General comment

The manuscript by Bigi et al. analyses aerosol absorption data from both in situ and passive remote sensing collected in the Po Valley, complemented by ancillary datasets of both atmospheric composition and meteorological records. Although limited to few measuring sites and to a relatively short period, it presents new data that can be of interest for the scientific community. The manuscript is sufficiently well structured and addresses relevant scientific questions within the ACP scope.

There are, however, some major weaknesses I would like the authors to address before recommending the manuscript for publication.

These are listed below followed by some additional details and/or minor comments

Major points:

I) A major drawback of the current data presentation is that the reported measurements refer to different periods depending on the instruments and site. It is thus not always clear if the results derived by different instrument at different sites, and summarized in Figures, are actually directly comparable. For example: are the diurnal cycles of quantities presented in Figure 2 and 3 based on data collected in the same periods? Is this true at both sites? A Table summarizing clearly the datasets used in the analysis and presented in the different Figures would be helpful. This table should also include clear indication of data filtering, when applicable. In fact, it is not always clear comparing Figures which data were filtered and how.

For example, in Section 2.2 the authors state that *'Due to these limitations, in the present study, retrievals during events of high altitude dust transport were discarded from the analysis.*' Similarly, in Section 2.4.1 the authors state that '*the days with significant dust load were discarded prior the application of the MWAA model to the in-situ data. Days with significant dust content were first identified for the atmospheric column, using the particle volume size distribution estimated by the AERONET inversion (Sinyuk et al., 2020), and subsequently compared with HYSPLIT back trajectories. Additionally, the impact of dust at ground levels on these days 255 was evaluated using the daily PM2.5 PM10 ratio by the in-situ measurements (Figure S1)*.' However, their Figures 6, 7 and 9 clearly show that dust affects most of the data-points presented there.

Thus a Table with clear indication of the datasets used in the analysis, and data filtering if any, would be beneficial.

II) A second aspect that would merit further explanation is the use of MLH data in this work. These come from ERA 5 (Sec 2.1) and are quite key in several parts of the analysis. As a first suggestion, I would encourage providing some more info on the ERA5 dataset used (e.g., spatial and temporal resolution,…). Then, I think it would be important to understand how this model-based information is representative for the sites under investigation (given that, if I am not mistaken, ERA5 spatial resolution is 25km). In Section 2.3, the authors report an expected error in the MLH of 50 m, but some additional comments could be added in relation to the expected error in the specific investigated site (for example some previous experimental studies measuring MLH and/or PBLH in the area could be used to this purpose). Additionally, in Section 3.2 the authors seem to attribute the discrepancies obtained (e.g. Figure 6) to an erroneous MLH estimate, so I think the authors should better comment on the use of this dataset.

III) The two weak points above combine into a particularly critical Section 3.2. To my opinion, the scientific methods and assumptions in this section are not fully valid and clearly outlined, as further detailed in the comments on Section 3 below (points 28-30). I would suggest considering eliminating this Section 3.2 of the manuscript, or, if not, revise and clarify it taking into account the relevant specific comments below.

Specific comments and minor suggestions

Introduction

1. First sentence introduces black carbon (BC) and states that this is often reported as 'equivalent black carbon' (eBC). Given the core subject of this paper, I think it would be important to explain here in which way BC and eBC are not actually synonyms and why the term 'equivalent' was therefore introduced.
2. Line 30: Better to use ' in the range' here.
3. Line 31: To be rephrased, removing 'resulted'
4. Line 83 Should better be: '..tropospheric layer. However, estimating the vertical distribution of aerosol or their columnar load..'. (In fact, strictly speaking AERONET does not measure the aerosol vertical distribution).
5. Line 52: Maybe the reference here could be updated with a more recent one
6. Lines 89-91: Although I understand what the authors mean, the sentence is not clear enough, please rephrase.
7. Line 92: Not sure 'by' is the correct preposition here.
8. Line 98: the study area is never introduced before in the text (it only appears in the Abstract), so this sentence should be rephrased or the study area should be introduced in the text before this statement.
9. Lines 111-113: Can you be more specific by quantifying the impact?

Section 2

10. I would suggest modifying slightly the title of the section (e.g. 'Measurement site and methods').
11. Line 174: I think the MLH dataset should not be introduced and described within the 'in situ surface measurement' section, being this not derived by in situ measurements but from global modelling.
12. Line 180: refer to Figure 1 here
13. Lines 195-198: the use of the term 'atmospheric thickness' seems incorrect here. If I understand, this is rather the thickness of the aerosol-loaded layer.
14. Line 202: It is not clear to me where this 15% comes from.
15. Lines 205-210: Sentence not clear, please rephrase. It seems from the previous discussion that HR depends on the thickness of the investigated layer, while this sentence states the opposite.
16. Line 211: how did you evaluate periods of desert dust transport? If this was done as described in Section 2.4.1, please refer to that section here.
17. I suggest to remove Section 2.3 and move discussion of uncertainty to the two relevant sections.
18. Both Section 2.4.1 and 2.4.2 should include at the beginning a brief overview of the methods. Some e readers may be not familiar of the principles of AAE- and SAE- based sources apportionment. This is indeed what is expected in the 'methods section'. Although the readers can be usefully referred to relevant literature, these methods should be at least briefly explained in this section (for Section 2.4.2, text now in Appendix A could be used for the purpose).
19. Lines 251-255: can you provide more details? Using the particle size distribution how? using backtrajectories how? Which threshold on the PM2.5/PM10 ratio has been used (this is not clear from Fig. S1).
20. Lines 270-275: the choice of the datasets used to derive AAE for BC and AAE for BrC is critical, in particular use of two completely different seasons may bias somehow the results. Also, it is clear that statistical significance of the two datasets is quite different (1782 data points vs 89 data points), can you further comment on that?
21. Lines 284-289: This paragraph is unclear. Please, rephrase to explain better. Figure S3 also clearly highlights the intense dust episode occurred at the end of February. It is also not clear to me if the 'biased' values of AAE BC and AAEBrC that are clearly dust affected have been excluded from the statistics in Table 1 or not.

Section 3

22. Line 295: What about Saturday's data? Also note a typo error for 'panel' here.
23. Lines 302-323: This part is basically a comparison with available literature. I think this extended text could be better summarized in a Table comparing results of this study with relevant literature (as done for the AERONET-based statistics).
24. The role of residential heating (other than BB) should be also commented when discussing the diurnal cycles of Figure 2. It is rarely mentioned within the text.
25. If kept (see main comment III above), Section 3.2 title should be revised (e.g. Comparing absorption optical depth from remote sensing and in situ values).
26. Line 392: replace 'were' with 'was'
27. Lines 400: Note that label 'a)' in the list is missing.
28. Lines 400-405: To my opinion the results in Figure 6 clearly suggest one main point: for the species showing higher absorption at longer wavelengths (880, 675 nm) assumption of a uniform absorption distribution through MLH is not valid. In fact, reasons for discrepancies listed by the authors as b) and c) would also affect results in the blue range (third panel), which is not the case.
29. Also note that definition and use of ApAOD is critical. First, it has the dimensions of a length; therefore the naming of this quantity seems not particularly suitable. Additionally, its definition assumes that $AAOD=abs_{insitu} \times ApAOD$, which might be not the case for example in cases of transport of elevated aerosol layers, as during dust transport events, in which aerosol layering might be different (e.g. $AAOD=abs_{insitu} \times H_{layer1} + abs_{aloft} \times H_{Layer2}$)
30. Lines 425-434: It is certainly true that '*Differences in AAOD might originate from a different atmospheric layering and mixing*' but this implies that statement at lines 425-433 needs to be rephrased. Although I understand this claim, in the current formulation the statement is not correct. In fact, the decrease in PM2.5 / PM10 ratio only indicates that some dust is reaching the ground, but is not telling anything about its vertical distribution.

References

31. References are often given in non-chronological order within the text. Please, check the journal instructions on this.
32. Section 2.2 should include reference to O'Neill et al. papers, on which most AERONET almucantars retrievals are based on.

Figures:

33. I think panels within Figures should be numbered (or labelled) and Figure captions should refer to this numbering (labelling) to be more readable. Please, check the journal instructions on this.
34. Figure S2: please improve color scale range, as in the current form it seems not optimal for the dataset presented (little variability among data dots color)

Tables:

35. Table 1: why reporting medians and associated median absolute deviance? Generally, when reporting median values, associated percentiles (typically 25-75th) are rather included. Are you sure the same statistical parameters are reported in the referred literature? Given the wavelength dependence of the AAE, probably the table could be restructured separating values by spectral range, making the table more readable.
36. Table 2: specify wavelength rather than 'IR', for homogeneity with the text and Figures
37. Table 3: I would remove this one. As well documented in the text, most variables have a marked diurnal cycle, providing Median and interquartile values here could be misleading and hide important information on daily variability. I would rather suggest to insert Figure S4 in the main text, showing the diurnal cycles of the same quantities (as done for the atmospheric composition ones).
38. Table A1 now is given before introducing Appendix A, and is not related to it

TITLE: A further suggestion is to include reference to the study area in the manuscript title, as it seems a bit too general in its current form.

Language: The language is not always fluent and precise, and some revision by a mother tongue is encouraged.