot of the fitting quality has also been included.

3. From Figs. 7 and 8, the MGA values do not agree with in situ values at times, even when both MGA and in situ data are available. This is, of course, independent of the availability of in situ measurements. Indeed, the authors consider only such simultaneous cases, but then argue that the MGA and in situ data are in agreement when considering error bars. What uncertainty do the error bars represent?

The plot intends to show to what degree the "in situ" and MAXDOAS agree each other. Symbols represent the diurnal mean and all error bars in this plot represent the standard deviation. Again we refer to the new error paragraph in which an estimation of the errors is given.

I feel that the differences seen with the common data in Fig. 8 (10-30 ppbv) are too large to support the validity of the MGA method. Considering this and the inadequately-justified assumptions mentioned above, I am skeptical of the validity of the MGA methodology, which the authors are proposing.

We think we have previously clarified the concerns of the reviewer. We do not agree that the assumptions are inadequately justified. The geometrical approximations have been widely used in the literature before us. The FT conditions, with no aerosols (AOD=0.02, typically), simplifies the modelling since the single scattering option provide good results and AMF can be accurately computed. Additionally the instrument is a high quality instrument (see CINDI results), detector-cooled, thermally regulated and a good STN ratio. We strongly believe that the method can be useful for a fast retrieve of tracers in the free troposphere and can be extended to other species such as IO or BrO, avoiding the inconveniences inherent to the LP-DOAS. In the text it is discussed that no full agreement it is expected since airmass scanned is not the same. In fact, one of the strong points of the method is the low sensitivity to "in situ" pollution that during daytime affect the surface (due to upwelling breeze). MAXDOAS measurements are representative to the Free Troposphere and as such it has to be understood. As it is the first paper on the method there are facing behaviours that we don't fully understand, as it is the diurnal variation what seems to be an artefact, and which is under study. We, however, have described in the manuscript what the limitations of the method in the present stage are.

4. I strongly suggest that the English throughout the manuscript be checked by a native speaker.

The English has been now reviewed by a native speaker.

Specific comments and technical corrections

Throughout the manuscript, please use DSCD, not SCD, as a parameter retrieved from MAX-DOAS. Please use either alpha or IEA consistently. We have used alpha for the equations to simplify notation.

(page 8236, lines 9-10): "mixing ratios concentrations" should be "mixing ratios."

It has been corrected in the text.

(page 8236, line 10): Please state the magnitude of errors quantitatively. This sentence has been removed from the text.

(page 8236, lines 10 and 11): "concentrations" and "concentration" should be "mixing ratios" and "mixing ratio," respectively.It has been corrected in the text.

(page 8236, line 17): "Observation" might be a better word here than "Distribution." It has been corrected in the text.

(page 8237, line 2): "know" should be "known." It has been corrected in the text.

(page 8237, lines 27 and 28, and page 8238, line 19): "Honninger" needs an umlaut over the "o".

It has been corrected in the text.

(page 8239, line 21): Perhaps "results mainly of" should be "results mainly from." It has been corrected in the text.

(page 8242, line 5 and other places): "thickness" is a preferable to "width." It has been corrected in the text.

(page 8242, line 6): "If the optical path ... after the scattering is..." should be "If the optical paths ... after the scattering are..." It has been corrected in the text.

(page 8243, line 8): What contribution comes from the station level? Please, refer to Puentedura et al, 2012.

(page 8243, lines 21-23): What is the statistical meaning of the "molecular error"? Is it the 1sigma random error? I do not think "molecular error" is an appropriate term. It is the root mean square error of the fit in molecules. We have changed "molecular error" by "root mean square error (RMSE)". (page 8244, line 22): What is meant by "the dominant synoptic wind exists"? This sentence has been changed in the text.

(page 8245, line 7): What exactly do the authors mean by "single scattering O3 differential box-AMFs"? I do not think that the box-AMFs calculated in this way are only for O3. "single scattering" seems unnecessary.

Figure 5 correspond to differential box-AMFs obtained for O3 considering the single scattering configuration.

(page 8245, line 10): "widht" should be "width", although "thickness" is preferable. It has been corrected in the text.

(page 8245, line 22): "photometer" should be "sun photometer." It has been corrected in the text.

(page 8246, line 17): Why do the authors use forecast data? Are any reanalysis data available?

Forecast is available to the authors. Analysis can also be obtained from the ECMWF but it doesn't worth while since, as stated in the text, AEMET people have made the comparison exercise with radiosondes over the station finding negligible differences in the inversion altitude.

(page 8246, lines 22-24): The authors state, "the relative azimuth was set to zero," although Table 2 indicates a relative azimuth of 180 degrees. Additionally, even if no appreciable difference is observed when the daily solar azimuth variation is considered, I suggest making RT calculations under actual azimuth conditions for each measurement to simplify the discussion.

We appreciate the reviewer for drawing our attention to this point. The instrumental azimuth is 0° but not the relative azimuth. Now it has been corrected in the text. RT calculations have been performed under actual azimuth conditions for O3 and NO2 when standard profile is considered, but no appreciable changes are observed in the results. Nevertheless, new figures have substituted the old figures 7 and 9.

(page 8246, line 25): Perhaps, "de optical path" should be "optical paths"? It has been corrected in the text.

(page 8247, line 4): It is unclear what "the assumed geometry" means? Do the authors mean that AMF-MGA relies more on the RT simulation? This sentence has been removed from the text.

(page 8247, line 5): This states that a similar diurnal evolution is seen for O4-MGA and AMF-MGA. Additionally, both quantities exhibit periodic patterns. It is possible that this is due to inadequate assumptions made in calculating both quantities, such as those I mentioned above (given as specific comments).

Please, refer to specific comments.

(page 8247, line 26): It is unclear what "light period" means. "Sunlight period" has been used instead.

(page 8247, lines 26-28): I do not understand the sentence starting, "As a result of the MBE..."

"As a result, the averaged..." has been used instead.

(page 8248, lines 3-5): I do not think that the O3 value from the ozonesonde at the Izana level agrees well with the estimated MGA values, because it differs by more than 10 ppbv. There is a difference of 10.5 ppbv between in situ and AMF-MGA ozone values. However, there is only a difference of 5.4 ppbv between O_4 -MGA and the ozonesonde. Taking into account that the ECC-O3 sonde relative uncertainity in the tropical troposphere at the level of the station is of 8-10% (Smit 2011) (~8 ppbv on day 208 for 80 ppbv mixing ration at the station level) the agreement is rather good. We have included this information in the text for the sake of clarity.

Smit, H.G., 2011. Quality Assurance and Quality Control for Ozonesonde Measurements in GAW. Technical Report. World Meteorological Organization. Herman G.J. Smit and the Panel for the Assessment of Standard Operating Procedures for Ozonesondes (ASOPOS).

(page 8248, line 9): What is meant by "observed differences"? I believe that MGA values do not agree with in situ values at times, even when both MGA and in situ data are available, as mentioned above.

In lines 5 to 9, we present the non simultaneity of in-situ and MGA results as a possible cause of the "observed differences" between the in-situ and MGA data, even if finally this

possibility is discarded in line 15 after discussion in lines 10 to 15. It is just a way to introduce this discussion.

Table 2: "Wave length" should be "Wavelength." It has been corrected in the table.

Fig. 5: Although the differential box-AMF is small in the upper troposphere and lower stratosphere, the contribution of these altitudes to the DSCD O3 can be significant as the O3 concentration is high there. I suggest checking this by adding a plot of vertical profiles of (O3 concentration) _ (differential box-AMF).

The stratospheric contribution of the measurements is eliminated when we subtract the SCD at IEA=90° from the measurement at 0°.

Fig. 7: The black and gray points are very difficult to distinguish. We have changed the colour in the figure.

Fig. 9: Why are the NO2 values measured with in situ instruments around noon on days 204 and 205 not as large as those on other days? On days 204 and 205, the agreement between the MGA and in situ NO2 values is good, but the differences in O3 are large, compared to other days. Please clarify this in the text.

Concentrations of both species are obtained in the same way. As mentioned in the text, our data have to be seen as the mean concentration within the optical path that is the background concentration in the free troposphere. Not necessarily data have to coincide with surface "in situ". This is quite obvious in NO2, since a narrow layer of MBL air containing large amounts of NO2 slips upslope dramatically increasing the background concentration around noon. The intensity of this effect is meteorological dependent and consequently varies day to day.

Additionally, why does NO2 increase around noon, when O3 does not exhibit a decrease due to MBE? It might help if the authors include the H2O results in this discussion.

As a matter of fact, the observed increase effect in NO2 is always larger than the decrease in O3 (Cuevas et al, 2013 and Volz-Thomas et al, 1993). O3 vertical distribution is almost the same in the MBL and the lower part of the FT whereas there is a clear difference between the two layers in NO2 due to the large populated areas near the coast. We believe, however, that the interpretation of the behaviour of NO2 and O3 in the surface is out of the scope of the present paper.