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Atmospheric Measurement Techniques Discussions

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7, C4023–C4043, 2014

Interactive Comment

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

Reply: We would like to thank the reviewer for his or her valuable comments, which greatly improved the manuscript. We have addressed all the comments raised by the referee in the response point by point and modified the manuscript accordingly. Below, we repeat the reviewers' comments and our replies to them. Note that in this reply U refers to the crosswind. The supplement is a zip-file of two files: (1) showing the difference between the new and previous manuscript and (2) the new manuscript.

Anonymous Referee #1

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Interactive Discussion

Discussion Paper

Received and published: 29 July 2014 This study uses a Doppler lidar to estimate spatial variability in wind speed along a scintillometer path. The lidar data are used to evaluate the performance of two methods to estimate the crosswind speed from the scintillometer. This is an ambitious setup, especially given the complexities of urban areas, and the limits of both measurement techniques are an important consideration. Data from two sonic anemometers located at either end of the scintillometer path are also used. The authors conclude that both methods to estimate the path-averaged crosswind from scintillometry work reasonably well, and suggest modifications to the methods under variable crosswind conditions. This work is a useful addition to the small number of studies evaluating the performance of scintillometer-derived cross-wind speeds. The aims of the work and the methodology are generally described clearly. But the text is often repetitive and should be made more concise. In many places, vague statements must be replaced with relevant details and precise wording – and statements must be substantiated. The results would benefit from a deeper and more rigorous analysis. If possible, inclusion of a longer data time-series would be advantageous, but even the relatively short study period presented needs to be analysed more thoroughly. I have some concerns about the treatment of the data and lack of detail in some areas. I will highlight these first, followed by more minor suggestions. Providing these issues are adequately addressed I recommend this manuscript for publication in AMT.

Specific comments: In terms of the data selected for analysis (Section 4.1), I have the following concerns: On the basis of Fig 2, the authors exclude the lidar data from gates between 2000-2500 m when calculating the path-averaged crosswind for comparison with the scintillometers. Not only does this seem to be a little artificially selective (would the comparison in Fig 3 would be worse if those data had been left in?) but this region is at the peak of the scintillometer path-weighting function. If these data can really be attributed to the church tower, would it not also influence the scintillometer data? It could be argued that for the most appropriate comparison those data should therefore be included. From Fig 2, there also appears to be large differences (of the opposite sign) at a distance of about 1.6 km, mostly for winds close to parallel to the path. If these data are not removed, the quality control may be introducing a bias into the lidar estimates of crosswind, which presumably would act to make the lidar crosswind values larger (more positive). If the authors have good reason to exclude the data between 2000-2500 m, it needs to be justified more carefully in the manuscript. For example, can the observed influence of the church tower be explained in terms of the behaviour of the wind field? Reply: As is visible in Fig. 1 of the article for the Doppler lidar beam with angle 196o passes right next to the church tower (as does the scintillometer beam). However, the other beam (174o) does not pass near the church. Therefore, it is highly likely that only one Doppler lidar beam is affected by the church. The duo-beam method assumes that the wind field measured by the two beam is this same, if indeed only one beam is affected by the church tower this assumption is violated. Thereby causing errors in U (x). Fig. 2 indicates that there is indeed some changes in sign in the difference in U_âŁě of the sonic anemometer and the Doppler lidar, which was the reason to exclude it. If indeed as we expect the wind field changes due to the church tower the scintillometer will also be affected. However, with the Doppler lidar setup used in this study it is not possible to estimate the change in U near the church tower. The reasons behind excluding the data are discussed more thoroughly in Section 4.1. The exclusions of the data between 2000 and 2500 m along the path is also explored more thoroughly by including with the average horizontal wind speed measured along the path measured by the Doppler lidar, as was suggested by reviewer 3.

Data are also excluded for a substantial proportion of the time-series due to wind direction. Given the frequency of occurrence of wind directions close-to-parallel to the path, and the spatial variability seen at these times, some analysis of the performance should be presented under these more challenging conditions. Can the suggested improvements to the scintillometry methods work in these conditions, or are the limitations of the measurements reached? Reply: Note that in Fig. 2 the difference between the sonic anemometer and the Doppler lidar are plotted. For these close-to-parallel wind directions it is not the scintillometer which shows problems, but the Doppler lidar. The

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reason behind this is that the Doppler lidar measures the parallel wind speed, in order to calculate the crosswind we use the two beams. A small error in the wind direction calculated by the duo-beam method in the case of a near parallel wind can result in a error in the sign of the crosswind. Since this study mainly focuses on the performance of the scintillometer in heterogeneous wind fields we decided to leave out the wind directions where the Doppler lidar estimates of U(x) could not be trusted.

Pg 6442, line 15-16: Does the restriction of 1.5 $\times$ path-averaged crosswind refer to the scintillometer or lidar path-averaged crosswind? Again, this seems to be selecting a subset of data using a rather arbitrary threshold to limit the possible case studies to times when better agreement between methods seems likely. Please justify. Reply: The path-averaged crosswind refers to the Doppler lidar estimates (after excluding 2000-2500 m), that this from the Doppler lidar is now mentioned in the text. In order to calculate the theoretical cumulative spectra and r_12 (τ) the values of U(x) are necessary along the complete scintillometer path. However, as mentioned before the values of U(x) around 2000-2500 m are untrustworthy due to the church tower. In order to ensure that these untrustworthy values as little as possible influenced the Doppler lidar estimates we selected cases where no spikes occurred in U(x) in between 2000 and 2500 m (by restricting U(x) in between 2000-2500 m not more than 1.5xU.

Pg 6442, Line 25-6: It is mentioned here that the lidar data are smoothed. Why was this deemed necessary and why was it not mentioned in Section 3? Reply: See response to reviewer below.

Pg 6444, Lines 9-11: The authors conclude that both measurement techniques (I presume this means scintillometry and lidar) are able to obtain path-averaged crosswind in the challenging urban environment. However, much of the data collected in possibly more challenging conditions has been excluded from the analysis (Section 4.1). I would like to see a more balanced approach to the discussion. Similarly for Lines 25-7 on Pg 6444. Reply: It indeed should be made clear that these conclusions are based on the results obtained given the data treatment of the study. In order to do so in Section 4.2 (previously pg. 6444 lines 25-27), the following sentence is added “However, bear in mind that in order to achieve these results certain wind directions and a certain section of the path were not take into account (see Section 4.1).” Both in the abstract and conclusion the following statement was added to put the conclusions in a better perspective “, given the data treatment applied in this study,”

Currently, the paper is somewhat contradictory: the urban setting has been selected for its variable wind conditions, yet data selection seems to reject data that appear ‘too variable’. The conclusions are then that these techniques work well in urban areas. However a more consistent and balanced argument is required. To summarise, if data are rejected there must be a good justification for doing so, the effect on the results should ideally be quantified and the conclusions must reflect the methodology (rather than suggesting reasonable performance, perhaps due to careful selection of data). Reply: It was not our intention to filter out data that were U is too variable. However, fact is that the heterogeneous wind fields poses challenges for both the Doppler lidar and the scintillometer. Therefore, we deemed it necessary to first evaluate the performance of the Doppler lidar to measure U (x) (as is done in Section 4.1) before comparing it with the scintillometer. From this evaluation it became evident that there are some data in the Doppler lidar which cannot be trusted. These data are excluded for the comparison with the scintillometer, but not without a reason. The wind directions are excluded because it is difficult to obtain the correct sign of U(x) when the wind is near parallel to the path. A section of the path is excluded because a church tower only influence one of the Doppler lidar beams and not the other. We changed some of the text in Section 4.1 to better clarify why the data are excluded.

Calculation of path-weighted crosswind from lidar and anemometers: Pg 6440, Lines 15-22: Given that the path-weighting function means the ends of the path contribute very little to the total path-averaged measurement, the decision to use anemometer data for the end 2.5% of the path is surprising. The sonics are at a different height to the scintillometer and lidar, and the increased uncertainty of the data for particular wind directions is mentioned in the preceding few lines (Pg 6440, Lines 10-12). With all these uncertainties, what was the reasoning behind including anemometer data in the lidar estimates of path-averaged crosswind? Reply: Given the lidar data did not always reach the full > 4km path, it was decided to allow the sonic data to supplement the lidar data in those cases – i.e. better than nothing given the small height differences and small weighting to the end of the scintillometer beams.

Additionally, if there are missing lidar data for certain range-gates, how is the path-averaged crosswind calculated? Are the path-weighting factors for each gate re-scaled to give at total of 100%? The missing range-gates usually occur further from the instrument, i.e. towards the higher part of the path. How might the availability of data have influenced the results? Reply: In case of missing data along the Doppler lidar path U is indeed calculated by scaling the path-weighting factor to give a total of 100 % (this is added in Section 3 in the paper). From Fig. 1 is it apparent that most of the high buildings are located at 0.5 until 2.5 km along the Doppler lidar path. Especially, 2.5 until 3.5 km along the Doppler lidar path the buildings are less tall (building height <40 m). In principal for these lower buildings the wind speed (and thereby the crosswind) should be higher. These wind speed are less often taken into account by the Doppler lidar, which can in theory result in an underestimation of U by the Doppler lidar.

This study uses lidar data as a reference to evaluate scintillometer crosswind speeds. There should be some discussion/quantification of the uncertainties in the lidar data too. Reply: In Section 4.1 we discuss the capability of the Doppler lidar to estimate U(x) by comparing it to sonic anemometer measurements. We even quantify it in Section 4.1 by stating the root mean square error between U measured by the Doppler lidar and one of the sonic anemometers. Also we refer to other work where Doppler lidar estimates of the wind are used (Hirsikko et al. 2014 in Section 4.1 and Wood et al 2013c in Section 4.2). There have been some changes in the text in Section 4.1 to clarify why some of the Doppler lidar estimates are not taken into account in the comparison. We think that the text as stated now in Section 4.1 evaluates the Doppler lidar

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estimates properly to continue with the comparison with the scintillometer estimates.

Discussion of complexities: There are several key issues which are not dealt with in sufficient detail given the subject of the paper: Pg 6434, Lines 21-2: Taylor's Frozen Turbulence Hypothesis. More detail would be helpful here, particularly for non-specialists. Provide more explanation of why frozen turbulence is important. Refer back to here on Pg 6436, Line 25-7. Reply: Key to why Taylor's frozen turbulence assumption is valid is that the scintillometer measures intensity fluctuations over short time-scale, which is now stated more clearly in the text. For the methods relying on r_12 (τ) Taylor's frozen turbulence assumption is important, since is causes the two signals to be identical to one another except for a time shift. The importance and explanation why it is applicable is now more clearly stated in Section 2.1.2 by adding the following sentences to the text "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). The signals of the two spatially separated scintillometer beams should thus be almost identical except for a time shift." The paper deals with spatial variability, but how might temporal variability in wind speed influence the measurements and results? Reply: Indeed temporal variability of the wind can influence both the Doppler lidar and the scintillometer estimates of U. The Doppler lidar sampled every 5 min 10 s for each ray. It is of course possible that these 10 s are not representative for the 5 min. This was one of the reasons to average two intervals of the Doppler lidar and compare 10-min averages of U. For the scintillometer the scintillation spectra are obtained over 10-min time intervals. A temporal variability in U will result in a smoothing of the spectra. Given the fact that the cumulative spectrum method takes into account multiple points in a spectrum the results are probably not significantly affected by this. r_12 (τ) is obtained over a reasonably short time scale (10 s). It is unlikely that the wind field changes significantly over such a short time step. Therefore, estimates of U of the scintillometer calculated by the lookup table method should not be significantly influenced by temporal variability in U. In the Section 5 we now added a paragraph to speculate about the influence of temporal variability of U on the U estimates of the scintillometer.

Pg 6438 Line 26 – Pg 6439 Line 2: The potential complications introduced by the roughness sublayer need to be addressed. A description of what is meant by the roughness sublayer is needed, plus an idea of its expected height/depth. State clearly which of the measurements are thought to be within the roughness sublayer. What is the effective measurement height and displacement height for the scintillometer? The methodological considerations and limitations due to the complex environment are not discussed in enough detail either generally (Section 2) or with reference to the results (Section 4). Reply: We address the roughness sublayer query in response to another reviewer. Note also that the site is described in many other papers for Helsinki about fluxes, lidars, scintillometers, etc – so we have the luxury of being somewhat brief in leaving out full details. And certainly the duo-beam method has major drawbacks.

Pg 6441, Line 26 – Pg 6442, Line 2: Spatial differences. These sentences are too vague and do not provide a satisfactory explanation. The data excluded constitute a substantial proportion of the total data. If the reasons for this observed behaviour are not understood, it calls into question the validity of the results at other times. Support the roughness sublayer / homogeneity hypotheses with, at least, a reasoned explanation and (if possible) some experimental evidence or the literature. Reply: We think the odd behavior has to do with the fact that under these near-parallel to the path wind directions it is harder for the Doppler lidar. This explanation is now more clearly stated in Section 4.1.

Pg 6442, Line 3-12: Influence of the church. Again, the explanation of the data needs more care. Firstly, more precision is needed in the description (Line 7: change to ‘2300 m from the transmitter’; please replace ‘somewhat’ by a distance). Can the analysis be improved from the statement that the church ‘causes problems’ for the duo-beam method? Can any of the other lidar rays be used to support the idea that the church is responsible for the behaviour seen? Reply: After 2300 m the following text was added “distance from the Doppler lidar (see Fig. 1b)”. Somewhat is now changed to 35 m from the 196o-beam. In order to explain the problem the church tower better the following text was added to the manuscript “The church alters the wind field of one of the Doppler lidar path (196O), while the other beam (174O) does not encounter this alteration. Thus, the wind field sampled by the two Doppler lidar beams are not homogeneous, which causes problems for the duo-beam method.”

Depth of analysis: In Section 4.1, the spatially-resolved crosswind speed from the lidar is presented (Fig 2) and discussed. Given the rarity of such a dataset, it surely warrants further discussion and analysis, particularly of the spatial variability and relation to the complex urban surface. Fig 2 could be a lot more informative. At present, the colour scale is hard to distinguish. Perhaps using a discrete colour scale would help separate a difference of 0 and 3-4 m s-1, or 7 and 13 m s-1. Reply: In order to add more depth a figure is added which shows the average horizontal wind speed and U_âŁě along the lidar beam as estimated by the Doppler lidar (see Figure 1 below or Figure 3 in the manuscript). The horizontal wind speed is somewhat higher there where the building height is low (>2.5 km along the path). Also the wind speed is less variable for these low building height. The estimates of U obtained by the Doppler lidar drops significantly around 2 to 2.5 km along the beam, it even changes sign. This is again an indication that around the location of the church tower errors occur of the Doppler lidar U(x) estimates. The figure and it’s implication on the result of the Doppler lidar estimates of the wind are now added in the manuscript.

Fig 2 shows the comparison between data from the lidar and south anemometer. Could it be informative to include a similar analysis (and plot) for the north anemometer? This would give the values in Fig 2 some context. Presumably the first few range-gates would be expected to show better agreement with anemometer north than anemometer south. This may allow the effects of diverging lidar beams to be investigated more thoroughly. Reply: The main aim of Fig. 2 was to investigate the capability of the

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Doppler lidar to estimate U(x) by comparing it to one of the sonic anemometer. From this figure it became apparent that there are some wind directions where the Doppler lidar estimates cannot be trusted as are some data points near the church tower. Indeed we expect that the north sonic anemometer would show a better agreement with the Doppler lidar for the first few range gates. However, the figure would still be dominated by the difference in U between Doppler lidar and sonic anemometer for the near –parallel wind directions (as is indeed visible in Figure 2 below) .Therefore, we do not think the manuscript would benefit from adding a similar figure for the north anemometer.

It would be nice to see more data from the different techniques. For example, I suggest adding a 2-D plot of the crosswind speed from the lidar (path position on y-axis, time on x-axis, colours indicating the value of the crosswind), alongside time-series plots for the crosswind from the scintillometer and two anemometers (showing both crosswind and absolute wind speed for the anemometers). This would give the reader a clearer impression of the datasets, aid interpretation of Fig 2 considerably and provide visual evidence of the points made in Section 4.1 (e.g. variation with absolute wind speed, wind direction, distance along path and between techniques). Including a figure similar to that suggested here should also facilitate a deeper analysis of some of the complexities of the measurement site and setup (influence of the church, spatial averaging, heights of the instruments) and provide more insight into the dataset. Reply: In order to exploit the data more thoroughly a plot with a time series of the crosswind is given (see Figure 2 below or Figure 4 in the new manuscript). From this figure it is apparent that the two methods to estimate U from the scintillometer data give very similar results. Further it is evident that the Doppler lidar estimates of U seems to fluctuate more vigorously than the other two measurement devices. However, the Doppler lidar estimates does seem to follow the estimates of the scintillometer better than the sonic anemometer (especially on DOY 180 from approximately 6:00 onwards). There is a clear difference between U measured by the north and south anemometer, with in general higher values measured by the south anemometer. These results are now

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discussed in the manuscript in Section 4.2.

Comparison with other studies: Pg 6444, Lines 1-9: Provide some relevant details of these studies to give some context to the statistics, such as path lengths, beam heights, average wind speeds, etc. The current comparison of RMSE values between these different studies is difficult to interpret meaningfully. Why might the performance differ between these studies? Is it possible the authors can offer more insight based on the current study and their previous work? Reply: This section was meant to give more insight into the value of the RMSE of this study compared to other studies. A RMSE of 0.73 m s-1 is reasonably low for comparisons in urban environments, as was shown for example in Wood et al. 2013c. Adding extra context by including path length, beam height etc would distract from the main intention of giving readers a feeling of the other studies. However, indeed to mention that the wind conditions where comparable is important, so this was added to the manuscript.

Four case studies: Pg 6447, Lines 12-15: The four case studies underestimate the crosswind although this is not seen in the scatter plots (Fig 3). Does this suggest that the four case studies are not especially representative of the data in Fig 3? More analysis and discussion is needed here to reach a satisfactory conclusion. Would it be possible to include results of more case studies? Use of only four 10-min periods is fairly limited. Reply: We do not feel it would be wise to include more case studies, Fig. 6 (in previous version Fig. 4) is already complicated as is adding more what only make it more so. We tried to select 4 cases which are representative for the measurement period. As visible in Fig. 6 for all cases the crosswind is reasonably variable along the path. We ourselves also do not fully understand why all the cases give an underestimation while this is not visible in the comparison.

Minor comments: Pg 6432, Line 4: '3 weeks'; Pg 6439, Line 6: 'from the 1 to the 15 October' = 15 days. Please correct. Reply: Corrected.

Pg 6432, Line 25: Change to 'perpendicular to the scintillometer path' Reply: Corrected.

Pg 6433, Lines 1-4: This is a bit vague – more explanation is needed. Give the resolution of the models. Explain what is meant by 'point measurements can more easily be biased than path-averaged values'. Reply: The resolution is now given in the text "($\sim$10 km)" and to explain the more easily biased "due to ground clutter" is added to the text.

Pg 6433, Line 6: f and τ have not been defined yet. Reply: Are now defined here.

Pg 6433, Line 13: x has not been defined yet. Reply: Are now defined here.

Pg 6433, Line 19: Add 'spatially' before 'variable'. Reply: Corrected.

Pg 6433, Lines 23-26: Suggest deleting as this is too much detail for the introduction and is repeated in Section 2.2 anyway. Reply: Text deleted/simplified as suggested.

Pg 6433, Lines 27-8: The Helsinki Urban Boundary-Layer Atmosphere Network probably only needs to be mentioned once, either here or on Pg 6439, Lines 4-5. Reply: Good point. Done.

Pg 6433, Line 28 – Pg 6434, Line 6: This is a lot of detail for the Introduction before the techniques have been fully presented and is partly repeated in later sections. Suggest deleting from here and moving any important details to the relevant sub-sections of 2.2. Reply: We think it is important to already in the introduction mention the challenges the urban environment posses to estimate U for the scintillometer and the Doppler lidar. The fact that the urban environment is also a challenging environment for the Doppler lidar is the reason we first investigate the applicability of the Doppler lidar to estimate U. Therefore, it is important to already in the introduction inform the reader about the challenged the urban environment posses for both measurement techniques.

Pg 6435, Lines 4-5 and Pg 6436, Lines 15-17: Suggest deleting these lines and instead adding 'for full details' to Pg 6435 Lines 1-2, to avoid repetition. Reply: Corrected.

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Pg 6435, Lines 19, 20: References needed here. Reply: Reference to van Dinther et al. (2013) added

Pg 6439, Lines 14 and 26: If the scintillometer path length is 4.2 km, why are range gates included only up to 4095 m? Reply: Sorry this was a typo, the lidar beams are taken into account until 4155 m.

Pg 6440, Lines 1-3: More detail about the factors limiting the range of the lidar would be helpful. What are the thresholds used to include/exclude data? What is the sensitivity of the instrument? What is it about the aerosol loading that prevents data retrieval from the furthest gates? Which of these factors was the biggest problem in this dataset (there are a lot of missing gates in Fig 2)? Reply: This is detailed in the referenced Hirsikko methods paper, and is based on an SNR limit. It is based on both experimental and theoretical considerations. Note that anyway, those bad data can easily be spotted in the Doppler wind data in the form of unrealistically erratic and windy data beyond the point of the given SNR limit. The missing data in fig2 is very normal. The Hirsikko paper goes into a lot of detail on these issues.

Pg 6440, Lines 10-13: Were any data from the sonic anemometers removed for the 'more uncertain' wind directions? Reply: Wind directions for flow-distortion impacts on the southern-anemometer did not occur during the period of campaign. (Anyway, it is normal to disregard such higher order statistics, e.g. turbulence, fluxes, etc for flow-distortion directions. Whilst the wind direction time series in Figure2 is only given as guide, and given uncertainties in wind direction of, say, 10deg has almost zero impact upon the figure and its interpretation.)

Pg 6443, Lines 2-18: The consideration of height differences is good, but this paragraph does not have a clear message. Please rephrase to read consistently and be more precise/detailed where helpful (e.g. explain why the stability would affect the agreement between techniques). Reply: Also by the suggestion of reviewer 3 this section is changed to the following text "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.". We also decided to leave out the comment about stability, since it would make this section unnecessarily complicated. The section is now shorter and more concise and focuses on the main message, which is that in principal the height difference between the scintillometer beam and the Doppler lidar beams should not result in a different U.

Pg 6444, Line 17: Change to '(here only four points are negative)' Reply: Corrected.

Pg 6448, Line 1: This is a bit misleading as neither the lidar nor scintillometer are horizontal, and the slope of the lidar and scintillometer paths are different. Please rephrase. Reply: Corrected to ".. the Doppler lidar was measuring alongside the scintillometer path.

Pg 6448, Lines 2-3: Should 'perpendicular' read 'parallel'? Reply: It should indeed be "parallel" this is now corrected in the text.

Pg 6448, Line 26 – Pg 6449 Line 1: What are the advantages of using two scintillometers instead of a single lidar? Reply: The main advantage of the scintillometer is the price, and also the ease to use is. These advantage are now mentioned in the manuscript.

Figure 1: Change 'building average' to 'average beam height' and 'building maximum' to 'maximum building height' in the caption and legend. Add labels for the scintillometer transmitter and receiver. Would it be more useful to colour this map according to building height as in Fig 1 of Wood et al. (2013c)? Reply: The changes suggested would not be correct, so we could not make them. Also we decided not to make the

figures more ‘busy’ by adding information that is already in the manuscript.

Figure 1-2 captions: Are the building heights calculated for ±250 m or ±25 m either side of the paths? Reply: The captions are correct. 25m in one figure; and 250m in the other figure. (i.e. to show the difference between the general environment compared to the area more-or-less directly under the beam.)

Figure 2: Suggest using a separate colour for the shading on the right hand panel (indicating the position of the church) to the shading on the lower panel (indicating directions close to parallel to the path). You could also mark the position of the church on Fig 1. Reply: The Church tower is separately indicated with the brown colour in Fig. 2. The red colours in the right panel and lower panel both indicate the data that have been left out in the analysis. Making these a different colour might confuse the reader, therefore we kept the same colour.

Figure 3: Although no negative values would be obtained for Fig 3a, I would suggest using the same axes limits (or using the same size plot scale) for Fig 3a and 3b to facilitate comparison between methods. Currently it is difficult to judge the relative scatter. Also add the regression lines to the plots. It would be more helpful to present N as number of samples (are these 10-min periods?) rather than a percentage. Reply: Corrected, except for the regression lines. The figure is already busy as is, adding regression lines would make the figure harder to read.

Figure 4: Please make the y-axis limits the same for the top row to facilitate comparison. Reply: Corrected.

Very minor comments:

Many of the following points are suggestions to reduce wordiness or unnecessary repetition. There are other places where the text could be improved and small errors corrected.

Pg 6432, Lines 7-9: Suggest deleting ‘If the scintillometer... ...urban environment’ as it opens up questions of other urban complexities which could present measurement issues, and in Lines 14-15 this is repeated anyway. Reply: Corrected.

Pg 6432, Line 15: Change ‘in detail’ to ‘detailed’ Reply: Corrected.

Pg 6433, Line 9: Change to ‘At these sites’ Reply: Corrected to “At such sites”

Pg 6434, Line 8: Change ‘In order to do so, firstly...’ to ‘Firstly...’ Reply: Corrected.

Pg 6434, Line 11 (and elsewhere): Change ‘validate’ to ‘evaluate’ Reply: Corrected.

Pg 6434, Line 20: Suggest ‘...turbulent. The receiver...’ Reply: Corrected.

Pg 6436, Line 2: Change to ‘For each of these five points, a value of...’ Reply: Corrected.

Pg 6436, Line 13: Change to ‘...are obtained over 10-min periods in this study.’ Reply: Corrected.

Pg 6437, Lines 12-23: Some important details here but the text could be more concise Reply: This section is rephrased it now reads “In this study, we will use the lookup table method to obtain U from r_12 (τ). A lookup table is created with values of the theoretical r_12 (τ) (using Eq. 3)) given a range of U values (resolution of 0.1 m s-1) and time-lag values (resolution of 0.002 s, equal to the measurement frequency of the scintillometer) (van Dinther and Hartogensis 2014). Note that U (x) is assumed to be constant when creating the lookup table. The estimate of U is obtained by comparing the measured r_12 (τ) values to the theoretical r_12 (τ) values of the lookup table. The theoretical r_12 (τ) that has the best fit with the measured r_12 (τ) thus yields the value of U.”

Pg 6438, Line 9-11: Delete ‘in this study’ and ‘given that the Doppler lidar was located near the receiver of the scintillometer’ Reply: Corrected.

Pg 6438, Line 14: Delete ‘from the Doppler lidar measurements’ Reply: Corrected.

Pg 6438, Line 16-9: The description of the duo-beam method seems fairly self-contained here. You can probably delete the sentence, 'A detailed description... is given here' as you already reference Wood et al. (2013c) in Line 13. Reply: This reference is the first (and only other one) where this method is published. We think it is important to keep the link to this method.

Pg 6439, Line 24: Delete 'which are typical for urban environments' Reply: Corrected.

Pg 6439, Line 26: Change to 'centred at distances of 105-9585 m from the instrument' Reply: Corrected.

Pg 6440, Line 14: Change 'anemometer' to 'anemometers' Reply: Corrected.

Pg 6441, Line 26, Should 'values' read 'differences'? Reply: Corrected.

Pg 6443, Lines 16-18: Delete 'For the scintillometer... ... in Fig 3' (repetition) Reply: Corrected.

Pg 6444, Line 8: Change to 'quality-checked' Reply: Corrected.

Pg 6445, Line 11: Delete 'Results are presented in Fig. 4.' Reply: Corrected.

Pg 6445, Line 12: Delete 'the results of' Reply: Corrected.

Pg 6445, Line 19-20: Delete 'We first focus on... ... panels of Fig. 4). Reply: Corrected.

Pg 6445, Line 22: Change 'points' to 'point' Reply: Corrected.

Pg 6445, Line 22-3: Suggest adding 'the retrieved value of' before U Reply: Corrected.

Equations 1 & 3: For consistency, suggest using Dr and Dt in both equations Reply: DR and DT used in both equations and in text.

Figure 3 caption, final line: Change 'line' to 'lines'. Reply: Corrected.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/7/C4023/2014/amtd-7-C4023-2014-

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Interactive Comment

supplement.zip

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

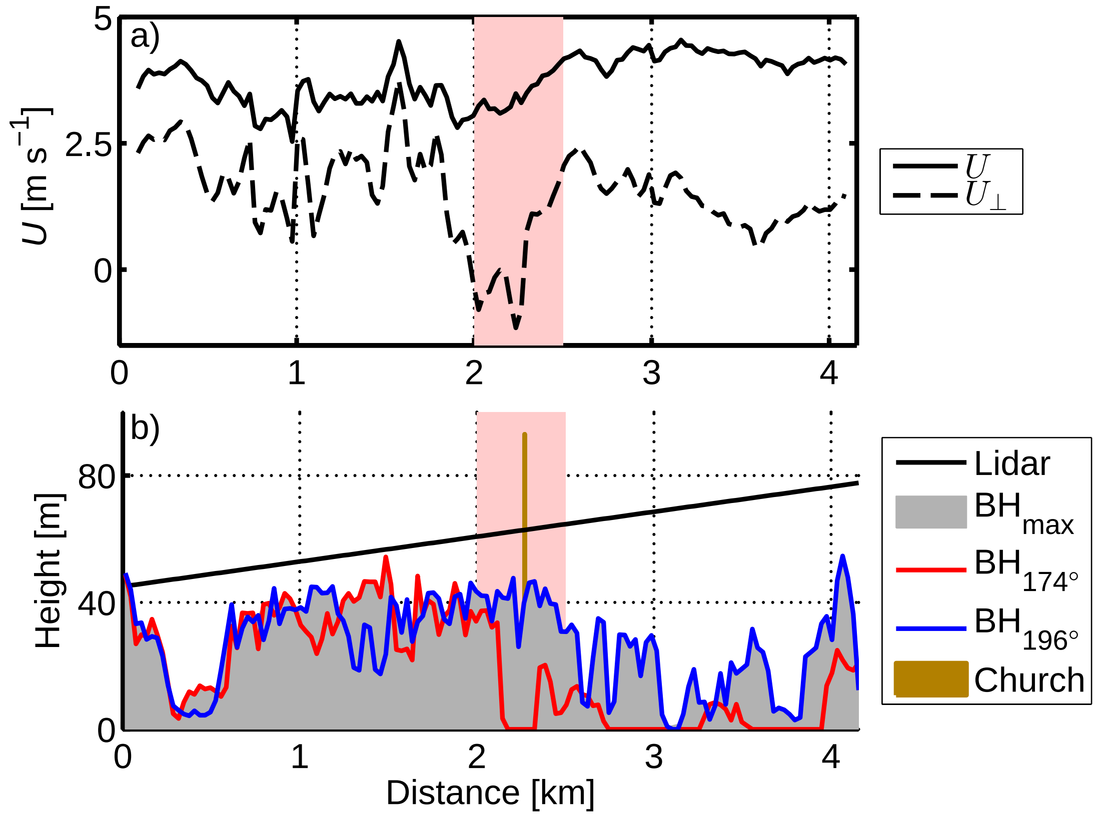

Fig. 1. (a) Average horizontal wind speed and crosswind speed estimated by the Doppler lidar. (b) The height (asl) of the Doppler lidar beam and building height (BH) ± 25 m laterally underneath the paths (tot

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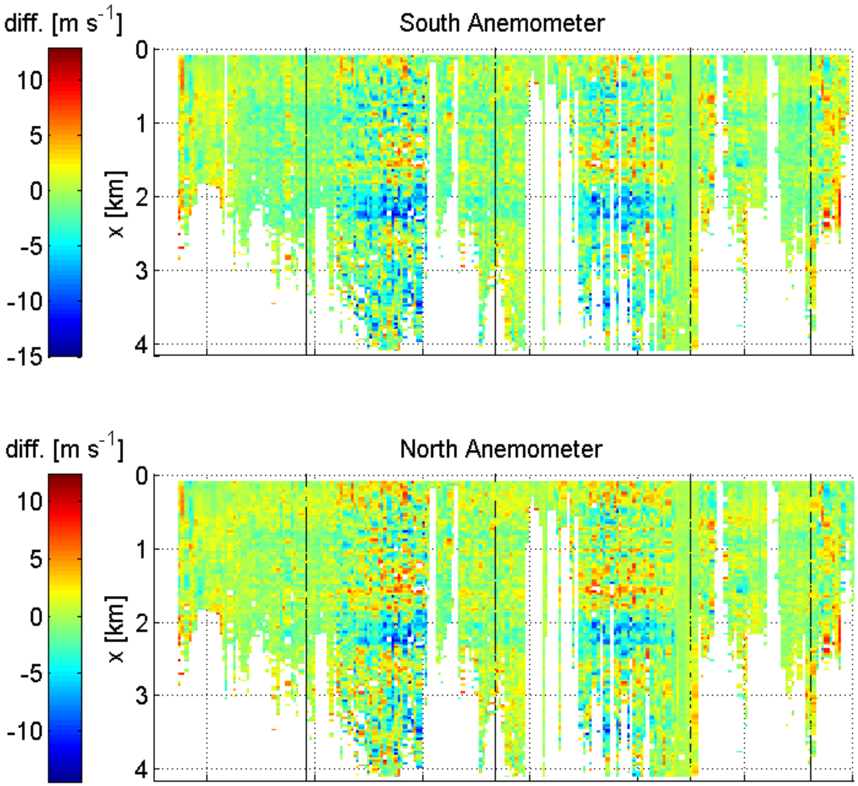

Fig. 2. The difference in U estimated by the Doppler lidar duo-beam method compared with the south anemometer (top) and north anemometer (bottom) as a function of Doppler lidar beam distance

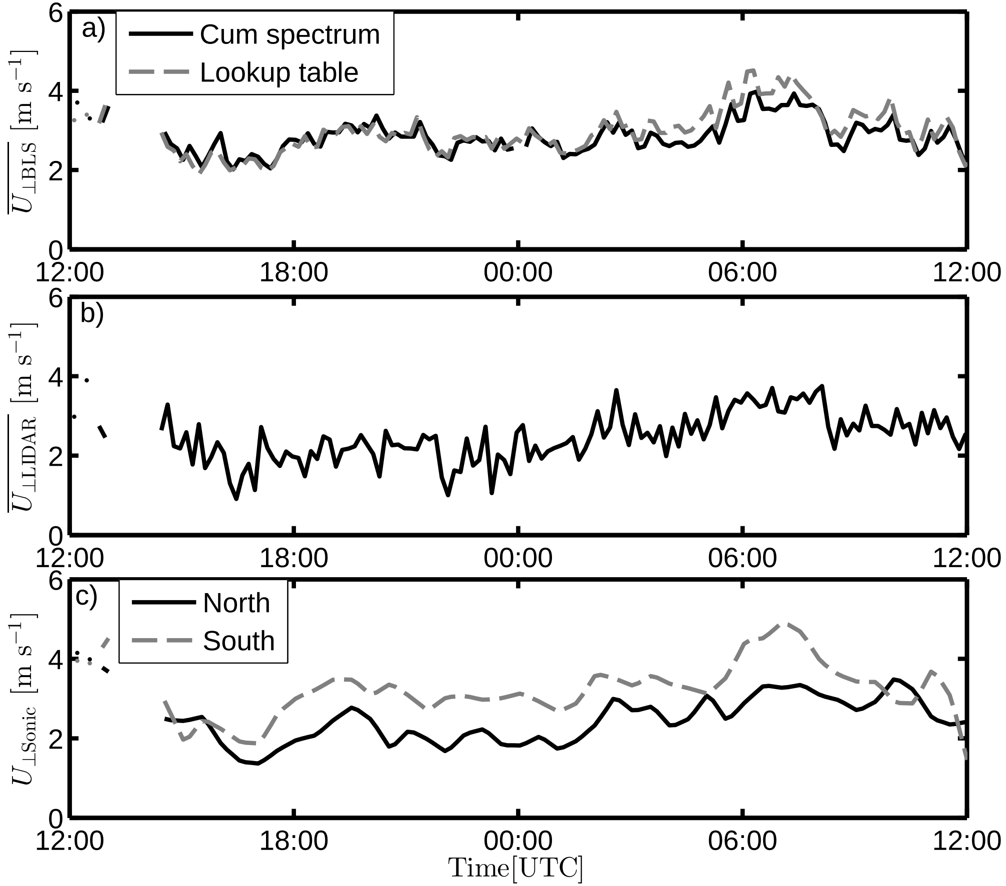

Fig. 3. Time series of U as estimated by (a) the scintillometer, (b) the Doppler lidar, and (c) the sonic anemometer for DOY 279 and 280.

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