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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Infrared and millimetre-wave scintillometry in the suburban environment – Part 2: Large-area sensible and latent heat fluxes" *by* H. C. Ward et al.

B. van Kesteren (Referee)

Bram.van-Kesteren@dwd.de

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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 ad-

dressed many technical issues, whereas the second part focuses more on the retrieval of turbulent fluxes and environmental aspects. The authors have shown a great competence and insight in the methods and understanding of scintillometry at the one hand and the urban environment at the other hand. The manuscript is generally of a high scientific quality, presenting innovative results, and very well written, so that I recommend publication after some minor revisions.

Some comments from Part 1 were found to be relevant here as well and are simply copied.

P 11222, line 3 and elsewhere – "evaporation" seems to be used throughout this paper as a synonym for "evapotranspiration", but not always. For accuracy, I would prefer the term "evapotranspiration" where applicable, or a definition at the beginning of the paper explaining what the authors think of when using the word "evaporation".

P 11222, line 4 – to what does "this technique" refer?

P 11223, line 23 – "refraction", it results from diffraction. Rewrite.

P 11223, line 21-25 – this is a very long and complex sentence. Please simplify.

P 11224, line 23 – it seems RPG sells the microwave scintillometers by now.

P 11225, last paragraph – the time frame of the experiment should be mentioned in the introduction, as well as the notion that seasonal variability is addressed in the paper. This introduction much better gives the objectives of this paper, than was the case in Part I. Nevertheless, the authors should better present the research themes.

P 11226, Eq. (2) – please, give a reference for this equation.

P 111226, line 20-21 – the authors should give some more information on their methodology regarding the retrieval of the friction velocity. This is a very general statement. Probably, the Kansas functions were used (Businger-Dyer)?

P 11227, line 2 – Braam et al. (2014) also gives a very useful assessment on methodology, measurement height, instrumentation and other aspects influencing the similarity functions.

P 11227, Eq. (5) – Ward et al., (2013b) gives a different definition in their Eq. (17). What happened to the 1/(1-q) term here?

P 11228, line 13 – the authors could give a reference to Part I here.

P 11229, line 9-20 – there is rather much repetition here as compared to Part I. Could it be shortened? In any case remains the question whether the authors could elaborate on the issue regarding the meteorological measurements being scaled to fit the scintillometer effective height?

P 11230, line 27 – add a comma after "fluxes" for readability.

P 11232, line 21 – recommend to change "two-wavelength structure parameters have been" to "the two-wavelength method has been"

P 11236, section 4.1.3 – at latest here, it would be good to mention that scintillometry is an indirect method for measuring fluxes (Wyngaard and Clifford, 1978).

P 11236, line 6-8 – the Meijninger studies did rely on absolute humidity instead of specific humidity and hence probably made some error (see Ward et al., 2013). Furthermore, Meijninger 2002 uses De Bruin similarity functions, whereas in 2006 they used the mean value of the Andreas and De Bruin functions; therefore, comparing these studies is a bit tricky. Lastly, they ignored the humidity effect on the Obukhov length. To conclude, analysing of these values must be done with care, even though the authors are right in addressing the uncertainty in the EC values.

P 11237, line 1 – so, the authors rule out the effect of the anthropogenic flux?

P 11237, line 13-15 – the values of DB93 and An88 are smaller than observed. Interesting to observe here is that Thiermann and Grassl (1992) seem to fit much better for neutral conditions. They derived their formulation from the variance-budget equations,

see e.g. Wyngaard and Kosovic (1994) or Andreas (1988) – his Eq. 4.30. Assuming phiT = phiM = 1, yields for z/L = 0, the value 6.37. This value has some clearly defined theoretical assumptions behind it (e.g. horizontal homogeneity, no advection . . .). Hence, it would be worth mentioning and discussing some of these details I think.

P 11239 – line 5-10 – "rejection of QE when IRGA windows are wet results in underestimated EC fluxes" and "(b) suffers a bias to "count" QE < 0 (dewfall) as QE> 0" . Are not the IRGA windows wetted as well during dewfall? This would lead to an overestimation of QE in case these situations are omitted from the analysis, wouldn't it?

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