

ESSD-2026-211

Answers of the authors to Reviewer#1

While the reviewer's comments are given in **black bold**, our answers are given below in [blue letters](#). Additionally, we added the changes made in the revised manuscript in [blue italic letters](#).

This kind of work we needed for policy makers: Characterization and applications of multi-decadal black carbon observations at a Mediterranean mountain site” is fundamentally about turning a rare, high-quality, long-term BC record into a reference dataset for trends, process understanding, and model evaluation in a sensitive regional climate system. This makes this work more specific. I am accepting this work with some major comments, which to be addressed and incorporated in the revised version.

[We thank the reviewer for raising several interesting points for discussion. Hereafter, the authors provide a point-by-point response to the reviewer's comments.](#)

Abstract:

Conceptual and novelty comments on Abstract

1. Novelty and positioning need one explicit sentence.

Authors state that the dataset is “one of the longest continuous eBC records in the Mediterranean troposphere,” but you never clearly articulate what this enables that was not possible before. For a broader reader, it would help to say explicitly what new capability this record unlocks (let me make it more specific e.g., first multi-decadal BC trend at a Mediterranean free-tropospheric site with fully documented uncertainty).

2. *Feedback of climate change* is vague. The phrase ‘understanding the potential feedback of climate change’ is imprecise. Do you mean BC–snow/ice albedo feedback, BC–circulation interactions, or changes in transport patterns affecting BC? Either specify or drop ‘feedback’ and say ‘impacts’ or ‘responses.’

3. Much of the abstract emphasises infrastructure and uncertainty, which is excellent for a data paper. However, the ‘Applications’ part is condensed and somewhat generic. You might consider whether one of the applications (e.g., the eBC trend or PBL coupling) can be framed more sharply as a key scientific result.

[We thank the reviewer for the suggestions. We revised the abstract to underline the novelty and scientific applicability of the dataset more, while retaining its original structure and emphasis on data quality, long-term documentation, and accessibility. In particular, we clarified that the long temporal coverage and high temporal resolution enable applications across different temporal scales. We also removed the potentially ambiguous reference to “climate-change feedback”.](#)

[Now the abstract reads:](#)

[... “Long-term observations of black carbon \(BC\) are essential for assessing the effectiveness of air-quality and climate policies, particularly in relation to human health and radiative forcing, respectively. We present an 18-year dataset \(2007–2024\) of equivalent black carbon \(eBC\) mass concentration derived from the aerosol absorption coefficient measured at the Monte Cimone WMO/GAW Global Station \(CMN, 2165 m a.s.l.\), a key high-altitude observatory in the Mediterranean climate-change hotspot. The dataset is accompanied by a detailed description of the historical evolution of the infrastructure and measurement procedures, together with a comprehensive assessment of the quality-assurance protocols and associated uncertainties. This documentation provides a comprehensive description and quantification of the data reliability over 18 years of measurements, necessary for the scientific valorisation of the dataset.](#)

[Applications of the dataset demonstrate: i\) a statistically significant decline in eBC concentrations over the last 18 years ii\) a strong linkage between eBC variability and boundary-layer dynamics when combined with ERA5 reanalysis; iii\) the potential to evaluate FLEXPART and other model simulations across multiple temporal scales, from short-term variability to long-term changes. The dataset, openly available through the ITINERIS HUB, provides one of the longest continuous in-situ eBC records from a high-altitude site in](#)

the Mediterranean region and enables investigation of eBC variability from short-term atmospheric processes to long-term changes, representing a valuable resource for climate studies, trend assessments, and multi-station integration within ACTRIS and GAW.” ...

Technical and small suggestions on Abstract

- **Two decades vs 18-year dataset.**

Authors are mentioning an 18-year dataset (2007–2024), but then speak of a ‘statistically significant decline ... over the last two decades.’ Strictly speaking, your observation period is under 20 years. Either justify ‘two decades’ (e.g., 2005–2024 with earlier ancillary data) or rephrase to ‘over the last 18 years’ or ‘since 2007.’

Corrected as suggested.

- **Mediterranean troposphere.** ‘One of the longest continuous eBC records in the Mediterranean troposphere’ is a strong claim. If Authors mean ‘Mediterranean free troposphere at high altitude’ or central Mediterranean region, it might be safer to soften that, unless you explicitly benchmark against all other Mediterranean BC time series in the paper.

We agree that this statement should be supported more explicitly, but not in the abstract. The 2007–2024 CMN record represents one of the longest continuous in-situ eBC, or absorption, record available for the Mediterranean troposphere and more in general Europe (Collaud Coen et al., 2020). Among older records we may find Manua Loa (1998) and Jungfraujoch (2002) and few more. Nonetheless, we understand the point made by the reviewer:

1. We have retained the statement in the abstract but clarified its geographical and observational context:

... “one of the longest continuous in-situ eBC records from a high-altitude site in the Mediterranean region” ...

2. We added another sentence in section 5.1:

... “Based on the work of Collaud Coen et al. (2020b), the current dataset represents among the longest uninterrupted record of eBC, derived from aerosol absorption coefficient, in European background sites. ”...

- **Variable agreement with FLEXPART is too vague.** As written, ‘variable agreement across temporal scales’ could be interpreted as anything from ‘acceptable’ to ‘poor.’ Consider clarifying whether this means, for example, ‘good agreement for seasonal and inter-annual variability but weaker at daily scales,’ or similar. So authors can rephrase it for better clarity.

We want to point out the comparison possibility rather than the results of the comparison. The statement was changed as :

... “the potential to evaluate FLEXPART simulations across multiple temporal scales, from short-term variability to long-term changes.”

- **List structured of presenting in the abstract.** Authors used “(i), ii), iii)” twice. It reads clearly but feels a bit heavy. Author might compress the first list (infrastructure, and uncertainties) into a single sentence to save words and give more room to applications part. This will make abstract more precise and beautiful.

We have condensed the first list into a single sentence, retaining the structured list only for the main applications and results of the dataset.

Introduction:

1. Conceptual framing.

‘BC is the sole aerosol particle with a positive effective radiative forcing’ This is too absolute. Current assessments attribute net positive effective radiative forcing to BC, while many other aerosol species (sulfate, nitrate, organic aerosol) are net cooling; however, some near-source absorbing organic aerosol (brown carbon) also contributes to warming, and IPCC language is more nuanced. Author can use this one instead: ‘BC is the dominant aerosol species with a positive effective radiative forcing.’

The sentence now reads:

... “BC is the sole aerosol particle with a net positive effective radiative forcing” ...

‘major responsible of health issues’The phrasing is awkward and over-general. PM as a whole is major; BC is a key component. I would suggest it in this fashion: **‘BC is a key component of particulate matter linked to adverse health outcomes.’**

Combining the suggestions of both reviewers, the sentence now reads:

... *“Although particulate matter remains the main aerosol metric used for air-quality assessment in Europe (e.g. Pisoni et al., 2025), BC is carrier of toxic and carcinogenic substances (Ali et al., 2021) being a key contributor of health issues. Hence, the recent European Union Ambient Air Quality Directive (Directive (EU) 2024/2881) identifies black carbon as a pollutant of emerging concern and includes its measurement at monitoring supersites..”* ...

‘Global BC trend’. The paragraph correctly notes that BC has generally decreased, while other species show mixed trends regionally. This is good context, but you might explicitly state why BC in the Mediterranean is particularly interesting (climate hotspot, mixture of European, North African, and shipping sources).

We added a new paragraph underlying the complexity of the Mediterranean region:

... *“In the Mediterranean region, these changes are particularly relevant because of its position at the interface between decreasing European (Savadkoohi et al., 2023) and substantial North African BC emissions (Lioussé et al., 2014), resulting in a complex mixture of aerosol particles with distinct sources and properties. This complexity is further influenced by ongoing climate change, which is expected to modify aerosol emissions, transport and atmospheric processing across the region (Drugé et al., 2021; Granakis et al., 2026).”* ...

1. **‘climate change feedback’** As in the abstract, **‘feedback’** is vague here. If Authors don’t analyse feedback mechanisms explicitly, better to say **‘climate impacts’** or **‘climate responses.’**

Changed to “impacts”.

1. **Normal** The first three sentences are strong, but the introduction then moves quickly into ‘total aerosol’ and general aerosol processes (circulation, precipitation, heat waves, economic growth). These are relevant, yet they dilute the focus on BC and Mediterranean mountain context. Authors can add this statement for better and comprehensive **‘deviations from common trends reflect interactions of emissions and meteorology.’**

Unfortunately, we do not understand exactly what part of the text has to be modified.

Then moving faster to the role of ACTRIS/GAW and why a mountain GAW site is strategic. The ACTRIS and GAW paragraph is excellent, but you could more explicitly position CMN as part of these global networks and one of the few long-term BC sites in the Mediterranean.

We modified the description of the site and contribution to ACTRIS-RI observations in the introduction. The description was kept short to avoid repetition with Section 2.

... *“The current work presents in-situ and long-term observations of eBC acquired at the Monte Cimone WMO/GAW Global Station (CMN, 2165 m a.s.l.), which is a long-term contributor to ACTRIS-RI observations in the Mediterranean basin.”* ...

Site and dataset description

1. **Author clearly specify eBC, the MAAP, and reference Petzold et al. (2013). That’s appropriate and essential for a data paper.**

Thanks

Author mentioned ‘CAMs and FLEXPART’ only once at the end. Since they are central to your applications, consider one more clause indicating what kind of model–data questions you address (e.g., transport pathways, background levels, seasonal cycles).

First were removed “CAMS”, since we did not show any comparison in the present manuscript. A closing statement was added at the end of Section 5.3:

... *“Additional FLEXPART products can be integrated with the eBC dataset to investigate long-term changes in BC source regions and source sectors, and their relationship with evolving emission patterns, including those associated with emission-reduction policies and climate change.”* ...

1. Mediterranean troposphere vs Mediterranean mountain site

We are not sure what the reviewer means here.

As noted earlier, calling CMN ‘one of the longest continuous records of eBC in the Mediterranean troposphere’ is a strong claim. It may be safer to specify ‘at a Mediterranean high-altitude site’ unless authors benchmark against other long records in the paper.

This is now clarified, see answer to later comment.

III. ERA5 land data is better than ERA5, Authors should have been used it, just clarify why have not gone with more fine resolution and quality meteorological reanalysis data.

ERA5 land data ([10.24381/cds.e2161bac](https://cds.climate.copernicus.org/cds/details/e2161bac)) Has indeed a better horizontal resolution. Unfortunately it does not provide the height of the boundary layer.

3.1 Multi angle absorption photometer Whether the inlet changes could introduce artificial shifts in the time series? Author should mention it.

No, this was checked with Pettitt break points analysis similar to Collaud Coen et al. (2020). See answer to another comment.

Minor language and style comments

- A few phrases should be revised for clarity: ‘Thanks to its design and built-in data correction algorithm’ make it like this ‘Owing to its design and built-in correction algorithm.’
- ‘the instrument was object of several intercomparison’ make it to this ‘the instrument has been the subject of several intercomparison studies.’
- ‘fully verified’ replace with this ‘extensively evaluated’ or ‘well documented’.

Considering the comments of both reviewers, the paragraph was modified deeply and it reads:

... “Although no official reference technique exists for measuring aerosol absorption, the MAAP is widely considered a reference method because of its instrument design and data-correction algorithm. Consequently, measurements from other techniques are often evaluated and corrected against the MAAP (e.g. Zanatta et al., 2016; Renzi et al., 2025), which remains one of the most widely used instruments to quantify the aerosol absorption coefficient and eBC in monitoring stations (Savadkoobi et al., 2024), intense field observations (Yuan et al., 2021) and laboratory/chamber experiments (Kalbermatter et al., 2022b). Its widespread use within the aerosol community has also led to extensive characterization and evaluation through several intercomparison studies (Müller et al., 2011; Asmi et al., 2021; Kalbermatter et al., 2022b). More recently, however, some studies have questioned the reliability of the MAAP as a reference method (Kalbermatter et al., 2022a; Yus-Díez et al., 2025) whereas others have reported smaller discrepancies (Romshoo et al., 2022; Heuser et al., 2025). These findings highlight the importance of carefully considering instrument-specific uncertainties when comparing aerosol absorption measurements.” ...

‘various model of nephelometers’ replace with this ‘various models of nephelometers.’

We used “models” to refer to different commercial nephelometer instruments produced by different manufacturers, all belonging to the same nephelometer class. To avoid ambiguity, we have revised the wording to ... “always measuring in parallel with a nephelometer instrument”...

‘temperature, relative humidity and pressure at the end of the sampling line’ better rephrase with this one ‘at the downstream end of the sampling line.’

Modified as suggested, also in other parts of the manuscript.

‘maintaining the total WAI at 150 LPM’ replace with this one ‘maintaining the total WAI flow at 150 L min⁻¹.’

Modified as suggested.

‘after which, pressure, temperature and relative were measured’ should be ‘after which pressure, temperature, and relative humidity were measured.’

Thanks for noticing the mistake.

‘through the various inlet systems’ should be ‘through different inlet systems’ or ‘through the inlet systems listed in Table 1’.

Modified as

... “through the inlet systems listed in Table 1” ...

The text needs substantial polishing for grammar, terminology, and internal consistency.

As suggested by the reviewer, the text underwent a full revision to improve its readability and consistency.

If available, Authors should also mention these:

- 1. the inlet transitions were tested against parallel measurements,**
- 2. any correction factors were applied,**
- 3. periods immediately before or after inlet changes were excluded from trend analysis**

We agree that parallel testing of inlets would have provided additional information on potential inlet-related artefacts. However, the specific constraints associated with the operation of a mountain observatory made such an approach impractical.

1) None of the inlet configurations were operated in parallel. Modifying an inlet line of approximately 8 m in length and several hundred kilograms required substantial structural intervention and maintaining two independent inlet lines was not technically feasible.

2) Consequently, no empirical correction factors for potential inlet-related artefacts could be derived.

Nevertheless, the inlet systems have been operated for more than 20 years (with aerosol observations beginning in 2002) under conditions designed to ensure laminar flow. For the period covered by the present dataset, no statistically significant breakpoint was identified using the Pettitt test in conjunction with the inlet changes.

3) During every inlet modernization, the entire instrumental setup was temporarily shut down; therefore, no measurements were collected immediately before or after the transition.

We appreciated this comment since it points out an important aspect that was considered during the data treatment but ignored during the writing phase. We now added a dedicated paragraph at the end of Section 3.2.1:

... “Inlet transitions were not tested through parallel measurements, and no empirical correction factors were applied. The inlet system was operated under conditions designed to ensure laminar flow, while no statistically significant breakpoint associated with inlet changes was identified in the dataset. During inlet modernization, measurements were interrupted and therefore no data were collected immediately before or after the transitions.”...

Next sub-section: Figure 1: Add one study area map as subplot in figure 1. This will make your title stronger and more easier to follow for the reader.

We were unsure how to integrate a study-area map into former Figure 1, which presented the identification of invalid measurements, or into Table 1, which summarized the evolution of the measurement conditions. To address the reviewer’s suggestion while maintaining a clear structure of the manuscript, we have instead added a new figure showing the location of the Monte Cimone Observatory (CMN) within the Mediterranean region.



Figure 1 Location and institutional framework of the Monte Cimone Observatory (CMN), including its managing institute, CNR-ISAC, and its participation in the ACTRIS, ICOS, and GAW observational networks. The map shows

the location of CMN in the northern Mediterranean region. Credit: European Union, Copernicus Sentinel-3 imagery.

Increase the font size for better readability of the figures.

Done

Section 4.3.1 Assumption of mass absorption cross-section: A few notes

- **Change “assumed a constant value” to sound more formal and precise.**

The sentence was modified to a more formal tone:

... “The mass absorption cross-section (MAC) was used to convert the aerosol absorption coefficient to eBC mass concentration. Because a site-specific MAC could not be quantified at CMN, we assumed a constant value. This assumption is crucial important since because MAC may vary varies as function of combustion conditions (Corbin et al., 2019, 2022) and degree of internal mixing (Zanatta et al., 2018; Yuan et al., 2021), which, in turns, evolves as function with of atmospheric conditions (Zanatta et al., 2025a) and cloud processing (Zanatta et al., 2023).” ...

- **Replace “in turns” with “in turn”.**

Done

- **Replace “as function of the season” with “with the season” / “seasonal changes”.**

The concept of seasonality was repeated too many time. We simplified as:

... “Considering the seasonal cycle of carbonaceous aerosol properties at CMN (Marinoni et al., 2008; Cristofanelli et al., 2013), it is fair to expect a seasonal variability of extensive optical properties” ...

Change “background station as CMN” to “a remote mountain station such as CMN”. If Authors should make this section a bit more cautious, which could be slightly soften the last part: With this one: ‘The geometric standard deviation (1.33) associated with the European MAC value suggests an uncertainty of approximately +30% and –25% in the derived eBC mass concentration.’

The full sentence now reads: ... “This value represents the properties of internally mixed black carbon particles under background conditions and is therefore considered suitable for converting the aerosol absorption coefficient measured at a remote mountain station such as CMN. Considering the geometric standard deviation of 1.33, the use of the “European mean MAC value” may introduce an additional uncertainty of +30% and –25% in the eBC mass concentration calculated at CMN.” ...

Section 4.3.2 Authors can cite this article as well; <https://acp.copernicus.org/articles/24/11585/2024/>

The suggested reference applies the same established methodology but does not provide a methodological development or geographical context that is specifically relevant to the present dataset. We therefore considered more representative references to be more appropriate for this statement.

Section 4.3.3 Define the dust-day screening more clearly,

Part of the text was modified as follows:

... “Days affected by Saharan dust transport were identified following the approach described by Duchi et al. (2016), combining information on the variability of coarse-particle number concentration (particles $>1\ \mu\text{m}$) with air-mass back trajectories. A day was classified as a dust-affected day when an enhancement in coarse-particle concentration coincided with back trajectories indicating transport over Northern Africa. Days not meeting these criteria were classified as dust-free. The resulting classification was then used to separate the dataset into dust and dust-free conditions. A comprehensive and updated analysis of Saharan dust transport events at CMN, including their occurrence, mass contribution and long-term variability, is provided by Vogel et al. (2025b).” ...

Explain the MAC choice, State the wavelength of the absorption estimate and frame the dust contribution as an approximate attribution rather than a definitive separation.

We modified the paragraph, to clarify the representativity of the two MAC_{dust}, and the overall and monthly contribution to absorption, stating more clearly the wavelength of reference:

... “Since optical properties of dust are highly variable, In Equation 2 we used two MAC_{Dust} representative of the

North African regions. Hence, we provided a lower and upper limit of AbsFrDust using $6.0 \times 10^{-3} \text{ m}^2 \text{ g}^{-1}$ (Sahara dust) and $16 \times 10^{-3} \text{ m}^2 \text{ g}^{-1}$ (Sahel dust) at wavelength of ($\lambda=635 \text{ nm}$; (Caponi et al., 2017). For the current work, we identified 706 dust transport days, representing 14% of the eBC dataset. Applying the two MACDust values, we obtained a median contribution of dust to absorption at 637 nm between 2.7% and 7.2%. Dust contribution appeared to have a seasonal variability, reflecting the strong event frequency and intensity. On monthly base, AbsFrDust may increase up to 5.8 - 16 % and decrease down to 0.6 - 1.7%. (Figure 2). It must be considered that PMDust is based on the quantification of the total coarse PM; hence, it might contain a non-negligible mass of non-dust component, associated with the local background or sea salt aerosol particles (Marenco et al., 2006). As a consequence, the AbsFrDust presented here describes an upper limit of uncertainty introduced by Saharan dust events at CMN.” ...

Comments on supporting materials:

- **Figure S1: If possible, Authors can add radiation data for the more comprehensive outcomes.**
Unfortunately, radiation measurements at Cimone do not cover the full analysed period nor the full seasonal cycle, due to too harsh conditions in winter months.
- **Figure S5: In Figure S5c Y-axis should be (eBC) rather than Planetary boundary layer height [m asl]. Correct it.**
Thanks for noticing.
- **Figure S6: In the winter FLEXPART result is over-estimates while in the summer its under-estimates. why? Discuss the limitation of the model.**
As presented in a former work (Herrmann et al., 2015), the limited horizontal resolution of the model requires a smoothed model topography that does not represent the mountain topography. The simulated winter BC concentration appear to be sensitive to the vertical representation of CMN in the model. However interesting, a full discussion on FLEXPART representation of altitude and vertical transport would result too technical for a ESSD publication. The main text (Section 5.3) was modified as ... *“In mountain regions, this effect might be amplified by vertical transport controlled by the PBL and by the representation of complex topography at the model resolution. The long-term BC dataset presented here can therefore support detailed evaluations of model performance across temporal scales and provide an observational benchmark for assessing the representation of vertical transport and the transition between boundary-layer and free-tropospheric conditions in mountainous regions.”* ...

Note: Added comments in the additional pdf-MS highlighted. (please follow pdf as well)

We have done our best to implement the requested changes in both documents. We would nevertheless like to point out that maintaining two separate sets of comments may increase the risk of inadvertently overlooking some information.