Overview:

This paper presents possibly useful advances in the application of large-aperture scintillometry (LAS) to determine refractive index structure parameters. The authors claim to present a new method to separate the contributions of atmospheric absorption (the imaginary part of the refractive index structure parameter) and atmospheric scintillation (the real part of the refractive index structure parameter). It appears that there is some new theoretical development, amongst a great deal of already published theory; the authors need to clearly distinguish their new derivations from earlier published work.

This is a potentially improved method to reject the unwanted influence of absorption on the scintillometer measurement, without relying on spectral separation (analyzing the spectrum of scintillation), enabling more accurate measurement of the real part of the refractive index structure parameter, and therefore better estimation of the sensible heat flux. Furthermore, this opens the new opportunity to measure the spatial variation of atmospheric absorption along the long measurement path, which may have applications in aerosol monitoring, and potentially trace gas monitoring. The paper could be improved by further discussion of these applications, and the subtle differences between long-path absorption spectroscopy and the absorption measurement presented here.

The presentation is reasonably clear, but the English needs to be improved in numerous places.

It is unclear from the paper, whether a fundamentally new approach has been derived, or more likely a variation on the already known method of spectral separation for the absorption and scintillation contributions to the measured spectrum. The auto-correlation analysis presented essentially fits two asymptotic lines to the time-delay auto-correlation – I challenge the authors to show how this is fundamentally different to determining the 'corner' frequencies shown in Fig.1, by fitting the dashed and solid lines shown – as described by the authors 'analyzing the spectrum of scintillation' (p.21301).

The authors have not demonstrated improved results over the aforementioned spectral analysis, using the time-delay auto-correlation, and only one 20 min period is analysed by the two separate methods; therefore, it has not been shown that the time-delay auto-correlation method has any advantage. It might be claimed that the measurement of the outer length scale of turbulence is a valuable benefit; however, the experimental performance of this measurement is not properly evaluated (again only one result is quoted, which may be considered to be approximately the measurement height, a 'rule of thumb' approximation of the outer length scale). The authors should estimate the uncertainty of their measurements. Overall, very few data are shown – Fig.4 uses a single 20 min data period, and Fig.6 & Fig. 7 show the same 24 hour period.

The LAS derived crosswind comparison is not really novel and comparison with a single cup anemometer over complex urban terrain, does not provide a scientific quantitative comparison. There is insufficient discussion of the Double-Point temperature fluctuation sensor – what is its response time? As the sensor separation is 0.8 m, it will not be able to measure the same inertial sub-range turbulence measured by the LAS; how is this frequency response mismatch dealt with?

Why do this comparison, if only to say that agreement will be limited by the vastly different spatial sampling (was the LAS path length still 960 m?). Fig.5(a) is a log-log scale plot, and seeing the spread of data, the apparent noise-floor of the LAS and the curve of the data, I do not agree that the comparison is 'very good'. The authors ought to be able to measure and state the instrumental noise floor of the LAS (which appears to be rather poor compared to some commercial LAS instruments).

Technical Comments:

1. P.21288 Lines 16-17 please expand as this is important and useful, that the imaginary part of the ARISP contains information on inhomogeneities of the absorptions (contrast with long-path spectroscopy).
2. P.21289,L.23-4 These assumptions need careful and critical justification; the absorption media may not be conservative nor passive, and sources may be above the surface (e.g. chimney stacks)? It is unlikely that the absorption and temperature sources will have the same spatial distribution at the surface – how does this affect the application of the theory?
3. Justify the assumption of isotropic turbulence in the urban environment (p.21290, L.9-10).
4. P.21292, L.10, please justify this assumption – it appears that the variances caused by the real and imaginary parts of the ARISP will be highly correlated because of their dependence on atmospheric turbulence and crosswind speed, as shown by Eq.4 and Eq.6?

Editorial Comments

1. P.21286, Line 25 change 'line-sight' to 'line-of-sight'.
2. L.26 – rephrase, turbulence alone does absorb light.
3. P.21287, L.6 change to 'intensity in the receiving plane'; change 'a distance' to 'some distance'.
4. L.11 change 'measure' to 'determine' (since this step is dependent on similarity theory, and is not a direct measurement).
5. L.19 change to 'contribution of absorption'.
6. P.21288, L.29 change to 'Finally, a brief conclusion is presented'.
7. P.21290, L.9 (and elsewhere) change 'isotopic' to 'isotropic'.
8. P.21293, L.1 change to 'commonly used'.
9. Other numerous minor errors in the English language need to be addressed.