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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of cloudiness and cloud type on the atmospheric heating rate of black and brown carbon" *by* Luca Ferrero et al.

Anonymous Referee #1

Received and published: 4 August 2020

The manuscript by Ferrero et al. acp-2020-264 titled "The impact of cloudiness and cloud type on the atmospheric heating rate of black and brown carbon" presents heating rate measurements of the atmosphere over the Po Valley, Italy.

The measurements are valuable as they are relatively rare in the community. The work is incremental on Ferrero et al. 2018, with the main incremental improvement being the automated separation of clouds into cloud types using radiometer measurements combined with Lidar-Ceilometer measurements. The introduction of lidar information into the automated cloud classification is novel and may be valuable to other work, yet was not thoroughly validated. **I recommend that the authors describe this cloud classification algorithm in detail in a separate paper and include more detailed validation work. If the authors do not follow this recommendation, they must**

provide a clear argument for why in the review responses and in the manuscript.

The actual presentation of the results in this manuscript is incredibly poor. Here I present 200 lines of comments which I had to make simply in order to understand the results. The discussion is long, dense, and disorganized. Most of these comments are on presentation and organization, at the level which is normally given to an author's first draft of a first manuscript. After I finally understood what was done and what the results were, I see a valuable data set. However, the scientific interpretation is on a similar level to the writing.

I fear that my scientific feedback has been drowned by the poor writing, manuscript organization, and figure presentation in this work. **To emphasize my main scientific comments I have used boldface text in the following.** The authors should streamline their manuscript by referring to Ferrero et al. 2018 whenever possible, by separating their cloud analysis from their light-absorbing aerosol analysis, and by clearly demonstrating whether or not there is any value to the different levels of information available here. Those levels are: 1) heating rate resolved in time, 2) heating rate resolved by time and cloud height, 3) heating rate resolved by time, cloud height, and cloud type.

My recommendation to the authors is to completely rewrite this manuscript and reinterpret the results. Since this work is incremental to earlier, well-presented work (Ferrero et al., 2018), and since the results are well supported if poorly presented and interpreted, I do not recommend rejection but major revisions to the Editor.

(NB: I have not numbered my feedback below. When the authors respond, please refer to my comments as "C1P2" for page C1, paragraph 2, etc. Please also copy and paste the comment before responding.)

1 General comments

The Introduction provides a strong motivation for the importance of HR and cloudiness, but the final 2 paragraphs are poorly structured. Please emphasize more the importance of cloudiness, including common levels to be expected (i.e. expand the discussion around Crock et al.) and feedbacks (i.e. expand the discussion around Perlwitz and Miller 2010, which is central to this work's motivation)

In many places in this manuscript the authors say that "F_λ is the radiation" when they seem to mean "spectral irradiance" (W/m2/nm). The word "radiation" is not accurate. On line 351 they suddenly use "irradiance". Be consistent. Avoid confusing your readers.

It appears that all reported quantities are strongly corelated for direct and reflected irradiance. If this is correct then please sum these two quantities in all figures. The presentation is too confusing.

The preceding work (Ferrero et al. 2018) included diurnal trends in irradiance, which would be valuable here. Why were they not included?

2 Unclear text or discussion

I would require that the authors add a glossary table (defining abbreviations) before publication due to the large number of symbols in the figures. Moreover, please improve the legends (e.g. 6b)

I will suggest that the authors change Fig 8, 9 axis labels from "Ci" to "cirrus" etc for all cloud types.

Figure 10's axis label should include HR.

79 Higher than clear sky conditions in certain localized regions only, or?

110 'Conversely' is not appropriate here

110 Please start a new paragraph at "This study". End this paragraph instead with "This study aims to fill this gap" or similar.

Fig S2. Please add an "uncorrected" panel to give readers an idea of the magnitude of the correction (which is related to uncertainties).

Line 168 What is the physical meaning of the C value being close to the GAW value? (e.g.: The particle size and single scattering albedo were typical of atmospheric monitoring sites. Collaud Coen et al., 2010)

171 Was the MRI built by U Milano-Bicocca? Please add manufacturer, even if it is homebuilt.

200 What is the uncertainty or accuracy of the Nimbus 15k? In other words, please mention the limitations as well as the strengths of this system.

238 "This is due to the negligible ..." This sentence is not accurate. The authors may instead consider the simple harmonic oscillator (Moosmuller et al. 2011, doi:10.5194/acp-11-1217-2011) or energy gap (Sun 2007 doi:10.1029/2007GL029797) models here.

243 change 'successfully' to 'previously'

251 First use of LAA on this line was not defined. Please introduce the concept in the introduction. It is a nice and useful abbreviation.

350 Give limits of the integral in the equation.

3 Opportunities to shorten and clarify the text

135 This information is redundant with lines 110-112, please shorten 110-112. 171-177 Please cite Cogliati immediately after introducing the MRI.

263 Change "N represents one of the possible 9 classes" to "N = 1,2,3, ..., 9, representing 9 classes of cloud fractions"

3.1 Heating rate measurements – Equation 1,2, and 3

There is no need for all 3 equations here. Delete Eq 1. Start with Eq 2 to introduce and define ADRE first, then the reader will understand Equation 3 (new Eq 2) naturally. Remove μ and replace it with $\cos\theta$. There is no need to introduce μ because it is only used twice in the manuscript, and anyway $\cos\theta$ is more easily understood.

Also, use the integral sign to specify "integral over the whole 2π" (line 209) instead of writing it only in words. Same for θ.

Equations 4 and 5 use subscripts dir, dif, ref to specify direct, diffuse and reflected radiation. Equations 1, 2, 3 use "nth type of F" to do the same. Choose one and stick to it. The text subscript is better, and the authors obviously agree as they used it later in the manuscript.

The term $F_{glo} = F_{dir} + F_{dif} + F_{ref}$ must be introduced already in the first equation of Section 2.2. Prepare the reader for Equation 6. I have to assume that this is the definition, the authors never gave it.

The only real difference between HR and ADRE in Figure 5 is the air density ρ. So, the authors should plot ρ in the figure and emphasize this in the caption to avoid confusing readers who are not familiar with HR or ADRE (in other words, most readers).

Since the main contribution of this manuscript is to discuss heating rates, why discuss ADRE at all? Leave that to the SI. Or, of the authors disagree, then discuss only ADRE. HR appears more valuable as ρ is a meteorological variable.

3.2 Cloud classification – Section 2.3.2, and Figures 2 and 3

This section is a mess.

Do not mix discussion and results in S 2.3.2. Review the literature first, then present your results. Present "failed analysis" in the SI, not in the main text. Use only 1 or 2 sentences in the main text for failed analysis.

This reviewer spent several minutes studying Figure 2 and writing the following comments before learning that it is a "this did not work" figure. The writing should make this clear immediately. Restructure S2.3.2 to fix this.

The section here concludes that the 2 literature methods discussed (Duchon 1999 and Harrison et al 2008) were not adequate, based on the conclusions of Harrison's work. So the authors introduce a new method, but with no validation of it. How can the reader trust this? I believe the author's work is valuable but the discussion needs to include validation.

Line 308 how many cases (%) were analyzed after this limitation?? The authors should not discard cases of multiple cloud layers. Simply include a category "Multiple layers" or "Complex cloud layers" or similar.

If I am to believe this section then the authors have contributed a numerical algorithm to the topic of automated cloud type analysis. Only 2 papers have been published on this topic, and most cloud type identification remains manual. This is the 3rd paper to contribute to this topic in 30 years, yet the authors did not include a solid analysis of the algorithm.

Either the authors have used an unvalidated algorithm in their work, or the authors should write an entire manuscript describing their validation of what seems to be a valuable contribution. Separating the cloud-algorithm work from the radiative heating work would mean removing Figures 2, 3, 4, 7, 14, and some SI figures from this to another manuscript. This would avoid breaking up the "BC+BrC" story.

I note that the Harrison et al. 2008 work was missing from the reference list.

Other comments:

Make Fig S3 axis labels consistent with the language in S 2.3.2. Put R on the x axis.

The use of a 20 minute interval in calculating SD for the Duchon and O'Malley method means that wind speeds are included in the measurement of cloudiness fluctuations. This must be discussed. How do wind speeds compare with this 20 minute interval?

Lines 281-314 break up this huge paragraph.

Figure 2 comments: Fig 2's legend is inaccurate, there is no dashed line in the legend. In Fig 2g, colour the red line in the same way that the points in Fig 2h are coloured. Move the entire figure to the SI. Consider adding photographs to this figure.

4 Results and conclusions

The results and discussion are too long, relative to the information content of the manuscript. The information is valuable but does not require extensive discussion.

As I mentioned above, the discussion is broken up by switching between the cloud analysis and the BC+BrC analysis. Start from the top and go down. Focus on the cloud effects before attributing RH to LAA afterwards and then to BC+BrC.

The authors introduce direct, diffuse, and reflected irradiance yet do not present the data consistently. Some figures separate all 3. Some figures present direct, diffuse, and total (Fig 14). Some figures (Fig 13) present sums of 2, in various combinations. Some figures combine all 3 as "global irradiance" others combine all 3 as "total irradiance". Please, assess your data, choose one message, and present it clearly to your audience. Follow Harrison et al. 2008 in presenting the diffuse fraction unless your data support an alternative. Figure 9 is the only figure that suggests a difference between direct and reflected, but the impact on heating rate is unclear because Figure 13 changed the presentation strategy.

The conclusions are similarly confused. Why are different cloud types discussed in detail when Figure 10 and 15 clearly show that the key predictor is oktas and not cloud type? Only high clouds (cirrus, cirrocumulus, and cirrostratus) do not follow this trend, presumably because they are well above the aerosol layers.

I do not see any support for the final conclusion that the cloud impact affected HR of BC more than of BrC. The absolute value of the BC HR was higher initially, so it would naturally change more. My interpretation of the authors' results is that there is no need to attribute cloud types in future work, and that cloud height data combined with diffuse fraction (Harrison et al. 2008) may be sufficient. If this work is to be extended to other monitoring sites the authors must address this point explicitly. Simpler measurements are more likely to be adopted by others.

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