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LETTER TO THE EDITORS

"THE NEW HEAT TRANSFER": COMMENTS ON O. A. SAUNDERS' REVIEW

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NOMENCLATURE

A,	heat exchange area (wall-boiling fluid interface);
e,	wall thickness;
	heat-transfer coefficient, $\dfrac{q}{T_0 - T'_w}$;
R,	electrical resistance of the wall;
T_0,	temperature of heating fluid;
T_w,	temperature of the wall at the boiling fluid interface;
T'_w,	temperature of the wall at the heating fluid interface;
U_0,	voltage;
V,	mean velocity of heating fluid.

Greek symbols

ΔT,	temperature difference between wall and boiling fluid;
ΔT_0,	temperature difference ΔT at $q = 0$;
ε,	temperature coefficient of electrical resistivity;
λ,	thermal conductivity.

Subscripts

in,	inlet;
out,	outlet;
int,	internal system (boiling fluid);
ext,	external system (heat source).

I SHOULD like to reply to Saunders' critical review [1], concerning Adiutori's work: "The New Heat Transfer" [2]. It is not my intention to add fuel to the controversy, but to attempt an approach, objectively and constructively, to the author's claims, as did Saunders—the first critical review, which appeared in [3], having to me seemed a systematically negative one. I shall go no further than to underline certain points, some of which are details, others more fundamental.

(1) On heat-transfer data correlation methods: Adiutori proposes a direct linking of q and ΔT, without recourse to the heat-transmission coefficient notion $q/\Delta T$. The correlation in the form $q(\Delta T)$ has the advantage of being clearer when heat-transfer phenomena are "non-linear", as in the case of radiation mentioned by Adiutori [2], Vol. 1, pp. 5–20 and 5–21, contrary to Saunders' affirmation; these facilitate interpretation of heat stability problems (see below, points 2 and 3).

(2) It is a pity that Saunders does not mention—among the original points that Adiutori proposes—the thermal stability concept and resultant stability criterion:

$$\left(\frac{\partial q}{\partial \Delta T}\right)_{\text{int}} > \left(\frac{\partial q}{\partial \Delta T}\right)_{\text{ext}} \quad \text{or:} \quad \left(\frac{\partial q}{\partial \Delta T}\right)_{\text{out}} > \left(\frac{\partial q}{\partial \Delta T}\right)_{\text{in}}.$$

This criterion, which is analogue to extended Ledinegg's stability criterion [4] as regards hydrodynamic stability problems, was put forward for the first time in 1964 by Adiutori [5]. Since then, several authors have likewise brought it forward, including Stephan, in 1965 [6]. Some experimental illustrations of this criterion were recently put forth by Hesse [7], and by Canon and Park [8]. Its application makes possible the design of a *stable* experimental

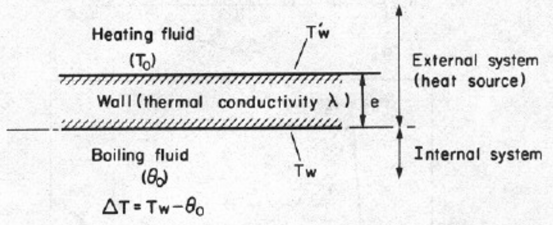

FIG. 1.

arrangement (boiling fluid + source of heat), thus permitting measurement of the pool boiling curve at all points, and particularly in the transition zone, where $\partial q_{\text{int}}/\partial \Delta T < 0$.

This criterion demonstrates the insufficiency of maintaining the heating system temperature at a constant, "by means of a fluid in condensation", in order to guarantee stability.

If one supposes that the heat-transfer properties between the heating fluid and the wall (Fig. 1) are linear and take the form: $q = h(T_0 - T'_w)$, when: $h = C^{te}$ (independent of T_0) and that the thermal conductivity λ of the wall plane is constant, the stability criterion implies that we have in the transition zone:

$$\left|\frac{\partial q_{\text{int}}}{\partial \Delta T}\right| < \left(\frac{1}{h} + \frac{e}{\lambda}\right)^{-1}.$$

The increase of velocity V of the heating fluid (if $h \sim V^n$, $n > 0$) and the decrease in wall thickness are thus favourable to stability. In the same way, there exists a maximum thickness, beyond which heat instability should become manifest, even if thermal resistance of the heating fluid is extremely slight:

$$e < e_{\text{maxi}} = \frac{\lambda}{\left|\dfrac{\partial q_{\text{int}}}{\partial \Delta T}\right|}.$$

What is more, it should be possible to obtain a stable arrangement when the wall is heated by Joule effect. In this case, it is necessary for the wall to present the highest possible resistivity increase coefficient with temperature ε. (The general approach is to obviate the temperature influence of the wall on its electric resistance by means of such materials as Nimonic.) Supposing, on first approximation, that wall heat resistance is negligible, we may say:

$$\frac{\partial q_{\text{ext}}}{\partial \Delta T} = \frac{\partial}{\partial T_w}\left(\frac{U_0^2}{AR}\right) = -\frac{U_0^2}{AR^2}\frac{\partial R}{\partial T_w}.$$

(U_0 is the applied electric tension, A the heating surface area, R the electric resistance of the wall.)

It may thus be seen that adoption of a high coefficient $\varepsilon(\partial R/\partial T_w \approx \varepsilon R)$ is an aid to stabilisation of the system: source of heat and boiling fluid. All these points are made in detail in Adiutori's work, and had already been made clearly explicit in [5].

(3) Berenson's remarkable experiments [9] are a particularly demonstrative illustration of the thermal stability concept. I have taken as example (Fig. 2), the variations $q(\Delta T)$ given by the author for test No. 10 (N–pentane). Each experimental point is demarcated by a number which I presume to correspond with the experimental results as

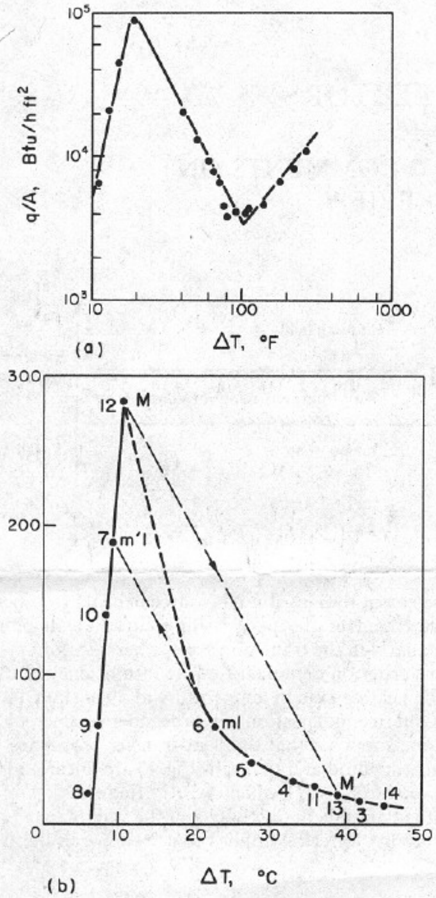

FIG. 2.

obtained in chronological order. If points 12 and 13 are joined, and likewise points 6 and 7, it may be seen that the two straight lines are parallel:

$$\frac{q_{12}-q_{13}}{\Delta T_{12}-\Delta T_{13}} \approx \frac{q_6-q_7}{\Delta T_6-\Delta T_7} = -9.2\,\mathrm{kW/m^2\,{}^\circ C}.$$

This property confirms the presence of instability in Berenson's experimental set-up, with hysteresis effect. In this test, no experimental point is obtained in the transition zone, between points 12 and 6.

Generally speaking, this indicates the interest of providing experimental results as they come, unselectively, as done, for example, by Hesse, and as requested by Adiutori.

(4) The *linear* plot of the relation $q(\Delta T)$ strongly suggests that the relations $q(\Delta T)$ are *linear* in the nucleate boiling zone *and* in the transition zone. These conclusions are confirmed by examining Hesse's results (transition zone and nucleate boiling zone), Grigull and Abadzik's results [10] (nucleate boiling) and those of Berenson (nucleate boiling and transition zones for tests No. 7–9). I realise that there is an apparent contradiction between the form of correlation given by Roshenow [11] in nucleate pool boiling: $q \sim \Delta T^n$ (when $n \approx 3$) and the correlation suggested by the above-mentioned authors' results, and set out as follows by Adiutori [2]:

$$q \sim (\Delta T - \Delta T_0).$$

The latter form essentially underlines the fact that only a minimal superheat of the wall is required in order for the first steam bubbles to appear. It would be desirable for this contradiction to lead to fruitful discussion, rather than polemics of a severe and sometimes over-impassioned nature.

(5) The application of the thermal stability concept to burn-out experiments in forced convection boiling is enlightening on some rather obscure points, in particular with regard to the possible presence of hysteresis (noted by certain experimenters and not by others). It makes possible the design of a thermally stable testing arrangement, by obviating the burn-out phenomenon insofar as this takes the form of a wall temperature excursion. But these developments go beyond the bounds of this particular analysis.

(6) As far as dimensional analysis is concerned, the discussion is more delicate, and I shall limit myself to the following remark: dimensional analysis can only strictly be carried out if all the general fundamental equations ruling the phenomenon in question are taken into consideration. If these are only broached in passing by means of simplificatory hypothesis, the dimensional analysis will become very difficult and may even lead to erroneous results. To neglect "secondary" variables may for example, incur distortion of obtained laws, a distortion often attributed—as Saunders rightly points out—to experimental errors. In this case, would another method not consist in working from these experimental results, in order to seek the better correlations that they suggest? This method was used with success to determine burn-out correlations. In 1968, Macbeth affirmed [12].

> "Burn-out correlations based on dimensional analysis have appeared (...), but the fluid properties used in these cases have been chosen on the basis of various assumptions without any demonstration that the properties used were the correct ones. They have, in fact, been shown (...) to be either incorrect or incomplete" (p. 210).

I believe that it is in this sense that Adiutori defends the primacy of the experiment.

I hope that this reply can be inserted in one of the future editions of your review; any debate necessarily contributes to technical progress. And it is precisely one of the merits of Adiutori's work to provoke such discussion. Unfortunately, however, this tends to become rather too "lively" {I refer to the discussion published in *Nucleonics* (December, 1974)—further to article [5]—and [3]}.

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