Reply to referee # 1

The authors would like to thank referee #1 for taking the time to review the manuscript. We are grateful for the comments and suggestions which allowed us to improve the manuscript. We followed their advice, and an additional consistency revision was made to the original text.

In the manuscript tracking the changes, modifications made to comply with referee #1's comments were highlighted in yellow, aside the replies stated in the next tables.

Specific comments:

Nº	Comment	Reply
1	Several statements regarding potential sources of PM_{10} and PM_5 are qualitative (Abstract, lines 34-40). A source apportionment (SA) should have been carried out to achieve quantitative conclusions about major sources impacting the monitoring site. Several manuscript authors have already done so for the closest urban area of La Paz – El Alto (Mardoñez et al, Source apportionment study on particulate air pollution in two high-altitude Bolivian cities: La Paz and El Alto, Atmospheric Chemistry and Physics Discussions, 1–41, https://doi.org/10.5194/acp-2022-780, 2022). A comparison of Chacaltaya SA results with those already published for the neighbor urban area would provide an in-depth quantitative analysis and would enhance the manuscript's scientific value. Most published SA studies present a chemical speciation campaign followed by application of a receptor model.	We have included the source apportionment of the Chacaltaya dataset made with EPA PMF v5.0.14 software. The high-altitude implies that the concentrations are pretty low, quite mixed during transport, and with a low range of concentrations, making variability low and co-linearity high. However, the obtained results are statistically sound, even if single sources were not obtained (except for a classical biomass burning source), in part due to the insufficient number of species to constrain the solution, and in part due to the aforementioned characteristics of high altitude sampling. We have added some sections about SA: section 2.7 in methods, and section 3.2 in results, and an extensive supplementary material. We use the SA results along the text to sustain the discussions.
2	In close connection with the above comment, the discussion in section 3.3 would benefit of presenting SA results beforehand, so seasonality would be discussed in terms of sources rather than by species (that may come from several sources).	SA results were included in the interpretation of section 3.3
3	In Section 3.1, two estimates of OM/OC ratio were used, because of seasonality. Is it possible to estimate that ratio by linear regression of (PM-inorganic mass) against OC? This could be carried out by season to account for such variability. In this way, the uncertainty in OM would be reduced.	This is a valuable suggestion and we will take it into account for future works, but in this case we consider that we do not have enough information to perform a statistically sound regression. Indeed, we only have 5 samples with measured PM-inorganic mass simultaneously to OC. * Please note that section 3.1 became section 3.5 after reorganization of the manuscript.
4	In section 3.2.2 (lines 363-376) it is discussed that OC/EC is ≈ 10 with little seasonality, and this is ascribed to long-range, aged aerosol dominates with a high SOA contribution to OC. I do not understand the hypothesis stated in lines 369-370: why is this hypothesis needed to explain these OC/EC ~ constant results?	The UV influence hypothesis is indeed not needed. For clarity, we have removed it from the paragraph. According to the SA, 29% of OC* has an urban origin. This confirms that the long-range transport dominates the OC* burden, and therefore this may be the reason why OC/EC presents little variability. The urban influence is not defining the seasonality of the OC/EC ratio.
5	Section 3.5: the discussion that ends with Table 6 would have improved with a SA result for Chacaltaya beforehand.	Section 3.5 was removed in agreement with the suggestion of referee # 2, but table 6 was moved to the supplementary material. * Please note that table 6 is table S7 in the revised version.
6	Conclusion section: I think there are contradictory statements here. First, on lines 630-631, it is mentioned that "La Paz and El Alto … activities… affect the aerosol chemical composition (at Chacaltaya) with EC, NO_3 … as traffic	We agree with the referee. The three paragraphs mentioned here were modified for clarity. In the revised manuscript, the aforementioned modified lines are 344-351 and 626-635.

	indicators… ". Then, in lines 636-637 it is stated that "OC/EC ratio … does not have a marked seasonality … likely due the permanent influence of long-range transport". However, OC is also emitted by traffic, and it is mentioned that OC/EC ratios for La Paz – El Alto range between 2 – 3.5 (approx.). Then, I do not understand why EC from La Paz -El Alto would impact Chacaltaya but not OC emitted from the very same area — given that in lines 369-379 the authors hypothesized that "… the high UV of the tropical atmosphere over the Altiplano could play a role in the impressively fast aging of the organic matter at this site when transported from the nearby urban area." This issue needs to be clarified.	

Technical corrections:

Nº	**Technical correction**	**Reply**
1	I think figure S12 should be referred to instead of S10 (line 122).	Corrected
2	In Section 3.3, Figure 5 is hard to visualize. I would recommend splitting it in several graphs, perhaps moving some to supplementary information.	We have split figure 5 in three, corresponding now to figures 5, 6 and 7.
3	Since this is not the first report about Chacaltaya measurements, sections 2.1 and 2.2 could be shortened by moving some paragraphs to Supplementary Information.	Section 2.1 was shortened, but 2.2 was not easy to shorten as it needs to explain the complexity of the sampling at this site.