

Interactive comment on “A new optical-based technique for real-time measurements of mineral dust concentration in PM10 using a virtual impactor” by Luka Drinovec et al.

Anonymous Referee #3

Received and published: 24 March 2020

The paper presents a new technique relying on combining a virtual impactor with an ethalometer, and supports it with a substantial body of laboratory and field data, including both instrument characterization and intercomparisons. The technique under development has the potential to bring significant improvement to dust monitoring and characterization, especially in the context of mixed aerosol types.

The text is well written and informative on the whole, but some stylistic detail should be improved, for example inconsistent use of different grammatical tenses (present, present perfect and past), making it difficult to differentiate between the authors' and previous work.

C1

Some detail is not clear, potentially even leading to misunderstanding. For example, the introductory section is somewhat intidy, lacking a logical progression, and ought to be improved.

The statement in lines 204-209 is unclear: is the nonlinearity due to filter saturation? And what are the "k values" - do they compensate for the nonlinearity? Are they constant over time, wavelength etc?

In the Supplement, tests of the virtual impactor (VI) are carried out with PSL, which has specific gravity close to that of water, less than half of that of typical mineral dust. Consequently, the geometric diameter of the latter is substantially smaller than the aerodynamic diameter (relevant in the context of the VI). This aspect is not highlighted and it is not always clear which diameter is discussed. Consequently, a reader using the reported (aerodynamic?) diameters could be misled into applying them to geometric dust sizes.

A constant value of the enhancement factor (EF) seems to be used. Yet EF is variable, as the authors' own data shows, and it will depend on particle aerodynamic size, hence the composition of the sample at any given time. This may be a major shortcoming, affecting the accuracy of the technique. Is it feasible to improve accuracy by using this dependence, perhaps taking advantage of on-line data? While this may not be possible with the current setup alone, the authors should comment on it and suggest potential solutions.

My general concern is about an unspoken shortcoming of the technique: it would fail if the dust and black carbon was internally mixed. As a warning to potential users, this should be highlighted, and the "climatology" of internal as opposed to external mixing described from known historical data.

Another absence is lack in the discussion (or introduction) of comparison of advantages and shortcomings with other methods, such as optical particle counting and aerodynamic sizing.

C2

Typos and corrections: Line 76: "allow for hourly" is written but "allow hourly" is meant.
Line 324: "Single" should be "single".

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-506, 2020.