COMMENTS ON 'A NUMERICAL SIMULATION OF BOUNDARY-LAYER FLOWS NEAR SHELTERBELTS' BY H. WANG AND E. TAKLE

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Abstract. We implemented the windbreak model of Wang and Takle to investigate why those authors could have obtained better agreement with the Bradley and Mulhearn wind reduction measurements in the far lee of a fence, than was obtained by Wilson using a very similar model. According to our experience the 'improvement' of the Wang-Takle simulations (relative to Wilson's) largely arises from their having used a too-shallow computational domain (8H, versus Wilson's 47H; H being the windbreak height).

1. Introduction

Recently Wang and Takle (1995; hereafter WT) described a method for the numerical simulation of shelter flows, and in applications of essentially that model they investigated the influence of windbreak alongwind thickness (Wang and Takle, 1996a) and the flow pattern in oblique winds (Wang and Takle, 1996b). We comment here on what we consider to be problems with the WT paper, including a discrepancy in the citation of the experimental data of Bradley and Mulhearn (1983; hereafter BM83), and some ambiguities in the model itself. We have attempted at length to reproduce the WT model, which we reconsider in relation to similar work by Wilson (1985; hereafter W85).*

2. WT and W85 Models of Ideal Shelter Flow

To focus on the degree of harmony of the WT and W85 models** with the BM83 data, the best available for a neutrally-stratified, equilibrium surface layer disturbed

* We are indebted to Drs. Wang and Takle for their willing provision of additional details on their model, which greatly helped our task.

** For convenience we speak of 'the W85 model' as if singular; whereas in fact, W85 analysed several models, including the simplest K-theory closure ($K = K_0(z) = k_v u_{*0} z$), through the '$k - \epsilon$' closure, to a second-order closure. However as regards the differences in model output, all these models are similar, and collectively different from WT.

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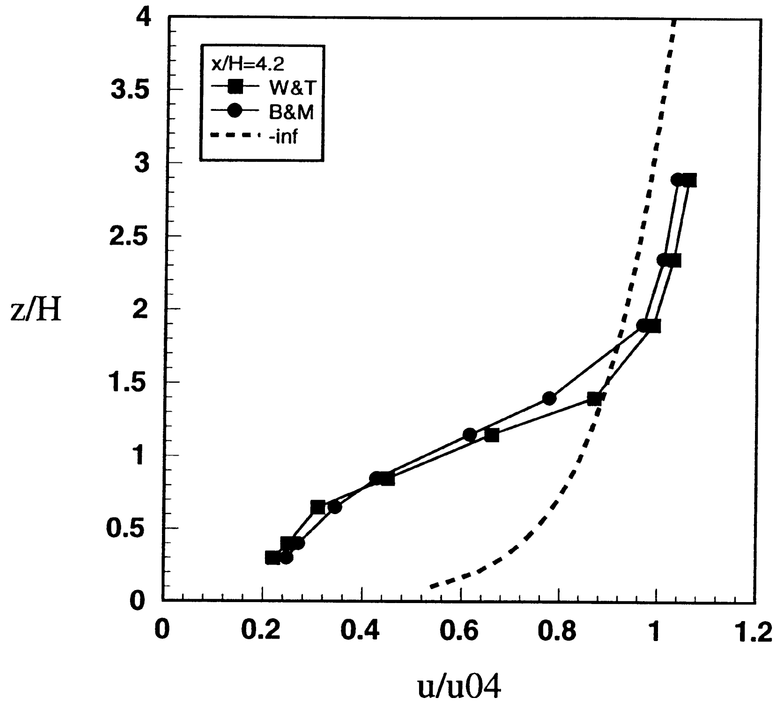

Figure 1. The mean wind profile $\bar{u}/\bar{u}_{04}$ (where $\bar{u}_{04}$ is the observed mean windspeed far upstream, at height z = 4 m) observed by Bradley and Mulhearn (1983) at distance x/H = 4.2 downstream from a windbreak, in comparison with the same data as quoted by Wang and Takle (1995), who extracted them from Wilson (1985). The Wang and Takle simulation of these data is given as their Figure 4b.

by a thin porous barrier, let us compare WT (Figure 4a) with Wilson's (Figure 7), and WT (Figure 4b) with Wilson's (Figure 4). WT report better agreement with the observations, and, in particular, their calculations do not manifest a feature common to all closures examined by W85 – an under-recovery (relative to observations) in the far lee. The motivating question behind our comments on WT is this: if the WT model performs so well, but W85 not so well, then why? What are the implications for calculating the variety of disturbed windflows of interest in micro- and agro-meteorology?

Now concerning WT (Figure 4b), which reports exceptionally good agreement of their model with the 'observed' wind profile $\bar{u}/\bar{u}_{04}$ at x/H = 4.2, the experimental points were stated by WT to have been taken from W85 (rather than from the original work of BM83) – but the 'observed' data are wrong, and differ from those provided by BM83 and by W85. Figure 1 compares the 'data', as cited by WT, against the actual field data for the same points; there are errors of up to about 12%.

Possible causes of differences (WT vs. W85) in model output can be classified as follows: differences in the governing differential equations (which are minor in this case, except possibly as regards specification of the fence drag, a point we discuss below); differences in the turbulence closure; differences in the boundary conditions; and differences in the numerical method (including such factors as domain size and resolution, as well as the discretisations).

2.1. Differences in turbulence closure

WT used a closure due to Mellor and Yamada (1982), in which the eddy viscosity is derived from the turbulent kinetic energy (TKE; for convenience we also sometimes label the TKE as 'E' or 'k') and the TKE-mixing length product (El), for both of which quantities transport equations are solved. This closure falls within the span of those tested by W85, and is logically very similar to the '$k - \epsilon$' closure.

We consider the differences between the WT and W85 closure schemes as probably not highly consequential. It is not hard to show, by extending the analysis of Wilson et al. (1990), that the common first-order closures are asymptotically equivalent, in the limit of a very porous barrier $k_r \ll 1$ (to first order in k_r, and zeroth order in m, the index in the power law for the equilibrium mean wind). Furthermore, for that asymptotic flow, the rate of recovery of the velocity field is very slow – giving grounds for suspecting that, even for larger k_r, a 'too-slow recovery' *may* be a characteristic deficiency of all models based on present closures. What is certain, is that the WT equation *must* imply a 'slow' rate of recovery, in the same manner as other treatments, in the case $k_r \ll 1$.

2.2. Differences in parametrization of drag

W85 specified the momentum sink for these ideal shelter flows as,

$$S_u = -k_r\,|\bar{u}|\,\bar{u}\,\delta(x,0)\,s(z,H) \tag{1}$$

where the delta function (units m^{-1}) localises momentum removal to x = 0, and the unit step function $s(z,H)$ localises to $z \leq H$. Using a 'control volume' method, there is no difficulty in implementation of such a sink, for the δ function merely has the effect, upon integration of the momentum equation throughout a control volume, of ensuring that the correct amount of momentum is removed near x = 0, i.e., within the control volume spanning the fence. No local refinement of the gridlength is called for. Care is necessary, however, in the case of an an elementary gridpoint discretisation, in finding the correct 'computational molecule' D_{ij}. This should normally be of the form

$$D_{ij} = \left\{ \begin{array}{cc} \Delta x^{-1}, & i = j \\ 0, & i \neq j \end{array} \right\} \tag{2}$$

where Δx cannot be independent of the corresponding interval in whatever computational molecules are employed for other terms in the equation.

WT treated the Bradley–Mulhearn fence not as an infinitesimally-thin barrier localised at x = 0, but as a barrier of finite width. They specified the momentum sink as

$$S_u = -c_d\, a\, \bar{u}\sqrt{\bar{u}^2 + \bar{v}^2 + \bar{w}^2} \tag{3}$$

where $a(x, z)$ is an effective drag-area density. This is an entirely logical procedure, especially for a thick barrier. Their projection of the mean drag force onto the x-axis is correct, and in principle superior to Wilson's $\bar{u}|\bar{u}|$, but that refinement is inconsequential for the simulations in question, with the mean flow normal to the fence ($\bar{v}$ = 0; small $|\bar{w}|$). Two important and related questions do however arise: how did WT determine $c_d a$ for the Bradley–Mulhearn fence from a given value of k_r, namely k_r = 2? Secondly, how did WT accomplish a region of non-zero $c_d a$ *thin enough* to resemble the Bradley–Mulhearn fence – a step that requires high spatial resolution – and simultaneously cover a model domain of order 10H × 100H, reportedly with uniform resolution?

As indicated by W85, an approximate relation between k_r and $c_d a$ for a barrier of finite alongwind-width is

$$k_r = \int_{-\infty}^{\infty} c_d a \, \mathrm{d}x = \sum_i (c_d a)_i \Delta x_i, \tag{4}$$

which ignores alongwind variation of the windspeed across the (finite-thickness) barrier. In a gridpoint* model, there is room for ambiguity in the evaluation of $c_d a$ through Equation (4), which involves summation over all grid-points spanned by the barrier. WT give no details. Thus in our opinion, it remains for the authors to ascertain and report whether they removed the correct amount of momentum from the simulated BM83 flow, and the means by which they did so.

2.3. Differences in Treatment of TKE Sources

The comments of Section 2.2 apply also to the manner in which WT dealt in their numerical procedure with fence-sources in their other equations. However in the case of the turbulent kinetic energy, there are additional differences (from W85) and difficulties.

Drag of the barrier on the flow extracts kinetic energy from the mean flow (MKE) at a rate of approximately $S_{\mathrm{MKE}} = c_d a |\bar{u}| \bar{u}^2$. That energy is transformed into TKE of the small 'wake eddies', i.e., to TKE at high wavenumber. Similarly, kinetic energy of the 'larger' eddies is transformed by the fence drag to kinetic energy in the wake scales. Nevertheless measured spectra of TKE generally do not

* By using the term 'gridpoint model' we mean to distinguish between numerical methods in which discretisation is applied to the differential equations (WT use such a model) and models in which discretization is carried out only after spatial integration of the equations, within a set of control volumes filling the computational domain (notably the SIMPLE scheme; Patankar, 1980, 1981).

reveal noticeably augmented energy at wake scales, a fact which is attributed to a concomitant high rate of dissipation of these small scales of motion.

It is impossible to know the 'correct' influence of the unresolved processes at the fence on TKE and its dissipation rate. Proper spatial averaging throws up 'formal' interaction terms, but in parametrization of these, practises vary. The question arises, ought the TKE refer to the *entirety* of the spectrum, or should it exclude wake-scale energy (WKE)? Logically, it is advantageous to split the TKE spectrum, separating WKE from SKE (SKE labelling 'larger-scale kinetic energy'), e.g., Shaw and Seginer (1985), and Wilson (1988). It can be argued (Wilson, 1988) that in the budget equation for SKE, the source S_{MKE} should be omitted (it appears instead in the WKE equation), while a *sink* of strength $Q = \alpha c_d a|\bar{u}|E$ (where α is order 1) should be included to account for wake conversion of SKE to WKE. Indeed, the equivalent (SKE–WKE) treatment used by W85 in the $\langle u'^2\rangle$-equation of a second-order closure provided good qualitative agreement of the simulated field of $\langle u'^2\rangle$ with the data (given by Finnigan and Bradley, 1983) from the BM83 flow.

Unlike W85, and following Green (1992) and Green et al. (1994), Wang and Takle implicitly considered their 'E' as measuring the total of TKE. They included S_{MKE}, and omitted the sink Q (above), which merely shifts energy across scale, without altering their E. Also following Green, WT in effect adjusted the dissipation rate, or rather, since their closure used the 'El' equation rather than the ϵ-equation, they adjusted empirically a source term in the 'El' equation.

2.4. Differences in Numerical Method

W85 used a variant (SIMPLEC; van Doormal and Raithby, 1984) of Patankar's (1980) popular SIMPLE method. As this is well documented, in general terms by Patankar and in its application to windbreak flow by W85, we need mention only the most significant differences from the WT procedure. SIMPLE is a control volume method, based on a staggered grid, and is applicable either to time-dependent or steady state problems.

WT used finite differences on a uniform, unstaggered grid. Pressure-velocity coupling was accomplished using a scheme of Chorin (1968), and involved solving the diagnostic equation for $\nabla^2\bar{p}$. The simulation proceeded through a time evolution to a steady state.

2.5. WT Interpretation of Differences (WT vs. W85)

WT appealed to their pressure field as explaining their 'faster recovery rate of windspeed'. In particular they noted (their Figure 5b) a weak gradient $\partial_x\bar{p}$ in the immediate lee, followed by a stronger gradient beyond that zone (these features are not terribly well demarcated on their figure, a contour diagram). This is quite different from the W85 mean pressure field (W85, Figure 11). The paragraphs that purport to explain how WT could have arrived at a different velocity field from essentially the same set of equations as W85 are difficult to understand.

This raises the possibility that field data for $\bar{p}$ about a windbreak might distinguish whether indeed the WT model is qualitatively superior. One of us (JDW) has carried out trials about a thin fence on bare ground, or within an alfalfa canopy. With or without a canopy, there is no compelling evidence of a leeward plateau in pressure.

3. New Calculations Using the WT Model and the W85 Model

3.1. WILSON'S (1985) SIMULATIONS REPRODUCED

As a starting point in the reconciliation of the WT and W85 models, one of us (CJM) programmed from scratch the W85 simulation labelled therein as 'K_0', i.e. the model with eddy viscosity $K(x, z) \equiv K_0(z) = k_v u_{*0} z$. The original (published) results were reproduced to within three significant figures. An obvious question is whether the higher resolution ($\Delta x/H$ = 0.5; $\Delta z/H$ = 0.1) of the WT simulations might account for their results. However, as indicated by W85, we found that a dramatic increase in resolution does not appreciably alter the outcome of Wilson's model. On the other hand, WT used a shallower domain than W85 found necessary, and we believe this has a bearing on their calculated velocity field.

3.2. IMPLEMENTATION OF THE WANG-TAKLE MODEL

3.2.1. *Using SIMPLEC rather than the WT Numerical Scheme*

We chose to examine differences between the WT and W85 models using SIMPLEC. This means we should reproduce WT exactly only in the limit that our numerical procedure is equivalent to theirs. Why then, do we investigate the WT model (in all aspects but for its numerical scheme) using SIMPLEC?

Firstly, whereas SIMPLEC is documented to the point of being unambiguously defined down to the algorithmic level, in all but a few matters of minor importance (such as location of gridpoints within the control volume), the WT numerical method is not fully known to us. Within the compass of a set of differential equations discretized using a specific choice of computational molecules, there remains much room for choice in computational procedure.

Secondly, we *assume* SIMPLEC is a 'correct' numerical procedure, in the sense that if used to integrate a 'consistent' discretization of the model equations (i.e., if applied to discretization equations having the property that 'truncation error' vanishes in the limit of small gridpoint separations), the solution provided using SIMPLEC will 'converge' to the correct solution in the same limit. This is essentially the promise of the Lax Equivalence Theorem (Ames, 1977), which in short implies that if a numerical method is correct, it is irrelevant. Thus, if the

WT numerical method is correct, we stand to loose nothing by opting instead for SIMPLEC. While, if it is incorrect, but we set up in all other respects the WT model, then our different findings might help reveal that problem.

3.2.2. *Additional Details of the Wang and Takle Model*

We were greatly helped by provision of the following information (E. Takle, pers. comm.) not available in the published literature. In order that the set of equations should produce a 1-dimensional (1D) solution with the ideal structure of an equilibrium surface layer, it is critical to eliminate finite-differencing errors in the estimation of the derivative $\partial\bar{u}/\partial z$ in the E and El equations. This was achieved by WT using (E. Takle, pers. comm.) a 'logarithmic derivative', namely

$$\left(\frac{\partial\bar{u}}{\partial z}\right)_{z(j)} \leftarrow \frac{\bar{u}(j+1)-\bar{u}(j-1)}{\Delta z}, \quad \Delta z \equiv z(j)\ln\left(\frac{z(j+1)}{z(j-1)}\right). \tag{5}$$

Using Equation (5), and provided that at heights $z(j-1)$, $z(j)$, $z(j+1)$ the windspeed is exactly equal to the ideal value, viz., $\bar{u}(j) = (u_{*0}/k_v)\ln[z(j)/z_0]$, a numerical estimate of the wind shear $\partial\bar{u}/\partial z$ at height $z(j)$ will return exactly the ideal value $u_{*0}/(k_v z(j))$. With Equation (5) WT achieved inflow profiles that were in 1-D equilibrium, so that if a barrier with k_r = 0 was imposed, the inflow profiles were retained for all x. Inflow profiles provided us by WT (H. Wang and E. Takle, pers. comm.) were very satisfactory (wind speed was logarithmic; TKE closely height independent).

We also learned from Dr Takle that the upper boundary condition on the quantity El was $\partial_z(El) = k_v E$, and that the barrier was given a small, finite width, resolved (as we understand it) by a high-resolution region on the elsewhere-uniform grid.

3.2.3. *Our implementation of WT*

As far as possible, our procedure in formulating the WT model for integration using SIMPLEC (we henceforth label this model as 'S-WT', for SIMPLEC-WT) was to follow W85; thus for any details not mentioned, please refer to W85.

One deviation from W85 in our construction of S-WT was that we co-located the quantities E, El with the $\bar{u}$-velocity gridpoints. This was so that we might implement the WT boundary conditions as naturally as SIMPLEC can permit. Imposition of the WT boundary conditions was not completely trivial, and in manner of achievement depended on our choice of variable- and boundary-positioning (e.g., will the ground run through gridpoints for TKE, or not?). In uniform-grid simulations, we were able to follow WT exactly, and specify $\partial_z(El) = k_v E$. However, in simulations using a non-uniform grid, it is difficult to unambiguously impose that condition, and so we instead set $El = k_v z E$, $E = E_0$ (E_0 the inflow TKE) at our uppermost gridpoint (z/H = 47). We satisfied ourselves, e.g., by studying the outcomes of choosing either flux- or concentration-type boundary conditions at 47H, that none of our choices regarding implementation of boundary conditions affected the outcome of our S-WT simulations.

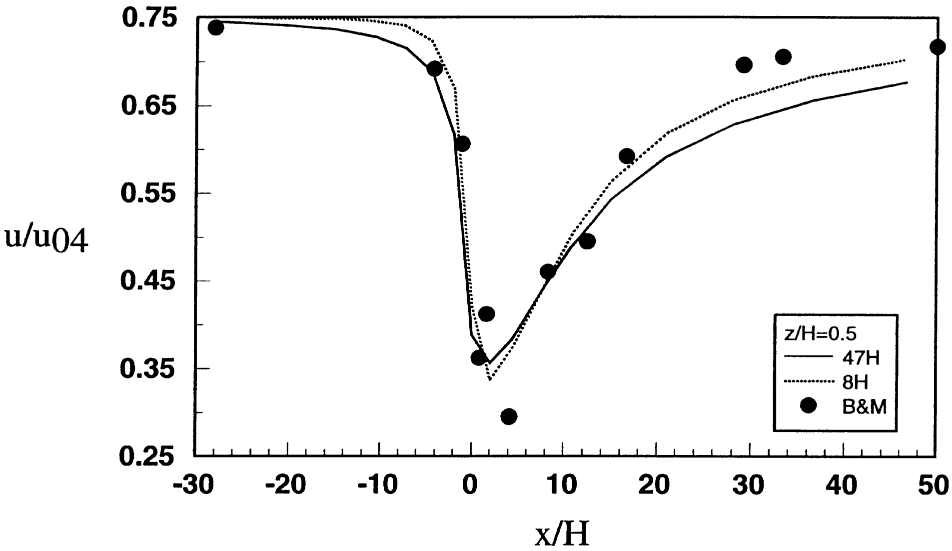

Figure 2. The influence of domain size on the outcome of windbreak simulations: our implementation ('S-WT ') of the Wang and Takle (1995) model, using domain depths of 8H (the value used by those authors), and 47H (as used by Wilson, 1985). A too-shallow domain makes illegitimate the assumption of non-disturbance at the top boundary, and promotes spuriously rapid recovery, through accentuating the model wind shear above the windbreak.

3.3. Outcome of SIMPLEC implementation of Wang and Takle ('S-WT')

Because our simulations using our implementation of the WT model are focused on the BM83 flow, we set k_r = 2.0, H/z_0 = 600. Figure 2 compares our S-WT simulations for two choices of the domain depth and grid resolution, the WT specification (8H with $\Delta x/H$ = 0.5, $\Delta z/H$ = 0.1), and the W85 specification (47H, with non-uniform and *coarser* resolution, $\Delta x/H \leq 2$, $\Delta z/H \leq 0.25$). In our opinion a domain of 8H is insufficient,* and influences the model results to an unacceptably-large degree. We do not believe this undue sensitivity of the calculated $\bar{u}$ field is due to the differing resolution.

Figures 3a,b show the outcome of a S-WT simulation, to the best of our knowledge following the WT specification (domain depth 8H, etc.). Also shown are the Bradley–Mulhearn data, and Wilson's '$k - \epsilon$' simulation. Both the models estimated the wind reduction in the near lee quite well; but whereas the '$k - E$' model gives a too-slow downstream recovery, the 'S-WT' model shows a recovery of the velocity field (see Figure 3b) in good agreement with the observations. This is consistent with the results reported by Wang and Takle.

* According to our simulations, the streamwise extent ($-30 \leq x/H \leq 50$) of the WT domain is also inadequate, though less seriously so. We find that the windbreak-induced pressure gradient does not vanish 30H upstream.

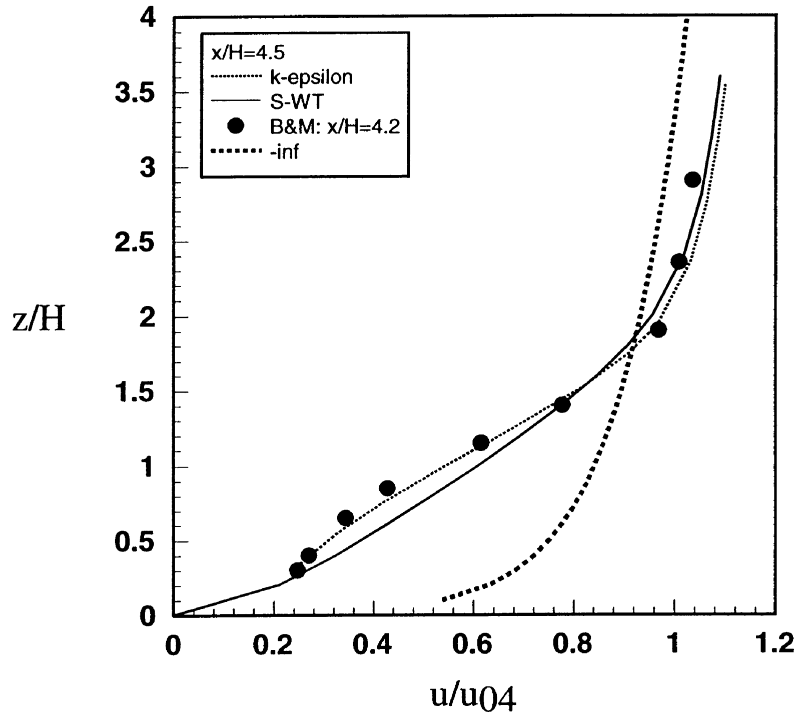

Figure 3a. Bradley–Mulhearn observations of the vertical profile $\bar{u}/\bar{u}_{04}$ at x/H = 4.2, versus simulations using our implementation of the Wang–Takle model ('S-WT'), and using the '$k - \epsilon$' model. Domain depth 8H, resolution $(\Delta x/H,\ \Delta z/H)$ = (0.5, 0.1). The approach profile is the semi-log law, with H/z_0 = 600, a close fit to the observed equilibrium profile.

However, when adequate domain depth is provided, the situation changes (Figures 4a,b). Both models underestimate the degree of speedup aloft (around z/H = 2) relative to observations, and the Wang–Takle model, like the '$k - \epsilon$' model, though not so badly, does not show the 'correct' rate of downstream recovery. The corresponding pressure fields are given in Figure 5. Our 'S-WT', as with the '$k - \epsilon$' model, shows no hint of a 'leeward pressure plateau'.

Finally, we were unable to reproduce anything close to the WT pattern of TKE. If we included in the E-equation the source term $S_{\text{MKE}} = c_d a \bar{u}^3$, and the corresponding source in the El-equation, as WT report having done (though we do not know how WT imposed these sources in their code), we obtained a peak TKE level that exceeded the approach value E_0 by 1700% (approach $E_0 = 6.6u_{*0}^2$; peak value in our simulation, $110u_{*2}^2$), which is certainly unrealistic. But WT reported a smaller increase, of about 400%, which is in reasonable qualitative agreement with the Finnigan–Bradley observations.

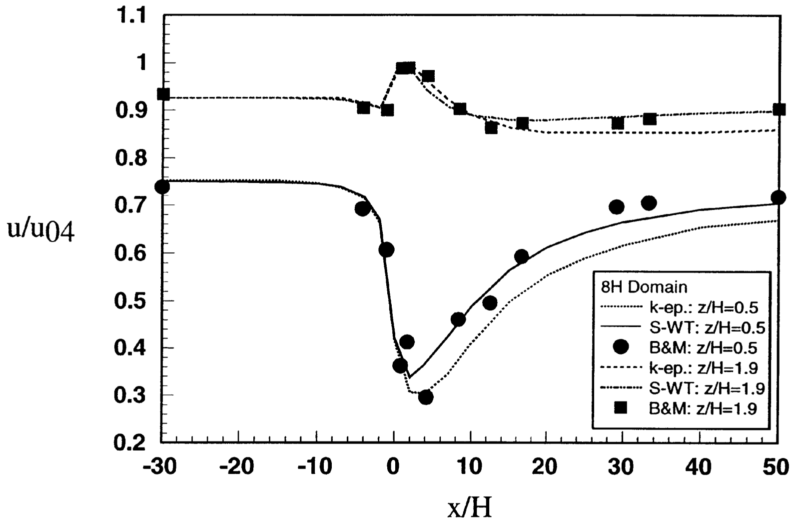

Figure 3b. Bradley–Mulhearn observations of the horizontal profile of $\bar{u}/\bar{u}_{04}$ at z/H = (0.5, 1.9), versus simulations using our implementation of the Wang–Takle model ('S-WT'), and using the '$k - \epsilon$' model. Domain depth 8H, resolution ($\Delta x/H$, $\Delta z/H$) = (0.5, 0.1).

Modestly differing treatments of TKE sources and sinks had but a small influence on the resulting wind-speed field.* This is surprising in view of the central role of the eddy viscosity in the $\bar{u}$-momentum balance, and we can only suggest a type of feedback probably exists in closures of the '$k - \epsilon$' type, such that a given 'error' in TKE leads to a compensating change in the accompanying length scale (e.g., through the ϵ-equation or its equivalent in the WT scheme, the 'El' equation). In view of WT's 400% TKE increase, and our 1700% increase, there is an unaccountable and major difference, whose cause we have not been able to determine, between the WT treatment of TKE and our implementation of what, as we read it, they did. Perhaps this unknown difference is the cause of our differing mean velocity fields. The 'correct' treatment of TKE sources and sinks at a porous barrier is unknown, and we will therefore not dwell on this point.

4. Conclusion

Although we were unable to reproduce the WT simulations exactly, we have shown that the implementation of their model (our 'S-WT' model) and Wilson's (1985) '$k - \epsilon$' model, respond in the same way to a reduction of the domain depth from an adequate 47H (Wilson's choice) to an inadequate 8H (WT), namely by producing a more-quickly recovering mean velocity field. Thus, we believe that differences

* A similar indifference of modelled $\bar{u}$ to details of the turbulence closure has been noted in the case of flow over hills (e.g., Taylor et al., 1987).

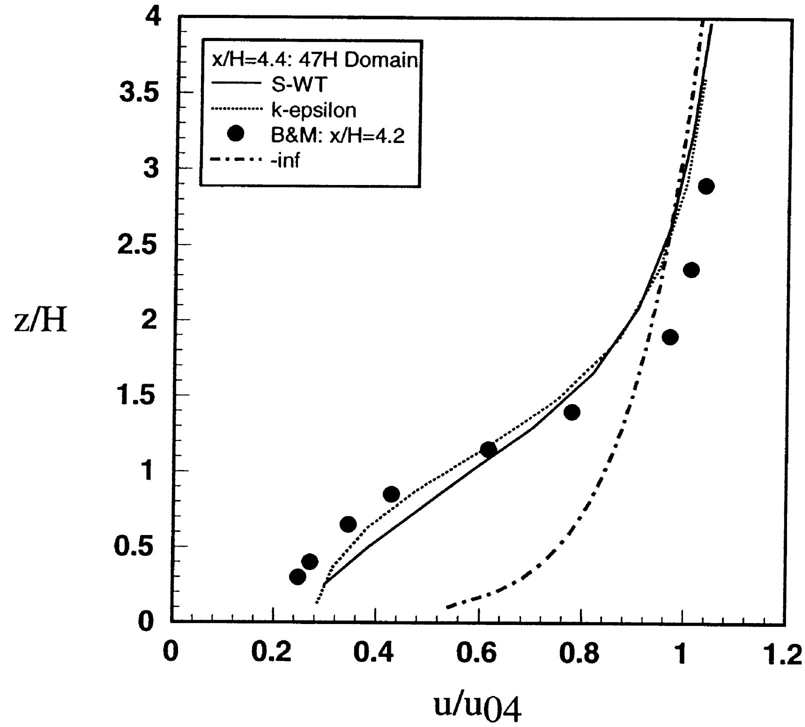

Figure 4a. Bradley–Mulhearn observations of the vertical profile $\bar{u}/\bar{u}_{04}$ at x/H = 4.2, versus simulations using our implementation of the Wang–Takle model ('S-WT'), and using the '$k - \epsilon$' model. Domain depth 47H, resolution $(\Delta x/H,\ \Delta z/H) \geq (2.0,\ 0.25)$. The '$k - \epsilon$' simulation reproduces that reported by Wilson (1985; Figures 4, 6).

between the simulations reported by WT and by W85 originate not from the closure, but from other differences: particularly the domain depth, but also (to an extent we cannot ascertain in the absence of a more-complete specification by WT of their model), their treatment of fence sources/sinks, and their numerical method.

We may have focused unduly here, on small differences between models. Indeed, Wilson (1985) may have overstated the significance of his finding, that his simulations recovered in the far wake somewhat more slowly than the observations suggested; he considered this a 'deficiency' of the closures examined. In fact, it is not clear that the 'deficiency' (of Wilson's simulations) or its 'correction' (by Wang and Takle) is significant. Perhaps these differences, between models, and relative to the Bradley–Mulhearn data, are too small to warrant attention, in view of what we might call an 'irreducible uncertainty level' of simulations. This includes a noise level arising from the variations in grid resolution, in domain size, in the manner of specifying windbreak influence on turbulent kinetic energy, etc., – variations that are bound to occur across differing studies. And for that matter, the uncertainty level of the data themselves is unknown – not to mention the fact that even highly

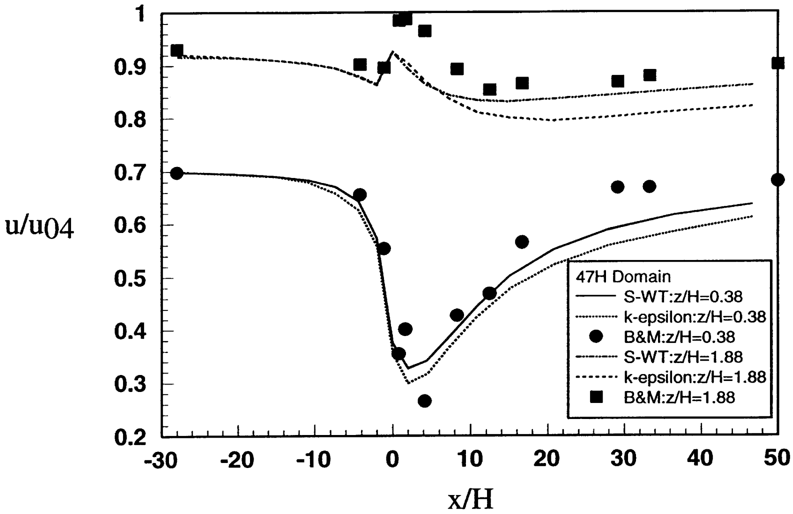

Figure 4b. Bradley–Mulhearn observations of the horizontal profile of $\bar{u}/\bar{u}_{04}$ at z/H = (0.5, 1.9), versus simulations using our implementation of the Wang–Takle model ('S-WT'), and using the '$k - \epsilon$' model. Domain depth 47H, resolution $(\Delta x/H, \Delta z/H) \geq$ (2.0, 0.25). The '$k - \epsilon$' simulation reproduces that reported by Wilson (1985; Figure 7).

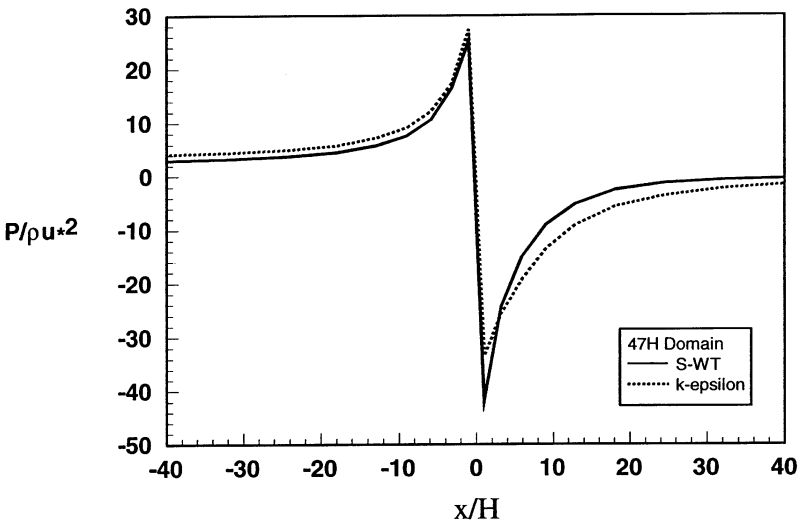

Figure 5. The mean pressure field, according to simulations using the 'S-WT' and '$k - \epsilon$' models. Domain depth 47H.

selective field data such as BM83 involve characteristic parameters (e.g., standard deviation of wind direction) not accounted in these two-dimensional models. We conclude therefore, that while the Wang-Takle model is not above criticism, their applications of it (e.g., to winds at oblique incidence to the windbreak) are likely correct, at least qualitatively, break new ground.

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