WIND ENERGY SCIENCE

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DISCUSSIONS

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Interactive comment

Interactive comment on "The fence experiment — full-scale lidar-based shelter observations" *by* A. Peña et al.

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We are very thankful for your comments because they are very helpful and constructive. Here follows our response item by item. The response is given as: xxx— response — xxx

As the authors comment, there are relatively few field data on wakes behind fences or indeed behind other surface mounted obstacles, and the study reported by Peña et al (2016) is a valuable addition. They could however have noted the study by Wilson (2004) of flow behind a windbreak of porosity 0.45. One advantage of Wilson's study was a high aspect ratio (fence length (L)/fence height(h)) of 91.2 compared to only 10 in the Peña et al case.

xxx— We were not aware of the study by Wilson (2004). The study is now referred to

in the revised version of the paper (first in the Introduction and later in the Conclusion and Discussion section)—xxx

In order to consider the flow as approximately 2D for comparison with the Counihan et al (1974) theory it would be desirable to have a much longer fence section. The measurements are relatively close to the fence (x/h <11 or x/L < 1.1) but pressure effects act in an isotropic manner and one might expect some departure from two dimensional behaviour.

xxx— We agree with the comment and we now add a sentence in the Conclusion and Discussion section related to this issue —xxx

The experimental situation is also complicated by the water to land roughness transition at the coastline about 78 m away from the fence. Checking Google Earth (my version of which still shows the fence in place) indicates virtually no step change of elevation at the waters edge. This is good, but there will be some impact from the roughness and possible heat flux changes within the internal boundary layer. The effective roughness lengths listed in Table 4, computed from friction velocity and wind speed measurements via Equations 1 and 2, are mostly of order 0.002 m.

xxx— We are aware of these issues and we mentioned them along the lines 12âĂŤ14 in Page 11 and lines 29âĂŤ30 in Page 13 of the original submission —xxx

There is an error in Equn 2 (exponent 1/2 should be 1/4), which I assume is typographical, but the roughness lengths are lower than normally expected values for a grass covered surface (∼0.01m).

xxx— Yes, this is a typo and $\frac{1}{4}$ is indeed used in the calculations. The low roughness lengths are due to the influence of the upstream surface conditions —xxx

The flow at the measurement heights 6m and 12m will not have fully adjusted to the land surface change by the time it reaches the fence where the internal boundary layer depth in neutral conditions for flow normal to the shoreline would be ∼8m from

simple guidelines, Walmsley et al (1989). Certainly the shear stresses at measurement heights will be low - more characteristic of the over water conditions - and lead to the computation of relatively low z0 values. Impacts on speed-up factors may however be small.

xxx— We agree with the reviewer's comment (see our statements in the original submission between lines 30âĂŤ34 in Page 17 and lines 4âĂŤ5 in Page 18) —xxx

The triple Doppler lidar windscanner approach is an excellent remote sensing addition to in- situ measurements with cups or sonics but there are limitations in terms of sampling volume and turbulence measurements. The flow being measured in this instance would be fully accessible with masts and sonics and these could have provided an alternative or supplementary measurement technique.

xxx— We agree with the reviewer and we believe that with the actual state of the WindScanner, it is still recommendable to supplement the measurements with those from cups and sonics. However, we didn't have the possibility to do so and we wanted to avoid distortion from the masts and booms both on the winds on the vertical plane and on the cups and sonics themselves (which seemed to be a concern in Wilson, 2004). It is also important to note that such flow is challenging for cup anemometers as their accuracy degrades with flow angle. We now also mention these challenges in the Conclusion and Discussion section —xxx

The direct application to wind turbine siting is rather limited since the measurements are restricted in height ($z/h < 3$) and downwind extent ($x/h < 11$).

xxx— We do not agree with the reviewer. A direct connection to turbine siting is made when, e.g., looking at small wind turbines. In some countries, like Denmark, small turbines (<25 kW) are normally installed close to buildings both vertically and horizontally ($z < 25$ m and $x < 20$ m). We now provide some more clear statements related to this in the Introduction of the revised version —xxx

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Additional turbulence measurements would have been useful for considerations of potential wear or breakdown for small wind turbines.

xxx— This is definitively true and perhaps we should have also tried to perform turbulence measurements with the three lidars focusing at the same point for a long period to look at spectra characteristics. We now also mention this in the Conclusion and Discussion section of the revised version —xxx

A minor suggestion related to Equations (10) and (11) would be that <ln(z0)> might be a better option than ln<z0>.

xxx— We tested both options and there is no difference when estimating z_o. This is because <z_o> in Eqns. 10 and 11 is the result of averaging the z_o values in table 3 within the appropriate \theta interval for each case. For example for case 2 (0+-30 deg.) we have 6 z_o values (-25:5:25) deg. —xxx

The WEMOD and WASP-Shelter models are discussed with the comment that they are based on Counihan et al (1974) and 2-D models. While that is true in part there is also careful consideration of wakes behing surface mounted 3D obstacles in Taylor and Salmon's (1993) WEMOD model and 3D effects may play a role in the present study. A major uncertainty in applications of WEMOD is the estimated value used for the parameters Ch and Chtilde based on the drag and couple on the object. For an infinite 2D fence WEMOD suggests that Chtilde= 0.8 (1-φ) where φ is the porosity (= 0 for the solid fence). However for a finite length fence section there is a suggestion that the coefficient (0.8) should be reduced (0.2 - 0.4) for "long low buidings". We have run both cases (0.8 and 0.4) for the 10h length fence and for all flow directions.

xxx— We agree with the reviewer and in the revised version we now add that both WEMOD and WAsP-shelter make considerations about 3D obstacles —xxx

Running WEMOD for the Peña et al solid fence and with z/h = 0.46 we get "speed-up" results, U(x,z)/U0(z), shown in Table I. The Peña et al results were extracted from

Figure 9 of their paper, estimating values appropriate to cases I and II for flow normal to the fence, cases III and IV for flow at + 30° and -30° to normal (ignoring the slight asymmetry in the set-up) and case IV for flow at -60° to normal. WEMOD results are averages of calculations at 1° intervals within +/- 15° of the nominal direction. We set $z0 = 0.002$m as a representative value. WEMOD is a "far wake" model, intended primarily for $x/h>6$ but even that range can be optimistic for a solid 2D fence. Comparing estimates with Chtilde = 0.8 with the Peña et al measurements at $z/h = 0.46$ it is clear that the WEMOD model overestimates the wind speed reductions at all x locations while with Chtilde = 0.4 it generally underestimates them until $x/h = 10$ where, perhaps fortuitously, they match for all flow direction bins.

The data set for a porous fence ($\varphi = 0.375$) is for flow at an angle of -30° to the normal to the fence (Case VII). At $z/h = 0.46$ we estimate the "speed-up" from Figure 9 in Peña et al to be 0.75 at x/h -= 10 and 0.45 at $x/h = 6$. The WEMOD model predicts less sheltering, even with Chtilde = 0.8, averaged over +/- 15° for this flow direction (from 30° to left of the upwind normal to the fence) and has corresponding speed-up values of 0.89 and 0.63.

In contrast to these examples of poor results from WEMOD, Table II presents comparisons with the measurements reported by Wilson (2003). As with Table I there is averaging of WEMOD calculations over 1° values within +/-15° of the wind directions indicated. There are sometimes a range of values extracted from Wilson's plots because of stability differences and the values in the table are intended to span neutral conditions. In general WEMOD values (with Chtilde = 0.8(1-φ) since the Wilson fence has $L/h = 91.2$ and we are looking at distances $x/h < 20$) are within or close to the range reported by Wilson. An exception is for $x/h = 4$ with flow normal and at 30° to the upwind normal to the fence but this is close to the fence and not in the far wake for which WEMOD application is anticipated.

There are differences between the plastic windbreak fencing used by Wilson and the porous wooden structure used in the Peña et al study, and in the different lengths of the

two fences but porosities were similar. A comparison of speed-up values for three flow directions (normal, +/- 30°, +/-60°) at x/h = 10, z/h $\approx$ 0.5 shows 0.73, 0.78, 0.89 with porosity 0.45 from Wilson and 0.75, 0.9, 1.0 with porosity 0.375 from Peña et al. For flow normal to the fence these are compatible but for 30° and 60° angles the relatively short fence in the Peña et al study may allow flow around the ends which increases the speed-up.

xxx— We appreciate that Dr. Taylor has already made some comparisons of his model with the data. That is the main purpose of the paper. We would like to remind that the reader does not have to extract results from the figures because we provide the data at the weblinks, which are mentioned in the paper. Also and most important, we believe that when performing model evaluation, consideration of the direction distribution is needed as we have also seen that the results are very dependent on this. The direction distribution per case is also available at the weblinks. We are very much certain that if the evaluation is performed taking into account the direction distribution, the results of WEMOD will generally be closer to the observations —xxx

In the near wake region there is relatively strong reverse flow (u < 0) but it is not clear to what extent the wind speed change is affected by the v component, especially for flow at 60° to the fence normal (Case IV, Fig 9, z/h = 0.21) where it appears that a vortex parallel to the fence may exist. Separate plots of u and v components, perhaps normalised by U0(h), in addition to Figure 9 could provide additional information.

xxx— We thought about showing both components (u and v) but, in the original submission, we decided to concentrate on the horizontal wind speed only. However, we now have both u and v components as we also think they provide additional information —xxx

p14 I am puzzled by the <~>ws combination since (p10) the $\sim$ symbol appears to indicate sonic measurement over a time interval corresponding to a full WS scan. Also I assume U(z) should be U(x,z) in this context

xxx— Page 10 states that $\sim$ corresponds to sonic statistics based on the time period that a full-scan is completed. Perhaps the problem is that we say "sonic statistics" and it should be just "statistics" no matter the instrument. This is now corrected in the revised version and U(x,z) is also now used when appropriate —xxx

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