

Interactive comment on “Observing crosswind over urban terrain using scintillometer and Doppler lidar” by D. van Dinter et al.

Anonymous Referee #3

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Review of amt-2014-131: Van Dinter et al., 2014

The research presented in this manuscript aims to validate the path-averaged crosswind obtained by a scintillometer over urban Helsinki. Validation is done with use of a Doppler wind-lidar and with two sonic anemometers. The authors claim that the challenging aspect in this study is the highly complex surface, which causes considerable deviations from the idealized homogeneous flow (for which both the lidar and the scintillometer have been validated before). As such the systems are pushed to their limits. After consideration of the data, they decide on omitting several cases and conclude that both methodologies from the scintillometer seem to work reasonably well, albeit that the lookup-table method has space for improvement. Also the lidar is judged as to perform well enough for its validation purpose.

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The set-up of the experiment, which has been performed within a larger framework, offers the unique possibility of validating crosswind estimates from scintillometry and wind lidar. These possibilities seem to have been weakly exploited. The authors do not convincingly show that they are truly considering a heterogeneous flow. Data that possibly indicates heterogeneity are omitted based on measurement-error considerations. Furthermore, in depth analysis of the data seems to be missing – or is only briefly addressed in a rather speculating way. Nevertheless, some sections positively stand out, e.g. large parts of the methods or the beginning of section 4.2, showing the competence of the authors. Unfortunately, the language of the manuscript is rather sloppy and imprecise, and several issues/sentences/words are frequently repeated. Considering the amount of native speakers on the author list, this raises the question whether the final version of the manuscript has been seriously read by others than the first author at all?

Summarizing the above, to my opinion the manuscript does not have sufficient quality at the moment, but I see its potential of being greatly improved within a limited period of time. Therefore, I recommend the manuscript for publication after serious consideration of the comments given below. The more fundamental comments will be given first, followed by the more minor comments.

Specific comments:

Analysis of heterogeneity

It is not fully sure if the large differences observed in Fig 2 are impossible or not. In such complex flows as over cities there are many windward and leeward eddies, turbulent vortices and all kind of very complex local flow dynamics. One easy way of tackling

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this problem is considering the radial wind along the scintillometer path instead of the crosswind for the same period. I believe these data are available to the authors and the methodology of determining this V_r is much more straightforward than determining the crosswind. In this way Fig 2 can be reproduced for the radial wind and from this assessment we can learn what kind of variability is possible in this area, even though the whole problem is turned by 90° . As such it can more objectively be judged if that what is seen in Fig 2 is real or not.

Additionally, P6443-6444, line 19-19: concerns figure 3: the authors should realize that identical plots can be made with respect to the south anemometer and with respect to the north anemometer, i.e. south or north anemometer on the x-axis vs. scintillometer on the y-axis. Only, if these plots are significantly worse, i.e. yield significantly worse statistics, than Fig 3; the authors have a point in arguing that the averaged values of the lidar and the scintillometer are real averaged values of the wind field. When these plots are comparable, this conclusion is seriously undermined – or in any case showing that the wind field is more or less homogeneous, not heterogeneous. Hence, such an analysis should be presented.

P 6447, line 16-17: “From the analysis of these four cases, it follows that the present cumulative spectrum method is better equipped to obtain U_{cross} than the lookup table method” – this conclusion is opposite to the conclusion drawn in the sentence on P 6444, line 26-27: “the lookup table method showed the best results, with the lowest RMSE and scatter”. So the first question is what method finally is the best one?

Furthermore, it raises the following questions: What do these opposite conclusions say about a) the representativeness of the 4 cases, and b) the variability of the crosswind along the path in the majority of cases in Fig 3? Now, I am tempted to conclude that for the majority of the data in Fig 3 the crosswind might not be as variable along the path as the authors say it is. This, in turn, gravely affects the main conclusions of the paper, because when the authors in fact consider a more or less homogeneous wind field, the main conclusion would be that the wind field over a city is in fact not as complex as is

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always thought. Please clarify!

Data omission

P 6441-6442, line 16-2: The authors seem to think from the idea that the crosswind estimates of the lidar may be faulty when the wind is mainly perpendicular to the path. Hence, they argue that the blue and orange parts in Fig 2, must be faulty and should be rejected, because it is unrealistic that such large differences in crosswind exist over such short distances. Hence the data are taken out. However, that what remains is clearly a less heterogeneous field. In fact, when pushing it to the limit, I could argue that at the end they remove as much data as to be left with a more or less homogeneous wind field. My question is: what is the effect of this on the conclusions? Are the authors truly looking at a heterogeneous wind field or are their conclusions based on a cleaned, but therefore homogeneous wind field?

P 6442, line 3-16: the church tower seems to cause challenges to the measurements. However, it is said that the church tower is somewhat to the east of the Doppler-lidar path. First, I wonder whether it is in- or outside the scan area 174° - 196° ? In case it is inside, it will cause heterogeneities in the estimated wind field all the time – regardless whether concerning the crosswind or the radial wind. When it is outside, it is of high importance to mention the upwind or downwind position of this tower. Only when the lidar and scintillometer are downwind of it, the tower will cause heterogeneities. Nevertheless, the same comment remains as mentioned before, what are the results worth when all the heterogeneities are removed?

In other words, must these data really be removed?

Effect of a variable wind field on the methods

Eq. (1), Eq (3), P 6435, line 17-20; P 6445, line 20-26: mathematically, $U(x)$ is a

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variable just as any other in Eq. (1) and (3). When $U(x)$ is constant over the whole area of interest, i.e. the scintillometer path, the solution to this equation is greatly simplified and can be obtained. In case of a variable $U(x)$ with x , the solution is much more difficult to obtain (if it can be obtained at all). The authors start a discussion on this issue using the 4 cases, but a more solid foundation could be found in a theoretical analysis. Some simple variable formulations could be a linear de-/increase or some kind of sinusoidal function, e.g. for $0 \leq x \leq 1$, $U(x) = 3+x$, $U(x) = 3+\sin(\pi x)$, or $U(x) = 3+\sin(2\pi x)$. By solving Eq. (1) and (3) for these, or other kind of variable winds, the authors readily get answers to what extend these simplified, but variable wind fields affect the methodology. To a certain extent this analysis may be similar to the one performed by Ward et al., (2011). In any case, the conclusions drawn on P 6445, line 26 and P 6446, line 25-26: are mathematically too obvious, and this in depth analysis may give more solid results.

P 6446, line 16-17: The authors relate the variability of the crosswind to the standard deviation along the scintillometer path. To me it seems they overlook the kind of variability that may occur along a path, e.g. linear, sinusoidal, exponential – similar issue as mentioned above.

subaveraging

P 6442, line 25-26: “a moving average of 5 points was applied” – do the authors mean that they average over 5 range gates, i.e. 120 m? What is the justification for that and to what extend does it affect the results?

Generating statistics

P6443-6444, line 19-19: regarding the results from the lookup-table method (Fig 3b), I consider the better statistics (correlation and RMSE) to be an artefact of not taking

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absolute values. As the cumulative-spectrum method uses absolute values, whereas the lookup-table method does not, the authors are somehow comparing apples and oranges. It is important to mention that the lookup-table method has the advantage that it can determine the sign of the crosswind, but for doing this qualitative comparison with the cumulative-spectrum method absolute values should be used as well (see also P6444, line 27-28).

Variable crosswinds along the path

Section 4.3 is generally lacking comparison to other sources in literature and the putting in context of the own research, please improve on this.

Justification of the study in the introduction

In the introduction P 6432-6433, line 25-4 and P 6433, line 10-12 form the justification of the study. However, at the moment it is fairly thin. The authors could elaborate on the roughness sublayer, properly introduce it, and give more insight why a simple point measurement would not be sufficient, i.e. explain the need for truly averaged crosswind estimates obtained the scintillometer (in cities and alpine environment - or other environments as well). At this moment, Wood et al., (2013c) and Poggio et al., (2000) are the only references for this, and they are merely quoted for stating that there is a need for such estimates.

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Minor comments

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Abstract

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P 6432, line 1: change “is measured” to “has been measured”

P 6432, line 2: delete “the urban environment of”

P 6432, line 6: what do the authors mean with “applicability”?

P 6432, line 8: to what does “also” refer in “it can also be used”?

P 6432, line 6-9: “The goal of this study is (...) be used in the urban environment.” – these sentences can probably be merged together.

P 6432, line 10: delete the comma after the closing bracket: “spectra),”

P 6432, line 11-12: methods do not compare to measurements: reword.

P 6432, line 12: “Doppler lidar measurements” – just as scintillometers lidars measures irradiances (in this case of a backscattered signal), wind velocities are derived from these measurements and are no more than estimates.

P 6432, line 12-15: “the challenging urban environment” “the complex urban environment” – delete one of the two instances.

P 6432, line 13: RMSE is undefined.

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Introduction

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P 6432, line 20: “general application” – this is only valid when talking about micrometeorology, in e.g. the optical related sciences the structure parameter of the refractive index is much more important.

P 6432, line 21: “The path can range from (...)” – this depends on the scintillometer in question, which in the present formulation remains unclear.

P 6432, line 22-23: “In this study the focus is on another application of scintillometers,

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which is the path-averaged crosswind (...)” – is the focus in this study on another application of scintillometry (whatever that may mean) or is its focus on the path-averaged crosswind or even on obtaining the path-averaged crosswind? In the latter cases you can delete “another application of scintillometers, which is”

P 6432, line 25: change “a path” to “the scintillometer path”

P 6433, line 8: delete “scintillometer measurements”

P 6433, line 9: change “on these sites” to “at such sites”

P 6433, line 8-10: The validation of (...) along the scintillometer path.” These two sentences keep hanging in the air for me, because I miss a logical deduction that follows from them.

P 6433, line 10, 13: the occurrence of “however” two times so closely after each other makes the text jumpy

P 6433, line 17, 18, 19, 21 and elsewhere: “measurement” – this refers to irradiance measurements done by the scintillometer (and the lidar) – all other variables are derived from these and are “estimates”.

P 6433, line 19: change “is variable” to “are variable”

P 6433, line 21: “is measurements” put either in plural or in singular – furthermore, measurements cannot be estimated (see the verb at line 22).

P 6434, line 4-5: In contrast to point i point ii is unclear to me – what is problematic about S11 and r12 being influenced by a variable $U(x)$ (see also in the specific comments)?

P 6434, line 10: “sonic anemometer measurements”: a reformulation is needed here. The study aims at validating crosswind estimates under heterogeneous wind conditions. However, local wind measurements like those from sonic anemometers are not suitable for this purpose.

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Theory and Methods

Scintillometry

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This section needs a careful revision for language. Some hints

P 6434, line 18: at latest here it should get clear that the study is dealing with optical large-aperture scintillometers.

P 6434, line 18-19: “light with a certain wavelength” – a scintillometer does not generally emit light, as a matter of fact only the surface-layer scintillometers from Scintec do so. Chance to “near-infrared radiation”

P 6434, line 19: “refracted” – the radiation is “diffracted” not “refracted” – “scattered” is another, more general alternative

P 6434, line 19-20: “The eddy field in the atmosphere is turbulent” – sloppy use of language. There is no such a thing as an eddy field (for scintillometry the refractive-index field is relevant) and eddies per definition indicate that the flow is turbulent, because eddies do not exist in laminar flow.

P 6434, line 20-21: “measures intensity fluctuations” – reformulate – technically it does not: the receiver measures the intensity of the incoming beam, this intensity fluctuates, which results in a standard deviation unequal to zero when analysing the measurements.

P 6434, line 21-22: “When Taylor’s frozen (...) in the eddy field” – some comments on this sentence:

- Reformulate “eddy field”
- “only driver” – what about buoyancy? “main driver” is probably better

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- Is the thesis brought here really dependent on the premise that the frozen-turbulence assumption is valid? See e.g. Potvin et al., 2005 Optical Engineering 44

P 6434, line 23: “scintillation signal” what is meant with this? Scintillation has not been put in a context before

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Scintillation spectra

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P 6435, Eq. (1): why is D not defined in terms of Dr and Dt – as is done in Eq. (3)?

P 6435, line 15: add “of the first kind” to “first-order Bessel function”

P 6436, line 1: “(...), which are the frequency (...)” it sound like a general truth, whereas I guess it is not. Reformulate

P 6436, line 5-6: change “(...) a constant for which the value is determined from the theoretical S11(f) (Eq. 1), (...)” to “(...) a unique constant for a given experiment that can be derived from Eq. (1) (...)” or something similar

P 6436, line 7: “The five different U-values (...)” add a “subsequently” or something similar at the beginning of this sentence for readability.

P 6436, line 8-9: “In this study (...) when U(x) varies.” – I think this sentence need to be reformulated, because from the mathematics it is known that CCS cannot be constant when U(x) varies. Probably the authors are more interested to what extend the formulation could still be used?

P 6436, line 13: switch “are” and “in this study”

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Time-lagged correlation function

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P 6436, line 19: change “spatially separated transmitters and receivers” to “horizontally displaced beams”

P 6436, line 20: “the sign” – the sign of what? Do the authors mean the crosswind direction?

P 6436, line 23-24: “dual-aperture” and “single-aperture” – probably “beam” is a better word than “aperture”.

P 6436, line 24: “more widely available” – how do the authors know this and is it really the point they want to make here? The BLS900 and the SLS-20/40 from Scintec are quite widely spread and both are dual or displaced-beam scintillometers. To me it seems to be more a cost issue. However, when bringing it to the point the real statement that matters, is that the S11(f) method is more generally applicable (because it works for both single- and displaced-beam scintillometers).

P 6436, line 26: “when frozen turbulence is assumed” . This statement is very similar to the statement on P 6434 line 21-22, although it is more clearly formulated here. Nevertheless, the point remains that too much emphasis is put on the premise here. The assumption of frozen turbulence is not that important, fact is that for the relative short separations of most displaced-beam scintillometers the turbulence does not change enough to prevent a peak in the temporal correlation function to be discovered. Be more clear in formulating this.

P 6437, line 7: add “of the first kind” to “zero-order Bessel function”

P 6437, line 8: “at location x on the path” – is this a relative or an absolute measure. In case it is relative, it is identical to x in Eq. (1) and does not need to be repeated here. In case it is not, another symbol should be used, because it is highly confusing with Eq. (1), where x is used for the relative position on the path.

P 6437, line 8-9: the definitions of D_r and D_t do not need to be repeated here, when
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they are introduced properly at Eq. (1).

P 6437, line 10: change “is given by” to “is obtained from”

P 6437, line 14: “related to” – vague, probably the authors mean “equal to”

P 6437, line 16: “is assumed to be constant.” – add “along the scintillometer path”

P 6437, line 16-18: “In order to obtain U , (...) of the lookup table.” – I do not get this sentence; probably it is related to the use of the word “given” at a position where I do not expect it. Reformulate.

P 6437, line 19: “The effects of having (...)” this sentence should start at a new line, because a new paragraph starts here.

P 6437, line 21: “(...) 139 steps of x with different values for $U(x)$.” – cryptic formulation. Do the authors mean that integration is done over each range of the 139 range gates of the Doppler lidar and that for each of these gates a different crosswind is assumed? Reformulate.

P 6437, line 23: “are averaged to 10 min.” – is this averaging done arithmetically?

Doppler lidar

P 6438, line 6-9: “In the returned signal (...) radial or along-beam wind).” – the measurement principle of the Doppler wind lidar is based on “heterodyne detection”, i.e. comparison of the originally emitted signal with the backscattered signal (which has the Doppler shift compared to the original signal). This should in any case be included in the description.

P 6438, line 9: “However, in this study (...)” – this sentence should start at a new line, because a new paragraph starts here.

P 6438, line 10-9: "(...) given that the Doppler lidar was located near the receiver of the scintillometer." – This clause does not make sense to me.

P 6438, line 13-14: change "where" to "which determines" and delete "can be estimated"

P 6438, line 12-15: "The required wind component (...), from which $U(x)$ can be determined." – this sentence is quite complex; probably the last clause can be fully deleted.

P 6438, line 17: "beam-pointing directions" add "in the horizontal plane" .

P 6438, line 20,24: is b or is θ_b the azimuth angle of the beam? I would suppose θ_b , where b is one of the two beams in question (this is implicitly stated in Line 25 where $V_{g1} = V_{g2}$). Can the authors clarify this?

P 6439, line 1-2: The authors argue that the implicit assumption of homogeneity may result in errors. This is true, but to my opinion it is not the complete part of the story. Also the angle between the beams is of relevance; the smaller the angle between the beams, the larger the propagation of uncertainties to the final wind estimates is. This is an aspect the authors should, for their set-up, at least mention briefly.

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Experimental setup

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P 6439, line 6: delete "the" before "1" and before "15" or add "st" and "th" to the numbers.

P 6439, line 9: change "BLS900 of Scintec (Rottenburg, Germany)" to "BLS900 (Scintec, Rottenburg, Germany)"

P 6439, line 14-16: the heights that are presented here by the authors (67 m and 52.9 m) are kind of non-information. More relevant heights are the average height above

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ground level, the building height and the average displacement height of the buildings; that is the information needed for knowing at what height the wind was determined.

P 6439, line 16: "near north-south-axis" – do the authors mean "nearly north-south"?

P 6439, line 21: change "each 4 s" to "every 4 s"

P 6439, line 21-22: Do I get it right that the lidar basically has been run in VAD modus at an elevation of 0.45° , and measurements at 10 different azimuths angles once every 5 minutes? This would mean that for each 10-min average there are 2 samples. Is that correct? So far it has remained unclear to me, please formulate more precisely. Furthermore, the important information is not that the ray lasts for 1 s, but how many samples are averaged over this period of time.

P 6439, line 24: "line-of-sight issues" – I assume that these issues have become obstacles in the wind field. To what extend do they cause problems?

P 6439, line 26: "4095m" – why is more than 100 m of lidar data rejected? The scintillometer path is 4.2 km long, isn't it?

P 6440, line 2: change "can" to "could"

P 6440, line 5: "scans" – what is meant with this? Did the lidar scan between 174° and 196° ? Or were the measurements done at these azimuth angles?

P 6440, line 8-9: delete "unit at Hotel Tornio" and "at the so-called SMEAR-III-Kumpula station" or introduce these two measurement sites properly.

P 6440, line 14: change "by each of the anemometer" to "by each of the anemometers"

P 6440, line 14-22: it seems to me that this would better fit in the methodology than in the experimental setup.

P 6440, line 19: "weight-averaged" – averaged over weight? It seems to be a slightly unlucky formulation.

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P 6440, line 21-22, change "(...) were available along the first 139 range-gates (i.e., corresponding to the scintillometer path)." to "(...) were available along the scintillometer path."

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Results and discussion

Doppler lidar path-resolved crosswinds

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P 6441, line 2: change the "–" to a "comma" or the "comma" to a "–"

P 6441, line 5, 9: "south anemometer" "north anemometer" – which of the two are we talking about?

P 6441, line 13-16: "It should be noted that (...) reasonably low in these conditions." – it is unclear to me what follows from this statement or what the authors want to say with it.

P 6441, line 21-23: "besides being parallel to the path," delete this clause, it is mentioned before in line 20 and the crosswind cannot be parallel to the path. . . An example how this sentence can go more fluently and be shortened: "Therefore, the corresponding U(x)-values are still moderate ($\leq 3 \text{ m s}^{-1}$)."

P 6441-6442, line 16-2: This section is quite wordy and should be written more concisely.

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Experimental setup

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P 6442, line 2: delete "even"

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P 6442, line 5: "by the large divergence" – are the authors implying that the divergence is getting problematic from a certain distance onward? In that case it is not clear to me why only the 2000-2500 m path is affected.

P 6442, line 8: delete "also" and switch "has" and "apparently"

P 6442, line 19-20: "To be included (...) Doppler-lidar data." – repetition; delete this sentence.

P 6442, line 21: change "This resulted in (...)" to "The exclusion resulted in (...)"

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Path-averaged crosswinds

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P 6443, line 2: "are" and "that" – make consistent, either plural or singular.

P 6443, line 4: "measuring exactly the same atmosphere" – do the authors mean "sample the same part of the atmosphere" ?

P 6443, line 6: delete "(given their difference in heights)"

P 6443, line 8: delete "(excluding the 2000-2500 m of the path)"

P 6443, line 8: change "using" to "taking into account"

P 6443, line 3-16: "The theoretical difference between (...) while in stable conditions the difference should be more" – this part is quite wordy. The authors could concisely say something like: "The height difference between the scintillometer and the lidar causes a negligible difference in the crosswind estimates. Assuming a neutral wind profile, this difference is merely 1.1%, which assures that even under the complex urban conditions of Helsinki the differences are unlikely to be larger than ...%."

P 6443, line 16-18: "For the scintillometer, (...) are used." – Repetition; delete this sentence.

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P 6443, line 23: how is the “path-weighted standard deviation along the scintillometer path” defined?

P 6443, line 27: change “difficulty in” to “difficulty of”

P6443-6444, line 19-19: These two paragraphs are generally well written, give clear results and compare the own result to values from literature. Well done!

P 6444, line 1: “reasonably” – this is a vague term, which can be interpreted in many ways. Besides this occurrence, the authors use it in 6443, line 21; 6555, line 5, 6, and 7 – the frequency of which makes it annoying.

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Variable crosswinds along the path

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P 6445, line 2-11: this section is very difficult to read and in many ways it is unclear what the authors want to express here. All the information I read here is present in Table 1, where it is visible at once what the highest values are. Personally, I tend to say that the crosswind is low in case C, then come A and B, and D is largest. The standard deviation is similar for case A,B, and D, and clearly higher for case C – which may be surprising due to the low average crosswind speed. In any case, that says everything in just two (in this case badly written) sentences. . .

P 6445, line 5-6: I do not find an average crosswind of 3.3 and 3.9 m s⁻¹ reasonably high. Maybe these values are high compared to the two other cases, but that should be mentioned.

P 6445, line 19-20: “We first focus on (. . .) panels of Fig. 4).” – delete this sentence, it is a copy of the sentence on line 12-13.

P 6446, line 1: “CSscint and CSvarU” – according to the definition on P 6445, line 16-17, this must be changed to “scint and VarU”, because the CS method as defined

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by the authors cannot be applied to the cumulative spectrum of these two, can it?

P 6446, line 3: switch “holds” and “also” and add “then” before “the value of U of the Doppler lidar”

P 6446, line 4: “For case D this is indeed the case” – the double use of the word “case” is a bit unlucky, because it has two different meanings.

P 6446, line 6: I think “However” should start at a new line, because here a new topic starts.

P 6446, line 6-13: The section is quite wordy for that what needs to be said.

P 6446, line 19-22: “However, the magnitude of r_{12} does not influence U obtained by the lookup table method, (. . .)” – first the authors tell that something stands out and then they say it is not important. If it is not important, then mention that the magnitude is not important, but do not mention that it stands out – such a formulation only causes confusion.

P 6446, line 22: add “does” at the end of the clause “but the shape of r_{12} .”

P 6446, line 24: “For all four (. . .) better than r_{12} .” – for case A and B I could argue the opposite.

P 6447, line 1-2: I do not get this conclusion. Isn’t this only the case when the peak of the correlation function moves to another position? In other words, I would think this does not happen upon broadening of the correlation function. Upon further reading I encounter P 6447, line 6, where it is written that “the peak in r_{12} also changes location”. Isn’t that the crucial fact in the whole story? My assumption is that the look-up table method functions on searching τ for which r_{12} is largest.

P 6447, line 3-4: “For case C and D the error is reasonably high with a value of 0.8 m s⁻¹” – how can I deduce the error from table 1? It is unclear to me how this error is defined and how the authors derive a value of 0.8 m s⁻¹ for both case C and D.

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P 6447, line 10-12: "Therefore, by also including (...) can be improved." – unclear language, reformulate.

P 6447, line 18: delete "also"

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Conclusions

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P 6448, line 10: "albeit with scatter" – this is vague terminology

P 6448, line 11: change "Still given" to "Still, given"

P 6449, line 13: change "indicates" to "indicate" Throughout the paper: "RMSE" – root mean squared error is maybe not an ideal terminology in this case. The authors do not know the true wind field. Therefore, I propose to use the terminology root mean squared difference: "RMSD".

P 6449, line 20, 22: "-0.2 m s⁻¹" and "-0.8 m s⁻¹" – where does the minus sign come from? Probably, these signs must be left out. Change accordingly.

P 6449, line 22-24: change "The lookup table method can, however, be adjusted by also including heterogeneous wind fields in the lookup table method;" to "The lookup-table method can however, be adjusted to include heterogeneous wind fields;"

P 6449-6450, line 26-3: How can this idea suddenly appear in the conclusion? To me it seems more an item of discussion and moreover, the idea has already been presented by Andreas (2000) "Obtaining Surface Momentum and Sensible Heat Fluxes from Crosswind Scintillometers" Figure 14, section 4. So, at least, refer to it. Furthermore, this paper could provide more relevant information to the authors.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 6431, 2014.