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Adiutori The New Heat Transfer 1974

THE NEW HEAT TRANSFER

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PREFACE

This book presents a new concept of heat transfer--a new way to think about heat transfer, a new way to correlate heat transfer experimental results, a new way to design heat transfer equipment, a new way to analyze heat transfer equipment, a new way to understand the behavior of heat transfer equipment. This new way will replace Fourier's way because the new way is simpler, more logical, and much more powerful. Any problem which can be solved with Fourier's concept of heat transfer can be solved more simply, more reliably, and more accurately with the new concept. And problems which have defied solution with Fourier's heat transfer are solved with dispatch using the new heat transfer.

I have written this book for engineers and educators--and anyone interested in science. It is neither a textbook nor a handbook. It is not intended to impress the reader with my erudition or to dumbfound him with mathematics. It is an attempt to describe the new heat transfer and its application to engineers and educators who are familiar with the old heat transfer. And I have tried to present the new heat transfer in such a way that educators could teach it and engineers could apply it at the same time the leaders of the scientific community are debating its pros and cons.

I well recognize (and have frequently been told) that my writing style little resembles twentieth century scientific prose. It is the style I prefer--and the style which to me seems best suited to my goal. I wish to be understood--and a clear understanding demands clear, straightforward language.

I know that many will imagine themselves offended by my critical examination of theories and concepts of long standing--particularly when this examination demonstrates that the old ways are no longer useful and must be abandoned. I regret the offense--it is no part of my purpose.

Many times in this book, I have had to choose between possible offense on the one hand and science on the other. Each time, I have chosen science. Any error in this book is an honest error--I have not propagated a single myth, correlation, concept, or conclusion for expediency. There is nothing in this book which I do not firmly believe--nor have I expressed an opinion in any area where I do not feel well qualified to do so.

Much of this book is at odds with what has been considered accomplished scientific fact for many decades. I do not pass over the differences lightly--each time, I attempt to show how and why the new way is better than the old way. Sometimes it may seem I am mocking the old ways. I am not. My purpose is to dispel the old ways at the same time I present the new ways. In science, there has always been room for only one way--the best way at the time.

In short, this is a book about a new and simple science which will replace an old and frightfully complex art--and I have written it for engineers and educators that they may independently decide between Fourier's way and my way.

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FOR FREEDOM

To resist the new heat transfer is to defend the proposition that confounded variables are preferable to separated variables.

INTRODUCTION TO THE NEW HEAT TRANSFER

This book presents a new concept of heat transfer and bridges the gap between the "new" heat transfer and the "old" or present day heat transfer. The principal difference between the new and the old is that the new heat transfer is formulated without heat transfer coefficients, resulting in a considerable simplification of virtually all problems in heat transfer design and analysis.

The old heat transfer is primarily concerned with the ratio of heat flow (q) to temperature difference (ΔT). This ratio is called the heat transfer coefficient "h" or "U". I emphasize the word ratio because, when one becomes consciously aware that h is not fundamental but is simply a ratio of two quantities, it should become obvious that the heat transfer coefficient is an unnecessary invention. The heat transfer coefficient is in fact nothing more than a shorthand way of writing "the ratio of heat flow to temperature difference" (just as "Hertz" is nothing more than a shorthand way of writing "cycles per second").

The new heat transfer is not at all concerned with the ratio of heat flow to temperature difference. On the contrary, the new heat transfer insists on keeping the heat flow and the temperature difference separate, thus doing away with heat transfer coefficients altogether. To illustrate the difference between new and old, a text dealing with the old heat transfer would correctly state

> A large fraction of this text is devoted to a study of the factors which control the coefficient h and the correlation of these factors so that h may be predicted. McAdams (1), page 6

which could also have been correctly stated in the more revealing form

> A large fraction of this text is devoted to a study of the factors which control the ratio of heat flow to temperature difference and the correlation of these factors so that the ratio of heat flow to temperature difference may be predicted.

The new heat transfer attaches no significance to the ratio of heat flow to temperature difference and completely avoids this ratio. The corresponding statement in a text on the new heat transfer would state

> A large fraction of this text is devoted to a study of the factors which influence the relationship between heat flow and thermal driving force and the correlation of these factors so that the relationship between heat flow and thermal driving force may be predicted.

The above statement by McAdams of course refers only to convective heat transfer, since the old heat transfer treats conduction and radiation in an altogether different way than it treats convective heat transfer. On the other hand, the corresponding statement in the new heat transfer refers to all three forms of heat transfer--conduction, radiation, and convective heat transfer. In the new heat transfer, all three forms of heat transfer are treated in the same way--as transport processes--and there is only one fundamental equation to describe

all three forms of heat transfer. This fundamental equation which is the cornerstone of the new heat transfer is

$$q = f_1(\text{system properties})\ f_2(\text{TDF}) \qquad (1)$$

where the f's refer to functions in a broad sense and TDF is shorthand for thermal driving force.[1] The function f_1 includes the effect of everything except the TDF, and the function f_2 includes only the effect of the TDF. In other words, we concern ourselves with a flow responding to a driving force. In all three forms of heat transfer, the problem is to determine the functionality between heat flow and thermal driving force and the parametric effect of the system properties.

For one dimensional conduction, the thermal driving force is (dT/dx) and we wish to determine the function $q_{cond}(dT/dx)$. For radiation, the thermal driving force is $(T^4_{source} - T^4_{sink})$ and we wish to determine the function $q_{radn}(T^4_{source} - T^4_{sink})$. For convective heat transfer, the TDF is $(T_{surface} - T_{fluid}) = \Delta T$ and we wish to determine the function $q_{conv}(\Delta T)$. Since both the conduction and radiation equations from the old heat transfer are already in the form of eq 1, they retain their old forms in the new heat transfer. The convection equation from the old heat transfer is not in the form of eq 1 and we must therefore look for a new form.

In the old heat transfer, the equation

1. It will be noticed that this fundamental equation tacitly assumes that the effect of system properties and TDF do not interact. Since this is a rather fine point of only secondary import, it is discussed in the Appendix.

$$q = h \, \Delta T \qquad (2)$$

is often presented as the fundamental equation for convective heat transfer. At first glance, eq 2 seems to be in the form of eq 1. However, this is merely a mirage which results from the fact that eq 2 is not the fundamental equation for convective heat transfer in the old heat transfer--it is merely the equation used to define h. In the old heat transfer, the fundamental equation for convective heat transfer is

$$(q/\Delta T) = f_3(\text{system properties})\ f_4(\Delta T) \qquad (3)$$

where the ratio $(q/\Delta T)$ is usually referred to by its other name, "h". Eq 3 is obviously not in the form of eq 1--it does not indicate a flow responding to a driving force nor does it suggest a transport process. Moreover, since it does not conform to eq 1, it cannot be part of the new heat transfer and is discarded.

In the new heat transfer, the function for convective heat transfer $q_{conv}(\Delta T)$ cannot be stated in a specific way. The reason is of course that there are a number of convective heat transfer processes--free convection, forced convection, boiling, condensation--and, as we know from the old heat transfer, the effect of the temperature difference depends strongly on the process. Therefore the function $q(\Delta T)$ must be written in the very general form

$$q_{conv} = f_1(\text{system properties})\ f_2(\Delta T) \qquad (4)$$

and the specific functionality for each convective process must be determined separately from experiment and/or analysis.

Although the difference described above between the new and the old heat transfer may at first glance seem secondary, it should be remembered that in the old heat transfer, virtually everything is done with heat transfer coefficients--experiments are performed to measure and correlate heat transfer coefficients, equipment is designed and optimized using heat transfer coefficients, and equipment is analyzed using heat transfer coefficients--and that heat transfer coefficients are nothing more than a shorthand way of saying "the ratio of heat flow to temperature difference". Thus, in the old heat transfer, the researcher, the designer, and the analyst are all primarily concerned with the ratio of heat flow to temperature difference. In the new heat transfer, no researcher, no designer, and no analyst is at all concerned with the ratio of heat flow to temperature difference--ie no one is interested in the heat transfer coefficient. In the new heat transfer, we deal with heat flow and thermal driving force separately and simply do not permit these two primary and dynamic variables to be confounded in a ratio. In the new heat transfer, we recognize that confounding these variables in a heat transfer coefficient will only add artificial complexity and confusion to the science of heat transfer, making simple problems difficult and making difficult problems border on the impossible.

In a mathematical vein, the difference between the new and the old heat transfer is that the primary variables are confounded in the old heat transfer and separated in the new heat transfer. (Confounded variables are simply variables that have not been separated in the normal, mathematical sense.) The reader should recall that the solution of equations generally begins with an attempt to separate the variables because equations with separated variables are much easier to solve than equations with confounded variables. In the old heat transfer, the invention of the heat transfer coefficient promotes the use of confounded variables because the coefficient is itself the result of confounding the primary variables heat flow and thermal driving force. In the new heat transfer, these variables remain separate, thus ensuring that experimental results will be correlated with separated variables and permitting equipment to be designed and analyzed with separated variables. The difference between the two heat transfer concepts is precisely the difference between confounded and separated variables. To resist the new heat transfer is to defend the proposition that confounded variables are preferable to separated variables!

It is axiomatic that any problem that can be solved with confounded variables can be solved more simply with separated variables. It therefore follows that any problem that can be solved with the old heat transfer can be solved more simply with the new heat transfer. Moreover, the enhanced simplicity not only ensures more reliable results but also, as we shall see in the later chapters, makes it possible to solve problems which are virtually impossible to solve with the old heat transfer--and which indeed have never been solved with the old heat transfer.

WHY THE NEW HEAT TRANSFER?

Before developing the new heat transfer, it seems appropriate to answer the question

> Why go to all the bother of abandoning the old heat transfer with its concept of the heat transfer coefficient when it is such a simple concept and has worked so well for so many years?

The answer is that the concept of the heat transfer coefficient does absolutely nothing positive for the science of heat transfer--it merely adds artificial complexity and confusion to what will be a much simpler and more logical science without it. The invention of the heat transfer coefficient does only one thing--it forces heat transfer researchers, designers, and analysts to think, correlate, design, and analyze using a ratio of the primary variables in place of the variables themselves, and this ratio makes it impossible to think clearly or to solve problems in the simplest possible way. For example, it is extremely difficult to "see" the functionality between y and x from the information that

$$(y/x) = (3/x) + 2$$

whereas it is no problem at all if one simply eliminates the ratio by separating the variables to obtain the equivalent result

$$y = 3 + 2x$$

Moreover, the solution of even this simple equation is more difficult in the confounded version--for instance, given that $y = 4$, the solution for x is quite obvious when the variables are separated and considerably less obvious when the variables are confounded. In fact, if we do not permit the variables to be separated, the solution for x will require an iterative procedure (just as in free convection problems, the solution for ΔT given q requires an iterative procedure in the old heat transfer whereas the solution is obtained directly in the new heat transfer).

Why the new heat transfer? Because the new heat transfer is much simpler and more logical than the old heat transfer--and because the new heat transfer is a science whereas the old heat transfer with its heat transfer coefficients will come to be viewed as the art which indeed it is.

A PREVIEW OF THE NEW HEAT TRANSFER

The new heat transfer is different, better, and much more powerful than the old heat transfer. Some of the differences/improvements are

1. There are no heat transfer coefficients--ie the ratio of heat flow to temperature difference has no significance in the new heat transfer. No thinking, correlating, designing, or analyzing is based on the ratio of heat flow to temperature difference.

2. Conduction, radiation, and convective heat transfer are all treated in the same way--as transport processes involving a flow responding to a driving force. There is only one fundamental equation for all three forms of heat transfer:

   $$q = f_1(\text{system properties})\ f_2(\text{TDF})$$

   where TDF is shorthand notation for thermal driving force.

3. Heat transfer correlations are not presented in the form of dimensionless correlations--ie correlations such as

   $$N_{Nu,\ free\ conv} = f(N_{Gr} \text{ and } N_{Pr})$$

   $$N_{Nu,\ forced\ conv} = f(N_{Re} \text{ and } N_{Pr})$$

   will simply disappear. In their place will be correlations in the form cited in 2 above.

4. There is a much lesser reliance on dimensional analysis and so called power laws. The results of dimensional analysis are accepted at face value--ie as results to be considered but not accepted unless verified in depth by experimental results.

5. Many problems which require trial and error or iterative solutions with the old heat transfer are solved directly in the new heat transfer.

6. Many problems which have never been solved with the old heat transfer are easily solved with the new heat transfer. For instance, the problem of the

stability of the heat transfer process (thermal stabilit never really arose in the old heat transfer, nor did it ever yield to solution. In the new heat transfer, the subject of thermal stability is an integral branch of the science of heat transfer and the solution of the problem presents no particular difficulty.

7. The pool boiling curve (PBC) is altogether different in the new heat transfer. The new PBC is obtained by rigorously defining PBC and requiring that boiling occur at <u>all</u> points of the PBC. (The old PBC is obtained in a purely phenomenological way and contains a non-boiling region.) The new PBC is based on an objective review of the literature data which indicates a highly linear relationship between q and ΔT during nucleate and transition boiling in a saturated pool. (In the old heat transfer, the same data led to the conclusion that the subject relationship was highly <u>nonlinear</u> during nucleate and transition boiling.) This new PBC will of course require extensive revision of the old boiling theory.

8. Forced convection boiling data are correlated in the form cited in 2 above, making it easily possible to optimize boiler designs. (It is virtually impossible to optimize boiler design with the old heat transfer.)

Now that we have previewed the new heat transfer, let us return to the time of Newton and trace the development of the old heat transfer from Newton's work up to the present.

1. I defined "thermal stability" and derived the generic criterion for this type stability in "New Theory of Thermal Stability in Boiling Systems" (2). This article was a page taken from the new heat transfer and is not part of the old heat transfer, even though it preceded this book by nine years. (Similarly for the several articles on thermal stability which have followed its lead.) It is interesting to note that, although the article was severely criticized at the time, my generic criterion is now "widely accepted" (as expressed recently by the editor of a noted technical journal, 1973, private communication).

NEWTON AND HEAT TRANSFER

Many American texts credit Newton with the concept of the heat transfer coefficient. For example, McAdams (1) states on page 5

> In 1701 Newton[4] defined the heat transfer rate q_c from a surface of a solid to a fluid by the equation
>
> $$q_c = h_m A (t_w - t) \qquad (1-4)$$
>
> where h_m is the coefficient of heat transfer from surface to fluid, excluding any radiation, A is the area of the surface, t_w is the surface temperature of the wall, and t is the bulk temperature of the fluid.

where reference 4 refers to Newton, I., Phil. Trans Roy. Soc. (London), <u>22</u>, 824 (1701). Also, Rohsenow and Choi (3) state on page 92

> The surface heat transfer coefficient, h, was first suggested by Newton (3) . . . .

where reference (3) is the same article cited earlier by McAdams.

However, on reading the article by Newton, one is surprised to find no mention of a heat transfer coefficient and indeed no indication of an understanding of the heat transfer coefficient concept. In order that the reader may decide for himself whether to credit Newton with the heat transfer coefficient, let us discuss Newton's article in some detail and quote extensively from those parts of the article which pertain to heat transfer.

Newton's article was entitled "A Scale of the Degrees of Heat", was published anonymously in 1701, and was only a few pages long. The main thrust of the article was not the flow of heat, but rather the presentation of a table which contained a temperature scale proposed by Newton and based on his experimental data. Turning to Newton's description of his table and his experimental technique,

> This table was constructed by means of the thermometer and red-hot iron. By the thermometer were found all the degrees of heat, down to that which melted tin; and by the hot iron were discovered all the other degrees; for the heat which hot iron, in a determinate time, communicates to cold bodies near it, that is, the heat which the iron loses in a certain time, is as the whole heat of the iron; and therefore, if equal times of cooling be taken, the degrees of heat will be in geometrical proportion, and therefore easily found by the tables of logarithms. . . . . .
>
> Having discovered these things; in order to investigate the rest, there was heated a pretty thick piece of iron red-hot, which was taken out of the fire with a pair of pincers, which were also red-hot, and laid in a cold place, where the wind blew continually upon it, and putting on it particles of several metals, and other fusible bodies, the time of its cooling was marked, till all the particles were hardened, and the heat of the iron was equal to the heat of the human body; then supposing that the excess of the degrees of the heat of the iron, and the particles above the heat of the atmosphere, found by the thermometer, were in geometrical progression, when the times are in an arithmetical progression, the several degrees of heat were discovered; the iron was laid not in a calm air, but in a wind that blew uniformly upon it, that the air heated by the iron might be always carried off by the wind, and the cold air succeed it alternately; for thus equal parts of air were heated in equal times, and received a degree of heat proportional to the heat of the iron; . . . . .

The reader should notice in the above that, contrary to the statement by McAdams, Newton does not mention surface temperature, area, radiation, or coefficient. Fourier discusses this same article by Newton and does not credit Newton with the heat transfer coefficient. Fourier states simply:

> Newton was the first to consider the law of cooling of bodies in air; that which he has adopted for the case in which the air is carried away with constant velocity accords more closely with observation as the difference of temperatures becomes less; it would exactly hold if that difference were infinitely small. page 458

In fact, Fourier seems to credit himself with the pioneering work on heat transfer coefficients. He performs some very elementary exercises with heat transfer coefficients and then observes

> We should have considered it useless to take notice of these consequences, if we were not treating here of entirely new problems, whose results may be of direct use. page 66

McAdams' eq (1-4) is often referred to as Newton's Law of Cooling. However, the mathematical expression of Newton's statements is actually

$$\frac{dT_{object}}{dt} \propto (T_{object} - T_{fluid})$$

and this equation is what should be cited as Newton's contribution to the science of heat transfer.

FOURIER AND HEAT TRANSFER

In the hundred years from Newton to Fourier, many loose ends were developed in heat transfer and were finally tied together in Fourier's "The Analytical Theory of Heat" (4). This comprehensive treatise marks the beginning of present day heat transfer. With the exception of radiation, there has been no fundamental change in the science of heat transfer since the publication of Fourier's work. Summarizing Fourier's contribution in his own words,

> But whatever may be the range of mechanical theories, they do not apply to the effects of heat. These make up a special order of phenomena, which cannot be explained by the principles of motion and equilibrium. We have for a long time been in possession of ingenious instruments adapted to measure many of these effects; valuable observations have been collected; but in this manner partial results only have become known, and not the mathematical demonstration of the laws which include them all.
>
> I have deduced these laws from prolonged study and attentive comparison of the facts known up to this time: all these facts I have observed afresh in the course of several years with the most exact instruments that have hitherto been used.
>
> To found the theory, it was in the first place necessary to distinguish and define with precision the elementary properties which determine the action of heat. I then perceived that all the phenomena which depend on this action resolve themselves into a very small number of general and simple facts; whereby every physical problem of this kind is brought back to an investigation of mathematical analysis. From these general facts I have concluded that to determine numerically the most varied movements of heat, it is sufficient to submit each substance to three fundamental observations. Different bodies in fact do not possess in the same degree the power to contain heat, to receive or transmit it across their surfaces, nor to conduct it through the interior of their masses. These are the three specific qualities which our theory clearly distinguishes and shows how to measure. page 2

With regard to the heat transfer coefficient, Fourier states

> We have taken as the measure of the external conducibility of a solid body a coefficient h, which denotes the quantity of heat which would pass, in a definite time (a minute), from the surface of this body, into atmospheric air, supposing that the surface had a definite extent (a square meter), that the constant temperature of the body was 1, and that of the air 0, and that the heated surface was exposed to a current of air of a given invariable velocity. This value of h is determined by observation. The quantity of heat expressed by the coefficient is composed of two distinct parts which cannot be measured except by very exact experiments. One is the heat communicated by way of contact to the surrounding air: the other, much less than the first, is the radiant heat emitted. page 31

> The differential equations of the movement of heat are mathematical consequences analogous to the general equations of equilibrium and of motion, and are derived like them from the most constant natural facts.
>
> The coefficients c, h, k, which enter into these equations, must be considered, in general, as variable magnitudes, which depend on the temperature or on the state of the body. But in the application to the natural problems which interest us most, we may assign to these coefficients values sensibly constant. page 465

Fourier's work was one of the outstanding scientific contributions of the nineteenth century and he was one of the outstanding mathematicians of all time. It is interesting to speculate on whether he would have promoted the heat transfer coefficient h had he known that, in the application to the natural problems which interest us most, we may not assign a value sensibly constant to h.

THE PRESENT VIEW OF THE HEAT TRANSFER COEFFICIENT h

The most obvious difference between the new[1] and the old heat transfer is of course the disappearance of the heat transfer coefficient. As the new heat transfer becomes generally accepted, the heat transfer coefficient will simply disappear, just as phlogiston and the caloric fluid disappeared in the nineteenth century.

At the present time (1973), the most widespread view of the heat transfer coefficient seems to be that it is a convenient way to set up a macroscopic science of heat transfer (although an alternative, less convenient way is usually not mentioned). Thus, in 1973, Rohsenow and Hartnett (5) stated

> In convective processes involving heat transfer to or from a boundary surface exposed to a . . . fluid stream, it is convenient to introduce a heat transfer coefficient h defined by Eq. (5), which is known as Newton's law of cooling

$$(q/A) = h(T_{fluid} - T_{surface}) \qquad (5)$$

There is also a fairly widespread view that, although the heat transfer coefficient concept has certain drawbacks, it is the best and only feasible way of setting up a macroscopic science of heat transfer. (I often get the reaction

1. In 1965, British Chemical Engineering published my article entitled "A New and Simple Concept for the Analysis of Nonlinear Heat Transfer Phenomena". Since this article was a page taken from the new heat transfer, the views expressed in that article are not treated as part of the "present" view of the heat transfer coefficient.

"But how will we design heat transfer equipment without heat transfer coefficients?" on observing that the science of heat transfer will be far better off without heat transfer coefficients.)

In virtually all texts on the old heat transfer, the first chapter acknowledges that the heat transfer coefficient is merely an invention--that "h" is merely shorthand notation for $q/\Delta T$. However, after the first chapter, the heat transfer coefficient is treated as a fundamental quantity which ranks in importance with heat flow and temperature difference--or rather is of greater importance. Thus Jakob's accurate observation on the old heat transfer (6)

> . . . the main question in the theory and practice of heat transfer by convection is to determine the function h. page 13

In the old heat transfer, the main question in the theory and practice of convective heat transfer is not the heat flow or the temperature difference--the main question is the heat transfer coefficient--this is treated as the fundamental quantity, even though in the first chapter it is generally stated that h has no fundamental character. In the first chapter of most texts, heat flow and temperature difference are important--in the later chapters, all the emphasis is on heat transfer coefficients and there is hardly any mention of heat flow and temperature difference.

In a text on the new heat transfer, the emphasis is altogether different--the text starts out dealing with heat flow and thermal driving force and deals with them throughout the text. In the new heat transfer, the shift to heat transfer coefficients is never made--in fact, heat transfer coefficients are never even mentioned (except historically).

THERMAL CONDUCTIVITY AND THE NEW HEAT TRANSFER

It should be noted that the new heat transfer retains the concept of thermal conductivity, even though k is actually a coefficient--ie it is the ratio of heat flow to temperature gradient. Thus the new heat transfer does not altogether preclude coefficients--it precludes only the convective heat transfer coefficient. This selectivity brings up the question "Why retain one coefficient and not the other?" The answer lies in the nature of coefficients.

Coefficients are useful when dealing with linear functions which pass through (0,0). In such a case, the coefficient is also the derivative of the function and therefore the specification of the coefficient <u>completely</u> defines the function and all its derivatives. To illustrate, all linear functions passing through (0,0) have the form

$$y = mx$$

Therefore

$$\text{coefficient} = (y/x) = m = (dy/dx) = \text{derivative}$$

Thus, given that y(x) is linear and passes through (0,0), the specification that the coefficient equals 3 completely defines y(x) and we may conclude that, for all values of x,

$$y = 3x$$

Coefficients are <u>not</u> useful when dealing with nonlinear functions or with linear functions which do not pass through (0,0). This lack of usefulness results because, in such cases, the coefficient gives virtually <u>no</u> information about the function y(x) or any of its derivatives. For instance, given that y(x) may be nonlinear, the specification that the coefficient equals 3 gives us no information about y(x) or any of its derivatives!

Conductive heat transfer is highly linear and passes through (0,0). Thus, the specification that k = 10 B/hr ft F tells us that, for all values of (dT/dx), the function q_{cond}(dT/dx) is completely described by

$$q_{cond} = 10\ (dT/dx) \quad B/hr\ ft^2$$

Convective heat transfer is often highly nonlinear. Thus the specification that h = 10 B/hr ft^2 F tells us nothing about the function $q_{conv}(\Delta T)$ or any of its derivatives. We know only that, at some undefined temperature difference, the ratio of heat flow to temperature difference is equal to 10 B/hr ft^2 F--and this is virtually no information.

To summarize, the main question in theory and in practice in the new heat transfer is the determination of q(TDF). Thus the new heat transfer retains the coefficient k because the conductive process is highly linear and passes through (0,0), resulting in coefficients which completely describe q_{cond}(dT/dx). The new heat transfer rejects the coefficient h because many convective processes are highly nonlinear, resulting in coefficients which give virtually no information about $q_{conv}(\Delta T)$ and which confound the primary variables q and ΔT.

SYMBOLS

f	denotes an unspecified function
h	convective heat transfer coefficient, ratio of heat flow to temperature difference
k	thermal conductivity, ratio of heat flow to temperature gradient
N_{Gr}	Grashof Number
N_{Nu}	Nusselt Number
N_{Pr}	Prandtl Number
q	heat flow per unit area, referred to simply as heat flow in the text
t	time
T	Temperature
ΔT	$T_{surface} - T_{fluid}$
TDF	thermal driving force; $TDF_{conduction} = dT/dx$; $TDF_{convection} = (T_{surface} - T_{fluid})$; $TDF_{radiation} = (T^4_{source} - T^4_{sink})$
U	convective heat transfer coefficient, ratio of heat flow to total temperature difference
x	denotes distance and also an unspecified parameter
y	denotes an unspecified parameter

REFERENCES

1. W. H. McAdams, Heat Transmission, 3d ed., McGraw-Hill, (1954)
2. E. F. Adiutori, New theory of thermal stability in boiling systems, Nucleonics, Vol 22 (5), (1964), p 92
3. W. M. Rohsenow and H. Choi, Heat, Mass, and Momentum Transfer, Prentice-Hall, (1961)
4. J. Fourier, The Analytical Theory of Heat, Paris, (1822); English edition, Dover, (1955)
5. W. M. Rohsenow and J. P. Hartnett, Handbook of Heat Transfer, McGraw-Hill, (1973)
6. M. Jakob, Heat Transfer, vol 1, Wiley, (1949)

INTRODUCTION TO CHAPTER 2, SIMPLE PROBLEMS

In the old heat transfer, simple analytical problems are solved by first computing the overall heat transfer coefficient U from

$$U_{local} = f_1(\text{system properties})\ f_2(\Delta T_{local,oa}) \qquad (1)$$

and then determining the heat flow from

$$Q_{total} = \int q_{local}\ dA \qquad (2)$$

where

$$q_{local} = U_{local}\ \Delta T_{local,oa} \qquad (3)$$

In the new heat transfer, we solve these same problems without U--ie without ever computing or being concerned with U, the ratio of heat flow to temperature difference. In the new heat transfer, eqs 1 to 3 are replaced by eqs 4 and 5:

$$Q_{total} = \int q_{local}\ dA \qquad (4)$$

where

$$q_{local} = f_a(\text{system properties})\ f_b(\Delta T_{local,oa}) \qquad (5)$$

The advantage of the new way is that the solution of virtually all heat transfer problems is vastly simplified. As the simple problems in this chapter illustrate, many problems which require iterative or trial and error solutions in the old heat transfer are solved directly and thus more simply in the new heat transfer. (I define a "direct solution" as one in which the problem is solved by obtaining one equation with one unknown; the equation may be linear or nonlinear and may be expressed graphically or analytically. The difference between direct and indirect solutions is obvious in the problems.)

In this chapter, we deal with three simple problems which are intended to accomplish the following:

a) illuminate the mathematical ineffectiveness of the heat transfer coefficient;

b) illustrate that problem solution is much simpler when coefficients are not used;

c) demonstrate the solution of simple heat transfer problems using the new heat transfer--ie without using heat transfer coefficients.

Each problem is arranged in four steps with each step beginning on a new page. The steps are:

1) problem statement and background;

2) description of experimental results and correlation in the old way;

3) problem analysis in the old way;

4) repetition of 2 and 3 in the new way.

My intent is that the reader work out the numerical result to the problem before turning the page to step 3. Then, when you feel comfortable with your analysis and result, turn the page to step 3 and compare your analysis with the old way analysis. After this comparison, it would be worthwhile to spend a few minutes predicting what step 4 is like--ie how will the overall solution differ when coefficients are not used? (Of course the numerical result will be the same, but everything else will be different.) The last step is to review the new and the old way solutions until you can confidently and independently state that the invention of coefficients did nothing except complicate and confuse what would otherwise have been a very simple problem.

At this point, I want to emphasize that the purpose of the new heat transfer is not merely to simplify the solution of problems which can be solved with the old heat transfer, but rather to simplify problems which are so artificially difficult in the old heat transfer that they have never been solved. As we shall see in the later chapters, heat transfer problems which have defied solution

with the old heat transfer are solved with dispatch in the new heat transfer. I hope you will bear with me and work through these simple problems, recognizing that they are primarily stepping stones to an understanding of the new heat transfer and the more difficult problems ahead which are our real concern.

PROBLEM PREVIEW

The three problems in this chapter deal exclusively with static behavior and thus earn the label "simple". In later chapters, we will deal with the dynamic behavior of heat transfer equipment. We will not consider dynamic problems "simple", even though we will have no great difficulty solving them in the new heat transfer. (Although the phrase "static and dynamic heat transfer behavior" has a strange ring in the old heat transfer, it is quite natural in the new heat transfer which consciously recognizes that the flow of heat is a dynamic process.)

The first two problems provide a more or less detached view of the coefficient concept by dealing with problems outside the realm of heat transfer. Problem 1 is primarily a mathematical exercise which deals with the strength of materials. It demonstrates the solution of problems with and without coefficients and illustrates the confusion which would result if the stress coefficient (ie the elastic modulus "E") were applied to inelastic (ie plastic and nonlinear) behavior in the same manner that the heat transfer coefficient is applied to nonlinear heat transfer phenomena in the old heat transfer. Problem 2 deals with an unspecified transport problem and illustrates the artificial complexity introduced by coefficients when dealing with any nonlinear transport process. The third problem deals with what is perhaps the simplest form of nonlinear heat transfer--free convection at a single interface. It demonstrates that the invention of the heat transfer coefficient results in iterative solutions to many elementary free convection problems which are solved directly in the new heat transfer.

Before going on to the problems, I would again like to suggest that you work through the problems in the stepwise fashion outlined on page 2. It seems to me that working through these simple problems is the best way to gain an appreciation of the simplicity and clarity of the new heat transfer.

PROBLEM 1 STRENGTH OF MATERIALS STEP 1

BACKGROUND

The Fuller Mfg. Co. is considering a new material, APCO-123, for use in their line of wagon axles. Unfortunately, there is no available information on the strain behavior of this new material. Fuller therefore sends tensile specimens to the Johnson Material Testing Laboratory with a request to obtain the required data and generate a design correlation (graphical or analytical) for APCO-123.

PROBLEM STATEMENT

Based on the design correlation by Johnson, what strain will result in APCO-123 from a stress of 52,000 psi?

PROBLEM 1 OLD WAY SOLUTION STEP 2

CORRELATING WITH COEFFICIENTS

The data is obtained at Johnson in the form load vs strain. This data is reduced to the form E vs strain. E is generally called the modulus of elasticity and is defined by Hooke's Law

$$\sigma = E\ \varepsilon \qquad (6)$$

(It should be noted that E might also have been called the stress coefficient since it is the ratio of stress to strain.) The experimental results are correlated in the form E(ε) and this correlation is presented graphically in Fig 1. (This correlation could also have been presented analytically, but the mathematics would obviously have been quite cumbersome.) This Figure is sent to Fuller along with the recommendation that the curve be used to predict the strain behavior of APCO-123.

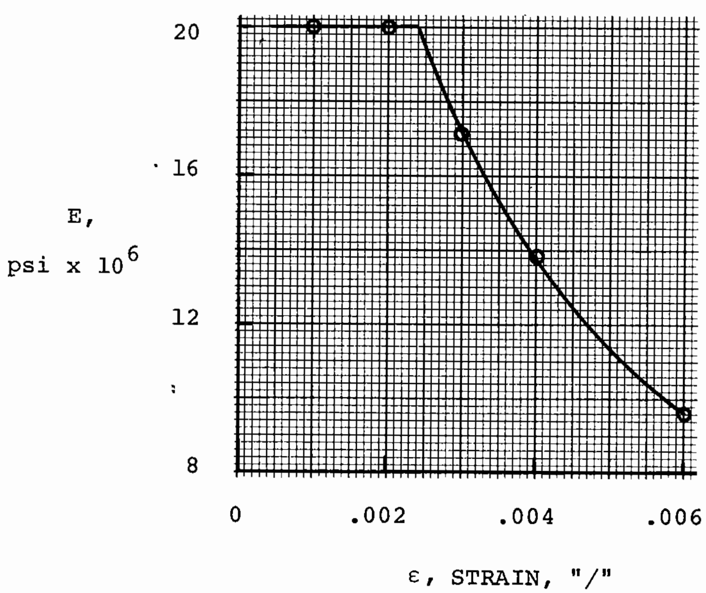


FIGURE 1

PROBLEM 1 OLD WAY SOLUTION STEP 3

PROBLEM ANALYSIS

The solution of the problem requires that we determine E from Fig 1 so that we may then determine the strain from Hooke's Law (eq 6). We are given that the stress is 52000 psi and thus the strain, using Hooke's law, is given by 52000/E. Inspection of Fig 1 shows that the value of E depends on the value of the strain and, since we do not know the strain, we cannot determine the coefficient E by simple inspection of Fig 1. Therefore, we must solve the problem indirectly by trial and error or by iterating. Let us iterate. We first assume a value for E, determine ε from ε = 52000/E, then use this value of ε to determine a new value of E from Fig 1. We then repeat this loop with the new value of E until we feel we have an accurate result. Thus

$E_1 = 20\text{x}10^6 \rightarrow \varepsilon = 52000/20\text{x}10^6 = .0026 \rightarrow E_2 = 19\text{x}10^6$

$E_2 = 19\text{x}10^6 \rightarrow \varepsilon = 52000/19\text{x}10^6 = .00274 \rightarrow E_3 = 18.2$ "

	.00286	17.8
similarly	.00292	17.4
	.00299	17.2
	.00302	17.1

ANSWER

A stress of 52000 psi will result in a strain of .00302 in APCO-123 based on the Johnson correlation.

WORKING TIME: 5 to 20 minutes

PROBLEM 1 NEW WAY SOLUTION STEP 4

CORRELATING THE NEW WAY

The data is obtained at Johnson in the form load vs strain. This data is reduced to the form stress vs strain and no significance is attached to their ratio. (Note that σ = load/area; E = load/(area x ε)). The experimental results are correlated in the form $\sigma(\varepsilon)$ and this correlation is presented graphically in Fig 2. This Figure is sent to Fuller along with the recommendation that the curve be used to predict the strain behavior of APCO-123.

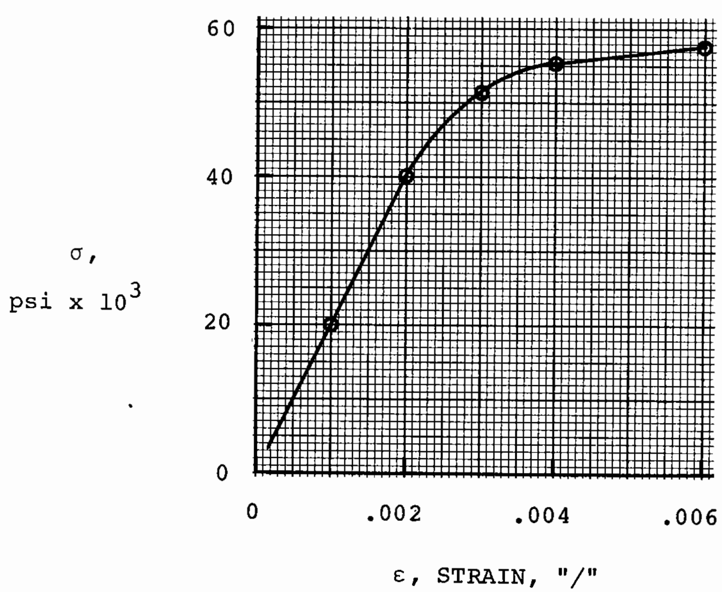


FIGURE 2

PROBLEM ANALYSIS

Inspection of Fig 2.

ANSWER

A stress of 52000 psi will result in a strain of .00305 in APCO-123 based on the Johnson correlation.

WORKING TIME: several seconds

DISCUSSION OF PROBLEM 1

Problem 1 illustrates that problems which require iterative or trial and error solutions when solved the old way can often be solved directly using the new way. It also demonstrates that, for simple problems of this kind, the new way is about 100 times faster, is more reliable (in the sense that it is quite easy to get the wrong answer using the old way), and is more accurate. It should also be noted that it is virtually impossible to "see" the functionality between stress and strain when the results are correlated the old way.

In Chapter 1, we noted that coefficients are useful only when they are also derivatives. We also noted that coefficients are also derivatives <u>only</u> if the function of concern is linear and passes through (0,0).

The function $\sigma(\varepsilon)$ is linear in the elastic region and passes through the point (0,0). Thus, the modulus of elasticity is useful in the elastic region not because it is the coefficient σ/ε, but because in this region

$$E = d\sigma/d\varepsilon \quad \text{and} \qquad (7a)$$

$$\sigma(\varepsilon = 0) = 0 \qquad (7b)$$

Equation 7 is the underlying reason for the usefulness of E in the elastic region--not equation 6. As the first problem shows, when eq 7 does not hold, the stress coefficient defined by eq 6 is not a useful concept and merely complicates what would otherwise be a very simple problem.

It should be noted that stress/strain results are <u>never</u> presented in the form of Fig 1. The stress/strain world uses the "new" way of Fig 2. And eq 6 is not really Hooke's Law. Hooke's Law was the observation that stress is proportional to strain in the elastic region. Eq 6 is the mathematical expression of Hooke's Law only if we recognize that E does <u>not</u> depend on ε. In other words, we must recognize that eq 6 describes the functional relationship between σ and ε in the elastic region and does not merely define E to be the ratio of stress to strain. (Note that $q = h\Delta T$ is identical to eq 6, yet tells us nothing about the functional relationship between q and ΔT--it merely defines h to be the ratio of q to ΔT.) The statement that a given material exhibits an E of 30×10^6 psi is understood to mean that the slope of the stress/strain curve and the ratio of stress to strain are 30×10^6 psi <u>throughout</u> <u>the</u> <u>elastic</u> <u>region</u>.

What we have actually done in Problem 1 is illustrate what would result if E were used in a manner analogous to the heat transfer coefficient, whereas E is actually used in a manner much more analogous to the thermal conductivity coefficient--ie E and k deal with essentially linear phenomena.

(Those who are familiar with plastic buckling will recognize that the modulus defined by eq 7a is generally called the tangential modulus. Those who are familiar with nonlinear electrical resistors will recognize that the electrical counterpart of eq 7a defines what is generally called the dynamic electrical resistance. There is no similar "dynamic heat transfer resistance" in the old heat transfer* because it simply does not recognize that heat transfer is a dynamic process. The new heat transfer recognizes the dynamic nature of the heat transfer process, but contains no dynamic heat transfer resistance because the new heat transfer does not use the concept of resistance. But this is taking us beyond the scope of Chapter 2--we will return to it in a later chapter.)

*Again I would emphasize that "the old heat transfer" does not include my two published articles which were pages taken from the new heat transfer, nor the several articles which have since followed their lead.

<u>PROBLEM 2 A TRANSPORT PROCESS STEP 1</u>

BACKGROUND

The Marshall Equipment Co. plans to add a new line of equipment. The function of this equipment is to transfer a substance we shall call "Y" from one fluid stream to another. Unfortunately, the Marshall Co. has no experience with Y transfer and knows only that it in some way depends on the difference in X between the two streams. In order to obtain fundamental design information for this new line, Marshall contacts the Jones Research Lab which has researchers who are knowledgeable on Y transfer. The designers at Marshall spell out the parametric range of interest and it is agreed that, for $10,000, Jones will perform the indicated experiments, obtain the data, correlate the results, and generate the design correlation necessary to deal with Y transfer.

PROBLEM STATEMENT

What value of ΔX is required in order to obtain a Y transfer rate of 8?

PROBLEM 2 OLD WAY SOLUTION STEP 2

CORRELATING WITH COEFFICIENTS

The Jones Research Lab is an old way lab and the researchers are familiar with Y transfer and with Smith's Law which states that

$$y = H \, \Delta X \tag{8}$$

where y = flow rate of Y, ΔX = difference in X between the two streams, and H = Y transfer coefficient. The researchers at the lab have the equipment built and perform the experiments necessary to measure the Y transfer coefficient H. (It is common practice in the old heat transfer to speak of "measuring heat transfer coefficients", even though coefficients do not exist and therefore cannot be "measured".)

The experimental results are correlated graphically and the resultant design correlation is the curve in Fig 3. This Figure is sent to Marshall along with the recommendation that it be used to design Y transfer equipment.

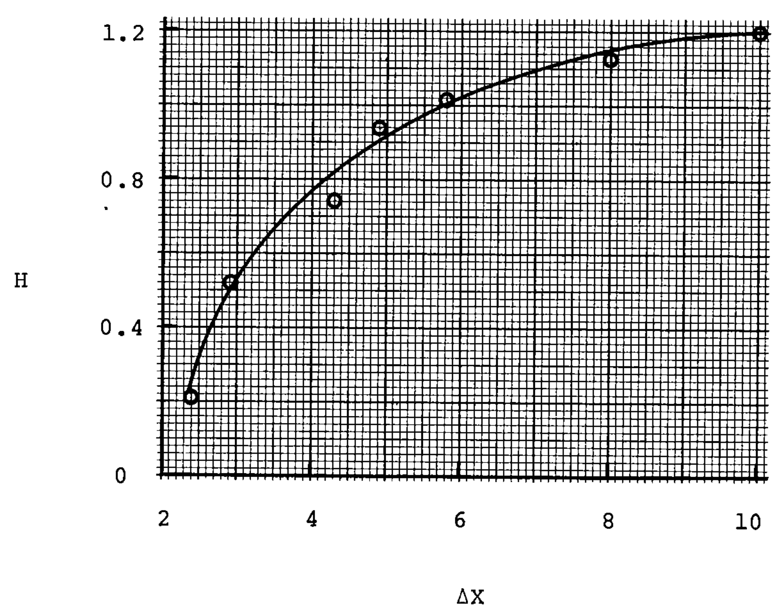


FIGURE 3

PROBLEM 2 OLD WAY SOLUTION STEP 3

PROBLEM ANALYSIS

We must solve for ΔX using the Y transfer coefficient correlation $H = f(\Delta X)$ expressed graphically in Fig 3 and Smith's Law, eq 8. From the problem statement and eq 8, we know that

$$\Delta X = y/H = 8/H \tag{9}$$

The problem therefore reduces to determining H from Fig 3. Inspection of Fig 3 indicates that, unfortunately, H is a function of ΔX and therefore we cannot determine ΔX in a direct fashion--we may iterate or try-and-error. Iterating as in Problem 1,

$$H_1 = 0.8 \rightarrow \Delta X = 8/0.8 = 10 \rightarrow H = 1.2$$

1.07

similarly 1.12

1.10

$\Delta X = 7.27 \rightarrow H = 1.11$ close enough

ANSWER

A Y transfer rate of 8 requires that $\Delta X = 7.27$.

WORKING TIME: 5 to 10 minutes

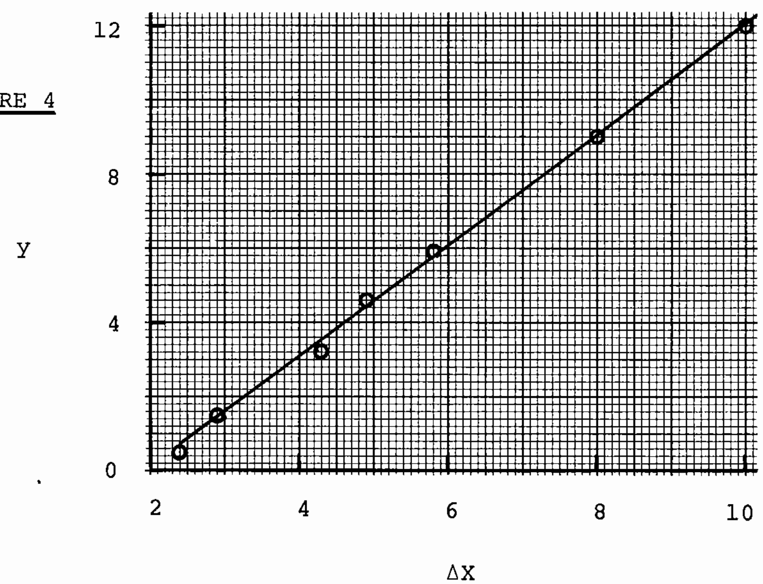
PROBLEM 2 NEW WAY SOLUTION STEP 4

CORRELATING THE NEW WAY

The Jones Research Lab is a new way lab and their researchers do not use coefficients for transport processes. The researchers at the lab have the equipment built and perform the experiments necessary to measure and correlate Y transfer.

The experimental results are correlated graphically in the form $y(\Delta X)$. The resultant design correlation is the curve in Fig 4 which is sent to Marshall with the recommendation that it be used to design Y transfer equipment.

FIGURE 4



PROBLEM ANALYSIS

Inspection of Fig 4.

ANSWER

A Y transfer rate of 8 requires that $\Delta X = 7.30$.

WORKING TIME: several seconds

DISCUSSION OF PROBLEM 2

Problem 2 illustrates the confusion introduced into transport phenomena in general by the invention of coefficients. It points out that many transport problems which require cumbersome, indirect solutions with coefficients are solved directly when coefficients are abandoned altogether.

Problem 2 also demonstrates that a highly linear function which does not go through (0,0) will result in a highly <u>non</u>linear coefficient function. For example, the analytic expression of the curve in Fig 4 is

$$y = 1.5(\Delta X - 2) \qquad (10)$$

This linear expression results in a highly nonlinear transfer coefficient function $H = y/\Delta X = f(\Delta X)$:

$$H = 1.5 - (3/\Delta X) \qquad (11)$$

which is of course the analytic expression of the curve in Fig 3.

It should be noted that, had we recognized that eq 11 was suggested by Fig 3, we could have solved Problem 2 directly by substituting eq 11 into Smith's Law (eq 8). Thus

$$y = H\,\Delta X = (1.5 - 3/\Delta X)(\Delta X) \qquad (12)$$

$$\therefore\ y = 1.5(\Delta X - 2) \qquad (13)$$

Eq 13 is of course a new way correlation--it is identical to eq 10--and allows the direct solution of both y and ΔX. However, it is important to note that this direct solution <u>using</u> H is nothing more than a roundabout way of <u>eliminating</u> H! We have in fact eliminated H by

1. obtaining data in the form $y(\Delta X)$;
2. inventing the ratio H;
3. transforming the data to the form $H(\Delta X)$;
4. determining the analytic expression $H(\Delta X)$, eq 11;

5. substituting the analytic expression $H(\Delta X)$ into Smith's Law (eq 8) in order to
6. obtain an analytic expression which does not contain H and which is in the form $y(\Delta X)$--the same form we started out with in step 1!!!!

Thus this "direct" solution using coefficients is actually an indirect way of eliminating coefficients.

These six steps demonstrate that the invention of coefficients leads only to the elimination of coefficients. The use of coefficients is in fact similar to a flight from Dallas to Ft. Worth via Peking--it can be done, but it involves a great deal of unnecessary mileage and the strong likelihood that we will miss the mark by a wide margin.

In the new heat transfer, we eliminate step 2, step 3, step 4, and step 5--we go directly from step 1 to step 6 and have no interest in the intermediate steps, the value of the coefficient, or "Newton's Law".

The above example demonstrates that, in the old heat transfer, the researchers and experimenters generate coefficients--and the designers and analysts eliminate them. In the new heat transfer, researchers and experimenters generate correlations in the form most useful for design and analysis and in the form most likely to be in accordance with nature. The end result is in fact that heat transfer is transformed from an art to a science.

PROBLEM 3 HEAT TRANSFER STEP 1

BACKGROUND

The Ace Electric Co. manufactures a line of C-12 resistors intended for operation in a large, essentially draft free, console. The designers must accurately predict the thermal behavior of these resistors because their performance is strongly temperature dependent. The designers decide that a generalized heat transfer correlation would not possess the required accuracy for this application. They therefore let a contract to the Doe Heat Transfer Laboratory to perform heat transfer experiments with production resistors in a production console, obtain and reduce the required data, and generate a design correlation which can be used to predict the thermal behavior of C-12 resistors at various power levels. The contract spells out the range of parameters to be investigated: surface heat flux 1 to 100 B/hr ft^2; ambient temperature 90 to 100 F; resistor size 0.5" d x 1.0" long; resistors mounted horizontally in the console supplied by Ace.

PROBLEM STATEMENT

What temperature rise above ambient will be experienced by a C-12 resistor which operates at a surface heat flux of 70 B/hr ft^2?

PROBLEM 3 OLD WAY SOLUTION STEP 2

CORRELATING WITH COEFFICIENTS

The Doe HTL is an old way lab and their researchers are familiar with the old heat transfer--ie they know that free convection heat transfer data are well correlated in the form

$$N_{Nu} = f(N_{Gr} N_{Pr}) \quad (14)$$

which can be rewritten as

$$h = f_1(\text{system properties})\ f_2(\Delta T) \quad (15)$$

Since the system properties are essentially invariant in the Ace application, they rewrite eq 15 in the form

$$h = f(\Delta T) \quad (16)$$

The researchers at the lab perform the experiments necessary to measure the heat transfer coefficient at various levels of ΔT and thus determine the function $h(\Delta T)$.

The experimental results are correlated graphically in the form $h(\Delta T)$ and the resultant design correlation is the curve in Fig 5. This curve is sent to Ace along with the recommendation that it be used to predict the thermal behavior of C-12 resistors.

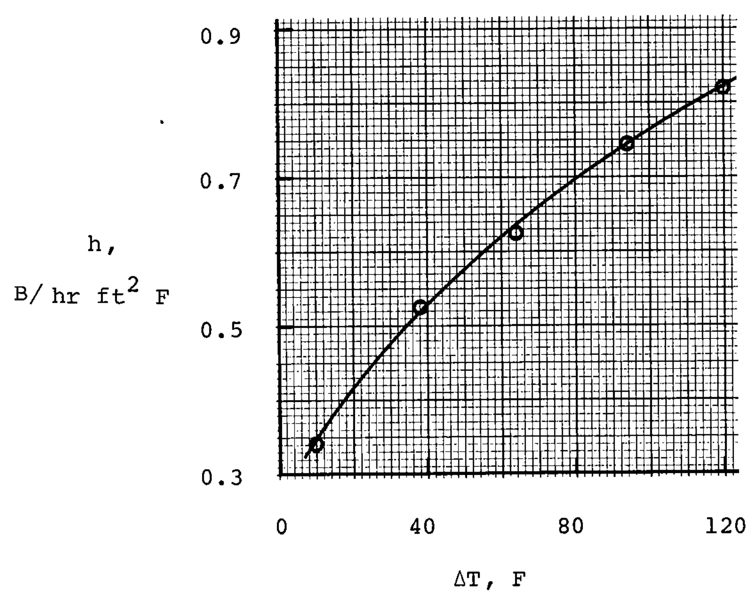


FIGURE 5

PROBLEM 3 OLD WAY SOLUTION STEP 3

PROBLEM ANALYSIS

The analysis of this problem is identical to the old way analyses of Problems 1 and 2.

ANSWER

A surface heat flux of 70 B/hr ft^2 will result in a surface temperature rise of 94 F above ambient.

WORKING TIME: about 5 minutes

PROBLEM 3 NEW WAY SOLUTION STEP 4

CORRELATING THE NEW WAY

The Doe HTL is a new way lab and their researchers do not use heat transfer coefficients. Their researchers perform the experiments necessary to measure q at various levels of ΔT and thus determine the function q(ΔT).

The experimental results are graphically correlated in the form q(ΔT) and the resultant design correlation is the curve in Fig 6. This Figure is sent to Ace along with the recommendation that it be used to predict the thermal behavior of C-12 resistors.

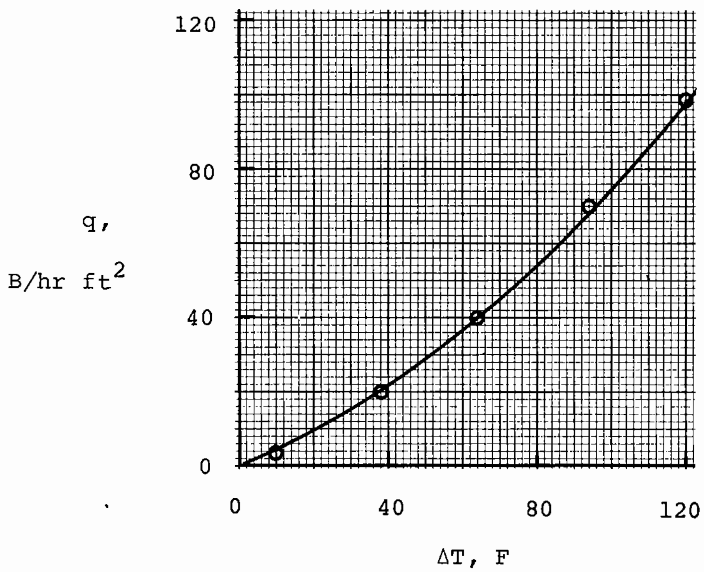


FIGURE 6

PROBLEM ANALYSIS

Inspection of Fig 6.

ANSWER

A surface heat flux of 70 B/hr ft^2 will result in a surface temperature rise of 96 F above ambient.

WORKING TIME: several seconds

DISCUSSION OF PROBLEM 3

Problem 3 illustrates that the invention of the heat transfer coefficient results in indirect solutions to many free convection problems which can be solved directly and thus more simply in the new heat transfer. In the old heat transfer, a direct solution is possible if ΔT is given, but an indirect solution is required if q is given and the problem is to solve for ΔT. If h is eliminated as in the new heat transfer, a single correlation is obtained which permits the direct solution of both q and ΔT.

As in the discussion of Problem 2, h can be eliminated by either not inventing it in the first place or by the roundabout process of inventing it in order to eliminate it. Needless to say, the best way to eliminate h is to not invent it.

RESULTS AND CONCLUSIONS

The problems in this chapter validate the statement made in Chapter 1, page 9:

> Many problems which require trial-and-error or iterative solutions with the old heat transfer are solved directly in the new heat transfer.

As illustrated by Problem 3, the old heat transfer requires that free convection problems be solved indirectly when q is given and ΔT is unknown. (I would have preferred to take Problem 3 from a text on the old heat transfer, but unfortunately the many texts I consulted had no examples of the trial-and-error solution for ΔT when q is given--the examples always illustrated the direct solution for q given ΔT. Indeed, I found that many texts on the old heat transfer give no hint that a large number of practical problems in heat transfer cannot be solved directly. One exception is Zemansky's "Heat and Thermodynamics", 4th ed., 1957, McGraw-Hill, and his discussion on page 93 of a practical problem in free convection:

> An arrangement encountered often in practice consists of a wall maintained at a constant temperature T_w, coated with a layer of insulating material of thickness x and of thermal conductivity k. The outside of the insulation is in contact with the air at atmospheric pressure and at temperature T_a. . . . Since h varies as the fourth root of $T - T_a$, the simplest way to solve for T is by trial and error. Thus, assuming T to be any arbitrary value, h is calculated and then multiplied by $T - T_a$. The value of $(k/x)(T_w - T)$ is then calculated and compared with $h(T - T_a)$. If these quantities are not equal, another value of T is chosen, and so on, until the equation
>
> $$(k/x)(T_w - T) = h(T - T_a)$$
>
> is satisfied.

In a larger sense, the problems in this chapter illustrate that the invention of h is not only unnecessary but is actually harmful. These problems illustrate that the invention of h results in a <u>hundredfold increase</u> in the time required to solve these simple problems. Moreover, they correctly suggest that less simple problems may be virtually impossible to solve with the old heat transfer (as we shall find in the later chapters).

The problems also suggest how to deal with convective heat transfer during the transition from old heat transfer to new heat transfer. All correlations which contain h are suspect. Each correlation should be transformed back to the form $q(\Delta T)$ and then compared with the underlying data which was necessarily obtained in the form $q(\Delta T)$. If the supporting data has been destroyed or is undecipherable, serious consideration should be given to repeating the experiments.

The problems also illustrate that the proper objective for the researcher and the experimenter is the correlation of heat transfer results in the useful form $q(\Delta T)$ rather than in the old heat transfer form, $h(\Delta T)$.

It should be noted that, since Nukiyama's (1) pioneering work on boiling, pool boiling researchers and condensation researchers have been correlating and comparing their heat transfer data in the desirable form $q(\Delta T)$ which of course fits in with the new heat transfer. However, and this is the important point, the new heat transfer involves the recognition that this form is not merely a convenient form for the comparison of data. Rather, $q(\Delta T)$ is the optimum form for the <u>design correlation</u> of boiling, condensing, and indeed <u>all</u> convective heat transfer phenomena--and $q(\Delta T)$ is the optimum form for the <u>design</u> and <u>analysis</u> of all types of convective heat transfer equipment.

The changeover from $h(\Delta T)$ to $q(\Delta T)$ as required in the new heat transfer will transform convective heat transfer from a complex art to a simple science.

SYMBOLS (see also Chapter 1, page 21)

A	area, in^2
E	stress/strain, psi
H	see page 12
Q	heat flow, B/hr
ΔT_{oa}	overall temperature difference
X	see page 12
y	" "
Y	" "
ε	strain, "/"
σ	stress, psi

REFERENCES

1. S. Nukiyama, J. Soc. Mech. Eng. Japan 37, No. 206, 367 (1934)

INTRODUCTION TO CHAPTER 3, STATICS

In Chapter 2, we solved some simple problems using both the old and the new heat transfers. The purpose of those problems was primarily to illustrate how and why the new way is better than the old way. We saw that the new way can effectively deal with simple nonlinear problems and that the old way can also deal with these same problems, but not effectively. We saw that the new way was 100 times faster than the old way and also was more reliable and more accurate.

In this chapter, we deal with somewhat less simple problems and with a different purpose. We are concerned with practical problems whose primary purpose is to demonstrate the design and analysis of real equipment using the new heat transfer. In this chapter, we design and analyse two heat exchangers--one containing only linear heat transfer phenomena which pass through the point $q\{\Delta T=0\}=0$, the other containing a highly nonlinear heat transfer phenomenon. We design and analyse these heat exchangers using the old heat transfer in Problem 1, the new heat transfer in Problem 2. Those readers who by now have no further interest in the old heat transfer need not bother with the nonlinear part of Problem 1.

OLD WAY VS NEW WAY

In the old heat transfer, the determination of the local heat flow q involves two steps: calculate the thermal conductance (h or U) or the thermal resistance (1/h or 1/U); calculate the local heat flow q from

$$q = h\Delta T \quad \text{or} \quad q = U\Delta T \qquad (1)$$

In the new heat transfer, there is only one step--calculate the local heat flow q from the function $q\{\Delta T\}$. (In the remainder of this book, { } will indicate functionality. Thus the expression $q\{\Delta T\}$ will indicate that q is a function of ΔT and will <u>not</u> indicate that q is to be multiplied by ΔT.) As demonstrated in the following problems dealing with equipment design and analysis, the new heat transfer cares absolutely nothing about the thermal resistance or the thermal conductance--it cares only about the heat flow and the thermal driving force.

Most texts on the old heat transfer draw an analogy between thermal resistance and electrical resistance in order to promote an understanding of thermal resistance. They also generally demonstrate the use of equivalent electrical linear resistance circuits as an aid in problem solving, particularly when dealing with radiation problems. Of course radiation is a highly nonlinear phenomenon and must first be "linearized" in order to fit it in with linear circuit theory. (I place quotes around "linearized" because I feel that, in this case, the word is improperly used. It would be more accurate to say that radiation is "coefficientized" or "ratioized" in order to fit it in with linear circuit theory. We shall return to this subject in the chapter dealing with radiative heat transfer.)

Kreith (Principles of Heat Transfer, 1st edition, 1958, pp 18-21, 89, 119-120, 159, 200-211, and also the 2nd edition) discusses the analogy between electrical resistance and thermal resistance at some length and correctly demonstrates the use of this analogy to develop thermal resistance circuits which are useful for problem solving in the old heat transfer. Kreith's description of this old way concept is quite adequate. The reader who has an historical interest in it should refer directly to Kreith's book on the old heat transfer.

In a very real sense, the old heat transfer was concerned not with the <u>flow</u> of heat, but with the <u>resistance</u> to the flow of heat. Thus Jakob's accurate observation on the old heat transfer which we noted in Chapter 1:

> . . . the main question in the theory and practice of heat transfer by convection is to determine the function h.

In the new heat transfer, we are not concerned with h--with the resistance to the flow of heat--we care only about the flow of heat. We do not make an analogy between the flow of heat and the flow of electricity because flow behavior and flow circuits are so simple

as to be virtually intuitive. Flow behavior, flow theory, and flow circuit theory are all well described by the following "flow law":

> The flow rate into a tank equals the flow rate out of the tank plus the storage rate in the tank.

And it is intuitive that this "law of flow" applies equally to the flow of heat, electricity, gases, liquids, solids, or anything else. (While the "law of flow" is usually expressed in a more elegant manner, I doubt that it can be expressed in a simpler or more useful manner.)

Thus, in the new heat transfer, we do not deal with thermal resistance--we deal with heat <u>flow</u>. We do not develop correlations which describe thermal resistance--we develop correlations which describe heat <u>flow</u>. We do not use resistance circuits to help solve complex problems--we use <u>flow</u> circuits. We do not draw analogies between the flow of heat and the flow of anything else--instead, we consciously recognize that heat transfer is heat <u>flow</u>--and that heat <u>flow</u> is just as simple and just as dynamic as the <u>flow</u> of anything else.

In the old heat transfer, the key word is resistance (conductance) and the keystone is the resistance (conductance) concept, h{ΔT}. In the new heat transfer, the key word is <u>flow</u> and the keystone is the flow concept, q{TDF}. While this difference may seem subtle in theory, it makes orders of magnitude difference in application--as we saw in the simple problems in Chapter 2 and as we shall see in the following problems dealing with the design and analysis of heat transfer equipment.

STATIC HEAT TRANSFER BEHAVIOR

The old heat transfer deals only with the static behavior of heat transfer equipment and it may at first seem strange to distinguish between static behavior and dynamic behavior. To simplify the transition from old heat transfer to new, this chapter deals only with static heat transfer behavior--Chapter 4 introduces the concept of the dynamic behavior of heat transfer equipment.

In static heat transfer analysis, we deal with the operating characteristics of the equipment in a steady-state sense--ie we deal with the potential steady-state operating points of the equipment. We do not concern ourselves with dynamic questions such as the manner in which the equipment responds to small changes (perturbations) in the vicinity of these operating points--or even whether these potential steady-state operating points represent stable or unstable conditions.

Those who are familiar with the old heat transfer will recognize that dynamic behavior was only hinted at in a very qualitative way in the old heat transfer. This is quite understandable because, as we shall see in the next chapter, the correct solution of dynamic problems using the old heat transfer would have been so cumbersome and so complex as to have been virtually if not identically impossible.

PROBLEM PREVIEW

The problems deal with the design and analysis of two components--the A/B heat exchanger in which all heat transfer phenomena are linear and pass through the point $q\{\Delta T=0\}=0$ and the A/C heat exchanger in which the heat transfer is nonlinear at one interface.

As pointed out in Chapter 1, the old heat transfer can deal effectively with linear heat transfer phenomena which pass through the point $q\{\Delta T=0\}=0$. Thus the reader will experience no difficulty in designing and analysing the A/B exchanger using the old heat transfer. In

fact, you may very well be able to design and analyse the A/B exchanger faster with the old heat transfer than with the new. However, I hope you will bear in mind that the purpose of the A/B exchanger is to promote an understanding of the new way by applying it to the only design/analysis problem which is simple enough to permit direct solution using the old heat transfer.

The intent of the A/B exchanger is to simplify the transition from old way to new way by providing a "common ground" where both the old and the new heat transfers are effective. On this common ground, the new way may seem strange, confusing, and indirect compared to the old way. However, please bear in mind that you are comparing a new tool which you are using for the first time with an old tool which you have known and used for years. If you cannot solve the A/B design/analysis problem as quickly or as easily with the new tool, please assume that the lack of proficiency results from a lack of familiarity with the tool and not from a deficiency in the tool. In any event, the A/C design/analysis problem will provide eloquent verification of this assumption. After considering the problem, I think most readers will refuse to design/analyse the A/C exchanger using the old heat transfer on the basis that it is a lot of extra work for nothing--which indeed it is!

Both heat exchangers are viewed as fluid/fluid heat exchangers in which the flow rates of the heat source and heat sink fluids are sufficiently high that the fluid temperature change through the equipment is negligible. In other words, we can typify the temperature distribution in each fluid by a single temperature rather than a temperature profile. (We could of course accomplish the same end by saying that we are dealing with a differential element of a heat exchanger. However, I feel it is simpler to visualize equipment behavior in terms of components which behave like differential elements rather than in terms of differential elements which must then be integrated to determine component behavior.)

In each design, the problem has been idealized in such a way that the heat flow will be uniform throughout the exchanger, allowing us to write

$$Q_{component} = q\, A_{component} \tag{2}$$

When dealing with equipment wherein q varies with location, we would of course have to obtain $Q_{component}$ from

$$Q_{component} = \int q\{A\} dA \qquad (3)$$

As pointed out on page 1 of Chapter 2, this same integration faces us in both the new and the old heat transfers. Since integration is the same in both heat transfers, we will not dwell on it here except to note that the integration indicated by eq 3 is required whenever q varies appreciably within the equipment.

STATIC ANALYSIS IN THE NEW HEAT TRANSFER

Static analysis is very simple in the new heat transfer. The only requirements are the following:

1. An understanding of the "flow law" on page 3.

2. The observation that

$$\Delta T_{total} = \Sigma \Delta T_i \qquad (4)$$

   ie the total temperature difference between the heat source and the heat sink in the equipment is the sum of the individual temperature differences across interfaces, walls, etc.

3. A knowledge of the functions $q_i\{TDF_i\}$ which describe the heat flow across interfaces, walls, etc. in the equipment.

Problem 2 demonstrates the manner in which these three inputs are applied to the design and analysis of heat transfer equipment in the new heat transfer.

PROBLEM 1 THE DESIGN AND ANALYSIS OF HEAT TRANSFER COMPONENTS USING THE OLD HEAT TRANSFER

BACKGROUND

The Marshal Equipment Co. plans to market custom made fluid/fluid heat exchangers for use in cooling electrical equipment. Two types will be marketed--Martherm A/Martherm B exchangers and Martherm A/Martherm C exchangers. (Martherm is the tradename for Marshall's thermal fluids.) The heat exchanger design and the type system in which they will operate are shown schematically in Figure 1:

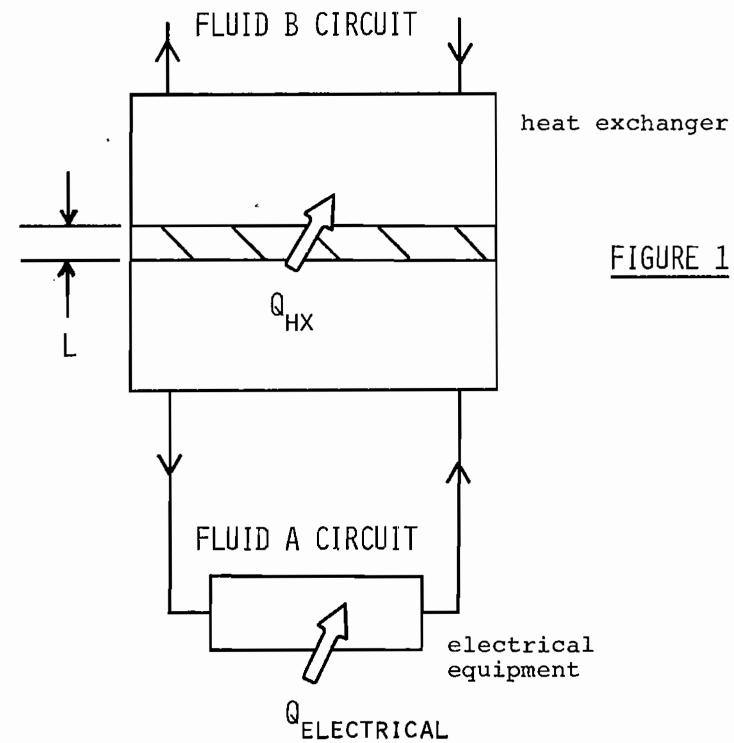


FIGURE 1

(The Martherm B(C) circuit is shown as an open loop since it is usually pumped from a large, constant temperature reservoir.) In the usual application, the flow rates in both circuits are quite high and thus the fluids experience little temperature change in passing through the equipment--ie

$$T_{\text{fluid A, in}} \simeq T_{\text{fluid A, out}} \qquad (5)$$

and similarly for fluids B and C. From their past experience with heat exchangers of similar design, Marshall knows that the heat transfer behavior is essentially constant throughout the equipment. (In the language of the old heat transfer, we would say that the heat transfer coefficients h_A, h_B, and h_C are uniform throughout the equipment.
In the language of the new heat transfer, we would say that $q_A\{\Delta T_A\}$, $q_B\{\Delta T_B\}$, and $q_C\{\Delta T_C\}$ are uniform throughout the equipment.)

The Ace Electric Co. places an order for one each of the new heat exchangers. The equipment specification from Ace states that the Martherm B and Martherm C will be drawn from large reservoirs which are maintained at 250 F. The temperature of the Martherm A is to be uncontrolled--ie it will float in accordance with the value of $Q_{electric}$ and the performance of the heat exchanger--ie the steady-state temperature of the Martherm A will be whatever is required in order that

$$Q_{electric} = Q_{HX} \qquad (6)$$

in the steady-state. The specification further states that there is to be a high T_A interlock which will shut down the electrical equipment in the event T_A exceeds 510 F and that $Q_{electric}$ will not exceed 100 KW in either the A/B or the A/C system. The specification also requires a standby mode for each system. In this mode, the fluid B(C) pump is secured, the fluid A pump is kept on the line, and auxiliary heaters must be provided to maintain the electrical equipment and the fluid A circuit at 490 F. The equipment is to be placed in the standby mode each evening and returned to the normal mode the following morning.

PROBLEM INPUTS

1. The equipment specification described above.

2. The background information described above.

3. From a literature search, the designer/analysts find that the heat transfer behavior of Martherm A and B are well correlated by

$$N_{Nu} = .023\ (N_{Re})^{.8}(N_{Pr})^{.4} \qquad (7)$$

which they rewrite in the form h{system properties}. Evaluating these properties for Martherm A and B in the subject heat exchanger at the specification flow rates, the designer/analysts find that

$$h_A = 1200 \text{ B/hr ft}^2 \text{ F} \qquad (8)$$

$$h_B = 1700 \text{ " "} \qquad (9)$$

4. From a literature search, the designers find that Martherm C is a very strange fluid and its heat transfer behavior in the subject heat exchanger is as described in Figure 2 (page 10).

5. Mechanical considerations dictate that the heat transfer wall be at least 0.01 ft thick and that it be made of one of the following metals:

$k_{metal\ 1} = 1.1$ B/hr ft^2 F

$k_{metal\ 2} = 6.2$ " "

$k_{metal\ 3} = 23.7$ " "

$k_{metal\ 4} = 118$ " "

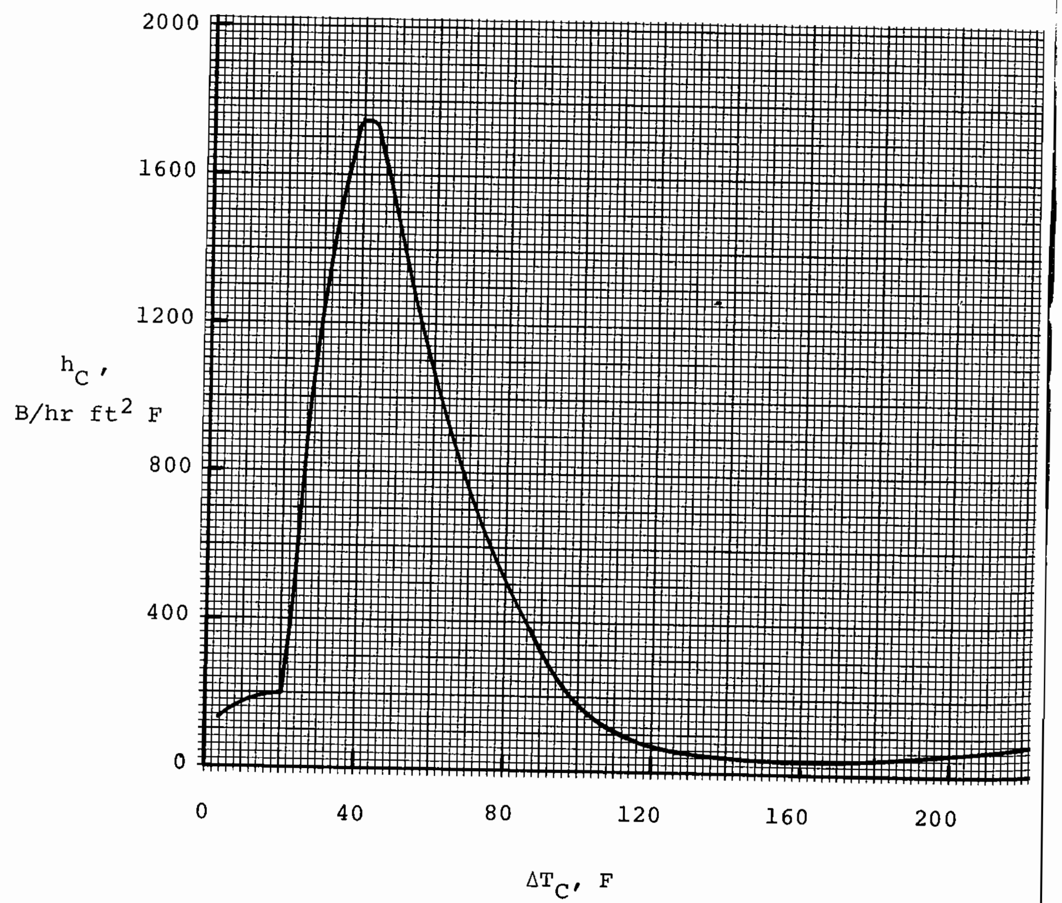


FIGURE 2

HEAT TRANSFER BEHAVIOR OF MARTHERM C--

OLD HEAT TRANSFER DESIGN CORRELATION

PROBLEM STATEMENT (real world)

Design (but do not overdesign) the two heat exchangers for the Ace order. Make certain that the heat exchangers will satisfy the equipment specification and also that they will work satisfactorily in the Ace application.

NOTE 1

At this point, I encourage the reader to forget all about the new heat transfer and make a stab at this real world problem using the old heat transfer--and without looking ahead in this chapter. You will have no difficulty designing a satisfactory A/B exchanger, but I doubt that any reader will come up with a satisfactory A/C design unless he first looks ahead to determine how it is done using the new heat transfer. (I have been doing this for the past ten years--solving problems using the new heat transfer and then perforce documenting them in the old heat transfer so that the solutions would be intelligible to others.)

NOTE 2

It is important to note that the heat transfer behavior underlying Figure 2 has immense practical significance. In a very real sense, this behavior underlies the prime mover of the nineteenth and twentieth centuries (and probably also the twenty-first). My point in mentioning this is to prevent the reader from concluding that the A/C exchanger problem has only academic interest and does not relate to the real world. We shall take up the behavior underlying Figure 2 in considerable detail in the later chapters.

PROBLEM 1 cont.

PROBLEM STATEMENT (textbook world)

Answer the following questions for the A/B exchanger and repeat for the A/C exchanger.

1. What is the minimum heat transfer surface area that will satisfy the Ace specification?

2. What value of T_A (in the range 250 to 510F) will result in the maximum possible value of q_{HX}?

3. Qualitatively describe the relationship between the thickness of the heat transfer wall and the maximum possible value of q_{HX}.

4. Are there any potential operating difficulties with the equipment? What are they?

5. How could this operating difficulty have been avoided by proper design?

6. If this operating difficulty were experienced in the field, what could be done to avoid or eliminate the difficulty?

NOTE

It will be noted that several of these questions are trivial with regard to the A/B exchanger since the correct answers are more or less obvious. However, the nonlinear interface in the A/C exchanger completely changes the nature of the problem and you will find that several of the obvious answers are the wrong answers! This is one of the major drawbacks of the old heat transfer--the obvious answer is often the wrong answer--and the right answer is virtually impossible to obtain whenever we are dealing with heat transfer which is highly nonlinear--and almost all heat transfer of practical importance is nonlinear!

If one insists on correct answers, he will find that they take about 100 times longer to obtain using the old heat transfer--and that the increased degree of difficulty so increases the likelihood of error that one might reasonably expect an unsatisfactory A/C design to result even if one were willing to spend an inordinate amount of time on its design.

PROBLEM 1 cont.

A/B HEAT EXCHANGER DESIGN AND ANALYSIS, OLD WAY
(answers to textbook world questions, pg 12)

1. The minimum heat transfer surface area will result from the highest possible U. U is given by

$$U = \frac{1}{1/h_A + 1/h_B + L/k} \qquad (10)$$

Eq 10 indicates that the maximum U will result from the minimum L and the maximum k. Therefore, using problem inputs 3 and 5, U_{max} is given by

$$U_{max} = \frac{1}{1/1200 + 1/1700 + .01/118} \qquad (11)$$

$$U_{max} = 664 \text{ B/hr ft}^2 \text{ F}$$

Now

$$Q = UA\Delta T \qquad (12)$$

$$A_{min} = \frac{Q_{spec}}{U_{max}\ \Delta T_{max}} \qquad (13)$$

$$A_{min} = 100\text{x}3413/664(510-250) = \underline{1.98 \text{ ft}^2}$$

2. Eq 13 indicates that the maximum possible value of q_{HX} will result from the highest possible value of T_A. In the range indicated, this would be <u>510 F</u>.

3. For a given material, the maximum possible value of q_{HX} is inversely related to the thickness of the heat transfer wall.

4. There are no potential operating difficulties with the equipment.

5. Not applicable.

6. Not applicable.

A/C HEAT EXCHANGER DESIGN AND ANALYSIS, OLD WAY
(answers to textbook world questions, pg 12)

I have not presented the old way solution to the A/C exchanger because I simply could not face the many hours of trial-and-error that would be involved--for nothing.

Those readers who still have faith in the old heat transfer should complete this part of Problem 1 by themselves. This will provide the best proof that it takes 100 to 1000 times longer to obtain and understand and analyse a satisfactory A/C design using the old heat transfer--if the reader can make the very long trip without drifting off course. I would emphasize that it can be done--a satisfactory A/C design can be obtained and analysed correctly using the old heat transfer--but it takes a great deal of time and a great effort to remain on course. (In the new heat transfer, the correct solution takes only a few minutes and involves only a very minor effort.)

Those readers who have no faith in the old or the new heat transfer should review Chapters 1 and 2.

Those readers who recognize that the old heat transfer is a thing of the past and will be replaced by the new heat transfer should omit this part of Problem 1 and should continue to the discussion of the transition period between the old and the new heat transfers.

THE TRANSITION PERIOD

Problem 2 illustrates the application of the new heat transfer to static problems in equipment design and analysis. It also illustrates how to deal with heat transfer during the transition period from old heat transfer to new heat transfer. (My estimate of the duration of this transition period would be ten to thirty years, based on similar transitions in the past.)

During the transition period, problem inputs from the literature will be obtained in the old heat transfer. Before any analysis is performed, these inputs must be transformed to the new heat transfer. For example, heat transfer correlations will for some time be reported in the literature in the old heat transfer form

$$N_{Nu} = f(N_{...})\, g(N_{...}) \qquad (14)$$

During the transition period, we will accept these old way correlations as the best available (until or unless new way correlations based on actual data are available). However, before any analysis is performed, we will transform these old way correlations to the useful form, $q\{\Delta T\}$.

Although this transformation of correlations from the old way to the new way is easily accomplished, it must be emphasized that there is a real danger that the researcher who generated the correlation simply did not recognize that h was a function of ΔT--and we should expect to find that many heat transfer phenomena reported in the literature have been correlated by assuming that h was independent of ΔT when in fact h was a strong function of ΔT. My point in mentioning this is that one cannot conclude that h is independent of ΔT simply because the researcher correlated the data by <u>assuming</u> that h was independent of ΔT, even if the correlation gave "satisfactory agreement". Only by obtaining and reviewing the data and comparing it with the transformed correlation will the designer/analyst be able to place his confidence in correlations obtained by researchers using the old heat transfer. The really disappointing part of this is that the comparison will seldom be possible because, in the old heat transfer, the data is not often published. (In the new heat transfer, the supporting data will always be published.)

REGIMES AND DIMENSIONAL ANALYSIS IN THE NEW HEAT TRANSFER--A PREVIEW

In the old heat transfer, it is common for researchers to divide nonlinear phenomena into so-called regimes of rather narrow breadth and to generate more or less linear correlations for each regime. As a result, what often happens is that the nonlinearity within the regime is not reflected in the correlation and consequently contributes to the scatter. Also, it is common to adjust the breadth of each regime not on the basis of a physical change in the phenomenon, but on the ability or inability to correlate the data with some preconceived model usually obtained by dimensional analysis.

The end result of this interplay between regimes and dimensional analysis is that oftentimes the breadth of the regime is little more than a reflection of the fact that the correlation derived by dimensional analysis simply did not agree with the data. And rather than reject the correlation because it did not agree with the data, the researcher simply restricted the breadth of the regime as required in order to obtain "satisfactory agreement" between the data and the correlation.

Or, if the breadth of the regime is already extremely narrow, the researcher may attribute the disagreement between data and dimensionless correlation to uncontrolled variables in the experiment. And conclude that these uncontrolled variables should be controlled and measured in some future experiment in order to obtain better agreement between the data and some more complex correlation obtained by additional dimensional analysis.

In summary, the invention of regimes and the attendant proliferation of correlations based on dimensional analysis largely prevents an understanding of nonlinear phenomena--and makes the effective design/analysis of equipment to handle nonlinear phenomena virtually impossible.

In Problem 2, the reader should notice that we handle the nonlinear behavior of Martherm C without inventing regimes and with nothing remotely resembling a correlation obtained by dimensional analysis. As we shall see in the later chapters, neither regimes nor dimensional analysis have much to do with the new heat transfer.

PROBLEM 2 THE DESIGN AND ANALYSIS OF HEAT TRANSFER COMPONENTS USING THE NEW HEAT TRANSFER

PROBLEM STATEMENT

Repeat Problem 1 using the new heat transfer.

TRANSFORMATION OF PROBLEM INPUTS FROM OLD HEAT TRANSFER TO NEW HEAT TRANSFER

1. Equation 7 is rewritten in the form

$$q = f(\text{system properties})\ \Delta T \qquad (15)$$

Evaluating these properties for Martherm A and B in the subject heat exchanger at the specification flow rates, the designer/analysts find that

$$q_A = 1200\ \Delta T_A \quad \text{B/hr ft}^2 \qquad (16)$$

$$q_B = 1700\ \Delta T_B \quad \text{"} \qquad (17)$$

2. The correlation $h_C\{\Delta T_C\}$ shown in Figure 2 (page 10) is transformed to the correlation $q_C\{\Delta T_C\}$ shown in Figure 3 (page 18).

3. The four metals are described by

$$q_{metal\ 1} = (1.1/L)(\Delta T_m) \quad \text{B/hr ft}^2 \qquad (18a)$$

$$q_{metal\ 2} = (6.2/L)(\Delta T_m) \quad \text{"} \qquad (18b)$$

$$q_{metal\ 3} = (23.7/L)(\Delta T_m) \quad \text{"} \qquad (18c)$$

$$q_{metal\ 4} = (118/L)(\Delta T_m) \quad \text{"} \qquad (18d)$$

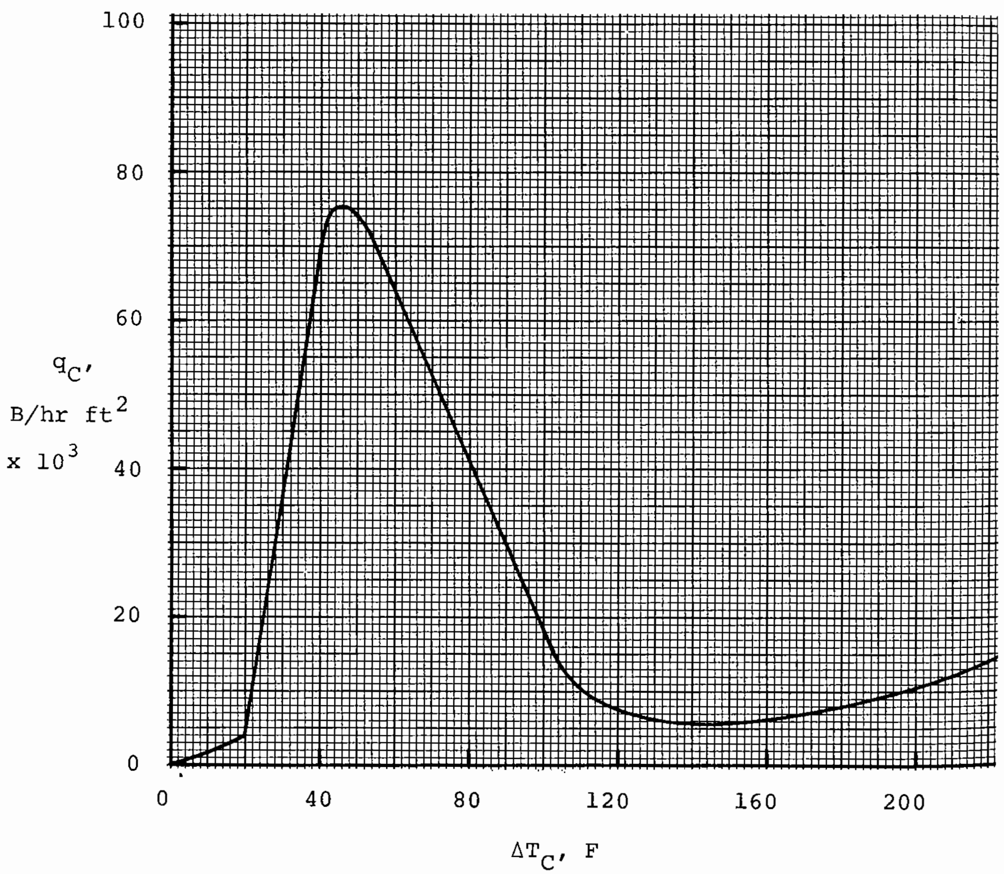


FIGURE 3

HEAT TRANSFER BEHAVIOR OF MARTHERM C--

NEW HEAT TRANSFER DESIGN CORRELATION

A/B HEAT EXCHANGER DESIGN AND ANALYSIS, NEW WAY
(answers to textbook world questions, pg 12)

1. Using the flow law from page 3 and also eq 4, inspection of Figure 1 indicates that

$$q_A = q_m = q_B = q \tag{19}$$

$$T_A - T_B = \Delta T_A + \Delta T_m + \Delta T_B = \Delta T_t \tag{20}$$

Equation 18 indicates that heat will flow most easily through the thinnest possible wall (.01 ft) of metal 4. For this case, combining eqs 16-20 yields

$$(q/1200) + (.01q/118) + (q/1700) = \Delta T_t \tag{21}$$

$$q = 664\ \Delta T_t \quad \text{B/hr ft}^2 \tag{22}$$

Now

$$Q_{HX} = q\ A_{HX} = Q_{spec} \tag{23}$$

Combining eqs 22 and 23,

$$A_{HX} = Q_{spec}/664\Delta T_t \quad \text{ft}^2 \tag{24}$$

Equation 24 indicates that the minimum area will result from the largest value of ΔT_t and this area is given by

$$A_{min} = (100)(3413)/(664)(260) = \underline{1.98\ \text{ft}^2} \tag{25}$$

2. Equation 22 indicates that the maximum possible value of q will result from the highest possible value of T_A. In the range indicated, this would be <u>510F</u>.

PROBLEM 2 cont.

3. For a given material, the maximum possible value of q is inversely related to the thickness of the heat transfer wall.

4. There are no potential operating difficulties with the equipment.

5. Not applicable.

6. Not applicable.

A/C HEAT EXCHANGER DESIGN AND ANALYSIS, NEW WAY
(answers to textbook world questions, pg 12)

1. Inspection of Figure 3 indicates that the maximum possible heat flux is 75,200 B/hr ft^2. Therefore, the minimum area which would satisfy the equipment specification is given by

$$A_{min} = Q_{spec}/q_{max} = 341300/75200 \qquad (26)$$

$$A_{min} = \underline{4.54 \text{ ft}^2}$$

2. The value of T_A which will result in the maximum possible q is whatever value will result in ΔT_C = 46F (since Figure 3 shows that this value of ΔT_C coincides with the maximum q). We can determine this value very simply as follows:

$$\Delta T_{t \text{ at max } q} = \Delta T_A + \Delta T_m + 46 \qquad (27)$$

Using eq 16, the above value of q_{max}, and assuming the same heat transfer wall used in the A/B exchanger,

$$\Delta T_{t \text{ at max } q} = 75200/1200 + 75200(.01/118) + 46$$

$$\text{"} \quad = 115 \text{ F}$$

$$T_{A \text{ at max } q} = 250 + 115 = \underline{365 \text{ F}}$$

PROBLEM 2 cont.

3. The thickness of the heat transfer wall could be increased considerably without affecting q_{max}. This increase would increase the value of $T_{A \text{ at max } q}$ but would not affect q_{max}.

4. There is a very serious operating difficulty with the above design. The difficulty is that the capacity of the heat exchanger decreases markedly as T_A increases above 370F. The end result is that, when the equipment is shifted from the standby mode to the normal mode (ie when T_A is about 490F), the heat exchanger can remove nowhere near 100 KW. Consequently, if the electrical equipment is operated anywhere near capacity shortly after shifting to the normal mode, the heat exchanger will be ineffective, the Fluid A circuit and the electrical equipment will heat up until the interlock is tripped at T_A = 510F, and then the electrical equipment will automatically shut down.

The difficulty may be best seen by determining the function $Q_{HX}\{T_A\}$ illustrated in Figure 4. (The determination of this function is quite similar to the analysis in 1. and 2. above.)

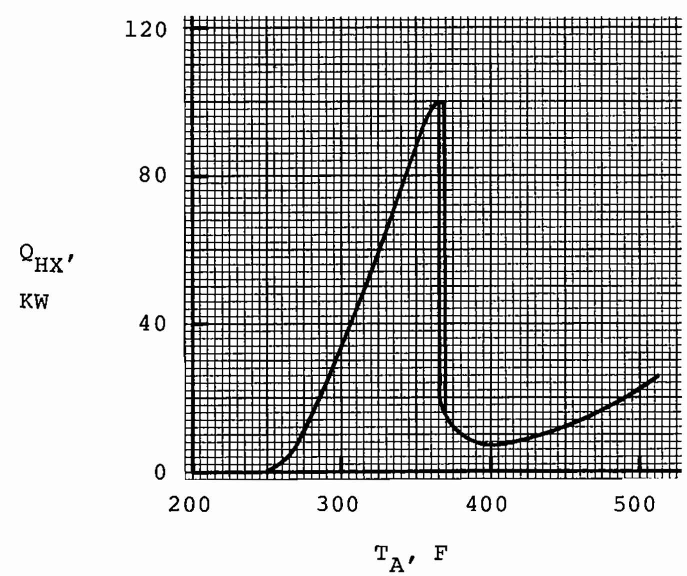


FIGURE 4

PROBLEM 2 cont.

Figure 4 shows that the A/C exchanger performs to specification if T_A IS BELOW 370 F and nowhere near specification if T_A is in the range 370-510F. In this latter range, the maximum capacity of the exchanger is only 25 KW! Figure 4 also shows that the exchanger can transfer only 8 KW if T_A = 400F. Thus, each morning when the operators shift from the standby mode to the normal mode, they are confronted with a system whose effective heat removal capacity is limited to 25 KW--and the only way they can increase the capacity is by lowering T_A below 370F--and the only way they can lower T_A below 370F is by restricting $Q_{electric}$ to some value below 8 KW (so that the heat removal through the exchanger will exceed the heat input from the electrical equipment, resulting in a negative heat storage, thus lowering T_A)--and it might take half the day to cool down the system this way--not to mention what this would do to Ace production schedules--or Ace management!

5. The operating difficulties could have been easily prevented by proper design of the heat transfer wall. The design intent would be to have the maximum heat flux (75200 B/hr ft^2) occur at T_A = 510F. Thus ΔT_C would equal 46F when ΔT_t = 260F and we may write

$$260 = \Delta T_A + \Delta T_m + 46 = 75200/1200 + \Delta T_m + 46$$

$$\Delta T_m = 151F$$

In order to obtain ΔT_m = 151F at q = 75200 B/hr ft^2, the required wall thickness for each of the four possible metals would be (using eq 18)

$$L_1 = 1.1x151/75200 = .00221 \text{ ft.}$$

$$L_2 = .01245 \text{ "}$$

$$L_3 = .0462 \text{ "}$$

$$L_4 = .237 \text{ "}$$

Metal 1 would be ruled out because it does not meet

PROBLEM 2 cont.

the minimum wall thickness requirement of .01 ft. The above thicknesses of metals 2, 3, and 4 would all work equally well and the final material selection would be based on cost/mechanical considerations. The improvement in equipment behavior which would result from any of the above wall thicknesses is best shown by determining $Q_{HX}\{T_A\}$ shown in Figure 5:

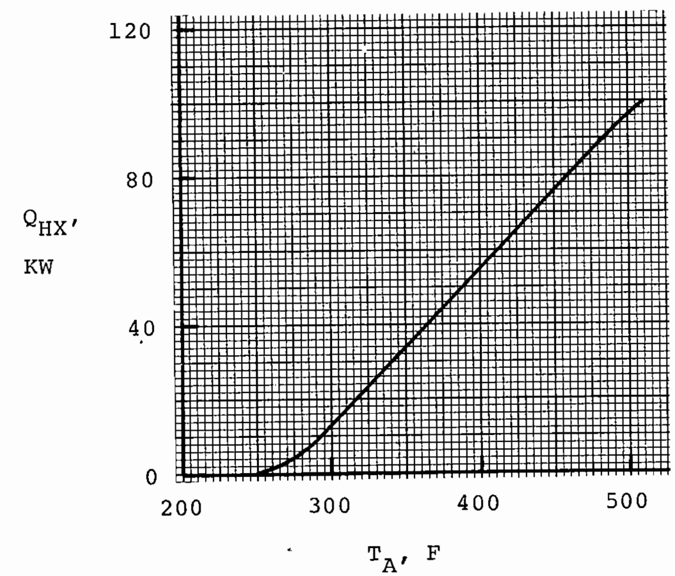


FIGURE 5

Thus, by simply selecting the right thickness of the right material, we have designed a heat exchanger which is highly satisfactory in place of one which is highly unsatisfactory--and with no increase in exchanger surface area or cost!

6. The field fixes are more or less obvious. The interested reader should be able to come up with at least three.

CONCLUSIONS

Problems 1 and 2 demonstrate that

1. The old heat transfer is an effective tool for design/analysis only when all heat transfer phenomena within the equipment are linear and pass through the point $q\{\Delta T=0\}=0$.

2. For all but the simple case cited in 1., the old heat transfer is an ineffective tool for the design/analysis of equipment. This ineffectiveness is the direct result of the invention of thermal resistance--ie of assigning importance to the ratio $q/\Delta T$--ie of inventing heat transfer coefficients.

3. The new heat transfer is an effective tool for the design/analysis of equipment to handle linear and/or nonlinear heat transfer phenomena. This effectiveness is the direct result of the fact that, in the new heat transfer, we do not invent thermal resistance--ie we assign no importance to the ratio $q/\Delta T$--ie we altogether avoid heat transfer coefficients.

4. The design/analysis of equipment to handle nonlinear heat transfer phenomena is orders of magnitude simpler using the new heat transfer. This simplification results in orders of magnitude improvement in the ability to optimize equipment design, to predict equipment behavior, to modify and improve equipment which is less than optimal, and to understand the behavior of real equipment.

It is extremely important to note that, although I have idealized the A/B and A/C exchangers in order to eliminate a myriad of inessential detail, both exchangers retain the essential features of real hardware. (Those who prefer a more elegant mathematical exposition--but one which is no more rigorous--may consider both exchangers as differential elements of less idealized equipment and may integrate the elemental results and so obtain the A/C design, the answers to the questions, and the counterparts to Figures 4 and 5.)

Although it has not been mentioned before, Problem 2

describes the application of the new heat transfer concept to equipment design/analysis in a very general way. Nothing in the analysis depends on whether we are dealing with liquids or gases, compressible or incompressible fluids, free or forced convection, one or two phases, Newtonian or non-Newtonian, boiling or condensing. The only real problem I have avoided is the problem of integration in equipment wherein the heat flow rate q is dependent on location. Since this integration faces us in both the old and the new heat transfers, I have not reviewed it here but have simply noted that integration will often be required when dealing with the design/analysis of real equipment.

It is also important to note that the nonlinear heat transfer behavior underlying Figures 2 and 3 is of great practical significance--the reader should not assume that I have invented this type behavior in order to put the new heat transfer in the best possible light. Had this been my intent, the problems would have dealt with a C/D exchanger where the behavior at both the C and D interfaces was nonlinear--or I would have invented a nonlinear behavior of considerably greater difficulty than that described by Figures 2 and 3.

With a great deal of patience and a great deal of time, it would be possible to deal with the A/C design/analysis problem in a successful way using the old heat transfer--but only because the problem was set up with a very simple form of nonlinear behavior. In the later chapters, we will deal with more difficult (but quite practical) problems dealing with nonlinear behavior. With regard to these more difficult problems, I doubt that anyone will object to the observation that their correct solution using the old heat transfer would be identically impossible.

Lastly, in this chapter I have offered (without proof) that the invention of regimes and dimensional analysis largely prevent an understanding of nonlinear phenomena and also prevent the effective design/analysis of equipment to handle nonlinear phenomena. By way of preview, I have pointed out that regimes and dimensional analysis have little to do with the new heat transfer.

THE TWENTY-FIRST CENTURY

Since the abandonment of thermal resistance results in orders of magnitude improvement in the science of heat transfer, one might well conclude that similar improvements await in other fields of engineering--perhaps from abandoning electrical resistance or the modulus of elasticity. While both of these concepts are unnecessary inventions which would better be abandoned, the resultant improvement would not parallel the improvement which results in heat transfer for two reasons:

1. In a practical and a contemporary sense, nonlinear behavior does not possess the pervasive importance in electricity or stress/strain that it does in heat transfer. Many electrical engineers work for years without dealing with nonlinear resistors. Many stress/strain engineers never deal with the plastic behavior of materials. On the other hand, the heat transfer engineer who is not frequently confronted with nonlinear thermal behavior is rare indeed.

2. The sciences of electricity and stress/strain presently deal quite effectively with dynamics and stability whereas the old heat transfer has still to come to grips with these important subjects. In both of these other sciences, it has long been recognized that linear concepts cannot be effectively applied to nonlinear phenomena unless they are first modified--thus the "dynamic electrical resistance" and the "tangent modulus". These have no parallel in the old heat transfer.

In the new heat transfer, we go a step beyond the mathematical treatment of dynamics and stability in electricity and stress/strain--ie we do not deal with nonlinear behavior using a modified linear concept. Instead, we altogether discard the linear concept and replace it with a concept which effectively deals with both linear and nonlinear behavior--and in the process obtain orders of magnitude improvement in our ability to deal with heat transfer in both a theoretical and a practical sense.

Although the abandonment of electrical resistance and the modulus of elasticity will not bring as great improvement as the abandonment of thermal resistance, I find it hard to believe we will carry these and other purely linear concepts into the twenty-first century.

SYMBOLS

A	area, ft^2
f	indicates function
g	" "
h	heat transfer coefficient, B/hr ft^2 F
h_A	heat transfer coefficient across the Martherm A interface, B/hr ft^2 F
k	thermal conductivity, B/hr ft F
L	thickness of heat transfer wall, ft (see Figure 1)
N	dimensionless number
$N_{...}$	unspecified dimensionless number
q	heat flow rate per unit area, B/hr ft^2
$q_A\{\Delta T_A\}$	refers to heat flow behavior across the Martherm A interface; in the new heat transfer, this expression replaces h_A; B/hr ft^2
Q	heat flow rate, B/hr or KW
T	temperature, F
T_A	bulk temperature of Martherm A, F
ΔT_A	temperature difference across the Martherm A interface, F
U	heat transfer coefficient in an overall sense, F

SUBSCRIPTS

A	refers to Martherm A fluid
B	" " B "
C	" " C "

SUBSCRIPTS cont.

HX	refers to heat exchanger
i	refers to ith value
max	refers to maximum value
min	refers to minimum value
Pr	refers to Prandtl
Re	refers to Reynolds
spec	refers to specification
t	refers to total

REFERENCES

I have not documented my descriptions of the methods used in the old heat transfer because I did not feel it was necessary. Those readers who are inclined to doubt that, in the old heat transfer, heat transfer coefficients are used to design/analyse equipment--or that an analogy is usually drawn between electrical resistance and thermal resistance--or that thermal resistance circuits are used as an aid in the analysis of linear and nonlinear thermal behavior--or that nonlinear phenomena are usually divided into narrow regimes--or that dimensional analysis has a great deal to do with the old heat transfer--should refer to any of the excellent textbooks or handbooks currently on the market--or to the latest scientific literature on heat transfer.

I have not documented the new heat transfer methods described here because they are largely without precedent. However, if the reader will study the two articles I had published almost ten years ago, I believe he will recognize that these methods are the application of my earlier theoretical and conceptual work. Those readers who wonder why I have waited almost ten years to publish the application of my theoretical and conceptual work will find the answer between the lines.

INTRODUCTION TO CHAPTER 4, DYNAMICS AND STABILITY

A reasonable definition of "equipment dynamics and stability" would seem to be the following:

> Equipment dynamics deals with the manner in which equipment behaves in the vicinity of potential steady-state operating points. In particular, it concerns the manner in which the equipment responds to changes in system parameters, the functional relationships among the system parameters which describe the system response, and the question of equipment stability--ie the ability of the equipment to resist perturbations from potential steady-state operating points and thus return to the unperturbed condition.

Although "dynamics" of course suggests transient behavior, we will often be concerned with dynamic behavior in a steady-state sense. For example, given a heat exchanger operating at steady-state with unspecified Q_{ss} and ΔT_t, a problem in dynamics would be

> How would the steady-state heat load respond to a decrease of 3F in ΔT_t? In other words, what change in steady-state heat load would result from a change of -3F in the value of ΔT_t? In other words, what value of ΔQ_{ss} would result from $\Delta\Delta T_t = -3F$?

Thus we are often concerned with dynamic behavior which can be inferred from a knowledge of static behavior. In such cases, we need not concern ourselves with the transient characteristics of the system such as inertia or capacitance and of course the analysis is greatly simplified.

In this chapter, we deal only with dynamic behavior which can be inferred from purely static behavior and we largely ignore truly transient effects. However, as we shall see in the later chapters, virtually all practical problems in equipment dynamics and stability can be solved with this greatly simplified type of dynamic analysis.

MATHEMATICAL CONCEPTS REQUIRED FOR DYNAMIC ANALYSIS

Unfortunately, the words "dynamics" and "stability" generally conjure up visions of high level mathematics--of complex transformations and other sophisticated mathematical tools which largely prevent an understanding of equipment behavior. Throughout this book, we avoid high level mathematics--we deal with dynamics and stability in a very simple way, being careful to use only elementary mathematical tools which will promote rather than prevent an understanding of equipment behavior.

In the new heat transfer, our primary aim is to deal effectively and in a practical way with equipment dynamics and stability--and, in order to promote the highest level of understanding, to deal with them in the simplest possible way. For example, the prediction of the period and amplitude of oscillations in unstable systems is a rather complex problem which we will generally deal with in only a qualitative way because it is seldom a practical matter. The lack of practicality of course arises from the fact that unstable equipment is seldom acceptable. The practical problem generally concerns the causes of instability and the manner in which these causes and the resultant oscillations can be avoided or, after the fact, eliminated.

Thus, in the new heat transfer, equipment dynamics deals with the causes of instability in a quantitative way and with the effects of instability in a qualitative way. We recognize that the quantitative prediction of the behavior of unstable equipment is a subject of only secondary import and we assign it to a less practical and more sophisticated later study in purely transient behavior. In the new heat transfer, we are quantitatively concerned with parametric stability criteria (ie equations which define the limits of stable operation in terms of equipment design parameters) and only qualitatively concerned with the behavior which results from violating these criteria.

In the remainder of this book, we deal with the dynamic behavior of real equipment using only the following mathematical concepts:

1. The concept of functionality--

$$y = y\{x\} \qquad (1)$$

2. The concept of the derivative--

$$\frac{dy}{dx} = \frac{\Delta y}{\Delta x} \quad \text{as } \Delta \to 0 \qquad (2)$$

3. The concept that derivatives can be approximated by small but finite increments--

$$\frac{dy}{dx} \simeq \frac{\Delta y}{\Delta x} \qquad (3)$$

$$\Delta y \simeq \frac{dy}{dx}\,\Delta x \qquad (4)$$

(Also called linearization.)

We shall find that these simple mathematical concepts are quite adequate to deal effectively and in a practical way with equipment dynamics and stability in the new heat transfer.

OLD WAY VS NEW WAY

The old heat transfer* simply does not come to grips with dynamics in general or stability in particular. Perhaps the best way to document this is to refer to the most modern textbooks and handbooks on heat transfer and search for a discussion of the stability of the heat transfer process--"thermal stability". The reader who performs this search will not find thermal stability quantitatively discussed in even the most modern texts or handbooks on heat transfer--nor is it quantitatively discussed anywhere else in the old heat transfer under any other generic name. An understanding of dynamics and thermal stability simply is not within the "knowledge envelope" of the old heat transfer.

* Again I would emphasize that "the old heat transfer" does not include my two articles published nearly ten years ago or the several Journal articles which have since followed their lead.

In my article on thermal stability (1), which of course was a page taken from the new heat transfer, I assigned the generic term "thermal stability" to the stability of the heat transfer process and derived the generic criterion for this type of stability. That article was described as a "hoax" by a group I have come to call

The Argonne Seven

because the group consisted of seven employees of The Argonne National Laboratory. The Argonne Seven drew up a petition in which they complained that my article

"must be a hoax"

and sent their petition to the editor of the periodical in which the article had appeared. I mention this here because The Argonne Seven were and are recognized authorities on the old heat transfer. Thus their violent and negative reaction to my article on thermal stability was actually eloquent testimony to the fact that the old heat transfer does not and probably cannot and certainly will not appreciate equipment dynamics and thermal stability.

The closest the old heat transfer comes to the subject of dynamics and thermal stability is the "intuitive" and qualitative proof often presented in relation to pool boiler operation in the so-called "transition boiling" regime. In this regime, the heat flow decreases as the ΔT increases--ie $q_i\{\Delta T_i\}$ is such that

$$\frac{dq_i}{d\Delta T_i} < 0 \qquad (5)$$

An intuitive proof is usually presented to describe why pool boilers employing an electrical heat source cannot operate in this transition regime whereas pool boilers employing a steam heat source can operate throughout this regime. We will not pursue this intuitive proof here except to note that it leads to results and conclusions which are largely incorrect.

In the new heat transfer, we deal with equipment dynamics and thermal stability in a rigorous, quantitative way using elementary mathematical concepts. And the solution of practical design/analysis problems in dynamics and thermal stability presents no particular difficulty.

DYNAMICS AND THE OLD HEAT TRANSFER

It is possible to apply the old heat transfer to a quantitative study of equipment dynamics. However, this application involves a great deal of extra work, a greatly decreased accuracy, and a greater likelihood of error. The difficulty with the old heat transfer is well illustrated in the following examples.

EXAMPLE 1

Suppose we are given that a 7.3 ft^2 heat exchanger exhibits the following heat transfer behavior at the design point:

$$U = 185 \text{ B/hr ft}^2 \text{ F}$$

$$\Delta T_t = 140 \text{ F}$$

The problem in dynamics is: How much additional heat load will the heat exchanger be able to handle if we increase the design ΔT_t by 5 F?

Using eq 4, we may write

$$\Delta Q \simeq \frac{dQ}{d\Delta T_t}(\Delta\Delta T_t) \qquad (6)$$

and, since we are given that $\Delta\Delta T_t$ = 5 F, the problem reduces to determining the derivative in eq 6. Now in the old heat transfer,

$$Q = UA\Delta T_t \qquad (7)$$

and, given eq 7, one is certainly inclined to write

$$\frac{dQ}{d\Delta T_t} = UA \qquad (8)$$

Thus the answer to the question would certainly seem to be (combining eqs 6 and 8 and the design information)

$$\Delta Q \simeq UA\Delta\Delta T_t = 185\text{x}7.3\text{x}5 = \underline{6753 \text{ B/hr}} \qquad (9)$$

Now let us see if we can obtain this same answer using the new heat transfer. In the language of the new heat transfer, this same design point would be described by

$$q\{\Delta T_t=140\} = 25900 \text{ B/hr ft}^2$$

and, as before, the problem is to determine the derivative in eq 6. In the new heat transfer,

$$Q = q\{\Delta T_t\}A \qquad (10)$$

replaces eq 7. Differentiation of eq 10 gives

$$\frac{dQ}{d\Delta T_t} = A\,\frac{dq}{d\Delta T_t} \qquad (11)$$

It is evident from eq 11 that we cannot determine the value of $dQ/d\Delta T_t$ because we have no information about the function $q\{\Delta T_t\}$ and thus cannot determine the value of $dq/d\Delta T_t$. The design information gave us one point on the function, but we obviously cannot determine the derivative of the function from the coordinates of a single point. Thus the correct reply to the question of additional heat load is that the answer is indeterminate from the information given. An increase of 5F in ΔT_t could result in an increased Q, a decreased Q, or an unchanged Q--we simply do not have the information required to quantitatively or even qualitatively answer the question.

Of course it must be possible to obtain the correct answer using both heat transfers. The fact that we obtained two different answers to the same question indicates that we made a mistake in applying either the old or the new heat transfer. Reviewing the old heat transfer solution, it can be seen that we made an unwarranted assumption in going from eq 7 to eq 8. We assumed that U was independent of ΔT_t--an understandable error in view of the way eq 7 is written. Eq 7 certainly leads us to believe that both A and U are independent of ΔT_t. (One does not often see eq 7 written in the form

$$Q = U\{\Delta T\}A\Delta T \qquad (12)$$

no matter how strong the functionality between U and ΔT!) And this is another shortcoming of the old heat transfer--it seems to suggest that U is independent of ΔT and causes us to mentally separate U and ΔT. We tend to think in terms of U--in terms of heat flow which is proportional to ΔT. Thus to increase heat flow, we increase ΔT--and expect Q to increase roughly in proportion to the increase in ΔT. The difficulty with this old heat transfer type of reasoning is that, when we are dealing with nonlinear heat transfer behavior (as we often are), it very readily leads to the wrong answer!

To correctly apply the old heat transfer to this simple problem in dynamics, we use eq 12, from which we write

$$\frac{dQ}{d\Delta T_t} = A\left(U + \Delta T\,\frac{dU}{d\Delta T_t}\right) \qquad (13)$$

From eq 13, it is obvious that we cannot determine the value of $dQ/d\Delta T_t$ because we have no information about the function $U\{\Delta T\}$ and thus cannot determine the value of $dU/d\Delta T_t$. Therefore the correct reply to the question of additional heat load is that the answer is indeterminate from the information given. (It should be noted that we have obtained the correct answer using the old heat transfer, but that it was extremely easy to obtain the wrong answer using the old heat transfer.)

EXAMPLE 2

Returning to Problem 3 in Chapter 2, suppose the problem is to determine the change in surface temperature which would result from an increase in the surface heat flux from an initial value of 70.0 to a final value of 71.2 B/hr ft². Now

$$T_s\{q=71.2\} - T_s\{q=70\} = \Delta T\{q=71.2\} - \Delta T\{q=70\} \qquad (14)$$

$$\text{"} \qquad \text{"} \qquad = \Delta\Delta T$$

Using eq 4,

$$\Delta\Delta T \simeq \frac{d\Delta T}{dq}(\Delta q) \qquad (15)$$

Since

$$\Delta q = 71.2 - 70.0 = 1.2 \text{ B/hr ft}^2$$

the problem reduces to the determination of $d\Delta T/dq$. Using the old heat transfer,

$$\Delta T = q/h \tag{16}$$

$$\frac{d\Delta T}{dq} = (h - q\frac{dh}{dq})(h)^{-2} \tag{17}$$

In the subject problem, $h = 0.745$ B/hr ft^2 F at $q = 70.0$ B/hr ft^2. Using these values and combining eqs 15 and 17,

$$\Delta\Delta T \simeq (.745 - 70\frac{dh}{dq})(.745)^{-2}(1.2) \tag{18}$$

which still leaves us with the problem of determining the value of dh/dq. The determination of dh/dq requires the following tedious and time consuming operations:

1. Transform the $h\{\Delta T\}$points on Figure 5 to the form $h\{q\}$
2. Plot the transformed points on a graph of h vs q and fit a curve through the points .
3. Measure the slope of the curve at $q =$ 70 B/hr ft^2; this slope is the desired value of dh/dq at $q = 70$ B/hr ft^2

Having done all this, we may now substitute the value obtained for dh/dq in eq 18 and obtain $\Delta\Delta T$ which, by eq 14, is also the desired change in surface temperature. If the reader would like to work through all this, I believe it will take you about thirty minutes to solve this simple problem in dynamics. I believe you would also agree that there is a strong likelihood of error compared to the solution of the same problem using the new heat transfer.

Now let us solve the same simple problem using the new heat transfer. As before, the problem is to determine the value of $d\Delta T/dq$ at $q = 70$ B/hr ft^2. From Fig 6, Chapter 2, measure the slope of the curve at $q =$ 70 B/hr ft^2 and obtain

$$\frac{d\Delta T}{dq} = +0.93 \text{ F/B/hr ft}^2 \text{ at } q = 70 \text{ B/hr ft}^2 \tag{19}$$

From eqs 15 and 19,

$$\Delta\Delta T = 0.93(+1.2) = +1.12 \text{ F} \tag{20}$$

From eqs 14 and 20, the answer to the problem is that the surface temperature would increase 1.12 F if the surface heat flux increased from 70.0 to 71.2 B/hr ft^2.
Estimated working time: 1 minute
Likelihood of error: small

In summary, the first example shows that it is quite easy to go astray mentally and actually using the old heat transfer. The second problem demonstrates that the proper application of the old heat transfer to simple problems in dynamics is many times more time consuming, more difficult, and more likely to introduce errors than the proper application of the new heat transfer. Though it has not been mentioned, it should be noted that the accuracy of the answer would be considerably poorer using the old heat transfer because of the numerous steps which are required in the old heat transfer and are unnecessary in the new heat transfer.

DYNAMICS AND THE MATHEMATICS OF THE OLD HEAT TRANSFER

When dealing with dynamic problems in heat transfer equipment, we will often be concerned with the manner in which the heat flow responds to the temperature difference--ie we will often want to determine the value of $dq/d\Delta T$. If we were to build a science of heat transfer dynamics on the old heat transfer, let us take a close look at how we would then determine this important derivative:

1. An experiment is performed in which q and ΔT are measured--ie the function $q\{\Delta T\}$ is determined empirically by researchers

2. The researchers transform the data from $q\{\Delta T\}$ to the form $h\{\Delta T\}$

3. The researchers correlate the data in the form $h\{\Delta T\}$

4. The designer/analyst uses the correlation to determine $dq/d\Delta T$ by noting that

$$q = h\Delta T \tag{21}$$

and therefore that

$$\frac{dq}{d\Delta T} = h + \Delta T\frac{dh}{d\Delta T} \tag{22}$$

It is of course the rather absurd form of eq 22 which makes the old heat transfer so unsuitable for a study of dynamics in general and stability in particular.

In the new heat transfer, the determination of $dq/d\Delta T$ is much simpler and much more direct. The researchers correlate the data <u>without</u> transformation--ie they correlate the data <u>in the form</u> $q\{\Delta T\}$, the same form in which the data were obtained. Thus the designer has merely to differentiate this $q\{\Delta T\}$ correlation in order to determine the value of $dq/d\Delta T$. And the new heat transfer parallel to eq 22 is the obvious expression

$$\frac{dq}{d\Delta T} = \frac{d(q)}{d\Delta T}. \tag{23}$$

The poor mathematical basis of the old heat transfer is perhaps better noted by rewriting eq 22 in the more revealing form

$$\frac{dq}{d\Delta T} = \frac{q}{\Delta T} + \Delta T\,\frac{d(q/\Delta T)}{d\Delta T} \tag{24}$$

which of course is a correct but obviously very poor way to determine $dq/d\Delta T$. (The proof that eq 24 is an identity is an exercise worth performing.) The difficulty with eq 22 is even better illustrated by noting that eq 22 is just like writing

$$\frac{dy}{dx} = \frac{y}{x} + x\frac{d(y/x)}{dx} \tag{25}$$

which is also an identity, but one which no one would think of using to evaluate dy/dx. For example, given that

$$y = 3x^2 + 2x + 4$$

the determination of dy/dx utilizing eq 25 gives

$$\frac{dy}{dx} = \frac{3x^2 + 2x + 4}{x} + x\,\frac{d((3x^2 + 2x + 4)/x)}{dx} \tag{26}$$

which, after correct manipulation, does give the correct answer. Thus we <u>can</u> correctly evaluate the derivatives of functions using equations like 22 and 25. But this old heat transfer method of evaluating derivatives is an order of magnitude more trouble than the new heat transfer method of simply noting that

$$\frac{dy}{dx} = \frac{d(3x^2 + 2x + 4)}{dx} = 6x + 2 \tag{27}$$

which of course has been the conventional method of evaluating derivatives for many many decades.

In summary, the poor mathematical foundation of the old heat transfer causes us to determine the important derivative $dq/d\Delta T$ in the indirect and cumbersome manner of eqs 22 and 25 and thus provides a poor basis for setting up a science of heat transfer dynamics. The mathematical foundation of the new heat transfer is such that it permits the determination of the important derivative $dq/d\Delta T$ in the direct and mathematically conventional method of eqs 23 and 27 and thus provides a sound basis for a science of heat transfer dynamics. It is self evident that the indirect method of the old heat transfer is conducive to error and, even when correctly applied, leads to results which are necessarily less accurate because of the extraneous input and analysis which it requires.

THE CONCEPT OF STABILITY

The concept of stability is best described using a simple example. Suppose we have a water system which includes a large, open tank which acts as a surge tank for the system. Water is continually flowing into and out of the tank as suggested in Figure 1:

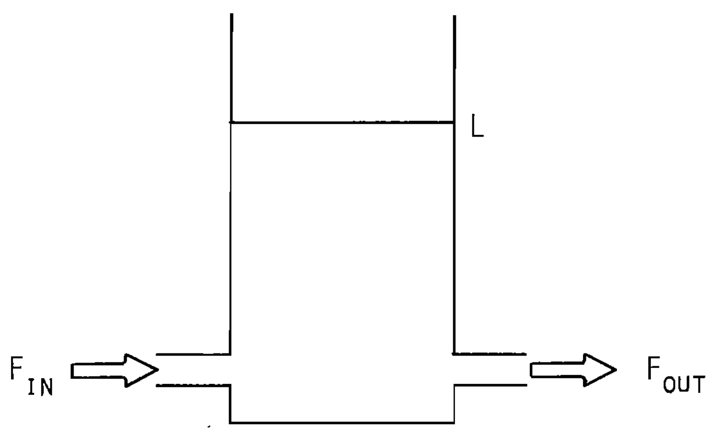


FIGURE 1

Further suppose that the level in the tank is at 74 inches and that, at this level,

$$F_{in} = F_{out} = 100 \text{ gpm at } L = 74"$$

Thus the system is at steady-state. Now suppose a workman passes by this steady-state system and, noticing the open tank, empties a bucket of water into it, causing the level to increase to 75 inches. The stability problem is:

Will the level tend to return to 74 inches?
Or will it tend to remain at 75 inches?
Or will it tend to increase even above 75 inches?

In other words, can the system resist this temporary, outside influence and return to the unperturbed condition represented by L = 74" and F = 100 gpm? If it can resist and return, we will conclude that the equipment is stable with regard to small changes in L at L = 74" and F = 100 gpm. If the system cannot resist and return, we will conclude that the equipment is unstable with regard to small perturbations in L at L = 74", F = 100 gpm.

In order to answer this stability question, we must know how tank level affects F_{in} and F_{out}--ie we must know something about

$$F_{in}\{L\} \quad \text{and} \quad F_{out}\{L\}$$

or something about

$$\Delta F_{in}\{\Delta L\} \quad \text{and} \quad \Delta F_{out}\{\Delta L\}$$

(If we work with the F functions, we will really be performing static analysis. If we work with the ΔF functions, we will be performing dynamic analysis.) Now suppose this system is such that

$$F_{in}\{L=75\} = 99 \text{ gpm} \qquad F_{out}\{L=75\} = 97 \text{ gpm}$$

Therefore, after the workman throws in the bucket of water,

$$F_{in} - F_{out} = 99 - 97 = +2 \text{ gpm} \tag{28}$$

and thus, since the storage rate in the tank is positive, the tank level would tend to increase above 75 inches. Since this direction is <u>away from</u> the unperturbed condition of L = 74", we conclude that the system does not resist small perturbations at L = 74", F = 100 gpm, and thus the system is unstable at this condition. (We cannot state that the system is unstable and let it go at that because the system might very well be stable at L = 64" and at L = 82". The information given is sufficient only to state that the equipment is unstable <u>at</u> L = 74", F = 100 gpm.)

Had we been given information about ΔF_{in} and ΔF_{out}, we would first note that, before the workman came along,

$$F_{in} - F_{out} = F_{sto} = 0 \tag{29}$$

(since we were given that $F_{in} = F_{out}$ before the workman came along). Eq 29 allows us to write

$$\Delta F_{in} - \Delta F_{out} = \Delta F_{sto} = F_{sto} \tag{30}$$

for the perturbed condition where L = 75". Now in this case, we are given only that $F_{in} = F_{out}$ prior to the workman--ie we are not given the value of F_{in} or F_{out}--and we must solve the problem using dynamic analysis. We are given that

$$\Delta F_{in}\{\Delta L=1\} = -1 \text{ gpm} \qquad \Delta F_{out}\{\Delta L=1\} = -3 \text{ gpm}$$

Therefore, using eq 30,

$$F_{sto} = (-1) - (-3) = +2 \text{ gpm} \tag{31}$$

and, in the same manner as before, the positive storage rate in the tank indicates that the equipment is unstable at L = 74" and whatever F{L=74} happens to be.

In the above example, we have determined the stability of the given equipment--ie we have determined the ability of the equipment to <u>resist</u> a temporary, outside influence--but it must be emphasized that we have determined the stability only at one potential steady-state operating condition. Therefore we cannot conclude that the equipment is unstable in a general way--we can conclude that it is unstable <u>only</u> at the condition we have analysed--it may be stable at virtually all other conditions or it may be unstable at all other conditions. We do not have sufficient information to describe the stability of the equipment anywhere but in the vicinity of L = 74".

We also do not have sufficient information to describe the transient behavior of the above system in the vicinity of L = 74"--we know it is unstable, but we do not know how this unstable system would behave--we know only that the level will not remain at 74" the way it would in a stable system. However, there are really only two broad alternatives--either the level in the tank would oscillate and never damp out, or the level would find some other condition where $F_{in} = F_{out}$ and where $\Delta F_{in}\{\Delta L\}$ and $\Delta F_{out}\{\Delta L\}$ were such as to permit stable operation.

THERMAL STABILITY

Thermal stability refers to the ability of heat transfer equipment to resist perturbations which tend to upset the flow of heat within the equipment. These perturbations may be thought of as temporary, outside influences, but it should be recognized that most processes contain their own perturbations in the microscopic randomness which is generally present--for instance, turbulence in fluid flow and the generation and collapse of bubbles in boiling. Thus thermally stable equipment must possess the ability to resist external and internal influences which tend to upset or disrupt the flow of heat within the equipment.

Internally generated perturbations are generally small in magnitude, but externally generated perturbations may be small or large. Fortunately, there is little practical need to consider large perturbations and so we will deal almost exclusively with small perturbations. Throughout this book, the word "stable" will imply "stable with regard to a vanishingly small perturbation".

To illustrate the concept of thermal stability with an example, suppose a heat exchanger is part of a steady-state system. In this particular heat exchanger, the temperature drop through the heat transfer wall is negligible and thus we can characterize the temperature distribution in the wall with a single temperature T_w = 370F. Suddenly some external influence causes a step increase of 0.3 F in the temperature of the heat transfer wall. The question with regard to thermal stability is:

> Will the temperature of the heat transfer wall tend to return to 370.0 F?
> Or will it tend to remain at 370.3?
> Or will it tend to increase above 370.3?

If the temperature of the wall tends to return to 370.0 F, the equipment exhibits thermal stability. Otherwise the equipment lacks thermal stability and is said to be thermally unstable. The thermal stability problem now becomes

> How does one determine whether the perturbed temperature of the heat transfer wall will tend to return to its initial value?

The answer is that we must determine how the small increase in wall temperature affected the heat flow rate into the wall and the heat flow rate out of the wall. When we have done this, we can compare the magnitude of these two effects and thus determine the sign of the heat storage in the wall. If this sign turns out to be positive, then the wall temperature would increase and we will conclude that the equipment is thermally unstable at the potential steady-state condition being considered. If the sign turns out to be negative, then the wall temperature will decrease toward the unperturbed value and we will conclude that the equipment is thermally stable at the steady-state condition being considered. If the change in heat inflow exactly equals the change in heat outflow, we will conclude that the equipment is thermally unstable (since it does not resist the perturbation) at the steady-state condition being considered.

Symbolically, we determine q_{sto} from

$$q_{sto} = \Delta q_{in} - \Delta q_{out} \tag{32}$$

which we obtain by reasoning identical to that used to derive eq 30. If $q_{sto} > 0$, the wall heats up; if $q_{sto} < 0$, the wall cools down. Thus the criterion for thermal stability can be expressed as

$$\Delta q_{in} < \Delta q_{out} \quad \text{for thermal stability} \tag{33}$$

in order that the wall will tend to cool down if some temporary influence should cause a small increase in wall temperature. (And conversely if some influence should cause a small decrease in wall temperature.)

In the later chapters where we deal with thermal stability in a highly quantitative way, we will not think in terms of a finite disturbance ΔT resulting in a finite Δq_{in} and finite Δq_{out}. Instead, we will deal only with the derivatives dq_{in}/dT and dq_{out}/dT, recognizing that

$$\Delta q_{in} \simeq \Delta T \frac{dq_{in}}{dT} \qquad \Delta q_{out} \simeq \Delta T \frac{dq_{out}}{dT} \tag{34}$$

Combining eqs 33 and 34 allows us to write the thermal

stability criterion in its most useful generic form:

$$\frac{dq_{in}}{dT} < \frac{dq_{out}}{dT} \tag{35}$$

In the above analysis, we have actually shown that eq 35 is necessary for thermal stability, but we have not yet shown that it is sufficient for thermal stability. In other words, we have shown that the equipment is definitely unstable if eq 35 is not satisfied, but only that the equipment may be stable if eq 35 is satisfied. However, as we shall see in a later chapter, eq 35 is both necessary and sufficient in a practical sense.

The above is an introductory description of the theory of thermal stability and an elementary--but rigorous--derivation of the generic criterion for thermal stability which we have shown to be necessary. If the generic criterion is not satisfied, the equipment will not operate in a stable manner--ie it will not resist small, temporary influences which tend to upset the flow of heat within the equipment. The end result of thermal instability is that the equipment will operate in one of two generally unsatisfactory ways:

> In an oscillatory manner--the temperatures and heat flows within the equipment fluctuate and never damp out.
>
> In a manner which exhibits pronounced hysteresis--an increasing heat source temperature results in considerably different behavior than a decreasing heat source temperature--there are "forbidden zones" in the equipment characteristics.

DERIVING PARAMETRIC CRITERIA FOR THERMAL STABILITY

With regard to stable performance, the design optimization of real equipment is most readily accomplished by deriving and applying parametric stability criteria--ie equations which define the limits of stable behavior in terms of equipment design parameters. The generic criterion for thermal stability (eq 35) is in a useful form for a discussion of thermal stability in a theoretical way, but in this form it is not very useful for equipment design. For equipment design, we must convert eq 35 to equations which describe in a quantitative way how the equipment design parameters together determine the thermal stability or instability of the equipment. In other words, we must express dq_{in}/dT and dq_{out}/dT in terms of designer controlled parameters such as heat transfer wall thickness and thermal conductivity, heat transfer behavior at wall/fluid interfaces, temperature coefficient of electrical resistivity if applicable, and the myriad of other parameters which can and do influence the magnitude of these derivatives in real heat transfer equipment.

The manner in which parametric criteria for thermal stability are derived from the generic criterion is best described by an example.

Suppose we are designing a heat exchanger like the one shown in Figure 1, Chapter 3, and we are given that the behavior at the wall/B interface is likely to be highly nonlinear. As designers, we would of course like to design a stable heat exchanger and thus we want to optimize those design parameters which influence thermal stability. Now stability requires that the generic criterion (eq 35) be satisfied everywhere in the heat exchanger from the heat source (Fluid A) to the heat sink (Fluid B). However, the location where it is most likely to be violated is at the nonlinear interface which, in this case, is the wall/B interface. Thus T in eq 35 refers to the wall/B (wB) interface and dq_{in}/dT refers to the manner in which the heat flow <u>into</u> this interface responds to the temperature of this interface. Symbolically,

$$\frac{dq_{in}}{dT} = \frac{dq_{in}}{dT_{wB}} \tag{36}$$

In a mathematical sense, we uncouple the heat exchanger at the wB interface and determine how the heat flow from Fluid A to the wB interface responds to the temperature of the wB interface. We do not concern ourselves with where the heat would go once it got to the interface. We recognize that this uncoupling is merely a mathematical artifice and the fact that we cannot duplicate it physically

is of no consequence. Our only concern is the determination of the function $q_{in}\{T_{wB}\}$. Once we have determined this function, we have then only to differentiate it in order to determine dq_{in}/dT in eq 35. Given dq_{in}/dT_{wB}, the stability optimization of the equipment requires merely that the design intent be to make this derivative negative and large (as suggested by eq 35).

Considering the uncoupled part of the exchanger between Fluid A and the wB interface, we may write by inspection of Fig 1, Ch 3 (and using the new heat transfer)

$$q_A = q_m = q_{wB} = q_{in} = q \tag{37}$$

$$T_A - T_{wB} = \Delta T_A + \Delta T_m \tag{38}$$

Now suppose we are given that

$$q_A = M\Delta T_A \tag{39}$$

$$q_m = (k/L)\Delta T_m \tag{40}$$

Combining eqs 37 through 40,

$$T_A - T_{wB} = q(M^{-1} + L/k) \tag{41}$$

Differentiating eq 41 and using eq 36,

$$\frac{dq_{in}}{dT} = -(M^{-1} + L/k)^{-1} \tag{42}$$

Since we were given that T_B is independent of q, we may write

$$\frac{dq_{out}}{dT} = \frac{dq_B\{\Delta T_B\}}{d\Delta T_B} \tag{43}$$

and thus the thermal stability criterion in parametric form is given by (combining eqs 35, 42, and 43)

$$-(M^{-1} + L/k)^{-1} < \frac{dq_B\{\Delta T_B\}}{d\Delta T_B} \quad (44)$$

With the stability criterion in this form, it is readily seen that the thermal stability of the heat exchanger is enhanced by making M as large as possible and L/k as small as possible. Also, if we can exert a design influence on the function $q_B\{\Delta T_B\}$, the design intent should be to make $dq_B/d\Delta T_B$ as positive as possible. The criterion also indicates that thermal stability is assured if $dq_B/d\Delta T_B$ is positive, since the left hand side of eq 44 is obviously negative.

It should be noted that, in the language of the old heat transfer, M in eq 44 is replaced by h_A and that eq 44 is valid only if h_A is independent of ΔT_A.

(I derived the generic criterion for thermal stability in my May, 1964 article in Nucleonics. I derived the parametric criterion represented by eq 44 at the Open Forum Session of the 7th National Heat Transfer Conference, August, 1964, using the old heat transfer. I used eq 44 in my discussion in the letters section of the Dec 1964 Nucleonics. I again derived eq 44 in my lecture given at Dartmouth College in the summer of 1965 at a course on two phase heat and mass transfer. The educator in charge of that course used it in his discussion in the letters section of the Oct 1965 Nucleonics. I repeatedly tried to arrange the Journal publication of this simple but important criterion and its derivation. Unfortunately, the reviewers' frame of reference was necessarily the old heat transfer and they invariably did not and perhaps could not appreciate the importance and significance of this criterion and its derivation. In any event, the reviewers would not permit publication or presentation of my work in the normal manner. Some years later (in 1968), eq 44 was published by someone other than myself.)

RESULTS AND CONCLUSIONS

The principal results of this chapter are:

1. Because of its poor mathematical foundation, the old heat transfer does not provide a proper base for a science of heat transfer dynamics and stability.
2. Equipment dynamics and thermal stability are not within the knowledge envelope of the old heat transfer. This can be verified by noting the absence of these subjects from even the latest and most advanced textbooks and handbooks on heat transfer.
3. The new heat transfer provides a proper base for a science of heat transfer dynamics and thermal stability.
4. The concepts of equipment dynamics, stability, and thermal stability are quite simple when placed in the framework of the new heat transfer. Problems in heat transfer dynamics and thermal stability are easily handled using the new heat transfer.

SYMBOLS

A	area, ft^2
F	flow, gpm
h	heat transfer coefficient, B/hr ft^2 F
k	thermal conductivity, B/hr ft F
L	level, in
M	dimensional constant, B/hr ft^2 F
q	heat flow per unit area, B/hr ft^2
Q	heat flow, B/hr
T	temperature, F
ΔT	temperature difference or change, F

$\Delta\Delta T$	change in ΔT, F
U	overall heat transfer coefficient, B/hr ft^2 F
x	unspecified parameter
y	" "

SUBSCRIPTS

B	refers to B fluid or B fluid interface
i	refers to ith or interface
in	refers to flow <u>into</u>
out	refers to flow <u>out</u> of
ss	refers to steady-state
sto	refers to storage
t	refers to total

REFERENCES

1. E. F. Adiutori, New theory of thermal stability in boiling systems, Nucleonics, vol 22 (5), (1964), p 92

INTRODUCTION TO CHAPTER 5, CONDUCTION, RADIATION, & RESISTANCE

In this chapter, we take up thermal resistance, thermal conduction, and thermal radiation in the new heat transfer. However, before we can effectively discuss these topics, we must work through a rather lengthy discussion whose purposes are to provide a precise vocabulary and to dissect and illuminate the old heat transfer concepts which are abandoned and replaced in the new heat transfer.

The discussion begins with a review of very simple math. My purpose in discussing such elementary material is more or less a matter of definition--I want the reader to understand precisely what I intend by such words as proportionalization, linearization, nonlinearization, etc. By way of caution, I emphasize that my use of such words may be several shades different than anyone else's. I apologize for this and note that the difference arises from a desire to be precise, even at the expense of appearing whimsical.

For example, electrical resistors commonly called "Ohm's Law resistors" (since their behavior is such that the voltage drop is proportional to current in accordance with Ohm's Law) are often called "linear resistors" to distinguish them from nonlinear resistors such as vacuum tubes and transistors. And circuits employing only this type of resistor are often referred to as"linear resistance circuits" to distinguish them from circuits employing nonlinear resistors. However, it seems to me that a reasonable definition of the word "linear" precludes such usage because it is misleading--it implies that Ohm's Law resistors are "linear" and that circuits of this type deal effectively with linear resistors, whereas Ohm's Law resistors are not "linear" in a general way and circuits of this type do not deal effectively with resistors which are "linear" in a general way. The truth is that Ohm's Law resistors are proportional resistors and circuits of this type deal effectively only with proportional resistors and thus should be called "proportional resistance circuits" to denote that they do not effectively deal with behavior which is either linear or nonlinear.

If this discussion of Ohm's Law resistors and circuits is confusing, the reader should recall that "linear" includes "proportional" as a special case. While the general case may be used to reach conclusions about the special case, it is equally true that the special case may not be used to reach conclusions about the general case unless one is willing to accept a strong likelihood of error. Thus we may not conclude that circuits which adequately deal with proportional resistors can also deal adequately with linear resistors, although we may of course conclude that a linear technique could deal effectively with proportional behavior. In fact, this latter statement is redundant because "linear" includes "proportional".

To ensure effective communication in this and later chapters, the reader should read through the entire discussion leading up to the new heat transfer concepts, even though parts of the discussion may seem ridiculously elementary. In fact, the reader will find that this discussion is essential to an understanding of this chapter and the remainder of the book--and that it leads to some very important conclusions about heat transfer in particular and engineering in general.

SIMPLE EQUATIONS

In order to simplify the discussion, let us limit ourselves to a discussion of what might be called "simple" equations--ie let us rule out for the moment both differential and integral equations and discuss only equations which contain neither derivatives nor integrals. Simple engineering equations are usually classified into two groups--linear equations and nonlinear equations. It seems reasonable to stipulate that nonlinear equations include simple equations of all forms with no restrictions and no exceptions. Some nonlinear equations are

$$y = 7.2x^2 + 3.6x + \sinh x + \sin x + 12.6 \qquad (1)$$

$$y = \sin x \qquad (2)$$

$$y = 3.2x + 14 \qquad (3)$$

$$y = 1.567x \qquad (4)$$

Pictorially, nonlinear equations or functions would include all those in Fig 1 and any other curves we might imagine.

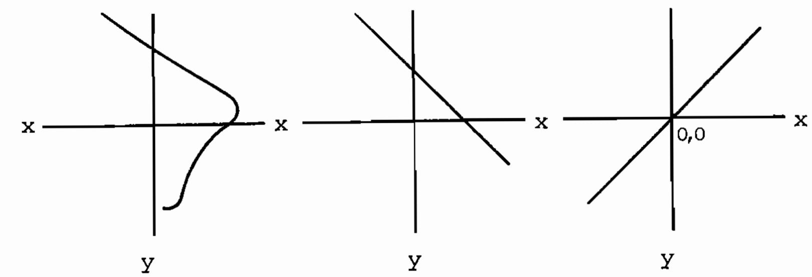


FIGURE 1

It also seems reasonable to state that linear equations or functions are all those which can be put into the form

$$y = mx + b \qquad (5)$$

and no others. We allow that m and b may assume any value without restriction and require that the exponents of both y and x be precisely unity. Thus, linear equations are straight line functions which may intercept the y axis at any value without restriction. The central function in Fig 1 is of course a linear function, as is the right hand function in Fig 1.

It should be noted that we do not specifically exclude linear equations when we refer to nonlinear equations in a general way--the linear form is a particular case of the more general nonlinear form. In the same way, it is useful to further break down the linear form and recognize a particular case of the linear form--the proportional form. Within this framework, "linear equations" have the general form of eq 5 and proportional equations are all those which can be put in the form

$$y = mx \qquad (6)$$

and no others. We allow m to assume any value without restriction and we require that the exponents of both y and x be precisely unity. Thus proportional equations are straight line functions which pass through the point y=0, x=0. The right hand function in Fig 1 is a proportional function.

It should be noted that proportional equations are to linear equations as linear equations are to nonlinear equations--each is a particular case of the more general case. Moreover, when we refer to linear equations, we do not specifically exclude proportional equations--proportional equations are a particular form of linear equations.

EQUATIONS AND CONCEPTS

The question now arises, "Of what possible significance are the above observations about simple equations?" The answer lies in the fact that proportional equations and proportional concepts are the basis for most twentieth century engineering--for example,

$$V = IR \qquad (7)$$

$$\sigma = E\varepsilon \qquad (8)$$

$$\dot{q} = h\Delta T \qquad (9)$$

Yet they are often regarded as "linear" equations and linear concepts, even though they are not "linear" any more than they are "nonlinear"--they are proportional equations which describe proportional concepts--they can be regarded as "linear" or "nonlinear" only in the sense that they are not specifically excluded when we refer to linear or nonlinear equations in a general way. They are particular forms of nonlinear as well as of linear equations--but that does not suggest that they be called "nonlinear equations". They are proportional equations and represent proportional concepts--to call them anything else is misleading.

The question now arises, "What difference does it make whether we recognize that equations 7 through 9 represent proportional or linear concepts?" The answer lies in the fact that proportional concepts are extremely ineffective--proportional concepts cannot deal effectively with either linear behavior or nonlinear behavior. We are thus led to the conclusion that twentieth century engineering is founded on concepts which effectively deal with only the most trivial behavior--proportional behavior--and which are altogether ineffective when dealing with either linear or nonlinear behavior--as we often are.

Once we accept the ineffectiveness of these proportional concepts which form the basis of twentieth century engineering, the next step is easy--these "old way" concepts must be retired--and they must be replaced with concepts which deal effectively with proportional behavior, with linear behavior, and with nonlinear behavior. And this is what The New Heat Transfer is really about--it is about the invention of concepts which effectively deal with nonlinear behavior, and it illustrates the application of such a concept to the science of heat transfer--but it could just as well have been The New Stress/Strain--or The New Electrical Engineering--or The New Fluid Flow.

It is a virtual certainty that Ohm's Law will be retired within the next few decades and be replaced with

$$V = f\{I\} \qquad (10)$$

and that Hooke's Law will be retired and replaced with

$$\sigma = f\{\varepsilon\} \qquad (11)$$

and that "Newton's Law" will be retired and replaced with

$$\dot{q} = f_1\{\text{sys. props.}\}\ f_2\{\text{TDF}\} \qquad (12)$$

and that the many other proportional concepts which provide the foundation for twentieth century engineering will also be retired--and will also be replaced with concepts which, like eqs 10-12, can effectively deal with all types of behavior without restriction. And these new concepts, like those represented in eqs 10-12, will provide the foundation for engineering in the twenty-first century.

TECHNIQUES

Throughout the remainder of this book, we will recognize three types of equations--proportional, linear, and nonlinear--and we will use the definitions described above. In the same vein, proportional behavior will refer to straight line behavior which exhibits a value of y=0 at x=0. Linear behavior will refer to straight line behavior with no restriction as to the value of y at x=0. Nonlinear behavior will refer to any type of behavior without restriction.

Now let us define what we mean by proportionalization, linearization, and nonlinearization. Proportionalization is the process of approximating the function y{x} with a proportional function given by

$$y = mx = (y_1/x_1)x \tag{13}$$

where (y_1,x_1) are the coordinates of a point on the function y{x}. For example, given that (y=7,x=3) is a point on the function y{x}, we approximate y{x} with the proportional function

$$y = (7/3)x = 2.333x \tag{14}$$

From this approximating function, we also estimate that

$$\dot{y} = 2.333 \tag{15}$$

$$\ddot{y} = 0 \tag{16}$$

Thus, when we approximate y{x} using the technique of proportionalization, the coordinates of a single point on the function are sufficient to completely describe the approximating function and all its derivatives. Of course the approximating function and its derivatives may bear no resemblance at all to y{x} and its derivatives, but this is the price one pays for this degree of "simplification".

Linearization is the process of approximating the function y{x} with a linear function given by

$$y = \dot{y}_1 x + (y_1 - \dot{y}_1 x_1) \tag{17}$$

where (y_1,x_1) are the coordinates of a point on the function y{x} and $\dot{y}_1$ is the derivative of the function y{x} at that point. Thus linearization requires a description of a point on the function and the specification of the first derivative of the function at that point. For example, given that $\dot{y}=-1.3$ at the point (y=7,x=3), we approximate the function y{x} with the linear function

$$y = -1.3x + (7 + 1.3(3)) = -1.3x + 10.9 \tag{18}$$

and, from eq 18,

$$\dot{y} = -1.3 \tag{19}$$

$$\ddot{y} = 0 \tag{20}$$

It can be seen that linearization approximates the function y{x} with a linear function which matches y{x} at the point (y_1,x_1) and <u>also</u> matches the first derivative at this point. It is of course this matching of the derivatives which makes the process of linearization so much more effective than proportionalization. Whereas proportionalization is ineffective when dealing with either linear behavior or nonlinear behavior, linearization is effective when dealing with linear behavior in general and with nonlinear behavior in the vicinity of the point (y_1,x_1).

Nonlinearization sounds like it should be the opposite of linearization. Therefore let us stipulate that nonlinearization involves approximating a straight line function with a curved function. Of course this seems paradoxical--certainly nothing is to be gained by such a process. Although the word nonlinearization is seldom used, the process of nonlinearization can and does result quite naturally from dimensional analysis and the use of log log graph paper in the old heat transfer--as we shall discuss in greater detail in the chapter on boiling.

CONCEPTS

It should be noted that proportionalization is such a poor mathematical tool that it is seldom used in pure mathematics, although it is often used in twentieth century engineering. When it is used to deal with proportional behavior, it does no harm. For instance, the proportional concept of static electrical resistance--R=V/I--is adequate to deal with Ohm's Law resistors. On the other hand, this same concept simply cannot cope with the behavior of vacuum tubes or transistors because they do not behave in a proportional manner. In this case, we require more information than just the static electrical resistance--we must invent the "dynamic electrical resistance" defined by

$$R_{dynamic} = \frac{dV}{dI} \tag{21}$$

This concept of dynamic resistance is of course a linear concept--ie we characterize the function V{I} with a straight line function (since we set the first derivative equal to a constant) and we place no restriction on the value of V at I=0. With the static resistance and the dynamic resistance--ie using a proportional concept <u>and</u> a linear concept--it is possible to deal with the nonlinear behavior of vacuum tubes and transistors--but it should be noted that we now require <u>two</u> concepts--ie two kinds of resistance.

In the old heat transfer, thermal resistance is the counterpart of static electrical resistance, but there is no counterpart to the concept of dynamic electrical resistance. Thus, in a very real sense, the old heat transfer has still to progress from proportional concepts to linear concepts. The end result is that, in the old heat transfer, we attempt to use the proportional concept of thermal resistance to analyze nonlinear thermal behavior--and it simply cannot be done effectively because thermal resistance is a proportional concept and proportional concepts are unable to cope effectively with nonlinear behavior.

The use of thermal resistance circuits is quite common in the old heat transfer. If the thermal behavior is proportional everywhere in the circuit--ie if $q \propto \Delta T$ for every resistor in the circuit, then thermal resistance

circuits are adequate to handle the problem. But if some resistance in the circuit exhibits linear or nonlinear behavior--for instance if free convection, boiling, condensation, or radiation is involved in the circuit--then thermal resistance circuits are not effective. They may be used in the analysis, but they do not assist in the solution--they serve only to complicate the solution and confuse the analysis.

In the old heat transfer, many practical problems must be solved by trial-and-error. This unsatisfactory method of solution results not from something inherent in the problems themselves, but from the application of proportional concepts to problems involving nonlinear behavior. In other words, the concept of thermal resistance is what actually causes much of the trial-and-error in the old heat transfer. Moreover, even if we were to invent dynamic thermal resistance, we would still be left with these same trial-and-error solutions and the only improvement would be that a satisfactory result could be obtained with fewer trials. Thus the culprit is not that we have static resistance and lack dynamic resistance--the culprit is that we have resistance--period.

PROPORTIONALIZATION--AN EXAMPLE

We have noted that proportionalization is a very poor mathematical technique. In fact, it is so poor that it has no place in the new heat transfer and the only reason we are examining it in detail is so that we can appreciate exactly what it is we are discarding. The ineffectiveness of proportionalization is well illustrated in the following example in which it is compared with the much more powerful process of linearization. In the example, we deal with y and x, but the example could just as easily deal with q and ΔT, or with V and I, or with σ and ε, or with . . . and . . .

Suppose we are dealing with the function y{x} shown in Figure 2 at the top of the next page and the only information we have about this function is that point A is on this function and that, at A, y=4, x=6, and (dy/dx)=-3. From this information, suppose we wish to estimate the value of y{x=5.5}. In this example, we will first make this

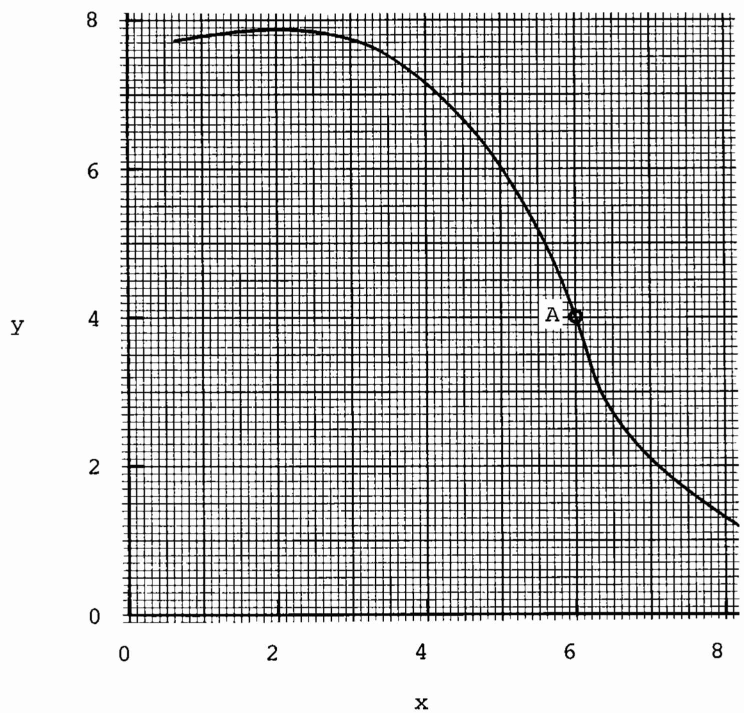


FIGURE 2

estimate using proportionalization and then we will repeat the estimate using the more powerful technique of linearization.

Using proportionalization, we approximate the function y{x} with the proportional function

$$y = (4/6)x = 0.667x \tag{22}$$

from which we estimate that y=3.67 at x=5.5. In a dynamic sense, we estimate that y would decrease 0.33 as a result of a decrease of 0.5 in x from its initial value at point A.

Using linearization, we approximate the function y{x} with the linear function

$$y = -3x + (4 + 3(6)) = -3x + 22 \tag{23}$$

from which we estimate that y=5.5 at x=5.5. In a dynamic sense, we estimate that y would increase 1.5 as a result of a decrease of 0.5 in x from its initial value at point A.

Turning to Fig 2, it can be seen that the true value of y{x=5.5} is 5.2. In a dynamic sense, y increases by 1.2 as a result of a decrease of 0.5 in x from its initial value at point A.

Comparing the true behavior of y{x} with the behavior estimated from proportionalization and linearization, we find

TABLE 1

	True Value	Estim. Value (proportion.)	Estim. Value (linearization)
y{x=5.5}	5.20	3.67	5.50
y{x=5.5}- y{x=6.0}	+1.20	-0.33	+1.50

Table 1 shows that, in this example, proportionalization is so poor that it results in an estimate which is not only quantitatively incorrect, but also qualitatively incorrect. It estimates that y would decrease when actually it would increase as a result of a decrease in the value of x.

The above analysis can of course be illustrated graphically. When we proportionalized y{x} above, we approximated y{x} with a straight line drawn from point A to the point 0,0 as shown in Fig 3 at the top of the next page. When we linearized y{x}, we approximated y{x}with a straight line drawn tangent to y{x} at the point A as shown in Fig 3.

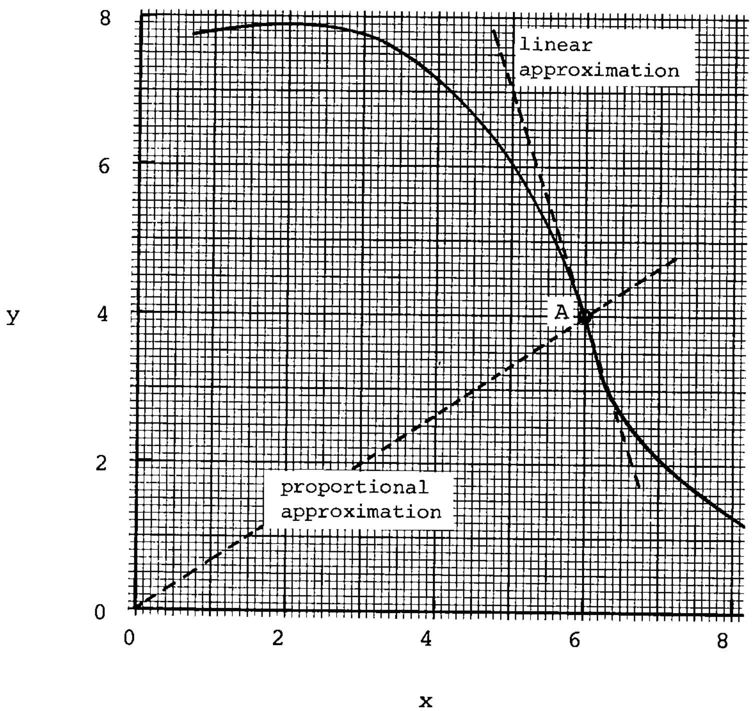


FIGURE 3

It should be noted that the linear approximation closely resembles y{x} in the vicinity of point A--and that the proportional approximation nowhere resembles y{x}--not in the vicinity of point A or anywhere else!

It is very important to note that we are not dealing here with some abstract mathematical exercise which has little bearing on reality. We are dealing here with the very foundation of twentieth century engineering. The proportional approximation in Fig 3 is the graphical equivalent of static electrical resistance--of thermal resistance--of elastic modulus--of . . . . The linear approximation in Fig 3 is the graphical equivalent of dynamic electrical resistance--of tangent modulus--of . . .

And the point to be drawn is that twentieth century engineering is founded on proportional concepts which only in some cases are augmented with linear concepts. And proportional concepts necessarily result in problem solutions based on proportionalization. And proportionalization is so ineffective that the word proportionalize has every right to be obscure--and to be omitted from most dictionaries.

STATIC THERMAL RESISTANCE IN THE NEW HEAT TRANSFER

Static thermal resistance has no place in the new heat transfer.

In the new heat transfer, we recognize that static thermal resistance is a proportional concept and therefore cannot cope with linear or nonlinear behavior. From the old heat transfer, we know that the bulk of heat transfer phenomena are linear and nonlinear. We therefore conclude that the heat transfer process is too complex to be based on the proportional concept of static thermal resistance and we insist on a more effective concept.

DYNAMIC THERMAL RESISTANCE IN THE NEW HEAT TRANSFER

Dynamic thermal resistance has no place in the new heat transfer.

I want to make it very clear that I am not promoting the concept of dynamic thermal resistance which of course would be defined by

$$h_{dynamic} = \frac{dq}{d\Delta T} \tag{24}$$

This dynamic thermal resistance is a linear concept which would augment but not replace the static thermal resistance given by

$$h_{static} = \frac{q}{\Delta T} \tag{25}$$

It is quite true that the addition of dynamic thermal resistance would bring about a great improvement in the old science of heat transfer. But it would not provide the nonlinear vehicle toward which we are striving--and which is represented by eq 12.

My 1964 article on "thermal stability" was, to my knowledge, the first to deal quantitatively with dynamic thermal behavior--the first to recognize the importance of (dq/dT) which of course is microscopically equivalent to (dq/dΔT). I did not define (dq/dΔT) to be the "dynamic thermal resistance" because, in the new heat transfer, this derivative is the natural result of the basic concept (ie eq 12) and is not in itself fundamental to the new heat transfer. In other words, (dq/dΔT) in the new heat transfer is not a matter of definition--it is the natural consequence of the single concept which is the foundation of the new heat transfer.

To my utter amazement, The Argonne Seven (see Ch 4, pg 4) petition revealed a lack of understanding of the mathematics underlying this simple, linear concept of dynamics. The Argonne Seven stated, in print (see Nucleonics, pg 6, Dec, 1964, which presents The Argonne Seven petition and four of the seven authors; for some reason, three names were omitted; my reply to their petition was addressed to the word "hoax" in the original version and, to my chagrin, the word "hoax" was omitted from the published version of The Argonne Seven petition--and without my knowledge--which to me seemed rather devious) that

> The most consistent fundamental error throughout the paper treats temperature as an independent variable.

ie one may not take derivatives with respect to temperature because temperature may not be an independent variable!!!

I found it very difficult to understand why these widely recognized authorities on the old heat transfer should have so little understanding of mathematics in general and dynamics in particular that they would fail to recognize the close parallel between dq/dT and the concept of dynamic electrical resistance.

The above statement by The Argonne Seven concretely documents my oft repeated observation that the old heat transfer is unable to cope with either linear or nonlinear thermal behavior.

THE NEW HEAT TRANSFER REPLACEMENT FOR THERMAL RESISTANCE

In the new heat transfer, we recognize that resistance is an invention--an unnecessary invention which, at best, results in proportional and linear concepts. We insist that the new heat transfer not be based on a proportional concept as in the old heat transfer--and that it not be based on a combination of proportional and linear concepts as in electricity and stress/strain. We insist that the new heat transfer be founded on what might well be called a "nonlinear concept"--ie a concept which is equally effective for all types of behavior without restriction. And this is precisely what the new heat transfer is founded on--the nonlinear concept contained in the fundamental equation of the new heat transfer--

$$q = f_1\{\text{system properties}\}\ f_2\{\text{TDF}\} \qquad (26)$$

The reason this equation--this concept--can deal effectively with all types of thermal behavior is that it does not dictate the form of thermal behavior--ie it does not require or even permit us to force thermal behavior into the molds

$$q = h_{static}\Delta T \qquad \text{or} \qquad (27)$$

$$dq = h_{dynamic}\, d\Delta T. \qquad (28)$$

The new heat transfer abandons both of these rigid molds--and in their place allows each heat transfer phenomenon to suggest whatever mold fits it best.

In the new heat transfer, there is no thermal resistance. The concept of thermal resistance is replaced by the concept contained in eq 26. I have chosen to call this new concept "thermal behavior".

By replacing the old concept of thermal resistance with the new concept of thermal behavior, we progress from the proportional mode of the old heat transfer to the nonlinear mode of the new heat transfer--and greatly improve our ability to deal with thermal behavior in both a theoretical and a practical way.

THERMAL RESISTANCE CIRCUITS IN THE NEW HEAT TRANSFER

There are no thermal resistance circuits in the new heat transfer.

THE NEW HEAT TRANSFER REPLACEMENT FOR THERMAL RESISTANCE CIRCUITS

In the new heat transfer, we replace thermal resistance circuits with thermal flow circuits. A thermal flow circuit is nothing more than a bookkeeping device which enables us to keep track of what is flowing where. It is simply a line diagram of the flow law discussed in Chapter 3.

THERMAL CONDUCTIVITY IN THE NEW HEAT TRANSFER

There is no thermal conductivity in the new heat transfer.

In Chapter 1, we briefly examined the concept of thermal conductivity k and observed that the new heat transfer does not preclude the use of k even though in the old heat transfer it is quite reasonable to think of k as a coefficient. Actually, the discussion in Chapter 1 was an oversimplification, and now we shall discuss k in a more realistic way.

In the old heat transfer, the equation

$$q = h\Delta T \tag{29}$$

does not mean that q is proportional to ΔT, but rather means only that h is defined to be $q/\Delta T$. Thus eq 29 is the trivial relationship

$$q = h\Delta T = \frac{q}{\Delta T}(\Delta T) = q \tag{30}$$

By simple analogy then, Fourier's equation in the old heat transfer

$$q = k\,\frac{dT}{dx} \tag{31}$$

does not mean that q is proportional to dT/dx, but rather this equation is merely a definition of k. Therefore, Fourier's equation is the trivial relationship

$$q = \frac{q}{dT/dx}(dT/dx) = q \tag{32}$$

Now, if Fourier's equation were actually an expression of the fact that the thermal conductance behavior of materials was, is, and will always be proportional, then the retention of Fourier's equation and the concept of thermal conductivity would do no real harm. To deal only with the present, what is the evidence that q is generally proportional to dT/dx? True, the experimental results dealing with thermal conductance behavior are generally presented as though q is indeed proportional to dT/dx--but the fact that the results are presented this way is hardly evidence that the functionality between q and dT/dx was investigated--or even considered. And the fact of the matter is that, in the old heat transfer, it is generally taken for granted that q is proportional to dT/dx, and experimenters seldom take any pains to measure k over a wide range of q--and seldom bother to specify the range of q over which k was measured. The end result is that, in the old heat transfer, one assumes that k is independent of q--ie that q is indeed proportional to dT/dx--whether such an assumption is warranted or not. The best documentation of the generality of this assumption in the old heat transfer is the fact that it is so seldom even questioned, let alone seriously considered.

We therefore view Fourier's equation as a proportional concept which may or may not presently be applied to linear and nonlinear thermal behavior, and we reject it in the new heat transfer. If all present materials exhibit proportional thermal conductance behavior, then the likelihood that the future will bring materials which are not proportional is sufficient to reject Fourier's equation and thermal conductivity.

THE NEW HEAT TRANSFER REPLACEMENTS FOR FOURIER'S EQUATION AND THERMAL CONDUCTIVITY

In the new heat transfer, Fourier's equation and the concept of thermal conductivity are replaced by the equation (concept)

$$q = f_1\{\text{system properties}\}f_2\{dT/dx\} \qquad (33)$$

The manner in which this equation (concept) is applied is best illustrated by an example.

Suppose we want to know the thermal transmittance behavior of material X. In both heat transfers, this behavior is empirically determined--ie we experimentally determine the function q{dT/dx}. In the old heat transfer, we would force the experimental results into the mold of eq 31 even if q were not proportional to dT/dx. Or in the old heat transfer, we would be altogether excused for determining the thermal transmittance behavior at only one level of q and assuming that this behavior--ie k--is the same at all levels of q (and this is the way it is generally done in the old heat transfer).

In the new heat transfer, we do not force the experimental results into a rigid mold--rather we use a very flexible mold which can assume whatever shape or form best fits the results. We permit the experiment to describe the functionality between q and dT/dx--and we then adjust eq 33 (our flexible mold) so that it takes the shape or form suggested by the experimental results. It is the flexible form--the non-rigidity--the free form--of eq 33 which makes it a "nonlinear concept"--ie a concept which effectively deals with all types of behavior without restriction.

Suppose the experiment indicates that, at 350 F, the thermal transmittance behavior of material X is such that

$$q = a(dT/dx)^{0.8} \qquad (34)$$

In the old heat transfer, we would force eq 34 into the form of eq 31 and of course end up with a variable k--ie a k which depends on q. In the new heat transfer, eq 34 is the final result of the experiment--it is the final form--it is the form q(TDF) which is precisely the form we strive for in the new heat transfer--it is the form which most resembles the thermal transmittance behavior of material X--it is the form most useful to us because it is the form which allows us to deal with the thermal behavior of material X in the simplest possible way. In the old heat transfer, we would alter this form--we would squeeze it into the form of eq 31--but in the new heat transfer, we recognize that to alter the form of eq 34 so that material X behavior would seem to "conform" to the behavior of all other materials would be to take a step backwards--and we refuse to take it.

Now of course the possibility exists that, in a practical sense, all materials do exhibit proportional thermal transmittance behavior. What then? Should we go back to Fourier's equation and thermal conductivity? The answer is no! For at least two reasons. First, we simply do not know what the future will bring in the way of new materials and/or what heat flux levels may become practical in the future. Who would dare even guess at the behavior of yet unborn or undreamed of materials? Who would even suggest that today's materials will behave in a proportional way at tomorrow's heat flux levels? We cannot predict the future--but we certainly can prepare for it--and that is the difference between Fourier's equation and eq 33--between thermal conductivity and what I have chosen to describe as "thermal transmittance". Fourier's equation can handle proportional behavior--eq 33 can handle all types of behavior without restriction.

Fourier's equation was adequate to deal with the past--eq 33 is adequate to deal with the future, no matter what it holds.

THERMAL RADIATION IN THE NEW HEAT TRANSFER

The discussion of thermal radiation in Chapter 1 was an oversimplification and now we shall take a better look at it. In the old heat transfer, a proportional concept is used to deal with thermal radiation--ie

$$q_{\substack{\text{radiation}\\ \text{emitted}}} = \varepsilon\sigma T^4 \qquad (35)$$

This concept is quite effective with gray bodies and black bodies--ie bodies whose thermal radiation behavior is such that their emissivities are independent of temperature. The reason is of course that such bodies exhibit proportional thermal radiation behavior--ie eq 35 actually holds only for black or gray bodies and does not hold for non-black/gray bodies. In the old heat transfer, we force the radiation behavior of non-black/gray bodies into the mold of eq 35. In the new heat transfer, we do not. In the new heat transfer, we replace eq 35 with the new general equation for thermal radiation,

$$q_{\substack{\text{radiation}\\ \text{emitted}}} = f\{T\} \qquad (36)$$

which of course leads to eq 35 for black/gray bodies and leads to whatever is suggested by the experimental evidence for non-black/gray bodies.

Now most engineering analysis is based on the simplifying assumption of gray body radiation. When this assumption is accurate, eq 35 results in the new heat transfer as well as in the old heat transfer. However, even in the case of gray body radiation, the analysis of real problems is vastly different in the new heat transfer because of the elimination of thermal resistance and thermal resistance circuits.

For example, Kreith's "Principles of Heat Transfer" contains a radiation/convection problem dealing with a shielded thermocouple. The problem is example 5-9 in the first edition, starts on page 209, and is solved with the old heat transfer using thermal resistance circuits and, of course, trial-and-error. (The same

problem is also in the second edition.) In the discussion leading up to this problem, Kreith states

> To include radiation in a thermal network involving convection and conduction it is convenient to define a unit thermal radiative conductance, or radiant-heat-transfer coefficient h_r . . . . . .
>
> The reason for using a radiant-heat-transfer coefficient, similar to the convective-heat-transfer coefficient, is that the rate of heat flow becomes then linearly dependent on the temperature difference and can be incorporated directly in a thermal network for which the temperature is the driving potential. A knowledge of the value h_r is also essential in determining the over-all conductance h for a surface to or from which heat flows by convection and radiation, since . . .
>
> $$h = h_c + h_r$$

(It should be noted that the above technique does not cause the rate of heat flow to become "linearly dependent on the temperature difference"--ie the fact that we use a proportional approximation to describe the radiant heat flow has absolutely no effect on the actual behavior of the heat flow which in this case happens to be nonlinear. What is actually done in the old heat transfer is to proportionalize the radiant heat flow behavior so that we may then use proportional circuits in an effort to solve this nonlinear problem.)

In the new heat transfer, this same problem is solved directly--ie we reduce the problem to one equation with one unknown and then we extract the root of this equation. The procedure is:

1. Draw a simple flow circuit to provide a visual inventory of the heat flow in the problem.
2. Write the heat flow equations in terms of the temperatures involved, just as we did in the Chapter 3 problems.
3. Reduce these equations to one equation, one unknown and extract the root.

RESULTS AND CONCLUSIONS

In this chapter, we have seen that the concepts of thermal resistance, thermal resistance circuits, thermal conductivity, and thermal radiation emissivity are all proportional concepts. We have noted that proportional concepts are effective when dealing with proportional behavior, but ineffective when dealing with either linear behavior or nonlinear behavior.

Because the bulk of heat transfer phenomena are linear and nonlinear, we have rejected the concept of thermal resistance. In the new heat transfer, we replace it with the nonlinear concept contained in eq 26 which I have chosen to call simply "thermal behavior".

Because the bulk of heat transfer phenomena are linear and nonlinear, we have rejected the concept of thermal resistance circuits. In the new heat transfer, we replace them with simple heat flow circuits which utilize the "flow law" described in Chapter 3. These heat flow circuits are nothing more than line diagrams which provide a visual inventory of the heat flow distribution in the problem.

Because the flow of heat within materials may presently be nonlinear and almost certainly will be nonlinear in the future, we have rejected Fourier's equation and its concept of thermal conductivity. In the new heat transfer, we have replaced them with eq 33 and its concept which I have chosen to call "thermal transmittance behavior".

Because all bodies are not black or gray bodies--ie because thermal radiation is not generally a proportional phenomenon, we have rejected the proportional Stefan-Boltzmann Law and its proportional concept of emissivity. In the new heat transfer, we replace them with eq 36 and its concept which I have chosen to call "thermal radiation behavior".

THE TWENTY-FIRST CENTURY

Engineering in the twenty-first century will bear little resemblance to present day (1973) engineering. For instance,

Hooke's Law will have passed away--
"Newton's Law" will have passed away--
Ohm's Law will have passed away--
Fourier's Law will have passed away--
Stefan-Boltzmann Law will have passed away--
heat transfer coefficients will have passed away--
mass transfer coefficients will have passed away--
thermal conductivity will have passed away--
thermal resistance will have passed away--
thermal resistance circuits will have passed away--
thermal radiation emissivity will have passed away--
thermal radiation absorptivity will have passed away--
static electrical resistance will have passed away--
dynamic electrical resistance will have passed away--
electrical resistance circuits will have passed away--
elastic modulus will have passed away--
tangent modulus will have passed away--
fluid friction factors will have passed away--
drag coefficients will have passed away--
lift coefficients will have passed away--
dimensional analysis will have passed away--
regimes will have passed away--

in short, a host of proportional equations and concepts presently used to deal with linear and nonlinear behavior will have passed away, taking with them a myriad of coefficients, resistances, factors, and moduli.

And in their place will be simple, nonlinear concepts---like those of eqs 10, 11, 12, 33, and 36--providing the foundation for a simple, logical science of engineering which will transform presently impossible problems into simple exercises.

SYMBOLS

a	a constant
b	a constant
f	denotes function
h	heat transfer coefficient
h_c	convective heat transfer coefficient
h_r	radiative " " "
I	current
k	thermal conductivity
m	a constant
q	heat flux
R	resistance
T	temperature
TDF	thermal driving force
V	voltage
x	an unspecified parameter
y	" " "
ε	strain or thermal radiation emissivity
σ	stress or Stefan-Boltzmann constant

INTRODUCTION TO CHAPTER 6, CONVECTION & DIMENSIONAL ANALYSIS

In this chapter, we first take up one phase, forced and free convection in the new heat transfer. We discuss the form of one phase convection heat transfer correlations in the new heat transfer and some advantages which derive from the new forms.

We then go on to a general discussion and critical appraisal of "dimensional analysis" which we take to mean

1. The invention of dimensionless groups or numbers.
2. The a priori deduction of dimensionless correlations.
3. The utilization of dimensionless groups or numbers.

Our purpose is to gain a better appreciation of the overall concept of dimensional analysis and to consider certain evidence of a general nature which suggests that dimensional analysis is not a useful concept.

After this general discussion and appraisal of dimensional analysis, we deal with the subject of film cooling in considerable detail. It will be apparent that the space devoted to film cooling is far out of proportion to its practical importance. However, it will be obvious that we are using film cooling as a vehicle to demonstrate that dimensional analysis can and does lead to results which bear little or no resemblance to reality--that dimensional analysis can and does lead to correlations which in no way depend on most of the correlating parameters--that dimensional analysis can and does lead to the conclusion that the parameters of little importance are the parameters of primary importance and that the parameter of primary importance is of no importance--that dimensional analysis can and does lead to a multiplicity of regimes where in fact only one is present--that dimensional analysis can and does largely prevent an understanding of the phenomenon being investigated and that dimensional analysis is largely responsible for the fact that there is virtually no data in the modern (1973) scientific/engineering literature.

Based on the vehicle of film cooling, we are led to the inescapable conclusion that dimensional analysis, like proportionalization and the heat transfer coefficient which it closely resembles, can and does lead away from reality--away from understanding--away from good design--away from good analysis. We therefore conclude that dimensionless groups and correlations are unreliable--they have outlived their usefulness--they are about to pass away--and they certainly have no place in the new heat transfer or the new engineering.

ONE PHASE FORCED CONVECTION HEAT TRANSFER--OLD WAY VS NEW WAY

In the old heat transfer, one phase forced convection heat transfer behavior is usually correlated in the form

$$N_{Nu} = a\, N_{Re}^{b}\, N_{Pr}^{c} \qquad (1)$$

with the notable exception of liquid metal correlations which may take the form

$$N_{Nu} = e\,(N_{Re}\, N_{Pr})^{f} \qquad (2)$$

<u>Assuming</u> that correlations like eqs 1 and 2 accurately describe one phase forced convection heat transfer behavior (an assumption about which I retain a certain healthy doubt), the new heat transfer form of eq 1 becomes

$$q = a\,(k/D)\, N_{Re}^{b}\, N_{Pr}^{c}\, \Delta T \qquad (3)$$

which, in a more revealing and useful form, is given by

$$q = a\; k^{1-c}\; D^{b-1}\; G^{b}\; \mu^{c-b}\; C_p^{c}\; \Delta T \qquad (4)$$

Similarly, the new heat transfer form of eq 2 is given by

$$q = e\; k^{1-f}\cdot C_p^{f}\; D^{f-1}\; G^{f}\; \Delta T \qquad (5)$$

Equations 4 and 5 illustrate the real contribution of dimensional analysis. With regard to eq 4, <u>if</u> we are willing to accept that one phase forced convection heat transfer behavior is such that

$$q = a\; k^{m}\; D^{n}\; G^{p}\; \mu^{r}\; C_p^{s}\; \Delta T \qquad (6)$$

then dimensional analysis tells us that it is not necessary to determine the five exponents of eq 6 because all five are determined by the two unknowns b and c in eq 4. And this is a wonderful saving because it markedly reduces the effort required to obtain a "general" correlation. For

instance, we approach a general correlation starting from eq 6 and use dimensional analysis to "simplify" eq 6 and obtain eq 4. Now, using eq 4, we can determine the effect of geometry on heat transfer behavior without ever perform experiments in which the geometry is varied!! Thus we can obtain a "general" correlation for this type heat transfer without ever varying D--without ever investigating differe geometries--without having to build more than one test section! Eq 4 tells us that we have only to vary G in th experiment in order to determine b and this will completel define the influence of D because the D exponent is (b-1). Thus, by the proper and classical utilization of dimension analysis, we can experimentally "determine" the effect of geometry on heat transfer behavior without performing any experiments on the effect of geometry!

Until one phase forced convection correlations are obtaine within the framework of the new heat transfer, equations like eqs 4 and 5 will form the basis for this type heat transfer. The manner in which equations like these are used in the static design and analysis of heat transfer equipment is described in Chapter 3.

ONE PHASE FREE CONVECTION HEAT TRANSFER--OLD WAY VS NEW WA

In the old heat transfer, one phase free convection heat transfer is usually correlated in the form

$$N_{Nu} = f\,(N_{Gr}\, N_{Pr}) \qquad (7)$$

which, in a more revealing and more useful form, is given

$$\frac{qD}{\Delta Tk} = f\,(\rho^{2} g \beta \Delta T D^{3} C_p/\mu k) \qquad (8)$$

When the functionality in eq 8 is expressed analytically in the old heat transfer, we will usually be able to separ q and ΔT quite simply and write

$$q = (k\Delta T/D)\, f\,(\rho^{2} g \beta \Delta T D^{3} C_p/\mu k) \qquad (9)$$

$$q = (k/D)\; f_1(\rho^{2} g \beta D^{3} C_p/\mu k)\; f_2(\Delta T) \qquad (10)$$

When the functionality in eq 8 is expressed graphically in the old heat transfer because the analytic expression would be too cumbersome, the separation of q and ΔT is somewhat more trouble. In this case, it is convenient to invent a dimensionless group I have chosen to call "the Jenner Number" which is defined by

$$N_{Jenner} = N_{Je} = qD^4\rho^2 g\beta C_p/\mu k^2 \qquad (11)$$

The particular advantage of the Jenner group is that q appears without ΔT and thus q and ΔT are separated when we correlate free convection heat transfer data using

$$N_{Je} = f_3(N_{Gr}\ N_{Pr}) \qquad (12)$$

(It should be recalled that q and ΔT are confounded--ie are not separated--in eq 7, thus requiring trial-and-error solutions for even simple problems where q is given and we require to know ΔT. Problem 3 in Chapter 2 is an example of such a problem.) With eq 12, we can solve directly for ΔT given q and for q given ΔT, even if it is expressed graphically.

That eq 12 is an identity and that it interprets free convection heat transfer behavior just as well as eq 7 may be seen by noting that

$$N_{Je} = N_{Nu}\ N_{Gr}\ N_{Pr} \qquad (13)$$

and therefore eq 12 is identical to writing

$$N_{Nu}\ N_{Gr}\ N_{Pr} = N_{Gr}\ N_{Pr}\ f(N_{Gr}\ N_{Pr}) \qquad (14)$$

which is obviously identical to eq 7.

To determine $N_{Je}\{N_{Gr}\ N_{Pr}\}$ from the graphical expression of eq 7 requires merely that we obtain the coordinates of some points on the curve of eq 7 and multiply each value of N_{Nu} by the corresponding value of $N_{Gr}\ N_{Pr}$ and so obtain $N_{Je}, N_{Gr}N_{Pr}$ coordinates which can then be used to plot the desired function. For example, pg 176 of McAdams "Heat Transmission", 3rd ed., and pg 305 of Kreith's "Principles of Heat Transfer", 1st ed., present the graphical expression of eq 7 for horizontal cylinders. To transform this graph from the confounded version of eq 7 to the separated version of eq 12, we pick several of the coordinates of the recommended curve which are correctly listed on the graph (it should be noted that five of the coordinates are obviously incorrect) and transform them as follows:

N_{Nu}	$N_{Gr}N_{Pr}$	$N_{Je}=N_{Nu}N_{Gr}N_{Pr}$
1.51	10	1.51x10
2.11	10^2	2.11×10^2
3.16	10^3	3.16×10^3
5.37	10^4	5.37×10^4

By plotting column 3 against column 2, we have the desired function $N_{Je}\{N_{Gr}N_{Pr}\}$ with q and ΔT separated as desired.

It is interesting to note that the Jenner Number would seem to be the first dimensionless group in which the heat flow q appears rather than h or k. For example, page 1-8 of Rohsenow and Hartnett's "Handbook of Heat Transfer" presents 19 dimensionless groups which are important in heat transfer. Of these 19, not a single one contains q.

Until one phase free convection correlations are obtained within the framework of the new heat transfer, equations like eqs 10 and 12 will form the basis for this type heat transfer. The manner in which equations like these are used in the static design and analysis of heat transfer equipment is described in Chapter 3.

DIMENSIONAL ANALYSIS--OLD WAY

Let us first agree that "dimensional analysis" refers to the derivation and utilization of dimensionless groups and equations. Thus our discussion will consider the desirability of dimensionless groups and equations in the broadest possible sense--ie we will consider not only the question of whether it is rational to derive dimensionless equations from dimensional considerations, but also the much broader question of whether it is desirable to even use dimensionless groups in general and dimensionless equations in particular.

The generation of dimensionless equations from dimensional analysis is based on the requirement that equations must be dimensionally consistent--ie we are not permitted to write an equation where the units on the left side are feet/hour and the units on the right side are B/pound. (Although it should be noted that this is frequently done in engineering--we get around this difficulty by noting that the constant of proportionality in the equation is a dimensional constant which has whatever units are required in order that the equation be "dimensionally consistent". In the case just cited, the constant of proportionality on the right hand side of the equation would have the units (feet pounds/B hour) and the equation would thus be dimensionally consistent, although I fail to see that the equation is any different or any better because we go through this exercise of assigning units to something which has no physical significance and which is in fact a pure constant.)

The generation of dimensionless equations from dimensional analysis is further based on the assumption that, if we put the important parameters in the process into the fewest number of dimensionless groups which can contain them all (and require that no group be such that it could be broken down into two or more dimensionless groups), we may then deal with these dimensionless groups rather than the individual parameters. The savings in this is that the number of dimensionless groups will necessarily be less than the number of parameters involved, resulting in a considerable "simplification" of the problem. The example usually cited is one phase forced convection heat transfer which we consider to depend on seven parameters:

$$h, D, k, V, \rho, \mu, C_p$$

Dimensional analysis tells us that there is no need to consider seven parameters since there are really only three parameters of importance--ie we can "simplify" the problem by considering only the dimensionless "parameters"

$$N_{Nu}, N_{Re}, N_{Pr}$$

As a result of this simplification, we have reduced a seven-dimensional problem to a three-dimensional problem. Not only does this simplify the task of determining a general correlation to describe this type of heat transfer behavior, it vastly reduces the amount of experimentation required to determine a general correlation. For example, we can determine the effect of geometry on this type of heat transfer behavior without performing any experiments dealing with the effect of geometry--we can determine the heat transfer behavior in pipes of all diameters by performing experiments with only one diameter--all because of the requirement that equations must be "dimensionally consistent" and because we have properly applied dimensional analysis.

Of course there are certain limitations to dimensional analysis which are recognized in the old heat transfer. For example, McAdams states on page 135, 3rd ed.,

> . . . the result obtained by applying dimensional analysis is limited by the validity and completeness of the assumptions made prior to the analysis. . . Experiment is the only safe basis for determining the correctness and adequacy of the assumptions. If the assumptions are known to be correct and complete, then the result of dimensional analysis, ie the logical grouping of the factors or variables into dimensionless groups, can be accepted without hesitation.

Without hesitation!!!! Thus dimensional analysis is of little use unless "the assumptions are known to be correct and complete". But when we are sure of the assumptions involved, we may use dimensional analysis "without hesitation".

Once we have accepted "the logical grouping of the factors or variables into dimensionless groups", then researchers and experimenters no longer need concern themselves with the real parameters--the problem is no longer how the physical parameters of the process relate to each other--the problem has become to determine how the imaginary parameters relate to each other--ie how the dimensionless groups relate to each other. Thus researchers and experimenters need not report the values of the physical parameters investigated in the experiment--they are of no importance. The important "data" are the values of the imaginary parameters in the experiment--the heat flow rate is not important--the diameter is not important--the temperature difference is not important--the "thermal conductivity" is not important--the important parameter is the Nusselt Number--and therefore only the Nusselt Number should be reported. And certainly this is a wonderful "simplification"--we require only one number instead of four.

Of course this "simplification" makes it identically impossible to determine what was actually done in the experiment--was the ΔT 3F or 300F--was the fluid velocity 1 fps or 27 fps--was the diameter .001 inches or 1.7 feet? But of course questions like these are unimportant and need not concern a designer or analyst who is trying to utilize the reported "results"--or even another researcher. It is not the values of the physical parameters in the experiment which are important--the important thing is the value of the imaginary parameters in the experiment. And how do we know that? Dimensional analysis tells us so!!!

Provided of course that the assumptions on which the dimensional analysis was based are "correct and complete". (A purist might observe that, if the assumptions were known to be correct and complete, it would be a misnomer to call them "assumptions". He might also note that very few articles in the literature contain the warning that "the assumptions used in this analysis are incorrect and incomplete and thus the results reported here should be disregarded".)

Another advantage of dimensional analysis is that it provides a great savings in the amount of data required in order to gain a general understanding of a given process. With dimensional analysis, it is possible to transform a very few data points into an entire universe--without hesitation! In fact, it is very difficult to prevent this transformation. For example, we can obtain a general correlation for one phase, forced convection heat transfer which will apply to essentially all liquids, all gases, all diameters, all turbulent mass flow rates, all heat flows, all ΔT's--from a few data points using one liquid, no gases, one diameter, one temperature difference, one heat flow rate, one mass flow rate--and we can do this "without hesitation"!!! Using dimensional analysis. And the old heat transfer.

DIMENSIONAL ANALYSIS--A CRITICAL APPRAISAL

In a very real sense, the concept of dimensional analysis exhibits every undesirable feature of the heat transfer coefficient--and to an even higher degree. There is actually very little difference between a dimensionless number and the heat transfer coefficient. A dimensionless number is the ratio of a group of physical parameters so selected that the group ratio possesses no dimensions. The heat transfer coefficient is the ratio of a group of physical parameters--q and ΔT--so selected that the ratio exhibits the units B/hr ft^2 F. Except for the fact that the heat transfer coefficient possesses units, there is essentially no difference between a dimensionless number and the heat transfer coefficient. In fact, the heat transfer coefficient can quite accurately be viewed as a "dimensional group" or a "dimensional number". And virtually every argument against the heat transfer coefficient applies equally to dimensional analysis.

In this short chapter, it would not be possible to detail all or even most of the undesirable results which derive directly from dimensional analysis. Instead, what I propose to do is to discuss in a general way some of its more undesirable aspects. Then, in the section on film cooling, we will see in detail how truly undesirable the results of dimensional analysis can be--how little resemblance can result between experimental conclusion and reality once the data have been forced through the rigid mold of the old way concept known as "dimensional analysis".

DIMENSIONAL ANALYSIS CLOUDS THE MIND

With dimensional analysis, we move away from the real world of physical parameters to the imaginary world of dimensionless groups or dimensionless numbers. In this imaginary world, these dimensionless groups which we have invented and christened--Nusselt number, Prandtl number, Jenner number--take the place of physical parameters--feet, F, hours, lbs--in the same way that the invention "h" replaces q and ΔT in the old heat transfer. The heat transfer coefficient is the once-removed relative of q and ΔT. With dimensional analysis, we work with the twice-removed relative of q and ΔT called the Nusselt number which is the ratio of q to ΔT times the ratio of D to k.

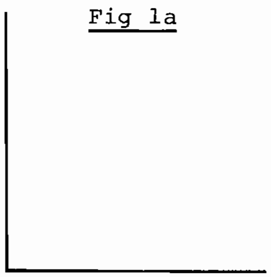
Now as long as we can travel back and forth between the real world and the imaginary world with great ease, then the invention of the imaginary world of dimensionless numbers can be viewed as a harmless exercise. In other words, if travel between the real world and the imaginary world were easy and unrestricted in both directions--ie if the trip were readily reversible--then the invention of the imaginary world of dimensionless numbers would be only a minor inconvenience and could be endured. Unfortunately, this trip is oftentimes made only with the greatest difficulty--and thus the solution of a problem in the imaginary world does not mean that we have a "good" solution in the real world--not if it requires so much effort to travel from the imaginary world to the real world that we lose the ability to relate the one to the other. In this latter case, we end up with a nice, neat solution in the imaginary world--and essentially no solution in the real world--which in the final analysis is the only place a solution is of any true value.

In summary, we have invented an imaginary world in order to aid in understanding the real world--and, instead of accomplishing this, the imaginary world largely prevents an understanding of the real world.

Rather than continue this discussion in the abstract, let us turn to a specific example. Anyone reading this book should be familiar with fluid flow--at least, he should have a working acquaintance with what I feel should be regarded as the first problem in fluid flow--ie the simplest, most fundamental problem in fluid flow. For example, I feel that the first problem in electrical behavior is the functionality between V and I for an Ohm's Law resistor--ie what is the nature of the curve in Fig 1a?

Fig 1a

V

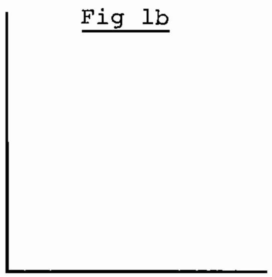


0,0 I

V{I} for an Ohm's Law resistor

Fig 1b

σ

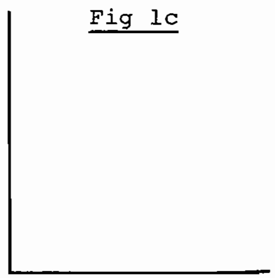


0,0 ε

σ{ε} for an ordinary metal

Fig 1c

ΔP



0,0 W

ΔP{W} for one phase incomp. flow in a constant area duct

Similarly, I feel that the first problem in strength of materials is the functionality between σ and ε for ordinary metals--ie what is the nature of the curve in Fig 1b? In this same vein, it seems to me that the first problem in fluid flow is the functionality between ΔP and W for one phase, incompressible flow through a given length of a constant section duct--ie what is the nature of the curve in Fig 1c for this type fluid flow? I have not filled in the graphs in Fig 1 because at this point, I would like the reader to do so before continuing the discussion.

Now the point of the above exercise is to demonstrate to the reader that, in spite of the fact that he is probably much more knowledgeable in the field of fluid flow than in the fields of electricity or strength of materials, he probably had little difficulty drawing in the functions in 1a and 1b and considerably more difficulty drawing in the function in Fig 1c. In fact, I doubt that any reader will correctly draw in the function in Fig 1c even given five or ten minutes, whereas he was probably able to draw in the functions in 1a and 1b in just a few seconds. The reason I don't expect the reader to be able to solve this first problem in fluid flow in a reasonable time is that I have tried this problem on many engineers and educators with years of fluid flow education and experience in their backgrounds--and to a man, I have never found anyone who could solve this simple, fundamental, first problem in fluid flow in even ten minutes, in spite of the fact that a reasonable solution time would be ten seconds.

Now the question arises, "Why is it so difficult to solve this simple, first problem in fluid flow when the corresponding problems in electricity and stress/strain are so simple they are readily solved by persons with but little background in these fields?" The answer to this question is that this first problem in electricity and stress/strain has been solved in the real world and does not require transformation from an imaginary world. On the other hand, this first problem in fluid flow has been solved in the imaginary world of dimensionless numbers--and the trip from the imaginary world to the real world is far from easy--at least it is a journey that cannot be made in a time span of ten or fifteen minutes.

How have we actually solved this problem in the field of fluid flow? By inventing the dimensionless number called the "friction factor" and the dimensionless number called the "Reynolds number"--by noting that the friction factor is a function of the Reynolds number and another dimensionless group called the "relative roughness"--and by then solving the imaginary problem of the relationship between these three imaginary parameters. And we have solved this imaginary problem--the solution is in a great many texts--but the solution of the real problem--the determination of how W relates to ΔP for the simple case of incompressible, one phase flow--that is another matter. The reader will look long and hard for a text on fluid flow which deals with this real world problem.

Virtually everyone reading this book would have no difficulty filling in Fig 1c if the question were posed in the imaginary world of dimensionless numbers--ie if the axes of Fig 1c were labeled "log f" and "log Reynold's number", most readers would have no difficulty filling in the curve from memory. But when posed in the real world, this simple problem is difficult even if one has ready access to a large library of engineering texts.

The solutions I get to this first problem in fluid flow fall into 3 classes: those which are incorrect because they indicate that $\Delta P \propto W$; those which are incorrect because they indicate that, roughly, $\Delta P \propto W^2$; those which are incorrect and where no function is drawn because the person solving the problem simply did not know how or where to begin. After this first incorrect solution is obtained, I point out a clue which I feel contains 99% of the answer to this problem. The clue is this:

> The real problem in this problem is to piece together the laminar regime and the turbulent regime and so get some idea of one phase, incompressible flow in the real world.

And even after giving this 99% clue, I still have never gotten the right answer to this simple problem--and it is a simple problem--it was solved years ago in the imaginary world--the only difficult part of the problem is the transformation of the solution from the imaginary world of dimensionless numbers to the real world of physical parameters.

Now it seems obvious to me that the purpose of the friction factor and the Reynolds number is to help us understand fluid flow--and if they cannot accomplish this, then there is simply no point to their continued existence. And the above example illustrates that they do not help us understand

even the simplest form of fluid flow--one phase, incompressible flow in a constant area duct. And if they do not help us understand even the simplest fluid flow phenomena, how could they possibly help us understand more complex phenomena--like two phase flow?

The conclusion to be reached from the above is that the imaginary parameters friction factor and Reynolds number not only do not help us understand even the simplest form of fluid flow behavior--they actually are instrumental in preventing an understanding of simple flow phenomena--and this is what I intend by the statement that "dimensional analysis clouds the mind"--which it certainly does as indicated above. Even if there were not a number of other impelling arguments against dimensional analysis in general and dimensionless groups in particular, I think the above example is sufficient evidence to altogether reject dimensional analysis--and to restrict ourselves to solving problems in the real world rather than inventing an imaginary world where problems are "easier" to solve.

DIMENSIONAL ANALYSIS IS NOT SCIENTIFIC

Let us now stand off from dimensional analysis in particular and consider science and the scientific method in a general way. Let us also substitute the words of one more familiar with the history and scope of science and quote from page 12 of "Asimov's Guide to Science":

> To the Greeks, experimentation seemed irrelevant. It interfered with and detracted from the beauty of pure deduction. Besides, if an experiment disagreed with a deduction, could one be certain that the experiment was correct? Was it likely that the imperfect world of reality would agree completely with the perfect world of abstract ideas, and, if it did not, ought one to adjust the perfect to the imperfect? To test a perfect theory with imperfect instruments did not impress the Greek philosophers as a valid way to gain knowledge.
>
> . . . it was Galileo who overthrew the Greek view and effected the revolution. . . He described his experiments clearly and so dramatically that he won over the European learned community. And they

accepted his methods along with his results.

> . . . (Galileo's) revolution consisted in elevating "induction" above deduction as the logical method of science. Instead of building conclusions on an assumed set of generalizations, the inductive method starts with observations and derives generalizations . . . from them.

The inductive method--the so-called scientific method--of Galileo starts with observations (data) and derives gneralizations (correlations) from them.

By no stretch of the imagination can dimensional analysis be considered an inductive method--a scientific method. Dimensional analysis is a deductive method--it attempts to deduce generalizations (correlations) apart from and independent of observations (data). It then permits observations (experiments)--experiments to obtain data which is immediately transformed to non-data--experiments whose only real purpose is to provide the non-data required to fill in those few constants which, unfortunately, could not be deduced from dimensional analysis.

In short, dimensional analysis is not a scientific method--it is a deductive method which has no place in the new heat transfer. The rejection of dimensional analysis will be a great stride forward for heat transfer in particular and for engineering in general.

DIMENSIONAL ANALYSIS UNDERMINES THE LITERATURE

The most important part of any scientific investigation is the data--the experimentally observed values of the physical parameters involved in the investigation. The data must be regarded as the heart of the investigation--the thing without which there could be no investigation, no experimental apparatus, no experimental procedure, no dimensionless numbers, no dimensionless correlations, no scientific report, no scientific publication. Everything literally rides on the back of the data--the data is the prime mover. Without data, science becomes mere speculation. The cardinal importance of the data is one of the few subjects which, in my opinion, is not open to question.

And yet, dimensional analysis says the data is not important--it does not matter whether the heat flow was 13 B/hr or 13000 B/hr--it does not matter whether the diameter was .01 in or.175 ft--it does not matter whether the ΔT was 5F or 175F. Dimensional analysis says that what matters is the non-data--the Nusselt number--the Grashof number--the Reynolds number. There is no need to clutter up the literature with the values of unnecessary physical parameters--with unwieldy data--not when we can "describe" the physical situation in a much briefer, "simpler", more elegant way with the non-data--ie with imaginary parameters--ie with dimensionless numbers--which we can and do deduce before the experiment. There is no need to induce correlations from a morass of data when we can deduce them in large part from dimensional analysis.

And what is the consequence of this statement by dimensional analysis that the data is not important? Why simply that we now possess a modern engineering/scientific literature which deals at great length with experimental apparatus, at great length with experimental procedure, at great length with sophisticated mathematical analysis, at great length with dimensionless parameters and dimensionless correlations, at great length with non-data--and which contains virtually no data! And this absence of data is not simply permitted by dimensional analysis--it is actually encouraged by dimensional analysis and its attendant non-data.

And what is the consequence of this absence of data? Of this preoccupation with non-data? Simply that, in most of the literature, there is no telling what was actually done in the experiment. There is no telling what was actually found in the experiment. There is no telling where knowledge ends and where speculation begins. There is no telling where science ends and where art begins. And there is no way a designer or analyst or researcher can utilize most of the literature--the non-data--the dimensionless numbers--the dimensionless correlations--the fruit of dimensional analysis--without making so many untenable assumptions that any conclusion could only be regarded as a hope--a prayer--a guess. And this is what I intend by the statement that "dimensional analysis undermines the literature"--which indeed it does.

FILM COOLING

"Film cooling" refers to the process of cooling a surface by injecting cooler fluid into its boundary layer. The intent or the desire is that the cooler fluid will remain in the boundary layer for some distance downstream of the injection point, thus protecting the surface from the hotter main stream fluid. With regard to the practical applications of film cooling, Hartnett, Birkebak, and Eckert (1) state:

> A promising method for protecting a surface exposed to a high-temperature environment involves the introduction of a coolant liquid or gas through discrete slots appropriately positioned along the surface . . . This cooling scheme, commonly called film-cooling, has already found application in gas turbines and rocket nozzles and in the future may be of value in cooling leading edges of hypersonic aircraft.

The film cooling fluid is often injected into the boundary layer from a slot which attempts to direct the cooling fluid in a direction more or less parallel to the cooled surface. The distance downstream of the slot is usually called x, the slot height h or s, and the ratio of the mass flow rate in the slot to that in the mainstream (ie G_s/G_m) is usually called M. The degree of cooling is usually described by an invention called "effectiveness" which is dimensionless and is expressed by

$$\eta = \frac{T_m - T_{aw}}{T_m - T_s} \qquad (15)$$

Thus, if the mainstream temperature is 200 F, the film cooling fluid 100 F, and the adiabatic wall temperature at the point of interest is 135 F, then the effectiveness η is 0.65.

Figure 2 at the top of the next page describes a film cooling scheme.

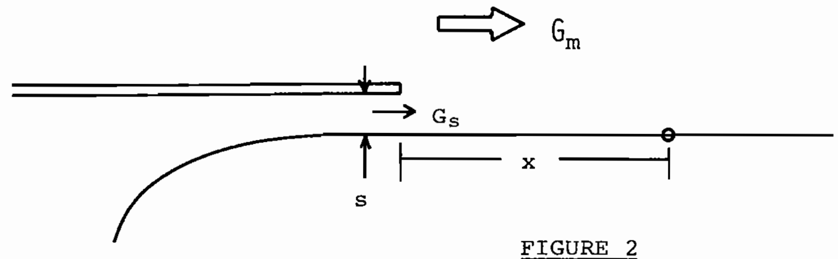


FIGURE 2

FILM COOLING--OLD WAY BY KAYS (2)

Kays (2) presents a brief summary of film cooling starting on page 250. He states:

> . . . there is . . . a considerable body of experimental data, the results of which can be presented in a simple manner.
> . . . It is found that η is primarily a function of a blowing rate parameter M. . . the width (or height) of the injection slot h, and the distance x. . . . using air with constant free-stream velocity, Wieghardt (3) presents the correlation

$$\eta = 21.8\ (x/Mh)^{-0.8} \qquad (16)$$

for $0.22 < M < 0.74$ and $x/h > 100$.

FILM COOLING--OLD WAY BY HARTNETT, BIRKEBAK, & ECKERT (1)

Hartnett, Birkebak, and Eckert (1) investigated film cooling. In describing their investigation, they state:

> In this study a free-stream velocity of 165 fps was used throughout the program. The injection studies were restricted to a single slot size and a single blowing rate.

The single slot height in the investigation was 0.123" and the single blowing rate in the investigation was such that the dimensionless group M was 0.28 at the single mainstream velocity of 168 fps.

With regard to their film cooling investigation, the authors state:

> . . . heated secondary air was injected from the slot and the resulting adiabatic temperature distribution was determined. The usual choice of parameters was made to represent the data. These same parameters are suggested in a semi-empirical analysis presented in the Appendix. . . . The results of the adiabatic wall-temperature distribution are shown in Fig. 24, in terms of the dimensionless distance from the slot leading edge, x/Ms . . . The data are correlated for x/Ms > 60 by the relation

$$\eta = 16.9\ (x/Ms)^{-8/10} \qquad (17)$$

The "analysis in the Appendix" largely deduces the equation

$$\eta = K'(x/Ms)^{-8/10}(N_{Re,s})^{2/10} \qquad (18)$$

which, the authors point out, agrees with "the theoretical results of Klein and Tribus . . . . " The authors also point out

> . . . the agreement of the present measurements with those of Wieghardt (3) is fair, and both investigations are in agreement with the conclusion of the Appendix; namely, that the effectiveness is proportional to $(x/Ms)^{-0.8}$ at distances far downstream of the slot.

The authors plot their η{x/Ms} results in Fig 24 which is a log log graph as suggested by the so-called power law of eq 18. There is very little scatter in Fig 24 and the curve drawn through the dimensionless results has the shape shown in Fig 3.

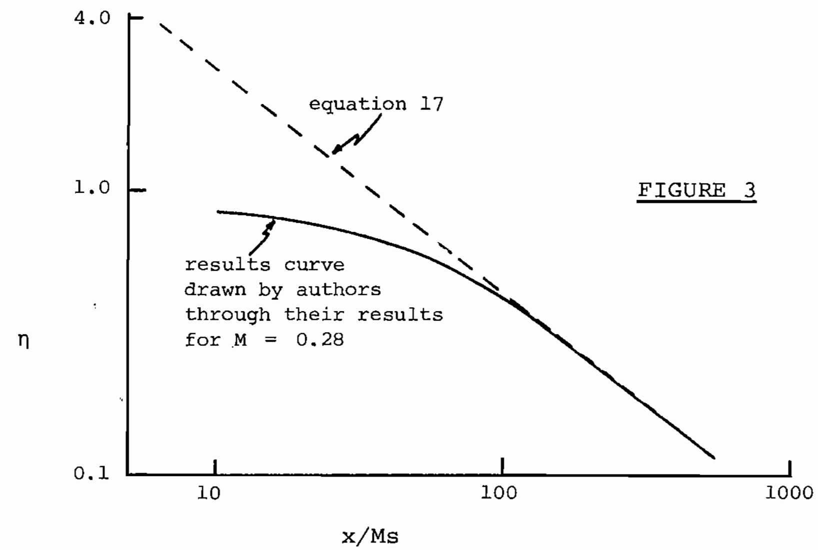


FIGURE 3

In Fig 3, the results curve is compared with the curve of eq 17. It can be seen that eq 17 agrees well with the measured results in the regime above (x/Ms) = 100 and does not agree with the measured results in the regime below (x/Ms) = 100. Since the analysis in the Appendix suggested that the data should correlate using a power law based on x/Ms, this disagreement between a priori analysis and measured results is attributed to a change in regime. Since the results diverge from the analysis at x/Ms values less than about 100, the authors restrict their correlation (eq 17) to values of x/Ms greater than 60--ie they restrict their correlation to the power law regime which one expects to find "far downstream of the point of injections". The value of x/Ms at which the regime changes from the one to the other is determined not from some physical change in the phenomenon of film cooling, but solely from the fact that the measured results do not agree with the a priori analytical results.

The authors compare their correlation (eq 17) with that by Wieghardt (eq 16) who had used a similar geometry and similar parameters and had investigated more than a single value of M. With regard to this comparison, the authors state:

> . . . we would expect that the value of Wieghardt's constant would be less than 16.9 as found in the present investigation . . . This inconsistency may possibly be the result of an inaccurate measurement by Wieghardt of the entering injectant air temperature. . . Notwithstanding the discrepancy in the value of the constant . . ., the agreement of the present measurements with those of Wieghardt is fair. . .

The authors compare their graphical correlation (from Fig 24) with the results of several other investigations in Fig 26 which is a log log plot of η{x/Ms} on which the authors plot the curve from Fig 24 and also the experimental results obtained with similar slots by other investigators. With regard to Fig 26, the authors state:

> The effectiveness values reported by these investigators is shown in Fig 26 as a function of the important parameter x/Ms . . .
> . . . a single curve can be drawn which will represent the reported values with a deviation of ±40 per cent. It is clearly not possible at the present time to state the specific causes of this variation but this must await more carefully controlled experiments in which the governing parameters are varied in a well-defined and systematic fashion. In the meantime, a reasonable estimate of the effectiveness for practical applications can be obtained from Fig 26.

It should be noted that the curve in Fig 26 is the curve derived from the authors' experimental results obtained at one free stream flow rate, one free stream pressure, one free stream temperature, one injection coolant flow rate, one slot height, one slot geometry, and several small temperature differences between injection coolant and free stream fluid.

It should also be noted that the authors do not recommend their analytical correlation (eq 17) which is valid only in the power law regime. Rather, they recommend their graphical correlation which is valid in both the "near slot regime" and the "far from slot regime". Moreover, this duality of regimes is what prevented the authors from correlating their results with a single correlation which would describe film cooling both near the slot and away from the slot.

FILM COOLING--OLD WAY BY SEBAN (4)

Seban (4) performed a film cooling experiment in which

> Injection occurred at a single tangential slot near the leading edge of the plate and the slot size was varied. All flows were turbulent and the injection velocities covered a range from much less to much greater than the free-stream velocity. . . .
> . . . as in the past experience with such systems, separate specifications (correlations) are needed for injection velocities greater and less than the free-stream velocity.

Referring now only to his results for M less than unity, Seban finds that his data obtained at various levels of M cannot be correlated with the dimensionless group x/Ms --he finds that the effects of x/s and M must be considered separately. (It should be noted that x/Ms consists of two dimensionless groups--x/s and M.) By analyzing his data with the three dimensionless groups η, x/s, and M, Seban arrives at the correlation

$$\eta = 25(M)^{1.2}\ (x/s)^{-0.8} \qquad (19)$$

or equally

$$\eta = 25(x/Ms)^{-0.8}\ (M)^{0.4} \qquad (20)$$

With regard to his correlation, Seban states

> Fig 5 shows the correlation of the results that is achieved on this basis and this representation is best for the consideration of the effect of the various system parameters. For instance, the best slot height for a given injection mass flow is the smallest slot, for at constant x and fixed flow, $G_s s$, the effectiveness in the power law region varies as $s^{-0.4}$. The maximum effectiveness wall, of course, is obtained with

$$G_s \simeq 0.9 G_m \qquad (21)$$

In other words, the correlation (eq 19 or 20) indicates that s should be made as small as possible but the experiment does not agree that the smallest s will result in the maximum cooling for a given amount of flow. The experiment indicates that the optimum s is whatever value of s will satsfy eq 21--and eq 21 is <u>not</u> satisfied by the smallest possible s. In other words, the correlation does not agree with the experiment with regard to the effect of s on film cooling behavior--at least not with respect to the optimum value of s. For the optimum value of s, we must rely on the experiment--not the dimensionless correlation of eq 19 or 20.

With regard to film cooling behavior at M values greater than unity, Seban states

> The increase of the effectiveness with increasing mass-velocity ratio that is portrayed in Fig 3 terminates at a mass-velocity ratio of the order of 0.90. At higher ratios the effectiveness is practically constant until a ratio of about 1.1 is attained, then the effectiveness begins to diminish. . . . Fig 10b, for the higher ratios (above 3), does indicate that in the limit the effectiveness is still of the order of what is attained at mass-velocity ratios of 0.60.

Seban indicates that the results obtained at M values above unity are not well correlated in the form $\eta\{x/Ms\}$. He therefore correlates these results using other dimensionless groups and plots the dimensionless results on Fig 11 which indicates a near slot regime and a power law regime far from the slot. Seban presents a correlation (eq 12) for the power law regime.

FILM COOLING DATA

The reduced data in a film cooling experiment would generally consist of a specification of the geometry and

> coolant flow rate, temperature, and pressure
> mainstream flow rate, temperature, and pressure
> $T_{aw}\{x\}$

Unfortunately, I have been unable to find a single article in the literature which reports film cooling results in anything even approaching this highly desirable form--in spite of the fact that it seems patently obvious to me

that no researcher should want to publish his results and correlations without publishing the substantiating data--the only thing which can prove that his results are good results and may be used with confidence--without hesitation. In fact, it seems to me that no Journal and no Journal Editor should even consider the publication of articles which purport to describe experimental results unless the reduced data are presented in tabular form and the raw data is made available through a documentation institute.

To digress for a moment, at the beginning of the twentieth century, there was a great deal of excitement all over the world because it was generally recognized that man would soon fly in a heavier-than-air machine which would respond to his every command and not be at the disposal of the wind. Universities and institutes all over the world were studying and publishing anything and everything which would or might bear on the problem. The Wright brothers were the first to solve the puzzle and it seems appropriate to ask "How much help did they get from the scientific literature of the time? Did they merely put together the pieces which were already in the literature? Or did they first make the pieces and then fit them together independent of the literature?" The best answer is provided in the words of the Wright brothers:

> Having started out with absolute faith in the existing scientific data, we were driven to doubt one thing after another till finally, after two years of experiment, we cast it all aside and decided to rely entirely upon our own investigations. Truth and error were everywhere so intimately mixed as to be undistinguishable.

It seems likely to me that, if one were to take the time required to review the literature at the turn of the century, he would find not that the scientific data were faulty, but rather that the data did not exist in the literature--that the literature had been so undermined by dimensional groups and dimensionless groups that there was essentially no data in the literature--a situation which still (1973) prevails more than 300 years after Galileo's discovery of the importance of the data.

To return to film cooling, the data are usually transformed into dimensionless groups which are reported graphically, usually on log log graphs suggested by power laws which in turn were suggested by a priori deduction. Very seldom are the dimensionless groups reported in tabular form in sufficient detail even to determine $\eta\{x,M,s\}$--ie tabular results are usually either incomplete or the parameters have been so grouped together--so confounded--that it is virtually if not identically impossible to separate them in order to determine the individual effects of the parameters investigated.

SUMMARY OF FILM COOLING RESULTS--OLD WAY

For the regime represented by low mainstream velocity, injection mass flow rate less than mainstream mass flow rate, low mainstream temperature, small temperature difference between injection coolant and mainstream fluid, coolant injection through tangential slots, there is widespread agreement that the important film cooling parameters are x, M, and s--ie these parameters influence and largely determine the film cooling effectiveness η. It is also widely agreed (or rather was widely agreed in the early 1960's) that, in the aforementioned regime, film cooling data are well correlated by expressions of the form

$$\eta = a(x/s)^b(1/M)^c \quad (22)$$

and that oftentimes b=c, allowing us to write simply

$$\eta = a(x/Ms)^b \quad (23)$$

On the other hand, it is recognized that better correlation should be possible and that it will require better experimentation and more dimensionless parameters. It is also recognized that these correlations apply only in the "far from slot regime" and that the "near to slot regime" would require a different correlation.

FILM COOLING--A CRITICAL APPRAISAL OF THE OLD WAY RESULTS

I wish to make it very clear that, in the following discussion, I am <u>not</u> suggesting that the old way concepts have been incorrectly applied to the study of film cooling. I am well aware that Wieghardt, Seban, Hartnett, Birkebak, and Eckert are widely recognized authorities on the old heat transfer and that they competently apply the old way concepts and methods. What I am suggesting is that they have <u>correctly</u> applied dimensional analysis--they have <u>correctly</u> applied a priori deduction--they have <u>correctly</u> applied regimes--they have <u>correctly</u> applied power laws--and that the <u>correct</u> application of these old way concepts leads to <u>results</u> which are largely incorrect because they bear little or no resemblance to reality--to the real behavior of the phenomenon known as "film cooling".

And these widely accepted results which bear little or no resemblance to reality incriminate the methods from which they were obtained--and lead us to conclude that these methods are not useful--and must be abandoned in the new heat transfer--and in the new engineering.

For example, Hartnett et al obtain their general correlation by investigating only one mainstream flow rate, one mainstream temperature, one injection flow rate, one slot height, one slot geometry--perfectly permissible using the old ways because, in the Appendix, they largely a priori deduce that the parameter of importance is x/Ms. Therefore they investigate various levels "of the important parameter x/Ms" and do not bother to investigate various levels of the unimportant physical parameters. Having accepted this dimensionless parameter x/Ms--this "logical grouping of the factors or variables"--and having accepted it "without hesitation" because the a priori deduction was based on assumptions which were neither incorrect nor incomplete--it was not necessary to vary x, M, and s to determine how they individually affect η. It was necessary only to vary x/Ms in whatever manner was easiest. Thus, it is not the experiment that tells us how x affects film cooling--how M affects film cooling--how s affects film cooling. The a priori deduction tells us how these parameters affect η. The experiment was necessary only to determine the constant 16.9 in eq 17 because, unfortunately, this constant could not be deduced a priori.

With regard to eqs 16, 17, and 19, note that they predict

$\eta = \infty$ at $x = 0$ in spite of the fact that it is physically impossible for η to exceed 1.0. Thus we have the anomalous situation of attempting to correlate with a function which predicts that $\eta = \infty$ when in fact η cannot exceed unity! In other words, eqs 16, 17, and 19 bear no resemblance to reality in the region near the slot.

And how is this handled with the old way concepts? How do we rationalize the fact that we are correlating with a function that predicts a physical impossibility? By inventing regimes which excuse us from having to correlate the phenomenon except in a very narrow region--we invent the regime near the slot and the regime away from the slot--and then, because the regime away from the slot seems to be a straight line on log log paper, we concentrate on the regime away from the slot--and let the regime near the slot take care of itself--even though the regime near the slot may be every bit as important in a practical sense as the regime away from the slot.

And how do we determine where one regime ends and the other regime starts? Is there some physical change that we observe in the process? No. The way we tell where one regime ends and the other starts is from the nonlinearity in the non-data. When the non-data no longer resembles the power law behavior suggested by the a priori deduction for the "away from slot regime", then it is manifest that we are in the "near to slot regime". In other words, where the function log η\{log x/Ms\} exhibits so much nonlinearity that it no longer resembles the straight line power law of the "away from slot regime" then by definition the process must be in another regime.

Should we entertain the thought that perhaps the need for two regimes arises from the fact that x/Ms (or x/s and M) is simply not the way to correlate the data? That perhaps by settling on power laws and the "logical grouping" x/Ms (or x/s and M) we have made it identically impossible to correlate the data with one correlation which suggests there is only one regime? No, there is no reason to entertain such thoughts--not unless the a priori deduction was based on assumptions which were incorrect and incomplete--and there is no reason to suspect that.

FILM COOLING--THE NEW WAY REVISIONS TO THE OLD WAY RESULTS

Let us now take a close look at film cooling behavior in the real world using the new way. Of course, what we would most like to do is to apply the new way concepts to a collection of film cooling data. Unfortunately, this cannot be done with literature data because the old way concepts did not require or even suggest that the film cooling data be published. Certainly the data is not in the articles by Seban or by Hartnett et al--nor is the real, physical data in any of the many other articles I consulted on film cooling.

Lacking the data, we will only be able to glimpse what film cooling will be like in the new heat transfer. But this glimpse will be sufficient to demonstrate that film cooling behavior in the real world bears very little resemblance to the old way results obtained in the largely imaginary world of dimensionless numbers and a priori deduction.

In the following discussion, we will consider only those aspects of film cooling on which there is (or was) widespread agreement--namely that, for $M < .9$, $s < .3$", $V_m \simeq 150$ ft/sec, $T_m \simeq 100$ F, $(T_m - T_s) \simeq 100$ F,

1. η is primarily a function of M, s, and x. In other words, η is strongly influenced by the value of M, the value of s, and the value of x.

2. Eqs 16, 17, and 19 indicate that η is primarily a function of M, s, and x.

3. There are two regimes in film cooling--the "near to slot regime" and the "far from slot regime". Because of this duality of regimes, it is not possible to correlate film cooling behavior with only one correlation.

4. The experimental results suggest that the "far from slot regime" follows a power law.

By considering film cooling within the framework of the new heat transfer, we will find that film cooling behavior bears little resemblance to any of these widely accepted conclusions from the old heat transfer. We will in fact find that:

1. The experiments by Seban and by Hartnett et al

should have led them to conclude that η in no way depends on M. Using the old ways, they concluded that η strongly depends on M whereas they should have concluded that η was identically independent of M as evidenced by their correlations, eqs 17 and 19.

2. The experiment by Hartnett et al should have led them to conclude that η in no way depends on s. Using the old ways, they concluded that η strongly depends on s whereas they should have concluded that η was identically independent of s as evidenced by their eq 17.

3. Eqs 16 and 17 state that η in no way depends on either M or s. In other words, the seeming dependence of η on M and s in these equations is only a mirage created by the illusory quality of dimensionless groups. Similarly, eq 19 states that η in no way depends on M.

4. The results reported by Hartnett et al are well correlated by a very simple correlation both near the slot and far from the slot. Since both of these old way "regimes" are well correlated by a single correlation, it is manifest that there is but one regime in the Hartnett et al experiment.

5. The experimental results by Hartnett et al do not suggest a power law in the region far from the slot or the region near the slot.

If the above statements are true (and they are), then we will be forced to conclude that twenty years of experimentation and correlation using the old way concepts of dimensional analysis, a priori deduction, the pervasive importance of power laws, and dimensionless groups resulted in very little understanding of the real behavior of film cooling. We will be forced to conclude that twenty years of the old ways did not even lead to a determination of which parameters were important and which parameters were unimportant.

And we will be forced to conclude that, since twenty years of the old ways could not accomplish what can be done in a few minutes using the new ways, then the old ways should and will be abandoned and replaced by the new ways.

DOES η REALLY DEPEND ON M?

Using the old ways, we a priori deduce that η depends strongly on x/s and M for M values less than about 0.9. We perform some film cooling experiments and show that reasonable correlation can be obtained in the form η{x/s,M} and oftentimes in the even simpler form η{x/Ms}. The ability to correlate the results using x, M, and s of course <u>seems</u> to verify our deduction--ie eqs 16, 17, and 19 <u>seem</u> to say that η really depends on M--that it makes a real difference whether the coolant leaves the slot at $G_s/G_m = 0.2$ or whether it leaves the slot at $G_s/G_m = 0.6$. But perhaps this is only a mirage--perhaps a new way look at these equations will reveal that they actually say something quite different.

The new heat transfer stresses the importance of physical parameters--of real things rather than imaginary things. In line with this new concept, let us write "the important parameter x/Ms" in terms of physical parameters. To do this, we must remove the invention "M" by substituting the physical parameters which were replaced by M when it was invented. To do this, we note simply that

$$M = G_s/G_m = W_s/sG_m \qquad (24)$$

where W_s is the coolant flow rate per inch of slot length. Using 24, we may write

$$x/Ms = xsG_m/W_s s = xG_m/W_s \qquad (25)$$

Thus, in terms of real, physical parameters, eqs 16 and 17 are really in the form $\eta\{xG_m/W_s\}$ and should be written

$$\eta = 21.8(xG_m/W_s)^{-0.8} \qquad (26)$$

$$\eta = 16.9(xG_m/W_s)^{-0.8} \qquad (27)$$

to denote that η <u>in no way</u> depends on either M or s!!!!

Eqs 16 and 26 are identical, eqs 17 and 27 are identical. All four eqs state that η depends on x, G_m, and W_s. All four eqs state that it is the amount of coolant injected through the slot that is important. All four eqs state that it does not matter one iota what the slot height s is or how the mass flow rate in the slot compares with the mass flow rate in the mainstream--ie M does not matter--s does not matter--the thing that does matter is W_s.

The only difference between eqs 16/17 and 26/27 is that eqs 16/17 contain the dimensionless group M and this invention creates a mirage. It causes eqs 16/17 to seem to state that M is of primary importance when they actually state that M is of no importance. And the invention M causes a parameter of primary importance--the coolant flow rate W_s--to be altogether overlooked and to be left out of the film cooling correlations. Coolant flow rate does not appear in eq 16. It does not appear in eq 17. It does not appear in eq 19. In spite of the fact that these equations actually state that coolant flow rate is of primary importance and that M is of no importance! Thus the end result of the dimensionless group M is to focus our attention on a parameter of no importance and to neglect a parameter of real importance.

And by simply replacing this imaginary parameter M with the physical parameters flow and dimension, we come to realize that what had been "proved" to be important using the old ways was actually proved to be of infinite unimportance. And to realize that a parameter of primary importance does not even appear in the old way correlations. And we conclude that eqs 16, 17, and 19 "prove" that η does not depend on M.

The new way expression of eq 19 is

$$(T_m - T_{aw}) = 25\, W_s^{1.2} G_m^{-1.2} s^{-.4} x^{-.8} (T_m - T_s) \tag{28}$$

which eliminates the inventions M and η and also states that "η" in no way depends on "M".

DOES η REALLY DEPEND ON s?

It is obvious from eqs 26/27 that the experiments by Wieghardt and by Hartnett et al "proved" that η in no way depends on s, even though it was concluded from eqs 16/17 that η strongly depended on s. On the other hand, eq 28 states that η depends on s. Since Seban's experiment included several slot sizes whereas the Hartnett et al experiment utilized only one slot, Seban obviously has the stronger argument. However, it should be noted that Seban's eq 19 seems to state that the M dependence differs whereas it is really the s dependence which differs from eqs 16/17.

ARE THERE REALLY TWO FILM COOLING REGIMES?

The best way to show that there are not two film cooling regimes is to show that the same data which gave rise to the conclusion that there are two regimes can be correlated with one correlation. If the reader will refer to Fig 24 in the article by Hartnett, Birkebak, and Eckert, he will find that the data near the slot and the data far from the slot are very well correlated by the simple expression

$$\eta = \frac{1}{1 + .0142(x/Ms)} = \frac{1}{1 + .0142(xG_m/W_s)} \tag{29}$$

which obviously is not a power law. Eq 29 correlates all the results in Fig 24 so well that I doubt that any reader will be able to distinguish between eq 29 and the curve drawn by the authors through their experimental points.

I do not recommend the use of eq 29 in spite of the fact that it excellently correlates all the results reported by Hartnett et al. The reason of course is that all the results reported deal with only one value of "M" and only one value of s and the new heat transfer does not permit generalizations based on investigations of limited scope. Eq 29 has only one purpose--to concretely prove that the Hartnett et al results are well correlated by a single correlation, thus proving that there are not two regimes of film cooling in their experiment because there is only one--and this one regime which includes both of the old ones does not suggest a power law behavior.

FILM COOLING SUMMARY--OLD WAY VS NEW WAY RESULTS

Twenty years of analysis, experimentation, and correlation using the old ways led to the conclusion that η strongly depends on M for $M < 0.9$. A few minutes analysis using the new ways to examine the old results leads to the realization that η little depends on M--and the realization that the old way correlations actually state that η is identically independent of M even though they seem to state that η strongly depends on M.

Twenty years using the old ways led to the conclusion that η strongly depends on s. A few minutes analysis using the new ways indicates that several of the better known old way correlations state that η is identically independent of s even though they seem to state that η strongly depends on s.

Twenty years using the old ways led to the conclusion that there are two film cooling regimes--a "near the slot regime" which is usually not correlated and a "far from slot regime" which is usually correlated with a power law. A few minutes analysis using the new ways demonstrates that the same evidence which seemed to suggest two regimes actually suggests only one regime--and this one regime does not suggest a power law.

Twenty years using the old ways led to the observation

> It is clearly not possible at the present time to state the specific causes of this variation (referring to the ±40% scatter in $\eta\{x/Ms\}$)

A few minutes analysis using the new ways demonstrates that correlations of the form $\eta\{x/Ms\}$ involve the unconscious assumption that η in no way depends on either M or s. Therefore any effect of M or s on η would contribute to the variation (scatter). And, since it seems quite likely that M and s possess at least second order importance, one of "the specific causes of this variation" was the fact that the authors correlated their results on the assumption that M and s were of no importance--and the authors were unaware of this unconscious, unstated, untenable assumption because of the illusory nature of dimensionless groups.

DIMENSIONLESS GROUPS IN THE NEW HEAT TRANSFER

There are no dimensionless groups in the new heat transfer. No Nusselt numbers, no Reynolds number, no Prandtl number, no M, no η.

THE NEW HEAT TRANSFER REPLACEMENT FOR DIMENSIONLESS GROUPS

In the new heat transfer, physical parameters take the place of dimensionless groups. We obtain the physical parameters by reversing the old heat transfer process of

inventing dimensionless groups by combining (confounding) physical parameters in group ratios in such a way that the group dimensions cancel. In the new heat transfer, we separate the dimensionless groups into their physical parameters and then insist that they retain their individual identity--ie we do not permit them to become lost in an imaginary group which attains its own identity only at the expense of the infinitely more important physical parameters.

We insist that all experimentation, all correlation, all design, all analysis be focused on real, physical parameters and we altogether avoid invented dimensionless groups (as well as invented dimensional groups like h). We recognize that dimensionless groups contain confounded parameters and we insist on working with separated parameters.

In the film cooling example, the invention of the dimensionless group M was largely responsible for preventing an understanding of the true behavior of film cooling using the old ways. In the new heat transfer, we eliminate the invention M--ie we separate the parameters which are confounded in M--ie we substitute the real, physical parameters for the imaginary parameter M. And as soon as we accomplish this simple task, it becomes obvious that what had been widely accepted for 20 years on the basis of the old ways was simply not true--that in fact the true behavior of film cooling was very nearly the opposite of what had been widely accepted for 20 years because of the shortcomings of the old ways.

This discussion of dimensionless groups sounds very much like the earlier discussion of the dimensional group h--and it should. It is a rerun of the same play, but with a larger cast. The cast in the h discussion was q and ΔT; the theme was "Should q and ΔT lose their identity in h--or should they regain their identity and so cause us to abandon h?" The cast in the dimensionless group discussion is the entire world of physical parameters and the theme is "Should the physical parameters of the real world lose their identity in an imaginary world of dimensionless numbers--or should they regain their identity and so cause us to abandon the imaginary world and its dimensionless numbers?"

In the new heat transfer, we reject the imaginary world of dimensionless numbers and return to the real world of physical parameters. We insist on individual parameters--we accept no substitutes--no dimensionless groups--no dimensional groups--no inventions--only reality.

POWER LAWS AND LOG LOG GRAPHS IN THE NEW HEAT TRANSFER

In the old heat transfer, virtually all phenomena are correlated with so-called power laws which of course are straight lines on log log graphs. In the new heat transfer, power laws are considered--along with every other mathematical form which can be imagined. In the new heat transfer, we do not strive to show that, if the results are plotted on a log log graph, a straight line can be drawn through some part of the results--some narrow regime. Instead, we recognize that the experiment will suggest its own correlation--that we must induce the form of the correlation from the data. We recognize that there is no reason why power laws should predominate in nature--no reason why Nature should prefer to trace out straight lines on log log graphs. (The chapter on boiling deals with this subject at greater length.)

A PRIORI DEDUCTION IN THE NEW HEAT TRANSFER

In the old heat transfer, a priori deduction is very important and is vigorously "defended". Thus, in the old heat transfer, one often hears "How does the data agree with the theory?" In the new heat transfer,a priori deduction is very unimportant and is held loosely and tentatively. Thus, in the new heat transfer, one would expect to hear "How does the theory agree with the data?" or "Did the data suggest a theory?"

In the new heat transfer, a priori deduction concerns itself with simple, more or less obvious questions. For example, we would note that η is bounded by 0 and 1.0 and we would conclude that η correlations should be similarly bounded. (This line of reasoning leads directly to eq 29--the simplest equation bounded by 0 and 1.0.)

CONCLUSIONS

1. One phase convective heat transfer correlations in the new heat transfer take the form of eqs 4, 5, 10, and 13 until new correlations are obtained within the framework of the new heat transfer.

2. Dimensionless groups have no place in the new heat transfer. They are replaced with physical parameters.

3. The a priori deduction of dimensionless groups and correlations has no place in the new heat transfer. It is replaced by the consideration of simple, more or less obvious questions.

4. Power laws and log log graphs have little do do with the new heat transfer.

CLOSING REMARKS

I am aware that my critical appraisal of the widely accepted results by Wieghardt, Seban, and Hartnett, Birkebak, and Eckert will be distasteful to some. By way of explanation, I note that dimensional analysis and dimensionless groups have been universally used for many decades in the old heat transfer and the old engineering. Therefore, it would be naive to suppose that dimensional analysis and dimensionless groups will die easily--that their death will be brought about by anything less than a highly specific appraisal proving that these concepts are not useful and should be abandoned.

I could have restricted the discussion of these old way concepts to their application in general rather than in particular. But this general discussion would not have been as impelling--it would not have been scientific because it would have lacked the data--the specific proof that dimensional analysis and dimensionless groups can and do cloud the mind and prevent rather than promote understanding.

The death of dimensional analysis and dimensionless groups will be a giant stride forward for heat transfer and for engineering. It is my hope and intent that this chapter be the death warrant for the old way concepts known as "dimensional analysis" and "dimensionless groups".

REFERENCES

1. J. P. Hartnett, R. C. Birkebak, and E. R. G. Eckert, Velocity distributions, temperature distributions, effectiveness and heat transfer for air injected through a tangential slot into a turbulent boundary layer, ASME Journal of Heat Transfer, pp 293-306, August 1961

2. W. M. Kays, Convective Heat and Mass Transfer, McGraw-Hill, 1966

3. K. Wieghardt, AAF Translation, No. F-TS-919-RE, Aug 1946

4. R. A. Seban, Heat transfer and effectiveness for a turbulent boundary layer with tangential fluid injection, ASME Journal of Heat Transfer, pp 303-312, Nov 1960

NEW SYMBOLS

C_p	heat capacity
G	mass flow rate
h	slot height or heat transfer coefficient
M	see eq 24
N_{Je}	Jenner number; see eq 11
s	slot height; see Fig 2
V	velocity or voltage
W_s	coolant flow rate per inch of slot length
x	see Fig 2
β	temperature coefficient of volume expansion
η	film cooling effectiveness; see eq 15
μ	viscosity
ρ	density

NEW SUBSCRIPTS

aw	adiabatic wall
m	mainstream
s	slot

INTRODUCTION TO CHAPTER 7, BOILING, CONDENSATION, & REGIMES

In a very real sense, boiling was the prime mover of the world in 1934. All over the world, the generation of electricity and the movement of freight and passengers over land and water was based largely on boiling. Yet very little was known about the boiling process in 1934--it was not yet known that q{ΔT} passes through a maximum and a minimum in pool boilers. It is generally agreed that, in 1934, Nukiyama (1) first made the discovery of the maximum and minimum in the so-called pool boiling curve--and this important discovery came more than one hundred years after the invention of the steam engine had established the immense practical importance of the boiling process.

That boiling equipment could be so highly developed on a world wide scale in 1934 with but little understanding of the boiling process is a tribute to what can be accomplished with engineering art. Engineering has always progressed by a combination of art and science and it is virtually impossible at any given time to determine where science ends and art begins. What often happens is that we point to the equipment to document the high level of our engineering science when the truth is that the equipment is primarily the result of engineering art--of years of careful trial-and-error progress supported by accurate bookeeping. With 1973 hindsight, we can see that the engineering art of boiling was decades ahead of the engineering science of boiling in 1934--and we can see that this had been true ever since the invention of the steam engine.

Nukiyama's work marks the beginning of a real understanding of the boiling process and led to the recognition that pool boiling data and condensing data are best compared in the form q{ΔT}. But it does not lead to the conclusions that boiling/condensing design correlations should be in the form q{ΔT}--that the proper form for the design/analysis of boiling/condensing equipment is q{ΔT}--or that the heat transfer coefficient h has no place in boiling or condensing.

In this chapter, we deal with boiling, condensing, and regimes in the new way with q{ΔT} and altogether avoid heat transfer coefficients. But it is very important to note that we are not simply discussing and comparing experimental data--we are trying to determine boiling and condensing behavior in the form q{ΔT} because this is the form required for equipment design and analysis in the new heat transfer--just as h{ΔT} is unquestionably the form required for equipment design and analysis in the old heat transfer.

THE DEVELOPMENT OF THE POOL BOILING CURVE--OLD WAY

After Nukiyama's important discovery of the maximum and minimum in the pool boiling curve, many researchers repeated his experiment and obtained similar results. An example is the experiment performed more than a decade later by Farber and Scorah (2). Their version of the "boiling curve" for pool boilers is shown in Figure 1:

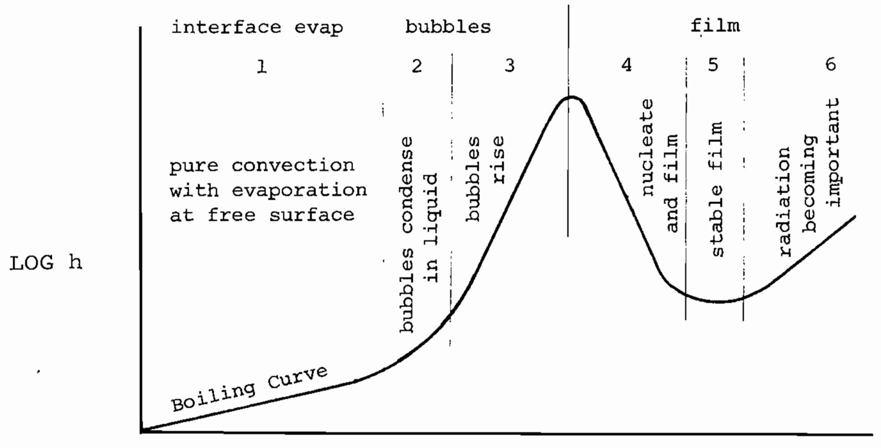


FIGURE 1

Boiling Curve by Farber and Scorah (2)

(Note that this 1948 "boiling curve" is in the form h{ΔT}.) This publication had been presented by Farber and Scorah at an ASME meeting in 1947 where they presented a large number of experimental "boiling curves" in the manner of Fig 1--ie in the form log h{log ΔT}. At the meeting, W. H. McAdams offered the following discussion of the Farber and Scorah presentation:

> In view of the interest in this field, it is hoped that the authors will publish the original data and not merely plots of coefficients versus temperature difference. It is difficult to read these (logarithmic) plotted values with reasonable precision. . .
>
> Because of the possibility of error in temperature measurement, it would be safer to plot heat-flux density versus temperature, rather than coefficient versus temperature in order to isolate any possible error in the abscissa.

Thus McAdams' comment that q{ΔT} was preferable to h{ΔT} had nothing to do with equipment design or analysis--it resulted from the fact that q{ΔT} is preferable "in order to isolate any possible error in the abscissa".

The Farber and Scorah "boiling curve" of Figure 1 is presented in many American heat transfer texts--for example, it is presented on page 400 by Kreith (3), on page 87 by Rohsenow (4), and on page 305 by Holman (5). (Kreith and Rohsenow both modified the boiling curve of Farber and Scorah by substituting q for h on the y axis, perhaps in response to McAdams' discussion. However, they both neglected to make the corresponding changes in the indicated regimes. These changes result from the fact that the maximum and minimum q's do not occur at the same ΔT's as the maximum and minimum h's.)

It is important to note that the pool boiling curve in the old heat transfer is obtained in a purely phenomenological way--ie "boiling curve" is not rigorously defined but is simply measured and presented in the form of Figure 1 or Figure 1 modified to q{ΔT}. We thus have the anomalous result that the "boiling curve" contains a non-boiling region--ie a region in which the heat is transferred without bubbles--without two phases intimately mixed together. In the boiling curve presentations by Farber and Scorah (2), Kreith (3), Rohsenow (4), and Holman (5), the words "boiling curve" are written right over that portion of the curve in which boiling does not occur. Just as in Fig 1 the words "boiling curve" are written over the non-boiling region where in fact the heat is transferred by pure convection and there are no bubbles in the liquid--ie no boiling.

q{ΔT} IN THE NUCLEATE BOILING REGIME--OLD WAY

Figure 1 was intended to describe the overall behavior of pool boiling rather than the functionality within each regime. Let us now consider the precise nature of q{ΔT} in the so-called nucleate boiling regime of the old heat transfer. Nucleate boiling occurs between the non-boiling region of the boiling curve and the maximum in the boiling curve--ie it occurs after the free-convection-with-evaporation-at-the-free-surface regime but before the maximum in q{ΔT}. With regard to this nucleate boiling regime, McAdams (6) states on page 378:

> . . . in the range of strong nucleate boiling, . . . the data of many observers may be expressed by
>
> $$q = a_1 \Delta T^n \qquad (14\text{-}1)$$
>
> where n is a constant ranging from 3 to 4 . . .

On page 502, Kreith (7) states:

> Using experimental data as a guide, Rohsenow (8) modified
>
> $$Nu_b = \frac{h_b D_b}{k_l} = \phi(Re_b)\psi(Pr_l) \qquad (10\text{-}1)$$
>
> (and obtained)
>
> $$\frac{c_l \Delta T_x}{h_{fg} Pr_l^{1.7}} = C_{sf}\,(q/\mu_l h_{fg})^{0.33} (g_c \sigma / g(\rho_l - \rho_v))^{0.165} \qquad (10\text{-}2)$$

Eq 10-2 is the widely referenced Rohsenow correlation for nucleate boiling and it indicates that $q \propto \Delta T^3$. Kreith (7) presents a table on pg 506 which lists values of C_{sf} for a variety of fluids and surface finishes and indicates that the ΔT exponent is 3 for these various fluids and surface finishes.

On page 312, Holman (5) states with regard to nucleate boiling:

> (For water) Levy (22) recommends the relation
>
> $$q = .00202 p^{1.33} (\Delta T_x)^3 \qquad (9\text{-}19b)$$
>
> $100 < p < 2000$psia

On page 13-27, Rohsenow (9) states

> Figure 23 shows a composite of nucleate boiling data for various organic fluids and surfaces (Armstrong (10)).

Figure 23 shows that this composite of many liquids and many surfaces all analysed together using statistical regression are best described by the equation

$$\Delta T = 11.48(.001q)^{0.293}$$

which states that $q \propto \Delta T^{3.41}$.

On page 13-17, Rohsenow (9) states

> Although the slope is predominantly in the neighborhood of 3 (referring to the slope of q{ΔT} on log log coordinates and thus to the ΔT exponent), observations are available with resulting slopes as low as unity . . . and as high as approximately 25 . . .

In summary, there has been widespread agreement over the last 20 or 30 years that, during nucleate boiling in a pool,

$$q \propto \Delta T^n \qquad (1)$$

and that "n is a constant ranging from 3 to 4", although some values are obtained outside this range.

q{ΔT} IN THE TRANSITION BOILING REGIME--OLD WAY

The transition boiling regime is generally regarded as the region between the maximum and the minimum in q{ΔT}. In this regime,

$$dq/d\Delta T < 0 \qquad (2)$$

making the performance of experiments in this regime somewhat more difficult because the equipment design must reflect thermal stability requirements.

The transition boiling regime has not been as exhaustively studied as the nucleate or film boiling regimes in which virtually all pool boilers exhibit thermal stability. One comprehensive investigation of the transition boiling regime was the experiment performed by Berenson (11) who obtained a considerable number of boiling curves. With regard to his data, Berenson states:

> Enough datum points were measured in each run to define the characteristic boiling curve completely.

With regard to his results, Berenson states:

> It was found, with the exception of some of the data presented in Fig 5, that the transition-boiling data lie along a straight line connecting the burnout point (ie the maximum in q{ΔT}) and the film-boiling minimum point (ie the minimum in q{ΔT}) on log-log graph paper. This is also true of the transition boiling data obtained by Braunlich (12) and Kaulakis and Sherman (13).

In summary, three separate investigations performed over a time span of about 25 years all indicated that, with few exceptions, q{ΔT} in the transition regime of pool boiling is such that it yields a straight line drawn between the maximum and minimum in q{ΔT} on log-log graph paper.

THE INITIATION OF BOILING

With regard to the initiation of boiling in pool boilers, it is generally accepted that a finite temperature difference is required to initiate and/or maintain boiling even if the liquid pool is saturated. In other words, boiling does not commence until the surface temperature is well above the saturation temperature of the liquid, and boiling ceases well before the surface temperature has decreased to the saturation temperature of the liquid. For example, on page 13-2, Rohsenow (9) states

> When a pool of liquid at saturation temperature is heated . . . the appearance of the first bubble . . . requires a significant finite superheat.

and on page 13-5, he states

> Some experiments have shown that at a heated surface in water at atmospheric pressure, boiling begins at around 30 F above saturation temperature.

With regard to equipment behavior at boiling inception, Corty and Foust (14) describe the following behavior when pool boiling organic liquids:

> When boiling occurred at a fairly high rate, (q = 15000 B/hr ft^2), the surface was densely covered with hundreds of bubble columns per square inch. As the flux was reduced, more and more of these died out (and finally) the few remaining nuclei died out one by one.
>
> If the reduction of flux was interrupted and energy increased again, the stage at which this increase was started governed the way in which nucleate boiling was reestablished at the higher rates.
>
> If all the active centers were allowed to disappear and the surface was kept free of bubbles for at least 10 to 15 min., it was possible to retain free-convection heat transfer to much higher ΔT's than would normally be required for vigorous nucleate boiling. Superheats of 40 to 50 F above the saturation temperature of the liquids were possible with no bubbles on the heat transfer surface even though the normal ΔT for violent nucleate boiling was only about 25 F. Such excessive superheats could be maintained for several minutes, but upon further increases the surface spontaneously broke into vigorous nucleate boiling, and the ΔT then decreased to the "normal" value. . . .
>
> If the last remaining active nuclei were just permitted to die out and the power input was immediately increased, the nuclei last active reactivated first. Upon further increase in power these nuclei became the centers of patches on which boiling occurred violently while the rest of the surface was bare. The widely varying readings of the thermocouples indicated that these patches were at temperatures characteristic of normal nucleate boiling, although the bare spots on the surface were at much higher temperatures, . . . At fixed rates of energy input the patches were stable, and they extended only when the power was raised. When flux was sufficiently great (about 40000 B/hr ft^2) the surface was again completely covered with bubbles and the hysteresis effect vanished.
>
> If the decrease in flux was interrupted while there still were a number of bubbles active, the pattern of patchwise boiling emerged again. In that case the larger number of bubble patches merged quickly to reestablish normal nucleate boiling . . . .

Corty and Foust (14) also indicate in their Figs 3-8 that there are no "active centers"--ie no locations where bubbles are generated--unless the wall superheat is greater than about 20 F.

With regard to the large superheat required to initiate boiling, Schnurmann and Lardge (15) obtained similar results with organic liquids. They report:

> The transition point from convection to nucleate boiling is clearly marked by a sharp drop of the temperature difference between the wire and the bulk of the liquid. . .
>
> At this transition point there may be a temperature drop of nearly 30 C with n-heptane and about 18 C with perfluoromethylcyclohexane.

THE UNUSUAL BEHAVIOR OF LIQUID METALS IN POOL BOILERS

Liquid metals have often been observed to exhibit unusual behavior in pool boilers. The pool boiling of liquid metals often results in highly oscillatory equipment behavior at heat fluxes in the lower portion of the nucleate boiling regime. For example, Colver and Balzhiser (16) state:

> Temperature fluctuations were observed in the boiling tube thermocouples during all runs. During several runs, pronounced bulk temperature fluctuations as well as pressure fluctuations were observed at low flux levels. Similar fluctuations have been reported in the literature (the authors list six references). . . Temperature fluctuations up to 150 F (were observed in surface thermocouples) at flux levels below 250,000 B/hr ft^2. In some cases the temperature increased rapidly to a point, then slowly (approximately 1 F/sec) to a maximum value. At this point it instantaneously dropped to the original value. Many times this drop exceeded the initial temperature rise and fell as much as 50 F below the original level. . . Each instantaneous drop in temperature was accompanied by a pressure surge and an audible bump. The largest bulk variation . . . was 70 F. . . Above 250,000 B/hr ft^2 the amplitude of fluctuations in the boiling tube decreased significantly (and) the bulk temperature and system pressure remained essentially constant.

> Examination of Figure 9, at heat fluxes below 250,000 B/hr ft^2, indicates that . . . the nucleate boiling process . . . was periodically lost with the system reverting back to convection heat transfer. The upward spikes then may represent a superheating of the system necessitated by a dying out of active nucleation sites. When the superheat was again great enough, the system broke into vigorous boiling with a resultant temperature drop. Many times this drop . . . approached the liquid saturation temperature. Immediately after this, the temperature fluctuations indicated stable boiling. After a time, dependent on heat flux level, stable boiling was lost and the process repeated. As the flux increased, the upward temperature spikes occurred more frequently and were of shorter duration. Finally, they were of such short duration that subsequent drops in temperature were nearly instantaneous. At high fluxes . . . fluctuations were generally not detected.

Similarly, Marto and Rohsenow (17) observed the following while pool boiling sodium:

> During nucleation, large boiler wall temperature fluctuations occurred which in some cases were as high as 150 F. . .
>
> These fluctuations were always accompanied by large variations in the test section noise level as determined from the phonograph cartridge. The sharp increase in noise level and the sudden decrease in wall temperature of the boiler always occurred coincidentally . . . This is interpreted to be the onset of nucleate boiling. After this "bump", nucleation may continue . . . as evidenced by the continued noise level and lower wall superheat. . . When the noise stops, the temperature rises gradually to its maximum value.
>
> When boiling is stable, the wall temperature remains at the lower level and the noise persists.
>
> All the unstable data show that, as the heat flux is increased, stability improves. . . The experimental results show that, around 200,000 B/hr ft^2, stable boiling occurs in most cases.

Marto and Rohsenow refer to this unusual behavior as simply "nucleate boiling instability"--ie nucleate boiling which results in unstable equipment behavior--ie they give it a name on the basis of the effect rather than the cause. (We shall see later that the cause of this unusual behavior is thermal instability of the equipment resulting from the unusual pool boiling curves often exhibited by liquid metals.)

SOME DEFINITIONS LEADING UP TO THE NEW POOL BOILING CURVE

In the old heat transfer, the phenomenological approach to the pool boiling curve gives the anomalous result that the pool boiling curve contains a non-boiling region. To guard against this anomaly in the new heat transfer, let us agree on the following definitions:

Boiling is a process in which bubbles are generated at or near a solid/liquid interface as the result of heat flow from the solid to the liquid.

Pool boiling is boiling in a pool through which there is no net fluid flow.

A pool boiler is a heat transfer apparatus which can accomplish pool boiling. See Fig 2.

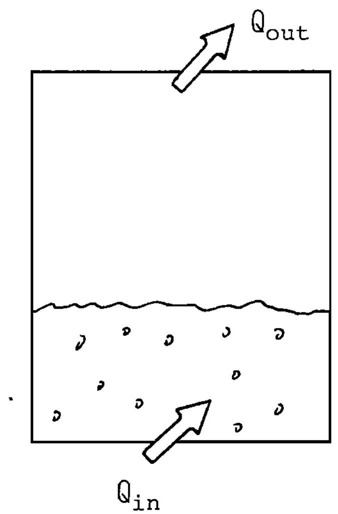


FIGURE 2

POOL BOILER

The pool boiling curve (PBC) is the function q{ΔT} which describes the boiling process in a pool boiler containing a saturated pool. In other words, the PBC is the locus of points relating q to ΔT for which boiling occurs in a pool boiler containing saturated liquid.

In our definition of PBC, it should be noted that we have made two changes from the intent of the old pool boiling curve:

1. We have required that boiling occur at all points of the PBC.

2. We have restricted the definition of PBC in the sense that it refers to pool boilers which contain saturated liquid. Since this is the usual condition of the liquid in pool boilers, we have placed only a minor restriction on PBC.

DEVELOPING THE NEW POOL BOILING CURVE

In the old heat transfer, dimensional analysis and the assumed pervasive importance of power laws leads to the obvious selection of log log graph paper for correlating, describing, and thinking about heat transfer behavior. As described in Chapter 6, the new heat transfer abandons these concepts and leads to the obvious selection of linear graph paper for correlating, describing, and thinking. Additionally, we recognize that log log graph paper is often misleading because it tends to distort any behavior which is not precisely described by a power law. For example, the linear equation

$$y = mx + b \tag{3}$$

is a curve on log log graph paper, but it is oftentimes closely approximated by an equation of the form

$$y = mx^n \tag{4}$$

The distortion arises in that, if b is finite, then the straight line approximation on log log graph paper will not indicate that n=1.0--it may indicate any value depending on the value of b. For example, the straight line on log log graph paper might indicate a slope of 3 in spite of the fact that the true x exponent was actually 1.0. Thus, the use of log log graph paper can (and does) result in approximating linear functions with nonlinear functions--ie of approximating straight lines with curves--ie leads to the technique we defined to be "nonlinearization" in an earlier chapter.

The power laws of the old heat transfer are straight lines on log log graph paper. When these power laws are transformed onto linear coordinates in the new heat transfer, we will expect them to take the forms shown in Fig 3:

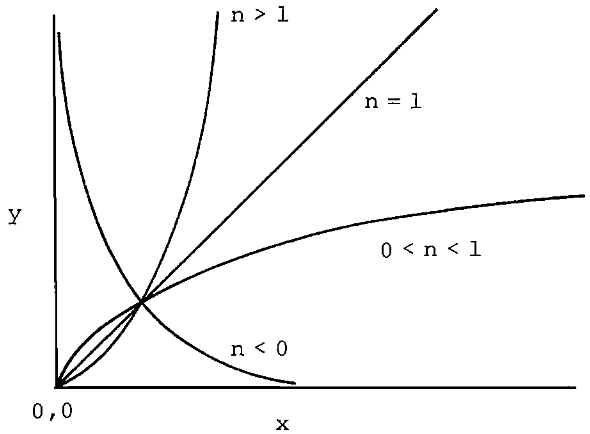


FIGURE 3

POWER LAWS

Thus we will expect the old pool boiling curve to resemble Fig 4 when transformed to the linear coordinates of the new heat transfer:

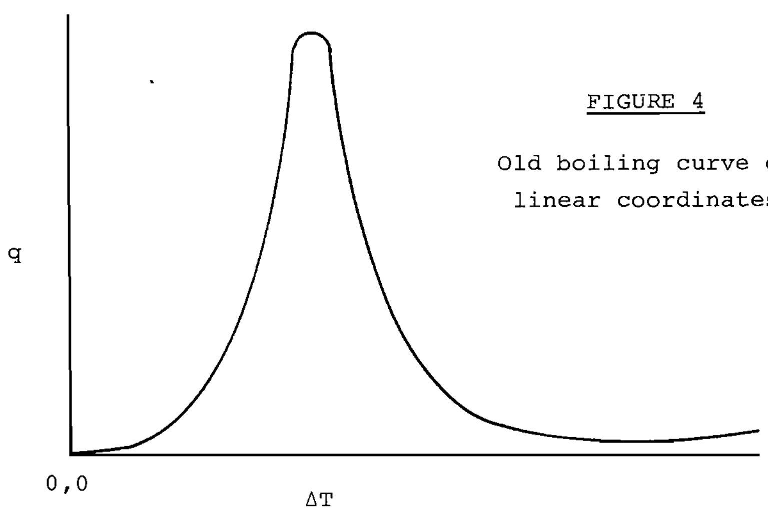


FIGURE 4

Old boiling curve on linear coordinates

Now let us forget the old heat transfer and the old "pool boiling curve" and turn our attention to the development of the new PBC. We will develop the new PBC by analyzing and utilizing the boiling data presented above--the data being the qualitative and quantitative observations of boiling behavior presented on pages 2 through 9 of this chapter--which we will augment with other data as necessary. First we will develop the individual characteristics of the new PBC and then we will put them together in the manner suggested by the data.

PBC CHARACTERISTIC 1--THE FORBIDDEN ZONE

OBSERVATION: Boiling requires a finite ΔT. In other words, the boiling process does not pass through the point ($q=0, \Delta T=0$) because there is a minimum ΔT below which boiling does not occur.

CONCLUSION: There is a "forbidden zone" through which the new PBC cannot pass. This forbidden zone is the shaded area in Fig 5: (Fig 5 is a linear graph)

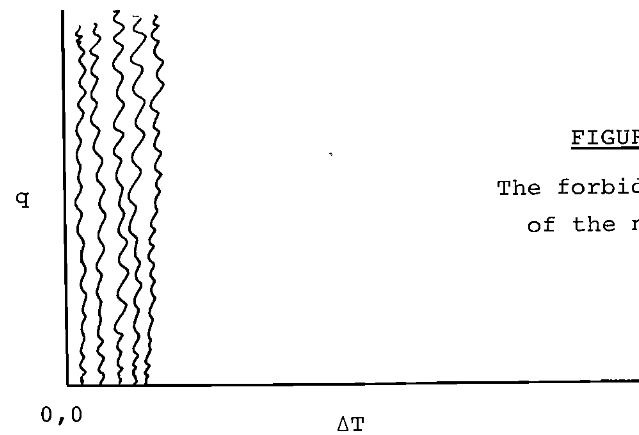


FIGURE 5

The forbidden zone of the new PBC

In this forbidden zone, heat transfer occurs without boiling--without bubbles in a liquid. The heat transfer mechanism in the forbidden zone is free convection from the solid/liquid interface to the free surface, evaporation at the free surface, and convection through the gas phase to the condensing region of the boiler. This free-convection-with-evaporation mode passes through the point ($q=0, \Delta T=0$) and, in the old heat transfer, is described by

$$q \propto \Delta T^n \qquad (5)$$

where n is usually taken to be about 5/4.

PBC CHARACTERISTIC 2--MAXIMUM AND MINIMUM q

OBSERVATION: There is a pronounced maximum and a weak minimum in q{ΔT}.

CONCLUSION: q{ΔT} passes through a maximum and a minimum, allowing us to add to Fig 5 as shown in Fig 6: (Fig 6 is a <u>linear</u> graph.)

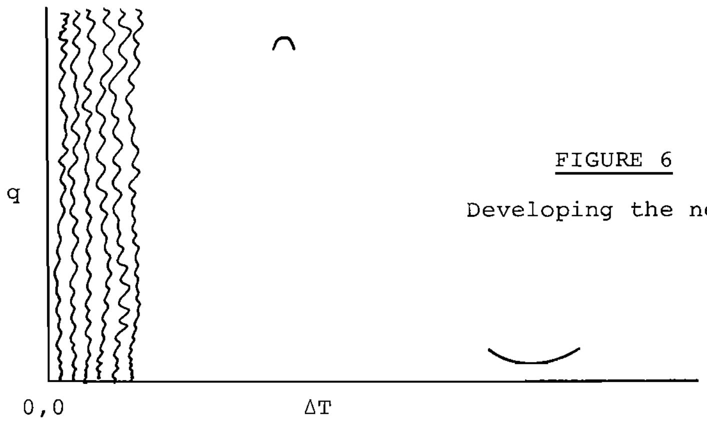


FIGURE 6

Developing the new PBC

PBC CHARACTERISTIC 3--INITIATION SUPERHEAT

OBSERVATION: The initiation of boiling often requires a ΔT considerably greater than that required to maintain boiling. The ΔT required to initiate boiling depends not only on system parameters, but also depends on the prior operating history of the equipment.

CONCLUSION: For a given pool boiling system and a given set of operating parameters, there is a range of ΔT required to initiate boiling. This ΔT_{init} must be specified as a range because it is often affected by the prior operating history of the equipment. Therefore we add on to Fig 6 as shown in Fig 7: (Fig 7 is a <u>linear</u> graph.)

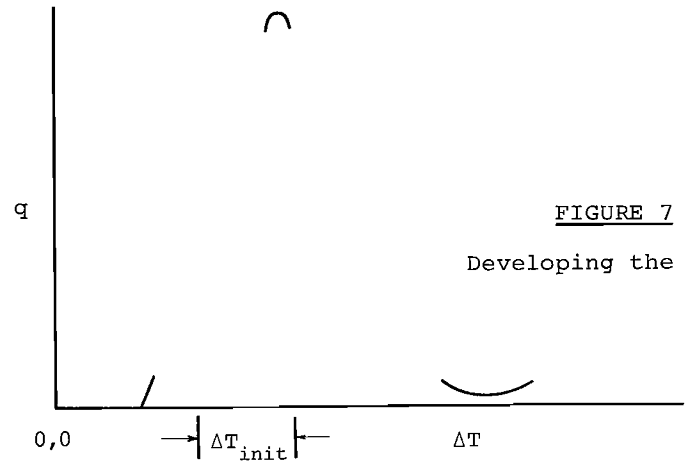


FIGURE 7

Developing the new PBC

PBC CHARACTERISTIC 4--q{ΔT} IN NUCLEATE BOILING

OBSERVATION: There is widespread agreement that, during nucleate boiling, $q \propto \Delta T^n$ where n is usually 3 to 4. This agreement is based on the fact that, if nucleate boiling data are plotted on log log graph paper and the best straight line is drawn through the data and the slope of this line is then measured, the slope is usually 3 to 4, suggesting that the exponent n is also 3 to 4.

CONCLUSION: The obvious conclusion is that q is indeed proportional to ΔT to the third or fourth power. However, it must be remembered that dimensional analysis, power laws, and log log graph paper all go hand in hand and are very widely used in the old heat transfer. Thus there is the danger that the value 3 or 4 is primarily a reflection of the distorting quality of log log graph paper and little reflects the real world behavior of nucleate boiling. In other words, if the old heat transfer data reduction involved simply plotting the data on log log graphs and measuring the slope of the best straight line through the data, the slope of the line may very well have been 3 or 4--but does that prove that the data are best correlated by a function of the form $q \propto \Delta T^n$ and that n is 3 or 4?

Only if we are willing to accept a priori that the data are best correlated by a function of the form $q \propto \Delta T^n$. But why should we be willing to accept a priori that nucleate boiling data are best correlated in the form $q_{nb} \propto \Delta T^n$? What evidence is there to suggest that this form is the best form--aside from the deductive and unreliable evidence of dimensional analysis, power laws, and log log graph paper?

Fortunately, we have one piece of hard evidence which bears on this question. PBC CHARACTERISTIC 1 tells us that, in the real world, the function $q_{nb}\{\Delta T\}$ can not pass through the point ($q=0, \Delta T=0$) because all boiling ceases before ΔT approaches 0. And this real world behavior in no way resembles the behavior of $q_{nb} \propto \Delta T^n$ which analytically must pass through the point ($q=0, \Delta T=0$). Thus if we require that the analytic function resemble the real world behavior in the vicinity of q=0, we will necessarily reject the power law of the old heat transfer.

Now of course if we invent the nucleate boiling regime and then use an analytic function to describe the behavior within the regime, the extrapolation of the function to the region outside the regime may pass through the point ($q=0, \Delta T=0$). But the fact that the analytic function may pass through this point is no reason to require that it must pass through this point! And drawing straight lines on log log graph paper involves the unconscious, unstated, untenable assumption that $q_{nb}\{\Delta T\}$ must pass through ($q=0, \Delta T=0$).

This untenable assumption results from the fact that

> ALL straight lines of positive slope on log log graph paper pass through the point 0,0.

This can be seen quite simply by noting that eq 4 is the equation of ALL straight lines on log log graph paper and eq 4 obviously passes through the point 0,0 for all values of m and all positive values of n.

The end result of the above is the realization that there is no sound basis for assuming that nucleate boiling behavior is described by a power law--no sound basis for correlating nucleate boiling data by drawing straight lines on log log graph paper. In fact, the existence of the forbidden zone strongly suggests that we abandon the form of eq 5 and substitute the form of eq 6 in our effort to correlate nucleate boiling data:

$$q_{nb} = m\Delta T^n + b \qquad (6)$$

The forbidden zone strongly suggests eq 6 because the forbidden zone suggests that b is not zero since the nucleate boiling function in the real world does not pass through 0,0. Eq 6 allows that b may or may not be zero and that n may or may not be accurately inferred from the slope of the best straight line on log log graph paper. In other words, the use of eq 6 allows us the luxury to plead ignorance--we can state quite frankly that we do not know whether the best analytic function will or will not pass through the point 0,0--and the question need not even concern us because the data will provide the answer--the true answer. Eq 6 also warns us that, since b may not be zero, we should not try to infer the value of n from the slope of straight lines on log log graph paper unless we are willing to accept a strong likelihood of error.

Since we may not confidently infer the value of n from slopes on log log graph paper, the question arises as to how can we infer n with confidence. The answer is by plotting the data on linear graph paper and then drawing the best curve through the data and then determining the analytic function which seems most suggested by the data. We may in fact find that the data suggest eq 5--or eq 6--or something altogether different from both of them.

Mesler and Banchero (18) dealt with this problem of functionality in the nucleate boiling regime and stated:

> It has been universal practice to use dimensional analysis to arrive at nucleate-boiling correlations; however, dimensional analysis, as it has been used, usually requires that the data at constant pressure fit an equation of the type (eq 5 above). To evaluate n, one plots the data on a log q vs log ΔT plot.

Rather than assume a power law, Mesler and Banchero made no a priori judgement about functionality and simply plotted their data on linear graph paper. They found that none of their nucleate boiling data suggested a ΔT exponent of 3 or 4. They found that all their data suggested that the ΔT exponent was essentially unity. And their data included some 600 experimental points obtained at seven different pressures from 15 psia to 515 psia using four different liquids--acetone, ethanol, benzene, and Freon 113! They found that all their data were very well correlated by straight lines on linear graph paper--and thus the true ΔT exponent--the ΔT exponent which most agreed with the data--with the real world behavior of nucleate boiling--was 1.0 and not 3 to 4!! And they found that these straight lines on linear graph paper which were strongly suggested by the data did not pass through the point ($q=0, \Delta T=0$)--and thus their results agree with the forbidden zone and demonstrate that any value of n that would have been inferred from straight lines on log log graph paper would necessarily have been in error because the data demonstrated that the nucleate boiling function does not pass through 0,0 and therefore that nucleate boiling is not well described by a power law.

Then, in order to determine whether their unusual result was generally applicable, Mesler and Banchero analyzed the data of Perry (19), the data of Cichelli and Bonilla (20), and the data of Corty (21). They found that the data of these other researchers also indicated that the ΔT exponent was 1.0 because the data of these other researchers also indicated straight lines when plotted on linear graph paper.

Now of course there is a riddle here--the data which indicates straight lines on linear graph paper also indicates straight lines on log log graph paper--the former method indicates an exponent of 1.0, the latter method indicates an exponent of 3--and 1.0 does not equal 3. Both answers can not be correct. What we must now decide is "Which is the stronger proof? Do straight lines on linear paper prove more convincingly that the exponent is 1.0? Or do straight lines of slope 3 on log log paper prove more convincingly that the exponent is 3?" To me, the choice is obvious--if the exponent were indeed 3, then the function should appear curved on linear paper. Since the function described by the data doesn't appear curved on linear paper, it is manifest that any nonlinearity in the data is of no theoretical or practical significance and we should accept the linear function suggested by the data rather than the power law suggested by dimensional analysis. We thus conclude that the nucleate boiling function suggested by the nucleate boiling data is

$$q_{nb} = a(\Delta T - \Delta T_o) \qquad \Delta T > \Delta T_o \qquad (7)$$

which ties in very well with the forbidden zone since it states that boiling will not occur unless ΔT exceeds ΔT_o in agreement with the forbidden zone.

To illustrate how well eq 7 agrees with the nucleate boiling data in the literature, let us now plot some literature data on linear graph paper. On page 13-18, Rohsenow (9) presents Fig 12 which is a log log graph of boiling data by Berenson (11). It can be seen that straight lines are drawn through the nucleate boiling regions of the data, that the slopes of the lines are indeed about "3 or 4", and that the data points fall fairly close to the lines, although it should be noted that the data "points" are as much as 5 F wide. I have taken the coordinates of these data points from Berenson (11) and they are tabulated in Table 1 below:

TABLE 1

NUCLEATE BOILING DATA BY BERENSON (11)

Run 2		Run 3		Runs 17/22		Run 31		Run 32	
q □	ΔT	q ■	ΔT	q ○	ΔT	q ●	ΔT	q +	ΔT
26000	43	7250	25	10500	9	13500	23	16000	14
40500	52	14500	36	20600	10	26500	27	29000	16
55000	66	24000	44	33500	11	49000	31	52000	19
56500	67	47000	56	62000	13	71000	35	79000	23
70000	76.5	74200	69	88500	14	86000	38	91000	26
79500	80	78500	73	90000	14	90000	42	96000	27
82000	85			96000	16			100000	29

UNITS: q = B/hr ft² ΔT = F

These data points are plotted on linear coordinates in Fig 8:

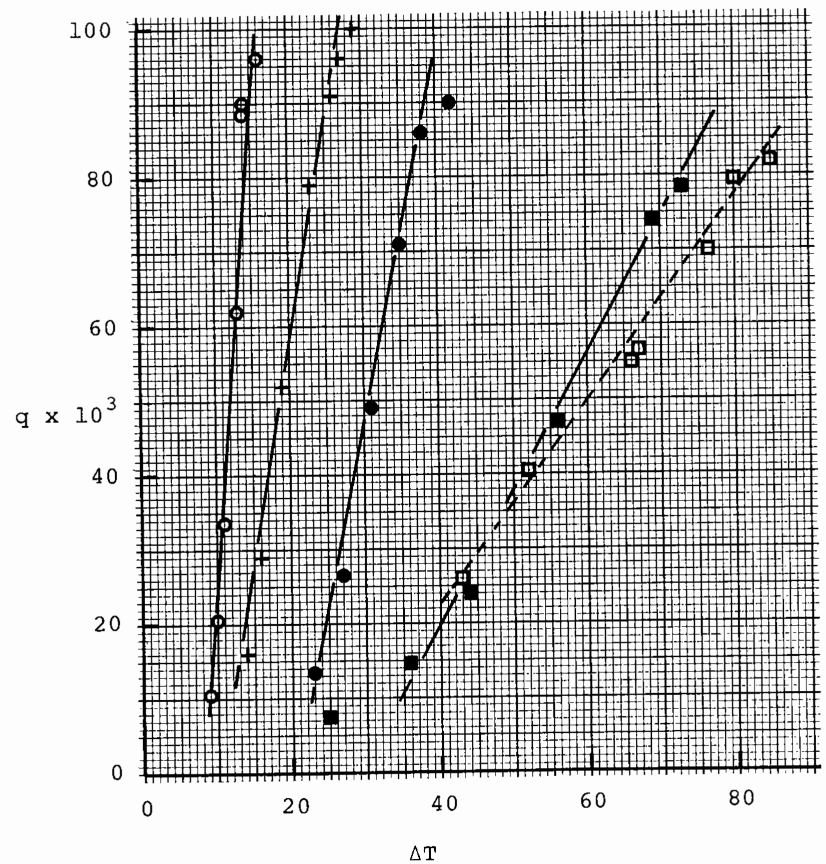


FIGURE 8

Nucleate Boiling Data by Berenson (11)

It is important to repeat that the Table 1 data used to prepare Fig 8 is the very same data shown by Rohsenow in the nucleate boiling region of his Fig 12 which is a log log plot. On Rohsenow's Fig 12, straight lines are drawn through the Table 1 data points and the slopes of the lines are indeed in the vicinity of 3 or 4--but does this prove that the Table 1 data are best correlated by $q \propto \Delta T^n$ and that n is in the vicinity of 3 or 4? Even a cursory glance at the Table 1 data on linear coordinates (Fig 8) indicates

> virtually every data point on Fig 8 falls within 2F of the straight, linear lines drawn through the data

Anyone who has actually performed heat transfer experiments and/or actually analyzed heat transfer data will recognize that the agreement between the lines and the data points on Fig 8 is not "satisfactory agreement"--it is great agreement! And this great agreement obtained with a ΔT exponent of 1.0 provides the definitive proof that the Table 1 data does not indicate a nucleate boiling exponent of 3 to 4 because it indicates an exponent of 1.0. And the fact that we reach this conclusion using the very same data which on log log paper seems to indicate an exponent of 3 to 4 should convince even the most skeptical reader that, during saturated nucleate pool boiling, there is a highly linear relationship between q and ΔT. And the article by Mesler and Banchero (18) demonstrates that this result is generally applicable because it is suggested by the data of Mesler and Banchero, it is suggested by the data of Perry (19), it is suggested by the data of Cichelli and Bonilla (20), it is suggested by the data of Corty (21), and Fig 8 shows that it is suggested by the data of Berenson(11). In short, the data suggest linearity.

With regard to PBC CHARACTERISTIC 4, we reach the conclusion that, during saturated nucleate pool boiling, there is generally a highly linear relationship between q and ΔT and that this relationship is generally described by equation 7 which indicates that there is a forbidden zone in which boiling will not occur. This forbidden zone is defined by the minimum ΔT which will sustain boiling and we have assigned the symbol ΔT_o to this minimum ΔT. The results of Corty and Foust (14) strongly suggest that ΔT_o will generally be less than ΔT_{init}.

We therefore add on to Fig 7 as shown in Fig 9. (Fig 9 is a linear graph.)

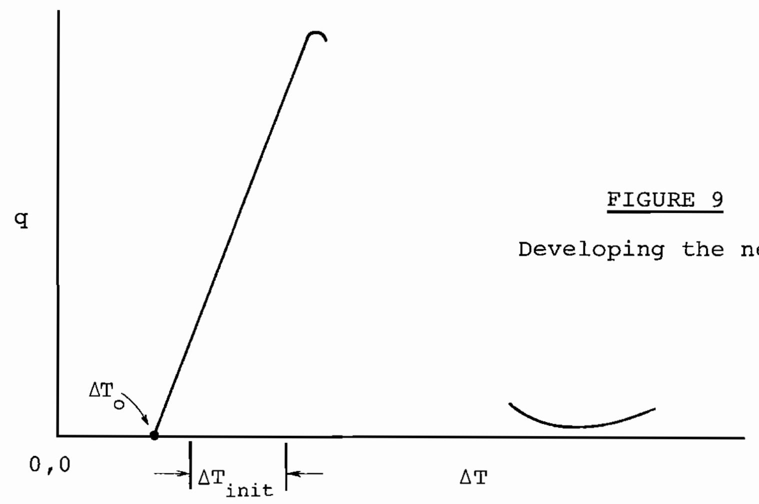


FIGURE 9

Developing the new PBC

PBC CHARACTERISTIC 5--q{ΔT} DURING TRANSITION BOILING

OBSERVATION: Transition boiling data are well correlated by straight lines connecting q_{max} and q_{min} on log log graph paper.

CONCLUSION: The obvious conclusion is that $q \propto \Delta T^n$ where n is less than zero. Thus we will again expect considerable curvature when we plot the data on linear coordinates. For example, Fig 12 on page 13-18 by Rohsenow (9) includes transition boiling data by Berenson (11). In the transition boiling region, Fig 12 shows straight lines drawn between the maximum and the minimum q as suggested by Berenson (see page 6, this chapter). The slopes of these lines indicate ΔT exponents of -1.8 to -14 which of course indicates pronounced curvature on linear coordinates. The transition boiling data in Rohsenow's Fig 12 are tabulated in Table 2. This very same data is plotted on linear coordinates in Fig 10, next page.

TABLE 2

TRANSITION BOILING DATA BY BERENSON (11)

Run 2 q □	ΔT	Run 3 q ■	ΔT	Runs 17/22 q ○	ΔT	Run 31 q ●	ΔT	Run 32 q +	ΔT
82000	85	78500	73	96000	16	90000	42	100000	23
4200	105	4000	110	16700	43	7700	82	6900	79
3400	110	4200	124	15000	46	4600	88	3800	87
3550	120	4850	142	10500	60	3700	99	3400	102
3850	130	7100	191	7700	70	3000	110	4000	121
4200	137	11000	258	5800	76	4000	121	4500	149
5950	160			6400	81	5100	154	7700	213
7250	181			5800	91	6700	193		
7730	206			6000	93	9600	245		
				5500	104				
				6700	109				
				5500	110				
				6450	138				
				6600	147				
				7250	180				
				8000	193				
				9350	226				

UNITS:
$q = B/hr\ ft^2$
$\Delta T = F$

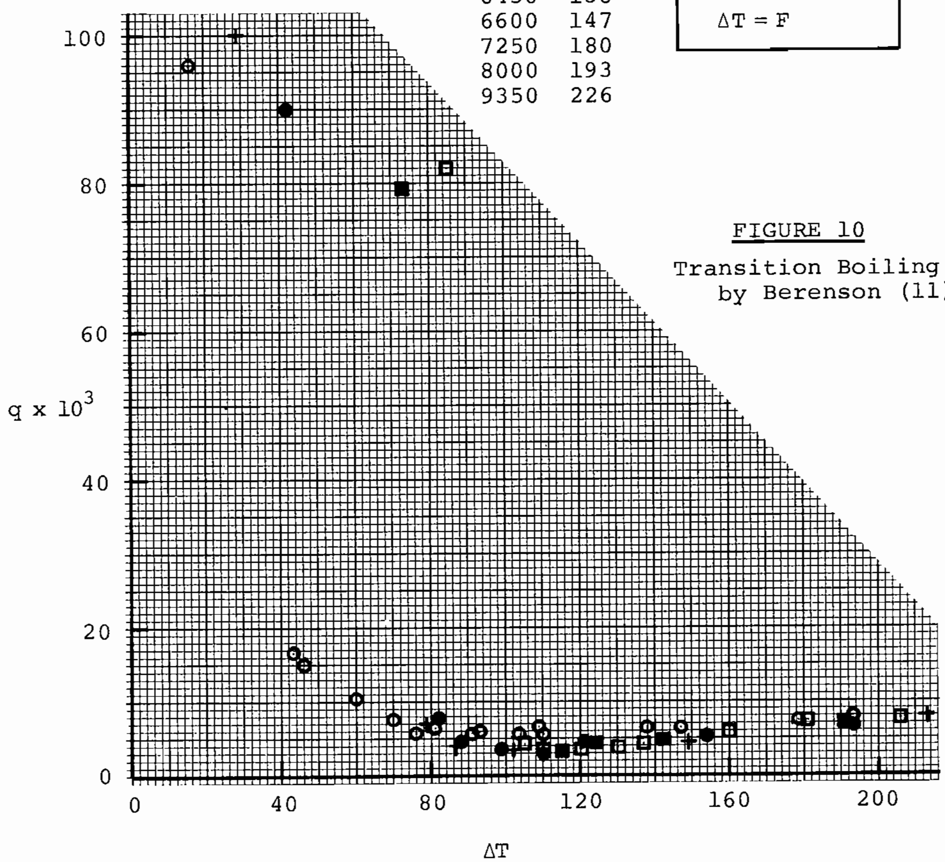


FIGURE 10

Transition Boiling Data by Berenson (11)

Table 2 is arranged such that q_{max} in each run is listed first and is followed by the transition and film boiling data in order of increasing ΔT. From this arrangement, it can readily be seen that Table 2 contains virtually no data in the transition region. Run 2 contains no data in 99% of the transition region. Run 3 contains no data in 100% of the transition region. Runs 17/22 contain no data in 88% of the transition region. Run 31 contains no data in 95% of the transition region. Run 32 contains no data in 96% of the transition region. This lack of transition boiling data tends to escape notice on log log graph paper, but it is well shown on the linear coordinates of Fig 10.

And what is the meaning of this lack of data? Simply that the observation that transition boiling data are well correlated by straight lines on log log paper has no foundation in fact and is simply the result of the vagaries of log log graph paper. Certainly this conclusion is not indicated by the Table 2 data which contains virtually no transition boiling data. And Table 2 is a complete tabulation of the transition and film boiling data reported by Berenson (11) for Runs 2, 3, 17, 22, 31, and 32. The end result is that we must reject the observation by Berenson because it is not substantiated by his data--the observation may be true, but Table 2 lacks the data required to make a meaningful judgement about functionality in the transition boiling region.

However, it should be recalled (page 6, this Chapter) that some of Berenson's data did not lie along a straight line connecting q_{max} and q_{min} on log log graph paper. This unusual data which was obtained in only a fraction of the runs is shown in Berenson's Fig 5. Due to the pronounced curvature on this log log graph, no line is drawn through the unusual data which was obtained in Runs 7, 8, and 9. The unusual data from Runs 7, 8, and 9 are tabulated in Table 3 and are plotted on linear coordinates in Fig 11, next page.

Table 3 and Fig 11 both indicate that Runs 7, 8, and 9 contained a good deal of data in the transition region--and it was in fact the presence of transition data in these runs which made them appear to be unusual--to seem to differ from the majority of the runs which in fact contained little or no transition data. The runs which contained no transition boiling data were well correlated by straight lines on log log graph paper--the runs which contained the desired data were not well correlated by straight lines in the transition region on log log graph paper. Since most of the runs did not contain the desired data, it was concluded that the transition region is generally a straight line on log log graph paper--but a more appropriate conclusion would have

TABLE 3

TRANSITION BOILING DATA BY BERENSON (11)

Run 7 q ○	Run 7 ΔT	Run 8 q ●	Run 8 ΔT	Run 9 q □	Run 9 ΔT
99000	30	100000	28	103000	34
93500	45	97000	41	95000	46
75000	72	90000	52	83000	72
40000	121	75000	84	64500	102
27200	145	28000	141	43500	123
18000	157	21500	153	23500	143
15000	171	10300	167	16500	157
9800	187	9800	187	9500	177
9000	209	9700	221	9200	194
9650	234	12000	266	9500	230
12000	267			10800	260

UNITS:
q = B/hr ft^2
ΔT = F

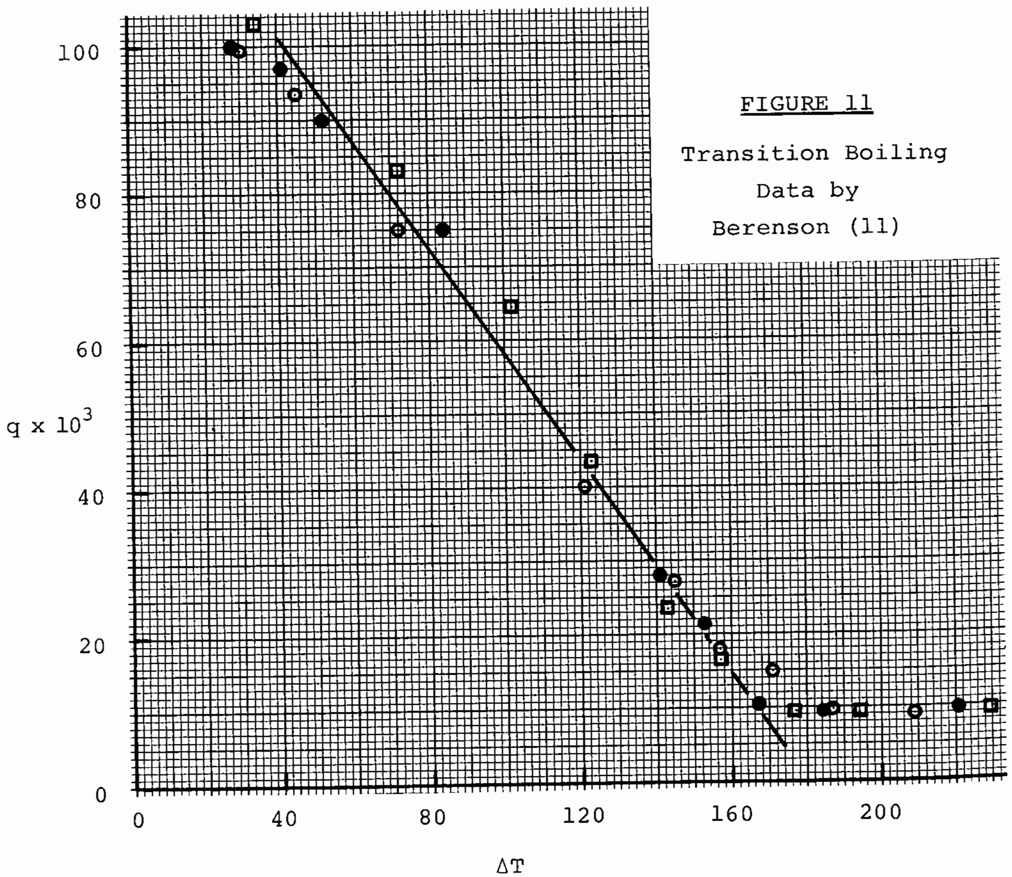


FIGURE 11

Transition Boiling Data by Berenson (11)

been that the runs did not generally contain the desired data. And Table 3 and Fig 11 indicate that those few unusual runs which contained the desired data strongly suggested that, during transition boiling in a pool, there is a highly linear relationship between q and ΔT. (As we shall see in Chapter 8, the reason many of Berenson's runs contained little or no transition region data was that his boiler design did not reflect the requirements of thermal stability and thus thermal instability largely prevented him from obtaining transition region data.)

With regard to PBC CHARACTERISTIC 5, we conclude that the available evidence strongly suggests that q{ΔT} is highly linear in the transition region and that more evidence is required to reach a firm, general conclusion.

THE NEW PBC

Putting together the above PBC CHARACTERISTICS, we obtain the new PBC shown in Fig 12. It should be noted that this new PBC bears very little resemblance to the pool boiling curve of the old heat transfer.

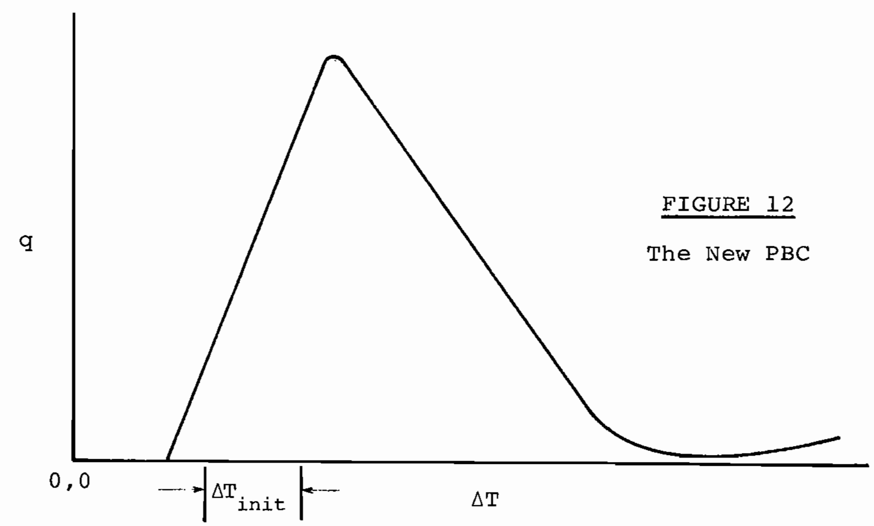


FIGURE 12

The New PBC

INTERPRETING THE POOL BOILING BEHAVIOR OF LIQUID METALS

In the above discussions of equipment behavior when boiling liquid metals, Colver and Balzhiser (16) and Marto and Rohsenow (17) describe the unusual equipment behavior which can result when pool boiling liquid metals. In these publications, the authors do not deal with the cause of the unusual equipment behavior.

It is seldom possible to work backward from the observed effects to the cause in a direct, incremental fashion. It is usually necessary to postulate a cause and then work forward to predicted effects. If the predicted effects do not agree closely with the observed effects, then the postulated cause is rejected and another cause postulated until finally agreement is obtained between predicted and observed effects.

In this Chapter, we postulate that the cause of the unusual equipment behavior often observed when pool boiling liquid metals is that the PBC for (solid)/(liquid metal) interfaces often has the shape shown in Fig 13. In Chapter 9, we will see that this type of PBC results in equipment behavior which agrees very closely with the behavior observed by Colver and Balzhiser (16) and separately by Marto and Rohsenow (17). In Chapter 9, we will conclude that the close agreement between predicted and observed effects strongly suggests that the PBC for (solid)/(liquid metal) interfaces often resembles the PBC of Fig 13.

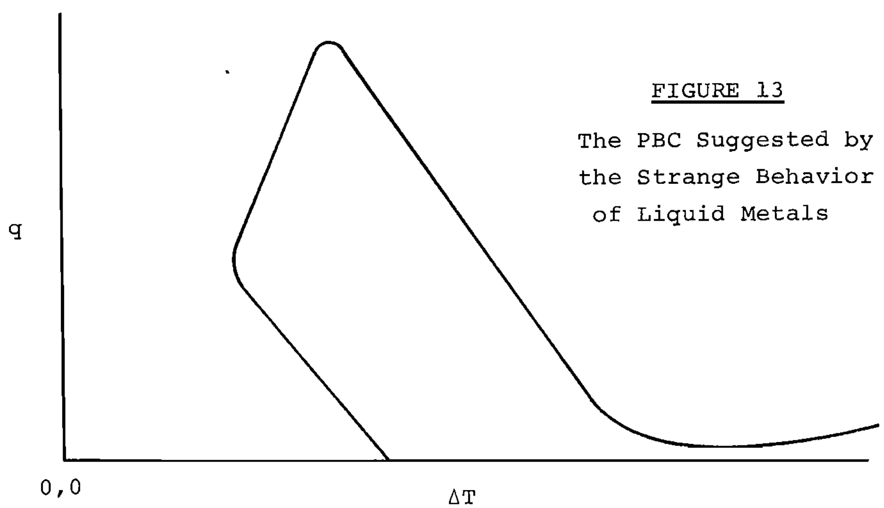


FIGURE 13

The PBC Suggested by the Strange Behavior of Liquid Metals

FORCED CONVECTION BOILING IN THE NEW HEAT TRANSFER

In the old heat transfer, pool boilers are regarded more or less as laboratory curiosities. In the new heat transfer, we recognize that a pool boiler behaves very much like a differential element of a forced convection boiler and that pool boilers can and do tell us a great deal about the behavior of forced convection boilers.

In the new heat transfer, we deal with forced convection boiling in much the same way we have dealt with pool boiling in this chapter. In other words, we use no heat transfer coefficients--no dimensional analysis--no assumed power laws--no log log graph paper. In forced convection boiling, we have a few more system parameters to deal with--such as mass flow rate, quality, length. We must also contend with local behavior rather than simply overall behavior--ie we must experimentally determine the behavior of small regions of forced convection boilers in order to infer $q\{\Delta T\}$ in a local sense which can then be integrated to determine overall boiler performance.

But the main task in forced convection boiling is essentially the same as in pool boiling--the graphical or analytical determination of $q\{\Delta T\}$ and the parametric effect of the system parameters on the function $q\{\Delta T\}$.

CONDENSATION IN THE NEW HEAT TRANSFER

In the new heat transfer, we deal with condensation in much the same way as with pool boiling. We use no heat transfer coefficients, no dimensional analysis, no assumed power laws, no log log graphs. The task is essentially the same as the task in pool boiling--the determination of $q\{\Delta T\}$ and the parametric effect of the system parameters.

REGIMES IN THE NEW HEAT TRANSFER

In the old heat transfer, highly nonlinear phenomena are generally broken up into more or less linear regimes which are analyzed separately. However, it should be noted that this is not the only way to deal with highly nonlinear phenomena. There are at least two other ways which are as effective as the invention of small regimes:

1. Obtain an analytic expression which describes the full spectrum of the nonlinear behavior

2. Obtain a graphical expression which describes the full spectrum of the nonlinear behavior

Method 1 is oftentimes undesirable because the complexity of the required analytic expression would often prevent rather than promote understanding and good design/analysis. Method 2 does not suffer from the shortcomings of regimes which tend to give a disjointed picture of natural phenomena nor does it suffer from the numbing complexity of analytic expressions generally required to describe highly nonlinear behavior. The end result is that Method 2 will find considerable use in the new heat transfer and there will be a marked decrease in the number and importance of regimes.

One difficulty with regimes and regime-oriented correlations may be seen by noting that there are 4 boiling regimes in Fig 13. If we generate an analytic expression for each of these regimes and add the natural convection regime and its analytic expression, the graphical interpretation of these five analytic expressions is shown in Fig 14:

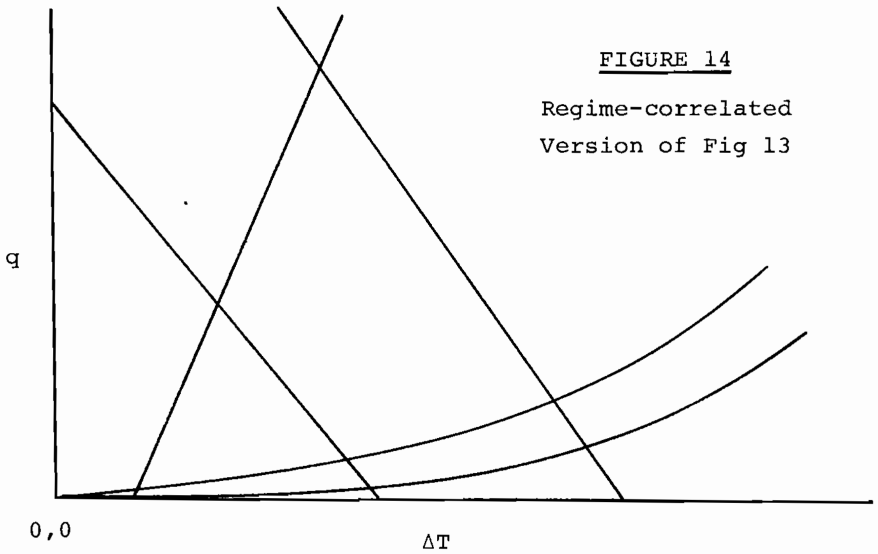


FIGURE 14

Regime-correlated Version of Fig 13

Figure 14 is the "picture" of pool boiler phenomena which results from research/experimentation/correlation in the regime-oriented old heat transfer. This "picture" is then given to the designer/analysts to be used in the design/analysis of real equipment. It is obvious that the "picture" represented by the regime-oriented Fig 14 is nowhere near as useful for design/analysis as the "picture" represented by the regime-free Fig 13.

RESULTS AND CONCLUSIONS

In this chapter, we have applied the concepts of the new heat transfer to the problem of pool boiling and have found that the pool boiling curve of the old heat transfer bears little resemblance to the pool boiling curve of the real world. We have found that the differences arose largely from a priori deduction, dimensional analysis, power laws, and log log graphs--and that by simply avoiding these old heat transfer concepts, we quite readily obtain results which are in much better agreement with behavior in the real world.

In this chapter, we dealt in considerable detail with pool boiling and we largely neglected forced convection boiling and condensation. My intent was to illustrate the methods of the new heat transfer by applying them in detail to pool boiling and to suggest without proof that these same methods work equally well with both forced convection boiling and condensation. (This omission is corrected in the later volumes of this book.)

In a much larger sense, my intent was to suggest that a great many twentieth century engineering correlations were obtained in much the same manner as the nucleate boiling exponent was determined to be "3 or 4" in the old heat transfer even though the data of the real world strongly indicate that this exponent is unity. And to suggest that all the power laws of the old heat transfer and the old engineering must be reviewed to determine whether they truly reflect the data of the real world or whether they merely reflect the utilization of log log graph paper resulting from dimensional analysis and assumed power laws.

In the old heat transfer, researchers generally strive to answer the question:

What power law is suggested by the data? What power law best fits the data? What is the equation of the best straight line that can be drawn on log log graph paper? Assuming the model $y=mx^n$, what does regression analysis tell us is the "best" value of n?

In the new heat transfer, researchers strive to answer the altogether different question:

What functionality is suggested by the data? What is the equation of the best curve that can be drawn through the data plotted on linear graph paper? Assuming the model most suggested by the data, what does regression analysis tell us about the "best" value of the constants in the model?

The pool boiling discussion in this chapter is intended to illustrate the difference between the two questions and their separate answers--and to demonstrate how to answer the second question in the new heat transfer and the new engineering. It is also intended to suggest that the answer to the first question leads to conclusions which are not necessarily in agreement with the real world--and this likelihood in turn suggests that all the power laws of the old engineering must be reviewed and appraised to determine that they accurately describe the real world. And we should expect this review and appraisal to be more than an academic exercise--we should expect to find a good many surprises--as surprising as the realization that the nucleate boiling exponent is not "3 or 4" because it is essentially 1.

SYMBOLS

a	a constant
D	diameter
g	gravity constant
h	heat transfer coefficient or enthalpy
k	thermal conductivity
n	a constant
Nu	Nusselt number
p	pressure
PBC	pool boiling curve defined on page 10
Pr	Prandtl number
Q	heat flow
q	heat flow per unit area
Re	Reynolds number
T	temperature
ΔT	temperature difference across an interface
ΔT_{init}	ΔT to initiate boiling
ΔT_o	minimum ΔT which will sustain boiling, saturated pool
μ	viscosity
ρ	density

SUBSCRIPTS

b	refers to bubble
fg	refers to fluid-to-gas conversion
l	refers to liquid
nb	refers to nucleate boiling
v	refers to vapor
x	refers to excess temperature above saturation

REFERENCES

1. S. Nukiyama, J. Soc. Mech. Eng., 1934, Japan, 37 (206), 367
2. E. A. Farber and R. L. Scorah, Heat transfer to water boiling under pressure, Trans. ASME, 1948, 79, 369
3. F. Kreith, Principles of Heat Transfer, 1st ed., 1958, International
4. W. Ibele, Modern Developments in Heat Transfer, 1963, Academic
5. J. P. Holman, Heat Transfer, 3rd ed., 1972, McGraw-Hill
6. W. H. McAdams, Heat Transmission, 3rd ed., 1954, McGraw-Hill
7. F. Kreith, Principles of Heat Transfer, 3rd ed., 1973, Intext
8. W. M. Rohsenow, A method of correlating heat-transfer data for surface boiling of liquids, Trans ASME, 1952, 74, 969
9. W. M. Rohsenow and J. P. Hartnett, Handbook of Heat Transfer, 1973, McGraw-Hill
10. R. J. Armstrong, Temperature difference in nucleate boiling, Int Jour Ht Mass Transfer, 1966, 9, 1148
11. P. J. Berenson, Experiments on pool-boiling heat transfer, Int Jour Ht Mass Transfer, 1962, 5, 985
12. R. H. Braunlich, Heat transfer to boiling liquids vacuum, thesis, MIT, 1941
13. E. G. Kaulakis and L. M. Sherman, thesis, MIT, 1938
14. C. Corty and A. S. Foust, Surface variables in nucleate boiling, CEP Symposium Series, 1955, 51, No. 17, p 1
15. R. Schnurmann and M. G. C. Lardge, Enhanced heat flux in non-uniform electric fields, Proc. R. Soc. Lond., 1973, 334, 71
16. C. P. Colver and R. E. Balzhiser, A study of saturated pool boiling potassium up to burnout heat fluxes, CEP Sym Series, 1965, 61, No. 59
17. P. J. Marto and W. M. Rohsenow, Nucleate boiling instability of alkali metals, Trans ASME, 1966, 88C, 183
18. R. B. Mesler and J. T. Banchero, Effect of superatmospheric pressures on nucleate boiling of organic liquids, AIChE Journal, March 1958, p 102
19. C. W. Perry, thesis, John Hopkins Univ, 1940
20. M. T. Cichelli and C. F. Bonilla, Trans AIChE, 1945, 41, 755
21. C. Corty, thesis, Univ Mich, 1951
22. S. Levy, Generalized correlation of boiling heat transfer, Trans ASME, 1959, 81C, 37

INTRODUCTION TO CHAPTER 8, THERMAL STABILITY

In Chapter 4, we took up the subject of the stability of the heat flow process--"thermal stability"--in an elementary way. We considered only the steady-state behavior of heat transfer (heat flow) equipment and showed that heat flow equipment would exhibit thermal stability only if the equipment behavior were such that the criterion

$$(dq_{in}/dT_i)_{ss} \; <^{n} \; (dq_{out}/dT_i)_{ss} \qquad (1)$$

were satisfied at all locations within the equipment. In criterion 1, the subscript i refers to an arbitrary location within the equipment, the subscript ss refers to steady-state, and the symbol $<^{n}$ signifies that the criterion is a necessary criterion for stability.

In Chapter 4, we showed that criterion 1 is necessary for thermal stability, but we did not show that it alone is sufficient to guarantee thermal stability. A sufficient condition for thermal stability is that all necessary criteria be satisfied. Thus if cr 1 were the only necessary criterion for thermal stability, it would then be both necessary and sufficient. On the other hand, if criteria other than cr 1 must also be satisfied, then cr 1 would be necessary but not sufficient to guarantee thermal stability. Since in Ch 4 we did not investigate the instantaneous behavior of the equipment, we had to assume the existence of an instantaneous stability criterion which might or might not be more restrictive than the steady-state criterion expressed in cr 1. We therefore were forced to conclude that cr 1 is necessary but it may not be sufficient to guarantee thermal stability. For that reason, we have written cr 1 with the inequality expressed by $<^{n}$ rather than $<^{ns}$ where ns signifies necessary and sufficient for stability.

In this Chapter, we take up the slightly more difficult problem of the instantaneous behavior of heat flow equipment and note that thermal stability in an instantaneous sense requires that the criterion

$$(dq_{in}/dT_i)_{inst} \; <^{n} \; (dq_{out}/dT_i)_{inst} \qquad (2)$$

be satisfied at all locations within the equipment. (In cr 2, the subscript inst refers to instantaneous.) By considering the instantaneous or transient behavior of heat flow equipment, we are able to demonstrate that cr 1 is more restrictive than cr 2. In other words, cr 2 will always be satisfied if cr 1 is satisfied. We therefore properly conclude that cr 1 is both necessary and sufficient. In other words, when cr 1 is satisfied, cr 2 is also satisfied and, since cr 1 and cr 2 represent all the necessary criteria, it follows that the equipment will be thermally stable if cr 1 is satisfied.

It is fortunate that cr 1 is both necessary and sufficient because this result means that we can rigorously appraise the thermal stability of real equipment by considering only steady-state behavior and altogether ignoring transient behavior. Thus we can rigorously appraise the thermal stability of real hardware by dealing with the simple functions $q_{ss}\{T_{ss}\}$ and this is a much simpler task than dealing with the functions $q\{T,t\}$.

The demonstration that cr 1 is both necessary and sufficient concludes the theoretical part of this chapter. We then take up the application of cr 1 to design/analysis problems dealing with real hardware.

THE TWO TYPES OF STABILITY

In Chapter 4, we derived a criterion for thermal stability based solely on the steady-state characteristics of the equipment. In a very real sense, the criterion described the "steady-state stability" of the equipment. However, since we ignored the transient characteristics of the equipment, we could not conclude that the steady-state criterion would guarantee stability in an overall sense--ie we could not conclude that "steady-state stability" was equivalent to stability in an overall sense. The possibility exists that the "transient stability" of the equipment is not described by cr 1 and that transient stability is more difficult to obtain than steady-state stability. In other words, the possibility exists that the equipment is stable in a steady-state sense and unstable in a transient sense--in which case we conclude that the equipment is unstable in an overall sense in spite of the fact that it exhibits steady-state stability. Thus overall stability requires both steady-state and transient stability.

To digress for a moment, those who are familiar with fluid flow stability may recall that fluid flow systems are often stable in a steady-state sense and unstable in a transient sense. In such cases, the steady-state stability causes the equipment to remain in the vicinity of the steady-state operating point while the transient instability causes the equipment to operate in an oscillatory manner about the steady-state operating point. In fluid flow systems, transient instability is caused by compressibility in certain regions of the system. The transient stability can be improved by removing the subject compressibility which in turn eliminates the oscillatory behavior. However, oscillatory behavior can also result from steady-state instability, in which case the removal of compressibility would not eliminate the oscillatory behavior.

Steady-state stability means that the equipment, given sufficient time, will finally generate a force which will oppose perturbations and tend to bring the system back to the unperturbed condition. Transient or instantaneous stability means that the equipment will instantly generate a force which will oppose perturbations. In real systems, it is possible to have either, neither, or both types of stability. When we refer to a "stable" system, we mean a system which exhibits both steady-state and transient stability--ie stable equipment will resist perturbations in both the short run and the long run.

We therefore conclude that the sufficient condition for thermal stability is that cr 1 and cr 2 be satisfied in order that the equipment resist perturbations in both the short run and the long run.

TWO TYPES OF DERIVATIVES

In stability analysis, it is very helpful to invent two types of derivatives--"steady-state derivatives" and "instantaneous derivatives" which we define as follows:

$$(dy/dx)_{ss} = dy_{ss}/dx_{ss} = \Delta y_{ss}/\Delta x_{ss} \text{ as } \Delta \to 0 \qquad (3)$$

$$(dy/dx)_{inst} = dy_{inst}/dx_{inst} = \Delta y_{inst}/\Delta x_{inst} \text{ as } \Delta \to 0 \qquad (4)$$

The reason it is helpful to invent these derivatives is that they permit us to rigorously appraise the stability of real hardware without dealing with t in a general way. For instance, in real thermal systems, we must write T{q,t} in the vicinity of thermal storage locations and the function T{q,t} is much more difficult to deal with than the simple function T{q}. In a very real sense, the derivatives in eqs 3 and 4 permit us to deal with T{q} at t=0 and t=∞ in place of dealing with T{q,t} in a general way. This may be seen by noting that we might also have defined the derivatives in eqs 3 and 4 in the following way:

$$(dy/dx)_{ss} = \Delta y/\Delta x \quad \text{as } \Delta\to 0,\ t\to\infty \qquad (5)$$

$$(dy/dx)_{inst} = \Delta y/\Delta x \quad \text{as } \Delta\to 0,\ t\to 0 \qquad (6)$$

With regard to thermal systems, the difference between these two types of derivatives may be seen by considering the thermal system described in Figs 1 and 2.

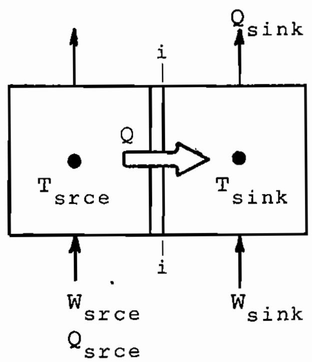


FIGURE 1

Simple Heat Flow System

(Differential Element)

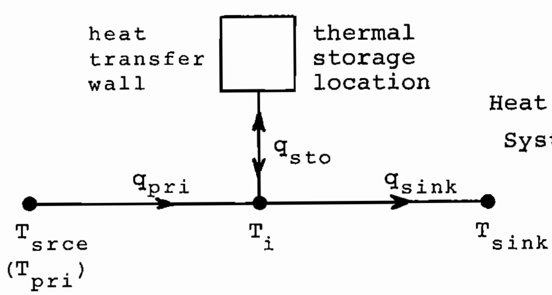


FIGURE 2

Heat Flow Circuit for System in Figure 1

It is obvious from Fig 2 that at all times t,

$$q_{sink} = q_{pri} - q_{sto}$$

Therefore at t=0 we may write

$$(dq_{sink}/dT_i)_{inst} = (dq_{pri}/dT_i)_{inst} - (dq_{sto}/dT_i)_{inst}$$

Figure 2 also shows that q_{pri} is uniquely determined by T_i--ie the transient behavior of the thermal storage locati shown can have no effect on $q_{pri}\{T_i\}$. Therefore we may write

$$(dq_{pri}/dT_i)_{inst} = (dq_{pri}/dT_i)_{ss}$$

Now in the steady-state, $q_{sto}=0$, allowing us to write

$$(dq_{sink}/dT_i)_{ss} = (dq_{pri}/dT_i)_{ss}$$

Combining eqs 6, 7, and 8 yields

$$(dq_{sink}/dT_i)_{inst} = (dq_{sink}/dT_i)_{ss} - (dq_{sto}/dT_i)_{inst}$$

Eq 9 illustrates the difference between steady-state and instantaneous derivatives in thermal systems. It should further be noted that, since heat capacity is positive for all known materials, it follows that

$$(dq_{sto}/dT_i)_{inst} > 0$$

for the case where q_{sto} is initially 0. Combining eqs 9 and 10 gives the important result that

$$(dq_{sink}/dT_i)_{inst} < (dq_{sink}/dT_i)_{ss}$$

for the case where q_{sto} is initially 0. Eq 11 is the relationship which allows us to prove that cr 1 is both necessary and sufficient for thermal stability. (The storage is off line in Fig 2 because this is the worst case for instantaneous thermal stability. In line storage yields $(dq_{in}/dT_i)_{inst} = -\infty$.)

THE NECESSARY AND SUFFICIENT CRITERION FOR THERMAL STABILITY

Criteria 1 and 2 are both necessary for thermal stability. Moreover, since they constitute all the necessary conditions, we may properly conclude that the necessary and sufficient condition for thermal stability is that criteria 1 and 2 be satisfied.

However, before concluding that we must in general consider both criteria 1 and 2, let us determine whether one of these criteria is always more restrictive than the other. If so, we can then select the more restrictive criterion and state that this single criterion is both necessary and sufficient for thermal stability.

In this vein, let us uncouple the system shown in Figs 1 and 2 at a point just downstream of T_i as shown in Fig 3. Plane ii is very close to location i which allows us to state that

$$T_{at\ plane\ ii} = T_i \qquad (12)$$

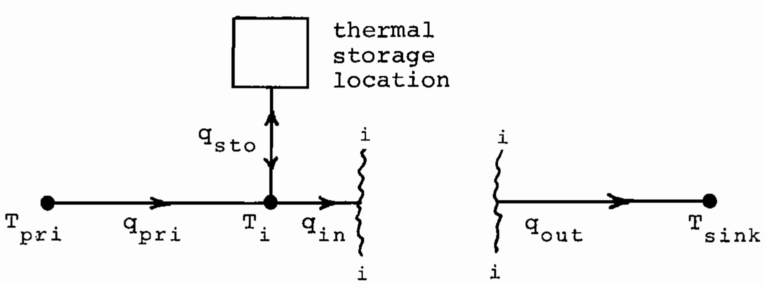


FIGURE 3

Uncoupling the System in Figure 2

Since there are no storage locations between T_i and T_{sink}, we may write

$$(dq_{out}/dT_i)_{inst} = (dq_{out}/dT_i)_{ss} \qquad (1$$

From eq 11, we may write

$$(dq_{in}/dT_i)_{inst} < (dq_{in}/dT_i)_{ss} \qquad (1$$

Combining cr 1 with eqs 13 and 14, we may write (15):

$$(dq_{in}/dT_i)_{inst} < (dq_{in}/dT_i)_{ss} <^{n} (dq_{out}/dT_i)_{ss} = (dq_{out}/dT$$

The above expression tells us that criterion 2 will always be satisfied if criterion 1 is satisfied. Therefore crite is both necessary and sufficient for thermal stability and we may write

$$(dq_{in}/dT_i)_{ss} <^{ns} (dq_{out}/dT_i)_{ss} \qquad (1$$

where the symbol $<^{ns}$ denotes that criterion 16 is both necessary and sufficient. We therefore obtain the fortuna result that the thermal stability of real equipment can be rigorously appraised solely on the basis of the simple functions $q_{ss}\{T_{ss}\}$.

It is interesting to note that the opposite result is obtained in the field of fluid flow stability. The instan aneous criterion for fluid flow is more restrictive than t steady-state criterion and therefore we may not rigorously appraise fluid flow stability solely on the basis of the simple functions $W_{ss}\{P_{ss}\}$--as we shall see in The New Flui

Before leaving the theory of thermal stability, it must be that controllers can be used to artificially induce therma stability. If such a controller is used and exhibits negl time lag--ie if it responds in a time which is small compa the time constant of the thermal storage location--then cr accurately applies to the controlled system. If the contr time lag is not negligible, then cr 16 will NOT be necessa and sufficient. A consideration of systems containing con of finite time lag is beyond the scope of this work. Howe we can deal with the problem in a practical way if we unde that instantaneous stability can be artificially induced o by controllers which exhibit negligible time lag.

APPLYING THE GENERIC CRITERION FOR THERMAL STABILITY TO THE DESIGN/ANALYSIS OF REAL HARDWARE

Criterion 16 is the generic criterion for thermal stability--ie it is the necessary and sufficient condition which must be satisfied in order that heat flow equipment will resist thermal perturbations in both the short run and the long run. Because it contains only steady-state derivatives, criterion 16 states that the thermal stability of real hardware can be rigorously appraised without considering transient parameters--ie we can ignore thermal inertia and thermal capacity when appraising thermal stability.

"Hard" systems contain no storage locations whereas "soft" systems contain finite storage. All real systems are soft systems. Within this framework, criterion 16 states that the thermal stability of hard systems is the same as the thermal stability of soft systems. Therefore, we may treat real systems as though they were hard systems and still obtain a completely rigorous result with regard to thermal stability.

The fluid flow stability of soft fluid flow systems is not necessarily the same as that of hard fluid flow systems. Thus the appraisal of fluid flow stability requires that real systems be treated as soft systems which of course is a more difficult problem. The appraisal of fluid flow stability is also more difficult because inertial effects must be considered in the flow of real fluids whereas there are no inertial effects in the "flow" of "heat". (In the new heat transfer, we think and analyze in terms of the "flow" of "heat"--and this more or less intangible "heat" has no mass and thus there are no inertial effects in "heat flow".)

Criterion 16 is an analytical tool and a design tool. In an analytical sense, it is used to appraise the thermal stability of real hardware in the following way:

1. Draw a heat flow circuit of the real hardware and ignore thermal storage
2. Uncouple the heat flow circuit at any location (interface) where $(dq_{out}/dT_i)_{ss} < 0$
3. Determine $q_{in}\{T_i\}_{ss}$ and $q_{out}\{T_i\}_{ss}$ analytically or graphically.
4. Determine $(dq_{in}/dT_i)_{ss}$ and $(dq_{out}/dT_i)_{ss}$ analytically or graphically.

5. Determine whether criterion 16 is satisfied. If it is satisfied, the hardware is thermally stable at the potential steady-state operating point being considered. Otherwise, the equipment is thermally unstable at the potential steady-state operating point being considered. (We take up the behavior of thermally unstable equipment in Chapter 9.)

In most practical applications, the only difficult step in the above is the determination of $(dq_{in}/dT_i)_{ss}$. This derivative answers the question:

> If it were possible to vary T_i independently, how would the steady-state flow of heat into location i respond to T_i, assuming that the equipment were left to its own devices (ie assuming no operator action)?

The reason this step is sometimes difficult is that it requires an understanding of the equipment behavior in an overall sense--ie it is <u>not</u> sufficient to understand merely the thermal aspects of the equipment--one must also underst the other processes which take place in the equipment and how they affect the heat flow process.

For instance, if the source of heat in the equipment is electrical, nuclear, chemical, or whatever, we must underst enough about these processes to be able to determine how T_i affects these processes and how these effects in turn affect the flow of heat into location T_i. Unless we are able to do this, we will be unable to determine $(dq_{in}/dT_i)_s$ or to appraise the thermal stability of the equipment.

For example, suppose we are dealing with a pool boiler wherein the boiler plate is "resistance" heated by electric The value of $(dq_{in}/dT_i)_{ss}$ is determined by the following chain:

> T_i affects the temperature of the boiler plate which affects the electrical resistivity of the boiler plate which affects the heat dissipation rate in the boiler plate which affects q_{in} and largely determines $(dq_{in}/dT_i)_{ss}$

Now if the temperature coefficient of electrical resistivit is positive and if the electrical power supply is such that it maintains a constant voltage drop across the boiler plate, then an increase in T_i will obviously increase the electrical resistance. This increase in resistance will decrease the electrical heat dissipation in the boiler plate since

$$Q_{elect} = V^2/R_{elect} \quad (17)$$

$$V = \text{constant} \quad (18)$$

The end result is that an increase in T_i brings about a decrease in Q which is equivalent to saying that

$$(dq_{in}/dT_i)_{ss} < 0 \quad (19)$$

Had the power supply been a constant current power supply, we would have obtained the opposite result--

$$(dq_{in}/dT_i)_{ss} > 0 \quad (20)$$

Had the power supply included a controller set to maintain a constant value of $(V_{elect}I_{elect})$--ie a constant power--we would have obtained

$$(dq_{in}/dT_i)_{ss} = 0 \quad (21)$$

From the analysis leading to eqs 19-21, we may conclude that the thermal stability of such a boiler can be optimized by designing the boiler plate to exhibit a large, positive temperature coefficient of electrical resistivity if the power supply is constant voltage. Alternatively, we could accomplish the same end by designing the power supply in such a way that

$$dQ_{elect}/dT_i < 0 \quad (22)$$

This could be accomplished by incorporating a thermostatic control in the power supply with the controller sensing the temperature somewhere in the boiler plate, preferably near the boiling interface.

That it would be easily possible to design an electrically heated pool boiler which would operate stably at all points of the boiling curve was first suggested by me in reference 1:

> A more important result of the derivation of the thermal stability criterion is the realization that the designer can vastly improve the stability of equipment by the use of simple design techniques. For instance, it was previously thought that with electrical heat input to the boiler plate . . . it would be impossible to avoid the temperature discontinuity (at the maximum in the boiling curve). (Criterion 16) shows that this is not true and that the discontinuity could be avoided altogether by selecting a material with a sufficiently large, positive temperature coefficient of resistivity. . A number of other equally simple techniques can be used to improve the stability of (nuclear) reactors and other types of heat transfer equipment.

As far as I know, Peterson and Zaalouk (2) were the first to pursue my suggestion to a conclusion in hardware. They built a pool boiler employing electrical heat generation in a wire and, as I had suggested would be easily possible, they did indeed find that

> No heater burnout can occur in the use of the new system since operation over the peak point is reliable and consistent.

Thus, in 1964, the new heat transfer demonstrated that it would be easily possible to design and build an electricall heated pool boiler which would not experience the phenomeno of "burnout". (At that time, the old heat transfer held that such an accomplishment was not possible--that burnout was inevitable in such a boiler.) In 1971, the boiler designed and constructed by Peterson and Zaalouk provided the empirical demonstration that my 1964 prediction was qualitatively and quantitatively correct.

THERMAL STABILITY--OLD WAY

In Chapter 4, I noted that the old heat transfer deals with thermal stability in an offhand, more or less intuitiv way which leads to results and conclusions which are largel incorrect. If the reader will refer to a modern heat trans text and look up the discussion of the maximum in the boiling curve and transition boiling, he will note that the intuitive reasoning of the old heat transfer is based on the inaccurate assumption that $(dq_{in}/dT_i)_{ss}$ is either zero or infinity. It therefore leads to the erroneous conclusio that the designer can exert little design influence on the thermal stability of the equipment--that he must accept either zero or infinity, depending on whether the heat source in the equipment is thermal which gives infinity or is electrical or nuclear which gives zero.

The realization that the designer can exert strong influenc on the thermal stability of real hardware should bring about a new generation of heat transfer equipment which can perform feats which would be deemed identically impossible within the framework of the old heat transfer. For instance, as I suggested in 1964, it is within the realm of possibility to design a nuclear reactor in which the core is cooled by pressurized water and yet the problem of fuel "burnout" does not exist and need not be guarded against by the reactor protective system. In the old heat transfer, this would be deemed identically impossible and no design effort would be expended in this direction.

Similarly, the intuitive reasoning of the old heat transfer leads to the conclusion that thermal stability is assured in a pool boiler utilizing a thermal heat source such as a condensing vapor or a moving fluid stream. Thus texts on the old heat transfer contain statements such as

> With a 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.

In the new heat transfer, it is readily apparent that such statements simply are not true and I demonstrated this point analytically in my 1964 article. Since that time, a number of researchers have published the empirical evidence that such statements are not true and thus that my 1964 analysis was qualitatively and quantitively correct.

In the old heat transfer, the analysis of thermal stability is actually based on analysing only part of the equipment--that part downstream of the boiling interface. It then views the temperature of this interface as "fixed" if the equipment contains a thermal heat source--and fails to recognize that there must be a temperature difference between the heat source and the boiling interface in order that heat will flow into the boiling interface--and fails to recognize that this heat flow is related to the temperature difference and therefore that $(dq_{in}/dT_i)_{ss}$ is finite. Similarly, the old heat transfer views electrical and nuclear heat input in the equipment as though these processes were altogether uncoupled from the heat transfer process--when in fact these processes are coupled in the equipment and thus the one affects the other, providing us with a powerful method of improving the thermal stability of hardware in which the heat source is electrical or nuclear.

In the old heat transfer, transition boiling is viewed as an unstable process. In the new heat transfer, we recognize that there is no such thing as an unstable process--it is the behavior of the equipment which may or may not be stable. With proper equipment design, any process can be made to occur in a stable manner. Thus it is the equipment design which determines stability or instability--stability is in the hands of the designer and it is not something that is predetermined by nature.

APPLYING THE GENERIC CRITERION FOR THERMAL STABILITY--

EXAMPLE 1 POOL BOILER WITH THERMAL HEAT SOURCE

Problem

A pool boiler is purchased from the Marshall Equipment Co. for the purpose of boiling fluid Y. From pool boiling studies performed at the Doe HTL, it is known that the PBC for fluid Y resembles Fig 12, pg 25, Ch 7 and that

$$-950 < (dq_{out}/dT_i)_{Y,bi} < 10,000 \text{ B/hr ft}^2 \text{ F} \qquad (23)$$

when fluid Y is boiled on a surface like that in the Marsha boiler. The problem is to determine whether the Marshall boiler can operate stably at all points on the PBC.

Equipment description

1. The Marshall pool boiler resembles Fig 2, pg 10, Ch 7.
2. The heat source for the boiler is a plant steam line connected to the steam chest which is an integral part of the pool boiler and which is directly below the boiler plate.
3. The thermal behavior at the steam/boiler plate interfac is such that the heat flow at this interface is describ by $q_{si} = 1700\ \Delta T_{si} = 1700(T_{sc} - T_{si})$ (24

   where sc stands for steam chest and si stands for steam/boiler plate interface.
4. The steam temperature in the steam chest is unaffected by the heat flow through the boiler. (Note that this i a simplifying assumption which is convenient for illus- tration but is not really necessary. Without this assumption, we would have the additional complication of having to account for the effect of variable T_{sc} on $(dq_{in}/dT_i)_{ss}$. This assumption allows us to ignore the equipment upstream of the steam chest and thus to begin our analysis at the steam chest.)
5. The pool boiler is shipped from Marshall with three interchangeable boiler plates of the same material but different thickness. The intent is that the purchaser install the plate of his choice when assembling the bo
6. The pool boiler is vented and thus the temperature of the boiling liquid is unaffected by the heat flow through the boiler.

Analysis

Uncoupling the system at the boiling interface in the pool boiler, we obtain Fig 4:

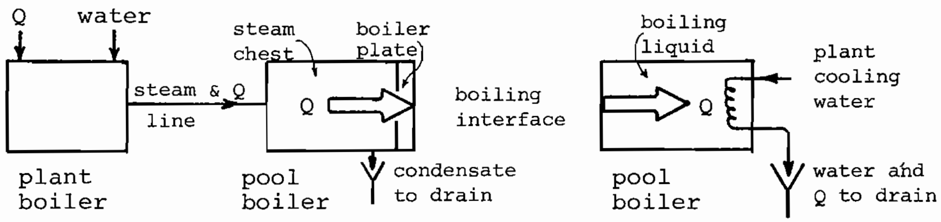


FIGURE 4

Uncoupling the System in Example 1

The given conditions 4 and 6 simplify the analysis because they tell us that the pool boiler behavior is unaffected by the equipment behavior upstream of the steam chest and downstream of the boiling liquid. Therefore we may restrict our analysis to the region between the steam chest and the boiling liquid in the pool boiler. The heat flow circuit for this region is given by Fig 5:

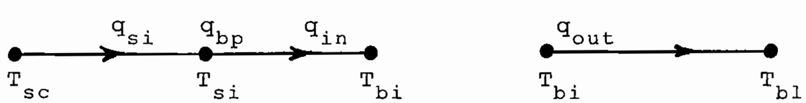


FIGURE 5

Steady-state Heat Flow Circuit for Example 1

From Fig 4, it can be seen that the heat flow behavior from the steam chest to the boiling interface depends on the thermal behavior at the si interface and through the boiler plate. The thermal behavior at the si interface is defined in eq 24. The thermal behavior of each boiler plate is inscribed on the boiler plates:

$$q_{bp1} = 500\ \Delta T_{bp1} = 500(T_{si} - T_{bi})_{bp1} \qquad (25)$$

$$q_{bp2} = 1500\ \Delta T_{bp2} = 1500(T_{si} - T_{bi})_{bp2} \qquad (26)$$

$$q_{bp3} = 3000\ \Delta T_{bp3} = 3000(T_{si} - T_{bi})_{bp3} \qquad (27)$$

Using the new heat transfer to appraise the thermal stability of the boiler with bpl installed, we note that

$$q_{si} \doteq q_{bpl} = q_{in} \qquad (28)$$

$$1700(T_{sc} - T_{si}) = 500(T_{si} - T_{bi}) = q_{in} \qquad (29)$$

$$T_{si} = (17T_{sc} + 5T_{bi})/22 = .773T_{sc} + .227T_{bi} \qquad (30)$$

Combining eqs 29 and 30, we obtain

$$q_{in} = 500(.773T_{sc} + .227T_{bi} - T_{bi}) \qquad (31)$$

$$dq_{in}/dT_{bi} = 500(-.773) = -387 \text{ B/hr ft}^2 \text{ F} \qquad (32)$$

Repeating the analysis for bp2, we obtain

$$dq_{in}/dT_{bi} = -797 \text{ B/hr ft}^2 \text{ F} \qquad (33)$$

Repeating the analysis for bp3, we obtain

$$dq_{in}/dT_{bi} = -1085 \text{ B/hr ft}^2 \text{ F} \qquad (34)$$

Using eqs 23, 32, 33, and 34 to evaluate stability from criterion 16, it can be seen that criterion 16 is satisfied at all points of the fluid Y PBC only if boiler plate 3 is installed.

Result

The Marshall pool boiler can operate stably at all points on the fluid Y pool boiling curve only if boiler plate 3 is installed. Design engineering therefore recommends that boiler plate 3 be installed at assembly and the other boiler plates be placed in storage.

APPLYING THE GENERIC CRITERION FOR THERMAL STABILITY-- EXAMPLE 2 POOL BOILER WITH ELECTRICAL HEAT SOURCE

Problem

A pool boiler is required for operation at 200,000 B/hr ft^2. At this design point, the thermal behavior at the boiling interface is such that

$$(dq_{bi}/dT_{bi}) = -300 \text{ B/hr ft}^2 \text{ F} \quad (35)$$

The Marshall Equipment Company recommends their Model E design in which the heat source is electrical resistance heating in the boiler plate. The problem is to determine whether the boiler can operate stably at the design condition represented by eq 35 and a heat flow of 200,000 B/hr ft^2.

Equipment description

1. The electrical power supply for the boiler puts out a constant voltage across the boiler plate independent of the current--ie independent of the power--ie independent of the heat flow through the boiler.
2. The temperature coefficient of electrical resistivity ε is .004/F for the boiler plate in the Model E design.
3. In the Model E design, the boiler plate is extremely thin and is constructed of a material which has a large thermal transmittance. A good approximation of the thermal behavior of the boiler plate is

$$q_{bp} = \infty \, \Delta T_{bp} \quad (36)$$

(It should be noted that this is a simplifying assumption which is convenient for illustration but that the problem can readily be solved even without this assumption. In this problem, eq 36 is not only convenient, it is also reasonably accurate in the sense that the result would be little affected if we substituted a more practical behavior for eq 36.)

Analysis

Using the new heat transfer, we note that

$$(dq/dT_{bp}) = (dq/dR_{bp})(dR_{bp}/dT_{bp}) \quad (37)$$

Since the power supply is constant voltage, we may write

$$q = q_{dc}(R_{dc}/R) \quad (38)$$

$$dq/dR = q_{dc}R_{dc}/(-R^2) \quad (39)$$

where R_{dc} refers to the resistance of the boiler plate at the design condition and q_{dc} refers to the heat flow into the boiling interface at the design condition. Now

$$R = R_{dc}(1 + \varepsilon(T - T_{dc})) \quad (40)$$

$$dR/dT = \varepsilon R_{dc} \quad (41)$$

Combining 37, 39, and 41 and noting from 36 that $T_{bp}=T_{bi}$ gives

$$(dq_{bi}/dT_{bi}) = -\varepsilon q_{dc} = -200{,}000\varepsilon = -800 \text{ B/hr ft}^2\text{F} \quad (42)$$

Using eqs 35 and 42 to appraise the stability at the design condition, criterion 16 shows that the boiler would indeed be stable at the design condition.

Result

The Marshall Equipment Company's Model E boiler can operate stably at the design condition represented by eq 35 and a heat flow of 200,000 B/hr ft^2.

Discussion

This example proves that electrically heated boiler plates can operate stably in the transition region of the pool boiling curve. I think the reader will not find a single modern (1973) text on the old heat transfer which would even suggest that this would be within the realm of what is physically possible.

APPLYING THE GENERIC CRITERION FOR THERMAL STABILITY-- EXAMPLE 3 POOL BOILER WITH ELECTRICAL HEAT SOURCE AND A THERMOSTATIC CONTROLLER WITH SENSOR IN THE BOILER PLATE

Problem

A pool boiler is required for operation at 200,000 B/hr ft^2. At this design condition, the thermal behavior of the boiling interface is such that

$$(dq_{bi}/dT_{bi}) = -3100 \text{ B/hr ft}^2 \text{ F} \qquad (43)$$

The Marshall Equipment Company recommends their Model EC design in which the heat source is electrical resistance heating in the boiler plate and the power supply includes a thermostatic controller with the sensor mounted on the back face of the boiler plate. The problem is to determine whether the Model EC boiler can operate stably at the design condition represented by eq 43 and a heat flow of 200,000 B/hr ft^2.

Equipment description

1. The temperature coefficient of electrical resistivity is 0 for the boiler plate in the EC design.
2. The thermal transmittance behavior of the EC boiler plate is given by

$$q_{bp} = 1200\ \Delta T_{bp} = 1200(T_{sen} - T_{bi}) \qquad (44)$$

   where sen denotes sensor.
3. The controller is such that it maintains the temperature at the sensor within a negligible band of the set point--ie

$$dT_{sen}/dq_{in} = dT_{sen}/dT_{bi} = 0 \qquad (45)$$

Analysis

From one dimensional "conduction" and eq 44, we may write

$$q_{in} = 2400(T_{sen} - T_{bi}) \qquad (46)$$

$$dq_{in}/dT_{bi} = 2400((dT_{sen}/dT_{bi}) - 1) \qquad (47)$$

Combining eqs 45 and 47, we obtain

$$dq_{in}/dT_{bi} = -2400 \text{ B/hr ft}^2 \text{ F} \qquad (48)$$

Using eqs 43 and 48, criterion 16 shows that the boiler would not be thermally stable at the design condition represented by eq 43 and a heat flow of 200,000 B/hr ft^2. We therefore conclude that not even the "perfect" controller assumed in this example is adequate to permit stable operation of the boiler at the design condition.

Result and design recommendation

The model EC boiler can not, as-built, operate stably at the design condition above. Stable operation at the design condition requires that the equipment be modified. Two possible "fixes" are: relocate the sensor closer to the boiling interface; decrease the thickness of the boiler plate. Both fixes result in a decreased temperature diffe between T_{sen} and T_{bi} at a given q_{in}, thus increasing the constant in eq 46. This constant reappears in eq 48 where it indicates that the increase in the constant results in improved thermal stability.

DISCUSSION

In Chapter 7, we noted that Berenson (3)obtained very litt data in the transition region of the PBC in spite of the f that his Sc. D. thesis title was "Transition Boiling Heat Transfer from a Horizontal Surface". The reason Berenson obtained little transition region data was that his boiler was usually thermally unstable in the transition region. The result of this thermal instability (as we shall see in the next chapter) was that the boiler refused to operate in most of the transition region--ie there was very pronou hysteresis in the performance of his boiler. The main rea for this thermal instability was that the boiler plate was intentionally made very thick --over 2"--in order to promo a large temperature drop through the boiler plate. As sho in Example 1, this design feature virtually guaranteed tha it would not be possible to obtain the desired data. Exam 1 shows that Berenson's boiler design could have been vast improved simply by decreasing the thickness of the boiler plate.

Based on the old heat transfer, the thickness of the boile plate should have had nothing to do with the thermal stabi of the boiler. Thus Berenson's boiler design was optimum in the old heat transfer even though it is far from optimu in the new heat transfer.

CONCLUSIONS

1. Criterion 16 is the generic criterion for thermal stability--ie it is the necessary and sufficient condi which must be satisfied in order that heat transfer equipment exhibit thermal stability.

2. Criterion 16 and Example 1 demonstrate that thermal stability is not assured in equipment with thermal heat sources.

3. Criterion 16 and Example 2 demonstrate that proper des can result in thermally stable equipment even if the heat source is electrical or nuclear.

4. Criterion 16 demonstrates that stability is a matter of equipment design--ie stability depends on design and not nature.

5. The examples in this chapter illustrate the manner in which the generic criterion for thermal stability is applied. The examples also demonstrate that the rigorous appraisal of the thermal stability of real equipment requires only elementary mathematics--ie requires nothing more than an understanding of simple derivatives.

NEW SYMBOLS

P	pressure
R	electrical resistance
V	voltage
W	fluid flow rate
ε	temperature coefficient of electrical resistivity

NEW SUBSCRIPTS

bi	boiling interface
bp	boiler plate
dc	design condition
inst	instantaneous
pri	primary
sc	steam chest
sen	sensor
si	steam/boiler plate interface
ss	steady-state
sto	storage

REFERENCES

1. E. F. Adiutori, New theory of thermal stability in boiling systems, Nucleonics, V 22 (5),p 92 (1964)
2. W. C. Peterson and M. G. Zaalouk, Boiling curve measurements from a controlled heat transfer process, ASME Journal of Heat Transfer, Nov 1971, p 408
3. P. J. Berenson, Experiments on pool-boiling heat transfer, Int Jour Ht Mass Transfer, Vol 5, 1962, p 985

INTRODUCTION TO CHAPTER 9, DYNAMICS

In the old engineering, heat transport is unconsciously viewed as a more or less static process--thus heat "transf In the new engineering, we view heat transport as a highly dynamic process. We recognize that the heat transport process behaves very much as though the thing we call "hea were actually "flowing" through the equipment. Therefore we think of heat transport as a process involving the "flo of "heat" and this gives rise to the new generic name

heat flow

which I intend will replace the generic name "heat transfe from the old engineering. In fact, the only reason this b is not entitled <u>The New Heat Flow</u> is that it presents the replacement for the old heat transfer and not for the old heat flow. However, my future books on the heat transport process will bear titles which reflect "heat flow". For instance, the text I am writing on this new science of hea transport is entitled simply <u>Heat Flow</u> and this will disti it from old way texts which bear titles reflecting heat "transfer". In this same vein, Journals which deal primar with heat and/or mass "transfer" will change their names to reflect heat and/or mass "flow" once the transition fro old way to new way has begun in earnest.

In the remainder of this book, I use the new generic name "heat flow" and thus we take up "heat flow dynamics" in this chapter. We take heat flow dynamics to mean the dynamic behavior of heat flow equipment in general and the functional relationships between the thermal and system parameters in particular. Thus our objective is to determ $q\{T_{source}\}$, $q\{t\}$, $q\{V\}$, $T_i\{t\}$, $T_i\{q\}$, $T_i\{T_{source}\}$, etc.

In the old engineering, fluid flow dynamics is much better understood than heat flow dynamics which is hardly even suggested. Moreover, since all flow processes are quite similar and since it is easier to think in terms of a tang fluid than in terms of a more or less intangible "heat", I develop heat flow dynamics by first developing fluid flow dynamics. In this way, I also hope to establish that it i reasonable and highly rational to think of heat transport as though it were indeed the "flow" of "heat"--and to sugg that the generic name for the heat transport process be changed from "heat transfer" to a name which better fits the new science presented in this book--"heat flow".

HEAT FLOW--THE NEW WAY

Several centuries ago, heat was viewed as an "imponderable fluid"--ie a weightless fluid--ie a zero mass fluid. This strange fluid was given the name "caloric fluid". It seems likely that this view of heat resulted from the correct observation that heat does seem to "flow" from one location to another. This early concept of heat later gave way to the molecular theory of heat which views heat as energy of molecular motion.

The new heat flow does not concern itself with the microscopic nature of heat--it is concerned only with the macroscopic behavior of the heat transport process. Thus the new heat flow in no way affects the present (1974) concept of the molecular nature of heat. However, the new macroscopic view of the heat transport process is altogether different from the old view--the new heat flow is a dynamic process, the old heat transfer is a static process.

In the new heat flow, we find it useful to think of macroscopic heat transport as though it were actually the "flow" of "heat". And we not only think in this new way, we also perform analyses in this new way--as though we were analyzing the "flow" of "heat" through the equipment. Scientists once thought of heat in terms of a weightless fluid flowing from one location to another, but we need not invent a pseudo-material substance for "heat"--we simply state that "heat" seems to "flow" and that the effects of this "flow" can be measured. And we conclude that since heat behaves this way, we can deal with it most simply by thinking of it and analyzing it as though heat transport were indeed heat flow. The fact that this flowing heat has no material substance need not and does not concern us--what concerns us is that heat seems to flow--and therefore we think of it and analyze it as though heat transport were heat flow.

As we shall see in this chapter, heat flow so closely resembles fluid flow that it may reasonably be viewed as a special, simple case of fluid flow. Moreover, this close resemblance between heat flow and fluid flow demonstrates that the mental and analytical artifice of "heat flow"--ie of treating heat transport as though it were indeed the "flow" of "heat"--is both reasonable and rational.

FLUID FLOW DYNAMICS

Before taking up heat flow dynamics, it is useful to consid fluid flow dynamics in some detail because

1. In the old engineering, there is a considerable understanding of fluid flow dynamics and virtually no understanding of heat flow dynamics.
2. It is easier to think in terms of the flow of a tangible fluid rather than the flow of heat which is more or less intangible.
3. It seems reasonable to expect the behavior of all flow systems to be quite similar irrespective of the flowing medium.

In this way, we can gain an understanding of heat flow dynamics by analogy to fluid flow dynamics and this is a simpler method than approaching heat flow dynamics as a subject which is completely foreign to the old heat transfe which indeed it is.

In Chapter 7, we noted that highly nonlinear phenomena can be dealt with in any of several ways which include

1. The invention of small, more or less linear regimes
2. Analytical expressions which describe the full spectrum of behavior.
3. Graphical expressions which describe the full spectrum of behavior.

We also noted that the last method would find increasing use in the new heat flow and that there would be a marked decrease in the use of the first method which is the one which predominates in the old heat transfer.

In this chapter, we deal at length with highly nonlinear phenomena and the inputs to our analyses are graphical expressions which describe the full spectrum of the equipment behavior. But I would emphasize that the analyses do not describe a graphical technique--they describe an analytical technique--and graphical expressions are used in the analyses because their analytical counterparts woul be so complex and cumbersome that the resultant mathematic would prevent rather than promote understanding. Every problem in this chapter which we analyze with graphical expressions is analyzed in essentially the same way with analytical expressions--the only difference is that graphi expressions are more visual (and thus more readily underst than analytical expressions.

Our study of fluid flow dynamics is based on the simple fluid flow system shown in Fig 1 below. Our study of heat flow dynamics is based on the heat flow equipment described in Example 1, pg 13, Chapter 8. In order that the fluid flow system behavior closely resemble the heat flow system behavior, I have shown the fluid flow system as an open loop--ie the pump discharge is not connected to the pump suction--ie the piping does not form a closed loop. For this same reason, it is further stated that the fluid is incompressible and that the hardware is such that inertial effects are negligible.

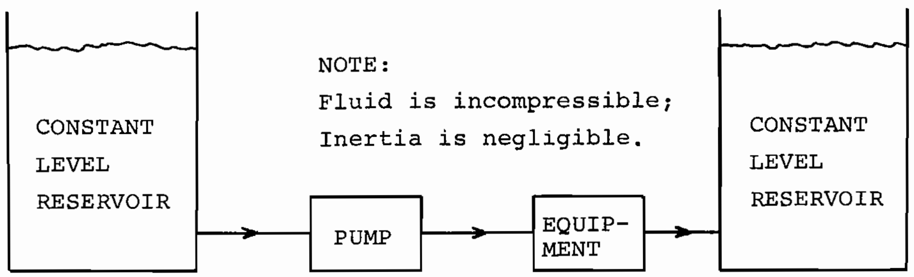


FIGURE 1

Fluid Flow System

Problem 1

If the pump in Fig 1 is turned on, what steady-state flow rate might result and would the flow rate be stable at this value?

Equipment description, Problem 1

1. The pump is an ON/OFF pump. The manufacturer's head curve (ie $P_{rise}\{W_{ss}\}$) is shown in Fig 2 below.
2. The hydraulic characteristic (ie $P_{drop}\{W_{ss}\}$) of the system external to the pump is shown in Fig 3 below.

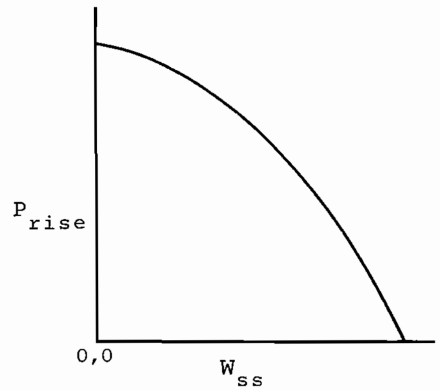


FIGURE 2

Pump head curve

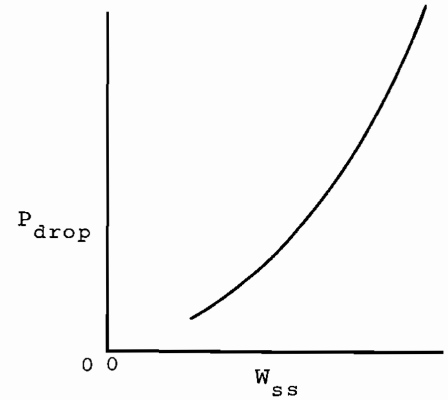


FIGURE 3

Hydraulic characteristic of system external to pump

Analysis, Problem 1

Figure 2 describes the steady-state behavior of what may be regarded as the uncoupled fluid "source". Figure 3 describes the steady-state behavior of the fluid "sink". Within this framework, Problem 1 asks the questions

> If the source in Fig 2 were coupled to the sink in Fig 3, what steady-state flow rate might result? Would the flow rate be stable at this value?

The answer to the first question is obtained by noting that, at steady-state, the pressure rise through the pump must equal the pressure drop in the system external to the pump. In other words, the steady-state flow rate must be whatever flow rate satisfies the relation

$$P_{rise}\{W_{ss}\} = P_{drop}\{W_{ss}\} \qquad (1)$$

Since in Problem 1 these functions are expressed graphically, we equate them as shown in Fig 4. The intersection in Fig 4 describes P_{rise}, P_{drop}, and W_{ss} with the pump ON and assuming that this intersection represents a stable operating point for the system.

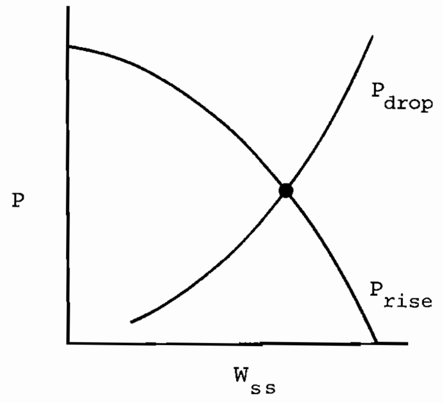


FIGURE 4

Solving eq 1 for W_{ss} in Problem 1

The appraisal of the fluid flow stability at the intersection in Fig 4 is simple and straightforward. Since we are given that the fluid is incompressible and that inertial effects are negligible, the necessary and sufficient condition for fluid flow stability in the Fig 1 system is

$$(dP_{rise}/dW)_{ss} < (dP_{drop}/dW)_{ss} \qquad (2)$$

(It must be emphasized that cr 2 is necessary and sufficient for the Fig 1 system only because this system is highly idealized. For less idealized fluid flow systems, cr 2 is necessary but not sufficient because it does not guarantee what we referred to as "instantaneous stability" in Chapter 8.) Criterion 2 is the heart of the stability analysis for Problem 1. It states that, given P_{rise} and P_{drop} in either analytical or graphical form, the appraisal of fluid flow stability in the Fig 1 system requires merely that we determine the derivatives of these functions with respect to W and then compare the relative magnitude of these derivatives using cr 2.

The derivatives in cr 2 are also the slopes of the functions in Fig 4. Therefore the appraisal of the fluid flow stability at the intersection in Fig 4 requires nothing more than an inspection of Fig 4 to determine whether the slopes are as required by cr 2. This inspection shows that the slope of the P_{rise} function is less than that of the P_{drop} function and thus the fluid flow is stable at the intersection in Fig 4. Thus the answer to problem 1 is that the steady-state flow is defined by the intersection in Fig 4 and the flow would be stable at this value.

Problem 2

Describe the fluid flow behavior of the system in Fig 1.

Equipment Description, Problem 2

1. The pump is a variable speed pump. The speed is selected by an operator setting a knob. The pump speed is uniquely determined by the knob setting. The manufacturer's pump head curves at several pump speeds are shown in Fig 5.
2. The hydraulic characteristic of the system external to the pump is shown in Fig 3, pg 5.

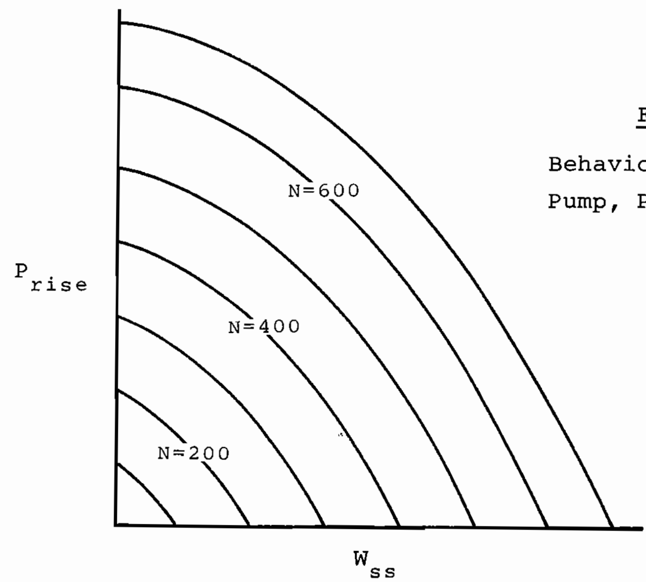


FIGURE 5

Behavior of Uncoupled Pump, Problems 2, 3, 4

Analysis, Problem 2

We again use eq 1 to determine the potential steady-state flow rates at the various pump speeds. The solutions for $W_i\{N_i\}$ are shown in Fig 6. Using cr 2 to appraise fluid flow stability, it can be seen that the fluid flow is stable at all flow rates--ie at all intersections in Fig 6.

In order to describe the system fluid flow behavior, we should describe how flow rate responds to operator action. In other words, the operator sets a knob which in turn sets the pump speed and thus we would like to determine how fluid flow rate responds to pump speed. Thus we wish to determine the function $W_{ss}\{N\}$. This function is defined by the $W_i\{N_i\}$ intersections in Fig 6. The coordinates of these intersections are plotted in the desired form $W_{ss}\{N\}$ in Fig 7. Fig 7 describes the fluid flow behavior of the system in Fig 1 and thus is the answer to Problem 2.

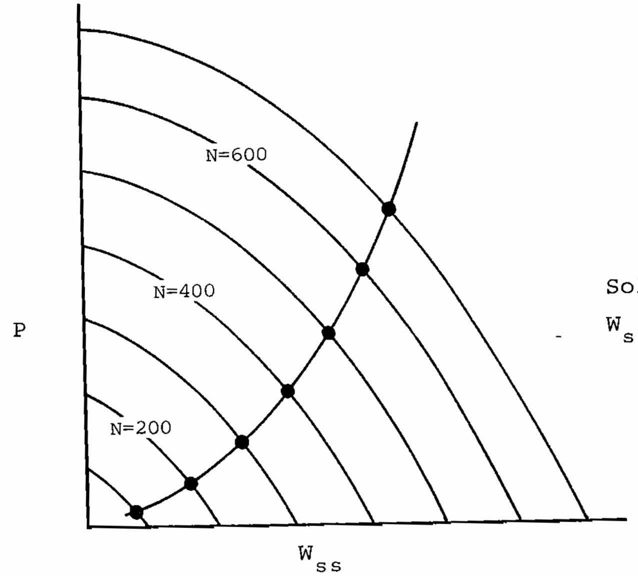


FIGURE 6

Solving eq 1 for W_{ss} in Problem 2

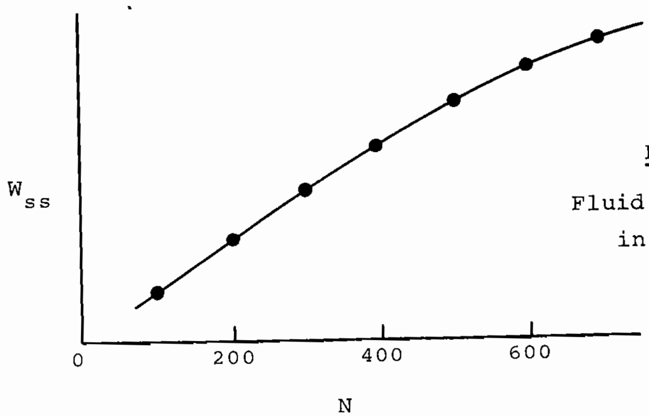


FIGURE 7

Fluid Flow Behavior in Problem 2

Problem 3

Describe the fluid flow behavior of the system in Fig 1.

Equipment Description, Problem 3

1. The pump is identical to the pump in Problem 2.
2. The hydraulic characteristic of the system external to the pump is shown in Fig 8.

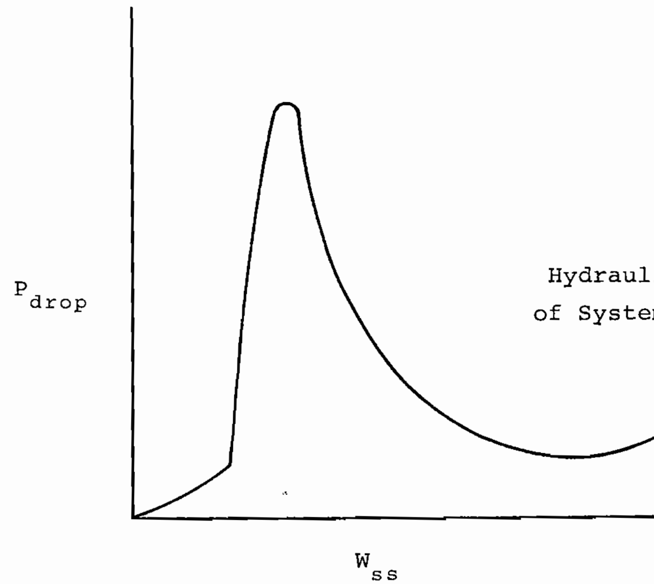


FIGURE 8

Hydraulic Characteristic of System External to Pump

Analysis, Problem 3

As in Problem 2, we solve for $W_i\{N_i\}$ and appraise the stability at each intersection using eq 1 and cr 2. The results of this analysis are shown in Fig 9. It should be noted that those intersections which do not satisfy cr 2 are marked with a "u" to denote that they represent unstable fluid flow rates.

As in Problem 2, we use the $W_i\{N_i\}$ information in Fig 9 to determine the function $W_{ss}\{N\}$ shown in Fig 10.

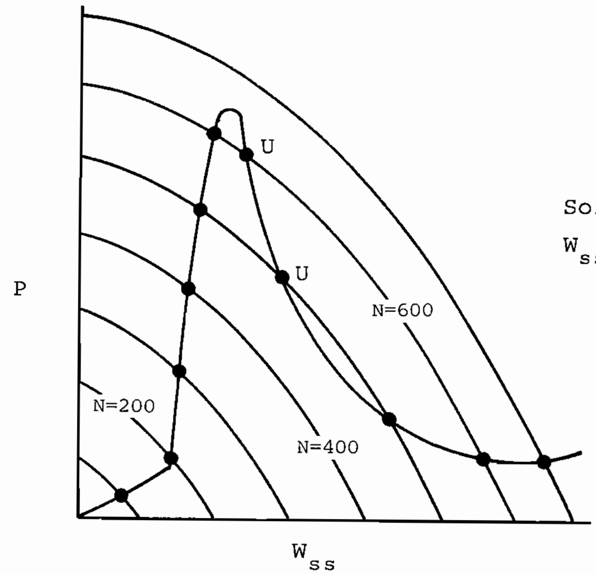


FIGURE 9

Solving eq 1 for W_{ss} in Problem 3

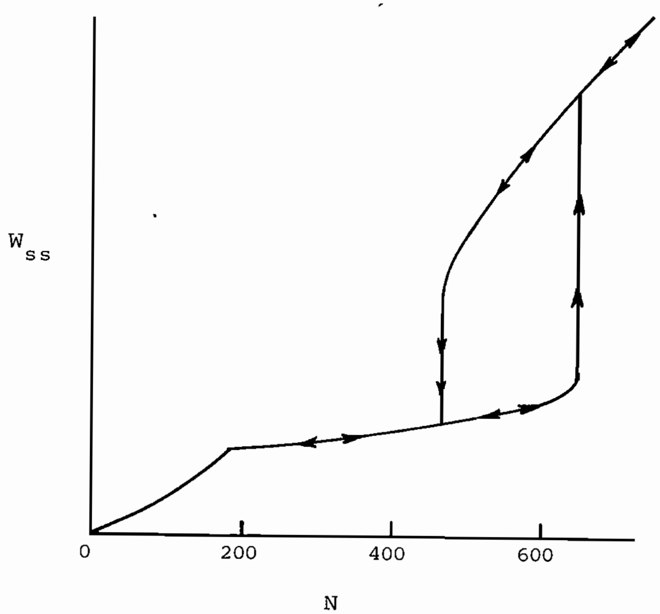


FIGURE 10

Fluid Flow Behavior in Problem 3

Let us now consider in some detail the behavior which would result if the system were somehow brought to one of the unstable conditions in Fig 9. For instance, what would happen if the system were brought to the unstable condition at a pump speed of 500 RPM? We know from our understanding of stability that the system would not resist the slightest flow perturbation--that a momentary increase in W would result in further increases and thus W would not tend to return to the value at the U intersection. Inspection of Fig 9 shows that, if W should momentarily increase above its value at the U intersection, then

$$P_{rise} > P_{drop} \tag{3}$$

(it should be recalled that P_{rise} and P_{drop} are steady-state functions). The inequality in eq 3 tells us that the flow will accelerate, since the relation

$$P_{rise} = P_{drop} + \Delta P_{acceleration} \tag{4}$$

must be satisfied at all times. Thus, as long as P_{rise} exceeds P_{drop}, the flow will accelerate. Fig 9 shows that, as W increases above its value at the U intersection, N=500, P_{rise} will exceed P_{drop} until the flow increases to the high flow intersection at N=500. At this new intersection, cr 2 is satisfied and thus the fluid flow rate would tend to remain at this high value--ie it would be stable at the high flow intersection. Similarly, a momentary decrease in W from the U intersection would cause the flow to decelerate until it reached the low flow intersection at N=500 where it would then remain because cr 2 would be satisfied. The manner in which the system would "leave" the unstable intersection at N=500 is shown in Fig 11.

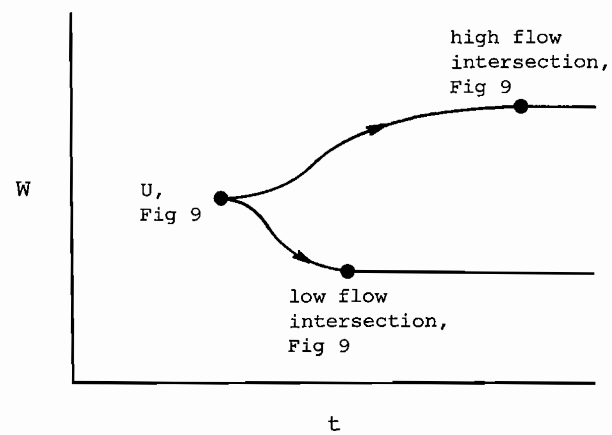


FIGURE 11

Effect of flow perturbations, system initially at U, N=500, Fig 9

Fig 11 tells us that the equipment will simply "refuse" to operate at the unstable intersections in Fig 9--that operation at N=400 or N=500 would result in operation at either of the stable intersections and not at an unstable intersection. Therefore, the unstable intersections have no effect on the function $W_{ss}\{N\}$ and are not represented in Fig 10.

Figure 10 is the solution to problem 3. Figure 10 states that the fluid flow rate is stable at all pump speeds and that there is marked hysteresis in the fluid flow behavior at pump speeds from about 450 to 650.

Problem 4

Describe the fluid flow behavior of the system in Fig 1.

Equipment Description, Problem 4

1. The pump is identical to the pump in Problem 2.
2. The hydraulic characteristic of the system external to the pump is shown in Fig 12.

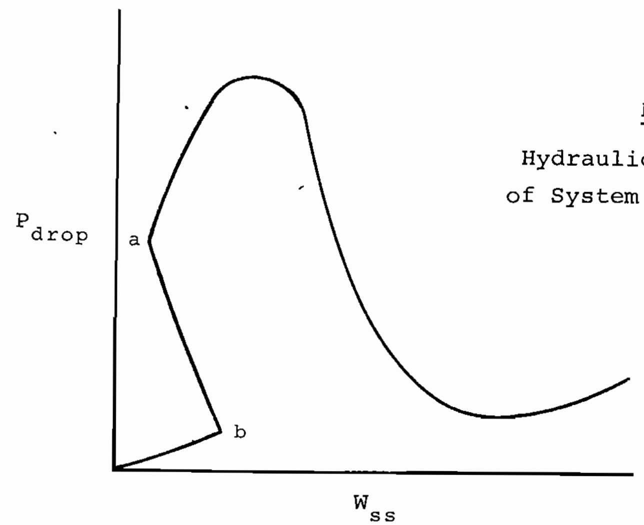


FIGURE 12

Hydraulic Characteristic of System External to Pump

Analysis, Problem 4

It should be noted that this problem differs from Problem 3 only in that region ab in Fig 12 has no counterpart in Fig 8. Thus the real problem in this problem is to determine how this ab region affects the system fluid flow behavior.

As in Problems 2 and 3, we solve for $W_i\{N_i\}$ and appraise the stability at each intersection using eq 1 and cr 2. The results of this analysis are shown in Fig 13. As before, unstable intersections are marked with a "U".

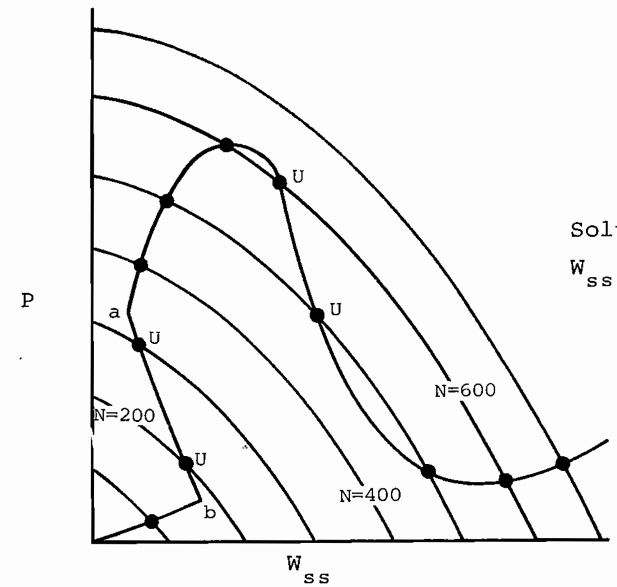


FIGURE 13

Solving eq 1 for W_{ss} in Problem 4

It should be noted in Fig 13 that the intersections in the ab region at N=200 and N=300 are unstable and that they are the only intersections at these pump speeds. (In Problem 3, the unstable intersections occurred at pump speeds which also had stable intersections at larger and smaller flow rates. Thus the result of the fluid flow instability in Problem 3 was simply that the equipment would "refuse" to operate at the unstable intersections and would, without operator action, "find" a stable flow rate as shown in Fig 11.) Therefore, in the ab region intersections, the equipment cannot "find" a stable intersection--ie a stable flow rate--and thus we must expect the flow rate to be oscillatory at all pump speeds which give intersections in the ab region. For example, if the equipment were initially at N=250 and the U intersection for this speed, a small perturbation in W would result in the flow rate eventually traversing the loop shown in Fig 14.

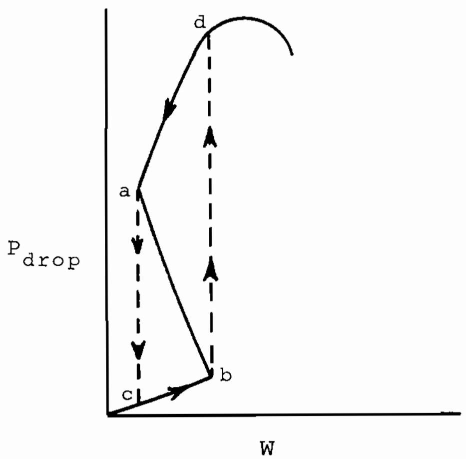


FIGURE 14

Oscillatory Behavior at ab Intersections, Prob 4

This loop is constructed by noting that, if the system were initially at any intersection in the ab region, a small positive perturbation in W would send the system to point b. At point b, $P_{rise} > P_{drop}$ and so the system must accelerate beyond W_b. However, as the flow rate accelerates beyond W_b, P_{drop} goes through a step increase to the value at point d. At point d, $P_{rise} < P_{drop}$ and so the system decelerates from point d. This process continues indefinitely around the bdac loop until the pump speed is changed to a value which does not result in an ab intersection. (It should be noted that this oscillatory behavior has nothing to do with compressibility--it results from what we called "steady-state instability" in Ch 8.)

The time-dependent behavior of W and P_{drop} on the bdac loop of Fig 14 are shown in Figs 15 and 16. The solution to Problem 4 is really Fig 17. Figure 17 indicates that the effect of the ab region in Fig 12 is an oscillatory flow rate at pump speeds from about 180 to 340.

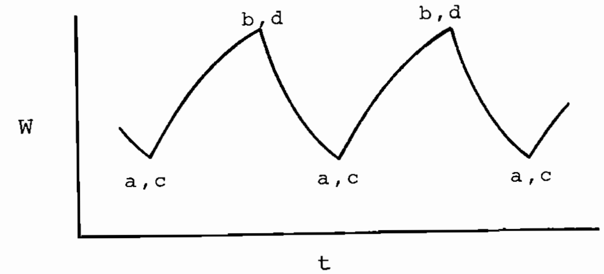


FIGURE 15

Time-dependent Behavior of W in Fig 14

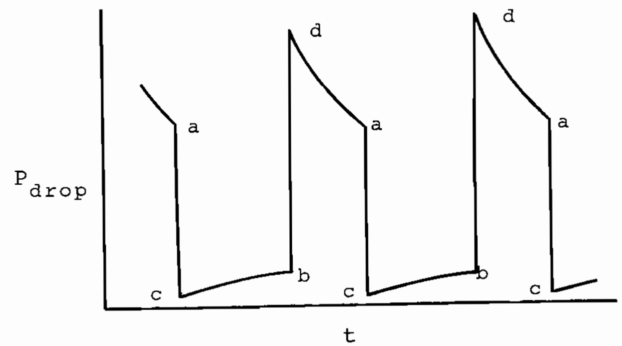


FIGURE 16

Time-dependent Behavior of P_{drop} in Fig 14

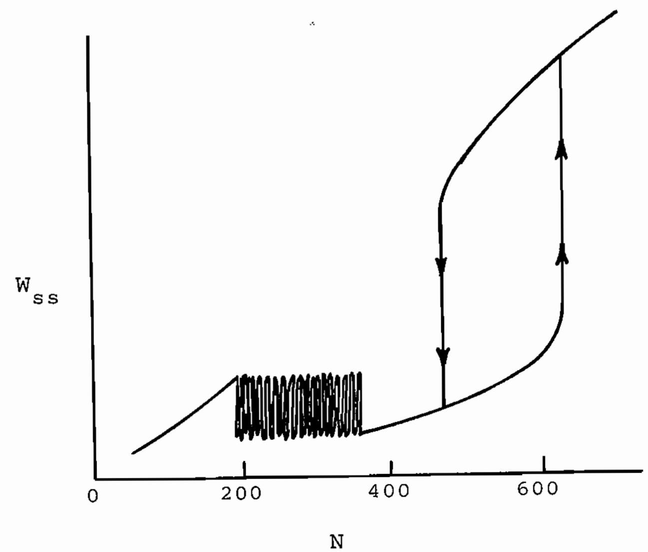


FIGURE 17

Fluid Flow Behavior in Problem 4

Problem 5

Determine P{N} in Problems 2, 3, and 4. (P refers to P_{rise} and P_{drop}.)

Analysis, Problem 5

In Problems 2, 3, and 4, $P_i\{N_i\}$ information is contained in Figs 6, 9, and 13. This information is presented in the desired form in Figs 18, 19, and 20 (next page).

FLUID FLOW INSTANTANEOUS INSTABILITY

Real fluid flow systems can satisfy cr 2 and still be unstable in an instantaneous sense. We avoid this type of instability here because there is no parallel in heat flow dynamics. As we noted in Ch 8, heat flow systems necessarily exhibit instantaneous stability if they possess steady-state stability. Therefore a discussion of fluid flow instantaneous instability would not aid our discussion of heat flow dynamics and so we avoid it.

HEAT FLOW DYNAMICS

The heat flow dynamics of real systems is essentially identical to the fluid flow dynamics of incompressible, low inertia systems such as that described in Fig 1 and analyzed in Problems 1-5. In fact, the entire discussion and analysis of fluid flow dynamics is readily transformed to heat flow dynamics. The transformation requires little more than the following substitutions:

1. Substitute the word "heat" for the word "fluid".
2. Substitute the heat flow system in Example 1, pg 13, Ch 8, for the fluid flow system in Fig 1.
3. Substitute the heat flow equipment upstream of the boiling interface for the pump
4. Substitute the heat flow equipment downstream of the boiling interface for the system external to the pump

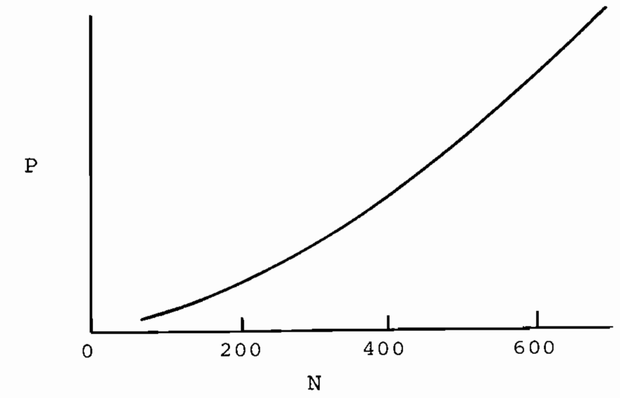


FIGURE 18

P{N} in Prob 2

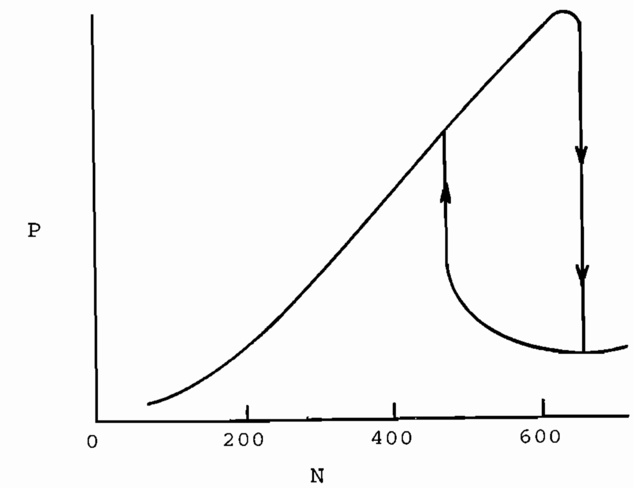


FIGURE 19

P{N} in Prob 3

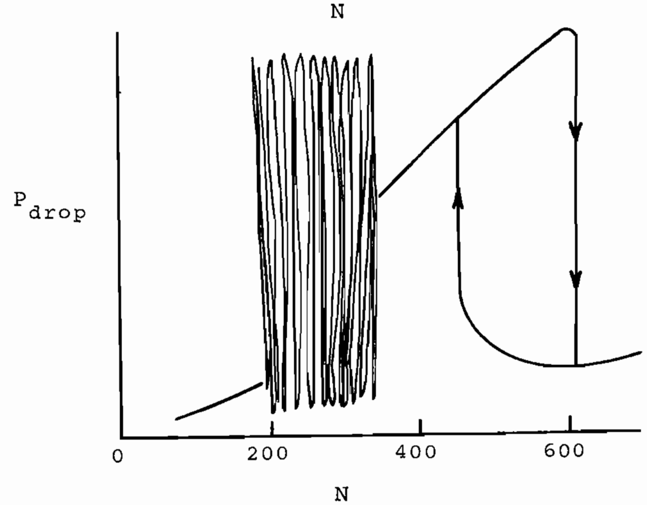


FIGURE 20

$P_{drop}\{N\}$ in Prob 4

5. Substitute Q for P; Q_{in} for P_{rise}; Q_{out} for P_{drop}; Q_{sto} for ΔP_{accel}.

6. Substitute T_{bi} for W.

7. Substitute $T_{steam\ chest}$ for N.

8. Substitute a knob which controls $T_{steam\ chest}$ for the knob which controls N.

With these substitutions, eqs 1, 2, 3, and 4 become

$$Q_{in}\{T_{bi}\} = Q_{out}\{T_{bi}\} \tag{5}$$

$$(dQ_{in}/dT_{bi})_{ss} < (dQ_{out}/dT_{bi})_{ss} \tag{6}$$

$$Q_{in} > Q_{out} \tag{7}$$

$$Q_{in} = Q_{out} + Q_{storage} \tag{8}$$

Similarly, the Figures are transformed as follows:

Fig 2 becomes $Q_{in}\{T_{bi}\}$

Fig 3 becomes $Q_{out}\{T_{bi}\}$

Fig 4 becomes $Q\{T_{bi}\}$

Fig 5 becomes $Q_{in}\{T_{bi}\}$

Fig 6 becomes $Q\{T_{bi}\}$

Fig 7 becomes $T_{bi}\{T_{sc}\}$

Fig 8 becomes $Q_{out}\{T_{bi}\}$

Fig 9 becomes $Q\{T_{bi}\}$

Fig 10 becomes $T_{bi}\{T_{sc}\}$

Fig 11 becomes $T_{bi}\{t\}$

Fig 12 becomes $Q_{out}\{T_{bi}\}$

Fig 13 becomes $Q\{T_{bi}\}$

Fig 14 becomes $Q_{out}\{T_{bi}\}$

Fig 15 becomes $T_{bi}\{t\}$

Fig 16 becomes $Q_{out}\{t\}$

Fig 17 becomes $T_{bi}\{T_{sc}\}$

Fig 18 becomes $Q\{T_{sc}\}$

Fig 19 becomes $Q\{T_{sc}\}$

Fig 20 becomes $Q\{T_{sc}\}$

It should be noted that Figs 18, 19, and 20 describe the heat flow dynamics of the subject heat flow system.

At this point, it is my intent that the reader transform the discussion and analysis of fluid flow dynamics to a discussion and analysis of heat flow dynamics as outlined above--in other words, pencil in the above substitutions and whatever other minor changes are required in the fluid flow version. This should result in an understanding of heat flow dynamics and of the strong parallel between fluid flow and heat flow--and an appreciation of the need to change the generic name "heat transfer" to the new generic name "heat flow".

PROBLEM 3 AND THE NEW PBC

The heat flow version of Problem 3 obviously deals with the pool boiling curve PBC of most liquids. (Note the close parallel between Fig 8 of Problem 3 and Fig 12 of Ch 7.) The heat flow version of Fig 19 describes the dynamic behavior of a steam-heated pool boiler in which heat is flowing into an ordinary liquid. The important thing to note is that the thermal instability of the equipment results in pronounced hysteresis in $Q\{T_{sc}\}$ even though there is no hysteresis in the PBC--ie no hysteresis in $Q\{T_{bi}\}$.

The presence or absence of the marked hysteresis in Fig 19 provides the concrete demonstration that the equipment is or is not thermally stable "at all points on the PBC". (It is interesting to note that, of the several articles which have followed my article on thermal stability, most fail to recognize this important point and thus fail to demonstrate in a conclusive way that their equipment was or was not thermally stable "at all points on the PBC".)

The pool boiler used by Berenson (1) was similar to the pool boiler in the heat flow version of Problem 3 in that it employed steam heat and the steam temperature could be varied more or less independently. With regard to hysteresis, Berenson states:

> In general, measurements were made as the temperature difference was increased and as it was decreased from point to point, in order to determine the accuracy of the measurements and search for any hysteresis in the boiling curve.

Berenson's boiling curve data--ie $Q\{T_{bi}\}$--do not show any significant hysteresis in the "boiling curve". But the fact that his results contained little transition region data strongly suggests that there was pronounced hysteresis in the $Q\{T_{sc}\}$ behavior of his boiler--and thus that the boiler was usually thermally unstable in the transition region. Unfortunately, Berenson presents no $Q\{T_{sc}\}$ data nor does he discuss or mention the hysteresis behavior of his boiler. This information would have provided the simple and incontrovertible evidence that his boiler was or was not thermally stable at all points on the PBC. The omission of this key data is understandable in light of the fact that there was no understanding of thermal stability at that time.

PROBLEM 4 AND THE PBC FOR LIQUID METALS

The heat flow version of Problem 4 obviously deals with the PBC postulated in Ch 7 for liquid metals. (Note the parallel between Fig 13 of Ch 7 and Fig 12 of Problem 4.) The heat flow version of Fig 20 describes the dynamic heat flow behavior of a pool boiler containing liquid which exhibits such a PBC. The heat flow versions of Figs 14, 15, and 16 describe the transient behavior of Q_{out} and T_{bi} in the "ab" region. In Fig 14, the upper part of the loop is in boiling and the lower part is in non-boiling--ie the heat flow mode in the lower part of the loop is free convection at the interface and evaporation at the free surface.

If the reader will compare Figs 14, 15, 16, and 20 with the empirical observations by Colver and Balzhiser (2) and by Marto and Rohsenow (3), he will note that these

Figures bear a remarkable similarity to the empirically observed behavior of pool boilers containing liquid metals. And the reader should rightly conclude that this close similarity substantiates and confirms the Ch 7 postulate that the PBC for liquid metals often resembles the shape shown in Fig 13 of Chapter 7.

RESULTS AND CONCLUSIONS

This chapter presents heat flow dynamics and demonstrates that it is a mathematically simple subject. Even though heat flow dynamics is completely foreign to the old heat transfer, it so closely parallels fluid flow dynamics that anyone knowledgeable in fluid flow dynamics will readily assimilate heat flow dynamics.

This chapter demonstrates that "heat" does "flow"--ie that heat transport can be dealt with simply and effectively by thinking of it and analyzing it as though it were indeed the "flow" of "heat". And this chapter strongly suggests that the new science presented in this book be given the generic name "heat flow".

CLOSING REMARKS

Perhaps the most difficult aspect of this chapter is the realization that the dynamics and stability of real hardware can be as simple as described here--that the highly sophisticated mathematics normally employed in the old engineering is not necessary to understand or to effectively deal with heat and fluid flow dynamics in general or stability in particular. Although I have not covered all the possible types of instability which can and do occur in real heat and fluid flow systems, I do believe I have been confronted with them all--and it is my first hand experience that they can all be handled with the same low level of mathematical sophistication employed in Chapters 4, 8, and 9.

The analyses in this chapter have centered about a pool boiler heat flow system. But it is my intent that this system also be viewed as a differential element of forced convection heat flow equipment. In this sense, this chapter deals with the behavior of forced convection equipment and we have in fact avoided only the minor problem of integration and not the larger problem of the behavior of forced convection equipment.

In references 4 and 5, I deal with the dynamic behavior of forced convection equipment in some detail. Unfortunately, the published version of my contribution to ref 4 was severely edited prior to publication. The unedited version is available as a monograph from the publisher of this book.

NEW SYMBOLS

N	pump speed
P	pressure
T_{sc}	steam chest temperature
W_{ss}	steady-state fluid flow rate

REFERENCES

1. P. J. Berenson, Experiments on pool-boiling heat transfer, Int Jour Ht Mass Transfer, 1962, 5, 985

2. C. P. Colver and R. E. Balzhiser, A study of saturated pool boiling potassium up to burnout heat fluxes, CEP Sym Series, 1965, 61, No. 59

3. P. J. Marto and W. M. Rohsenow, Nucleate boiling instability of alkali metals, Trans ASME, 1966, 88C, 183

4. Alkali Metals Boiling and Condensing Investigations, Quarterly Progress Report 4, prepared for NASA by GE, 1963

5. E. F. Adiutori, Non-linear heat transfer phenomena, British Chemical Engineering, Dec 1965

EPILOG

In this first volume of The New Heat Transfer, I have tried to describe the shortcomings of the old heat transfer and to erect the foundation and framework of the new heat flow in a logical and straightforward manner. Only Chapter 1 deviates from this game plan. Chapter 1 indicates that the old way concepts of thermal conductivity and radiative emissivity find their way into the new heat flow when the truth is that these old way concepts are abandoned in the new heat flow. By way of explanation, my intent in the first three chapters is to focus the reader's attention on the heat transfer coefficient and its drawbacks, even at the expense of being less than honest in the first chapter.

Volume 2 will deal in depth with research and development in the new heat flow and Volume 3 with design/analysis. This is not to say that Volume 1 is incomplete but only that it does not exhaust the subject. Volume 1 is intended to permit engineers and educators to independently choose between Fourier's concept of heat transfer and my concept of heat flow. And it is intended to permit engineers and educators to apply and to teach the new heat flow.

This book previews "the new engineering"--an engineering science based not on the proportional concepts of the 17th, 18th, and 19th centuries but on the new, nonlinear, free form concepts presented herein. This book illustrates the application of these new concepts in general by demonstrating their application in particular to the science of heat flow. The application of these new concepts to other branches of engineering science requires little more than the substitution of a different set of terminology for the heat flow terminology in this book--much as in Chapter 9 the transformation of fluid flow dynamics to heat flow dynamics required little more than the substitution of a different set of key words and symbols.

The most distinguishing feature of this new engineering is its remarkable simplicity. This simplicity will bring about an engineering education of such broad scope and such simple content that it would be deemed identically impossible from the viewpoint of the old engineering. The next few decades will bring about the transformation from old engineering to new engineering on a universal and practical scale--and will be an exciting and rewarding period for all whose lifework is engineering science.

APPENDIX 1

INTERACTIONS IN EQUATION 1, CHAPTER 1

The new science of heat flow presented in this book is based on eq 1, Ch 1:

$$q = f_1(\text{system properties})\ f_2(\text{TDF}) \qquad (1)$$

Implicit in this equation is the assumption that the effect of system properties is separable from and does not interact with the effect of TDF. In other words, f_1 in no way depends on TDF and f_2 in no way depends on system properties. This prevents us from writing correlations such as

$$q = 1.7\ \Delta T^{.21D} \qquad (2)$$

since this states that the effect of ΔT depends on the magnitude of D--ie it states that ΔT and D interact. Similarly, we must correlate on the basis of system properties evaluated at bulk temperature rather than the so-called film temperature of the old heat transfer.

It is more or less obvious that f_1 can always be expressed independent of TDF but the converse is considerably less obvious. However, we know from the old heat transfer that f_2 will seldom if ever depend on system properties. Therefore the assumption about separability implicit in eq 1 seems rational, particularly in light of the remarkable simplicity which results from this assumption.