Answer to the referee
Higher absorption enhancement of black carbon in summer shown by two year measurements at the high-altitude mountain site of Pic du Midi Observatory in the French Pyrenees
(Preprint egusphere-2023-570)

Please find below the reviewer's comments in black and our responses in blue. The line numbers in the responses refer to the new version of the paper.

I am glad that Tinorua and co-authors incorporated some of my previous suggestions. In some parts, the manuscript's quality and readability improved since the first submission. However, this version still suffers from very similar flaws compared to the first draft. The presentation of the results is chaotic since there is little cohesion between the text and the figures. I notice an overall superficiality in justifying technical choices, presenting the data and interpreting the results. Finally, the results are not clearly summarized in the conclusion section, which is a very confusing mix of results, references and speculations. Considering the number of specific comments that can be found as follows (editing, grammar, nomenclature, sequence of figures, etc…), I have the impression that the manuscript was not carefully controlled before resubmission. Despite the high interest in the dataset, I cannot recommend the publication of the manuscript in ACP. I leave the choice of full rejection or resubmission after major changes to the editor, waiting for the pending reviews from other referees.

We thank the reviewer for his deep work of revision. A large part of the comments deal with the SP2 data processing to retrieve the rBC mass concentration. We acknowledge that the PSI SP2 toolkit is the most common tool used to process SP2 data. However, for the purposes of our study, a Python-based treatment process was developed, in order to meet our specific needs (processing very large quantities of data, transparency of data processing, using Linux,...). Other studies like Taylor et al. (2015) have also developed their own data processing. Although it would be interesting to make an inter-comparison of the different methods, this is not the aim of this paper.
Before the second submission, the paper was reviewed by all the co-authors and we did our best to provide the most accurate version possible.

MAJOR COMMENTS

MULTIPLE SCATTERING CORRECTION
The C value was changed from 3.63 to 3.22 according to Yus-Díez et al. (2021). Considering the importance of this change and the direct effect on absorption coefficient and MAC, the choice should be explained better, and should not be based, solely, on the fact that Montsec d'Ares is a mountaintop site 200 km away from PDM. More specific comments:
- In the main text of Yus-Díez et al. (2021), I could find values between 2.51 and 2.36 for Montsec d'Ares. These values are calculated using a different type of filter and normalized against the MAAP at one single wavelength.
- Finding the value 3.22 in Yus-Diez requires some work since is available only in a table in the supplementary.
- In Yus-Diez, the value of 4.05 is specific for 880 nm and is retrieved by comparing the Aethalometer with an offline polar photometer, which is not commercially available nor widely diffused.
- 3.22 is a second factor obtained using the offline polar photometer working as a MAAP. Since no detailed information is provided in the supplementary of Yus-Díez et al. (2021), I believe that 3.22

is calculated normalizing the absorption of the offline polar photometer to the absorption of the MAAP.

- The use of a wavelength-dependent C, is justified in this specific case since SSA values are quite high at PDM. In this SSA region, C values might drastically increase.

Unfortunately, none of these points are addressed in the manuscript nor in the supplementary. So, the authors are required to describe and justify more in detail their choices. This comment should be taken very seriously since the same problem was observed in the first round of reviews.

REPLY: -The instrumental set-up did not allow us to determine the value of C, as we would have had to deploy another absorption-measuring instrument. We therefore used a C value obtained from the literature, as is done in the large majority of publications using aethalometer data. We chose a value obtained close to PDM (less than 200 km), on a similar type of site (remote mountain site) and with SSA values close to those observed at PDM. As highlighted by the reviewer, the high SSA values measured at the PDM supports the use of a C value adapted for 880 nm, which can be found in the supplementary material of Yus Diez et al. (2021). The precise information on where to find this information in Yuz-Diez et al. (2021) has been added in lines 146-149:

"The multiple scattering parameter used to correct the measured attenuation was set to 3.22, according to the value obtained at λ=880 nm by Yus-Diez et al. (2021) at the mountainous site of Montsec d'Ares located less than 200 km from the PDM (see Table S3 in the Supplements of Yus-Diez et al. 2021)."

- If a wavelength-dependent C is used at 880 nm, I imagine that the σap used to calculate SSA at 635, 525 and 450 nm are corrected with a wavelength-dependent C. Same applies to AAE and SAE.

- First submission: The mean σap,880 was 0.27 Mm−1 (L220). Second submission: The mean σap,880 was 0.27 Mm−1 (L267). So, are the data corrected for a C value of 3.22 or not?

L267: Since C was decreased from 3.63 to 3.22. I expect a value of σap higher than the previous submission (also 0.27 Mm-1). Please verify your numbers.

REPLY: We noticed a mistake on the C value written in the fist submission. In facts, the C value applied in the first and the second submission was the same value of 3.22. This is why the $\sigma_{abs,880}$ and SSA values did not change between the first and the second submission. Both AAE and SAE are independent of C. AAE is calculated as an absorption ratio, and there is no absorption in SAE calculation.

NOMENCLATURE

Work needs to be done to harmonise nomenclature:

- Although this might be interpreted as a single little mistake, it is an irritating one. The authors use "refractive BC" instead of "refractory BC" in the full text.

- Harmonise the nomenclature for scattering and absorption…. σsca -σabs or σsp -σap

REPLY: We apologize for these nomenclature mistakes which have been corrected. We changed σ_{ap} to σ_{abs}.

BANDRATIO

In my previous review, I mentioned that colour ratio might be used (potentially) to distinguish direct biomass-burning events. Although the analysis of colour ratio usually provides very noisy results, which are hard to interpret, it could be used to identify the potential influence of BB (Schwarz et al., 2006; Dahlkötter, 2014). So, the author's statement "the color ratio only provides information on the presence of dustparticles" is fundamentally wrong. In the revised manuscript I did not expect to see a full colour-ratio analysis over two years, but, at least, a better justification on why it was not

REPLY: In their study, Schwarz et al., (2006) used the scattered/ incandescence peak light intensity ratio in order to characterize the rBC mixing state. However, the color ratio – defined as the broad-band incandescence signal over the narrow-band incandescence signal – has not been used to analyze the rBC emission source or composition. Dahlkotter et al (2014) found different color ratios between rBC from a forest fire plume and rBC from a Creek fire plume. Nevertheless, as shown by (Moteki et al., 2010) the color ratio is also a function of the size and the shape of rBC particles. Thus, from our point of view the analyses of the color ratio for a proper source apportionment is delicate and less documented than the $\Delta M_{rBC}/\Delta CO$ ratio analysis.

PYTHON CODE

The SP2 community urgently needs open-source software to treat the data.

REPLY: We fully agree with the reviewer that there is a strong need of a SP2 data processing tool compatible with all operating systems. Our goal was to develop a Python program because the PSI SP2 toolkit was not suitable for our study, given the large volume of data to be processed and the compatibility with LINUX, which is used at Météo-France.

So, the authors should consider a more careful evaluation of the two codes. In this regard, I have some more comments:

- As written Text S1 suggest that the PSI toolkit is wrong while the python code is right. Indeed, the IGOR code has its limits, but it has been around for more than a decade, so I would be careful with some statements.
- When addressing the differences between the two codes, the authors should be able to properly identify the causes. Without a proper evaluation/comparison of the two codes, the concluding statements could also be rewritten as: “The Python code might be less sensitive than the PSI toolkit due to a different selection of valid individual signals. …”
- “These different possibilities have not been explored in detail”. To be truly honest, this must have been done before the submission of the current manuscript. From my point of view, this is a crucial mistake from the authors.

REPLY: - We agree with the reviewer that a more robust and deeper work on the biases between all the SP2 toolkits – not only the PSI – should be done. However, this work requires additional analysis and statistical tools which are out of the scope of this paper.

- We changed the end of the Text S1 by using more nuanced arguments :
“The values provided by the PSI toolkit may be more noisy than the Python software due to different filters applied to the individual signals, a difference in the flowrate sampling, or a different estimation of the baseline of the incandescence peak height, leading to biases in individual masses.”

- The mass difference can be mostly attributed to the calibration curve. I would be very curious to see a comparison between number concentration.

REPLY: We have quantified the part of the uncertainties due to the calibration curve (see Figure 1). By considering the average mass size distribution of the campaign, a difference of 8 % on the total M_{rBC} was found between the two calibration curves. The bias in Figure S1 seems to be systematic and may be partly due to the calibration curve. However it does not explains the different amplitudes of M_{BC} variations, which may be related to the flow rate sampling or the baseline calculation.

- If the igor code counts invalid signal as real particles, shouldn’t the toolkit concentration be higher? Figure S1 shows the opposite.
- According to the values provided in the supplementary, analysing the same dataset with the IGOR toolkit would lead to a 20% lower MrBC and to a 20% higher MAC. I would not consider this difference to be negligible. If we consider that C values decreased from 3.63 to 3.22 (absorption increase of roughly 10%), the software and constant choices will introduce a MAC uncertainty of 30%.

REPLY: The PSI SP2 toolkit exhibits both positive and negative biases compared to our data treatment, leading to an overall lower mean M_{rBC}..

- As explained in the previous answers, the $\sigma_{abs,880}$ did not change between the first and the second submissions because the C values applied are the same.

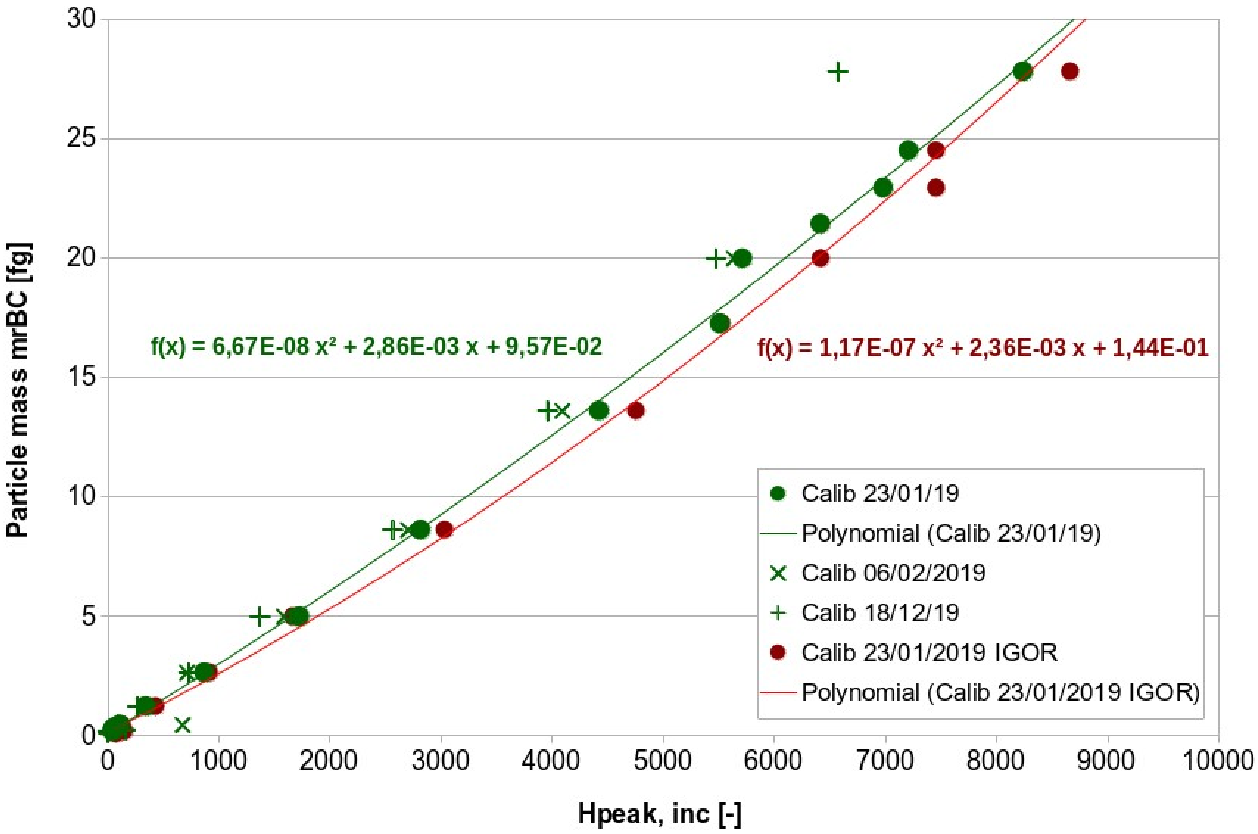

Figure 1: Calibration curve of the rBC mass concentration data processing using the PSI SP2 toolkit (in red) or the Python program of this study (in green).

ABSORPTION ENHANCEMENT AND MAC

As already raised in the first review, I have my doubts about the relevance of Eabs, as calculated and treated here. It is worth mentioning absorption enhancement if the available data allow quantifying the mixing state of BC…since the lensing effect is a direct consequence of coating formation. Without data on coating thickness, no real optical closure could be presented. I thus suggest the authors to: 1) focus on MAC variability in the full paper rather than EABS; 2) dedicate a very short paragraph, listing all possible uncertainty (RI in primis), to the overall Eabs that might characterize on average PDM.

REPLY: Several studies cited in the paper in line 210 calculated E_{abs} using a $MAC_{bare,rBC}$ value estimated from the Mie theory. The text from line 211 to line 219 already discusses the impact of the morphology assumption on $MAC_{bare,rBC}$ and the relevance of using Mie theory in its calculation. In the section 3.3.4 lines 390-392, the calculated $MAC_{bare,rBC}$ value are comparable with the values obtained in the literature and summarized by Liu et al. (2020).

We added a text in lines 215-218 to discuss the refractive index impact on $MAC_{bare,rBC}$:

"In addition to the morphology, the $MAC_{bare,rBC}$ calculation is also very sensitive to the refractive index of rBC core (Sorensen et al., 2018). Liu et al. (2020) summarized the changes in MAC values induced by the use of different refractive indexes. They reported deviations from -7 % to -35 % to the MAC_{BC} value of 7.5 m^2 g^{-1} recommended by Bond and Bergström (2006)."

SUMMARY AND IMPLICATIONS FOR CLIMATE MODELS

As already mentioned in the first round of reviews, the conclusions are too speculative. From my point of view, the new section worsened compared to the first submission. I strongly advise the authors to:

- Strictly and precisely describe their conclusive results. As it is, it is extremely hard to separate the results of this paper from previous works.
- Avoid any long discussions on global modelling and related parametrization of ageing, scavenging and absorption.

REPLY: The paragraph about implications for climate models was added following a suggestion of an other reviewer (RC #3 of the first round :"Lines 420-427: To me these are the most important lines in the manuscript – the implications of your findings. I am a little disappointed that this is relegated to one brief paragraph and that your major conclusion re: wet scavenging is not as thoroughly assessed in the paper as is likely warranted given the conclusion. I would like to see the implications section for climate models more rigorously discussed.")

UNITS

Many figures feature an unusual notation for units. As an example, "ng.m-3". From my experience, the use of a dot as a unit separator is unusual. None of the recently published ACP manuscripts presents this type of notation.

REPLY: The notation of the units has been changed.

SPECIFIC COMMENTS

L54: explain what Eabs is

REPLY: A description of E_{abs} is already provided in lines 53-54:

"Numerous studies have demonstrated that coating of BC with non-absorbing materials is accompanied by an enhancement of light absorption (E_{abs}) through the so-called lensing effect "

L54-56: please, try to avoid such a long listing of references. My former supervisor would call this "lazy bibliography work". Try to identify the works most pertinent works needed to send your message and help the reader identify who did what.

REPLY: We mentioned many references to highlight the large number of studies.

L71-76: This description might fit better in Section 2.1

REPLY: This part has been moved to the Section 2.1.

L77: remove "in the indicated sections"

REPLY: This has been removed.

L92-93: Is this campaign called "Hygroscopic properties of black carbon"? Is this important information? If yes, please mention it in the abstract or introduction.

REPLY: The campaign is called h-BC for hygroscopic properties of black carbon. Event though no hygroscopic properties are presented is this paper, data from this paper were collected during this campaign.

The campaign has been mentioned in the introduction in line 70-71 as follows:

"This study presents two-year continuous measurements of BC and aerosol properties conducted during the Hygroscopic properties of Black Carbon (h-BC) campaign at the high-altitude long-term monitoring station Pic du Midi (PDM)."

L100: sampled air
REPLY: This has been modified.

L101: inside the room or in the inlet?
REPLY: The air was heated to 20 °C inside the inlet and maintained at this temperature inside the room.

L114 give a reference for the density.
REPLY: We added the reference of Moteki and Kondo (2010).

L123-125: Here I have the same question as in the first round of comments. Mode1 is extrapolated from the Sp2 measurements in the 90-100 nm range? If this is true, is it reasonable to fit a lognormal curve on 10 nm?
REPLY: As explained in the paper in lines 123-134, the rBC size distribution measured between 90 and 580 nm has been fitted with a sum of three modes, in order to minimize the differences between the measurements and the fit (see Fig. S2 a). The first mode was constrained between 50 and 100 nm, because these limits allow the representation of the first mode peaking at around 130 nm in the sum of the three modes.
A study on the impact of the fitting procedure – and in particular the representation of the rBC particles under 90 nm of diameter - on M_{rBC} is actually in preparation for submission in AMT journal.

L142: …,880,950 nm. Be consistent with line 137.
REPLY: This has been modified.

L217: The histogram in Figure S6 shows the dominance of periods with RH above 90%. I am wondering how many days have been removed from the two years period.
REPLY: We filtered the hours when relative humidity was above 95 %, which represented 24 % of the total hours in the campaign.

L253: please use the sectors indicated above.
REPLY: This has been modified.

L264-284: There is incongruency between the sequence of properties discussed in the text and presented in figure 3. If I am not wrong, panels b and c are not discussed here. So, to be consistent, ¬ap should be Figure 3b and ¬sp should be Figure 3c.
F3: I would not fill the gaps between march and august 2020 for panel a, b and e. SAE is shortly discussed and AAE is not mentioned in the text. I guess they can both be removed from figure 3, since they are included in Figure 4 and discussed after. It is particularly not nice to see the wavelengths in legend not in decreasing or incresing order 635-450-525, please correct.
REPLY: The order of the panels has been changed in Figure 3, as well as the legends in the SSA panel. The line between March and August has been removed.
The AAE panel has been removed in line with the reviewer comment. The SAE panel has been kept in the paper since its temporal variations are discussed in lines 275 and 280.

L279-280: Remove the sentence about BC. When speaking of BC you can recall absorption. Try to keep a linear sequence of topics and figures.
REPLY: The sentence about BC has been removed.

L285-309 and F4: I have the impression that the graph does not provide a clear “speciation” of the aerosol optical properties. If the authors want to draw some evident conclusions from this analysis, the time resolution of the AAE, SAE and SSA should be decreased to at least 1 day. Alternatively, the Cappa method could be applied to summer-winter (on daily resolution)9, wet-dry and BL-FT cases (on hourly resolution)

REPLY: As shown in section 3.1, PDM is influenced by air masses from various locations and by dynamic and chemical processes on fine time scales. As a result, the aerosol optical properties at PDM cant be very different from one day to another. The hourly variations of E_{abs} presented in section 3.4 and Fig. 7 suggest that a time resolution of one hour is appropriate to study the aerosol optical properties at the PDM. So we believe that an hourly time resolution for the classification of the dominant aerosol type is more appropriate.

S3.3: Maybe this is a problem of my PDF reader, but the numbering of subsections is missing, as in the first submission

REPLY: The subsections have been numbered.

F6: since the concentration is normalized, only one colour scale is needed

REPLY: Figure 6 has been edited.

L399-400: the diurnal variability of Eabs is poorly described. As it is, it does not provide crucial information and can be easily removed. Nonetheless, it might reflect, in terms of MrBC or BC/CO the daily cycle of BL and FT, as also shown by the authors in the reply to my first review. Excluding spring and summer, the diurnal analysis could be modified and used to introduce section 3.4.2. Clearly, this change will require some rethinking and additional work.

REPLY: Figure 7 was added to the paper to highlight the seasonal variability of E_{abs} and in particular the opposite diurnal pattern between winter and summer. It is these diurnal and seasonal variations in E_{abs} shown in the figure that motivated the approach chosen in section 3,4 to assess the factors influencing these variations.

The paragraph in lines 398-403 has been reorganized as follows:

"Figure 7 further shows the diurnal variation of E_{abs} for each seasons. There was a notable opposite diurnal profile between seasons in E_{abs} with midday showing a minimum around 1.7 in winter, and a maximum around 2.9 in summer. Spring and autumn showed intermediate patterns with less regular E_{abs} throughout the day. These observations suggest that different sources and/or processes drove the seasonal contrast in rBC properties. The following section aims at investigating potential drivers of E_{abs} variations, including rBC wet scavenging, dominant rBC sources and transport pathways. Particular attention will be paid to winter and summer because these seasons differ greatly, whereas spring and autumn behaviors appear intermediate"

L413-414: I am glad that the authors implemented my comments. However, I think that the statement here is not exact. The goal of removing BL periods is to decrease the influence of air masses with different BC/CO ratios caused by different sources.

REPLY: We thank the reviewer for his suggestion. We changed the sentence in line with the reviewer's suggestion as follows in lines 411-412 :

"In order to decrease the influence of the difference sources on $\Delta M_{rBC}/\Delta CO$ compared to the effect of wet scavenging, periods for which the site was under PBL influence were filtered."

L415: Looking at Figure 8a, a value of 2.1 ng m−3 ppbv−1 is associated with precipitation-free back trajectories. In the second round of reviews, this sort of mistake should be avoided.

REPLY: We corrected this mistake and thank the reviewer for its awareness.

L418: please remind the readers that measurements that occurred at RH above 90% are removed.

REPLY: A sentence has been added in line 416-417:

"As a reminder, time periods when PDM was under precipitations or humidity > 95 % have been filtered before the analysis."

L425-429:It would be nice to provide some numbers here. Otherwise is hard to compare with the studies cited (provide some numbers here too, please)

REPLY: The sentences in lines 429-437 have been modified and some numbers have been added: "However no significant change on the mean rBC core diameter was noticed between wet and dry conditions (mean $D_{rBC,core}$ of 177 and 182 nm, respectively), as well as in the presence of precipitations during the transport of rBC or not (mean $D_{rBC,core}$ of 177 and 182 nm, respectively).

This result contrasts with previous studies showing a decrease in rBC size due to wet scavenging (Kondo et al., 2016; Moteki et al., 2012; Taylor et al., 2014; Liu et al., 2020a). For example, Kondo et al. (2016) found a change in $D_{rBC,core}$ between 13 and 20 nm depending on the season, while Liu et al. (2020) and Moteki et al. (2012) measured rBC cores ~ 32 nm lower in air masses affected by wet removal. The insignificant effect of wet scavenging on the modal diameter of rBC core size distribution could be explained by the size of rBC core sampled at PDM that was higher than the one described in these studies."

L434-441: The authors are considering only nucleation scavenging. Rightfully, fresh BC particles are hydrophobic, thus non-cloud-active. However, fresh and aged BC particles could be removed by wet scavenging below the cloud by impaction scavenging or inside the cloud by interstitial scavenging too. So, I would not indulge in a long discussion about supersaturation, when there might have been various competing removal mechanisms.

REPLY: Nucleation scavenging was found to be the most efficient process in the rBC removal (Jacobson, 2012).

Interstitial scavenging affects mostly particles smaller than 100 nm (Pierce et al., 2015), which have a very low presence at PDM (rBC mean diameter of 180 nm). Thus, we think it is important to focus on the impact of rBC nucleation scavenging on E_{abs}. Following the relevant reviewer's remark, a sentence has been added to include the impaction process in the discussion in lines 442-445:

"Furthermore, the rBC wet removal by impaction is also a size-dependent process which could has been responsible for the removal of small rBC particles (Croft et al., 2010). However interstitial scavenging affects mostly particles smaller than 100 nm (Pierce et al., 2015), which have a very low presence at PDM (rBC mean diameter of 180 nm)"

F8: Eabs is not discussed in the text. And I agree with this choice. But, why it is still shown in the figure? I would remove panel c and d and potentially replace it with Drbc, see following comment.

REPLY: E_{abs} was discussed in the text in lines 423-424: "Figures 8 c-d show in contrast little influence of precipitation and RH on the rBC absorption enhancement, with a constant median E_{abs} value of around ∼ 2.1."

F9: there is no need for two panels, show the absolute or normalized concentration. It is unclear to me how the RH lines are defined…RH>85% during precipitation period and RH<85% in no precipitation period? Potentially, Figure 9 could be merged with figure 8 removing the Eabs panels. Why it is "ng.m-3"? Remove this omnipresent point from units.

REPLY: The legend in Fig. 9 has been modified. The RH criteria is based on RH measurements at the PDM while precipitation criteria is based on the presence of precipitation along the HYSPLIT back-trajectories. Normalized plot has been deleted. The unit has been corrected.

L444: check the subscript for ΔmrBC/ΔCO

REPLY: This has been corrected.

L444-445: I already expressed my doubt on ΔMrBC calculated as MrBC. Figure 10a clearly shows that there is substantial variability in MrBC values depending on BL conditions. Especially in BL

conditions. So, I still think that ΔMrBC should be calculated as the difference between the background concentration and the current concentration. In any case, as already mentioned, if you decide to keep the current calculation, the ratio must be called MrBC/ ΔCO. Otherwise, this is misleading. Moreover, I cannot find the number associated with the results shown in Figure 10c.
REPLY: : Most studies have calculated ΔM_{rBC} as M_{rBC} and kept the Δ in the notation (Choi et al., 2020; Kanaya et al., 2016; Kondo et al., 2016; Pani et al., 2019). They justified their choice by the shorter lifetime of rBC in the atmosphere – several days - compared to the CO lifetime – a few months (Bey et al., 2001; Park et al., 2005). Although we understand the reviewer's point of view, we decided to use the same notation as in the literature.

The sentence in lines 462-465 has been modified according to the new numbering of Figure 10 and numbers have been added :
"The higher ΔM_{rBC} /ΔCO in PBL conditions (2.5 ± 2.3 ng m^{-3} $ppbv^{-1}$ for the mean ± STD) than in FT conditions (0.6 ± 0.1 ng m^{-3} $ppbv^{-1}$ for the mean ± STD) may indicate additional sources from biomass combustion from the valley (Fig. 10b), which could be attributed to either residential wood heating or stubble-burning that is still a common practice in the Pyrenees (González-Olabarria et al., 2015)."

L446-448: No values given for Eabs. Eabs should be shown in panel c and not b. Please try to maintain the same sequence in the text and in the figures/panels.
REPLY: Some values in lines 467-468 has been provided as follows :
"Figure 10c shows that PBL conditions were associated with lower E_{abs} values (1.5 ± 0.3 for the mean ± STD) than FT conditions (1.9 ± 0.4 for the mean ± STD). "

The numbering in Figure 10 has been corrected.

L449-450: Please reformulate the sentence reporting the mean or median concentration….as written, it is weird
REPLY: The sentence in lines 456-458 has been modified as follows:
"In winter, we measured higher M_{rBC} values and variability in PBL conditions (39.5, 30.0 and 105 ng m^{-3} for the median, 25th, and 75th percentiles, respectively) than in FT conditions (33.5, 10.4 and 45.4 ng m^{-3} for the median, 25^{th}, and 75^{th} percentiles, respectively) (Fig. 10a)."

L452-454: unclear, please rephrase.
REPLY: The sentence in lines 460-461 has been modified as follows:
"During the night, pollution from the surface is trapped by the low height of the PBL and cannot reach the PDM. At the same time, the cleaner air transported in the FT may contribute to the dilution of M_{rBC} at the PDM."

L459-460: Again, no values are provided.
REPLY: Some values has been added in lines 468-470 as follows:
"Surprisingly, the M_{rBC} did not vary between BL and FT influence with values of 75.4 ± 33.2 (Mean ± STD) ng m^{-3} and 80.2 ± 46.6 ng m^{-3} respectively, meaning that the thermally driven PBL injection did not significantly impact M_{rBC} measured at PDM (Figure 10d)

L468-478: rBC loading and BC/OC are shown in Fig 10d and f not d and e
REPLY: This has been corrected according to the new numbering.

L479-491: I still have doubts about this subsection. First, I do not find a clear reason explaining the variability of eabs. Second, Section 3.4.2 is supposed to discuss FT/BL dynamics, which is not treated in this part of the text and in Figure 11. In my opinion, all these parts should be removed.
REPLY: We have shown in section 3.4.2 that in summer, the influence of FT/BL does not explain

the E_{abs} variability, and the diurnal aerosol size distribution shows a predominant transport in the FT in summer. Thus, an analysis on $\Delta M_{rBC}/\Delta CO$ ratios has been conducted to investigate the dominant rBC sources transported in the FT, which could explain the E_{abs} variability. Thus, the paragraph in lines 489-501 and Figure 11 aims to provide further explanations on processes playing a role in the high E_{abs} values in summer.

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