Atmos. Meas. Tech. Discuss., 7, C686–C693, 2014
www.atmos-meas-tech-discuss.net/7/C686/2014/

Atmospheric Measurement Techniques
Discussions

Interactive comment on “Performance of a mobile car platform for mean wind and turbulence measurements” *by* D. Belušić et al.

D. Belušić et al.

danijel.belusic@monash.edu

Received and published: 25 April 2014

We thank Dr Moravek for carefully reading the manuscript and for his constructive and detailed comments. The responses are given below, marked with **R**.

1 General comments

As stated in the comments below, at a few points the interpretation of the results is not as precise. One important issue is the comparison of tower and car measurements, since it proves the applicability of the mobile car platform in this study. Here, a general measure of the agreement between tower and car measurement would be advisable. Alternatively, one should state more clearly the expected agreement for such a comparison (i.e. by putting it in context to studies using aircraft measurements).

R: We have calculated the bias and normalized RMSE, which are now included in the Supplement.

Besides, there is one important comment for future experiments: The authors state that the design of the mobile car mobile platform was motivated to study the stable boundary layer at night. However, the presented tests were carried out during midday, where most likely no stable conditions occurred. Also, the presented results are based on only small data sets. Hence, for further studies it is recommended to perform also tests at night and over a longer period. This would be important in order to judge whether the presented design is also suited for measurements in the stable boundary layer since night time fluxes are usually smaller and therefore prone to larger uncertainties. This might not apply for the turbulence structure of the ABL.

R: We agree. We have included a paragraph at the end of Conclusions that addresses this issue.

2 Specific comments

P. 950, L. 21-24: As the use of a moving platform is the key point of this publication, I suggest elaborating on the purpose of aircraft observations or moving platforms here (and not only refer to the references). E.g., like: ”One alternative is research aircraft observations since they/which allow... They have received considerable attention..., while/and aircraft sizes...”

R: Done.

P. 951, L. 8: Add “using aircraft” or similar after “techniques” to stress that the following advantages of cars are in relation to aircraft.

R: Done.

P. 953, L. 14-15: “smaller amplitude of car motions” - do you mean compared to aircraft motions? Please specify.

R: Yes; we have added “compared to aircraft motions”.

P. 956, L. 16: Add height of wind speed measurement.

R: Done.

P. 957, L. 2-6: How was the area/street characterized (high rise buildings or rather suburban) and were these surroundings expected to influence the speed bump test?

R: It is a suburban location with a homogeneous grass park upwind of the street. This description is now included in the manuscript.

P. 957, L. 7-17: This experiment can be seen as a separate test, which is not only important for the correction of the speed bump test as it was used here, but is of general use for evaluation of wind speed and turbulence data from moving cars. If so, I suggest putting it in an extra section or changing the title of this section accordingly. The results described here would then be presented in Sect. 4.4 or 4.5.

R: It actually was a separate section in an early version of this manuscript, but since there were no sufficient data for further analysis, we choose to report on it within this section. We have, therefore, opted for changing the title of this section.

P. 957, L. 19: Were the corrections in 4.1 applied to the data both in test 1 and 2? Please specify. If different, also specify which data the found corrections (p. 958) are based on (only data from test 1 or also from test 2)?

R: The corrections are applied to all instrumented car data, which is now specified in the manuscript. The sources of the found corrections are also given now.

P. 958, L. 12: “This” may be misleading in this context. I suggest using “The method applied here”, or similar.

R: Done.

P. 958, L. 15-16: What do you conclude here? I.e., were non-stationarities and hori-

zontal heterogeneities large, so that it had an effect on the described corrections? How large are the corrections and could uncertainties in the correction explain some of the discrepancies between the car and tower measurements?

R: We have included additional discussion of our conclusions. During the first test, the non-stationarity was large and has most likely influenced the results. However, it is difficult to quantify the error of the correction factors, because the correction is based only on the car measurements. Using the tower measurements to correct for non-stationarity is not feasible due to different averaging times and unknown effects of horizontal heterogeneity.

P. 959, L. 16: There seems to be a systematic deviation between car and tower measurements, e.g. for the horizontal wind speed in u-direction (Fig. 3). u measured by the car seems to have a negative deviation from the tower measurements. This may hint to an influence of the car speed, which is not corrected for, since u is orientated closely to the direction of the track. If so, are there suggestions how the corrections or the alignment of wind vector could be improved? Or is the result satisfactory, i.e. within the expected range of uncertainty?

R: The bias in u would seem to be a reflection of the limitations in using a fixed-point wind measurement to compare with the car measurement along a horizontal traverse; that is the differences would likely be due to nonstationarity and horizontal heterogeneity in the wind field. The car speed shouldn’t directly enter into this (although perhaps not completely if there are systematic wind changes), as this should mostly be taken care of by our removing any bias detected when the car traverses the track in the opposite direction. This is now discussed in the text.

P. 959, L. 24-25: Is there a reason why the tower measurements were most likely subject to non-stationarities in the temperature measurements and the car measurements were not? Could the non-stationarities be verified using a stationarity criteria?

R: The main reason is that a single tower flux data point is an average over 10 min, while a car data point is obtained during less than 1 min. In that respect, the car is an almost-instantaneous measurement influenced by horizontal heterogeneity, while the tower is influenced only by non-stationarity. Furthermore, in the cases where the Taylor's hypothesis cannot be applied (e.g., when the mean wind is perpendicular to the car tracks), there is no reason to expect the tower and car fluxes to result from the same flow structures.

As for testing the non-stationarity, there is a myriad of tests and assumptions that could be used, one of which would certainly find the non-stationarity since it is always present to a certain extent in ABL time series. But in our opinion, this would be of limited use for understanding the differences between the car and tower fluxes.

P. 959, L. 25: There is factor of up to 2 between the different heat fluxes, which might be also due to varying fetch conditions of car and tower measurements. Also, there are significant differences due to the measurements with sonic and TC as it is described below. The expression "agree well" is hence misleading. I would suggest writing: "are in the same range between XX and YY K m s^{-1}, or similar.

R: Done.

P. 959, L. 25: A general question on the flux calculations: Did you apply other correction methods which are not mentioned (e.g. coordinate rotation (for tower fluxes), crosscorrelation between w and TC). If so, please state it in the methods section. If not, could this account for some of the systematic differences?

R: We did not apply other corrections to the measurements of the variables on the tower and car. This may account for some small systematic differences in the fluxes, but we do not think that it would change any of our conclusions. Additionally, the correction procedures might not work properly in the current conditions. For example, the very weak winds and variable wind direction over 40 min (total record length) could result in spurious corrections from double coordinate rotation, while the record length is too short for the planar-fit method. Finally, the tower sonic was carefully levelled, and

the terrain is flat and rather homogeneous, so the corrections are not expected to be significant.

P. 959, L. 27: I suggest writing "This heat flux difference: : :" or specify it more clearly to distinguish between the differences between sonic + TC and car + tower.

R: Done, together with modifications addressing other reviewers' comments.

P. 960, L. 6: Is it possible to give a measure of the goodness of the agreement? From first visual inspection the difference in the $\overline{u'v'}$ flux does not seem to be significantly better than, e.g., for $\overline{u'w'}$. As the comparison of the two approaches is challenging, it might be therefore better to give a statistical measure of the flux agreement or the fluxes itself.

R: Bias and normalized RMSE have been calculated and are included in the Supplement. We have also clarified what we mean by better agreement for $\overline{u'v'}$.

P. 960, L. 15-16: Did you also try the opposite test, i.e. extending the sampling period of the car measurements by merging the measurements from the each track at one car speed? If this is possible, the comparison with the tower fluxes would be interesting.

R: We agree — this would smooth the car fluxes and remove some of the noticed issues. However, one of the main issues with the system was that the sign of $\overline{u'w'}$ changed when driving in opposite directions along the same track. Likewise, some other errors of the car measurements can be quantified only when comparing measurements from opposite directions. Therefore, merging the measurements for opposite directions would solve some problems, but at the expense of disguising possible errors. Since the main goal of this paper is to detect and analyse all potential errors of the system, we choose to keep the discussion in its current limits.

P. 961, L. 6-14: To me the effect on $\overline{w'T'}$ is even more significant than as for the other scalars (when comparing the tower fluxes with the TC-fluxes from the car), hence, it might be worth mentioning here.

R: It is now mentioned, together with the temperature variance.

P. 962, L. 8: Please specify which $\overline{w'T'}$ *measurement you refer to. From the car measurements, the heat flux calculated from the sonic increases with time, however, this was attributed to the overestimation at higher speeds. The TC heat fluxes from the car measurements do not increase with time. For the tower heat fluxes, only the last three tracks exhibit an increase of the heat flux with time. Instead, there is a steady increase of* $\overline{w'^2}$ *from the car measurements with time in Fig. 4. Could this hint to an increase of turbulence at the sensors at higher car speeds, which would lead an increase in the variance spectra with increasing car speeds?*

R: Good point — the increase in the vertical heat flux is not that obvious for all the four instruments. However, the temperature variance clearly increases with time for all the instruments, indicating the time-developing mixing. This has been corrected in the manuscript. On the other hand, the car vertical velocity variance, although somewhat increasing, remains very close to the tower measurements, and hence does not imply an overestimation by the car. Finally, the increase is very small and thus negligible considering the relatively wide range of tested car speeds.

P. 963, L. 1-4: The difference between the two contributions (black and purple lines) seems to be between 1 (for u), 2 (for w) and 3 (for v) orders of magnitude rather than just one.

R: We have rephrased this to *at least* rather than *about* an order of magnitude smaller.

P. 963, L. 22: Are there any improvement that can be made to the presented setup, which you may suggest in this section for future turbulence measurements using cars? Also referring to its application in the stable ABL (i.e., whether the presented setup could be also used for the stable ABL or which further tests/development would have to be done in the future) would be important since this was given as a main motivation of the presented study.

R: We now discuss possible improvements of the system, as well as the application to the stable ABL, in the Conclusions.

P. 969: I would prefer writing "average horizontal car speed" if the average car speed is shown.

R: Done.

P. 971: If possible, mark both the 700 and 900 m track.

R: Done.

3 Technical comments

P. 950, L. 2: Typically no comma before "because" is used (only use comma to avoid misreading). This also applies for the rest of the manuscript.

R: This and a number of other such occurrences have been corrected.

P. 953, L. 8: Add country of manufacturer.

R: Done.

P. 963, L. 12-15: Use past tense since this is section describes the method that was used.

R: Done.

P. 975, top-right panel: y-axis unit should be also "m^3 s^{-2}".

R: Corrected.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 949, 2014.