extent. There also remains the question of the difference in the definition of the Rayleigh number between that based on the result of linear stability analysis and the empirical one of Boger and Westwater, and it was not clear which definition was used in comparison.

Another statement also deserves comment: "Tien and Yen state that the critical Rayleigh number of the onset of convection varies with the temperature of heated boundary. This possibility was examined also in the present study. The results, Fig. 12, show no effect, nor does the temperature of the cold boundary affect the critical Rayleigh number." The critical Rayleigh number based on linear stability analysis is found to be a function of parameter $A$ defined by equation (3), and is not just a function of heated boundary temperature. The most one can conclude from Fig. 12 appears to be that the empirically defined Rayleigh number used by the authors is independent of the heated boundary temperature. Even this conclusion is perhaps somewhat questionable, since only very few data points among those indicated in the lower curve of this figure were obtained when maximum-density effect was involved. In any event, it is difficult to conceive how this conclusion can be used to either invalidate or, for that matter, substantiate the results based on linear stability analysis.

A more interesting point of Heitz and Westwater's work is the revelation of its disagreement with the experimental data reported by Yen [16]. This is difficult to explain since both investigations were carried out with considerable care, and in view of the agreement of both sets of measurements to other investigators in the limiting case when the maximum-density effect is absent. Heitz and Westwater reported the agreement of their data with that of Schmidt and Silveston [9]. Yen's result, however, was also found to agree with earlier work in the limiting case of no density inversion. The fact that Heitz and Westwater's results agree with those of Catton and Edwards [10] does not add credence to the accuracy of their data insofar as the effect of maximum density is concerned, since the substance employed by Catton and Edwards in their experiment exhibits no density-inversion effect. It therefore appears that only through further experimentation can this controversy be clearly resolved. In this connection, it may be worthwhile to point out that in both Yen's work and that of Heitz and Westwater, a phase change occurred continuously during the course of measurements. The experimental conditions, therefore, can be described, at best, as pseudo-steady-state. In the future, it may be advisable to conduct measurements without this complication so that a true steady state can be realized.

Additional References


Authors' Closure

We appreciate the interest shown by Tien and Yen in our work and their discussion of our recent article.

In our introduction we presented a brief and representative summary of previous contributions to the study of natural convection in confined liquids. We acknowledge and are aware of many valuable contributions in addition to those mentioned.

The models of Davis [8] and Charshon and Sani [15] still appear to be among the most rigorous thus far presented. The "theory" of Tien, Yen, and co-workers is not, in fact, theory, but rather mathematical modeling. The results of such modeling should not be referred to as "theoretical." For example, the assumption that the density of water depends on temperature as a parabolic function, symmetric about 4 deg C, is not a physical fact, although it is very convenient mathematically.

The article by Heitz and Westwater is the presentation of an experimental study in which multiple, independent means were used to detect critical Rayleigh numbers. The results, as indicated in the title of the article, were determined for $L/D$ from 0.5 to 8 and at no point were they extrapolated to an $L/D$ of 0. We are confident of our results and have compared them with the results of other investigators (Fig. 13) to illustrate areas of agreement and areas of disagreement.

It is quite true that Tien did not include the parameter $L/D$ in his work [17]; a basic assumption is that $L/D \cong 0$. It was Tien, however, who chose to substantiate his work by comparing his results with experimental data, obtained by Boger and Westwater [14], for which $L/D$ ranged up to 3. Our Fig. 15, "Comparison of new data with results of Tien," was included to illustrate that Tien's model does not appear to be valid for $L/D > 0$. The critical Rayleigh numbers, labeled "Tien model," were calculated using Tien's model and the data presented by Tien in [17, Table 1]. As mentioned in our paper, incorrect values of liquid depth $d$ were used by Tien. Correct values were used in our Fig. 15.

We are aware that Tien, Yen, and co-workers expressed the critical Rayleigh number in terms of the parameter $A$ which is a function of the upper and lower boundary temperatures and the maximum-density temperature. Yen [16] also presented the empirical relationship

$$Ra_{cr} = 1.42 \times 10^9 \exp(-6.64 \times 10^{-3} T_0)$$

where $T_0$ is the temperature of the heated boundary. We do not feel that the critical Rayleigh number is correctly represented by the lower boundary temperature alone, or even by the parameter $A$ alone. Our Fig. 14, "Effect of $L/D$ on $Ra_{cr}$; data of Yen," was plotted using the data of Yen [16, Table 1], and clearly shows that the critical Rayleigh number is a function of $L/D$ as well as boundary temperatures, even as $L/D \to 0$. Our Fig. 12 further shows that the critical Rayleigh numbers are not a sole function of the heated boundary, but rather a function of temperature differences and $L/D$.

Reasons for the disagreement between Yen's experimental data and the data of other investigators, Fig. 13, are not known at this time. We believe that all investigations were carried out with considerable care and agree with Tien and Yen that a possible explanation lies in the fact that data were obtained at pseudo steady state. In the region of interest, our critical Rayleigh numbers were obtained under conditions such that, in terms of interfacial motion, phase change could not be detected. During the final 30 min of approach to transition, $Ra$ was also constant to within measuring accuracy.

On the Rohsenow Pool-Boiling Correlation

W. M. Rohsenow

Frost and Li present new data for pool-boiling of water at sub-atmospheric pressures (0.92-14.45 psia) and plot it on the correlating coordinates suggested in their reference [1] in 1951. At that time available Prandtl number data was wrong and led to an exponent on the Prandtl number.

2 Professor, Department of Mechanical Engineering, Massachusets Institute of Technology, Cambridge, Mass.
of 1.7. This remains correct for all other fluids but for water it should be 1.0.

The attached Fig. 1 is the authors' Fig. 6 with their data curves plotted on the same coordinates except the $Pr^{1.0}$ is changed to $Pr^{1.8}$ in the abscissa. Agreement with the correlation is then excellent. The data is correlated by

$$\frac{C_{sf} \Delta T_a}{h_f \Delta T_a} = 0.019 \left( \frac{q/A}{\mu_k h_f} \right)^{0.33} (Pr)^{1.8}$$

The coefficient 0.019 is valid for the surface used in these tests and has been found to depend on the cavity-size distribution in the solid surface.

**Authors' Closure**

The authors wish to thank Professor Rohsenow for his interest and suggestions for this paper. The authors also agree that with the exponent of Prandtl number equal to 1.0 the Rohsenow pool-boiling equation would better correlate the data presented in this work.

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**Heat Transfer Due to Combined Free and Forced Convection in a Horizontal and Isothermal Tube**

**D. R. Oliver.**

It is good to see the results of natural convection heat transfer work carried out in a tube of low $L/D$ (28.4). I feel that the authors are correct in asserting that the $L/D$ term is of little importance. In one of our papers [15] subsequent to that quoted by the authors [2], the $L/D$ term does not appear and the result is an equation rather similar to that quoted by Jackson, Spurlock, and Purdy [18]. I should like to have seen this equation tested by the type of plot shown in Fig. 3 (which greatly enlarges the "scatter" due to the use of Nu$^3$ rather than Nu as in Figs. 4, 5, and 6).

The authors have utilized a wide range of experimental data in arriving at their final equation, which may be written as

$$Nu = \left( \frac{\mu_k}{\mu_v} \right)^{0.14} = 1.75(Gz + 0.12Gz^0.88Gr^{0.29}Pr^{0.32})^{0.33}$$

However, I am not too happy with the apparent importance of the Graetz number (power 0.88) in the natural convection term. All our work in tubes of high $L/D$ showed natural convection effects dying away at high flow rates. I can understand the argument of Brown and Thomas that high mass flow rates should permit the natural convection driving forces to remain high, but would expect the power on $Gz$ to be much lower than 1.33 or 0.88. The inclusion of a function of $Gz$ in the natural convection term also implies that no convective heat transfer will occur in the absence of forced flow. In fact this heat transfer will be quite large whilst significant temperature gradients remain within the liquid.

There is general agreement over the omission of the $L/D$ term, but a new problem has arisen regarding the relative importance of $Gz$ on the one hand, and $(Gr Pr)$ on the other, in the natural convection term. Recent reviews [16] have not focussed attention on this problem, but workers active in the field should regard it as worthy of further attention. The authors are to be complimented on an interesting and stimulating paper.

**Additional References**
