LETTERS

Thermal Stability View Disputed

. . . Argonne Comments

DEAR SIR:

The undersigned, having read "New Theory of Thermal Stability in Boiling Systems" by E. F. Adiutori (NUCLEONICS, May 1964, p. 92), find it totally unsatisfactory.

A complete and critical review of Mr. Adiutori's article would require an effort much greater than and entirely inconsistent with the effort required to determine the article's lack of merit. However, some of us feel compelled to point out a few of the more obvious errors, not to imply that there are not more, equally important misleading statements:

1. The most consistent fundamental error throughout the paper treats *temperature* as an *independent variable*. In treating cases involving internal heat generation (the author's "fixed heat input" systems), temperature is always the *dependent* variable. In other words, one cannot postulate a temperature change as the *cause* of thermal instability in such a system involving heat transfer from a solid surface to a fluid, whether boiling or not. In a "boiler," more properly an "evaporator," as used by the author, the wall temperature may be thought of as an independent variable, although, in practice, the change in temperature is actually dependent upon a change in fluid temperatures on the primary or high-temperature side of an evaporative heat exchanger.

2. There is no recognition here of the *fact* that the flow and boiling-heat-transfer behavior of the fluid is the independent variable and is therefore the forcing function in a perturbation analysis of the systems considered in the article.

The preceding comments demonstrate the inherent difficulty in critically reviewing the article. Since the author's basic premises are wrong, any further corrections cannot necessarily validate any of the author's conclusions. However, once again, it may be worthwhile to present a partial list of erroneous statements.

3. The opening paragraph suggests a quantitative analysis; nearly the entire article is qualitative in nature. Also, it has been recognized for years that "boiling-heat-transfer coefficients are not very helpful . . . ," etc.

4. The general definition of thermal stability on p. 94 is not clear descriptively and, mathematically, almost preposterous; for example, under the stated perturbation, a condition such as

$$T(t = \infty) = T(0) + \Delta T$$

could be just as "thermally stable" as any of the author's cases. Among other things on this page, along curves d and c in Fig. 3, either dT/dt changes sign in the interval $0 < t < \infty$, or the coordinate system is something that mathematicians ought to know about. Again, it should be emphasized that these traces are the *results* of a power or boiling phenomenon perturbation and cannot be justified as the *cause* of any instability considered here.

5. To be more specific, the statement at the top of the right-hand column on p. 96, ". . . at burnout the temperature cannot increase without limit in a constant temperature system," defies analysis in English.

6. The section on Reactor Instability (p. 98) is *approximately* the reverse of the actual behavior of a boiling reactor. The behavior of the *spatial and time variation of void fraction* constitutes the forcing function in a boiling reactor.

7. It is very difficult to understand how the liquid superheat threshold (p. 99) has any influence on reactor stability in the cases considered here. The quote from ref. *8* by Anderson and Lottes refers to the fact that oxide fuel elements possess a much lower thermal diffusivity than metal fuels and therefore will dampen power feedbacks from temperature fluctuations to a greater degree. The stability referred to here is the per cent temperature difference oscillation when compared with the over-all fuel temperature difference.

8. The discussion of liquid-metal boilers (pp. 90–101) is full of confusion. A "once-through boiler" is by definition one which produces saturated vapor or, more probably, superheated vapor, so that a mention of "high quality" is meaningless.

The remaining paragraphs do not touch upon the basic problems at all, because the discussion is based upon the erroneous conclusions of the article's first few pages.

—J. B. HEINEMAN, H. FAUSKE,
P. A. LOTTES and B. M. HOGLUND
Argonne National Laboratory
Argonne, Illinois

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. . . Westinghouse Comments

DEAR SIR:

In his article Adiutori (*1*) discusses the problem of stability of boiling systems, including the phenomenon of burnout or DNB (departure from nucleate boiling), in terms of the so-called thermal stability of the heating surface. This is to our minds a highly oversimplified view. We would also contend that the "widely accepted conclusions" enumerated in the beginning of his paper are not erroneous, as he claims. They are conditional and not as simple as the author states.

Next, in the discussion of applications, the author promises vast improvements in the thermal stability of heat-transfer equipment; yet all but one of these improvements are left to the reader's imagination. Without making an attempt to list all the statements with which we cannot agree, we might further point out that the suggestion regarding electrical heaters appears to be in error because the temperature excursion at DNB is a *local* phenomenon. Hence, the current to the electrically heated surface is virtually unchanged and the heat generation at the hot spot is augmented rather than decreased if the material has a large positive temperature coefficient of resistivity.

In our minds the complexity of the boiling-stability problem also rules out the view held by many [*see accompanying letter from Argonne*] that DNB is a problem of the fluid side only. Our observations indicate that the DNB mechanism involves both the fluid side and the solid side. One must take into

Nucleonics

December 1964

. . . Other Comments

Following are excerpts from letters received from others in the field specifically requested by NUCLEONICS *to comment on both Adiutori's article and the Argonne letter:*

The limited mathematical notation which is used indicates a naive conception of engineering analysis. Most of the technical ideas which the author is apparently trying to convey are either erroneous or they have been known for years. . . . The Argonne group was, if anything, charitable in its comments.

—GEORGE LEPPERT
Stanford Univ., Stanford, Calif.

The author attempts to explain too many of the stability problems found in two-phase systems by means of a multivalued temperature versus heat-flux curve. . . . I must agree with the Argonne people that temperature is not an independent variable and that the paper does not give a quantitative analysis of stability. The author, however, makes a good point when he states that the consequences of burnout need not be the same in a constant-heat-flux system and a fluid-to-fluid heat-exchange system. . . . I must admit that the quality of the paper is considerably below what I would consider acceptable for NUCLEONICS.

—S. LEVY
General Electric Co. (APED)
San Jose, Calif.

I have had considerable difficulty in trying to decide whether or not the paper made a real contribution. One basic difficulty is that the author appears to be control oriented and does not use the "jargon" which has been developed in the boiling field, a field which happens to be extremely specialized.

—KURT GOLDMAN
United Nuclear Corp.
White Plains, N. Y.

I fully agree with the group from Argonne. . . . Mr. Adiutori's piece is amazing in that it seems to ignore anything done before on the stability of boiling systems.

—MARIO SILVESTRI
CESNEF, Milan, Italy

. . . Author's Reply

DEAR SIR:

Although I believe that critics should generally be ignored, the nature of these criticisms does not allow me this luxury. I am left no choice but to demonstrate to the reader that the critics simply do not know what they are talking about and that their opinions are therefore of no value.

Argonne critics:

These critics are obviously unaware of the fact that, in any *real* system, the independent variable is *never* temperature—or heat flux—or flow rate. In real systems, the independent variables are the settings on the "cranks"—i.e., the settings on the valves and knobs. Thus their insistence that derivatives be taken only with respect to "independent" variables requires that derivatives be taken only with respect to crank settings. (I hasten to assure the reader that their criticism has no hidden and profound meaning—it is just as ridiculous as it sounds.)

These critics have failed to understand that my method of handling derivatives is widely used in other applications and that they use it themselves without understanding it! The method is simply to uncouple the system mathematically and then to analyze the separate behavior of the uncoupled pieces.

In this analysis, one *pretends* that some parameter can be independently varied and then analyzes the separate pieces to determine how their performance is related to the "independent" parameter. The fact that this is a mathematical artifice that we may not be able to duplicate physically is of no consequence.

This is precisely the method whereby one obtains Fig. 1, which is a well known technique for appraising the hydraulic analog of thermal stability. The thermal counterpart of Fig. 1 is presented in Fig. 2, which of course results from my concept of thermal stability. It should be noted that the "heat-in" curve in Fig. 2 is inclined, whereas the "old" graphical method was based on the erroneous conclusion that the heat-in curve is always either horizontal or vertical. This error of course resulted from trying to classify systems on the basis of the "dependence" or "independence" of the temperature. The reader should note that this is *precisely* the error these critics are making and in which they would like me to join them.

The reader should also note the following with regard to Figs. 1 and 2:

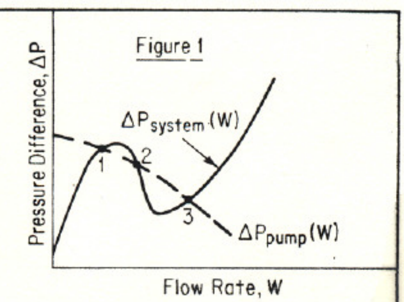
1. *Both* figures apply only to highly idealized systems (although this fact has apparently not been realized in the case of Fig. 1).

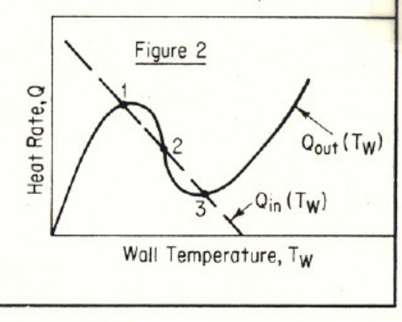
2. *Both* figures require the determination of derivatives taken with respect to a "dependent" variable. However, these critics obviously do not understand that the stability criterion for Fig. 1 is simply

$$(d\Delta P/dW)_{\text{pump}} - (d\Delta P/dW)_{\text{system}} \measuredangle_{s} 0$$

which of course is very similar to the criterion I *derive* for thermal stability. Moreover, if these critics take the view that flow rate is an "independent" variable, why is it that the flow rate cannot be set at W_2 in Fig. 1? An "independent" variable can be set at any intermediate value desired—which of course is true for the valves and knobs in the system.

In summary, this criticism proves that these critics have no understanding of the nature of real systems and that they are unable to relate mathematical abstraction to physical reality. Can the reader doubt that their opinions are of no value?





IN BOTH FIGURES, intersection 2 is unstable, as evidenced by the separate stability criteria (the derivation of the criterion for Fig. 1 is presented in one of my unpublished manuscripts)

Westinghouse critics:

Item 1: Any intelligent reader will discern that my concept concerns the thermal stability of systems—not heating surfaces. These critics do not even understand the written word—small wonder that they did not understand my article—or any other!

Item 2: The "widely accepted conclusions" are *wrong*, and I have not simplified them. To illustrate, Rohsenow states on p. 138 of ref. *1*: "With condensing vapor as the heat source on one side of a wall, any point on the entire (pool boiling) curve can be reached under stable conditions." My concept of thermal stability demonstrates that this conclusion is wrong and the experiments by Berensen (*2*) proved it, although apparently neither Berensen nor Rohsenow realized it.

In his experiments, Berensen built just such a boiler as described above, and he reported the results of 20 experiments, each of which purports to cover the entire pool boiling curve. If the reader will plot Berensen's results on *linear* graph paper, he will find that there is essentially *no* data in the so-called transition region for 17 of the 20 experiments. The reason for this predominant lack of data was that his boiler was usually NOT thermally stable in the transition region. The fact that this was not recognized is doubly important for the following reasons:

1. Since nothing was known about thermal stability at that time, Berensen understandably did not look for thermal instability, and as a result he did not report it, even though it was there.

2. Owing to a lack of understanding of thermal stability, Berensen correlated the transition region "results" of the 17 experiments which contained no such data! The 3 experiments which contained the desired data did not "agree" with the majority and were therefore treated as oddballs!

3. Since nothing was known about thermal stability at that time, Berensen understandably did not design his boiler with thermal stability in mind. As a result, he *intentionally* built in a very thick boiler plate, virtually guaranteeing that he would not get the desired data.

Nucleonics
December, 1964

Now of course no one can be expected to utilize knowledge which does not even exist. On the other hand, every effort must be made to understand and utilize new knowledge no matter what quarter it comes from. That my concept of thermal stability has not emanated from the universities or the national laboratories is perhaps regrettable. However, it is no less true that it represents new knowledge to the science of heat transfer. It is now incumbent on the universities and those who control the scientific literature to see to it that this new knowledge is not wasted—and to make it possible for me to publish my work in a less brief and more satisfactory manner. Whether they shall prove equal to the task is a matter for their conscience—not mine.

—E. F. ADIUTORI
Stability Consultants
Cincinnati 18, Ohio

REFERENCES

1. W. M. Rohsenow in "Modern Developments in Heat Transfer," W. Ibele, ed. (1963)
2. P. Berensen. Transition boiling heat transfer from a horizontal surface, Sc.D. thesis, MIT (1960); also *Int. J. Heat Mass Transfer* **5**, 985 (1962)

Nucleonics
December, 1964