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Letters to the Editors

Comments on "The flow and heat transfer in the wedge-shaped liquid film formed during the growth of a vapour bubble"

In a recent paper J. Mitrovic [1] stated that some models for nucleate boiling heat transfer [M9,M10–M13][1] are inconsistent. We do not agree with his statements leading to this conclusion. He entirely misinterpreted the above mentioned papers and also part of the literature quoted in his article. It is noteworthy that he did not present a new model, but instead only disqualified existing models.

His reasoning is as follows:

He asserted that the authors of [M10–M13] assumed the axial velocity u_η in the liquid layer underneath the vapour bubble to be zero. He then showed that ignoring the axial velocity u_η leads to serious errors and inconsistencies in the model.

We are surprised to learn from his paper that our model is based on this assumption $u_\eta = 0$. Instead, already in [2] which was the basis for the micro region model [M10–M13] it was emphasised that the equations used are those for a creeping flow in a thin liquid film. As is well known from text books [e.g. 3,4] a creeping flow can be assumed when $\mathrm{Re}_\delta = \bar{u}\delta_0/\nu_l \rightarrow 0$ where $\bar{u}$ is the reference velocity in the film, δ_0 the reference film thickness and ν_l the liquid viscosity. In our case we have $Re_\delta < 10^{-4}$. With this and the additional assumption $(\delta_0/L_0)^2 << 1$ (L_0 is the characteristic length of the film) the full Navier–Stokes equations reduce to the equations used in our model:

$$\frac{\partial p_l}{\partial \xi} = \nu_l \rho_l \left(\frac{\partial^2 u_\xi}{\partial \eta^2} \right) \tag{1}$$

and

$$\frac{\partial p_l}{\partial \eta} = f_n \tag{2}$$

where f_η are the body forces in η-direction. These two equations deliver for given body forces the radial velocity $u_\xi(\xi, \eta)$ and the liquid pressure $p_l(\xi, \eta)$. The axial velocity $u_\eta(\xi, \eta)$ then results from the continuity equation. As in the Nusselt theory for film condensation, for heat transfer calculations only a mass balance over a cross section of the film is needed. It gives the evaporating mass flow. Therefore we did not explicitly calculate and publish values $u_\eta(\xi, \eta)$.

We asked ourselves what led J. Mitrovic to assert that we set $u_\eta = 0$. This might have been caused by two misleading remarks in the PhD-thesis of J. Hammer [M10] saying that in the thin liquid film $|u_\xi| >> |u_\eta|$ and therefore a flow parallel to the wall can be assumed[2]. Hammer furthermore said that under this assumption the term $\partial u_\eta/\partial \eta$ can be neglected in the continuity equation. Although these remarks were made a careful study of the following equations in [M10] would have easily revealed that the equations used are nothing else than those for a creeping thin film flow ($Re_\delta \rightarrow 0$, $(\delta_0/L_0)^2 << 1$) and do nowhere presume $u_\eta = 0$. As a consequence the many conclusions of J. Mitrovic based on the assertion $u_\eta = 0$ do not hold.

Apart from this principle error many other mistakes, serious misinterpretations of references and quantitatively unproved speculations are made in this paper. We do not discuss all of them, but confine ourselves to the most important in the following.

The paper starts with printing errors in the Navier–Stokes equations (M1)[3] and (M2). For the problem discussed the Navier–Stokes equation in η-direction reduces to Eq. (M7) corresponding to the above Eq. (2). The body force, however, includes gravity *and* adhesion forces:

$$f_n = -\rho_l g + \frac{\partial}{\partial \eta} \left(\frac{A_0}{6\pi\eta^3} \right) \tag{3}$$

J. Mitrovic omitted the adhesion term because it "leads to a singularity as η tends to zero". As is well known, attraction or repulsive forces tend indeed to infinity for $\eta \rightarrow 0$. They cannot simply be omitted in thin films close to a solid wall. This error leads to some

[1] Reference numbers starting with an "M", e.g. [M10], correspond to the numbers in the paper of J. Mitrovic [1].

[2] Note that $(u_\eta/u_\xi)^2 << 1$ corresponds to $(\delta_0/L_0)^2 << 1$ which was used to simplify the Navier–Stokes equations. Calculations with our model give velocity ratios $(u_\eta/u_\xi)^2 < 0.09$ in the thin film region.

[3] Equation numbers starting with an "M", e.g. (M1), correspond to the numbers in the paper of J. Mitrovic [1].

consecutive errors and finally ends up with Eq. (M17) which contradicts the well known augmented Young–Laplace equation for thin liquid films (e.g. [M22]). According to Eq. (M17) for sufficiently thick films we would obtain the erroneous result $p_l = \sigma K$ independent of the gas pressure.

The doubts of J. Mitrovic concerning the use of a disjoining pressure term $A_0/(6\pi\delta^n)$ with $n=3$ are purely speculative. We agree with him that for polar substances this term should be modified. However up to now reliable experimental data for these substances are not available. On the other hand Wayner [5,6] and Stephan [2] have shown that considerable uncertainties from the used disjoining pressure term do not seriously affect the heat transfer results.

J. Mitrovic's statement that "the whole theory breaks down" due to the neglected influence of surface roughness is not justified. The effect of surface roughness on thin film heat transfer models was discussed e.g. by Faghri [M5], Khrustalev and Faghri [7] and Stephan [2]. Their results indicate that not the absolute height of surface roughness as assumed by J. Mitrovic is the important parameter, but much more the curvature of the solid wall. Curvatures are statistically distributed and the overall effect on the heat transfer exists indeed, but is not very pronounced as is confirmed by experimental results (e.g. [8],[M26]).

The few quantitative estimates J. Mitrovic made in chapter 4 concerning the velocity ratio at the interface $u_{\eta\delta}/u_{\xi\delta}$ (Eq. (M24)) and the "dynamical pressure jump" (end of chapter 4.6c) are incorrect. To derive Eq. (M24) J. Mitrovic assumed $\partial p_l/\partial\xi = \text{const}$, which can not be concluded from the references [M10,M11] quoted in his paper. Instead, the liquid pressure p_l is a strong function of ξ as shown in [M10,M11]. Consequently the maximum value for the velocity ratio is different from his estimate $u_{\eta\delta}/u_{\xi\delta} \approx -0.52$. The numerical calculations with our model yield instead values $-0.3 < u_{\eta\delta}/u_{\xi\delta} < 0.1$. He estimated the "dynamical pressure jump" using $q_I = 10^7$ W/m^2 taken from [M10] for the refrigerant R114 at $p = 2.47$ bar. The values for the vapour density $\rho_v = 1$ kg/m^3 and the heat of evaporation $\Delta h = 10^5$ J/kg, however, are not those for R114 but fictitious values. Using the real values for R114 at the given pressure ($\rho_v = 18.16$ kg/m^3, $\Delta h = 1.26 \times 10^5$ J/kg) we obtain $(q_I/\Delta h)^2/\rho_v = 3.5 \times 10^{-3}$ bar instead of 0.1 bar. The conclusion of J. Mitrovic that this effect should be accounted for in our model therefore is not supported.

As a conclusion from Eqs. (M25) and (M26) J. Mitrovic states that "the decrease of the film thickness δ as ξ increases, forced by equation (26), is clearly in contradiction with the basic idea of the model". One should keep in mind that the assumptions leading to these equations, namely $u_\eta = 0$ and $\partial p_I/\partial\xi = \text{const}$, are unjustified and hence also these conclusions.

Equation (M28a) for the heat flux q_δ at the interface is based on a wrong mass balance with $u_\eta = 0$ as shown in Fig. 2(c) and (d). He pretends that we made such a mass balance, however we did not. From Eq. (M28a) J. Mitrovic derived Eq. (M33) in order to prove that the slope of the "TPL" for large heat fluxes $q_\delta \to \infty$ or low radial velocities $u_{\xi\delta} \to 0$ approaches $\pi/2$. This holds only if the velocity $u_{\xi\delta}$ can be considered to be independent from the heat flux q_δ which is not the case. In the limiting case $u_{\xi\delta} \to 0$ we obtain $q_\delta \to 0$ and therefore the slope $\partial\delta/\partial\xi$ in Eqs. (M33) and (M34) becomes indefinite. For supporting his reasoning J. Mitrovic refers to papers of Straub [M21] and Wayner et al. [M22,M23]. In these papers a possibility of the existence of a convex/concave interface is discussed for the case of extreme high heat fluxes during the transition from nucleate to film boiling. These heat fluxes are far above those discussed in our papers and beyond the aim of our model. It is noteworthy that also these authors make use of an adsorbed thin film underneath the vapour bubble during nucleate boiling.

With Eq. (M38) J. Mitrovic proposes to use a velocity profile that does not make use of the usually adopted boundary condition of negligible shear stress at the interface. He states "that Eq. (M38) does not make the model more complex in comparison to equation (M11)". Thereby he overlooks the fact that through the introduction of the new boundary condition $u_{\xi\delta}$ the number of unknowns exceeds the number of equations.

In a further remark J. Mitrovic states that in our model "the position of the film interface is considered to change jumpwise radially outwards, as the bubble grows. Between any two subsequent jumps the interface remains fixed in space. ... All transport processes occurring are assumed to be steady and time-independent". It is true that we neglected the internal energy stored in the thin liquid film during bubble growth compared with the enthalpy of evaporation. Nevertheless the problem is transient because of the moving interface. For numerical reasons we solved the equations for small finite time steps. This is a usual numerical treatment and does not at all mean that through the use of finite time steps the underlying physical problem becomes "jumpwise".

We do not comment on the choice of words used by J. Mitrovic to disqualify the models of other authors.

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In the paper by J. Mitrovic, "The flow and heat transfer in the wedge-shaped liquid film formed during the growth of a vapor bubble", published in the Int. J. Heat Mass Transfer Vol. 41, No. 12, pp. 1771–1785, 1998, several inaccurate statements have been made. It is somewhat disappointing to note that Dr. Mitrovic neither read the reference [9] of his paper carefully nor tried to study the reference [30] which formed the basis of the work reported in reference [9]. It has been clearly stated in both papers that the developed model was for vapor stems supporting mushroom type of bubbles formed in fully developed nucleate boiling. It was also discussed that the time taken for the formation of vapor stems was assumed to be much smaller than the time for which the vapor stems existed on the heater surface. As such quasi-static analysis was justified. No attempt was made in the paper to model the transient growth of a vapor bubble. In the analysis we had used lubrication type of theory and had neglected interia and convection terms in the momentum and energy equations respectively. We had also assumed that the velocity normal to the heater was much smaller than the radial velocity but, as the author has acknowledged, vertical velocity at the interface was included in the energy balance. Through order of magnitude analysis it can be shown that the assumptions we made are not unrealistic and lead to little error in the final solution.

When micro-layer becomes only a few molecules thick, the temperature of the outer layer of the molecules approaches the wall temperature and the heat flux across the micro-layer becomes zero. This in turn leads to no flow of liquid near the triple point. From the analysis we were able to determine stable shapes of the vapor steps as a function of the magnitude of Hamakar's constant and wall superheat. A comparison of the predictions with the available data was also made.

Dr. Mitrovic, on the other hand, has made only qualitative arguments based on his perception of the physics of the process without providing any concrete validation. He shows that at the triple point the heat flux should be infinite. How is this possible considering the fact that molecules sticking to the wall will not evaporate even at a temperature equal to the wall temperature? There are several other physically incorrect statements in the paper. For example, in reference to Fig. 2d, the statement, "Due to proximity of the surface, the mass flow rate $d\mathring{m}_L$ in this channel is very low but the heat flux at the 'outlet' of the channel is very large."

Finally, it would have been useful if Dr. Mitrovic had done some quantitative analysis of the stem size and microlayer thickness, based on his perceptions, and compared his results with the data and/or predictions from models of others.

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