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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.

Anonymous Referee #1

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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 crosswind 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?

- 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

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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?

- 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.

- 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?

- 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.

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).

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

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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?

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?

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.

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.

- The paper deals with spatial variability, but how might temporal variability in wind speed influence the measurements and results?

- 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

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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).

- 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.

- 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?

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.

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

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thoroughly.

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.

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?

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.

Minor comments:

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

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

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'.

Pg 6433, Line 6: f and τ have not been defined yet.

Pg 6433, Line 13: x has not been defined yet.

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

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

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.

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.

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.

Pg 6435, Lines 19, 20: References needed here.

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?

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

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the furthest gates? Which of these factors was the biggest problem in this dataset (there are a lot of missing gates in Fig 2)?

Pg 6440, Lines 10-13: Were any data from the sonic anemometers removed for the 'more uncertain' wind directions?

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).

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

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.

Pg 6448, Lines 2-3: Should 'perpendicular' read 'parallel'?

Pg 6448, Line 26 – Pg 6449 Line 1: What are the advantages of using two scintillometers instead of a single lidar?

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)?

Figure 1-2 captions: Are the building heights calculated for $\pm$250 m or $\pm$25 m either side of the paths?

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.

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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.

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

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.

Pg 6432, Line 15: Change 'in detail' to 'detailed'

Pg 6433, Line 9: Change to 'At these sites'

Pg 6434, Line 8: Change 'In order to do so, firstly...' to 'Firstly...'

Pg 6434, Line 11 (and elsewhere): Change 'validate' to 'evaluate'

Pg 6434, Line 20: Suggest '...turbulent. The receiver...'

Pg 6436, Line 2: Change to 'For each of these five points, a value of...'

Pg 6436, Line 13: Change to '...are obtained over 10-min periods in this study.'

Pg 6437, Lines 12-23: Some important details here but the text could be more concise

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

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

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.

Pg 6439, Line 24: Delete ‘which are typical for urban environments’

Pg 6439, Line 26: Change to ‘centred at distances of 105-9585 m from the instrument’

Pg 6440, Line 14: Change ‘anemometer’ to ‘anemometers’

Pg 6441, Line 26, Should ‘values’ read ‘differences’?

Pg 6443, Lines 16-18: Delete ‘For the scintillometer... ... in Fig 3’ (repetition)

Pg 6444, Line 8: Change to ‘quality-checked’

Pg 6445, Line 11: Delete ‘Results are presented in Fig. 4.’

Pg 6445, Line 12: Delete ‘the results of’

Pg 6445, Line 19-20: Delete ‘We first focus on... ... panels of Fig. 4).’

Pg 6445, Line 22: Change ‘points’ to ‘point’

Pg 6445, Line 22-3: Suggest adding ‘the retrieved value of’ before U

Equations 1 & 3: For consistency, suggest using Dr and Dt in both equations

Figure 3 caption, final line: Change ‘line’ to ‘lines’

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