We thank Referee#2 for the helpful comments. These comments helped to substantially improve the manuscript. Below we give detailed answers to the individual reviewer comments in blue.

Comment 1: The manuscript sometimes refers to the three-wavelength photoacoustic as "multiwavelength". I would say the term "three-wavelength", which is already used in the manuscript more than once, is much more appropriate, especially in the context of other methods of measuring or estimating absorption, like the 7-wavelength aethalometer.

We replaced multi- by three-wavelength photoacoustic spectrometer throughout the manuscript.

Comment 2: This comment is more substantial and relates to the interpretation of the diffusion flame samples. The diffusion flame was operated under various conditions, which resulted in varying % of "OC" being measured, where OC is defined by thermal evolution of carbon in an inert atmosphere.

The manuscript currently discusses this OC % in direct comparison to the OC % of atmospheric aerosols. This is incorrect. The OC that is measured in the particles produced by fuel-rich diffusion flames has been shown to represent incompletely graphitized soot (Maricq 2014). This stands in contrast to the typical OC found in the atmosphere, which forms from "normal" organic material via terpene oxidation, hydrocarbon evaporation, etc. These "normal" organics will be very different from partially graphitized soot, in terms of their volatility, reactivity, light absorption, and hygroscopicity. Therefore, the material currently described as OC is not comparable to atmospheric OC, and the two should not be compared. At the simplest level, a similar OC% between the diffusion flame soot and atmospheric particles clearly does not warrant a comparison of the MAC between such samples.

Of course, the diffusion flame OC is still a reproduceable and well-defined material, so the reported MAC values are likely to be useful to others. But the authors should make this distinction clear, and change the discussion at "Discussion of the chamber results", on page 4 first paragraph, on page 12 second paragraph, and wherever else is relevant.

Note also that the chemical uniqueness of the partially graphitized flame soot also explains why the SP2 did not observe incandescence signals (page 9, line 30). Since the material is incompletely graphitized, what was measured as EC may have formed by charring during OC/EC analysis, and what was originally present apparently had a MAC that was too low for the SP2 to bring it to incandescence. Alternatively, the material may not have been refractory enough to incandesce.

This is indeed an important point and we revised the manuscript as described in the following to make the difference between flame soot OC and atmospheric OC clearer.

P4, first paragraph: At this point we have a general introduction of the paper's structure. In connection with the soot generation we used the term „surrogates", without a validation of the comparability to atmospheric carbonaceous particles. This might be somewhat misleading to the reader and, therefore, we changed "… carbonaceous particles with different OC content" to "… combustion particles with different OC content" to be more precise.

In the discussion of the chamber experiments, P11 of the original manuscript, we added: "It is important to note that the CAST propane diffusion flame generator was used as a reproducible source for analogue combustion aerosol. By changing the fuel-to-oxygen ratio, the influence of incomplete combustion – manifested by increasing organic carbon (OC)

content – on the absorbing properties of the soot aerosol could be systematically investigated. However, combustion OC material does not necessarily represent all atmospheric OC compounds as most of this material stems from secondary processes like the oxidation of terpenes. This should be kept in mind, when comparing our laboratory results with results from field measurements."

On P12, L42 we replaced "This finding relates to results obtained in field measurements by Kondo …" with:
"Even though these laboratory studies are not directly comparable to atmospherically processed combustion emissions and, furthermore, different methods were used in the laboratory and field studies, it should be mentioned that comparable MAC values of BC are also deduced from atmospheric measurements. Kondo et al…."

Comment 3: On page 7 line 1, the LOD is discussed. What was the averaging interval? What was the influence of longer averaging times? An Allan variance analysis would be the best way to answer these questions. What I am requesting is given by e.g. Onasch et al. (2015).
The averaging interval was 20 s. We will act on the suggestion to investigate the influence of longer averaging times.

List of minor comments:
I also have a few minor comments after reading the manuscript carefully.

Page 2, line 35 – since BrC typically does not increase indefinitely with decreasing wavelength, please give a number to "shorter wavelengths"

P2, L35 was changed to:
"For BC, the imaginary part of the refractive index is nearly wavelength independent over the visible and near-UV spectral range. In contrast, the imaginary part of the refractive index of brown carbon (brC) continuously increases from the red over the blue to at least the near-UV spectral range."

Page 3, line 30 here cite Collaud Coen et al. Atmos Meas Tech 2010, 3, 457-474.

On P3, L31 we added:
(Collaud Coen et al. 2010, Lack et al. 2008)

Page 3, line 41, why is there weak cross sensitivity to particle light scattering?

On P3, L41 we removed:
"… or only weak …"

Page 5, line 26, have the authors investigated whether a different transmission efficiency of such gases as NO2 might cause bias in the background measurements? Some gases may interact significantly with the PM filter.

Until now, we haven't found any indications for a substantial interaction of gaseous species with the background filter material that causes a detectable bias.

Page 6, line 4, is the instrument temperature controlled? Could variations in temperature lead to a bias in the corrected laser power?

No, the instrument is not temperature controlled. We agree with the Reviewer that variations

in the temperature can result in a drift of the characteristics of the acoustic resonator (but not in the laser power measurement as this done with a thermopile detector that is less prone to drifts in ambient temperature). The impact of a temperature drift onto the cell constant of the resonator and, consequently, the measured absorption coefficient is currently being investigated. However, the acoustic resonator has a low enough quality function, so that a drift of a few degrees should not result in a significant change of the cell constant.

Page 6, line 29, what does field proven mean? Proven against what reference?

On P6, L29 we removed:
field-proven

Page 10, line 24 and elsewhere. Ideally a quantitative statistical analysis would be given, such as a two-tailed t-test, instead of the statement that values agree nicely or not.

We compared the results from two different campaigns where different methods and instruments were used. So, a qualitative statement on the comparability of the results is justified as otherwise a comprehensive discussion of all instrumental and methodical errors is necessary which is certainly beyond the scope of this manuscript.

Page 12, line 11 Here the reader wonders what the difference between the different EC/OC protocols is, especially with respect to their charring artifacts (see OC% discussion above). A short comment or citation would be helpful.

We added citation of the following paper to the manuscript:
Watson et al., Aerosol and Air Quality Research,Vol. 5, No.1, pp. 65-102, 2005

Page 13, line 36, "home heating season" would be clearer.

P13, L36 was changed to:
home heating season

Page 13, line 37, why were the other periods described if only 31 October is included here?

Here we want to give a short description of the overall weather situation, and especially the change in the conditions from warm to cold, which we believe is important for the observed change in the wavelength-dependent aerosol absorption.

Page 14 line 20, were the limits of sensitivity of the SP2 with respect to particle size corrected for?

No, these sensitivity limitations are not corrected.

Figure 8 is hard to read and includes some labels which are not descriptive, like numbconc, scattSP2 and BBHG.

Figure 8 is changed in the revised manuscript.