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Atmospheric Measurement Techniques Discussions

Interactive comment on "Infrared and millimetre-wave scintillometry in the suburban environment – Part 1: Structure parameters" *by* H. C. Ward et al.

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The authors present a two-part study in which they present the results of the first long-term application of an optical-microwave scintillometer system over Swindon, UK. In the first part, they present the results in terms of structure parameters and in the second part they present the results in terms of the heat fluxes.

Indeed, both manuscripts present research novel in many aspects. The application of a combined optical-microwave scintillometer system has been presented before, but never for such an extensive time period, nor over the city centre. This first part is

addressing many technical issues at a high scientific level. They show strengths and weaknesses of all the scintillometric methods, as well as those of eddy-covariance measurements. On occasion the first manuscript points a bit too much towards the second manuscript to my opinion; the technical results and the results of the structure parameters already have value in themselves. Nevertheless, the manuscript is generally of a high scientific quality, presenting innovative results, and very well written, so that I recommend publication after minor revisions.

P 11171 – 17773, Introduction – the introduction gives a remarkably good overview of the state of the art in recent and older literature. Nevertheless, the part describing the objectives of this study (P 11173, line 24ff) is rather limited. To my opinion it does not get clear to readers who are not so familiar with the topic what this study contributes to the literature. It is stated that the presented dataset is "by far the longest dataset that uses these techniques", but it has not been stated yet, how long this period is. Furthermore, the sentence "Methodological considerations (. . .) and seasonality are explored" is rather vague and saying little. To my surprise, the conclusion, section 6, actually does a much better job in describing this relevance. Hence, the authors should make it more clear in the introduction already what this study contributes.

P 11170, Line 9 – "unique", the use of this word is somewhat confusing here. It raises the thought that a rather exotic scintillometer is used, and with that the question on the representativeness of the results presented here. I would leave it out here.

P 11170, line 11-12 – "humidity fluctuations and the so-called", add a comma between "fluctuations" and "and".

P 11170, line 12 – this sentence is hard to understand when one does not know the contents of the paper already. In itself the pairing of the two wavelengths already offers sensitivity to the humidity and temperature correlation. Mentioning the bi-chromatic method for this purpose is therefore unclear at this point when not introducing the two-wavelength method as well.

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P 11170, line 15 – to what does “the techniques” refer?

P 11170, line 20-21 – “The energy (. . .) companion paper.”, it is unclear to me what this sentence is meant to say here, because you already introduced the companion paper before (line 9).

P 11171, line 12 – “refraction”, it results from diffraction. Rewrite.

P 11171, line 12 – introduce a semicolon after “beam” for readability

P 11171, line 13 – the refractive index of an eddy is not determined by the density of constituent ones as is written here. Instead, the refractive index is defined as the factor with which the speed of an electromagnetic wave (speed of light, c) in a medium is reduced as compared to that in vacuum. Hence, the refractive index of an eddy is determined by the temperature and moisture content of the eddy itself. Rewrite.

P 11171, line 17 – “humidity fluctuations are also important”, writing it down like this suggests that temperature fluctuations are still important for millimetre or radiowaves, whereas their effect is rather limited. Rewrite

P 11171, line 18 – “Peak sensitivity”, sensitivity to what?

P 11171, line 21 – suggest to delete “other”.

P 11172, line 1-2 – “On the whole, (. . .) are-averaged fluxes.”, how did these studies determine the blending height, that this conclusion can be drawn from them?

P 11174, line 8, This definition of structure parameters in this line does not suffice. It perfectly applies to variances as well. However, in contrast to variances, the structure parameter is not dependent on the ensemble average of y(x), because the structure parameter considers, as Tatarskii expresses, only fluctuations smaller than the spatial separation δ. Rewrite.

P 11175, line 1-4 – AT and Aq are given in Ward et al. (2013b) as AT and Aq. I guess the latter instance of AT should be At? Otherwise the sentence makes no sense.

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P 11175, line 10 and Eq. (4). – “can be approximated”, this formulation rightly suggest that the “=”-sign in Eq. (4) should be replaced by an “$\approx$”-sign.

P 11175, line 13 – “typical atmospheric conditions”. The question is: typical for where and when? Tropics? Swindon during summertime? Swindon during wintertime? Be more specific.

P 11176, line 16 – put a comma after “method” and delete “obtained” for readability

P 11177, line 1 – “most assumptions”, what is meant with this? Maybe, “MOST assumptions”?

P 11177, line 11 – this sentence is formulated somewhat confusing. I guess that plotting Cn2n2 versus β “reveals” the minimum, rather than letting it “occur”.

P 11177, line 16-18 – “In practice, (...) of the instrument.”, this sentence is vague. Please reformulate. So far as I get it, there is a region, where Cn2n2 is biased due to the bad SNR. Is that correct?

P 11177, line 19ff – do the authors describe a new aspect of the above presented equations from “For low β, (...)” onwards? If not, then it is unclear to me how the Cn2n2 minimum relates to these last sentences.

P 11179, line 20ff – METsub was installed at a height of 10m a.g.l. Could the authors elaborate whether these measurements were scaled to fit the scintillometer effective height?

P 11181, line 26 – $\sigma\chi 2$ does not depict the covariance, maybe the authors can add $\sigma\chi 1\chi 2$ (c.f. Eq. 14c)? Furthermore, the relation between the intensity measurements and the log-amplitude (co)variances does not become clear from this paper. I think it should be shown that var(ln(I)) = 0.25var(ln(χ)), or change it in the equations altogether.

P 11182, line 21-24 – “For the MWS (...) along the BLS-MWS path.”, does fog also affect the MWS signal?

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P 11182, line 27 – what do the authors mean with “reasonable thresholds”?

P 11184, line 1 – could the authors elaborate on how well this methodology works during clouded weather or during winter time?

P 11185, line 14-15 – only experts will get what the authors imply here. I think they mean the path averaging over the licor and sonic sensors and their respective separation distance? Be more specific and give the corresponding references (Hill, 1991, Phys. Fluids A 3, 1572-1576 and Hartogensis, 2006).

P 11186, line 4 – “sharp minima”, sharp minima are not visible from the plots of figure 5a or 5b. The point the authors try to make in line 4-5, can only be illustrated when the corresponding figures have their y-axis logarithmically scaled. Hence, I would recommend to scale the y-axis of Fig 5a and 5b logarithmically, rather than linearly.

P 11188, line 9 – “means evaporation”, these two words seem to interrupt the flow of the sentence and work confusing to me. I would recommend suitable punctuation for readability.

P 11190, line 20 – add a comma after “theory”.

P 11193, line 16-17 – recommend to rewrite “higher in the atmospheric boundary layer than is ideal,” to read “to be above the surface layer,”

P 11193, line 19ff – Solignac (2012) indicates that high-pass filters may artificially reduce Cn2 at low crosswinds, i.e. cause underestimation of Cn2. To what extend do the authors think that their filter of 0.06 Hz affects the estimates of Cn2n2 and Cn1n2? This is an aspect that needs to be discussed at latest in this paragraph.

Another issue related to low crosswinds and recently analyzed in Lindenberg is the following: At low crosswind, the friction velocity usually is low as well, indicating that the inner scale length is large. Hill and Clifford (1978) indicate that $D/l0 > 20$ suffices for ignoring the spectral bump in the spectrum. Nevertheless, for the LITFASS set-up (4.8 km path, 43 m a.g.l.), a positive bias of Cn1n1, resulting from ignoring the l0-depence

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of the spectrum, is as large as 30% for u* going to zero. The issue is described in more detail in Hartogensis (dissertation from 2006), appendix 5a. The effect of ignoring the bump on Cn1n2 and Cn2n2 is negligible. This information is just given here for consideration, and it would be great if the authors could elaborate their thoughts on it.

P 11197, line 12 – add “of the first kind and zeroth and first order.” After “Bessel functions”.

P 11198, Eq. A1 – the term J0(K|d|) is only valid when the receivers and detectors of both scintillometers are identically separated at each side, otherwise the term should become J0(K|dt(1-x/L)+ dr(x/L)|), see Lüdi et al., Eq. (9) or Hill and Lataitis (1989) Eq. (1).

P 11198, line 6 – I recommend removing “is often applied”.

P 11198, line 11 – change “size of the Fresnel zone” to “maximal diameter of the first Fresnel zone”

P 11198, line 11 – the variable “F” has not been defined before. Furthermore, I recommend giving its definition as well “F = $\sqrt{(\lambda L)}$”

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 11169, 2014.