

Atmos. Meas. Tech. Discuss., referee comment RC2
<https://doi.org/10.5194/amt-2022-21-RC2>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on amt-2022-21

Anonymous Referee #3

Referee comment on "A dual-wavelength photothermal aerosol absorption monitor: design, calibration and performance" by Luka Drinovec et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2022-21-RC2>, 2022

Drinovec et al. provides a detailed characterization and thorough discussion on the design, calibration and performance as well as the associated uncertainty of a dual-wavelength photothermal aerosol absorption monitor in determining aerosol light absorption. The paper is well written and enough details are provided for the readers to understand the instrument and the tests performed. I have a few comments/suggestions for authors to consider. I recommend publication after these comments are addressed.

Page 5, Line 132 stated "NO₂, which absorbs strongly in the blue-green part of the spectrum, is a commonly used calibration gas." And immediately in Line 134: "Calibration with NO₂ is very common, especially for climate studies, because it is usually performed at green wavelengths, where the solar spectrum features a maximum." The two sentences are redundant. The authors can choose to keep one.

Page 10, Line 294 mentioned "A new photothermal interferometer configuration...". Here it's not clear as written if the new configuration is relative to what was described above or older design documented elsewhere, and the authors should clarify it.

Page 12, Line 359: What is the selected optimum modulation frequency? Also can the authors comment on the larger error bars in the signal response observed for the 1064 nm channel?

Page 16, Line 441: "...which proved very stable over a very long period...". It's difficult to assess what is considered "very stable" and "very long period". The authors should add more support to this statement.

Section 4.1: Is the calibration of the 532 channel using NO₂ gas mixture single-point or multi-point calibration? It sounds like the calibration part is single point, and then the validation used multi-point NO₂ concentrations to prove linearity. I suggest that the authors combine results of the NO₂ calibration and validation (e.g. move Figure 11a and related discussion) to Section 4 for better flow of the manuscript.

Page 17, Line 478, what is known about the refractive index of dry nigrosin particle (powder)? Did the authors try to measure the refractive index of nigrosin before dissolution? And here needed more evidence about why the authors consider the RI of solid nigrosin film a better representation of the true RI value over the aqueous solution.

Page 17, Line 493 stated that the real part of the RI at 633 nm is determined at 1.81 in this study, which "agree well" with the value obtained by Bluvshstein (2017). This seems contradictory to the earlier statement in Line 483: "This difference can be attributed to the variability in the nigrosin quality provided by the manufacturer or the measurement method." Can the authors explain this?

Figure 12: Can you translate the noise (standard deviation) to error bars and add them to Figure 11?