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LETTERS TO THE EDITORS

Discussion of "Comparison of turbulence models for the natural convection boundary layer along a heated vertical plate"

IN THE above paper, Henkes and Hoogendoorn [1] (henceforth referred to as HH) assumed the responsibility for conducting a comparison among various phenomenological models of turbulence. Specifically, they sought to establish the relative worthiness of the models examined for predicting two-dimensional, steady, constant property, buoyancy-driven, turbulent flow along a heated vertical flat plate. Among the models evaluated was one developed by us (To and Humphrey, TH [2]), for calculating variable property flows along strongly heated flat plates and in cavities. Apparently, the TH model did not fare well in the comparison performed by HH but only recently did we learn about the fact, in the course of consulting the literature on a problem which led us to the paper by HH.

Although late, it is critical to alert the readers of HH about a serious inconsistency in their work. This led HH to draw unfavorable, but incorrect, conclusions concerning the earlier study by TH which, if left unexplained, could detract from the significance of that pioneering contribution. We note with dismay and considerable regret that in the course of their comparative evaluation HH did not attempt to discuss with us the findings that led them to criticize our model. Had they done so we would have gladly collaborated with them towards an understanding of the differences they observed and the clarifications would have spared the scientific community some serious misinformation. Surely, such should be the spirit and practice among responsible researchers interested in the accelerated advancement of understanding and the accurate dissemination of knowledge.

In spite of the details in their paper, we are not aware of the efforts made by HH to ensure the correct implementation of the model by TH. However, we note the following points.

(1) There is a serious error in Table 2 of HH for the value of the local Nusselt number calculated for $Gr = 10^{11}$ and $Pr = 0.72$ according to their implementation of the TH model. This is given as $Nu_x = 679$ by HH when, in fact, interpolation of the values calculated by TH, given in their Fig. 2, yields $Nu_x = 550 \pm 10$. The latter value is within less than 12% of the experimental measurement quoted by HH and as good as any other calculated values listed in Table 2.

(2) In Fig. 3(b) of HH, the authors show that the quantity $\nu\varepsilon/u_0^4 \to \infty$ as $\zeta \to 0$ or, equivalently, $y \to 0$. This is very perplexing since TH imposed the boundary condition $\varepsilon = 2\nu \times (\partial k^{1/2}/\partial y)^2$ at the wall. TH showed that $\varepsilon \to 2\nu(\partial k^{1/2}/\partial y)^2$ as $y \to 0$ and that $2\nu(\partial k^{1/2}/\partial y)^2$ is finite at $y = 0$. Therefore, in the model of TH ε is finite in the vicinity of the wall. To further confuse matters, Fig. 3(a) of HH shows that $\partial k^{1/2}/\partial y$, computed according to the HH version of the TH model, remains a finite quantity as $y \to 0$. However, as already mentioned, the corresponding value of ε computed by HH becomes unbounded as $y \to 0$! This incongruous result was never observed by TH and it points to an improper implementation of the TH model by HH. In fact, inspection of Fig. 3(b) in the HH paper strongly suggests that the authors incorrectly used the wall function (from the standard k–ε model) for obtaining ε when implementing the TH model. If this is not the case, then we must conclude that HH evaluated $(\partial k^{1/2}/\partial y)_{\text{wall}}$ incorrectly, since this quantity is needed to specify the wall boundary condition for ε.

(3) An error by HH in the calculation of ε using the TH model would seriously affect the calculated value of Nu_x in two ways:

(a) Directly, through the calculation of $\nu_t = C_\mu f_\mu k^2/\varepsilon$. This is because if $\varepsilon \to \infty$ then $\nu_t \to 0$, thus eliminating turbulent diffusion. This will induce a sharper temperature gradient at the wall and hence an abnormally large value of Nu_x.

(b) Indirectly, through the sink term in the k equation, where a large value of ε induces a small value of k which, again, works to reduce ν_t.

(4) A question arises regarding the implementation by HH of the standard k–ε model. In their paper the authors state that ε was computed only at nodes with $y^+ > 11.5$. However, as a comparison of Figs. 3(a) and (b) in their paper will show, k was computed at nodes with y^+ considerably smaller than this. How was k computed for $y^+ < 11.5$ if ε was undefined (or unknown) in the range $0 < y^+ < 11.5$?

In summary, we believe that HH have implemented the TH model incorrectly, rendering invalid their conclusion that the TH model 'considerably deviates' from the other models they evaluated, and raising the need to interpret with extra caution the results and conclusions they obtained for all the models explored.

We remind the reader that the TH model does not contain extraneous (unphysical) terms in the transport equations for k and ε, and that it yields completely consistent asymptotic behavior, both for $y \to 0$ and for low levels of turbulence. The addition of an extraneous term by HH to the ε equation in the TH model, simply to obtain better predictions of Nu_x, is entirely artificial and inconsistent with the model formulation. We also reiterate that in the work of TH, "no attempt has been made to modify previously established values of the (model) constants in order to improve agreement between measurements and predictions of the flow investigated". While the use of standard k–ε model constants could account for some of the discrepancies observed by TH, the use of constants optimized for free-convection flows would only marginally affect their results.

We wish to end this letter on a positive note, with a specific recommendation to the Editors of *Int. J. Heat Mass Transfer*. In the process of reviewing future papers written for the express purpose of conducting comparative evaluations among numerical models of complex thermofluids phenomena, some representative fraction of the authors of the studies being compared should be involved in the matter. This is indispensable if accurate implementations of the various numerical models and the correct interpretation of results are to be guaranteed. One possible way to achieve this would be through an extended paper-review committee that would include at least one author of each major study being compared and which would be headed by one or more editors of *Int. J. Heat Mass Transfer*. It would be the charge of the editor(s) to coordinate a scholary and unbiased review of the comparison paper. Provision should also be made for any of the authors being compared to publish written commentaries on the comparison, especially if these serve to explain important discrepancies, clarify inaccuracies and advance understanding in general. If such an activity is pursued in good faith and with rigorous intellectual honesty the scientific community will be better served and public opinion concerning its research endeavors substantially enhanced.

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Authors' Reply

THE AIM of our paper [1] was to compare different existing low-Reynolds-number turbulence models, using the same numerical code, for the turbulent boundary layer along the heated vertical plate. One of the models compared was the low-Reynolds-number model of To and Humphrey. In their Letter to the Editor, Humphrey and To criticize some aspects of our paper.

We agree with their remark (4) that our Fig. 3 shows that we did not satisfy the restriction $y^+ > 11.5$ when applying the standard k–ε model. We actually applied the wall function for k and ε in the standard k–ε model at the first inner computational grid point, not necessarily with $y^+ > 11.5$. This is not very clear from the text; we only ended Section 2 with the remark that most natural convection calculations take the first inner grid point at $y^+ < 11.5$.

The main criticism of Humphrey and To on our paper is, however, their suggestion (remark (1)) that we incorrectly implemented their To and Humphrey low-Reynolds-number model, causing a serious error in our Table 2. Humphrey and To want to "alert the readers of Henkes and Hoogendoorn about a serious inconsistency in their work". We prefer to make clear that there seems to be an inconsistency *between* one of our calculations and a calculation of To and Humphrey [2]. We also referred to this inconsistency in our paper (Section 4): "Present results agree up to a graphical accuracy, except for the results with the To and Humphrey model, which considerably deviate".

In our study we extensively checked that the numerical results presented in our paper indeed seem to be accurate solutions of the equations listed:

(i) we thoroughly checked that the discretized equations were correctly coded;
(ii) we checked the accuracy of the numerical results by grid refinement;
(iii) we compared with solutions of other authors, using their models.

Of course, because of the criticism, we once again checked the correctness of the implementation of the To and Humphrey model in our code. We also recalculated the solution and refined the grid from 25×25 to 400×400 grid points (the distribution of grid points chosen is slightly different from ref. [1]). Table 1 gives Nu_x and the value of ε at the wall for $Gr_x = 10^{11}$ (and $Pr = 0.72$) using the low-Reynolds-number model of To and Humphrey. The grid-independent value of Nu_x is 674, which is only 0.7% below the value listed in Table 2 of ref. [1]. Further, as expected, ε_w remains finite at the wall. This clarifies remarks (2) and (3) of Humphrey and To: we did not incorrectly apply a wall function for ε_w, but evaluated the expression $\varepsilon_w = 2\nu(\partial k^{1/2}/\partial y)_w^2$, as dictated by the To and Humphrey model. The numerical value resulting for ε_w does not become unbounded, but is simply too large to be visible in our Fig. 3. It is strange that Humphrey and To are perplexed by our sharp increase of ε in the inner layer. Figure 10 of To and Humphrey [2] in which ε is plotted as part of the energy budget of the k-equation, shows the same behaviour: also here we see that ε for $y \to 0$ grows too fast to get ε_w in the figure.

As we described in Section 4 of ref. [1], our code was checked to recalculate the low-Reynolds-number results of Patel *et al.* for the forced-convection boundary layer. Also the results of Cebeci and Khattab and Lin and Churchill for the hot vertical plate were recalculated. Hence all these calculations checked with the results of other authors, whereas only the result as published by To and Humphrey [2] could not be reproduced by our code: at $Gr_x = 10^{11}$ they find $Nu_x = 550$, whereas we find a 23% larger value. Indeed the value calculated by To and Humphrey is very close to the experimental value. To and Humphrey checked the numerical accuracy of their result by only slightly refining the grid

Table 1. Code of Henkes and Hoogendoorn applying the To and Humphrey model ($Gr_x = 10^{11}$, $Pr = 0.72$)

Grid	Nu_x	$\dfrac{\varepsilon_w \nu}{(g\beta\Delta T\nu)^{4/3}}$
25×25	664.9	10.42
50×50	671.0	11.99
100×100	673.3	12.29
200×200	674.0	12.35
400×400	673.8	12.38