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Atmospheric Measurement Techniques Discussions

Interactive comment on "Observing crosswind over urban terrain using scintillometer and Doppler lidar" *by* D. van Dinther et al.

D. van Dinther et al.

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Anonymous Referee #3

Received and published: 24 September 2014 Reply: We would like to thank the reviewer for his or her valuable comments, which greatly improved the manuscript. In the revised version of the manuscript we tried to accommodate all the suggested modifications. Below, we repeat the reviewers' comments in and our replies follow. Note that in this reply U denotes the crosswind. The supplement is a zip-folder with the follwing 2 pdf-files: (1) showing the difference between the before submitted manuscipt and the new one and (2) the revised manuscript.

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. 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 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 Vr 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 90âŮę. As such it can more objectively be judged if that what is seen in Fig 2 is real or not.

Reply: It is true that the same analysis can be done for the radial wind. However, when doing so only one of the Doppler lidar beams can be compared, while in order to obtain U(x) correctly the radial winds of both Doppler lidar beams not to be correctly measured. We, thus appreciate the suggestion of the reviewer but decided not to include it in the manuscript (also due to space issues, since two other figures are already added by the suggestion of reviewer 1).

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.

Reply: The intention of this study was to investigate the capability of the scintillometer to estimate U in an environment where the wind field is heterogeneous. Essential for this study is therefore measurements of U along the scintillometer path. To our knowledge estimates of the wind field along a path can only be made by Doppler li-

dar. Comparison of scintillometer measurements with sonic anemometer have been done in other studies (e.g., Wood et al. 2013). Therefore, we decided to focus on the comparison between estimates made by Doppler lidar and scintillometer.

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 Ucross 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?

Reply: There is indeed a difference in which method is better in the direct comparison between Doppler lidar and scintillometer and the four cases. For the comparison the lookup table shows slightly better results with a lower scatter (R2 of 0.53 compared to 0.47) For the four cases the cumulative spectrum showed better results with more similar estimates of U as the Doppler lidar than the lookup table method. In the end overall the results are reasonably similar for both methods. Given the fact that from the lookup table method also the sign of U can be estimated and U can be estimated over a short time period (âĹij 10 s) we think this method is better than the cumulative spectrum method.

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 always thought. Please clarify!

Reply: Fig. 5 (in previous version Fig. 3) indicates that in the analysis also data are considered where U(x) is heterogeneous, judging by the high standard deviation (>2.5

m s-1 for 50 % of the time)

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.

Reply: We only use the standard deviation to quantify the variability of U along the path, the assumption of normal distributions (as necessary for outlier detection) is not necessary. 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?

Reply: A moving average is averaging in this case 5 points while stepping through the data, in other words in this case a point is averaged with its 2 neighbouring points on either side. Therefore, you end up with a dataset with still different values for every 30-m range gates, although the results are smoothed (extremes are taken out). This is a course of action that we took at first when we saw that the estimates of U(x) of the Doppler lidar beams fluctuated extremely vigorously for some cases (as shown in Fig. 2 of the paper). However, later we looked more critical for which instances the Doppler lidar data could not be trusted (as described in Section 4.1 in the manuscript). Thus the moving averaging was not necessary anymore, and this statement should not have entered the manuscript. Therefore, in the new version of the manuscript this sentence is deleted, sorry for the confusion.

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 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).

Reply: When U of the lookup table method is taken as an absolute values the regression statistics are as follows: RMSE=0.73 m s-1, y= 0.76x+0.83 and R2=0.53. This is indeed somewhat worse (higher RMSE and lower R2) than when looking at non-absolute values. These values and the comparison with the cumulative spectrum are now mentioned in the manuscript.

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.

Reply: To our knowledge it is the first time that a measured U(x) field is used to calculate the theoretical S_11 (f) and r_12 (τ) and thus the influence of a variable U(x) on the estimated U by the scintillometer. Thus it is not possible to compare to other sources in literature.

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.

Reply: As mentioned in the introduction the wind speed and direction are variable in an urban environment. Anyway, we added more references for the reader. And also we reduced emphasis on the "roughness sublayer" as a term, given the ongoing live discussion on its definition.

Minor comments Abstract P 6432, line 1: change “is measured” to “has been measured” Reply: Corrected.

P 6432, line 2: delete “the urban environment of” Reply: We think it is good to emphasize that the measurements were taken in an urban environment. Therefore, we kept “the urban environment of”.

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

P 6432, line 8: to what does “also” refer in “it can also be used”? Reply: The also is removed.

P 6432, line 6-9: “The goal of this study is (. . .) be used in the urban environment.” – these sentences can probably be merged together. Reply: The line “If the scintillometer... ...urban environment” is removed, as suggested also by reviewer 1.

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

P 6432, line 11-12: methods do not compare to measurements: reword. Reply: Changed to “The values of U of both method ...”

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. Reply: The U measurements are now referred to as U_âŁě estimates.

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

P 6432, line 13: RMSE is undefined. Reply: RMSE is now defined.

Introduction 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. Reply: ”in micrometeorology” was added to the sentence.

P 6432, line 21: “The path can range from (. . .)” – this depends on the scintillometer in question, which in the present formulation remains unclear. Reply: “depending on the type of scintillometer used” is now added to the sentence.

P 6432, line 22-23: “In this study the focus is on another application of scintillometers, 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” Reply: Changed to “In this study the focus is on obtaining the path-averaged value of the crosswind from a scintillometer”

P 6432, line 25: change “a path” to “the scintillometer path” P 6433, line 8: delete “scintillometer measurements” Reply: Corrected.

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

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. Reply: These two sentences are mend to emphasize that so far validation studies occurred on flat grassland sites. The link to the other sentence is that although the validation are done for homogenous U there is also a need for scintillometer U in complex terrain (which we investigate in this study). To emphasize the link the however in sentence 10 is changed to despite that and it is specified that the U measurements are made by the scintillometer.

P 6433, line 10, 13: the occurrence of “however” two times so closely after each other makes the text jumpy Reply: At line 10 “However” is changed to “Despite that, . . .”

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”. Reply: The U measurements are now revered to as estimates

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

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

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)? Reply: The problem is that the scintillometer retrieval algorithms do not take the influence of a variable U(x) into account. That the algorithms do not do so is now added in the text.

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. Reply: This section is reshuffled, it now reads: “The main goal of this study is to investigate the performance of the scintillometer to measure U in conditions where U (x) is variable. In order to do so, scintillometer estimates of U are compared to estimates that of the Doppler lidar. However, also for the Doppler lidar the heterogeneous wind conditions are challenging. Therefore, before the scintillometer and Doppler lidar U estimates are compared to each other the applicability of the Doppler lidar to estimate U(x) is investigated by comparing with sonic anemometer measurements.”...

Theory and Methods Scintillometry 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. Reply: The following sentence is added to

the section “In this study, a large aperture scintillometer is used of which the transmitter emits near-infrared radiation”

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” Reply: Changed to radiation (in this study, near infrared)

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

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. Reply: Changed to “The atmosphere is turbulent, leading to an eddy field which constantly changes.

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. Reply: Changed to “The intensity measured by the receiver, therefore, fluctuates on short time-scales (âĹij1 s)”

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 Reply: When frozen turbulence is assumed (which is applicable on such short time scales) buoyancy does not drive a change in the eddy field. In order to clarify this line 21-22 is changed to “For these time-scales Taylor’s frozen turbulence assumption is valid . . .” - 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 Reply: The lookup table method does rely on the assumption of frozen turbulence, since r_12 (τ) will only show a peak at a certain time-lag when

the same eddy field is measured by the two scintillometers. Indeed Potvin et al. (2005) showed that the decorrelation time of the scintillometer signal decreases under conditions where the longitudinal wind component is high. However, this does not imply that frozen turbulence is not valid. A fast decorrelation time of r_12 (τ) can however result in an overestimation of U. However, van Dinther and Hartogensis (2014) already showed that over a reasonable short scintillometer path of âĹij500 m the influence of the longitudinal wind component is small and not visible for U>2.5 m s-1. For the longer path used in this study we expect the longitudinal wind component to have even less influence on the estimates of U. In order not to complicate the text further we decided not to mention the effect of the longitudinal wind component on decorrelation time in the manuscript.

P 6434, line 23: “scintillation signal” what is meant with this? Scintillation has not been put in a context before Reply: Scintillation signal refers to the intensity fluctuations measured by the receiver of the scintillometer. This is now stated in the text.

Scintillation spectra

P 6435, Eq. (1): why is D not defined in terms of Dr and Dt – as is done in Eq. (3)? Reply: Now also DR and DT are used in Eq. 1.

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

P 6436, line 1: “(. . .), which are the frequency (. . .)” it sound like a general truth, whereas I guess it is not. Reformulate Reply: Corrected in this study defined as. . ..

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 Reply: Corrected to “a unique constant, which depends on the experimental setup and scintillometer used,”

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

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? Reply: Corrected to “to what extend the . . .... holds”

P 6436, line 13: switch “are” and “in this study” Reply: On suggestion of reviewer 1 changed to “The cumulative spectra are obtained over 10-min periods in this study”

Time-lagged correlation function P 6436, line 19: change “spatially separated transmitters and receivers” to “horizontally displaced beams” Reply: Corrected

P 6436, line 20: “the sign” – the sign of what? Do the authors mean the crosswind direction? Reply: Change to “crosswind direction (i.e., sign of (U_âŁě) ÌĚ

P 6436, line 23-24: “dual-aperture” and “single-aperture” – probably “beam” is a better word than “aperture”. Reply: In order to keep with previous literature (van Dinther et al. 2013 and van Dinther and Hartogensis 2014) we decided to keep the word with 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). Reply: Changed to “while scintillation spectra can in principal be obtained from every type of scintillometer.”

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. Reply: Indeed the eddy field barely changes as it is being transported from the one scintillometer to the other. In order to emphasize this better this section is changes to "For a dual-aperture scintillometer the two transmitters and receivers are in general setup with only a small separation distance (âĹij10 cm) between the two. Therefore, it takes a short time for the eddy field to travel from the one beam to the other, making that the eddy field barely changes (i.e., frozen turbulence assumption can be assumed)."

P 6437, line 7: add "of the first kind" to "zero-order Bessel function" Reply: Corrected

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. Reply: It is the relative location on the path, so the same as Eq. 1. Now changed to "at location x"

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

P 6437, line 10: change "is given by" to "is obtained from" Reply: Corrected

P 6437, line 14: "related to" – vague, probably the authors mean "equal to" Reply: We indeed mean equal to, so we corrected this.

P 6437, line 16: "is assumed to be constant." – add "along the scintillometer path" Reply: The abbreviation U(x) already implies along the scintillometer path, so this does not need to be added.

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. Reply: In order to clarify this sentence "for the different values of U" added to the sentence.

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

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. Reply: reformulated to ". . . is integrated over the 139 range gates of the different value of U(x) estimated by the Doppler lidar"

P 6437, line 23: "are averaged to 10 min." – is this averaging done arithmetically? Reply: Yes the averaging is indeed done arithmetically, which is now mentioned in the manuscript.

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. Reply: text added to the manuscript.

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

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. Reply: the sentence was deleted as recommended by reviewer 1.

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

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. Reply: We edited slightly, but kept the bulk in because we think it is an important part of the methods.

P 6438, line 17: “beam-pointing directions” add “in the horizontal plane” . Reply: Corrected

P 6438, line 20,24: is b or is θb the azimuth angle of the beam? I would suppose θb, where b is one of the two beams in question (this is implicitly stated in Line 25 where Vg1 = Vg2). Can the authors clarify this? Reply: Corrected

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. Reply: As suggested, this is discussed briefly, as the end of section 2.2.

Experimental setup P 6439, line 6: delete “the” before “1” and before “15” or add “st” and “th” to the numbers. Reply: Corrected

P 6439, line 9: change “BLS900 of Scintec (Rottenburg, Germany)” to “BLS900 (Scintec, Rottenburg, Germany)” Reply: Corrected 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 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. Reply: We agree that numerical site description is important, so we added more to the manuscript.

P 6439, line 16: “near north-south-axis” – do the authors mean “nearly north-south”? Reply: Corrected to “nearly north-south”

P 6439, line 21: change “each 4 s” to “every 4 s” Reply: Corrected

P 6439, line 21-22: Do I get it right that the lidar basically has been run in VAD modus at an elevation of 0.45âŮę, 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. Reply: The samples (PRF) is added to the text. The reviewers other presumptions are correct. 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? Reply: This is implicit in the text. The city is clearly relatively fixed in space and time. So either the lidar beam encounters a building or not, and one gets data up to the first building hit. So we chose beams that were able to cover the full scintillometer path.

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? Reply: Corrected. This was a typo in the text.

P 6440, line 2: change “can” to “could” Reply: Corrected

P 6440, line 5: “scans” – what is meant with this? Did the lidar scan between 174âŮę and 196âŮę? Or were the measurements done at these azimuth angles? Reply: Scans is indeed confusing, what we mean to say here is that two of the Doppler lidar U(x) estimated were averaged. To clarify this the word scans is replaced by estimates.

P 6440, line 8-9: delete “unit at Hotel Torni” and “at the so-called SMEAR-III-Kumpula station” or introduce these two measurement sites properly. Reply: text now improved as suggested.

P 6440, line 14: change “by each of the anemometer” to “by each of the anemometers” Reply: Corrected

P 6440, line 14-22: it seems to me that this would better fit in the methodology than in the experimental setup. Reply: We agree that indeed the placement of this section is a problem. However, the main reason we did this here is that in section 2 the theoretical background and method of both the Doppler lidar and scintillometer are described in detail. Besides these two measurements technologies two sonic anemometers were

also measuring at the two ends of the scintillometer path. We decided not to add a methods and theory section about sonic anemometer to section 2, since this technology is not the focus in this study and only contributes very minor to U (2.5 %). Making section 3, the section where we introduce the sonic anemometer in more detail and also the section where we have to mention that we added these measurements to the estimates of U. Therefore, we decided to keep line 14-22 in section 3 (experimental setup).

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

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." Reply: Corrected

Results and discussion Doppler lidar path-resolved crosswinds P 6441, line 2: change the "–" to a "comma" or the "comma" to a "–"Reply: The comma is changed to "-"

P 6441, line 5, 9: "south anemometer" "north anemometer" – which of the two are we talking about? Reply: south anemometer is added to the sentence.

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. Reply: Line 13-16 means to give a possible reason why the Doppler lidar can have problems in determining the sign of U(x) in conditions where the wind is near parallel to the beam. In these conditions a small error in the estimated wind direction can result a wrong sign of U(x). In order to make this reasoning more clear in the text the lines are changed to "It should be noted that the sign of U(x) is determined by the wind direction estimated by the Doppler lidar. When the wind is near parallel to the path, a small error in the estimated wind direction can result in an error of the sign of U(x)".

P 6441, line 21-23: "besides being parallel to the path," delete this clause, it is men-

tioned 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 m s-1)." Reply: Sentence is corrected as suggested

P 6441-6442, line 16-2: This section is quite wordy and should be written more concisely. Reply: This section is reworded to more clearly state why it is more difficult to obtain the correct sign of U when the wind is near-parallel to the path. Further, in order to decrease the wordiness the following sentence is deleted "However, in general the U-values are reasonably low in these conditions."

Experimental setup P 6442, line 2: delete "even" Reply: Corrected

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. Reply: We agree that the phrasing as it was could be confusing. What we mend to say is that the two beams are probably measuring different wind fields, caused by the church tower close by the 196o beam. Therefore, "by the large divergence" is now deleted from the text and the sentence is now changed to "This is probably caused by differences in the wind fields measured by the two beams, since the 196-beam passes near to a high church tower (Kallio, about 93 m asl)...."

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

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

P 6442, line 21: change "This resulted in (. . .)" to "The exclusion resulted in (. . .)" Reply: Corrected Path-averaged crosswinds

P 6443, line 2: "are" and "that" – make consistent, either plural or singular. Reply: Corrected to "is compared"

P 6443, line 4: “measuring exactly the same atmosphere” – do the authors mean “sample the same part of the atmosphere” ? Reply: Changed to “sample the same part of the atmosphere exactly”

P 6443, line 6: delete “(given their difference in heights)” Reply: Corrected

P 6443, line 8: delete “(excluding the 2000-2500 m of the path)” Reply: It is important to note that this section of the path is excluded, since the results depend on this exclusion.

P 6443, line 8: change “using” to “taking into account” Reply: Corrected

P 6443, line 3-16: “The theoretical difference between (. . .) while in stable conditions the difference should me more” – this part is quite wordy. The authors could concise themselves in saying 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 unlikely are larger than . . .%.” Reply: This section is changed to “However, the height difference between the scintillometer and the Doppler lidar beam causes a negligible difference in the U estimates. Assuming a neutral wind profile the difference in U is merely 1.1 % (with a higher U estimate of the scintillometer), which assures that the height difference between the two measurement devices should not influence the comparison. Note that this 1.1 % is only an approximation, in reality the comparison is more complicated since part of the measurements are done in the roughness sublayer where logarithmic wind profiles are not applicable.”

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

P 6443, line 23: how is the “path-weighted standard deviation along the scintillometer path” defined? Reply: The fluctuations of U(x) along the path are weight-averaged according to the scintillometer path-weighting function. Thus, fluctuations of U(x) in

the middle of the path contribute more to STD_U than fluctuations at the ends of the path. In order to clarify this in the manuscript the following sentence is added “Note that the plots in Fig. 5 are colored with the standard deviation path-averaged by the scintillometer path-weighting function STD_U, i.e., fluctuations of U(x) in the middle of the path contribute more to STD_U than those at the ends of the path).”

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

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! Reply: Thank you.

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. Reply: Done (removed).

Variable crosswinds along the path 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. Reply: Indeed we agree with the reviewer that this section unclear. We meant to give a short quantification of the four cases, but indeed this information is clearly visible in the table and figure. Therefore we decided to delete this text from the manuscript.

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. Reply: High is changed to moderate

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. Reply: Corrected

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 by the authors cannot be applied to the cumulative spectrum of these two, can it? Reply: With CSScint and CSvarU not the results of the cumulative spectrum methods are meant, but the cumulative spectrum itself. Therefore, CSscint refers to the cumulative spectrum measured by the scintillometer, and CSvarU refers to the cumulative spectrum calculated by Eq. 1 using the Doppler lidar estimates of U(x).

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

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. Reply: Corrected to “For case D this is indeed true”

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

P 6446, line 6-13: The section is quite wordy for that what needs to be said. Reply: Done (text streamlined).

P 6446, line 19-22: “However, the magnitude of r12 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. Reply: In order to avoid confusion for the readers the phrase “what stands out” is removed from the manuscript.

P 6446, line 22: add “does” at the end of the clause “but the shape of r12.” Reply: Corrected

P 6446, line 24: “For all four (. . .) better than r12.” – for case A and B I could argue the opposite. Reply: We agree with the reviewer, thus we changed this to “For cases C and D ....”

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 r12 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 r12 is largest. Reply: The lookup table method does not only look at the peak in r_12 (τ), but at the general shape of r_12 (τ). It find the best fit between a measured r_12 (τ) and a theoretical r_12 (τ). The lower U the wider r_12 (τ), thus a widening of r_12 (τ) due to a variable U(x) can be misinterpreted as a lower U. In order to clarify this in the manuscript the sentence now reads “The fact that variable U(x) causes a wider r_12 (τ) can cause an underestimation of U obtained by the scintillometer, since a wider r_12 (τ) is normally associated with lower U-values”

P 6447, line 3-4: “For case C and De 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. Reply: For clarity the following sentence is added in the text “The error in this study is defined as the difference between U estimated by the Doppler lidar and U obtained from r_12 (τ)”

P 6447, line 10-12: “Therefore, by also including (. . .) can be improved.” – unclear language, reformulate. Reply: Reformulated to “If the lookup table was expanded to also include variable U(x) field the results of the lookup table method in a more challenging environment could be improved.”

P 6447, line 18: delete “also” Reply: Corrected

Conclusions P 6448, line 10: “albeit with scatter” – this is vague terminology Reply:

Albeit change to “although”

P 6448, line 11: change “Still given” to “Still, given” Reply: Corrected

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”. Reply: “indicates” is changed to “indicate”. And we changed to RMSD throughout.

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. Reply: Corrected

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;” Reply: Corrected

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. Further-more, this paper could provide more relevant information to the authors. Reply: Sorry we were not aware of the fact that Andreas also made this suggestion in Andreas (2000). This article is now referred to in the manuscript. Furthermore, Section 5 is now referred to as “Conclusion and Outlook”.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/7/C4044/2014/amtd-7-C4044-2014-supplement.zip

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