Review of Evaluation of equivalent black carbon (EBC) source apportionment using observations from Switzerland between 2008 and 2018 by S. Grange et al.

Grange et al., submit a revised version of their earlier manuscript. I recommend publication pending correction of one major issue and a number of small revisions.

Major issue:

It is well beyond the scope of this article (and journal) to discuss in detail the effectiveness of various emission control technologies, BC abatement strategies, environmental management and the effectiveness of regulations. These important issues are often complex and nuanced, but are discussed in very brief terms in this work.

For example the authors write: 'the diesel market share for passenger vehicles sharply declined in the wake of the Volkswagen emission scandal ("dieselgate") after 2015, and this market change may also be a contributory factor in the EBCTR trends observed after about 2016.' A simpler explanation for reductions in traffic related BC might be the decline in the non-DPF diesel fleet share, and given that in many circumstances that DPF-equipped Diesel passenger cars will emit less PM than gasoline cars, the statement is highly speculative about an important public health topic.

In another example the authors state 'If BC was perfectly controlled at the source of emission, Switzerland could achieve a 6–14 % reduction of PM2.5 concentrations to aid with the compliance with the annual and daily limits set for PM2.5'. It is not clear to me if this should be taken as a suggestion to ignore the issue of BC in Switzerland or focus on it. It is also not feasible. The question of whether to focus on BC in order to reduce total PM would depend on many factors including the marginal abatement costs compared to other sources. Given that the BC fraction is quite low, there is likely to be other sources with lower marginal abatement costs, i.e. 'low hanging fruit', and so this sentence would actually suggest the issue is not important. What about co emissions? Reducing BC emissions would reduce the total PM by more than the BC fraction alone via associated reductions in NOx, VOCs, primary organics etc. Clearly the issue is not as simple as presented here.

In the conclusions the authors state 'This is evidence of ineffective management of reducing BC and PM emissions from domestic wood burning which has been noted in the past'. No citation given, and a potentially defamatory statement.

All such discussion should be removed, especially since doing so would in no way weaken the authors' conclusions regarding aethalometer data treatment.

Minor issues:

The authors use absorption at 950 nm to determine EBC for trends and as one of the wavelength pairs in the aethalometer model. While this is fine (the only advantage of 880 nm I can see is that the error is slightly lower), and is also used by Zotter et al., the absorption in 880 nm channel converted using collocated EC is presented as the 'strict' definition of EBC and is even presented as such in Fig. 1. I suggest to revisit Sect.

1.2 where this is discussed, possibly changing the figure to show either both the more common definition and the definition used in the paper.

On Pg. 9 the authors discuss a decline in the MAC value at Rigi, suggesting this is instrument related. However, there does appear to be a continued decline after the instrument change and so a real effect is possible and might merit further investigation (though not necessarily as a part of this work).

A number of small corrections and suggestions now follow:

Pg. 1, Ln 6: change *BC emitted by different combustion processes have* to *BC emitted by different combustion processes has*

Pg. 3, Ln 4-10: Here it would be natural to introduce the term Ångstroem exponent when discussing spectral dependence of absorption from different sources, and to mention briefly that a major issue with aethalometer model is that it is highly sensitive to this parameter which must be assumed a priori. Another issue is that the choice of wavelength pair influences the model results.

Pg. 4, Ln 2: Change *EBC is only a measurement definition* to *EBC is only an operational definition*

Pg. 4, Ln 10: Change *but* to *it*

Pg. 5, Figure caption suggests that all Swiss EBC monitoring sites are included, but this is not the case since (at least) Jungfraujoch also has EBC measurements, hence the caption overstates how comprehensive the work is. Suggest changing to 'The six Swiss monitoring sites in this study' or similar.

Pg. 9, Ln 3: I think the authors should mention that there is come clear variation in MAC due to instrument changes and that a large part of the standard deviation in MAC is due to this. This is pretty clear from the figure.

Pg. 9, Ln 3 (and elsewhere): Consistently use a space between numbers and units.

Pg. 13, Fig. 6: Caption is too brief to understand the figure on its own. Please indicate in the caption that the figure shows EBC concentrations vs wind speed and direction and that the numbers in the figure are wind speed in m s^{-1}.

Pg. 17, Ln 22: Quote marks are not needed, since although the implication seems to be that there are anthropogenic influences on wildfires, they are nevertheless classified as natural. I suggest either stating the issue explicitly e.g. 'from naturally caused combustion processes (which are nevertheless influenced by anthropogenic activity [citation])' or use 'wildfires'.