Author's response to the letters by V. Dhir, J. Hammer and P. Stephan

A full discussion of the many issues raised by Dr. J. Hammer and Professors V. Dhir and P. Stephan is not possible in this short response. But I hope the following comments give some insights into the problem.

It is useful to start with the equation for the heat flux q derived in my paper,

$$q = \rho_L \Delta h \left(u_{\xi\delta} \frac{\partial \delta}{\partial \xi} - u_{\eta\delta} \right). \tag{31}$$

The equation is valid for models discussed in the paper which allow heat transfer only in one direction giving isotherms in the film parallel to the wall surface.

In his letter, Dr. Dhir states that the heat flux at the triple point (Three-Phase-Line, TPL) should be infinite. This, however, was not claimed in the paper nor is Dhir's conclusion derivable from Eq. (31) within the range of its applicability. In the models in question, the interface is inaccessible by a heat flux for $\partial\delta/\partial\xi \to \infty$ and in this case an evaporation is impossible. If the model restriction concerning the heat transfer in the film is removed, the heat flux at the film surface is readily deduced from the mass and energy balances when written e.g. in terms of $(\rho\vec{u}_\delta,\ \mathrm{d}\vec{A})$ and $(\vec{q},\ \mathrm{d}\vec{A})$ with the vectors $\vec{q}\{q_\xi + u_{\xi\delta}\rho h,\ q_\eta + u_{\eta\delta}\rho h\}$, $\vec{u}_\delta\{u_{\xi\delta},\ u_{\eta\delta}\}$ and $\mathrm{d}\vec{A}\{\mathrm{d}A_\xi,\ \mathrm{d}A_\eta\}$. Then, Eq. (31) follows at the requirement $q_\xi = 0$, while in the limiting case $\partial\delta/\partial\xi \to \infty$, the resulting interfacial heat flux is $q_\xi \to \rho_L\ \Delta h u_{\xi\delta}$, which is finite.

Dr. Dhir's remark, "*There are several other physically incorrect statements in the paper*", being a generalisation, is disregarded. Concerning his reference to Fig. 2d it must be emphasised that the figure illustrates the interrelation between the heat flux and the liquid flow rate according to models discussed in my paper. As may be taken from [2], a local heat flux of about 1.5×10^7 W/m^2 is obtained at a film thickness of nearly 0.003 μm, and a "flow channel", placed at this distance above the wall surface, must continually deliver the corresponding liquid flow. One of the questions pursued in my article was, "How should this be possible when a non-slip condition on the wall is assumed and the vapour–liquid interface is fixed in space."

Dr. Hammer and Prof. Stephan (HS) try to show that the axial velocity component u_η is not zero in their model. However, to treat this question unambiguously, let us repeat some of their derivation steps, starting with Eq. (3.35) in [1]

$$\frac{\partial p_L}{\partial \xi} = \nu_L \rho_L \left(\frac{\partial^2 u_\xi}{\partial \eta^2} + \frac{1}{\xi}\frac{\partial u_\xi}{\partial \xi} - \frac{u_\xi}{\xi^2} + \frac{\partial^2 u_\xi}{\partial \xi^2} \right).$$

To get from this expression the final model equation (Eq. (HS1), or Eq. (8) in my paper), must be

$$\frac{1}{\xi}\frac{\partial u_\xi}{\partial \xi} - \frac{u_\xi}{\xi^2} + \frac{\partial^2 u_\xi}{\partial \xi^2} = 0.$$

The latter is shown in [1] to be satisfied when

$$\frac{u_\xi}{\xi} = -\frac{\partial u_\xi}{\partial \xi}$$

which is the equation of continuity at $u_\eta = 0$. Therefore, their statement,

> "*The axial velocity $u_\eta(\xi,\ \eta)$ then results from the continuity equation*",

does not hold, the equation of continuity is used up with the requirement $u_\eta(\xi,\eta) = 0$ giving the model equations[1] (HS1) and (HS2). The HS arguments with creeping flow do not alter the nature of the issue.

In view of this fact, it is curious to see that HS are learning from my article that their model affirms $u_\eta = 0$.

The next question by HS is concerned with the body force in a liquid film arising from wall-film interactions; they recommend Eq. (HS3). Now, combining this equation with Eq. (HS2) and performing integration, one obtains

$$p_\ell(\xi, \eta) = -\rho_\ell g \eta + \frac{1}{6\pi}\frac{A_0}{\eta^3} + \varphi(\xi)$$

giving $p_\ell \to \infty$ at $\eta = 0$.

To avoid such a singularity, the action of a wall upon the liquid in a film adhering to the wall surface—if discussed in terms of pressure—is usually taken to be constant over the whole film thickness and mostly written as A/δ^{n}. In conclusion, if—in agreement with literature, but not necessarily satisfying—the wall-film interaction is expressible as a function of the film thickness alone, the body force, adopted in my paper, is correct.

The remarks in the paragraph following Eq. (HS3) upon my Eq. (17) is insofar unfounded as Eq. (17) is taken to illustrate the change of the pressure in the

[1] In [2], the line following the heading "3.2 Liquid transport in the micro region", one finds: "For the modelling of the transverse liquid flow in the micro region a one-dimensional laminar boundary layer is assumed." Now, if a boundary layer is one-dimensional, all properties of such a layer, e.g. velocity, can change along one co-ordinate only. Actually, in a liquid wedge, the transverse liquid velocity is a function of both co-ordinates (ξ and η), and the flow field is two-dimensional.

liquid with ξ, as is clearly stated in the lines just preceding the expression. In this respect, Eq. (17) coincides with the corresponding one of HS, and their reference to the Young–Laplace equation is placed incorrectly. Since the interface is fixed in space, as assumed in the model, the pressure in the vapour is constant (during evaporation time step) and does, therefore, not affect the film flow.

The contents of several paragraphs in the HS-letter need not be examined in detail. Concerning their statement, "*...considerable uncertainties from the used disjoining pressure term do not seriously affect the heat transfer results*", one may observe that, in their model, just the change of this pressure should govern the liquid transport in the wedge towards the fixed TPL. On the other hand, if the flow of the liquid does indeed have less effect on the heat transfer—which is plausible because of the sliding of the TPL towards the liquid bulk—then, the model as a whole loses its central idea. Concerning the surface roughness it should be emphasised that my remark on this point is not addressed to the overall boiling process, in which case the interaction between surface roughness and boiling heat transfer must be considered along with the superheat of the liquid layer adjacent to the heating surface. Instead, the remark is concerned with a three-dimensionality of the flow arising from surface roughness which is not described by the models reported so far.

It should be stressed that physical properties of R114 have not been used in my paper for numerical illustrations. Further, instead of a comment in connection with $\partial p_L/\partial\xi \approx$ const, as described in my article, we may note that, for example in Fig. 7.2 in [1], the pressure distribution shows an inflection point, the neighbourhood of which is well approximated by a straight line, resulting in $\partial^2 p_L/\partial\xi^2 \approx 0$. In addition, in the region with a negligible wall effect, it is $p_c = \text{const.} = 2\sigma/r(t)$ over a considerable film portion. With this in mind, the inquire, "How, then, in this region should liquid flow be possible", can scarcely be overlooked. The hint just given also holds for the fifth paragraph following Eq. (HS3).

The remainder of the HS-letter does hardly contribute to clarify the roots of the discrepancies mentioned there. However, we should note that the heat flux, even in the absorbed film region, *is by no means zero*. The analysis given in my article is justifiable, and the derivative $\partial\delta/\partial\xi$ is unequivocally determinable. The HS statement on the indeterminacy of the system with $u_{\xi\delta}$ according to Eq. (38) is incorrect; the quantities to be obtained are $u_{\xi\delta}$, $u_{\eta\delta}$, δ and q; they follow from Eqs. (21), (23) and (31) along with the heat flux dictated by the temperature fields in the phases.

One last remark should be made concerning the non-stationary effects. The equations HS used for heat transfer in the heater wall and the liquid film as well follow from the energy equation $\rho c\partial T/\partial t + div\vec{q} = 0$ when both the derivative $\partial T/\partial t$ and the convective transport are zero. As the equations do not know the variable time, consequently $div\vec{q} = 0$, and the temperature does not change in time, whatever processes—also including a sliding of the TPL—are taking place. As HS point out, "*Nevertheless the problem is transient because of the moving surface*". This is true, but it is an entirely different issue. Because the governing equations have to satisfy the boundary, but not the initial conditions, the temperature fields know nothing about their history.

If e.g. one tries to link two subsequent steady-state temperature fields in the heating wall, one will unavoidably have to accept all the uncertainties arising from the construction of a "right" bubble shape. The bubble shape, on the other hand, reflects the whole bubble kinetics and is unknown in advance. When boiling is taking place on a plain, ideally smooth horizontal plate, the rotational symmetry of a bubble seems allowable. For a horizontal tube, however, the case where HS demonstrated an excellent agreement between calculations and experiments, both the instantaneous bubble shape and bubble departure diameter change with the position of the bubble generation site on the tube circumference in a largely unknown manner.

Steady-state conditions are scarcely expected to satisfactorily approach reality. As is well known, the temperature in the heater near the bubble formation site changes with time more when the heat flux is lower [3]. In this context, the results reported by Ilyin et al. [4] and Welch [5] are highly instructive. Welch's work is a direct numerical simulation of bubble growth. Starting from equilibrium of a system consisting of a liquid pool, a solid wall, and a vapour bubble, adhering to the wall surface, the temperature of the system (all three phases)[2] is then raised by a few degrees. This is the state which corresponds to the birth of a vapour bubble on a wall-liquid contact surface, despite the fact that in reality the system is not isothermal. Welch's isotherms show a heat flow from the liquid to the wall. The boundary conditions chosen by Welch (pinned TPL, adiabatic outer wall surface) do not weaken the importance of his results for obtaining a better understanding of processes of bubble growth, particularly not at lower heat fluxes. Ilyin et al. studied experimentally the bubble kinetics and, regarding our present purpose, their results are best presented by the authors themselves:

[2] I would like to express my thanks to Dr. Welch for a private communication stating more precisely the initial conditions in the simulations.

"*Secondly, interference fringe pattern in the thin liquid layer beneath the bubbles shows the direction of local heat flux not only from the liquid wedge to the bubble, but also from the liquid to the subcooled metal in certain stages of the vapour bubble growth*."

For reason of clarity, we may add that the term "subcooled metal" means the heating wall. The experiments were performed with water at atmospheric pressure, at a heat flux of 35.8kW/m^2, a wall temperature of 110.5°C, and a water bulk temperature of 99.5°C.

Needless to say that a reversal of the heat flux during the bubble growth is undetectable on the basis of steady-state models.

Corrigenda

The sign "$-$" preceding v_L in Eq. (1) of my article must be replaced by "$+$", the co-ordinate η in the last term in the parenthesis in Eq. (2) by the co-ordinate ξ. On page 1782, left column, eighth line from bottom, the word "fluxes" should be replaced by the term "fixes". On page 1783, the second paragraph from bottom, first line, it should read "Another way...", instead of "Another was...". However, these typewriting errors do not affect the analysis given in the paper.

References

[1] J. Hammer, Einfluß der Mikrozone auf den Wärmeübergang beim Blasensieden (Dissertation, Universität Stuttgart), VDI-Fortschrittsberichte, Reihe 19, Nr. 96, VDI-Verlag, 1996.
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[3] V.I. Tolubinskiy, A.A. Krivesko, Yu.N. Ostrovsky, On fluctuations of the temperature of the heated surface at an active nucleation site, Heat Transfer—Soviet Research 4 (6) (1972) 30–35.
[4] I.N. Ilyin, V.P. Grivtsov, S.R. Yaundalders, Holographic interferometry studies of temperature profiles in thermal boundary layer in free convection and bubble boiling, in: Proceedings 7th International Heat Transfer Conference, 4, 1982, pp. 55–59.
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