THE NEW ENGINEERING

THIRD EDITION

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for Donna

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Preface 1

Why the new engineering is *much* better than conventional engineering.

Conventional engineering is founded on laws in the form of proportional equations such as Eq. (1a).

$$q = h\Delta T \qquad (1a)$$

Rearranging Eq. (1a) results in Eq. (1b).

$$h = q/\Delta T \qquad (1b)$$

Combining Eqs. (1a) and (1b) results in Eq. (1c).

$$q = (q/\Delta T)\Delta T \qquad (1c)$$

Eqs. (1a), (1b), and (1c) are *identical*.

Eqs. (1a), (1b), and (1c) are analogs of Eq. (2), as are other conventional engineering laws in the form of proportional equations (such as $E = IR$ and $\sigma = E\varepsilon$).

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

Eq. (2) is *never* used in pure mathematics because, if y is a nonlinear function of x, (y/x) is a *third variable*, and greatly complicates the solution of the equation. In pure mathematics, Eq. (3) is used because it *always* has only *two variables*.

$$y = f\{x\} \qquad (3)$$

In the new engineering, laws are analogs of Eq. (3). Laws like Eq. (1) are *replaced* by laws like Eq. (4), and analogs of (y/x) such as $q/\Delta T$ (aka h) are *abandoned* because they are *unnecessary* and *undesirable*.

$$q = f\{\Delta T\} \qquad (4)$$

In short, this book describes a new engineering science founded on laws that are analogs of Eq. (3) rather than Eq. (2). The new engineering science is *much* better than conventional engineering science because Eq. (3) is a *much* better foundation than Eq. (2).

PREFACE 2

Why my articles and books focus on conventional engineering methodologies and conclusions that are *wrong*.

In 1963, I transferred from GE's Knolls Atomic Power Laboratory (KAPL) in Schenectady to GE's Space Power and Propulsion Systems (SPPS) in Cincinnati. In my new position, I was in charge of a test facility that had been designed and built to obtain data that would enable the thermal and hydraulic design of a liquid metal boiler for the power plant in a space vehicle.

In the previous nine years since graduating from Yale University with a bachelor's degree in chemical engineering, my work concerned forced convection heat transfer in water-cooled nuclear reactors for naval application. Conventional engineering deals quite well with this type heat transfer because it exhibits proportional behavior—heat flux is proportional to temperature difference.

I very soon realized that conventional engineering is *not* adequate to deal with heat transfer to boiling liquid metal because this type heat transfer exhibits *highly nonlinear* behavior. That realization resulted in the new heat transfer and the new engineering.

I knew I would have to write journal articles about the new engineering, so I contacted Professor Harding Bliss (Yale) to request his advice. (He was my favorite engineering professor.) His advice was:

- Join the AIChE. (Professor Bliss was active in the AIChE, and was the editor of the *AIChE Journal* when I contacted him in 1963.)

- In articles for publication, *never say that anyone ever did anything wrong.*

(Years later, I realized that Professor Bliss was warning me that engineering journals generally reject articles that prove someone or something is *wrong*. In 1963, I was too naïve to understand what he really meant.)

I joined the AIChE, but did not agree that my articles should "never say that anyone ever did anything wrong". My articles were going to promote a new science of engineering, and I would oftentimes have to prove that someone or something was *wrong*.

In the preface to *The New Heat Transfer*, my first book, I explain that I find fault with *theories* and *concepts, not* with persons who use them.

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.

Science has *always* included theories and concepts that are *wrong*, and it would be *impossible* to right wrongs if it were necessary to "*never say that anyone ever did anything wrong".*

Everything in science should be questioned, even theories and concepts that have been globally accepted for *centuries*. *Everything* should be questioned by *everyone*, not just members of academia and researchers at national laboratories. The things that are *wrong* should be identified and corrected in the scientific literature and in text books. And the scientific literature should be open to everyone—not just members of academia and researchers at national laboratories.

Since my articles usually prove that someone or something is *wrong*, it is perhaps understandable that no article of mine has ever been published in an American engineering journal. However, in 1964, one of my articles was *favorably reviewed and accepted* for publication in the *AIChE Journal.* But it was *never* published in the *AIChE Journal.*

It was never published in the *AIChE Journal* because Professor James W. Westwater (at that time, head of the chemical engineering department at the University of Illinois) sent a letter to Professor Editor Bliss in which he requested that my article *not* be published.

Thirty years later, my article was published in *Japanese Society of Mechanical Engineers International Journal*, Series B, Vol. 37, No. 2, 1994, pp 394-402. And the article was just as correct and relevant and important in 1994 as it had been in 1964 when it was accepted/rejected for publication in the *AIChE Journal.*

The mission of every person who loves science should include:

- Identify theories/concepts/views/methodologies that are widely accepted and *wrong*.

- Submit articles for publication that identify current ways that are *wrong*, and present new ways that are *right*.

If no one ever wrote or said that someone or something was *wrong*:

- The earth would still be flat.

- The sun would still travel around the earth.

- The elements would still be earth, air, fire, and water.

- The surface of the moon and the surfaces of all stars would still be perfectly smooth.

- Outer space would still be filled with ether.

- Electricity would still be two fluids—a positive fluid, and a negative fluid.

- Heat would still be a fluid named caloric.

- Phlogiston would still be the only thing consumed in fire.

Many of my articles, and all of my books, concern a new engineering science that is better than conventional engineering science. In order to prove that the new engineering is better, I *must* prove that certain assumptions and conclusions that underlie conventional engineering science are *wrong,* and result in methodology that is unnecessarily difficult to learn and to apply.

And I *must* prove that assumptions and conclusions that underlie the new engineering science are *right*, and result in methodology that is *much* simpler to learn and to apply, and therefore more likely to result in conclusions that are right. For example:

- It is axiomatic that *different things cannot be proportional*. Ants *cannot* be proportional to giraffes. Desks *cannot* be proportional to airplanes.

- Hooke (1676) was *wrong*. Stress *cannot* be proportional to strain because stress and strain are different things. Hooke *should* have concluded that the *numerical value* of stress is proportional to the *numerical value* of strain.

- Ohm (1827) was *wrong*. Electromotive force *cannot* be proportional to electric current because electromotive force and electric current are different things. Ohm *should* have concluded that the *numerical value* of electromotive force is proportional to the *numerical value* of electric current.

- Fourier (1822) was *wrong*. Heat flux *cannot* be proportional to temperature difference because heat flux and temperature difference are different things. Fourier *should* have concluded that the *numerical value* of heat flux is proportional to the *numerical value* of temperature difference.

- If Hooke, Ohm, and Fourier had correctly concluded that only *numerical values* were proportional:

  - The parameter symbols in their equations would have represented numerical value but *not* dimension.

  - Their equations would have indicated that the *numerical value* of a parameter equals a *constant* times the *numerical value* of another parameter.

  - Their equations would have been *dimensionless* and *dimensionally homogeneous*.

  - Homogeneity would *not* have required that parameters be *created by assigning dimensions* to what were initially considered constants of proportionality—parameters such as modulus (the ratio of stress to strain), electrical resistance (the ratio of electromotive force to electric current, and heat transfer coefficient (the ratio of heat flux to temperature difference).

    When parameters such as modulus, electrical resistance, and heat transfer coefficient are used in the solution of proportional problems, they are *constants*, and problems are easy to solve. But when they are used in the solution of nonlinear problems, they are *variables*, and they *greatly complicate* problem solutions.

    When parameters such as modulus, electrical resistance, and heat transfer coefficient are *not* used in the solution of nonlinear problems (as in the new engineering), the mathematics of engineering science is *greatly simplified* because the number of variables is *decreased*.

- For almost 100 years, American heat transfer texts have generally referred to Eq. (1) as "Newton's law of cooling". It is *wrong* to say that Eq. (1) is Newton's law of *cooling* because cooling is a *transient* phenomenon, and Eq. (1) is a *steady-state* equation. A steady-state equation *cannot* describe a transient phenomenon.

$$q = h\Delta T \qquad (1)$$

- It is *wrong* to say that Eq. (1) is "Newton's law of cooling" because Newton and his contemporaries considered equations like Eq. (1) to be *irrational*. In their view of dimensional homogeneity, it is *wrong* to multiply dimensioned parameters, and Eq. (1) *requires* that dimensioned parameter h be multiplied by dimensioned parameter ΔT.

- It is *wrong* to say that Newton conceived Eq. (1) and h in 1701 because they were conceived by Fourier in the nineteenth century. (See Adiutori (1974, 1989, 1990, and 2005), and Bejan (1993 and 2013).)

- In the nineteenth century, Eq. (1) described the relationship between q and ΔT. It said that q is proportional to ΔT, and h is the constant of proportionality. Eq. (1) was also a law because it was felt that the relationship was *always* proportional.

  But since early in the twentieth century, Eq. (1) has *not* described the relationship between q and ΔT. It states that the relationship may be proportional, or linear, or nonlinear. Because "Eq. (1)" does not describe a relationship, it is *neither* an equation *nor* a law.

  Since sometime near the beginning of the twentieth century, Eq. (1) has been a *definition*. It *defines h*. It states that *h is q/ΔT*. It states that *h and q/ΔT are identical and interchangeable*. It states that *h is a symbol for q/ΔT*. And because Eq. (1) is a definition, it should be expressed in the form of a definition, as in Definition (2).

$$h \equiv q/\Delta T \qquad (2)$$

  Combining Eq. (1) and Definition (2) results in Eq. (3).

$$q = (q/\Delta T)\Delta T \qquad (3)$$

  Eq. (3) is *identical* to Eq. (1), and reveals that Eqs. (3) and (1) are analogs of Eq. (4), and h is an analog of (y/x).

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

Eq. (4) is *anathema* in mathematics because, if y is a nonlinear function of x, (y/x) is a *third variable*, and greatly complicates the solution of nonlinear equations. By the same reasoning, "Eq. (1)" is mathematically undesirable because, if q is a nonlinear function of ΔT, $q/\Delta T$ (aka h) is a *variable*, and greatly complicates the solution of nonlinear problems.

- For almost 100 years, it has been generally agreed that nucleate boiling data exhibit highly nonlinear behavior. It is *wrong* to state that nucleate boiling data exhibit highly *nonlinear* behavior because nucleate boiling data exhibit highly *linear* behavior.

  In 1964, I submitted an article to the *AIChE Journal* that analyzes nucleate boiling data, and explains that the *erroneous nonlinear conclusion* resulted from induction methodology that is *not rigorous*. The article also describes induction methodology that *is* rigorous, and applies it to nucleate boiling data in the literature. Rigorous induction methodology results in the *correct* conclusion that nucleate boiling data in the literature exhibit highly *linear* behavior.

  The article was favorably reviewed and accepted for publication by Professor Editor Bliss. Shortly before its intended publication, Professor Editor Bliss received a letter from Professor James W. Westwater, head of the chemical engineering department of University of Illinois. In the letter, Professor Westwater requested that my article *not* be published. Because of Professor Westwater's letter, my article was rejected, and *never* published in the *AIChE Journal.*

  But in 1994, *thirty years after* the article was accepted/rejected by the *AIChE Journal*, it was published in a journal of the Mechanical Engineering Society of Japan.

- In 1964, I submitted an article for publication that appraised the widely accepted conclusion that *transition boiling* data indicate a highly non-linear relationship between heat flux and temperature difference. The article states that it is *wrong* to state that the relationship in the transition boiling region is highly *nonlinear* because transition boiling data in the literature clearly indicate that the relationship is highly *linear*. The article was rejected, but was presented in 1991 at the 3rd ASME-JSME Thermal Engineering Joint Conference in Reno, Conference Proceed-ings, v 2, pp 51-58. (The 1964 version is in Appendix 3.)

- For several decades, it was widely agreed that the important parameters in film cooling are x, m, and s. It was also widely agreed that film cooling data correlate well using the dimensionless group x/ms. (x is the distance from the film coolant slot exit, m is the ratio of mass flow rate in the slot at the slot exit to mass flow rate in the mainstream at the slot exit, and s is the height of the slot.)

  Even if film cooling data correlated *perfectly* using the dimensionless group x/ms, it would be *wrong* to state that the important parameters in film cooling are x, m, and s because x/ms is *independent of m and s*.

  Note that m is inversely proportional to s. Therefore ms is independent of m *and* s. In terms of *independent* parameters, the dimensionless group x/ms is the dimensionless group $xG_{mainstream}/W'_{coolant}$. ($W'_{coolant}$ is coolant flow rate per unit slot *width*.) Therefore, if film cooling data correlated *perfectly* using x/ms, the *correct* conclusions would be:

  - The important parameters in film cooling are x, $G_{mainstream}$, and $W'_{coolant}$.
  - Parameter m (or s) has *no effect* on film cooling. (It is generally agreed that m (or s) *has* a considerable effect on film cooling. Because m is dependent on s, it would be wrong to say that m *and* s have no effect on film cooling because it would imply that m and s are independent.)

  If film cooling data do *not* correlate perfectly using x/ms, the correlating parameters should be x, $G_{mainstream}$, $W'_{coolant}$, and m (or s).

  The lesson learned is that the parameters in dimensionless groups should *always* be identified by *independent* parameters.

Preface 3

My last word (probably).

This book represents the completion of an endeavor that began in 1963 when I first realized that "heat transfer coefficient" should be *abandoned.* Since 1963, the promotion of the new engineering has been my life work. I have met stubborn resistance from the small group that controls American engineering journals, and although I am not wealthy, I have spent hundreds of thousands of dollars advertising the new engineering.

Nothing discourages me. The new engineering is an important contribution to engineering science. I am duty bound to promote its global acceptance, no matter how long it takes, or how much resistance I encounter.

Thirty years ago, in a discussion about the new engineering, a colleague said something negative about the new engineering. He then said

But don't let me discourage you.

To which I replied

Not to worry. It can't be done.

I met someone at an engineering society meeting who I had not seen for many years. He asked

Are you still promoting the new engineering?

To which I replied

If you can tell I am breathing, you will know that I still am.

Many years ago, Ventuno Press (my company) had a booth at an engineering convention/society meeting. Attendees would stroll by my booth, and some would discuss the new engineering with me. One gentleman walked up and said to me:

This has nothing to do with money, does it?

It made me very happy to know that someone sensed that my motivation has nothing to do with money.

When he was quite old, Dr. Albert Schweitzer was asked if he regretted sacrificing his life for the natives of Africa. He replied:

There was no sacrifice. I am one of the greatly privileged.

Dr. Schweitzer was a German citizen who spent much of his life in French Equatorial Africa medically caring for the natives. During the world wars, he was an enemy alien, and was taken to France to be placed in concentration camps. At one of the camps, the food was so bad that the inmates complained to the person in charge, who replied:

Our kitchen is run by some of the finest chefs in Europe, but if you inmates think you can do better, I will turn the kitchen over to you.

He turned the kitchen over to the inmates, and Dr. Schweitzer found that the food prepared by the inmates was *much* better than the food prepared by "the finest chefs in Europe". Dr. Schweitzer asked one of the inmate chefs

How is it possible that you inmates can prepare food that is so much better than the food prepared by the finest chefs in Europe?

To which the amateur replied

To be a good cook, one must know a great many things. But the most important is to do the cooking with love and care.

Conventional engineering science is the brainchild of Joseph Fourier. (See Adiutori (2005).) For 200 years, engineers have looked at the world through Fourier's eyes. The new engineering replaces Fourier's brainchild, and is a much better way to look at the world.

The new engineering made it possible for me to solve certain highly nonlinear problems that are so difficult to solve using conventional engineering that they had *never* been solved.

I could *never* have solved highly nonlinear problems using conventional engineering because the solutions are so difficult. But I was able to solve several highly nonlinear problems *quite easily* using the new engineering because it greatly simplified the solutions. The simplification resulted because the new engineering does *not* use parameters such as h, R, and E that are *variables* when dealing with nonlinear problems.

I was 30 years old when I conceived the new engineering, and I am now 84 years old. My first paper (Adiutori (1964)) was published in *Nucleonics*. (See Appendix 5.) It was a page taken from the new engineering in that it concerned heat transfer analysis *without* heat transfer coefficients.

Although the article was rigorously correct and important, it was out of the mainstream of thought, and consequently was greeted by a storm of protest that helped guarantee that none of my papers would ever be published in an American journal of engineering. (None were.)

Shortly after publication, *Nucleonics* received a letter from seven "experts" (presumably Ph. D. experts) who worked at the Argonne National Laboratory. The letter stated:

The undersigned, having read "New Theory of Thermal Stability in Boiling Systems" (NUCLEONICS, May, 1964, pp. 92-101), conclude that this article must be either a hoax, or that the paper reviewing procedures followed by NUCLEONICS are in need of evaluation.

H. K. Fauske/J. B. Heineman/B. M. Hoglund/P. A. Lottes/
J. F. Marchaterre/R. R. Rohde/R. P. Stein

The Letters section of the December 1964 issue of *Nucleonics* included:

- A sanitized version of the Argonne letter[1].
- A list of alleged errors in the article.
- Letters from international heat transfer "experts" who had been asked by *Nucleonics* whether they were inclined to agree with me, or with the Argonne Seven.
- My reply.

The verdict was *unanimous*. *Everyone* agreed with the Argonne Seven that my article "was totally unsatisfactory". The last two paragraphs of my published reply to the Argonne letter states:

Now of course no one can be expected to utilize knowledge which does not even exist. On the other hand, every effort must be made to understand and utilize new knowledge no matter what quarter it comes from. That my concept of thermal stability has not emanated from the universities or the

[1] The original letter was revised to "The undersigned . . . find it (my article) totally unsatisfactory." The number of signers was also revised from seven to four.

national laboratories is perhaps regrettable. However, it is no less true that it represents new knowledge to the science of heat transfer.

It is now incumbent on the universities and those who control the scientific literature to see to it that this new knowledge is not wasted—and to make it possible for me to publish my work in a less brief and more satisfactory manner. Whether they shall be equal to the task is a matter for their conscience—not mine.

I am chagrined to report that, for more than fifty years, "those who control the scientific literature" definitely did *not* "make it possible for me to publish my work in a less brief and more satisfactory manner". The truth is, they made it *impossible* for me to publish my work in the normal manner—in engineering journals.

I am also chagrined to report that my 1964 concept of thermal stability has still *not* found its way into most (if not all) American heat transfer texts. For example, a 755 page text published in 2011 entitled *A Heat Transfer Text Book* by Lienhard (University of Houston) and Lienhard (MIT) makes *no mention of thermal stability*—no mention of the fact that heat transfer is a *dynamic* process that can and sometimes does result in hysteresis and undamped oscillations. By default, heat transfer texts generally indicate that heat transfer is a *static* process.

(In the 18th century, Edward Jenner was a country doctor who invented the world's first vaccine—a vaccine for smallpox, a disease that killed an estimated 300 to 500 million people in the twentieth century. (Smallpox was eradicated worldwide in 1980.) Although he was only a country doctor, Edward Jenner was elected a Fellow of the Royal Society of London in 1788, following the Society presentation and publication of his study of the cuckoo.

Even though he was a Fellow of the Society, when he proposed to speak about his smallpox vaccine at a Society meeting, the proposal was *rejected.* Cows are used in the preparation of Jenner's vaccine, and the "scientists" who controlled the Society considered it demeaning to suggest that animals could be useful in the medical treatment of humans.

Jenner paid no attention to the "scientists" who controlled the Society. He went to a printer in London and ordered 500 copies of a pamphlet that described how to prepare and administer his vaccine. He mailed the *free* pamphlet to doctors around the world, and fortunately they were more open-minded than the "scientists" who administered the Society.)

In 1973, I gave up trying to arrange the journal publication of my many papers, all of which were out of the mainstream of thought. In 1974, I formed Ventuno Press in order to market my first book entitled *The New Heat Transfer,* Adiutori (1974).

As noted in the following from page 5-5 of Volume 1 of *The New Heat Transfer*, the title of the book is a misnomer:

And this is what The New Heat Transfer is really about—it is about the invention of concepts that deal effectively with proportional behavior, with linear behavior, and 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.

In 1975, I received a letter from the copyright agency of the USSR dated May 28. (See pages 306 - 308.) The letter states:

The "Mir" Publishers, Moscow are interested in translating and publishing in Russian of "The New Heat Transfer, eugene f. adiutori", 1974. The book is expected to be published in 1976 in 5,000 copies at the retail price of 1.70 rubles approximately.

Herewith we are enclosing in triplicate the draft agreement for acquisition of rights to translate and publish in Russian the above book. In case you will find the terms and conditions of the draft agreement acceptable, please sign it and return all the copies to us.

Yu. GRADOV, Director, Export & Import Department

It is probably not necessary to say that I found "the terms and conditions of the draft agreement acceptable". I was *elated*. I felt that the Russian edition would make it difficult for the leaders of the heat transfer world to publicly ignore me, and would make it impossible for them to publicly or even privately portray me as a crackpot. I was *wrong*.

My website, thenewengineering.com, includes narratives that describe how I have promoted the new engineering, and how others have responded. It also includes many of my published papers and books, and much of my correspondence. Everything on the site may be downloaded for personal use without charge.

The following is a slightly revised version of the preface in *The New Heat Transfer* published in 1974. It is included here because it is just as true now as it was 43 years ago when I wrote the original version.

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 engineering and its application to engineers and educators who are familiar with conventional engineering. And I have tried to present the new engineering in such a way that educators could teach it and engineers could apply it at the same time the leaders of the engineering 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 they 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 that 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 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 conventional way. Sometimes it may seem I am mocking conventional ways. I am not. My purpose is to dispel conventional 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.

Eugene F. Adiutori
July 8, 2017

NOMENCLATURE

- Parameter symbols represent numerical value *and* dimension if they refer to conventional engineering methodology.
- Parameter symbols represent numerical value but *not* dimension if they refer to new engineering methodology. Where appropriate, dimension units that underlie parameter symbols are specified in nomenclatures within the text.
- $f\{I\}$ indicates "function of I".
- $V\{I\}$ and $V = f\{I\}$ refer to an equation or graph that describes the relationship between V and I.
- $\leq_U$ indicates unstable if the inequality is satisfied.

SYMBOLS

a	arbitrary constant, or acceleration
A	area
b	arbitrary constant
c	arbitrary constant
C_p	heat capacity
d	arbitrary constant
D	diameter
E	electromotive force or elastic modulus σ/ε
f	friction factor
$f\{x\}$	function of x
g	gravity constant
h	$q/\Delta T_{BL}$ (heat transfer coefficient)
I	electric current

k	q/(dT/dx) (thermal conductivity)
L	length, or length of a copper wire of a standard diameter
m	mass or arbitrary constant
n	arbitrary constant
M	y/x, mathematical analog of parameters such as R, h, E
N	dimensionless parameter group identified by subscript
P	electric power, or pressure, or load
q	heat flux
Q	heat flow rate
R	V/I (electrical resistance)
s	distance traversed
t	time or thickness
T	temperature
U	$q/\Delta T_{TOTAL}$ (overall heat transfer coefficient)
v	velocity
V	electromotive force, emf
W	fluid flow rate
x	distance
y	arbitrary variable
β	temperature coefficient of volume expansion
ε	strain or roughness
μ	absolute viscosity
ν	kinematic viscosity

ρ density

σ stress

SUBSCRIPTS

BL refers to boundary layer

CIRC refers to circuit

COMP refers to component

COND refers to conductive

CONV refers to convective

FALL refers to a subsystem in which emf or pressure falls

Gr refers to Grashof number $g\beta\Delta TL^3/\nu^3$

IN refers to a subsystem that includes the heat source

LM refers to log mean

Nu refers to Nusselt number hD/k or equally $qD/\Delta Tk$

OUT refers to a subsystem that includes the heat sink

parallel refers to type of electrical connection

Pr refers to Prandtl number $C_p\mu/k$

PS refers to power supply

Re refers to Reynolds number $(D/A\mu)W$

RISE refers to a subsystem in which emf or pressure rises

SINK refers to heat sink

SOURCE refers to heat source

wall refers to wall

Chapter 1

An overview of the new engineering.

1 How parameters such as h, R, and E greatly complicate the solution of nonlinear problems.

Conventional engineering is founded on laws such as Eqs. (1-1a) to (1-3a), and parameters such as h, R, and E.

$$q = h\Delta T \tag{1-1a}$$

$$E = IR \tag{1-2a}$$

$$\sigma = E\varepsilon \tag{1-3a}$$

- Eq. (1-1a) states that h is $q/\Delta T$.
- Eq. (1-2a) states that R is E/I.
- Eq. (1-3a) states that E is σ/ε.

Therefore Eqs. (1-1b) to (1-3b) are *identical* to Eqs. (1-1a) to (1-3a).

- $q = (q/\Delta T)\Delta T$ (1-1b)
- $E = I(E/I)$ (1-2b)
- $\sigma = (\sigma/\varepsilon)\varepsilon$ (1-3b)

Equations (1-1) to (1-3) work well with *proportional* problems because $q/\Delta T$ (aka h), E/I (aka R), and σ/ε (aka $E_{elastic}$) are *constants*.

Equations (1-1) to (1-3) do *not* work well with *nonlinear* problems because $q/\Delta T$ (aka h), E/I (aka R), and σ/ε (aka E_{secant}) are *variables,* and *greatly* complicate solutions.

Any problem that can be solved using q, $q/\Delta T$, and ΔT can also be solved *without* using $q/\Delta T$. If a heat transfer problem concerns nonlinear behavior, it is *much* easier to solve if $q/\Delta T$ (aka h) is *not* used, as demonstrated by the example problem on the next page. (Similarly if E/I or σ/ε is *not* used in the solution of a nonlinear problem.)

1.1 Solving a moderately nonlinear problem using both conventional engineering and new engineering.

Problem statement: Fluids 1 and 2 are separated by a flat wall. Calculate the heat flux through the wall.

Conventional Eng'g.	**New Eng'g.**
Given	**Given**
$\Delta T_{total} = 120$	$\Delta T_{total} = 120$
$h_1 = .40(\Delta T_1)^{.33}$	$\Delta T_1 = 1.99q^{.75}$
$t_{wall}/k_{wall} = .05$	$\Delta T_{wall} = .05q$
$h_2 = .80(\Delta T_2)^{.50}$	$\Delta T_2 = 1.16q^{.667}$
Analysis	**Analysis**
$U = 1/(1/h_1 + t/k + 1/h_2)$	$\Delta T_{total} = \Delta T_1 + \Delta T_{wall} + \Delta T_2$
$U = 1/(1/.4(\Delta T_1)^{.33} + .05 + 1/.8(\Delta T_2)^{.50})$	$120 = 1.99q^{.75} + .05q + 1.16q^{.667}$
$U = ???$	$q = 141$

- Note that there are *three* variables (U, ΔT_1, and ΔT_2) in the second U equation, and only *one* variable in the corresponding new equation. Three variables are required because the conventional engineering solution is based on $q = h\Delta T$. Only *one* variable is required in the corresponding new equation because the new engineering solution is based on $\Delta T = f\{q\}$. (See Section 1.7.)

- The example problem demonstrates that problems that can be solved using conventional engineering and q, ΔT, U, h, and k, can also be solved using the new engineering and q and ΔT. The problem also demonstrates that, if q is a nonlinear function of ΔT, the solution using the new engineering is *much* simpler.

- Except for the last line in the table, the equations on the left are *identical* to the equations on the right. (To prove this, replace h_i by $q/\Delta T_i$, replace U by $q/\Delta T_{total}$, replace t_{wall}/k_{wall} by $\Delta T_{wall}/q$, then separate q and ΔT.)

- In order to complete the conventional solution, additional equations must be found so that the number of equations equals the number of unknowns. These equations are then solved simultaneously to determine U, then U is multiplied by ΔT_{total} to determine q.
- The conventional engineering solution is so complicated that only heat transfer engineers are likely to be able to complete the solution.
- The new engineering solution is so simple that anyone who took Algebra 1 in high school, and knows how to use Excel, can complete the solution in about a minute.

1.2 Highly nonlinear problems.
The above example problem demonstrates that *moderately* nonlinear problems can be solved more simply using the new engineering.

A *highly* nonlinear example problem was *not* selected because highly nonlinear problems are extremely difficult to solve using conventional engineering—ie using ratios such as $q/\Delta T$, E/I, σ/ε (aka h, R, E).

Many highly nonlinear problems are quite simple to solve if ratios such as $q/\Delta T$, E/I, σ/ε are *not* used. The following are examples of highly nonlinear problems that are solved quite simply using the new engineering.

Given a description of a pool boiler in which the heat source is condensing steam:

- *What stability criterion must be satisfied so that the boiler will operate stably throughout the transition boiling region?*
- *What design changes would improve the thermal stability of the boiler?*
- *What would cause undamped oscillations?*

Chapters 3, 6, and 7 demonstrate that many highly nonlinear problems can be solved quite simply if the solution is based on the new engineering.

1.3 Mathematics.
Equations (1-1b) to (1-3b) reveal that laws such as Eqs. (1-1a) to (1-3a) are analogs of Eq. (1-4).

$$y = (y/x)x \qquad (1\text{-}4)$$

Eq. (1-4) is *never* used in mathematics because, if y is a nonlinear function of x, (y/x) is a *variable,* and Eq. (1-4) contains *three variables*.

(Very early in mathematics, one learns that it is desirable to "separate the variables"—ie to ensure that *no term in an equation contains more than one variable*. If a term contains more than one variable, that term is an additional variable, and complicates solution of the equation.)

In mathematics, the preferred form of x,y equations is Eq. (1-5) because equations in this form *always have two variables*.

$$y = f\{x\} \qquad (1\text{-}5)$$

Eq. (1-6) *should* be the preferred form of q,ΔT equations because it *always* has two variables. (Similarly for E,I and σ,ε equations.)

$$q = f\{\Delta T\} \qquad (1\text{-}6)$$

In conventional engineering, *equations* like Eq. (1-6) are *not used* because they are *inhomogeneous*. Inexplicably, *charts* of equations like Eq. (1-6) *are* used even though they too are *inhomogeneous*.

1.4 The genesis of Eq. (1) and h.
American texts generally refer to Eq. (1-1a) as "Newton's law of cooling", but Eq. (1-1a) and h were conceived by Fourier. (See Adiutori (1974, 1989, 1990, and 2005) and Bejan (1993 and 2013).)

$$q = h\Delta T \qquad (1\text{-}1a)$$

From his data, Fourier concluded that, *in forced convection cooling to atmospheric air*, heat flux is *proportional* to boundary layer temperature difference. Symbolically, Fourier concluded that

$$q \ \alpha \ \Delta T \qquad (1\text{-}7)$$

in which q is the numerical value and dimension of heat flux, and ΔT is the numerical value and dimension of temperature difference.

When Proportion (1-7) is transformed to an equation, a *constant* is introduced.

$$q = m\Delta T \tag{1-8}$$

Fourier would not accept Eq. (1-8) because it is *not* dimensionally homogeneous. In order to make Eq. (1-8) homogeneous, *Fourier assigned* to *constant* m the dimension that would make Eq. (8) homogeneous[2]. He also assigned m the symbol h, resulting in the dimensionally homogeneous Eq. (1-1a).

$$q = h\Delta T \tag{1-1a}$$

1.5 Fourier's error.

It is axiomatic that different things *cannot* be proportional. For example,

- Ants *cannot* be proportional to giraffes because they are different things, and different things *cannot* be proportional.
- Desks *cannot* be proportional to airplanes because they are different things, and different things *cannot* be proportional.

From his data, Fourier concluded

Heat flux is proportional to temperature difference.

But heat flux *cannot* be proportional to temperature difference because heat flux and temperature difference are different things, and different things *cannot* be proportional.

The proportionality that Fourier observed in his heat transfer data concerned *numerical values*. The proportionality had *nothing to do with dimensions*. Fourier *should* have concluded that

*The **numerical value** of heat flux is proportional to the **numerical value** of temperature difference.*

---

[2] In Fourier's view of dimensional homogeneity, it was rational to assign dimensions to constants. In the modern view, it is *irrational* to assign dimensions to constants. Eqs. (1-1) to (1-3) should now be considered irrational because they are the result of irrationally assigning dimensions to constants.

1.6 The impact of Fourier's error.

If Fourier had correctly concluded that the *numerical value* of heat flux is proportional to the *numerical value* of temperature difference:

- Fourier's parameter symbols would have represented numerical value but *not* dimension.

- Eq. (1-8) would have been dimensionless and homogeneous *as written*, and it would have been accepted as the "law of convective heat transfer". It would have stated that the *numerical value* of heat flux equals a constant times the *numerical value* of temperature difference.

  $$q = m\Delta T \qquad (1\text{-}8)$$

- Because Eq. (1-8) would have been homogeneous *as written*, Fourier would have had no reason to assign dimension to m. Consequently, he would *not* have conceived h.

- By the same reasoning, Fourier would *not* have conceived k.

- Fourier's view of dimensional homogeneity would have been

  Rational parametric equations are inherently dimensionless and dimensionally homogeneous.

1.7 The new engineering.

In the new engineering:

- Primary parameters (such as q and ΔT, or E and I, or σ and ε) *are always separate* ***and*** *explicit*. In other words, primary parameters are *never* in ratios such as $q/\Delta T$ and E/I, and they are *never* implicit.

- Parameter symbols in rational equations represent numerical values, but *not* dimensions.

- If an equation is quantitative, the dimension units that underlie parameter symbols *must* be specified in an accompanying nomenclature.

- Because parameter symbols in equations are dimensionless, *all* rational parametric equations are inherently dimensionless and dimensionally homogeneous.

- Eq. (1-1) and h are *abandoned* and replaced by Eq. (1-9), the *law of convective heat transfer behavior*. Eq. (1-9a) states that the *numerical value* of ΔT_{BL} is *always* a function of the *numerical value* of q_{conv}.

  $\Delta T_{BL} = f\{q_{conv}\}$ (1-9a)

  $q_{conv} = f\{\Delta T_{BL}\}$ (1-9b)

- Eq. (1-2) and R are *abandoned* and replaced by Eq. (1-10), the *law of resistive electrical behavior*. Eq. (1-10a) states that the *numerical value* of I is *always* a function of the *numerical value* of E. Eq. (1-10) applies to *all* conductors whether they do, or do not, obey Ohm's law.

  $I = f\{E\}$ (1-10a)

  $E = f\{I\}$ (1-10b)

- Young's law and elastic modulus and secant modulus are *abandoned*. All three are replaced by Eq. (1-11), the *law of axial stress/strain behavior*. Eq. (1-11a) states that the *numerical value* of axial stress is *always* a function of the *numerical value* of axial strain. Eq. (1-11) applies in both elastic and inelastic regions.

  $\sigma = f\{\varepsilon\}$ (1-11a)

  $\varepsilon = f\{\sigma\}$ (1-11b)

- *All* laws that are analogs of Eq. (1-4) in conventional engineering are replaced by analogs of Eq. (1-5) in the new engineering.

  $y = (y/x)x$ (1-4)

  $y = f\}x\}$ (1-5)

- *All* parameters that are analogs of y/x (such as h, R, E) are *abandoned*. They are *not* replaced because they are *not* necessary and they are *not* desirable.

- Fluid friction factor is *abandoned* because it *implicitly* includes both primary parameters flow rate and pressure drop. Fluid friction factor equations are replaced by equations in which flow rate and pressure drop are *separate and explicit*. (See Chapters 10 and 11.)

- Group parameters in equations should be replaced by individual parameters in order to reveal parameter functionality.
- Temperature dependent parameters in correlations should be replaced by temperature so that correlations can be evaluated without reference to property tables. (See Chapter 13).
- Because all rational parametric equations are *dimensionless*, there is no sound basis for dimensional analysis.

1.8 The "dimensional equations" widely used in mid-twentieth century.

Although equations in which parameter symbols are dimensionless may now seem bizarre, they were widely used in mid-twentieth century. They were "dimensional equations", and constants in the equations were "dimensional constants". Eq. (24) is an example:

For the turbulent flow of gases in straight tubes, the following ***dimensional equation*** *for forced convection is recommended for general use:*

$$h = 16.6\, c_p (G')^{0.8} / D_i')^{0.2} \qquad (24)$$

where c_p is the specific heat of the gas at constant pressure, B.T.u./(lb.)(°F), G' is the mass velocity, expressed as lb. of gas/sec./sq. ft, . . . and D_i' is in inches Perry (1950)

Eq. (24) would be *in*homogeneous if parameter symbols represented numerical value *and* dimension. Note that the dimensions that underlie the symbols are specified in an accompanying nomenclature, as required if parameter symbols represent numerical value but *not* dimension.

Equations in which parameter symbols are dimensionless are still used, but not widely. For example, Holman (1997) lists "simplified equations" such as the following in Table (9-3). Specified units are watts, meters, and Centigrade.

$$h = 5.56(\Delta T)^3$$

1.9 The dimensional homogeneity of charts.

A chart of σ vs ε is a chart of Eq. (1-11a)

$$\sigma = f\{\varepsilon\} \qquad (1\text{-}11a)$$

If σ and ε represent numerical value *and* dimension, Eq. (1-11a) *and* σ vs ε charts are dimensionally *inhomogeneous.* Therefore in conventional engineering, they are *irrational.*

If σ and ε represent numerical value but *not* dimension, Eq. (1-11a) and σ vs ε charts are dimensionally *homogeneous.* Therefore in the new engineering, they are *rational*.

1.10 How to transform parametric equations from conventional engineering to new engineering.

In conventional engineering, primary parameters (such as q and ΔT, or E and I, or σ and ε) are often *combined and implicit* in *dimensional* groups such as h (the dimensional group $q/\Delta T$) and R (the dimensional group E/I). In heat transfer and fluid flow, primary parameters are also often combined in *dimensionless* groups such as Nusselt number ($qD/\Delta Tk$).

In the new engineering, primary parameters are *always separate and explicit.* In order to transform conventional engineering equations to new engineering equations:

- In heat transfer coefficient equations, replace h by $q/\Delta T$, U by $q/\Delta T_{total,}$ t_{wall}/k_{wall} by $\Delta T_{wall}/q$, Nusselt number by ($qD/\Delta Tk$), Stanton number by $q/\Delta T\rho C_p u$, then separate q and ΔT.
- In modulus equations, replace E by σ/ε, then separate σ and ε.
- In electrical resistance equations, replace R by E/I, then separate E and I.

1.11 A comparison of conventional and new engineering.

- In conventional engineering, parameter symbols in rational equations represent numerical value *and* dimension.

  In the new engineering, parameter symbols in rational equations represent numerical value, but *not* dimension.

- In conventional engineering, analogs of y/x, such as $q/\Delta T$ (aka h), σ/ε (aka E), and E/I (aka R), are *often* used.

  In the new engineering, analogs of y/x are *never* used.

- In conventional engineering, most laws are analogs of Eq. (1-4).

  In the new engineering, all laws that are analogs of Eq. (1-4) in conventional engineering are analogs of Eq. (1-5).

- In conventional engineering, parametric equations are dimensionally homogeneous if all terms in an equation have the same dimension.

  In the new engineering, parametric equations are *inherently* homogeneous because parameter symbols do *not* represent dimension. Consequently, *all* rational parametric equations are *inherently dimensionless*.

- In conventional engineering, dimensional analysis seems *rational* because parameter symbols in equations represent dimensions.

  In the new engineering, dimensional analysis is *not* rational because rational parametric equations are *dimensionless*. Therefore rational parametric equations contain no dimensions to be analyzed.

- In conventional engineering, moderately nonlinear problems are *difficult* to solve, and highly nonlinear problems are essentially *impossible* to solve, because parameters such as h, R, E are *variables*.

  In the new engineering, nonlinear problems are much easier to solve because variables such as h, R, E are *not* used, thereby reducing the number of variables, and making it possible to solve nonlinear problems in the simplest way.

- In conventional engineering, the focus is on *contrived* parameters such as h (contrived from the ratio $q/\Delta T$), R (contrived from the ratio E/I), and E (contrived from the ratio σ/ε).

  In the new engineering, the focus is on *real* parameters such as q and ΔT, E and I, and σ and ε. Contrived parameters such as h, R, E are *never* used.

- Because conventional engineering is founded on analogs of $y = (y/x)x$, it is necessary to learn and use mathematical methodology that little resembles pure mathematics, and complicates the solution of practical problems.

  Because the new engineering is founded on analogs of $y = f(x)$, it uses the same methodology learned in pure mathematics—methodology that makes it possible to solve *all* problems in the *simplest* way.

- Stability problems are difficult to solve using conventional engineering. Thermal stability problems were *never* solved using conventional engineering.

  Many stability problems are quite simple to solve using the new engineering. (See Chapters 3, 6, and 7.)

- The conventional engineering view of dimensional homogeneity is irrational because it is based on irrational premises such as “dimensions may be multiplied, but may *not* be added”.

  The new engineering view of dimensional homogeneity is rational because it is based on the rational premise that dimensions may not be multiplied or divided. (This is essentially the same view that was globally accepted by the science community for 2000 years, including superstars such as Euclid, Galileo, and Newton. See Chapter 16.)

- In conventional engineering, it is seldom necessary to specify the dimension units of parameter symbols in equations because rational equations are almost always dimensionally homogeneous.

  In the new engineering, if a parametric equation is *quantitative,* the dimension units that underlie parameter symbols *must* be specified in an accompanying nomenclature.

1.12 How engineering curricula are affected by the abandonment of laws such as Ohm’s law and parameters such as h, R, E.

The abandonment of conventional laws such as Ohm’s law and Fourier’s laws of heat transfer, and contrived parameters such as h, R, and E, will have the following effects on engineering curricula:

- Students will learn to focus on *real* parameters such as stress and heat flux rather than *contrived* parameters such as h (contrived from the ratio of heat flux to temperature difference), R (contrived from the ratio of electromotive force to electric current), and E (contrived from the ratio of stress to strain).

- Students will learn how to solve engineering problems with the primary parameters *separated,* the same methodology learned in mathematics, rather than *combined* in contrived parameters such as h, R, and E.

- *Throughout* their engineering education, students will learn to think and to solve problems in a way that deals effectively with *all* forms of behavior—proportional, linear, and nonlinear.

Chapter 2

Problems that demonstrate the static analysis of resistive electric circuits using the new engineering.

2 .1 Summary

This chapter contains example problems that demonstrate the static analysis of resistive electric circuits using behavior methodology—ie methodology based on the law of resistive electrical behavior, E = *f*{I}. (The dynamic analysis of resistive electric circuits is the subject of Chapter 3.)

The problems demonstrate that the static analysis of resistive electric circuits using behavior methodology is simple and direct whether circuit components exhibit proportional, linear, or nonlinear behavior.

2.2 Using the law of resistive electrical behavior, E = *f*{I}, to analyze resistive electrical components.

In the new engineering, problems that concern resistive electrical components generally include the following specified information:

- A quantitative description of the component's resistive electrical behavior in the form of Eq. (2-1a) or (2-1b), or a chart of E vs I or I vs E.

  $E = f\{I\}$ (2-1a)

  $I = f\{E\}$ (2-1b)

- A specification of the dimension units that underlie E and I.
- The value of E or I.

The analysis is as follows:

- If the value of I is given, the given equation or chart that describes E{I} or I{E} is used to determine the value of E, and conversely.
- The power dissipated in the component is determined from Eq. (2-2).

  $P = EI$ (2-2)

2.3 Using the law of resistive electrical behavior, E = *f*{I}, to analyze resistive electrical circuits.
In the new engineering, problems that concern resistive electrical circuits generally include the following specified information:

- Drawing of the circuit.
- An equation (or chart) in the form of Eq. (2-1a) or Eq. (2-1b) for each component in the circuit.
- Specification of the dimension units that underlie E, I, and P.
- The overall electromotive force or electric current.

The analysis is based on the following:

- If the components are connected in series, the electromotive forces are additive, and the electric currents are equal.
- If the components are connected in parallel, the electromotive forces are equal, and the electric currents are additive.
- The power dissipated in each component is determined from Eq. (2-2).

2.4 A preview of the problems.
Example problems (2-1) through (2-3) demonstrate the static analysis of resistive electric components using behavior methodology:

- The component in Problem (2-1) exhibits proportional behavior.
- The component in Problem (2-2) exhibits moderately nonlinear behavior.
- The component in Problem (2-3) exhibits highly nonlinear behavior.

Example problems (2-4) to (2-6) demonstrate the static analysis of series connected electric circuits using behavior methodology.

- Problem (2-4) concerns analysis of a circuit in which all components exhibit proportional behavior.
- Problem (2-5) concerns analysis of a circuit in which one component exhibits moderately nonlinear behavior. Note that the analysis differs from the analysis in Problem (2-4) only in that $I_{CIRC}\{V_{CIRC}\}$ is a

proportional equation in Problem (2-4), and a nonlinear equation in Problem (2-5).

- Problem (2-6) concerns analysis of a circuit in which one of the components exhibits highly nonlinear behavior that is described graphically. Note that the analysis differs from the analysis in Problem (2-5) only in that the analysis is performed graphically rather than analytically.

- Problems (2-7) to (2-9) differ from Problems (2-4) to (2-6) in that they concern series-parallel connected circuits instead of series connected circuits.

2.5 Example problems that demonstrate the analysis of resistive electric components using the law of resistive electrical behavior, E = *f*{I}.

2.5.1 Problem (2-1)
In Figure (2-1), what power supply emf would cause a current of 7.2 amperes? What power would be dissipated in Component A?

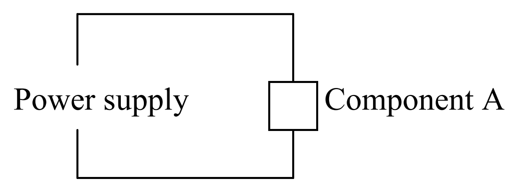


Figure 2-1 Electric system, Problems (2-1) to (2-3)

2.5.2 Problem (2-1) Given
The electrical behavior of Component A is given by Eq. (2-3).

$$V_A = 5.6\ I_A \qquad (2\text{-}3)$$

The dimension units that underlie the parameter symbols are volts, amps, and watts.

2.5.3 Problem (2-1) Analysis
- Substitute the specified value of I_A in Eq. (2-3):

$V_A = 5.6\ (7.2) = 40.3$ (2-4)

- Substitute in Eq. (2-5):

$P_A = V_A\ I_A = 40.3(7.2) = 290$ (2-5)

2.5.4 Problem (2-1) Answer
An emf of 40.3 volts causes a current of 7.2 amps in Component A. The power dissipated in Component A is 290 watts.

2.5.5 Problem (2-2) Statement
In Figure (2-1), what current would be caused by a power supply emf of 75 volts? What power would be dissipated in Component A?

2.5.6 Problem (2-2) Given
The electrical behavior of Component A is given by Eq. (2-6).

$V_A = 4.7\ I_A^{1.4}$ (2-6)

The dimension units that underlie the parameter symbols are volts, amps, and watts.

2.5.7 Problem (2-2) Analysis
- Substitute the specified value of V_A in Eq. (2-6):

$75 = 4.7\ I_A^{1.4}$ (2-7)

- Solve Eq. (2-7), and obtain $I_A = 7.23$.

- Substitute in Eq. (2-2):

$P_A = V_A\ I_A = 75(7.23) = 542$ (2-8)

2.5.8 Problem (2-2) Answer
In Figure (2-1), a current of 7.23 amps is caused by a power supply of 75 volts. The power dissipated in Component A is 542 watts.

2.5.9 Problem (2-3) Statement
In Figure (2-1), what power supply emf would cause a current of 20 amps? What power would be dissipated in Component A?

2.5.10 Problem (2-3) Given

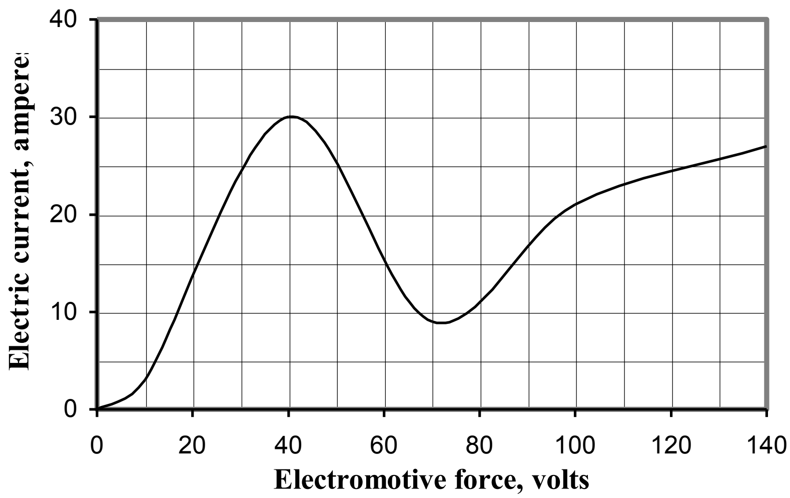
The electrical behavior of Component A is described in Figure (2-2).

The dimension units are volts, amps, and watts.

2.5.11 Problem (2-3) Analysis

- Inspect Figure (2-2) and note that a current of 20 amps would result from a power supply emf of 26, 55, or 97 volts.
- Substitute in Eq. (2-9):

Figure 2-2 Electrical behavior of Component A, Problem (2-3)



$P_A = V_A I_A$ (2-9)

$P_A = 20(26) = 520$ *or* (2-10)

$P_A = 20(55) = 1100$ *or* (2-11)

$P_A = 20(97) = 1940$ (2-12)

2.5.12 Problem (2-3) Answer

In Figure (2-2), a current of 20 amps would be caused by an emf of 26 or 55 or 97 volts. The power dissipated in Component A would be 520 or 1100 or 1940 watts. The information given is not sufficient to determine a unique solution. (See Chapter 3.)

2.6 Example problems that demonstrate the analysis of resistive electric circuits using E = *f*{I}.

2.6.1 Problem (2-4) Statement
What are the values of emf, electric current, and electric power for each component in Figure (2-3)?

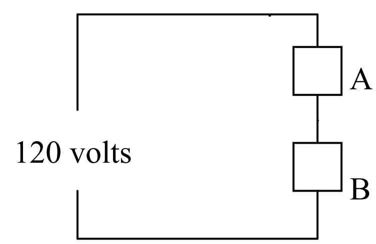


Figure 2-3 **Electric system in Problem (2-4)**

2.6.2 Problem (2-4) Given
The electrical behavior of Components A and B is given by Eqs. (2-13) and (2-14).

$$V_A = 17\ I_A \tag{2-13}$$

$$V_B = 9.4\ I_B \tag{2-14}$$

The dimension units that underlie the parameter symbols are volts, amps, and watts.

2.6.3 Problem (2-4) Analysis

- Inspect Figure (2-3) and note that, since Components A and B are connected in series, their emf values are additive, and their electric currents are equal.

  $$V_A + V_B = V_{CIRC} \tag{2-15}$$

  $$I_A = I_B = I_{CIRC} \tag{2-16}$$

- Determine $I_{CIRC}\{V_{CIRC}\}$ by combining Eqs. (2-13) to (2-15), and using Eq. (2-16).

  $$17I_{CIRC} + 9.4I_{CIRC} = V_{CIRC} \tag{2-17}$$

- Solve Eq. (2-17) for $V_{CIRC} = 120$, and obtain $I_{CIRC} = 4.55$.
- Solve Eqs. (2-13) and (2-14) for V_A and V_B:

$$V_A = 17(4.55) = 77.3 \tag{2-18}$$

$$V_B = 9.4(4.55) = 42.8 \tag{2-19}$$

- Solve Eq. (2-2) for P_A and P_B:

$$P_A = V_A\, I_A = 77.3(4.55) = 352 \tag{2-20}$$

$$P_B = V_B\, I_B = 42.8(4.55) = 195 \tag{2-21}$$

2.6.4 Problem (2-4)Answer
For Component A, the values of emf, electric current, and electric power are 77.3 volts, 4.55 amps, and 352 watts. For Component B, the values are 42.8 volts, 4.55 amps, and 195 watts.

2.6.5 Problem (2-5) Statement
What are the values of emf, electric current, and electric power for each component in Figure (2-4)?

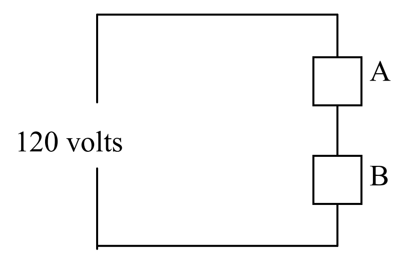


Figure 2-4 Electric system in Problem (2-5)

2.6.6 Problem (2-5) Given
The electrical behavior of Components A and B is given by Eqs. (2-22) and (2-23):

$$V_A = 3.6\, I_A \tag{2-22}$$

$V_B = 4.8\ I_B^{1.5}$ (2-23)

The dimension units that underlie the parameter symbols are volts, amps, and watts.

2.6.7 Problem (2-5) Analysis

- Inspect Figure (2-4) and note that Components A and B are connected in series. Therefore their emf values are additive, and their electric currents are equal.

  $V_A + V_B = V_{CIRC}$ (2-24)

  $I_A = I_B = I_{CIRC}$ (2-25)

- Determine $I_{CIRC}\{V_{CIRC}\}$ by combining Eqs. (2-22) to (2-24), and using Eq. (2-25):

  $3.6I_{CIRC} + 4.8I_{CIRC}^{1.5} = V_{CIRC}$ (2-26)

- Solve Eq. (2-26) for $V_{CIRC} = 120$, and obtain $I_{CIRC} = 7.26$.

- Determine I_A and I_B from Eq. (2-25).

- Substitute in Eqs. (2-22) and (2-23):

  $V_A = 3.6(7.26) = 26$ (2-27)

  $V_B = 4.8(7.26)^{1.5} = 94$ (2-28)

- Substitute in Eq. (2-2):

  $P_A = V_AI_A = 26(7.26) = 189$ (2-29)

  $P_B = V_BI_B = 94(7.26) = 682$ (2-30)

2.6.8 Problem (2-5) Answer

For Component A, the values of emf, electric current, and electric power are 26 volts, 7.26 amps, and 189 watts. For Component B, the values are 94 volts, 7.26 amps, and 682 watts.

2.6.9 Problem (2-6) Statement
What are the values of emf, electric current, and electric power for each component in Figure (2-5)?

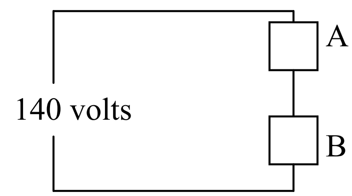


Figure 2-5 Electric system in Problem (2-6)

2.6.10 Problem (2-6) Given
The electrical behavior of Component A is given by Eq. (2-31). The electrical behavior of Component B is given by Figure (2-6).

$$V_A = 3.89\ I_A \qquad (2\text{-}31)$$

The dimension units that underlie the parameter symbols are volts, amps, and watts.

2.6.11 Problem (2-6) Analysis
- Inspect Figure (2-5) and note that:

$$V_A + V_B = V_{CIRC} \qquad (2\text{-}32)$$

$$I_A = I_B = I_{CIRC} \qquad (2\text{-}33)$$

- Determine $I_{CIRC}\{V_{CIRC}\}$ over a range that includes 140 volts:

  - Select (I_B, V_B) coordinates from Figure (2-6).

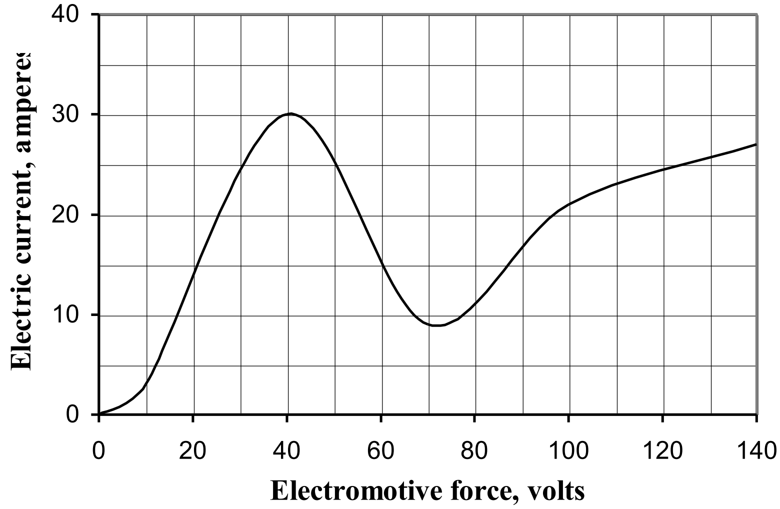
  - At each (I_B, V_B) coordinate, use Eqs. (2-31) and (2-33) to calculate $V_A(I_B)$.

  - Use Eq. (2-32) to calculate V_{CIRC}.

  - The calculated (V_{CIRC}, I_{CIRC}) coordinates are in Table (2-1).

  - Plot the $I_{CIRC}\{V_{CIRC}\}$ coordinates from Table (2-1). The plotted range must include $V_{CIRC} = 140$. The plot is Figure (2-7).

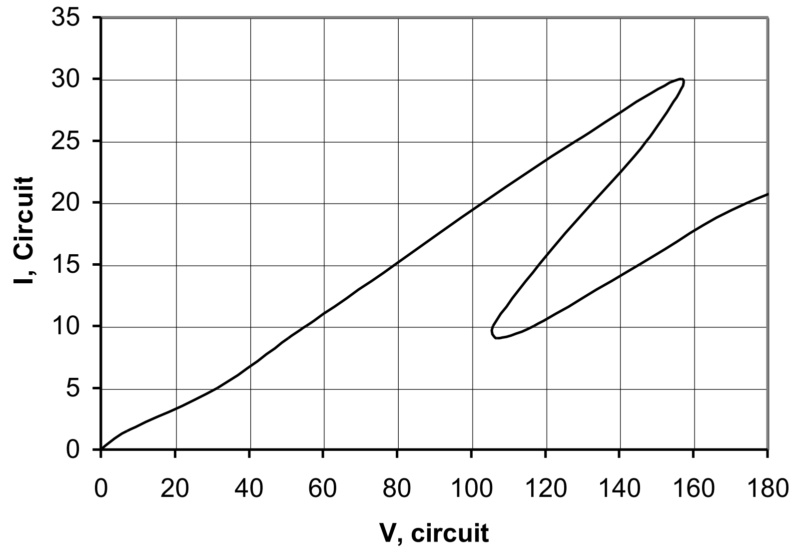
Figure 2-6 **Electrical behavior of Component B, Problem (2-6)**



I_B or I_{CIRC}	V_B	V_A	V_{CIRC}
1.5	7.0	5.8	12.8
5.0	12.0	19.5	31.5
9.0	15.6	35.0	50.6
10.0	16.5	38.9	55.4
15.0	21.0	58.4	79.4
20.0	25.5	77.8	103.3
25.0	31.0	97.3	128.3
30.0	40.0	116.7	156.7
25.0	50.0	97.3	147.3
20.0	55.0	77.8	132.8
15.0	60.0	58.4	118.4
10.0	67.0	38.9	105.9
9.0	72.0	35.0	107.0
10.0	78.0	38.9	116.9
15.0	87.0	58.4	145.4
20.0	97.0	77.8	174.8
25.0	124.0	97.3	221.3

Table 2-1 **Calculate $V_{CIRC}\{I_{CIRC}\}$ coordinates, Problem (2-6)**

Figure 2-7 Circuit electrical behavior, Problem (2-6)



- Note in Figure (2-7) that there are 3 possible solutions for I_{CIRC} at V_{CIRC} = 140. The solutions are 14, 22, and 27 amperes.

- Substitute in Eq. (2-33) to determine I_A and I_B.

  $I_A = I_B = I_{CIRC}$ = 14 or 22 or 27

- Substitute in Eq. (2-31) to determine V_A.

  $V_A = 3.89\ I_A = 3.89(14 \text{ or } 22 \text{ or } 27)$

- Substitute in Eq. (2-32) to determine V_B.

  $$V_B = V_{CIRC} - V_A = 140 - V_A \tag{2-34}$$

- Substitute in Eq. (2-2) to determine P_A and P_B.

  $P_A = V_A I_A$

  $P_B = V_B I_B$

2.6.12 Problem (2-6) Answer

The circuit in Figure (2-5) has potential operating points at the three intersections in Figure (2-7). The problem statement does not contain sufficient information to uniquely determine the current at 140 volts. At the intersections, the emf, electric current, and power dissipated for Components A and B are listed in Table 2-2.

Component A	Component B
105 volts, 27 amps, 2800 watt	35 volts, 27 amps, 950 watts
86 volts, 22 amps, 1900 watts	54 volts, 22 amps, 1200 watts
54 volts, 14 amps, 760 watts	86 volts, 14 amps, 1200 watts

Table 2-2 Answer to Problem (2-6)

2.6.13 Problem (2-7) Statement

What are the values of emf, electric current, and electric power for each component in Figure (2-8)?

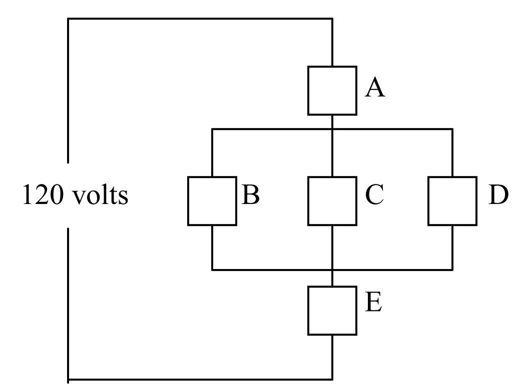


Figure 2-8 Electric system in Problem (2-7)

2.6.14 Problem (2-7) Given
The electrical behavior of the components in Figure (2-8) is given by Eqs. (2-35) through (2-39).

$$V_A = 4.7\ I_A \tag{2-35}$$

$$V_B = 3.4\ I_B \tag{2-36}$$

$$V_C = 5.4\ I_C \tag{2-37}$$

$$V_D = 4.2\ I_D \tag{2-38}$$

$$V_E = 2.4\ I_E \tag{2-39}$$

The dimension units that underlie the parameter symbols are volts, amps, and watts.

2.6.15 Problem (2-7) Analysis

- Inspect Figure (2-8) and note that:

$$I_B + I_C + I_D = I_{CIRC} \tag{2-40}$$

$$I_A = I_E = I_{CIRC} \tag{2-41}$$

$$V_B = V_C = V_D = V_{BCD} \tag{2-42}$$

$$V_A + V_{BCD} + V_E = 120 \tag{2-43}$$

- Determine $V_{BCD}\{I_{CIRC}\}$ by combining Eqs. (2-36) to (2-38) and (2-40), and using Eq. (2-42):

$$V_{BCD}/3.4 + V_{BCD}/5.4 + V_{BCD}/4.2 = I_{CIRC} \tag{2-44}$$

$$\therefore V_{BCD} = 1.394\ I_{CIRC} \tag{2-45}$$

- Determine $V_{BCD}\{I_{CIRC}\}$ by combining Eqs. (2-35), (2-39), and (2-43), and using Eq. (2-41):

$$V_{BCD} = 120 - 4.7\ I_{CIRC} - 2.4\ I_{CIRC} \tag{2-46}$$

- Determine V_{BCD} and I_{CIRC} by combining Eqs. (2-45) and (2-46):

$$I_{CIRC} = 14.13 \tag{2-47a}$$

$$V_{BCD} = 19.7 \tag{2-47b}$$

- Substitute in Eq. (2-41) to determine I_A and I_E.
- Substitute in Eqs. (2-35) and (2-39) to determine V_A and V_E.
- Substitute in Eq. (2-42) to determine V_B, V_C, and V_D.

$$V_B = V_C = V_D = V_{BCD} = 19.7 \tag{2-48}$$

- Substitute in Eqs. (2-36) through (2-38) to determine I_B, I_C, and I_D.
- Substitute in Eq. (2-4) to determine the power dissipated in each component.

2.6.16 Problem (2-7) Answer

	volts	**amperes**	**watts**
A	66.4	14.13	938
B	19.7	5.79	114
C	19.7	3.65	72
D	19.7	4.69	92
E	33.9	14.13	479

Table 2-3 Answer to Problem (2-7)

2.6.17 Problem (2-8) Statement
What are the values of emf, electric current, and electric power for each component in Figure (2-9)?

2.6.18 Problem (2-8) Given
The electrical behavior of the components in Figure (2-9) is given by Eqs. (2-49) through (2-54)

$$V_A = 1.5\, I_A^{1.3} \tag{2-49}$$

$$V_B = 4.2\, I_B \tag{2-50}$$

$$V_C = 2.6\ I_C^{.70} \tag{2-51}$$

$$V_D = 5.2\ I_D \tag{2-52}$$

$$V_E = 2.1\ I_E^{1.5} \tag{2-53}$$

$$V_F = 1.2\ I_F \tag{2-54}$$

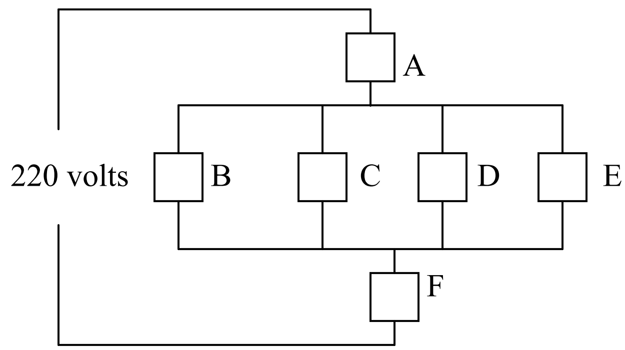


Figure 2-9 Electric system in Problem (2-8)

2.6.19 Problem (2-8) Analysis

- Inspect Figure (2-9) and note that:

$$(I_B + I_C + I_D + I_E) = I_A = I_F = I_{CIRC} \tag{2-55}$$

$$V_B = V_C = V_D = V_E = V_{BCDE} \tag{2-56}$$

$$V_A + V_{BCDE} + V_F = 220 \tag{2-57}$$

- Determine $V_{BCDE}\{I_{CIRC}\}$ by combining Eqs. (2-50) to (2-53) and (2-55), and using Eq. (2-56).

$$V_{BCDE}/4.2 + (V_{BCDE}/2.6)^{1.429} + V_{BCDE}/5.2 + (V_{BCDE}/2.1)^{.667} = I_{CIRC} \tag{2-58}$$

- Determine $V_{BCDE}\{I_{CIRC}\}$ by combining Eqs. (2-49), (2-54), and (2-57), and using Eq. (2-55):

$$V_{BCDE} = 220 - 1.5\ I_{CIRC}^{1.3} - 1.2\ I_{CIRC} \qquad (2\text{-}59)$$

- Solve Eqs. (2-58) and (2-59). The result is $V_{BCDE} = 22$, $I_{CIRC} = 35.5$.

- Use the calculated values of V_{BCDE} and I_{CIRC} to sequentially determine:

  - I_A and I_F from Eq. (2-55).

  - $V_{B,}$ V_C, V_D, and V_E from Eq. (2-56).

  - I_B, I_C, I_D, and I_E from Eqs. (2-50) through (2-53).

  - V_A and V_F from Eqs. (2-49) and (2-54).

- Determine the power dissipated in each component from Eq. (2-4).

2.6.20 Problem (2-8) Answer
For each component in Figure (2-9), the emf, electric current, and power are listed in Table (2-4).

Component	**emf** volts	**electric current** amperes	**power** watts
A	155	35.5	5500
B	22	5.2	115
C	22	21.1	465
D	22	4.2	92
E	22	4.8	105
F	43	35.5	1530

Table 2-4 Answer to Problem (2-8)

2.6.21 Problem (2-9) Statement
What are the values of emf and electric current for each component in Figure (2-10)?

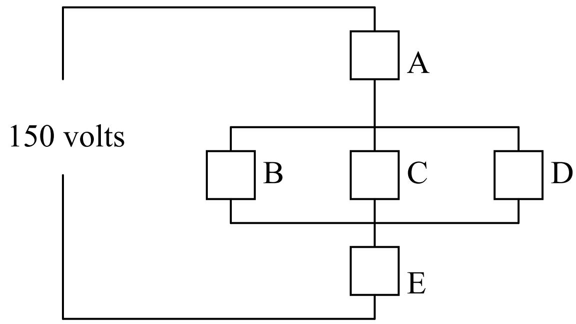


Figure 2-10 **Electric system in Problem (2-9)**

2.6.22 Problem (2-9) Given
The electrical behavior of Components A, B, C, and E is given by Eqs. (2-60) to (2-63). The electrical behavior of Component D is given by Figure (2-11).

$$V_A = 1.22\, I_A^{1.2} \tag{2-60}$$

$$V_B = 12.7\, I_B \tag{2-61}$$

$$V_C = 16.3\, I_C \tag{2-62}$$

$$V_E = 1.03\, I_E \tag{2-63}$$

The dimension units that underlie the parameter symbols are volts, amps, and watts.

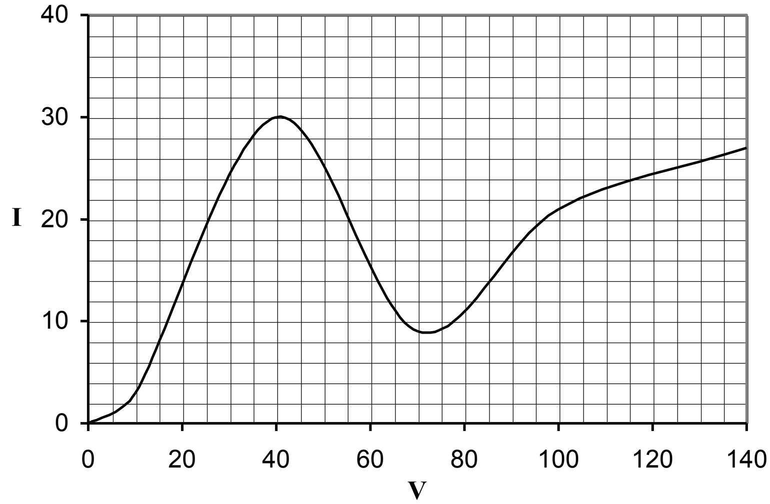
2.6.23 Problem (2-9) Analysis
- Inspect Figure (2-10) and note that:

$$(I_B + I_C + I_D) = I_A = I_E = I_{CIRC} \tag{2-64}$$

$$V_B = V_C = V_D = V_{BCD} \tag{2-65}$$

$$V_A + V_{BCD} + V_E = V_{CIRC} \tag{2-66}$$

<u>Figure 2-11</u> Electrical behavior of Component D, Problem (2-9)



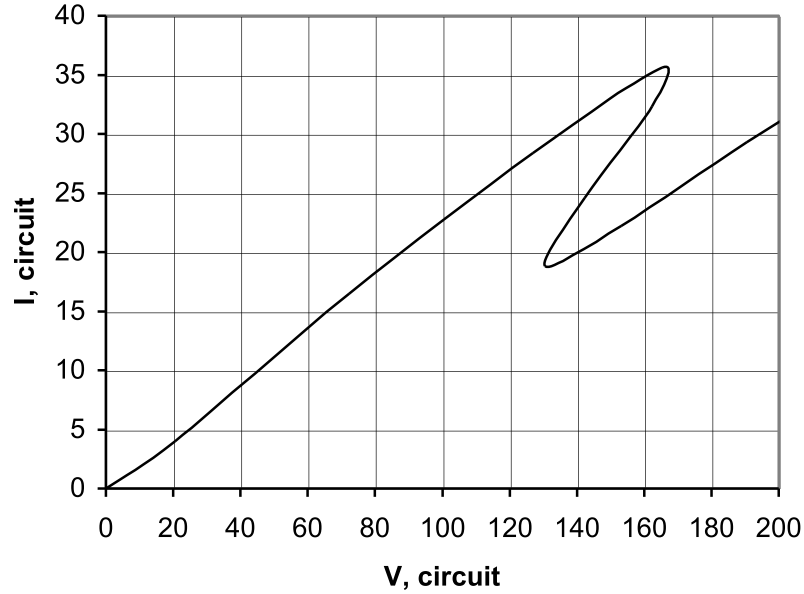
- Determine coordinates of $(I_{CIRC})\{V_{CIRC}\}$ in the following way:
    - List several coordinates of (V_D, I_D) obtained from Figure (2-11).
    - Calculate $I_B\{V_D\}$ and $I_C\{V_D\}$ from Eqs. (2-61), (2-62), and (2-65).
    - Add $I_B\{V_D\}$, $I_C\{V_D\}$, and $I_D\{V_D\}$, and obtain $(I_B + I_C + I_D)\{V_D\}$.
    - Note that $(I_B + I_C + I_D)\{V_D\} = (I_{CIRC})\{V_{BCD}\}$.
    - Calculate $V_A\{I_{CIRC}\}$ using Eq. (2-60).
    - Calculate $V_E\{I_{CIRC}\}$ using Eq. (2-63).
    - Calculate V_{CIRC} using Eqs. (2-66) and (2-65).
- The calculated results are in Table (2-5).

V_D	I_D	I_B	I_C	I_{CIRC}	V_A	V_E	V_{CIRC}
10	2.6	0.8	0.6	4.0	6.4	4.1	20.6
20	13.7	1.6	1.2	16.5	35.3	17.0	72.3
30	24.2	2.4	1.8	28.4	67.7	29.3	126.9
40	30	3.1	2.5	35.6	88.7	36.7	165.4
50	25.3	3.9	3.1	32.3	79.0	33.3	162.2
60	16.2	4.7	3.7	24.6	57.0	25.3	142.3
70	9	5.5	4.3	18.8	41.3	19.4	130.6
80	11	6.3	4.9	22.2	50.4	22.9	153.2
90	16.7	7.1	5.5	29.3	70.3	30.2	190.5
100	21	7.9	6.1	35.0	87.0	36.1	223.0
110	23	8.7	6.7	38.4	97.2	39.6	246.8
120	24.5	9.4	7.4	41.3	106.1	42.6	268.6
130	25.5	10.2	8.0	43.7	113.5	45.0	288.5
140	27	11.0	8.6	46.6	122.6	48.0	310.6

Table 2-5 Calculate (I_{CIRC}, V_{CIRC}) coordinates, Problem (2-9)

- Plot the $I_{CIRC}\{V_{CIRC}\}$ coordinates from Table (2-5) in Figure (2-12).
- Figure (2-12) indicates 3 solutions: V_{CIRC} = 150: I_{CIRC} = 22, 28, 33.
- Use the I_{CIRC} solutions to sequentially determine:
  - I_A and I_E from Eq. (2-64).
  - V_A from Eq. (2-60), V_E from Eq. (2-63).
  - V_{BCD} from Eq. (2-66).
  - V_B, V_C, and V_D from Eq. (2-65).
  - I_B and I_C from Eqs. (2-61) and (2-62).
  - I_D from Figure (2-11.
- The calculated results are in Table (2-6).

Figure 2-12 Circuit electrical behavior, Problem (2-9)



2.6.24 Problem (2-9) Answer

For each of the solutions in Figure (2-12), the emf and electric current for the components are given in Table (2-6). The problem statement does not contain sufficient information to determine a unique solution.

V_A	I_A	V_B	I_B	V_C	I_C	V_D	I_D	V_E	I_E
81	33	35	2.8	35	2.1	35	28	34	33
67	28	54	4.3	54	3.3	54	21	29	28
50	22	77	6.1	77	4.7	77	10	23	22

Table 2-6 Solution of Problem (2-9)

2.7 Conclusions

The problems in this chapter demonstrate that electrical behavior methodology is a simple and direct method of performing the static analysis of electrical circuits that contain components that exhibit proportional and/or nonlinear behavior.

Chapter 3

Analyzing the stability of resistive electrical circuits using the new engineering

3 Summary

Instability in resistive electrical circuits is a practical problem only if a component exhibits such highly nonlinear behavior that dI/dE is negative over some part of the system operating range.

In this chapter, the stability and performance of resistive electrical circuits are analyzed using the new engineering—ie using behavior methodology—ie using $E = f\{I\}$. The example problems demonstrate that behavior methodology deals simply and effectively with resistive electrical circuits, even if they contain components that exhibit the extremely nonlinear behavior described in Figures (3-3) and (3-7).

3.1 The stability question.

The stability analyses in this chapter answer the question:

> If a system is initially at a potential operating point, will the system resist a *very small* perturbation, and return to the potential operating point?

If the answer is "no", the system is "unstable" at the potential operating point—ie it *will not* operate in a steady-state manner at that point. However, it may be quite stable at other potential operating points.

If the answer is "yes", the system is *conditionally* "stable" at the potential operating point—ie it will operate in a steady-state manner at that point *provided* all perturbations are small. The system is only *conditionally* stable at the potential operating point because, even though it is stable with respect to *small* perturbations, it may be unstable with respect to *large* perturbations.

3.2 The effect of instability.

If a system is initially at an unstable operating point and is left alone, the system will tend to leave the unstable point. The result will be either hysteresis or undamped oscillation, depending on the behavior of the circuit components.

3.3 Uncoupling the system in order to analyze stability.

In the stability analysis of a system, it is generally convenient to:

- Uncouple the system—ie divide it into two subsystems.
- Analytically determine the dynamic behavior of each subsystem.
- Analytically determine the system dynamic performance that would result if the two subsystems were coupled.

The above method is used herein to analyze the stability of resistive electric systems. The systems analyzed contain a power supply and a circuit of several components, one of which exhibits highly nonlinear behavior—ie includes a region in which (dI/dE) is *negative*. The method includes the following steps:

- Uncouple the system to obtain two subsystems. One subsystem contains the highly nonlinear component and all components connected in parallel with it. The other subsystem contains the rest of the system including the power supply.
- Since the emf rises in the subsystem that includes the power supply, the subscript "RISE" is used to refer to this subsystem. E_{RISE} is the *absolute* value of the difference in emf between the uncoupled ends of the RISE subsystem.
- Since the emf falls in the subsystem that includes the highly nonlinear component, the subscript "FALL" is used to refer to this subsystem. E_{FALL} is the *absolute* value of the difference in emf between the uncoupled ends of the FALL subsystem.
- Determine $I_{FALL}\{E_{FALL}\}$.
- Determine $I_{RISE}\{E_{RISE}\}$.
- Plot $I_{FALL}\{E_{FALL}\}$ and $I_{RISE}\{E_{RISE}\}$ together on the same graph.
- Note that intersections of $I_{FALL}\{E_{FALL}\}$ and $I_{RISE}\{E_{RISE}\}$ are potential operating points.
- Use Criterion (3-1) to appraise the stability of the system at potential operating points.

3.4 The criterion for resistive electrical system instability.

Criterion (3-1) is the criterion for resistive electrical system instability:

$$(dI/dE)_{RISE} \geq_U (dI/dE)_{FALL} \tag{3-1}$$

The criterion states:

> If a subsystem in which the emf rises is coupled to a subsystem in which the emf falls, the resultant system will be *unstable* at a potential operating point if $(dI/dE)_{RISE}$ is greater than or equal to $(dI/dE)_{FALL}$. (The symbolism $\geq_U$ indicates "unstable if satisfied".)

The criterion describes stability with regard to *very small* perturbations. Therefore:

- If the criterion is satisfied at a potential operating point, the system is unstable at that potential operating point.

- If the criterion is *not* satisfied at a potential operating point, the system is stable at that potential operating point with respect to very small perturbations. However, it may *not* be stable with respect to large perturbations.

In this chapter, a system is described as "stable" at a potential operating point if Criterion (3-1) is *not* satisfied. However, it must be recognized that "stable" is used as a shorthand expression for "stable with regard to very small perturbations".

The system design objective is generally "stable with respect to perturbations inherent in the system". Fortunately, background perturbations in real systems are generally quite small. Thus there is usually little practical difference between "stable with respect to small perturbations", and "stable with respect to perturbations inherent in the system".

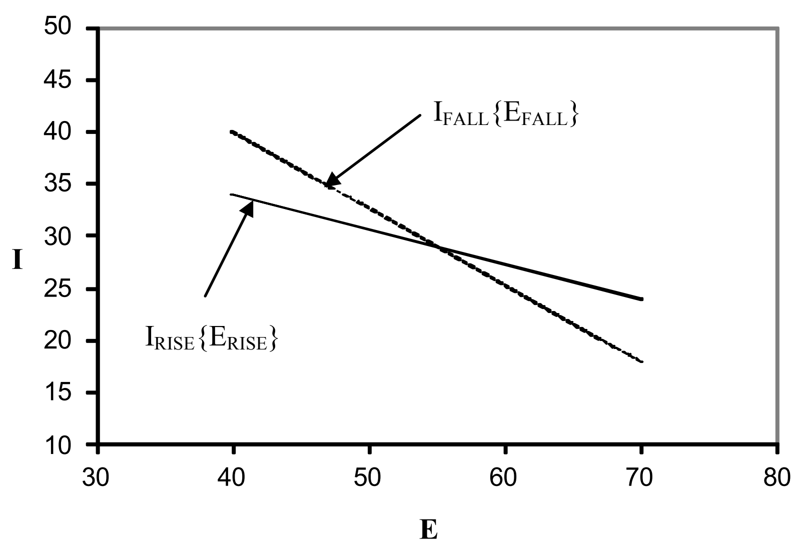
3.5 Verifying Criterion (3-1).

Criterion (3-1) can be verified by showing that, if a circuit is initially at a potential operating point at which the criterion is satisfied, a small perturbation will tend to increase with time.

Figure (3-1) describes the electrical behavior of two subsystems, $I_{FALL}\{E_{FALL}\}$ and $I_{RISE}\{E_{RISE}\}$, in the vicinity of an intersection. If the two subsystems are connected in a circuit, the stability of the circuit at the intersection can be appraised in the following manner:

- Assume that the system described in Figure (3-1) is initially operating at the intersection.
- Suddenly the system experiences a small, positive perturbation in E.
- The positive perturbation causes I_{RISE} to be greater than I_{FALL}.
- Because I_{RISE} is greater than I_{FALL}, E *increases* with time.
- An increasing E indicates that the positive perturbation is growing, and that the system is not returning to the potential operating point. Therefore the intersection in Figure (3-1) is an unstable operating point.

Figure 3-1 Potential operating point.



- To determine whether Criterion (3-1) also indicates instability, note that the slope of $I_{RISE}\{E_{RISE}\}$ is greater than the slope of $I_{FALL}\{E_{FALL}\}$. Since this satisfies Criterion (3-1), the criterion also indicates instability. (Since both slopes are negative, the *greater* slope is *less* steep.)

- Since the above analysis and Criterion (3-1) are in agreement, the analysis validates Criterion (3-1).

- If $I_{FALL}\{E_{FALL}\}$ and $I_{RISE}\{E_{RISE}\}$ were interchanged, a positive perturbation in E would cause E to decrease, Criterion (3-1) would not be satisfied, and the system would be stable at the intersection.

3.6 Stability analysis of an electric circuit—Problem (3-1).

Problem (3-1) demonstrates:

- How to analyze an electric circuit for instability.

- How to determine the effect of instability on system behavior.

3.6.1 Problem (3-1) Statement

Describe the behavior of the system in Figure (3-2) over its operating range of 0 to 400 volts. In other words, determine $I_{PS}\{E_{PS}\}$ for $E_{PS} = 0$ to 400. (Note that subscript PS refers to power supply.)

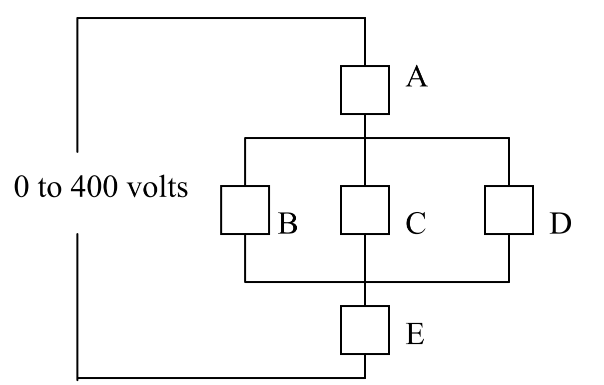


Figure 3-2 Electric Circuit in Problem (3-1)

3.6.2 Problem (3-1) Given

The electrical behavior of Components A, B, C, and E is given by Eqs. (**3**-2) to (**3**-5). The behavior of Component D is given by Figure (3-3).

$$E_A = 1.8\ I_A \qquad (3\text{-}2)$$

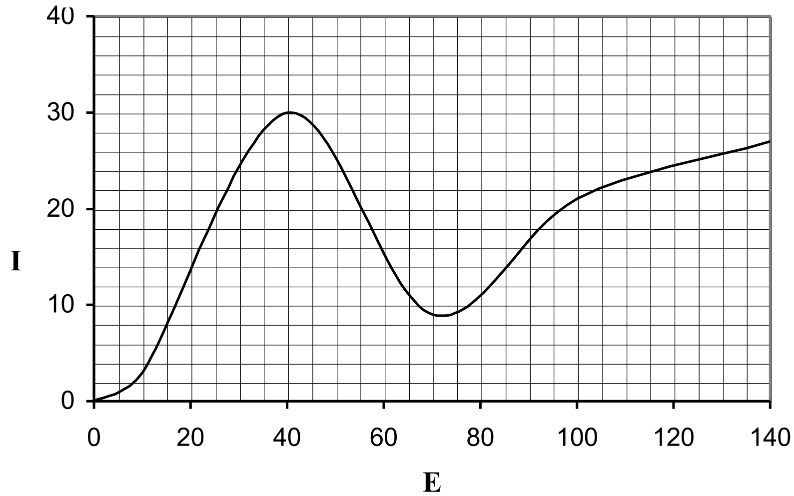
$E_B = 12.7\ I_B$ (3-3)

$E_C = 16.3\ I_C$ (3-4)

$E_E = 4.5\ I_E$ (3-5)

The dimension units are volts, amps, and watts.

Figure 3-3 Electrical behavior of Component D, Problem (3-1)



3.6.3 Problem (3-1) Analysis

- Uncouple the system to obtain a subsystem that contains Components B, C, and D. Use FALL to refer to the BCD subsystem, RISE to refer to the rest of the system including the power supply.

- Inspect Figure (3-2) and note that:

  $I_B + I_C + I_D = I_A = I_E = I_{PS} = I_{RISE} = I_{FALL}$ (3-6)

  $E_{FALL} = E_B = E_C = E_D$ (3-7)

  $E_{RISE} = E_{PS} - E_A - E_E$ (3-8)

- Write an equation for $I_{RISE}\{E_{RISE}\}$ by substituting Eqs. (3-2) and (3-5) in Eq. (3-8), and using Eq. (3-6).

  $E_{RISE} = E_{PS} - 1.8 I_{RISE} - 4.5 I_{RISE}$ (3-9a)

$\therefore I_{RISE} = 0.1587(E_{PS} - E_{RISE})$ (3-9b)

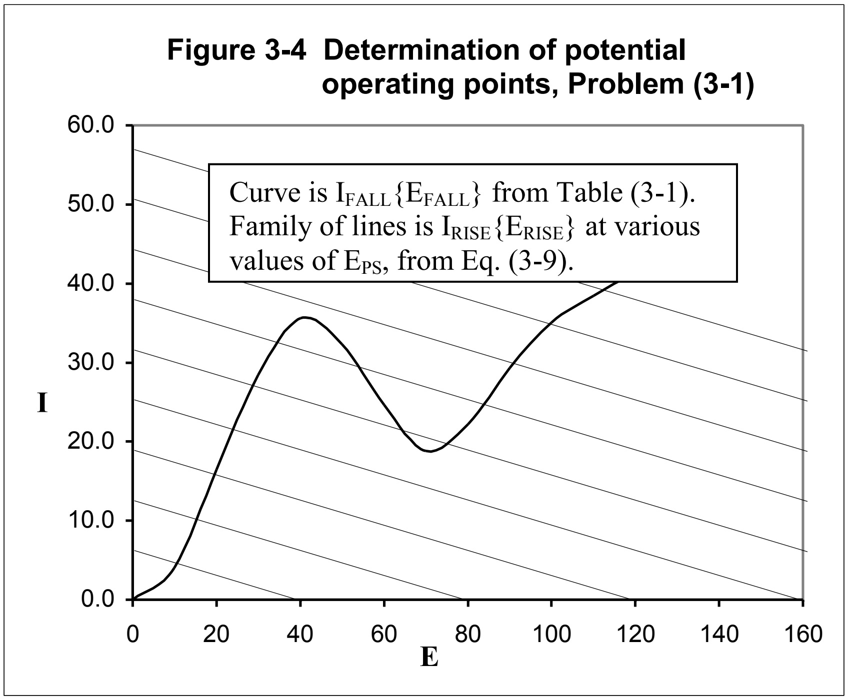
- Determine coordinates of $I_{FALL}\{E_{FALL}\}$ in the following way:
  - List several coordinates of (E_D, I_D) obtained from Figure (3-3).
  - At each coordinate, calculate $I_B\{E_D\}$ and $I_C\{E_D\}$ from Eqs. (3-3), (3-4), and (3-7).
  - At each coordinate, add $I_B\{E_D\}$, $I_C\{E_D\}$, and $I_D\{E_D\}$, and obtain $(I_B + I_C + I_D)\{E_D\}$.
  - Note that $(I_B + I_C + I_D)\{E_D\} = I_{FALL}\{E_{FALL}\}$.
  - The $I_{FALL}\{E_{FALL}\}$ calculated results are listed in Table (3-1).

E_D	$I_D\{E_D\}$	$I_B\{E_D\}$	$I_C\{E_D\}$	$(I_B + I_C + I_D)\{E_D\} = (I_{FALL})\{E_{FALL}\}$
0	0	0	0	0
10	2.6	0.8	0.6	4.0
20	13.7	1.6	1.2	16.5
30	24.2	2.4	1.8	28.4
40	30	3.1	2.5	35.6
50	25.3	3.9	3.1	32.3
60	16.2	4.7	3.7	24.6
70	9	5.5	4.3	18.8
80	11	6.3	4.9	22.2
90	16.7	7.1	5.5	29.3
100	21	7.9	6.1	35.0
110	23	8.7	6.7	38.4
120	24.5	9.4	7.4	41.3
130	25.5	10.2	8.0	43.7
140	27	11.0	8.6	46.6

Table 3-1 Calculation of $(I_{FALL})\{E_{FALL}\}$ coordinates, Problem (3-1)

- On Figure (3-4), plot $I_{FALL}\{E_{FALL}\}$ coordinates from Table (3-1).

- On Figure (3-4), use Eq. (3-9) to plot $I_{RISE}\{E_{RISE}\}$ at various values of E_{PS}. Cover the range E_{PS} = 40 to 400 volts in increments of 40 volts. Note from Eq. (3-9) that E_{PS} is equal to the value of E_{RISE} at $I_{RISE} = 0$.



- Note that intersections in Figure (3-4) are potential operating points.

With regard to stability at potential operating points, note the following:

- Stability can be determined by inspection of Figure (3-4). As indicated by Criterion (3-1), operation at an intersection is unstable if the slope of $I_{RISE}\{E_{RISE}\}$ is greater than the slope of $I_{FALL}\{E_{FALL}\}$. (Since both slopes are negative, the greater slope is *less* steep. Therefore, intersections in Figure (3-4) are unstable if the $I_{FALL}\{E_{FALL}\}$ curve is *steeper* than the $I_{RISE}\{E_{RISE}\}$ curve.)

- Intersections throughout most of the negative slope region of $I_{FALL}\{E_{FALL}\}$ are unstable.

- All unstable intersections are in the negative slope region of $I_{FALL}\{E_{FALL}\}$.

- All unstable intersections are on $I_{RISE}\{E_{RISE}\}$ lines that have 3 intersections. Only the middle intersections are unstable.

- Because the unstable intersections are middle intersections, a positive perturbation would cause operation to shift to the higher voltage intersection, and a negative perturbation would cause operation to shift to the lower voltage intersection. Since operation is stable at both the higher and lower voltage intersections, the system would remain at either intersection.

3.6.4 Problem (3-1) Answer

Problem (3-1) requires that $I_{PS}\{E_{PS}\}$ be determined over the power supply range of 0 to 400 volts. Coordinates of $I_{PS}\{E_{PS}\}$ are obtained in the following manner:

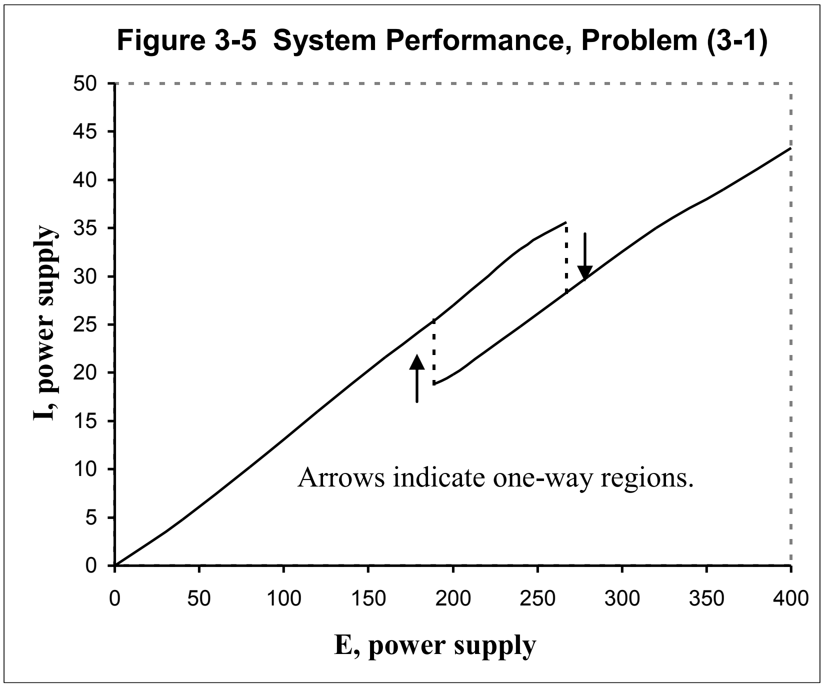
- Inspect Figure (3-4) to determine the values of E_{RISE} and I_{RISE} at intersections.

- Substitute the intersection values of E_{RISE} and I_{RISE} in Eq. (3-9) to determine (E_{PS},I_{RISE}) coordinates at intersections.

- Note that I_{PS} and I_{RISE} are equal, and therefore E_{PS},I_{RISE} = E_{PS},I_{PS}.

- Plot the (E_{PS},I_{PS}) coordinates of the stable intersections. Do not plot the coordinates of unstable intersections because the system automatically leaves unstable intersections, and goes to stable intersections, where it remains.

Figure (3-5) is the desired answer—a description of system performance $I_{PS}\{E_{PS}\}$ over the power supply range 0 to 400 volts. Note the pronounced hysteresis.



3.7 How to eliminate hysteresis

Figure (3-5) indicates that the system in Problem (3.1) exhibits pronounced hysteresis when the power supply delivers between 190 and 270 volts. Assuming that the behavior of the nonlinear component cannot be altered, the hysteresis can be eliminated by modifying Components A and E. The required modification can be determined by noting the following:

- The hysteresis in Figure (3-5) results from the multi-valued solutions in Figure (3-4). Therefore the hysteresis would be eliminated if all solutions were single-valued.

- All solutions would be single-valued if the slope of the $I_{RISE}\{E_{RISE}\}$ lines were more negative than the most negative slope region of $(I_{FALL})\{E_{FALL}\}$.

- The slope of the $I_{RISE}\{E_{RISE}\}$ lines could be made more negative by altering the electrical behavior of Components A and E.

- The most negative slope of $(I_{FALL})\{E_{FALL}\}$ is –0.77 amps/volt. The slope of the $I_{RISE}\{E_{RISE}\}$ lines is –0.16 amps/volt, obtained by differentiation of Eq. (3-9).

- The slope of the $I_{RISE}\{E_{RISE}\}$ lines can be decreased to a value less than –0.77 amps/volt by modifying Components A and E so that the sum of the 1.8 in Eq. (3-2) and the 4.5 in Eq. (3-5) is decreased by a factor less than (.16/0.77) = 0.21. This modification ensures that all potential operating points are stable, and therefore the modified system will operate without hysteresis throughout its normal operating range of 0 to 400 volts.

3.8 Stability analysis of an electrical system—Problem (3-2).

Problem (3-2) differs from Problem (3-1) in that the system instability results in undamped oscillation as well as hysteresis. Note that undamped oscillation results in spite of a power supply that delivers a constant emf.

Undamped oscillation is possible only if a component exhibits I{E} behavior that includes a region in which dI/dE is negative, *and* the region is bounded by points at which dI/dE is infinity.

3.8.1 Problem (3-2) statement

Describe the performance of the system in Figure (3-6) over the range 0 to 140 volts.

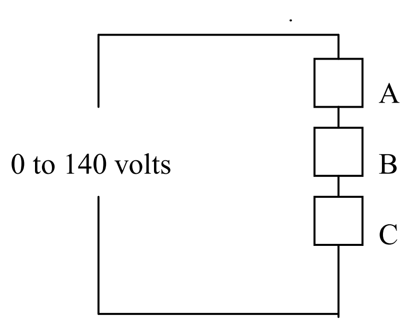


Figure 3-6 Electric Circuit in Problem 3.8

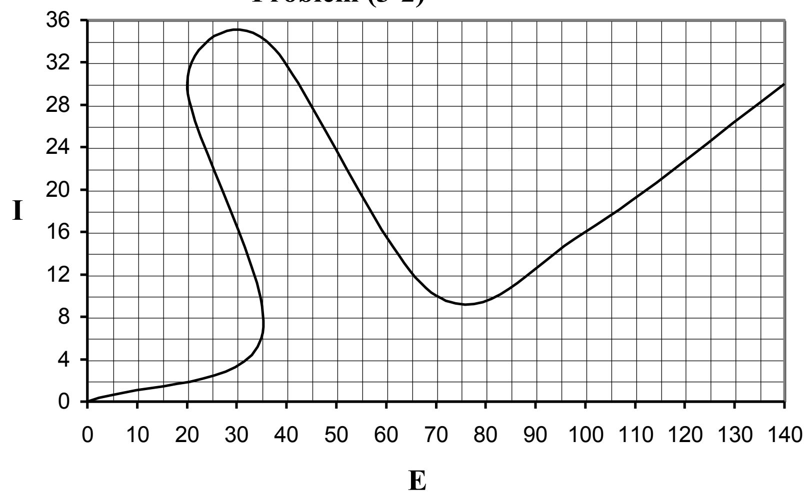
3.8.2 Problem (3-2) Given
The electrical behavior of Components A and B is given by Eqs. (3-10) and (3-11). Electrical behavior of Component C is given by Figure (3-7).

$$E_A = .85\ I_A \tag{3-10}$$

$$E_B = 1.45\ I_B \tag{3-11}$$

The dimension units are volts, amps, and watts.

Figure 3-7 Electrical behavior of Component C, Problem (3-2)



3.8.3 Problem (3-2) Determination of potential operating points.
Uncouple the circuit so that the FALL subsystem contains only Component C, and the RISE subsystem contains the remainder of the system, including the power supply.

Inspect Figure (3-6) and note that:

$$I_A = I_B = I_C \tag{3-12}$$

$$E_{FALL} = E_C \tag{3-13}$$

$$E_{RISE} = E_{PS} - E_A - E_B \tag{3-14}$$

- Write an equation for $I_{RISE}\{E_{RISE}\}$ by substituting Eqs. (3-10) and (3-11) in Eq. (3-14), and using Eq. (3-12).

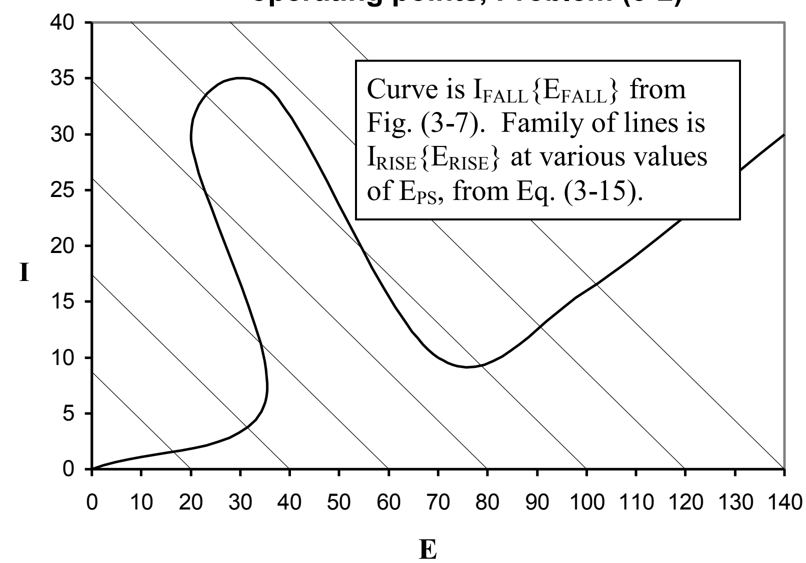
$$E_{RISE} = E_{PS} - .85 I_{RISE} - 1.45\, I_{RISE} \quad (3\text{-}15a)$$

$$\therefore I_{RISE} = 0.435(E_{PS} - E_{RISE}) \quad (3\text{-}15b)$$

- Note that Figure (3-7) describes both $I_C\{E_C\}$ and $I_{FALL}\{E_{FALL}\}$.

- Plot $I_{FALL}\{E_{FALL}\}$ from Figure (3-7) on Figure (3-8). Also on Figure (3-8), use Eq. (3-15) to plot $I_{RISE}\{E_{RISE}\}$ for various values of E_{PS}. Cover the range E_{PS} = 0 to 140 volts in increments of 20 volts. Note from Eq. (3-15) that, on each curve, E_{PS} is equal to the value of E_{RISE} at $I_{RISE} = 0$.

Figure 3-8 **Determination of potential operating points, Problem (3-2)**



- Note that intersections of $I_{FALL}\{E_{FALL}\}$ and $I_{RISE}\{E_{RISE}\}$ are potential operating points. Also note that operation is stable at some intersections, and unstable at others.

3.8.4 Problem (3-2) Stability at intersections.
With regard to stability at the intersections in Figure (3-8), note the following:

- In Figure (3-8), the electrical behavior of the FALL subsystem includes a maximum and a minimum in $I_{FALL}\{E_{FALL}\}$, *and* a maximum and minimum in $E_{FALL}\{I_{FALL}\}$.

- The maximum and minimum in $I_{FALL}\{E_{FALL}\}$ occur at (I,E) coordinates of (35,30) and (9,76). The maximum and minimum in $E_{FALL}\{I_{FALL}\}$ occur at (E, I) coordinates (35,7) and (20,30).

- Note that E_{PS} is constant along $I_{RISE}\{E_{RISE}\}$ lines, and therefore the value of E_{PS} on each line can be determined by inspection of Figure (3-8), since Eq. (3-15) indicates that $E_{PS} = E_{RISE}\{I_{RISE} = 0\}$).

- Note in Figure (3-8) that, when E_{PS} is greater than 50 volts and less than 87 volts, $I_{RISE}\{E_{RISE}\}$ intersects $I_{FALL}\{E_{FALL}\}$ in the region between the maximum and minimum in $E_{FALL}\{I_{FALL}\}$.

- Note that, when $I_{RISE}\{E_{RISE}\}$ intersects $I_{FALL}\{E_{FALL}\}$ in the region between the maximum and minimum in $E_{FALL}\{I_{FALL}\}$, only a single intersection results, and it is unstable. Since there is only one intersection, the system cannot "find" a stable intersection, and it remains in an unstable condition.

3.8.5 Problem (3-2) Behavior at an unstable, single intersection.
To determine the system behavior that results from a single, unstable intersection, refer to Figure (3-8), and suppose that the system is initially at $E_{PS} = 80$:

- The system suddenly receives a small, positive perturbation in E.

- The positive perturbation causes E to increase because, in the perturbed condition, I_{RISE} is greater than I_{FALL}.

- When E increases to the maximum in $E_{FALL}\{I_{FALL}\}$ at (35,7), the mismatch between I_{RISE} and I_{FALL} causes a step increase to (35,34), since it is the only operating point at E incrementally greater than 35.

- At (35,34), the mismatch between I_{RISE} and I_{FALL} causes E to decrease to the minimum in $E_{FALL}\{I_{FALL}\}$ at (20,30).

- At (20,30), the mismatch between I_{RISE} and I_{FALL} causes a step decrease to (20,2).

- At (20,2), the mismatch between I_{RISE} and I_{FALL} causes E to increase to the maximum in $E_{FALL}\{I_{FALL}\}$ at (35,7), and the cycle repeats.

Note in Figure (3-8) that, when E_{PS} is greater than 50 volts and less than 87 volts, a single, unstable intersection results. Therefore, when E_{PS} is between 50 and 87 volts, I_C and E_C endlessly traverse the loop shown in Figure (3-9).

Also note in Figure (3-8) that, when V_{PS} is greater than 92 volts and less than 113 volts, three intersections result. As in Problem 3.6, the middle intersection is unstable, and the instability results in hysteresis.

In summary, inspection of Figure (3-8) indicates that the system exhibits:

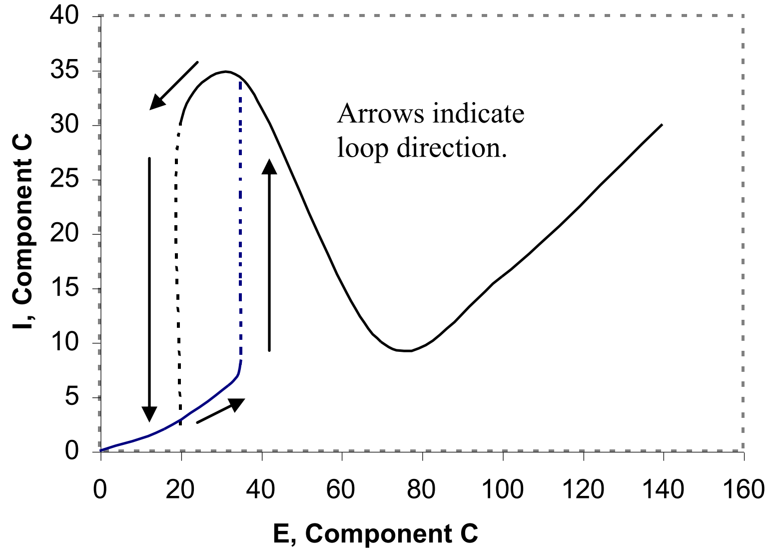
- Undamped oscillation when $I_{RISE}\{E_{RISE}\}$ intersects $I_{FALL}\{E_{FALL}\}$ in the region between the maximum and minimum in $E_{FALL}\{I_{FALL}\}$. This occurs when the power supply delivers 50 to 87 volts, and results in the endless loop shown in Figure (3-9)..

- Hysteresis when $I_{RISE}\{E_{RISE}\}$ intersects $I_{FALL}\{E_{FALL}\}$ in the region between the maximum and minimum in $I_{FALL}\{E_{FALL}\}$. This occurs when the power supply delivers 92 to 113 volts.

3.8.6 Problem (3-2) Answer
Figure (3-10) is the answer to Problem (3-2). It describes the system performance over the power supply range of 0 to 140 volts.

Coordinates of $I_{PS}\{E_{PS}\}$ were obtained from $I_{RISE}\{E_{RISE}\}$ intersections in Figure (3-8) by noting that $I_{PS} = I_{RISE}$, and $E_{PS} = E_{RISE}\{I_{RISE} = 0\}$. Unstable intersections are *not* plotted.

Figure 3-9 Endless loop that results when power supply delivers 50 to 87 volts, Problem (3-2)

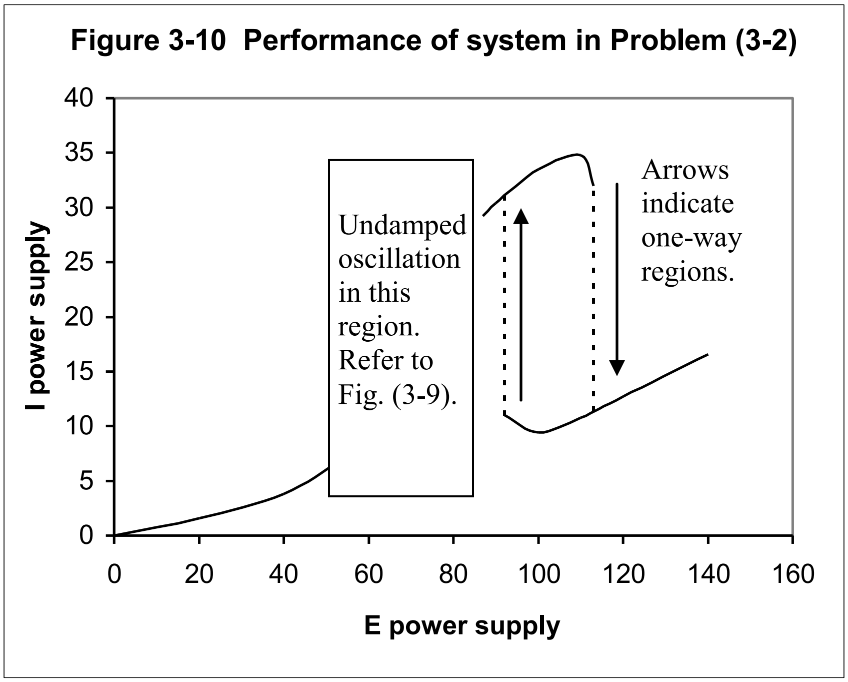


3.9 How to eliminate undamped oscillations

Assuming that the behavior of Component C cannot be altered, the undamped oscillations noted in Problem 3.8 can be eliminated by modifying the system so that there are no intersections in the region between the maximum and minimum in $E_{FALL}\{I_{FALL}\}$.

Note in Figure (3-8) that, if the $I_{RISE}\{E_{RISE}\}$ lines were steeper than the $E_{FALL}\{I_{FALL}\}$ line in the linear region between the maximum and minimum in $E_{FALL}\{I_{FALL}\}$, there would be no single intersections in that region, as desired.

In the linear region between the maximum and the minimum in $E_{FALL}\{I_{FALL}\}$, the slope is -1.13 amps/volt. Therefore, if the system were modified so that the slope of the $I_{RISE}\{E_{RISE}\}$ lines were ≤ -1.13 amps/volt, the single solutions would be replaced by triple solutions, and the region of undamped oscillation would be replaced by a region of hysteresis.



The slope of the $I_{RISE}\{E_{RISE}\}$ lines is determined by the behavior of Components A and B. To attain the desired slope, Components A and B must be modified so that the 0.435 in Eq. (3-15) becomes equal to or greater than 1.13.

If Components A and B were modified as required, the system performance would be affected in the following ways:

- Undamped oscillatory behavior would be eliminated. It would be replaced by hysteresis at power supply voltages in the vicinity of 35 volts. The extent of the hysteresis would depend on the behavior of the modified Components A and B.

- The hysteresis that occurred in the original design (between 92 and 113 volts) would be eliminated, since the steepest slope between the maximum and minimum in $I_{FALL}\{E_{FALL}\}$ is $-.85$ amps/volt.

3.10 Conclusions

Stability analysis of resistive electrical systems is simple and effective using behavior methodology.

Chapter 4

Example problems that demonstrate heat transfer analysis using the new engineering.

4 Summary

This chapter contains example problems that demonstrate heat transfer analysis using behavior methodology—ie using $q = f\{\Delta T\}$. The problems demonstrate that the solution of both proportional and nonlinear heat transfer problems is simple and direct if the solution is based on $q = f\{\Delta T\}$.

In Chapter 5, these same problems are presented in terms of the h methodology used in conventional engineering, but the problems are not solved. If the reader will solve the problems using h methodology, he will better appreciate the simplicity that results when $q = f\{\Delta T\}$ is used instead of $q = h\Delta T$.

4.1 Electrical analog of heat transfer.

In much of the nineteenth century, h and R were analogs because both were correctly viewed as proportionality constants that applied *globally*.

However, sometime near the beginning of the twentieth century, h and R *ceased* to be analogs. It is still true that R is *always* a constant because it is applied *only* to conductors that exhibit proportional behavior. But h is *not* always a constant. It is a constant when applied to boundary layers that exhibit proportional behavior, and a *variable* when it is applied to boundary layers that exhibit nonlinear behavior (as in heat transfer by free convection, condensation, and boiling).

In the new engineering, heat transfer phenomena and resistive electrical phenomena are truly analogous, as shown in the following table:

Heat transfer	Electrical analog
Temperature difference, ΔT	Electromotive force, E
Heat flux, q	Electric current, I
The law is $q = f\{\Delta T\}$.	The law is $I = f\{E\}$.

In fact, the analogy between electrical phenomena and heat transfer phenomena is so close that, in order to transform the electrical problems in Chapters 2 and 3 to heat transfer problems, little more is required than substituting q for I, and ΔT for E.

4.2 Heat transfer analysis.

In conventional engineering and in the new engineering, heat transfer analysis generally concerns one or more of the following:

- Determine the heat flux from a heat source to a heat sink.
- Determine the temperature profile from a heat source to a heat sink.
- Determine the total heat flow rate Q by integrating the local heat flux over the heat transfer surface.

Integration in the new engineering is the same as integration in conventional engineering. Therefore little space in this book is devoted to integration.

In this chapter, problems that concern the determination of heat flux and temperature profile are solved using the new engineering. These same problems are presented in Chapter 5 in terms used in conventional engineering, but the problems are not solved. Solutions based on conventional engineering are intended to be determined by the reader.

4.3 The relationship between q and dT/dx in conventional engineering, and in the new engineering.

In conventional engineering, conductive heat transfer behavior is described by Eq. (4-1) in which the symbols represent numerical value and dimension.

$$q_{cond} = k(dT/dx) \qquad (4\text{-}1)$$

All practical materials indicate a proportional relationship between q and dT/dx. Therefore, as a practical matter, Eq. (4-1) correctly states that q_{cond} is *always* proportional to dT/dx, and k is *always* the proportionality constant between q_{cond} and dT/dx.

In the remainder of this book, it is assumed that all materials exhibit proportional conductive behavior described by Eq. (4-1) in which k is the *dimensionless* proportionality constant between q and dT/dx.

However, if material is ever discovered or invented that does not exhibit a proportional relationship between q and dT/dx, Eq. (4-1) and k must be abandoned, and replaced by $q = f\{dT/dx\}$.

4.4a. The law of convective heat transfer in conventional engineering.

In American heat transfer texts, Eq. (4-2a) is generally referred to as "Newton's law of cooling".

$$q = h\Delta T_{BL} \qquad (4\text{-}2a)$$

However, Equation (4-2a)) should be referred to as "Fourier's law of convective heat transfer" because Eq. (4-2a) and h were conceived by Fourier. (See Adiutori (1990).)

In much of the nineteenth century, Eq. (4-2a) was a law that stated q is *always* proportional to ΔT_{BL}. Sometime near the beginning of the twentieth century, it was realized that q is *not* always proportional to ΔT_{BL}, and therefore it became necessary to do one of the following:

- Abandon Eq. (4-2a), and generate new methodology that would apply to both proportional and nonlinear behavior.
- Restrict the application of Eq. (4-2a) to proportional phenomena (analogous to restricting Ohm's law to conductors that obey Ohm's law), and generate other methodology to be applied to nonlinear phenomena.
- Transform Eq (4-2a) from an equation that describes the relationship between q and ΔT_{BL}, to a *definition of h*.

It was decided to transform Eq. (4-2a) into a definition of h. At that point, Eq. (4-2a) should have been replaced by Definition (4-2b). And Definition (4-2b) should no longer have been referred to as a "law" because laws describe parametric behavior, and Definition (4-2b) does *not* describe parametric behavior.

$h \equiv q/\Delta T$ (4-2b)

However, Eq. (4-2a) was *not* replaced by Definition (4-2b), and Equation (4-2a) continued to be referred to as a law rather than a definition.

In summary, there is in fact *no* law of convective heat transfer in conventional engineering because Eq. (4-2a) does *not* describe the relationship between q and ΔT. Equation (4-2a) merely defines h to be a symbol for the ratio (q/ΔT), a ratio that is oftentimes a *constant*, and oftentimes a *variable*.

4.4b The law of convective heat transfer in the new engineering.
In the new engineering, Eq. (4-3) is the law of convective heat transfer behavior.

$q = f\{\Delta T\}$ (4-3)

The impact of replacing Eq. (4-2a) with Eq. (4-3) is reflected in the difference between Eqs. (4-4) to (4-6) and Eqs. (4-4h) to (4-6), and the difference between Figures (4-1) and (4-1h):

$q = .023(k/D)N_{Re}^{.8}N_{Pr}^{.4}\,\Delta T$ (4-4)

$q = 5.8\ \Delta T$ (4-5)

$q = 2.5\ \Delta T^{1.33}$ (4-6)

$h = .023(k/D)\ Re^{.8}\ Pr^{.4}$ (4-4h)

$h = 5.8$ (4-5h)

$h = 2.5\ \Delta T^{0.33}$ (4-6h)

Figure 4-1 Heat transfer behavior, new engineering

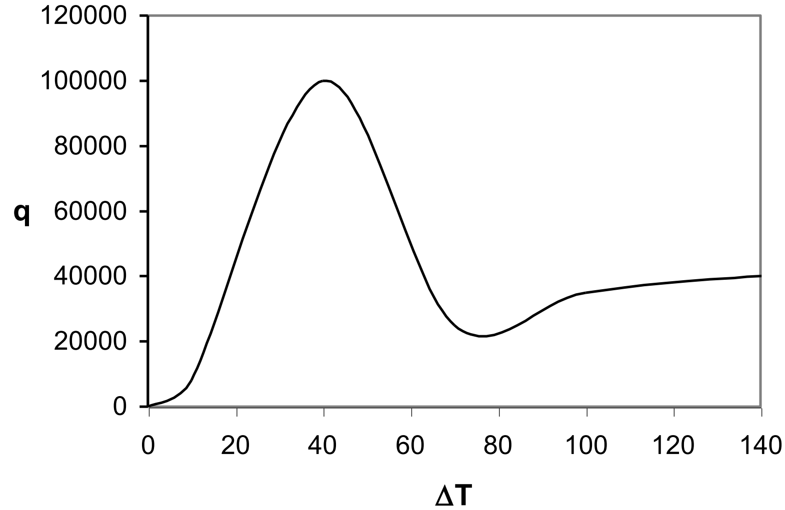
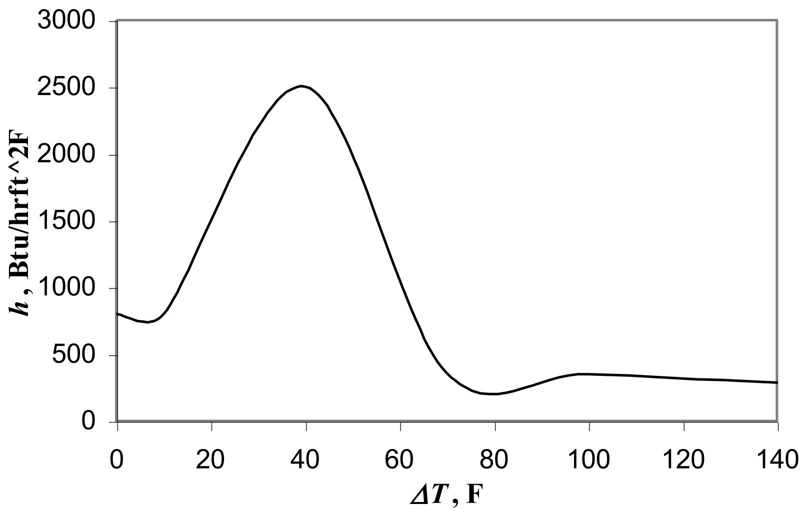


Figure 4-1h Heat transfer behavior, conventional engg.



4.5 Parameter groups in the new engineering.

In the new engineering, primary parameters such as q and ΔT are *always* separate and explicit. This requires that parameter groups be used somewhat differently. For example:

- Parameter groups that include *both* q and ΔT are *abandoned*. For example, Nusselt number and Stanton number include both q and ΔT (since they contain *h*, and *h* is *q/ΔT*). Therefore Nusselt number and Stanton number are *never* used.

- Parameter groups that include *either* q or ΔT are used, but *only* with the individual parameters shown *explicitly*.

- Parameter groups that include *neither* q nor ΔT may be used in explicit or implicit form. For example, Reynolds number contains neither q nor ΔT. Therefore Reynolds number may be used in explicit or implicit form when it concerns heat transfer. However, it must be used in explicit form when it concerns fluid flow.

4.6 How to transform h correlations to ΔT = *f*{q} correlations.

Heat transfer coefficient correlations are transformed to heat transfer behavior correlations in the following way:

- Substitute q/ΔT for *h*.

- Separate q and ΔT.

For example, Eq. (4-7) is a heat transfer coefficient correlation.

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

Eq. (4-7) is transformed to a ΔT{q} correlation as follows:

- Note that N_{Nu} is qD/ΔTk.

- Replace N_{Nu} in Eq. (4-7) by qD/ΔTK.

$$(qD/\Delta TK) = .023\ N_{Re}^{.8}\ N_{Pr}^{.4} \qquad (4\text{-}8)$$

- Separate q and ΔT and rearrange.

$$\Delta T = q/(.023\ (k/D)\ N_{Re}^{.8}\ N_{Pr}^{.4}) \qquad (4\text{-}9)$$

Eq. (4-9) is in the desired form $\Delta T = f\{q\}$.

4.7 The behavior form of $UA\Delta T_{LM}$.

In conventional heat transfer, the overall heat transfer coefficient is assigned the symbol U. In heat exchanger analysis, if U is independent of location, total heat flow Q_{TOTAL} is calculated from Eq. (4-10). (Subscript LM refers to log mean.)

$$Q_{TOTAL} = U\ A\ \Delta T_{LM} \qquad (4\text{-}10)$$

Eq. (4-10) is converted to behavior form by noting that

$$U\ \Delta T_{LM} \equiv q\{\Delta T_{LM}\} \qquad (4\text{-}11)$$

Combining Eqs. (4-10) and (4-11) gives Eq. (4-12):

$$\therefore Q_{TOTAL} = q\{\Delta T_{LM}\}\ A \qquad (4\text{-}12)$$

Eq. (4-12) is the behavior form of Eq. (4-10). Both equations are applicable only if q is everywhere proportional to ΔT_{total}, *and* the value of the proportionality constant is independent of location.

4.8 How to determine $\Delta T_{wall}\{q\}$ equations.

- Obtain $k\{T_{wall}\}$ from the literature.

- Estimate $T_{wall\ average}$.

- Determine $k\{T_{wall\ average}\}$.

$$q = (k\{T_{wall\ average}\}/t_{wall})\Delta T_{wall} \qquad (4\text{-}13)$$

$$\Delta T_{wall} = q\ t_{wall}/k\{T_{wall\ average}\} \qquad (4\text{-}14)$$

4.8 Heat transfer analysis using behavior methodology.

A typical problem in heat transfer analysis is to determine the heat flux that would result from connecting a heat source fluid to a heat sink fluid. The source fluid and sink fluid are often connected through a wall as shown in Figure (4-2).

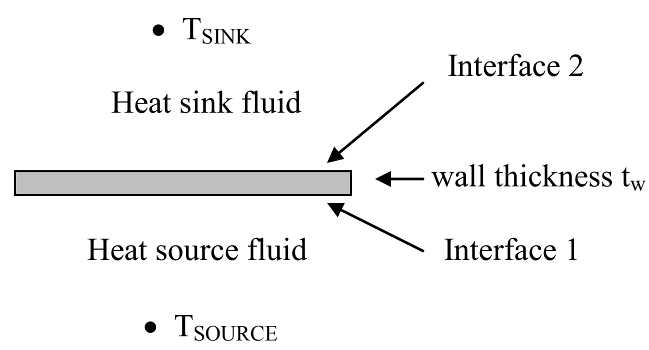


Figure 4-2 Typical heat transfer configuration

The problem statement typically specifies:

- Geometry of the wall and fluid passages.
- Identity of source fluid, sink fluid, and wall material.
- Temperatures and flow rates of source fluid and sink fluid.

The following information is obtained from the literature:

- Generalized $\Delta T = f\{q\}$ or $q = f\{\Delta T\}$ digital or graphical correlations for the source fluid boundary layer and the sink fluid boundary layer. (Until the new engineering is globally accepted for some time, correlations in the current literature will continue to be presented in *h* form. They are transformed to $q = f\{\Delta T\}$ or $\Delta T = f\{q\}$ form in the manner described above.)
- Thermal conductivity of material. (No conversion is required other than noting that k is no longer a dimensioned parameter. It is now the numerical value of the proportionality constant between q and dT/dx, in dimensions specified in the accompanying nomenclature.)

The analysis is performed as follows:

- Obtain a specific $\Delta T = f\{q\}$ equation for each boundary layer by evaluating the generalized correlations at the specified geometry, flow rate, temperature, etc.

- Obtain a specific $\Delta T = f\{q\}$ equation for the wall.

- Note that Eq. (4-15) describes the configuration in Figure (4-2).

$$\Delta T_{TOTAL} = T_{SOURCE} - T_{SINK} = \Delta T_1 + \Delta T_2 + \Delta T_{WALL} \qquad (4\text{-}15)$$

- Substitute the given value of ΔT_{TOTAL} and the specific q functions for ΔT_1, ΔT_2, and ΔT_{WALL} in Eq. (4-15).

- Solve the equation for q.

The above procedure applies for any number of walls and boundary layers in series.

4.10 Heat transfer behavior methodology—Problem (4-1).

Problem (4-1) concerns the determination of heat flux and temperature profile using behavior methodology. All heat transfer elements in the problem exhibit proportional behavior. The solution of the problem is simple and direct using behavior methodology, and is also simple and direct using h methodology.

4.10.1 Problem (4-1) Statement

What is the heat flux in the Figure (4-3) configuration? What is the wall temperature at each interface?

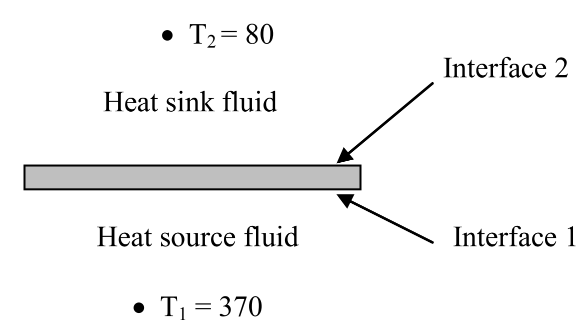


Figure 4-3 Heat transfer configuration, Problem (4-1)

4.10.2 Problem (4-1) Given

- Equipment drawings.
- The wall is flat, and .01 ft. thick.
- Identity of source fluid, sink fluid, and wall material.
- Flow rate of source fluid and sink fluid.
- Equations (4-16) and (4-17), obtained from conventional engineering literature.

$$N_{Nu} = .023\ N_{Re}^{.8}\ N_{Pr}^{.4} \tag{4-16}$$

$$k_{wall} = 14.5 \tag{4-17}$$

- The dimension units are Btu, hr, ft, F.

4.10.3 Problem (4-1) Analysis

- Transform Eq. (4-16) to $\Delta T_{BL} = f\{q\}$ by substituting $(qD/\Delta Tk)$ for N_{Nu}, separating q and ΔT, and rearranging. The result is Eq. (4-18).

$$\Delta T_{BL} = q/(.023\ (k/D)\ N_{Re}^{.8}\ N_{Pr}^{.4}) \tag{4-18}$$

(Note that this transformation will not be necessary after the new engineering has been globally accepted for some time.)

- Obtain a $\Delta T = f\{q\}$ equation for each boundary layer by evaluating Eq. (4-18) at the given conditions. Assume this was done, and Eqs. (4-19) and (4-20) resulted.

$$\Delta T_1 = q/158 \tag{4-19}$$

$$\Delta T_2 = q/85 \tag{4-20}$$

- Obtain a specific $\Delta T = f\{q\}$ equation for the wall based on the given information.

$$\Delta T_{wall} = q\ t_{wall}/k_{wall} = q\ (.01/14.5)\Delta T_W = q/1450 \tag{4-21}$$

- Obtain an equation that relates q and ΔT_{TOTAL}:

- Note that the configuration in Figure (4-3) is described by Eqs. (4-22) and (4-23).

$$\Delta T_{TOTAL} = T_1 - T_2 = \Delta T_1 + \Delta T_{wall} + \Delta T_2 \quad (4\text{-}22)$$

$$q_1 = q_W = q_2 = q \quad (4\text{-}23)$$

- Combine Eqs. (4-19) to (4-21) and Eq. (4-22), and use Eq. (4-23). Eq. (4-24) is the result.

$$\Delta T_{TOTAL} = (370 - 80) = q/158 + q/85 + q/1450 \quad (4\text{-}24)$$

- Solve Eq. (4-24) for q. The result is q = 15,400.

- The wall temperature at Interface 1 is determined from Eq. (4-25), using Eq. (4-19) and the calculated value of q.

$$T_{wall\ int1} = T_1 - \Delta T1 \quad (4\text{-}25a)$$

$$T_{wall\ int1} = 370 - 15400/158 = 273 \quad (4\text{-}25b)$$

- The wall temperature at Interface 2 is determined from Eq. (4-26) using Eq. (4-20) and the calculated value of q.

$$T_{wall\ int2} = T_2 + \Delta T_2 \quad (4\text{-}26)$$

$$T_{wall\ int2} = 80 + 15400/85 = 261 \quad (4\text{-}27)$$

4.10.4 Problem (4-1) Answer

- The heat flux in Figure (4-3) is 15,400 B/hrft^2.

- The wall temperature at Interface 1 is 273 F.

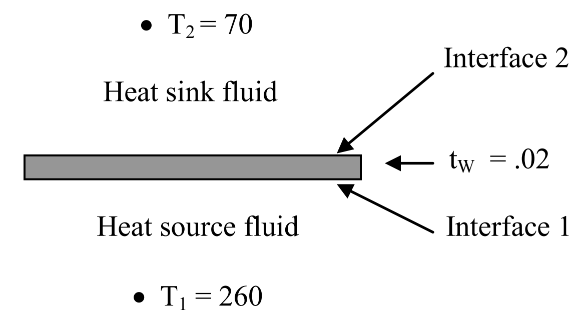
- The wall temperature at Interface 2 is 261 F.

4.11 Heat transfer behavior methodology—Problem (4-2).

Problem (4-2) differs from Problem (4-1) in that the boundary layers exhibit moderately nonlinear behavior. In spite of the nonlinearity, the solution of the problem is simple and direct using behavior methodology. Note in the next chapter that the solution of this problem is *indirect* (and considerably more difficult) if h methodology is used.

4.11.1 Problem (4-2) Statement

What is the heat flux in the Figure (4-4) configuration? What is the wall temperature at each interface?



<u>Figure 4-4</u> Heat transfer configuration, Problem 4.11

4.11.2 Problem (4-2) Given

- Equipment drawings
- Identity of source fluid, sink fluid, and wall material
- Source fluid and sink fluid are stagnant.
- Equations (4-28) to (4-30) were obtained from conventional engineering literature.

$$N_{Nu1} = 0.15\,(N_{Gr1}\,N_{Pr1})^{.33} \tag{4-28}$$

$$N_{Nu2} = 0.47\,(N_{Gr2}\,N_{Pr2})^{.20} \tag{4-29}$$

$$k_w = 8.6 \tag{4-30}$$

- The dimension units are Btu, hr, ft, F.

4.11.3 Problem (4-2) Analysis

- Transform Eqs. (4-28) and (4-29) from heat transfer coefficient correlations to heat transfer behavior correlations by substituting ($qL/\Delta TK$) for N_{Nu}, and separating q and ΔT.

$$q_1 = 0.15(k/L)(g\beta L^3/\nu^2)^{.33}\,N_{Pr}^{.33}\,\Delta T_1^{1.33} \tag{4-31}$$

$q_2 = 0.37(k/L)(g\beta L^3/\nu^2)^{.20}\ N_{Pr}^{.20}\ \Delta T_2^{1.20}$ (4-32)

- Evaluate the parameter groups in Eqs. (4-31) and (4-32) using the given information and the literature. Equations (4-33) and 4-34) result.

$q_1 = 2.80\ \Delta T_1^{1.33}$ (4-33)

$q_2 = 1.64\ \Delta T_2^{1.20}$ (4-34)

- Obtain a q{ΔT} equation for the wall.

$q_w = (k_w/t_w)\ \Delta T_w = (8.6/.02)\ \Delta T_w = 430\ \Delta T_w$ (4-35)

- Obtain an equation that relates q and $\Delta T_{TOTAL.}$
  - Obtain Eqs. (4-36) and (4-37) by inspection of Figure (4-4).

    $\Delta T_{TOTAL} = T_1 - T_2 = \Delta T_1 + \Delta T_W + \Delta T_2$ (4-36)

    $q_1 = q_W = q_2 = q$ (4-37)

  - Use Eqs. (4-33) to (4-35) to substitute for ΔT_1, ΔT_2, and ΔT_W in Eq. (4-36). Combine the result with Eq. (4-37).

    $\Delta T_{TOTAL} = 260 - 70 = (q/2.80)^{.75} + (q/1.64)^{.833} + (q/430)$ (4-38)

- Solve Eq. (4-38) and obtain q = 585.

- The wall temperature at Interface 1 is obtained from Eq. (4-39), using Eq. (4-33) and the calculated value of q.

$T_{W1} = T_1 - \Delta T_1$ (4-39)

$T_{W1} = 260 - (585/2.80)^{.75} = 205$

- The wall temperature at Interface 2 is obtained from Eq. (4-40), using Eq. (4-34) and the calculated value of q.

$T_{W2} = T_2 + \Delta T_2$ (4-40)

$\therefore T_{W2} = 70 + (585/1.64)^{.833} = 204$

4.11.4 Problem (4-2) Answer

- The wall heat flux in Figure (4-4) is 585 Btu/hrft^2.
- The wall temperature at Interface 1 is 205 F.
- The wall temperature at Interface 2 is 204 F.

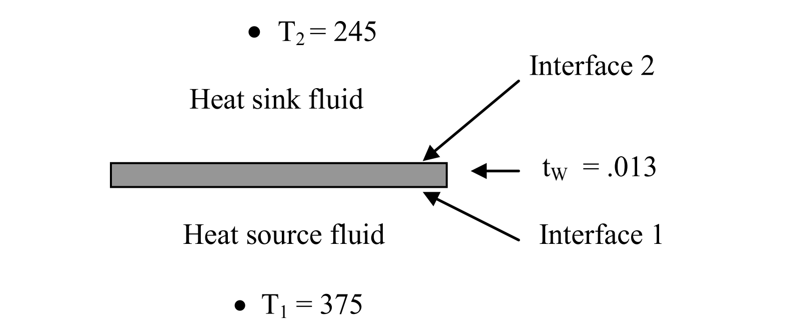
4.12 Heat transfer behavior methodology—Problem (4-3).

Problem (4-3) differs from Problems (4-1) and (4-2) in that one of the boundary layers exhibits *highly* nonlinear behavior. In spite of the pronounced nonlinearity, the solution is simple and direct using behavior methodology.

In the next chapter, this same problem is stated in terms of h methodology. The reader is encouraged to solve the problem using h. Note that the solution using h is *in*direct, and is *much* more difficult.

4.12.1 Problem (4-3) Statement

What is the heat flux in the Figure (4-5) configuration? What is the wall temperature at each interface?



<u>Figure 4-5</u> Heat transfer configuration, Problem (4-3)

4.12.2 Problem (4-3) Given

- Equipment drawings
- Identity of source fluid, sink fluid, and wall material
- Flow rate of source fluid and sink fluid

- The heat transfer coefficient correlation for boundary layer 1 is Eq. (4-41).

$$N_{Nu} = .023\ N_{Re}^{.8}\ N_{Pr}^{.4} \tag{4-41}$$

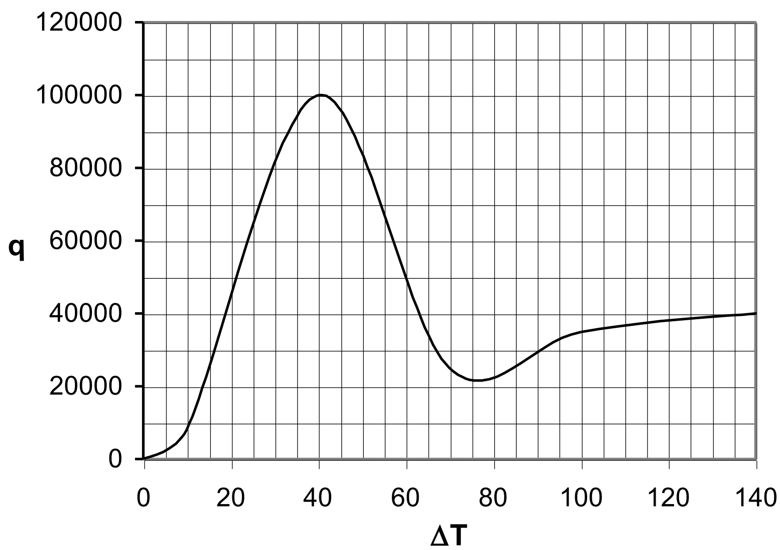
- The heat transfer behavior of boundary layer 2 is described in Figure (4-6).

- The thermal conductivity of the wall is

$$k_w = 110 \tag{4-42}$$

- The dimension units are Btu, hr, ft, F.

Figure 4-6 Heat transfer behavior of Interface 2, Problem (4-3)



4.12.3 Problem (4-3) Analysis

- Transform Eq. (4-41) from a heat transfer coefficient correlation to a heat transfer behavior correlation by substituting (q/ΔT) for *h*, and separating q and ΔT. The result is Eq. (4-43).

$$q = .023\ (k/D)\ N_{Re}^{.8}\ N_{Pr}^{.4}\ \Delta T \qquad (4\text{-}43)$$

- Obtain a specific q{ΔT} equation for boundary layer 1 by evaluating Eq. (4-43) at the given conditions. Assume the result is Eq. (4-44).

$$q_1 = 775\ \Delta T_1 \qquad (4\text{-}44)$$

- Obtain a specific q{ΔT} equation for the wall.

$$q_W = (k_{Wall}/t_{Wall})\ \Delta T_{Wall} = (110/.013)\ \Delta T_W = 8460\ \Delta T_W \qquad (4\text{-}45)$$

- Calculate coordinates of $q\{\Delta T_{TOTAL}\}$:

  - Inspect Figure (4-5) and note that

    $$\Delta T_{TOTAL} = T_1 - T_2 = \Delta T_1 + \Delta T_W + \Delta T_2 \qquad (4\text{-}46)$$

    $$q_1 = q_W = q_2 = q \qquad (4\text{-}47)$$

  - Select $(q_2, \Delta T_2)$ coordinates from Figure (4-6).

  - Calculate $\Delta T_1\{q_2\}$ using Eqs. (4-44) and (4-47).

  - Calculate $\Delta T_W\{q_2\}$ using Eq. (4-45) and (4-47).

  - Calculate $\Delta T_{TOTAL}\{q\}$ using Eq. (4-46).

  - The calculated $(q, \Delta T_{TOTAL})$ coordinates are in Table (4-1).

- Plot $(q, \Delta T_{TOTAL})$ coordinates from Table (4-1) in Figure (4-7).

$q_{2\ or}\ q$	ΔT_2	ΔT_1	ΔT_W	ΔT_{TOTAL}
5000	8	6	1	15
10000	11	13	1	25
20000	14	26	2	42
40000	19	52	5	76
60000	24	77	7	108
80000	30	103	9	142
90000	33	116	11	160
100000	40	129	12	181
90000	48	116	11	175
80000	51	103	9	164
60000	57	77	7	141
40000	63	52	5	120
30000	67	39	4	110
22000	75	28	3	106
30000	90	39	4	133
35000	100	45	4	149

Table 4-1 Calculation of (q, ΔT_{TOTAL}) coordinates, Problem (4-3)

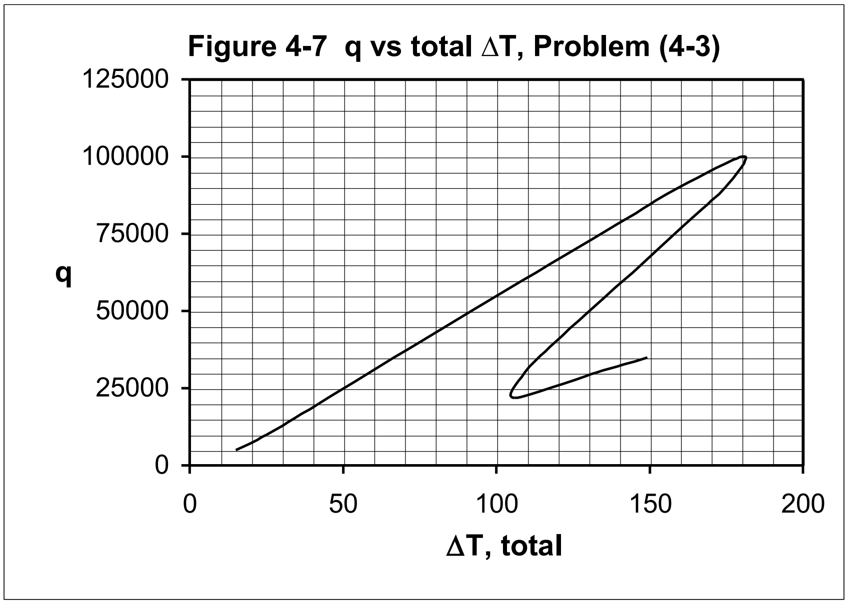
- Figure (4-7) indicates that q{ΔT_{TOTAL} = 130}= 29000, or 50000, or 72000. The given information is not sufficient to uniquely determine q{ΔT_{TOTAL} = 130}.

- Eqs. (4-48) and (4-49) are obtained by inspection of Figure (4-5) .

$$T_{W1} = T_1 - \Delta T_1 = 375 - \Delta T_1 \qquad (4\text{-}48)$$

$$T_{W2} = T_{W1} - \Delta T_W \qquad (4\text{-}49)$$

- The wall temperature at Interface 1 is obtained from Eq. (4-48), using Eq. (4-44) and the q values from Figure (4-7). The results are T_{W1} = 338, or 310, or 282 at q = 29000, or 50000, or 72000.



- The wall temperature at Interface 2 is obtained from Eq. (4-49), using Eq. (4-45) and the q values from Figure (4-7). The results are T_{W2} = 335 or 304 or 273 at q = 29000 or 50000 or 72000.

4.12.4 Problem (4-3) Answer

The given information is not sufficient to determine a unique solution at q{ΔT_{TOTAL} = 130}. The three possible solutions are:

q	T_{W1}	T_{W2}
29000	338	335
50000	310	304
72000	282	273

(In order to determine a unique solution at q{ΔT_{TOTAL} = 130}, it is necessary to know the value of ΔT_{total}{t} since the last time the system operated below the hysteresis region—ie at ΔT_{total} less than 105.)

4.13 Conclusions

Proportional and nonlinear heat transfer problems are solved in a simple and direct manner using behavior methodology.

Chapter 5

The heat transfer coefficient form of the problems in Chapter 4.

5 Summary

The problems in this chapter demonstrate that problem solutions based on h are simple and direct if the phenomena of concern exhibit proportional behavior, but are neither simple nor direct if the phenomena of concern exhibit nonlinear behavior. Because h is $q/\Delta T$, h makes it necessary to solve nonlinear problems in an *indirect* and unnecessarily complex manner. Nonlinear problems can be solved in a direct and simple manner *only* if q and ΔT are kept *separated*, as in behavior methodology.

In Chapter 4, heat transfer problems are stated and solved using heat transfer behavior methodology. In this chapter, the problems in Chapter 4 are restated (but not solved) using h methodology

The problems, figures, and equations in this chapter are *identical* to those in Chapter 4. They differ only in form. The behavior form is used throughout Chapter 4, the coefficient form is used throughout this chapter.

Corresponding problems, figures, and equations in this chapter have the same identifying numbers used in Chapter 4, except that 5 replaces 4. For example, Problem (5-1) is the coefficient form of Problem (4-1), and Eq. (5-20) is the coefficient form of Eq. (4-20).

The reader is encouraged to solve the problems in this chapter using h methodology. By comparing her/his h methodology solutions with the behavior solutions presented in Chapter 4, the reader will gain a good appreciation of the simplicity that results from using heat transfer behavior methodology instead of h methodology.

5.1 Heat transfer coefficient methodology—Problem (5-1).

Problem (5-1) is identical to Problem (4-1) except that it is expressed in terms of h methodology, and is intended to be solved by the reader using h methodology. All heat transfer elements in the problem exhibit proportional behavior, and consequently the solution of Problem (5-1) is simple and direct using h methodology.

5.1.1 Problem (5-1) Statement

What is the heat flux in the Figure (5-3) configuration? What is the wall temperature at each interface?

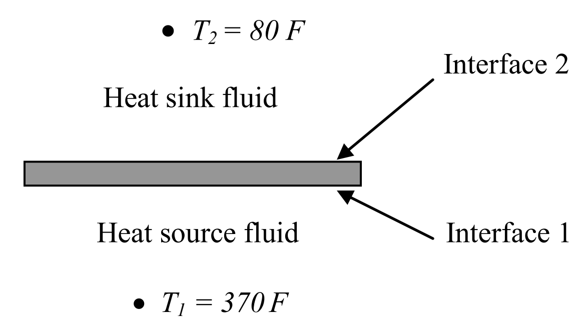


Figure 5-3 Heat transfer configuration, Problem (5-1)

5.1.2 Problem (5-1) Given

- Equipment drawings

- The wall is flat, and .01 ft. thick.

- Identity of source fluid, sink fluid, and wall material

- Flow rate of source fluid and sink fluid

- The heat transfer coefficients for boundary layer 1 and boundary layer 2 are described by Eq. (5-16), obtained from conventional engineering literature literature.

$$N_{Nu} = .023\ N_{Re}^{.8}\ N_{Pr}^{.4} \qquad (5\text{-}16)$$

- The thermal conductivity of the wall is described by Eq. (5-17).

$$k_{wall} = 14.5\ \text{Btu/hrft} \qquad (5\text{-}17)$$

- The fluid and geometry parameters in Eq. (5-16) were evaluated using given information and the literature. Eqs. (5-19) and (5-20) resulted.

$$h_1 = 158 \text{ Btu/hrft}^2\text{F} \qquad (5\text{-}19)$$

$$h_2 = 85 \text{ Btu/hrft}^2\text{F} \qquad (5\text{-}20)$$

5.1.3 Problem (5-1) Analysis and answer
(To be determined by the reader.)

5.2 Heat transfer coefficient methodology—Problem (5-2)
In Problem (5-2), the boundary layers exhibit moderately nonlinear behavior. Because of the nonlinearity, the solution of the problem is necessarily *indirect* (and considerably more difficult) if it is based on heat transfer coefficient methodology.

.

5.2.1 Problem (5-2) Statement
What is the heat flux in the Figure (5-4) configuration? What is the wall temperature at each interface?

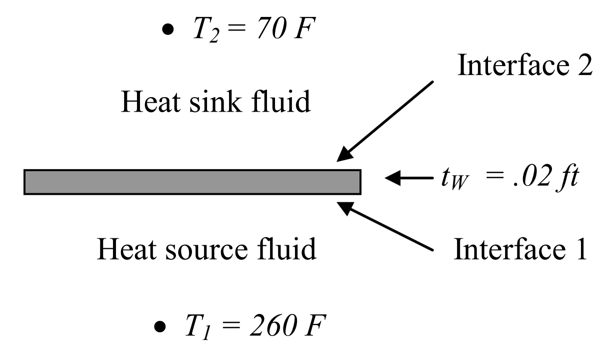


Figure 5-4 Heat transfer configuration, Problem (5-2)

5.2.2 Problem (5-2) Given

- Equipment drawings

- Identity of source fluid, sink fluid, and wall material

- Source fluid and sink fluid are stagnant.

- The heat transfer coefficient at boundary layer 1 is described by Eq. (5-28). The heat transfer coefficient at boundary layer 2 is described by Eq. (5-29).

  $$N_{Nu1} = 0.15\ (N_{Gr1}\ N_{Pr1})^{.33} \qquad (5\text{-}28)$$

  $$N_{Nu2} = 0.47\ (N_{Gr2}\ N_{Pr2})^{.20} \qquad (5\text{-}29)$$

- The thermal conductivity of the wall is given by Eq. (5-30).

  $$k_W = 8.6 \text{ Btu/hrftF} \qquad (5\text{-}30)$$

- Evaluate the parameter groups in Eqs. (5-28) and (5-29) using the given information and the literature. Equations (5-33) and (5-34) result.

  $$h_1 = 2.80\ \Delta T_1^{0.33} \qquad (5\text{-}33)$$

  $$h_2 = 1.64\ \Delta T_2^{0.20} \qquad (5\text{-}34)$$

5.2.3 Problem (5-2) Analysis and answer

(To be determined by the reader.)

5.3 Heat transfer coefficient methodology—Problem (5-3)

Problem (5-3) differs from Problems (5-1) and (5-2) in that one of the boundary layers exhibits *highly* nonlinear behavior.

Because of the highly nonlinear behavior, the solution of the problem is *extremely* difficult if heat transfer coefficient methodology is used.

The reader is encouraged to solve the problem using h. The solution of this problem using heat transfer coefficients is so difficult that it is unlikely that any reader will determine the correct, complete solution.

5.3.1 Problem (5-3) Statement

What is the heat flux in the Figure (5-5) configuration? What is the wall temperature at each interface?

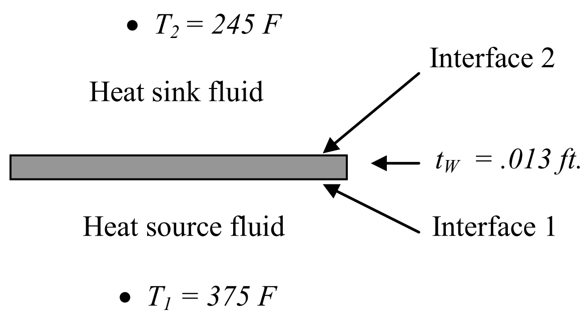


Figure 5-5 Heat transfer configuration, Problem (5-3)

5.3.2 Problem (5-3) Given

- Equipment drawings

- Identity of source fluid, sink fluid, and wall material

- Flow rate of source fluid and sink fluid.

- The heat transfer coefficient at boundary layer 1 is described by Eq. (5-41).

$$N_{Nu} = .023\, N_{Re}^{.8}\, N_{Pr}^{.4} \tag{5-41}$$

- The fluid and geometry parameters in Eq. (5-41) were evaluated, and Eq. (5-44) resulted.

$$h_1 = 775\ \text{Btu/hrft}^2\text{F} \tag{5-44}$$

- The heat transfer coefficient at boundary layer 2 is described by Figure (5-6).

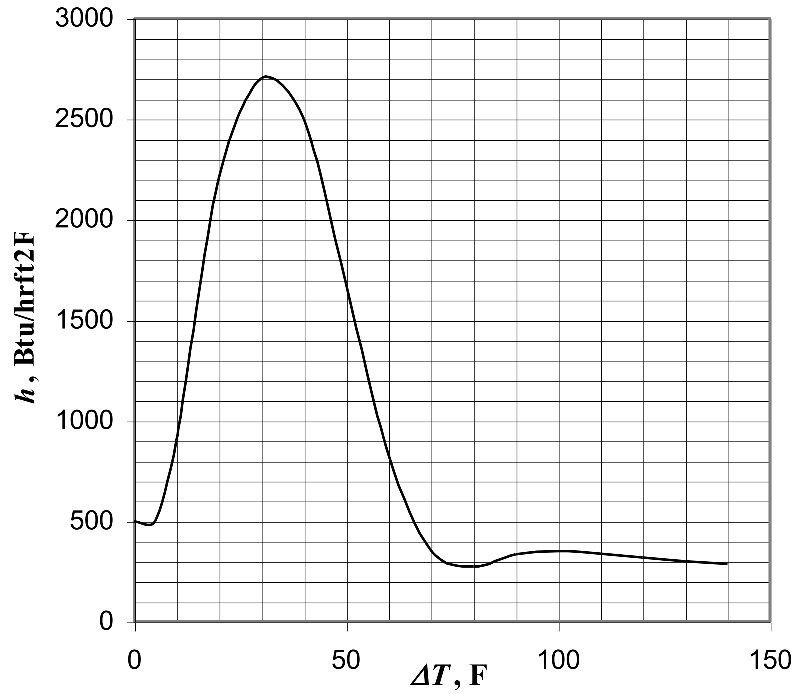


Figure 5-6 Heat transfer coefficient at Interface 2, Problem (5-3)

- The thermal conductivity of the wall is given by Eq. (5-42).

$$k_{wall} = 110 \text{ Btu/hrftF} \tag{5-42}$$

Problem (5-3) Analysis and Answer
(To be determined by the reader.)

Chapter 6

The “thermal stability” of heat transfer systems.

6 Summary

“Thermal instability” (the instability of the heat transfer process) is a practical possibility only if a system includes a boundary layer that exhibits a negative value of ($dq/d\Delta T_{BL}$) in the system operating range. This type of behavior is commonly exhibited only by boiling boundary layers, and thus thermal instability is a practical problem only in boiling systems. The problem is particularly severe in boiling liquid metal systems.

In this chapter, the thermal stability of heat transfer systems and the behavior of unstable heat transfer systems are analyzed using behavior methodology—ie using $q = f\{\Delta T\}$. The analyses can also be performed using coefficient methodology, but the extreme nonlinearity involved causes stability analyses based on coefficient methodology to be so difficult that there is little point in considering them.

The problems in this chapter illustrate that behavior methodology deals with heat transfer systems in a simple and direct manner, even if they contain boundary layers that exhibit the highly nonlinear behavior that can cause hysteresis and undamped oscillations.

6.1 Thermal stability.

The thermal stability analyses in this chapter answer the question:

> *If a heat transfer system is initially operating at a potential operating point, will the system resist a very small perturbation, and return to the potential operating point?*

If the answer is “no”, the system is “unstable” at the potential operating point—ie it will *not* operate in a steady-state manner at that point. However, it may be quite stable at other potential operating points.

If the answer is “yes”, the system is *conditionally* “stable” at the potential operating point—ie it will operate in a steady-state manner at that point *provided* all perturbations are small. The system is *conditionally* stable because, even though it is stable with respect to *small* perturbations, it may be unstable with respect to *large* perturbations.

6.2 The effect of thermal instability.

If a system includes a boundary layer that exhibits a highly nonlinear $q\{\Delta T_{BL}\}$, and the system is initially operating at an unstable point, the system will tend to leave the unstable point. One of the following will result:

- The system heat transfer rate will monotonically increase or decrease until the system arrives at a stable operating point. The system will remain at that operating point.

- The system will operate in an undamped oscillatory manner until a change in the system settings causes a transition to a stable operating point.

The $q\{\Delta T_{BL}\}$ behavior of the highly nonlinear boundary layer determines whether or not undamped oscillatory behavior is a possibility. It is a possibility *only* if the $q\{\Delta T_{BL}\}$ behavior of the highly nonlinear boundary layer contains a region in which $dq/d\Delta T_{BL}$ is negative, *and* the region is bounded by points at which $dq/d\Delta T$ is infinite.

6.3 Thermal stability analysis.

In stability analysis, it is often convenient to:

- Uncouple the system—ie divide it into two subsystems.

- Analytically determine the behavior of each subsystem.

- Analytically determine the system performance that would result from coupling the subsystems.

The above method is used here to analyze the thermal stability of heat transfer systems. The systems analyzed contain a heat source fluid, a heat sink fluid, and a wall that separates the two fluids. One of the fluid boundary layers exhibits behavior that includes a region in which $(dq/d\Delta T_{BL})$ is negative. The method includes the following steps:

- Uncouple the system at the interface that adjoins the highly nonlinear boundary layer. One subsystem includes the highly nonlinear boundary layer and its fluid. The other subsystem includes the wall, the other boundary layer, and its fluid.

- The wall surface that adjoins the nonlinear boundary layer is the interface between the two subsystems, and is referred to by the subscript INTERFACE.

- Subscript "IN" refers to the subsystem that includes the heat source fluid. IN is used to indicate that heat flows from the source fluid INto the interface.

- Subscript "OUT" refers to the subsystem that includes the heat sink fluid. OUT is used to indicate that heat flows OUT of the interface and into the sink fluid.

- Determine $q_{IN}\{T_{INTERFACE}\}$.

- Determine $q_{OUT}\{T_{INTERFACE}\}$.

- Plot $q_{IN}\{T_{INTERFACE}\}$ and $q_{OUT}\{T_{INTERFACE}\}$ together on the same graph.

- Note that intersections of $q_{IN}\{T_{INTERFACE}\}$ and $q_{OUT}\{T_{INTERFACE}\}$ are potential operating points.

- Use Criterion (6-1) to appraise thermal stability at potential operating points.

6.4 The criterion for thermal instability.

Criterion (6-1) is the criterion for thermal instability:

$$dq_{IN}/dT_{INTERFACE} \geq_U dq_{OUT}/dT_{INTERFACE} \qquad (6\text{-}1)$$

The criterion states:

> If a heat source subsystem is coupled to a heat sink subsystem, the heat transfer will be *unstable* at a potential operating point if $dq_{IN}/dT_{INTERFACE}$ is greater than or equal to $dq_{OUT}/dT_{INTERFACE}$. (The $\geq_U$ symbolism indicates "unstable if satisfied".)

Note that:

- In order to accurately appraise stability, the system *must* be uncoupled at the interface that adjoins the boundary layer that exhibits highly nonlinear thermal behavior.

- The criterion describes thermal stability with regard to *very small* perturbations.

- If the criterion is satisfied at a potential operating point, the heat transfer is unstable at that potential operating point.

- If the criterion is *not* satisfied at a potential operating point, the heat transfer is stable at that potential operating point with respect to very small perturbations. However, it may be unstable with respect to large perturbations.

In this chapter, heat transfer is "stable" at a potential operating point if Criterion (6-1) is *not* satisfied. However, it must be recognized that "stable" is used as a shorthand expression for "stable with respect to very small perturbations".

The system design objective is generally "stable with respect to perturbations inherent in the system". Fortunately, perturbations inherent in real systems are generally quite small. Thus there is usually little practical difference between "stable with respect to small perturbations", and "stable with respect to perturbations inherent in the system".

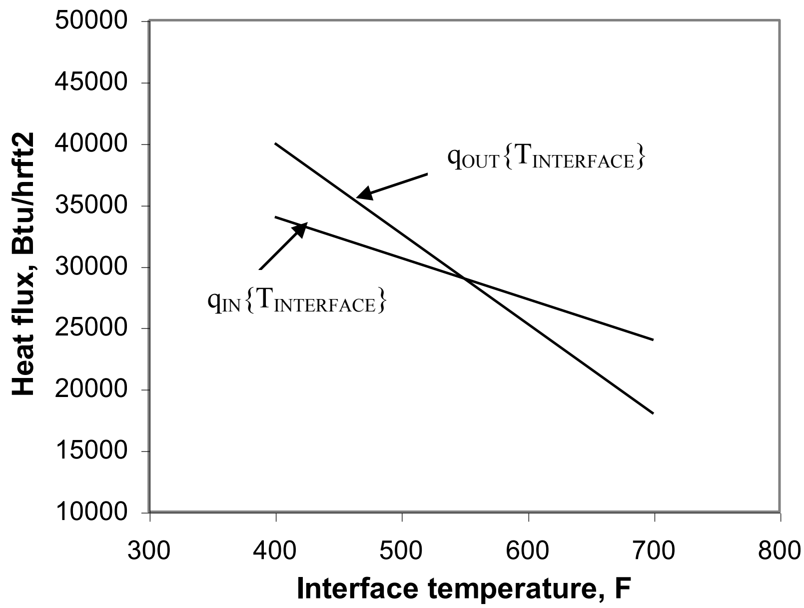
6.5 Verifying Criterion (6-1).

Criterion (6-1) can be verified by showing that, if a heat source/sink system is initially at a potential operating point, a small perturbation will tend to grow if Criterion (6-1) is satisfied.

Figure (6-1) describes the heat transfer behavior of uncoupled subsystems. The intersection in Figure (6-1) is a potential operating point because q_{IN} and q_{OUT} are equal at the intersection. The stability at the intersection can be appraised in the following way:

- Assume that the system described in Figure (6-1) is initially operating at the intersection.

Figure 6-1 Potential operating point



- Suddenly the system experiences a very small, positive perturbation in $T_{INTERFACE}$.

- The positive perturbation causes $q_{IN}\{T_{INTERFACE}\}$ to be greater than $q_{OUT}\{T_{INTERFACE}\}$. In other words, the heat flow into the interface exceeds the heat flow out of the interface.

- Because the heat flow into the interface is greater than the heat flow out, the temperature of the interface *increases* with time.

- An increasing $T_{INTERFACE}$ indicates that the system is not returning to the potential operating point. Therefore the intersection in Figure (6-1) is an unstable operating point.

- To determine whether Criterion (6-1) also indicates instability, note that the slope of $q_{IN}\{T_{INTERFACE}\}$ is greater than the slope of $q_{OUT}\{T_{INTERFACE}\}$ Since this satisfies Criterion (6-1), the criterion indicates instability. (Note that, since both slopes are negative, the *greater* slope is *less* steep.)

- Since the above analysis and Criterion (6-1) are in agreement, the analysis validates Criterion (6-1).

- Notice that, if $q_{IN}\{T_{INTERFACE}\}$ and $q_{OUT}\{T_{INTERFACE}\}$ were interchanged in Figure (6-1), a positive perturbation would cause $T_{INTERFACE}$ to decrease with time. Therefore Criterion (6-1) would not be satisfied, and the system would be stable at the intersection.

6.6 Hysteresis in heat transfer systems—Problem (6-1).
Problem (6-1) demonstrates:

- How to analyze a heat transfer system for thermal instability.

- How to determine the effect of thermal instability on system performance.

6.6.1 Problem (6-1) Statement
Describe the performance of the heat transfer system in Figure (6-2) for the source fluid temperature range of 250 to 475 F. (Note that the system may be envisioned as a differential element in a vented pool boiler.)

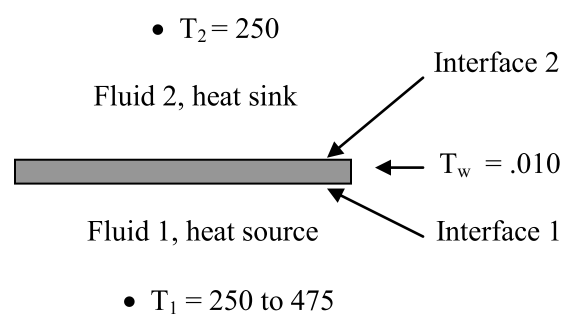


Figure 6-2 Heat transfer configuration, Problem (6-1)

6.6.2 Problem (6-1) Given
- Equipment drawings.

- Identity of heat source fluid, heat sink fluid, and wall material.

- Flow rate of source fluid and sink fluid.

- The heat transfer behavior of boundary layer 1 is described by Eq. (6-2).

$$q_1 = .023\ (k/D)\ N_{Re}^{.8}\ N_{Pr}^{.4}\ \Delta T_1 \qquad (6\text{-}2)$$

- Evaluation of Eq. (6-2) gives Eq. (6-3).

$$q_1 = 830\ \Delta T_1 \qquad (6\text{-}3)$$

- The heat transfer behavior of the wall is described by Eq. (6-4).

$$q_{wall} = 113\ \Delta T/t_{wall} \qquad (6\text{-}4)$$

- The behavior of boundary layer 2 is described by Figure (6-3).

- The dimension units are Btu, hr, ft, F.

6.6.3 Problem (6-1) Analysis

- Substitute in Eq. (6-4), and obtain Eq. (6-5).

$$q_{wall} = 11300\ \Delta T_{wall} \qquad (6\text{-}5)$$

Uncouple the system at Interface 2 (the interface adjacent to Fluid 2), and determine $q_{IN}\{T_{int\ 2}\}$ and $q_{OUT}\{T_{int\ 2}\}$.

- Determine $q_{IN}\{T_{int\ 2}\}$ from inspection of Figure (6-2), and Eqs. (6-3) and (6-5).

$$T_1 - T_{int\ 2} = \Delta T_1 + \Delta T_{wall} = q_1/830 + q_{wall}/11300 \qquad (6\text{-}6)$$

$$q_1 = q_W = q_{int\ 2} = q \qquad (6\text{-}7)$$

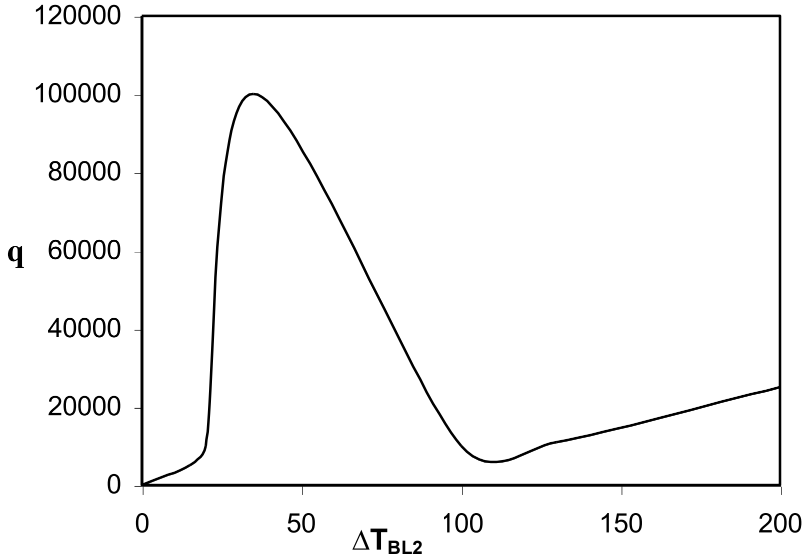
$$T_1 - T_{int2} = q/830 + q/11300 = q_{IN}/773 \qquad (6\text{-}8)$$

$$q_{IN} = 773\ (T_1 - T_{int\ 2}) \qquad (6\text{-}9)$$

Note that Figure (6-3) is in the form $q_{OUT}\{\Delta T_{OUT}\}$, and transform it to $q_{OUT}\{T_{int\ 2}\}$ using Eq. (6-10). In other words, transform Figure (6-3) to the desired form by adding 250 to the coordinates on the x axis. The result is the curve in Figure (6-4).

$$T_{int\ 2} = T_2 + \Delta T_{BL2} = 250 + \Delta T_{BL2} \qquad (6\text{-}10)$$

Figure 6-3 Heat transfer behavior of BL2, Problem (6-1)



- On Figure (6-4), plot Eq. (6-9) for various values of T_1 over the operating range from 250 to 475 F.

- Note that intersections in Figure (6-4) are potential operating points.

6.6.4 Problem (6-1) Answer
Figure (6-5) is the answer to Problem (6-1). It is a $q\{T_1)$ chart that covers the source fluid temperature range of 250 to 475 F. The chart is prepared as follows:

- Note from Eq. (6-9) that $T_1 = T_{int\,2}\{q = 0\}$. Therefore, on each of the straight lines in Fig. (6-4), T_1 equals the value of $T_{int\,2}$ at $q = 0$.

- Determine (q,T_1) coordinates at intersections from Figure (6-4).

- Appraise the stability at each intersection by applying Criterion (6-1).

- Plot the $(q,T_1\}$ coordinates of the stable intersections on Figure (6-5). Do not plot the coordinates of unstable intersections because the system will not remain at unstable intersections.

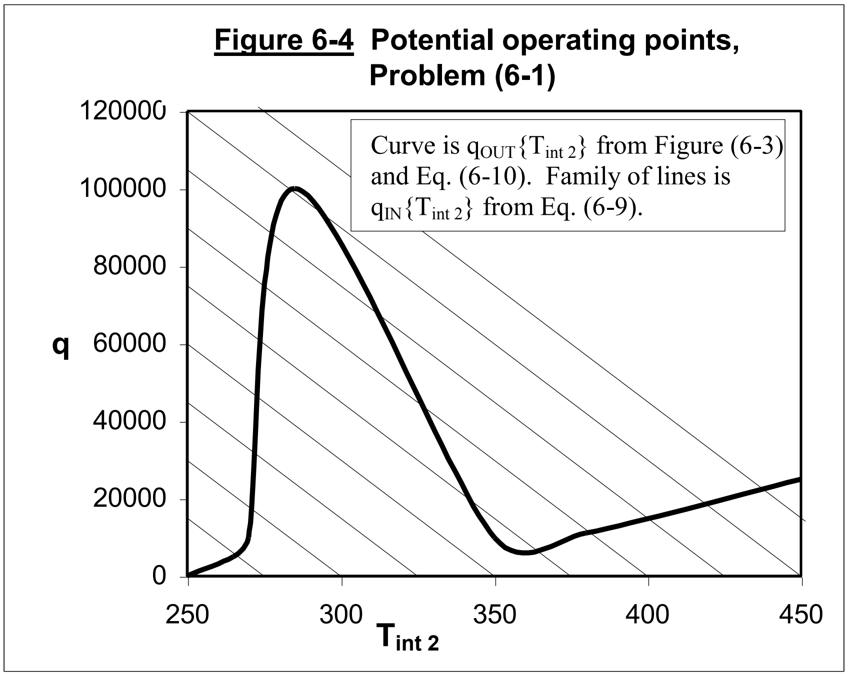
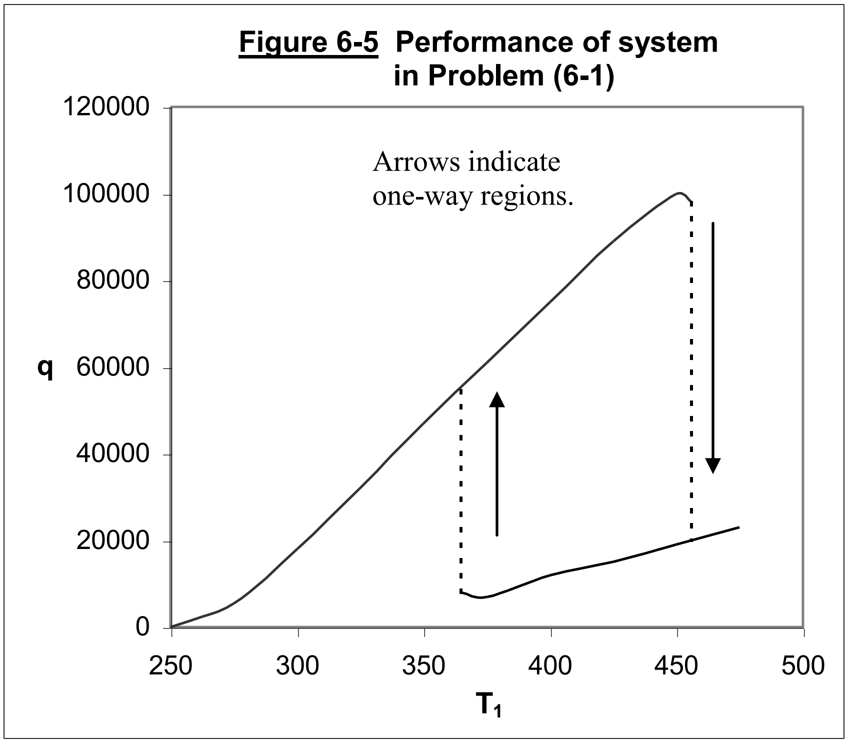


Figure (6-5) is the desired answer—a description of system performance $q\{T_1\}$ over the T_1 range of 250 to 475 F. Note the pronounced hysteresis when T_1 is in the range 365 to 455 F.

6.7 How to eliminate hysteresis.

The system in Problem (6-1) can be modified to eliminate hysteresis in either of two ways:

- Adjust the T_1 controls so that T_1 cannot be increased above 440 F. This would prevent hysteresis, and would allow the system to operate at maximum heat flux.
- Modify the system so that the slope of the $q_{IN}\{T_{int\,2}\}$ lines in Fig. (6-4) is more negative than the most negative slope of the $q_{OUT}\{T_{int\,2}\}$ curve.
- Inspection of Figure (6-4) indicates that the most negative slope of the $q_{OUT}\{T_{int\,2}\}$ curve is approximately -1670 B/hrft^2. Therefore, in order to prevent hysteresis over the full range of the equipment, the modification must result in $dq_{IN}/dT_{int\,2} < -1670$. In other words, the constant in Eq. (6-9) must be increased to at least 1670.



The constant in Eq. (6-9) is determined by the heat transfer behavior of boundary layer 1 and the heat transfer wall. The former is described by Eq. (6-3), the latter by Eq. (6-4). Assuming that the heat transfer wall cannot be changed, the constant in Eq. (6-9) can be increased only by increasing the constant in Eq. (6-3). The required increase is determined in the following way:

- Let x be the value of the constant in Eq. (6-3) that will result in a constant of 1670 in Eq. (6-9).

- Substitute x for 830 in Eq. (6-7), and note from Figure (6-2) that $q_1 = q_W = q_{IN} = q$.

  $\Delta T_{IN} = q/x + q/11300$ (6-11)

- Note that $dq_{IN}/dT_{int\,2} = -dq_{IN}/d\Delta T_{IN}$, and therefore

  $dq_{IN}/d\Delta T_{IN} = 1670$ (6-12)

$\therefore\ q_{IN} = 1670\ \Delta T_{IN}$ (6-13)

- Combine Eqs. (6-11) and (6-13).

$q/1670 = q/x + q/11300$ (6-14)

$\therefore x = 1960$

Therefore, hysteresis would be eliminated over the entire operating range of the system if Boundary Layer 1 were made to exhibit the behavior described by Eq. (6-15).

$q_1 = 1960\ \Delta T_1$ (6-15)

6.8 Validation of the stability analysis in Problem (6-1).

The stability analysis in Problem (6-1) is validated by results reported in Berenson (1960) and (1962), a benchmark pool boiling experiment often cited in heat transfer literature.

The system in Problem (6-1) may be viewed as a small region of a pool boiler in which the heat source is a condensing fluid or a heated liquid, and the heat sink is a boiling liquid. This closely corresponds to the pool boiler in Berenson's experiment.

Figure (6-5) is the result of the stability analysis of the pool boiler in Problem (6-1). The boiler contains a liquid that exhibits the "pool boiling curve" shown in Figure (6-3). The curve in Figure (6-3) resembles the generally accepted pool boiling curve for liquids such as those used in Berenson's experiment. Therefore, if the stability analysis of Problem (6-1) is correct, the results of the stability analysis will agree with behavior reported by Berenson.

Note the following analytical results in Problem (6-1):

- The boiler may, or may not, be able to operate throughout the transition region of the pool boiling curve—ie throughout the region between the maximum and the minimum in Figure (6-3). The ability to operate throughout the transition region depends on the heat transfer behavior of the heat source boundary layer, the design of the heat transfer wall, and the boiling boundary layer.

- The boiler may, or may not, exhibit the pronounced hysteresis shown in Figure (6-5). The presence and extent of the hysteresis depends on the heat transfer behavior of the heat source boundary layer, the design of the heat transfer wall, and the boiling boundary layer.

Note that the following results reported by Berenson do in fact agree with the results of the stability analysis of Problem 6.6.

- Berenson reports the results of 20 runs that purport to "define the characteristic boiling curve completely". The runs included various boiling liquids and various treatments of the boiling surface. Of these runs, 3 contain data throughout the transition region, and 17 contain essentially no data in the transition region. The lack of data in 17 runs surely reflects an inability to obtain the data, since the primary purpose of the experiment was to investigate transition boiling, as reflected in the title of Berenson's thesis.

- Berenson does not report the temperature of the heat source fluid. However, if the temperature of the heat source fluid is estimated from information given in the thesis, it is evident that the boiler exhibited pronounced hysteresis in the 17 runs that contained essentially no data in the transition region.

For a more comprehensive treatment of this subject, see Adiutori (1991).

6.9 Problem (6-2) Undamped oscillation.

Problem (6-2) differs from Problem (6-1) in that the system instability results in undamped oscillation as well as hysteresis. Note that undamped oscillation results even though the heat source temperature and the heat sink temperature are constant.

6.9.1 Problem (6-2) Statement

Describe the performance of the heat transfer system in Figure (6-6) over the source fluid temperature range of 350 to 600 F. (Note that the system may be envisioned as a small region of a vented pool boiler.)

6.9.2 Problem (6-2) Given

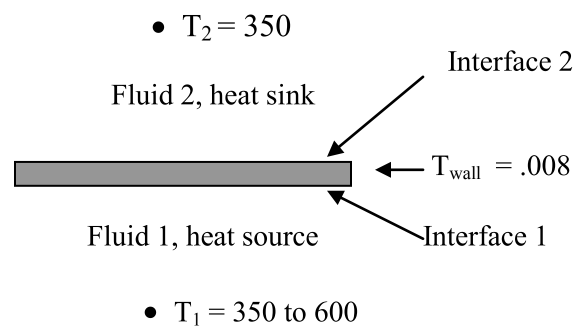
- Equipment drawings.

- Identity of heat source fluid, heat sink fluid, and wall material.

- Flow rate of source fluid and sink fluid.

- The heat transfer behavior of boundary layer 1 is described by Eq. (6-16).

$$q_1 = .023\ (k/D)\ N_{Re}^{.8}\ N_{Pr}^{.4}\ \Delta T_1 \qquad (6\text{-}16)$$



<u>Figure 6-6</u> Heat transfer configuration, Problem (6-2)

- Assume that evaluation of Eq. (6-16) gives Eq. (6-17).

$$q_1 = 760\ \Delta T_1 \qquad (6\text{-}17)$$

- Heat transfer behavior of the wall material is described by Eq. (6-18).

$$q_W = 95\ \Delta T/t_{wall} \qquad (6\text{-}18)$$

- Heat transfer behavior of boundary layer 2 is described by Fig. (6-7).

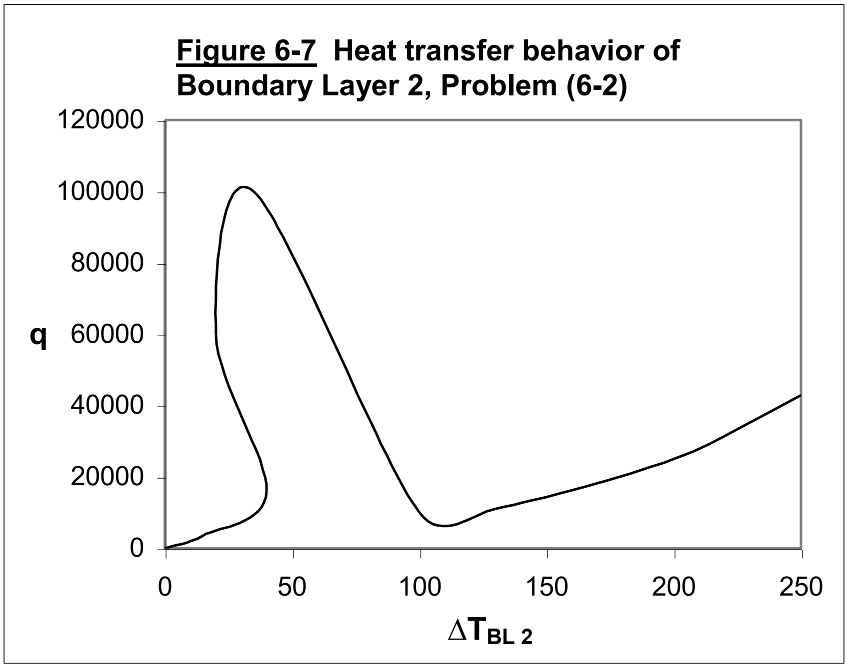
- The dimension units are Btu, hr, ft, F.

6.9.3 Problem (6-2) Analysis—Determination of potential operating points.

- Substitute in Eq. (6-18):

$$q_W = 95\ \Delta T_W/.008 = 11900\ \Delta T_W \qquad (6\text{-}19)$$

- Uncouple the system at Interface 2 (the interface adjacent to Fluid 2), and determine $q_{IN}\{T_{int\ 2}\}$ and $q_{OUT}\{T_{int\ 2}\}$.



- Determine $q_{IN}\{T_{int\,2}\}$ from inspection of Figure (6-6), and from Eqs. (6-17) and (6-19).

$$T_1 - T_{int\,2} = \Delta T_1 + \Delta T_W \tag{6-20}$$

$$T_1 - T_{int\,2} = q_1/760 + q_W/11900 \tag{6-21}$$

$$q_1 = q_{int\,2} = q_{IN} = q \tag{6-22}$$

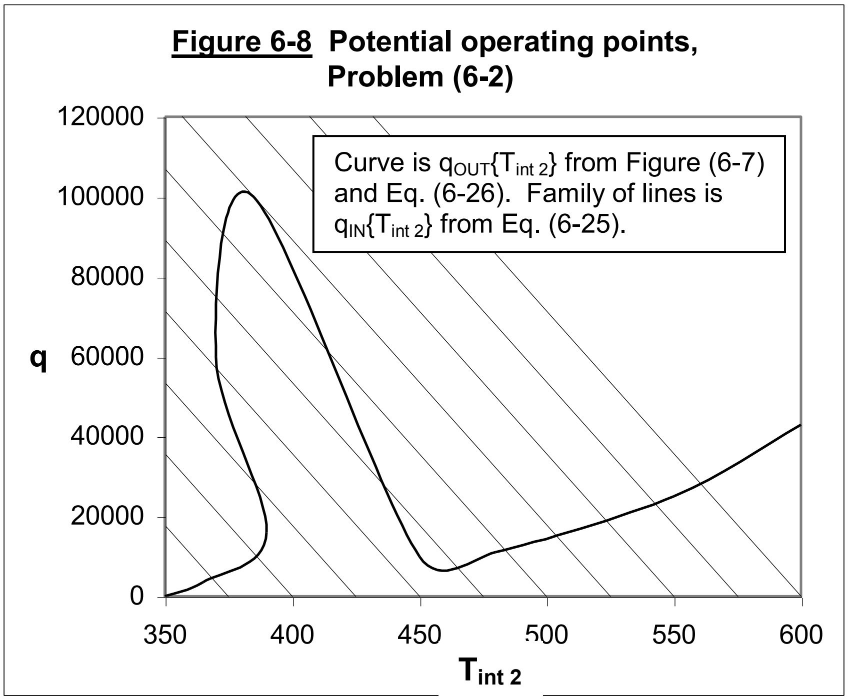
$$T_1 - T_{int\,2} = q/760 + q/11900 \tag{6-23}$$

$$T_1 - T_{int\,2} = q_{IN}/714 \tag{6-24}$$

$$q_{IN} = 714\,(T_1 - T_{int\,2}) \tag{6-25}$$

- Note that Figure (6-7) is in the form $q_{OUT}\{\Delta T_{BL\,2}\}$, and transform the figure to $q_{OUT}\{T_{int\,2}\}$ using Eq. (6-26). In other words, transform Figure (6-7) to the desired form by adding 350 to the coordinates on the x axis. The result is the curve in Figure (6-8).

$$T_{int\,2} = T_2 + \Delta T_{\,BL\,2} = 350 + \Delta T_{BL\,2} \tag{6-26}$$



- On Figure (6-8), plot Eq. (6-25) for various values of T_1 over its operating range of 350 to 600 F.

- Note that intersections in Figure (6-8) are potential operating points of the system.

6.9.4 Problem (6-2) Analysis—Thermal stability at the potential operating points in Figure (6-8).

With regard to stability at the potential operating points in Figure (6-8), note the following:

- In Figure (6-8), $q_{OUT}\{T_{int\,2}\}$ includes a maximum and a minimum in $q_{OUT}\{T_{int\,2}\}$, *and* a maximum and a minimum in $T_{int\,2}\{q_{OUT}\}$.

- The maximum and minimum in $q_{OUT}\{T_{int\,2}\}$ occur at (q_{OUT},$T_{int\,2}$) coordinates of (101000,382) and (7000,460). The maximum and minimum in $T_{int\,2}\{q_{OUT}\}$ occur at ($T_{int\,2}$,q_{OUT}) coordinates of (390,16000) and (370,67000).

- T_1 is constant along $q_{IN}\{T_{int\,2}\}$ lines in Figure (6-8). Eq. (6-25) indicates that $T_1 = T_{int\,2}$ at the x intercepts in Figure (6-8). In other words, $T_1 = T_{int\,2}\{q_{IN} = 0\}$. Therefore the value of T_1 on each of the $q_{IN}\{T_{int\,2}\}$ lines is readily determined by inspection of Figure (6-8).

- Note in Figure (6-8) that if T_1 = 415 to 455:
  - $q_{IN}\{T_{int\,2}\}$ intersects $q_{OUT}\{T_{int\,2}\}$ in the region between the maximum and minimum in $T_{int\,2}\{q_{OUT}\}$.
  - There is only one intersection on each $q_{IN}\{T_{int\,2}\}$ line, and it is unstable.

- Also note in Figure (6-8) that if T_1 = 465 to 525:
  - $q_{IN}\{T_{int\,2}\}$ intersects $q_{OUT}\{T_{int\,2}\}$ in the region between the maximum and minimum in $q_{OUT}\{T_{int\,2}\}$.
  - There are three intersections on each $q_{OUT}\{T_{int\,2}\}$ line, and only the middle intersections are unstable.

6.9.5 Problem (6-2) Analysis—Behavior at unstable, singular solutions.

To determine the system behavior that results from an unstable, singular solution, refer to Figure (6-8), and suppose that the system is initially operating at the intersection on the q_{IN} line for T_1 = 425—ie the q_{IN} line that intercepts the x axis at $T_{int\,2}$ = 425.

- The system suddenly receives a small, positive perturbation in $T_{int\,2}$.

- The positive perturbation causes q_{IN} to be larger than q_{OUT}. The mismatch between q_{IN} and q_{OUT} causes $T_{int\,2}$ to increase.

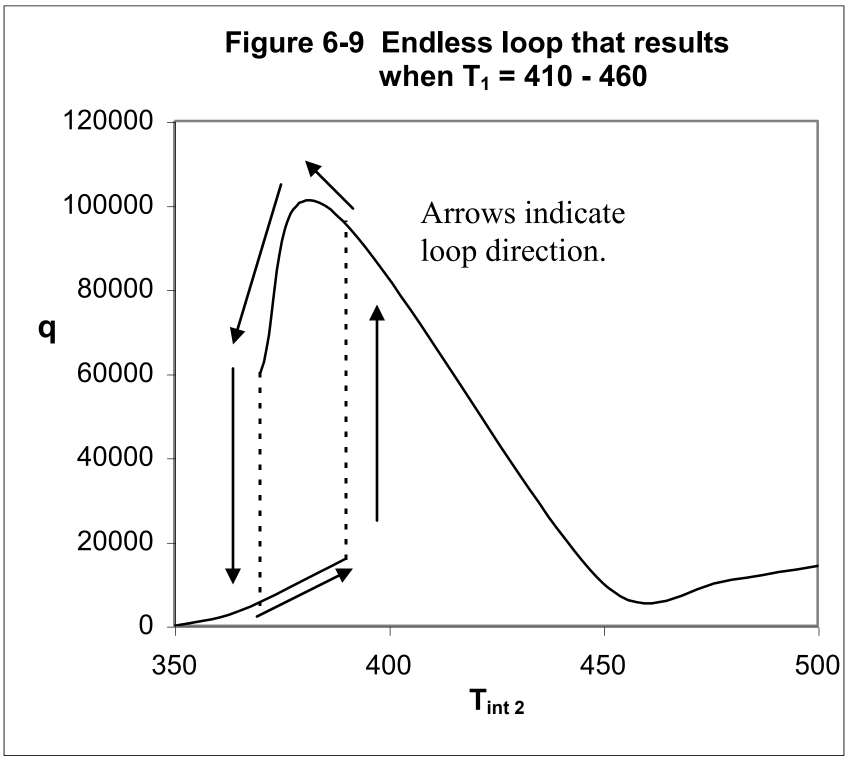
- When $T_{int\,2}$ increases to the maximum in $T_{int\,2}\{q_{OUT}\}$ at (390,16000), the mismatch between q_{IN} and q_{OUT} causes a step increase to (390,96000), since that is the only operating point at $T_{int\,2}$ incrementally greater than 390.

- At (390,96000), q_{IN} is smaller than q_{OUT}. The mismatch between q_{IN} and q_{OUT} causes $T_{int\,2}$ to decrease to the minimum in $T_{int\,2}\{q_{OUT}\}$ at (370,67000).

- At (370,67000), the mismatch between q_{IN} and q_{OUT} causes a step decrease to (370,5000), since that is the only operating point at $T_{int\ 2}$ incrementally smaller than 370.

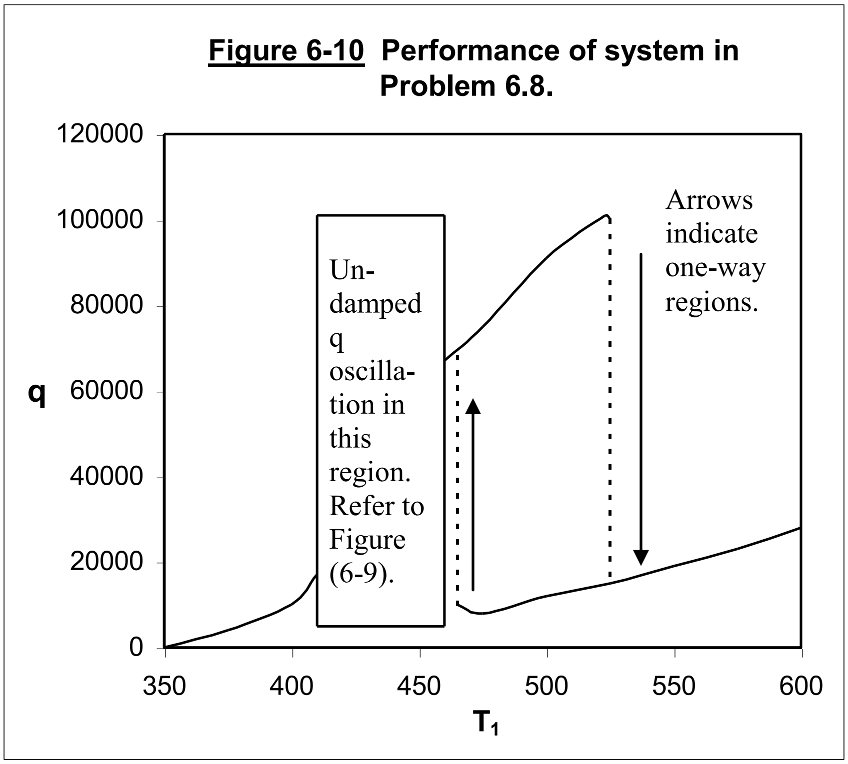
- At (370,5000), q_{IN} is larger than q_{OUT}. The mismatch between q_{IN} and q_{OUT} causes $T_{int\ 2}$ to increase to the maximum in $T_{int\ 2}\{q_{OUT}\}$ at (390,16000), and the cycle repeats.

As noted above, a single, unstable solution results when T_1 = 415 to 455. Therefore, when T_1 = 415 to 455, the system endlessly traverses the loop shown in Figure (6-9).



6.9.6 Problem (6-2) Answer

Figure (6-10) is the answer to Problem (6-2). It is a chart of $q\{T_1\}$ over the Fluid 1 temperature range of 350 to 600 F. Coordinates of $q\{T_1\}$ are obtained from the intersections in Figure (6-8) by noting that, on each straight line, $T_1 = T_{int\ 2}\{q_{IN} = 0\}$. Unstable intersections are not plotted.



6.10 Validation of the stability analysis in Problem (6-2).
The stability analysis in Problem (6-2) is validated by results reported in Marto and Rohsenow (1966). Their results were obtained from a pool boiler in which the boiling fluid was a liquid metal.

The pool boiler in Problem (6-2) contains a liquid that exhibits the "pool boiling curve" shown in Figure (6-7). Adiutori (1964) suggests that the pool boiling curve for liquid metals resembles the curve in Figure (6-7).

If the stability analysis of Problem (6-2) is correct, and if in fact the pool boiling curve for liquid metals resembles Figure (6-7), then behavior predicted in the analysis will agree with behavior reported by Marto and Rohsenow.

Note the following analytical results in Problem (6-2):

- As indicated in Figures (6-9) and (6-10), the boiler exhibits undamped, oscillatory behavior when operated near the lower end of its operating range.

- As indicated in Figure (6-9), the undamped oscillation includes periods in which boiling occurs, and periods in which boiling does not occur.

  (In Figure (6-9), there is no boiling on the bottom leg of the loop. In this region, heat transfer is by convection in the liquid, and evaporation at the free interface. Inexplicably, the widely accepted "pool boiling curve" includes a region in which boiling does not occur.)

- As indicated in Figure (6-9), when boiling occurs, the wall temperature decreases with time. When boiling does not occur, the wall temperature increases with time.

- As indicated in Figure (6-10), when boiler operation is brought into the upper region of its operating range, the undamped oscillations cease, and the boiler operates in a steady manner.

Note that the following observations by Marto and Rohsenow do in fact agree with the stability analysis of Problem (6-2).

> *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 experiment results show that, around 200,000 B/hrft^2, stable boiling occurs in most cases.*

6.11 How to eliminate undamped oscillation.

Assuming that the behavior of boundary layer 2 cannot be modified, the undamped oscillation can be eliminated by modifying the system to make the $q_{IN}\{T_{int\,2}\}$ lines sufficiently steep to avoid singular, unstable solutions. In other words, the design objective is to make the lines sufficiently steep that all unstable intersections lie on $q_{IN}\{T_{int\,2}\}$ lines that make three intersections with $q_{OUT}\{T_{int\,2}\}$.

Note in Figure (6-8) that, if the system were modified so that the slope of the $q_{IN}\{T_{int\,2}\}$ lines were $\leq$ -2000 B/hrft^{2}F, triple intersections would replace single intersections, and hysteresis would replace undamped oscillations.

The slope of the $q_{IN}\{T_{int\,2}\}$ lines is determined by the behavior of boundary layer 1 and the heat transfer wall. To attain the desired slope, boundary layer 1 and/or the heat transfer wall must be modified so that the constant in Eq. (6-25) becomes equal to or greater than 2000.

If boundary layer 1 and/or the heat transfer wall were modified as required, the system performance would be affected as follows:

- Hysteresis would occur at heat source fluid temperatures in the vicinity of 390 F. The extent of the hysteresis depends on the behavior of the modified boundary layer 1 and heat transfer wall.
- The hysteresis that occurred in the original design (over the interval T_1 = 465 to 525) would be eliminated, since the steepest slope between the maximum and minimum in $q_{OUT}\{T_{W2}\}$ is –1520 B/hrft^{2}F.

Chapter 7

Problems that demonstrate stress/strain analysis using the new engineering.

7 Summary
This chapter includes example problems that illustrate stress/strain analysis using stress/strain "behavior" methodology—ie methodology in which stress (σ) and strain (ε) are *separate* and *explicit*. The problems include proportional and nonlinear phenomena, and demonstrate that stress/strain analysis is simple and direct using behavior methodology.

The problems in this chapter are based on an idealized material. It is idealized in that operation over the entire stress/strain curve is reversible, and therefore the material is not subject to permanent strain. The material is idealized so that the analyses in this chapter will be closely analogous to electrical and heat transfer analyses in previous chapters. The impact of permanent strain is the subject of Chapter 9.

7.1 The relationship between σ and ε.
The relationship between σ and ε is determined empirically, and the data are used to prepare stress/strain charts. Figure (7-1) describes a more or less typical stress/strain chart.

Note in Figure (7-1) that, at small values of strain, σ is proportional to ε, and therefore σ/ε is *constant*. This region is the elastic region.

Also note in Figure (7-1) that, at large values of strain, σ is a highly nonlinear function of ε, and therefore σ/ε is a *variable*. This region is the inelastic region

7.2 Stress/strain moduluses
There are three types of stress/strain modulus: elastic modulus, secant modulus, and tangent modulus.

The elastic modulus is the *constant* defined by Definition (7-1a). It is defined only in the elastic region of the stress/strain curve—ie the region in which σ/ε is essentially constant.

$$E_{\text{elastic}} \equiv (\sigma/\varepsilon)_{\text{in the elastic region}} \qquad (7\text{-}1a)$$

The secant modulus is the *variable* defined by Definition (7-1b). It is defined throughout the stress/strain curve, but usually refers specifically to the inelastic region.

$$E_{secant} \equiv \sigma/\varepsilon \qquad (7\text{-}1b)$$

The tangent modulus is the *variable* defined by Definition (7-1c). It is defined throughout the stress/strain curve, but usually refers to the inelastic region.

$$E_{tangent} = d\sigma/d\varepsilon \qquad (7\text{-}1c)$$

In the elastic region, the three moduluses are equal.

Figure (7-1) is a typical stress/strain curve. Figure (7-1M) is the same typical stress/strain curve described in terms of secant modulus and strain.

7.3 Stress/strain "behavior", the function σ{ε}.

In behavior methodology, the analysis of stress/strain problems is based on the "behavior" of σ and ε—ie is based on σ{ε}. Note that

- Stress/strain data are usually obtained in the form σ{ε}.
- The form σ{ε} is also the form required for analysis in behavior methodology.
- If a problem is concerned with both the elastic and inelastic regions, Figure (7-1) is an example of the behavior form σ{ε} required for analysis.
- If a problem is concerned *only* with behavior in the elastic region, Eq. (7-2) is an example of the behavior form σ{ε}.

$$\sigma = 30 \times 10^6\ \varepsilon \qquad \text{for } \varepsilon < .002 \qquad (7\text{-}2)$$

Figure 7-1 Typical stress/strain curve

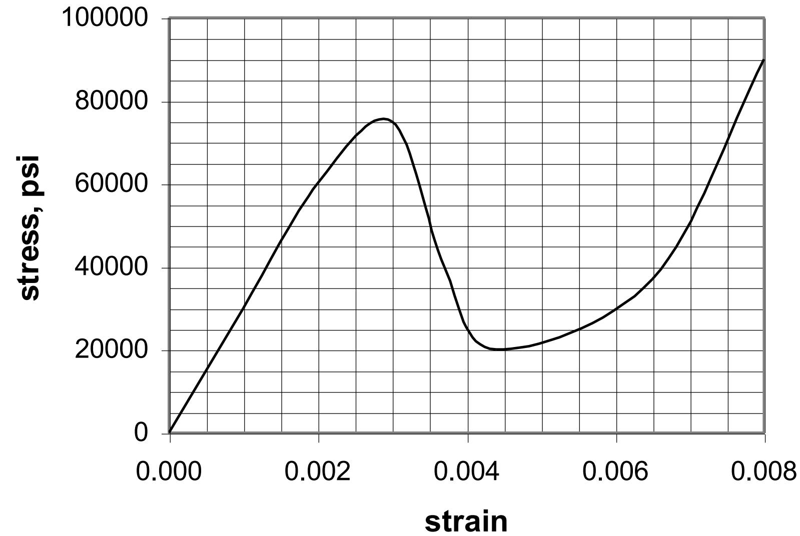
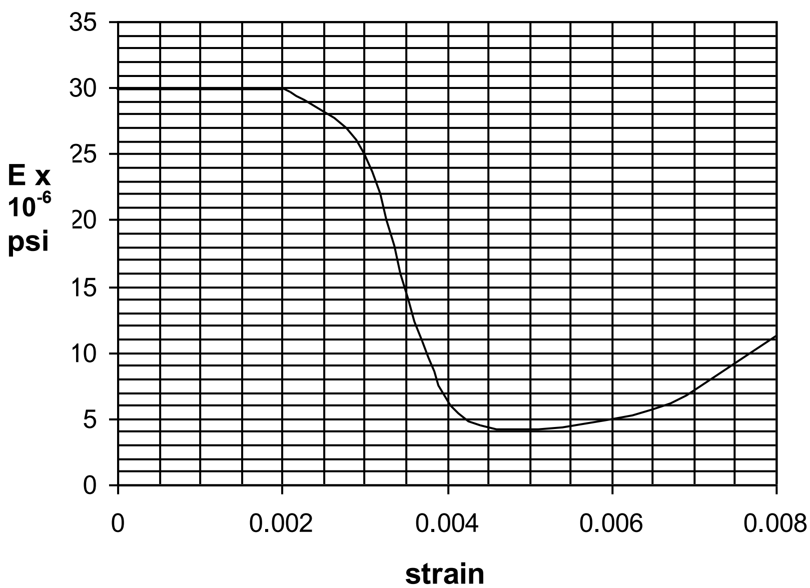


Figure 7-1M Secant modulus form of Figure 7-1



7.4. Modulus methodology.

In modulus methodology, the analysis of stress/strain problems is based on Eq. (7-2).

$$\sigma = E\,\varepsilon \tag{7-2}$$

If a problem concerns the elastic region, it can be solved in a simple and direct manner using $E_{elastic}$ because $E_{elastic}$ is a *constant*.

If a problem concerns the inelastic region, it *cannot* be solved in a simple and direct manner using E_{secant} because E_{secant} is a *variable* in the inelastic region—an unnecessary variable that greatly complicates the solution of problems that concern inelastic behavior.

7.5 Behavior methodology.

In behavior methodology, the relationship between σ and ε is described in the form $\sigma\{\varepsilon\}$. Consequently all problems are solved with σ and ε *separated*, thereby eliminating both the *constant* $E_{elastic}$ and the *variable* E_{secant}.

If a problem concerns only the elastic region, the solution is simple and direct using *either* modulus methodology or behavior methodology because $E_{elastic}$ is a *constant*. But if a problem concerns the inelastic region, the solution is much more complicated using modulus methodology because E_{secant} is a *variable*.

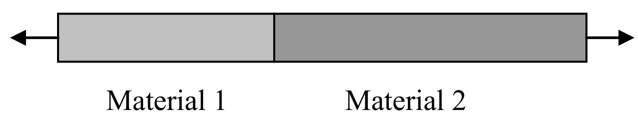
7.6 Problem (7-1)

Problem (7-1) serves two purposes:

- It demonstrates how to use behavior methodology to solve stress/strain problems that concern the elastic behavior of a bar. More specifically, it demonstrates how to solve elastic problems with σ and ε *separated* rather than combined in σ/ε.

- It demonstrates that the solution of proportional problems based on behavior methodology is as simple and direct as solution based on modulus methodology.

7.6.1 Problem (7-1) Statement
What axial load would increase the length of the bar below by .005 feet? What stress and strain would result in each material?



7.6.2 Problem (7-1) Given

- Material 1 is 2 feet long, and its behavior is described by Eq. (7-3). σ is in psi.

$$\sigma_1 = 25 \times 10^6 \varepsilon_1 \quad \text{for } \varepsilon_1 < .002 \tag{7-3}$$

- Material 2 is 3 feet long, and its behavior is described by Eq. (7-4).

$$\sigma_2 = 40 \times 10^6 \varepsilon_2 \quad \text{for } \varepsilon_2 < .002 \tag{7-4}$$

- The cross-section of the bar is everywhere 4 in^2.

7.6.3 Problem (7-1) Analysis

- From inspection of the given figure and information,

$$\sigma_1 = \sigma_2 = \sigma \tag{7-5}$$

$$2\varepsilon_1 + 3\varepsilon_2 = .005 \tag{7-6}$$

- Substituting the given information in Eq. (7-6), and using Eq. (7-5), gives Eq. (7-7).

$$2\sigma/(25\text{x}10^6) + 3\sigma/(40\text{x}10^6) = .005 \tag{7-7}$$

- Solution of Eq. (7-7) gives $\sigma = 32{,}300$.

- Since the cross-sectional area of the bar is 4 in^2, the 32,300 psi stress indicates that a load of 129,200 lbs would increase the length of the bar by .005 ft.

- Eq. (7-3) indicates that a stress of 32,300 psi causes a strain of 0.0013 in Material 1.

- Eq. (7-4) indicates that a stress of 32,300 psi causes a strain of 0.00081 in Material 2.

7.6.4 Problem (7-1) Answer

- A load of 129,200 lbs would increase the length of the bar by .005 ft.

- The load would result in a stress of 32,300 psi in both materials.

- The strain in Material 1 would be .0013. The strain in Material 2 would be .00081.

7.6.5 Problem (7-1) Discussion

- Problem (7-1) is stated and solved using behavior methodology—ie with σ and ε *separate* and *explicit*.

- The problem is stated and solved *without* using σ/ε, demonstrating that σ/ε is unnecessary—ie demonstrating that E is unnecessary.

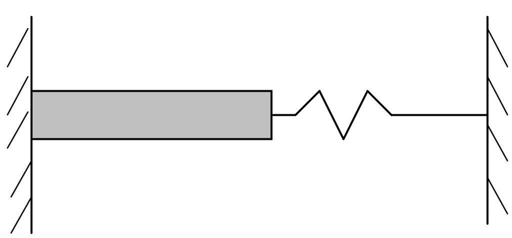
- The problem solution using behavior methodology is as simple as solution using modulus methodology.

7.7.1 Problem (7-2)

Problem (7-2) demonstrates the application of behavior methodology to elastic problems that concern a spring and a bar. (Problem (7-4) is the same as this problem except that the bar is in the inelastic region.)

7.7.2 Problem (7-2) Statement

What are the stress and strain values in the bar shown in the following sketch?



7.7.3 Problem (7-2) Given:

- No load length of bar = 4.950 ft.
- Bar cross-section = 4 in^2
- No load length of spring = 2.000 ft.
- Distance between walls = 7.000 ft
- The behavior of the spring is described by Eq. (7-8). (P is load in pounds, L is length in feet.)

$$P_{spring} = 6 \times 10^6 \, \Delta L_{spring} \qquad (7\text{-}8)$$

- The behavior of the bar material is described by Eq. (7-9). σ is in psi. The bar has always operated in the elastic region.

$$\sigma_{bar} = 30 \times 10^6 \varepsilon_{bar} \qquad \text{for } \varepsilon_{bar} < .0025 \qquad (7\text{-}9)$$

7.7.4 Problem (7-2) Analysis

- Note that $P_{bar} = P_{spring}$.
- Obtain expressions for $(P_{bar})\{\varepsilon_{bar}\}$ and $(P_{spring})\{\varepsilon_{bar}\}$.
- Equate $(P_{bar})\{\varepsilon_{bar}\}$ and $(P_{spring})\{\varepsilon_{bar}\}$, and solve for $\varepsilon_{bar.}$

The expression for $(P_{bar})\{\varepsilon_{bar}\}$ is obtained by noting that

$$(P_{bar}) = \sigma_{bar} A_{bar} \qquad (7\text{-}10)$$

Substituting given information in Eq. (7-10) gives

$$(P_{bar})\{\varepsilon_{bar}\} = 120 \times 10^6 \varepsilon_{bar} \qquad \text{for } \varepsilon_{bar} < .0025 \qquad (7\text{-}11)$$

The expression for $(P_{spring})\{\varepsilon_{bar}\}$ is obtained by calculating the coordinates of two points on $(P_{spring})\{\varepsilon_{bar}\}$, and noting that the spring load is a linear function of ε_{bar}.

The coordinates of one point are calculated by noting that

$$\Delta L_{spring}\{\varepsilon_{bar} = 0\} = .05 \qquad (7\text{-}12)$$

$\therefore\ P_{spring}\{\varepsilon_{bar} = 0\} = .05 \times 6 \times 10^6 = 300{,}000$ (7-13)

The coordinates of a second point are calculated by noting that

$\Delta L_{spring}\{\varepsilon_{bar} = .01\} = 0$ (7-14)

$\therefore\ P_{spring}\{\varepsilon_{bar} = .01\} = 0$ (7-15)

Since $(P_{spring})\{\varepsilon_{bar}\}$ is a linear function, Eqs. (7-11) and (7-15) indicate that the function is described by Eq. (7-16).

$(P_{spring})\{\varepsilon_{bar}\} = 300{,}000 - 30 \times 10^6\ \varepsilon_{bar}$ (7-16)

Equating $(P_{bar})\{\varepsilon_{bar}\}$ and $(P_{spring})\{\varepsilon_{bar}\}$ from Eqs. (7-11) and (7-16) gives

$120 \times 10^6 \varepsilon_{bar} = 300{,}000 - 30 \times 10^6\ \varepsilon_{bar}$ (7-17)

Solution of Eq. (7-17) indicates that $\varepsilon_{bar} = .0020$. Therefore Eq. (7-9) applies, and indicates that $\sigma_{bar} = 60{,}000$.

7.7.5 Problem (7-2) Answer
The tensile stress in the bar is 60,000 psi. The strain in the bar is .0020.

7.7.6 Problem (7-2) Discussion

- The problem is stated and solved using behavior methodology—ie with σ and ε separate and explicit.

- The problem is stated and solved *without E*, demonstrating that *E* is unnecessary.

- The problem solution based on behavior methodology is as simple as solution based on elastic modulus methodology.

7.8 Problem (7-3a)
Problem (7-3a) concerns the inelastic region of a bar. The problem is presented and solved using behavior methodology.

7.8.1 Problem (7-3a) Statement
Given the behavior described in Figure (7-1), use behavior methodology to determine the strain that would result from a stress of 40,000 psi.

7.8.2 Problem (7-3a) Analysis and answer
The analysis of Problem (7-3a) using behavior methodology involves inspection of Figure (7-1). This requires approximately ten seconds, and involves little likelihood of error.

Inspection of Figure (7-1) indicates that a stress of 40,000 psi would result in a strain of .0013, .0037, or .0066 in the bar. The given information is not sufficient to determine a unique answer.

7.9 Problem (7-3b)
Repeat Problem (7-3a) using modulus methodology.

7.9.1 Problem (7-3b) Analysis
The analysis of Problem (7-3b) using modulus methodology involves inspection of Figure (7-1M), and is to be performed by the reader.

7.9.2 Problem (7-3b) Answer
The correct and complete answer is that a stress of 40,000 psi would result in a strain of .0013, .0037, or .0066 in the bar. The given information is not sufficient to determine a unique answer.

7.10 Discussion of Problems (7-3a) and (7-3b).
The solution of Problem (7-3a) is simple and direct because Figure (7-1) can be read directly. The reason it can be read directly is because the y axis is dependent on σ and *not* on ε, and the x axis is dependent on ε and *not* on σ.

The solution of Problem (7-3b) is *not* simple and direct because Figure (7-1M) *cannot* be read directly. The reason it cannot be read directly is because both axes are dependent on ε. Therefore if the stress is given and the strain is unknown, the chart cannot be read directly because both the x coordinate and the y coordinate are dependent on the *unknown* value of ε.

In order to solve Problem (7-3b), Figure (7-1M) must be read indirectly by trial-and-error or iteration. Both methods require much more time, and have a much greater likelihood of error, than direct reading.

If the reader solved Problem (7-3b) using modulus methodology, he undoubtedly found that it takes *much* longer to solve Problem (7-3b) than to solve Problem(7-3a), and it involves a *much* greater likelihood that the calculated answer will *not* be correct and complete—ie will not agree that the strain could be .0013, .0037, or .0066.

Even though Problem (7-3) may seem trivial, it accurately reflects the large difference in complexity between solving inelastic problems with σ and ε kept *separate* (as in behavior methodology), or solving them with σ and ε *combined* in modulus σ/ε (as in modulus methodology).

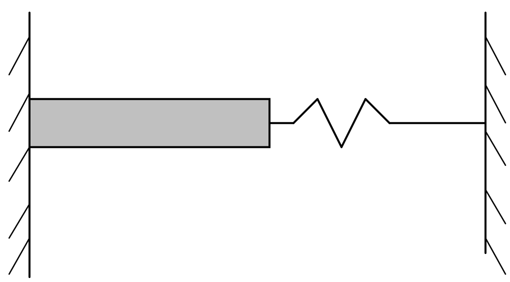
7.11 Problem (7-4a)

Problem (7-4a) is the same as Problem (7-2) except that the bar is in the inelastic region. The problem is presented and solved using behavior methodology.

Note that inelastic Problem (7-4a) is analyzed in the same simple and direct manner as elastic Problem (7-2). The sole difference is that the analysis of Problem (7-2) is digital, whereas the analysis of Problem (7-4a) is necessarily graphical because the behavior of the nonlinear component is described graphically.

7.11.1 Problem (7-4a) Statement

Use behavior methodology to determine the steady-state stress and strain values in the bar below.



7.11.2 Problem (7-4a) Given

- No load length of bar = 4.950 ft.

- Bar cross-section = 4 in^2

- No load length of spring = 2.000 ft.
- Distance between walls = 7.000 ft
- The behavior of the spring is described by Eq. (7-8).
- The behavior of the bar material is described by Figure (7-1).
- The dimension units of stress are pounds and inches.

7.11.3 Problem (7-4a) Analysis

- Note that $P_{bar} = P_{spring}$.
- Obtain expressions for $(P_{bar}/A_{bar})\{\varepsilon_{bar}\}$ and $(P_{spring}/A_{bar})\{\varepsilon_{bar}\}$.
- Equate $(P_{bar}/A_{bar})\{\varepsilon_{bar}\}$ and $(P_{spring}/A_{bar})\{\varepsilon_{bar}\}$, and solve for $\varepsilon_{bar.}$

$(P_{bar}/A_{bar})\{\varepsilon_{bar}\}$ is described graphically in Figure (7-1). The expression for $(P_{spring}/A_{bar})\{\varepsilon_{bar}\}$ is obtained from Eq. (7-16) by dividing all terms by A_{bar}.

$$(P_{bar}/A_{bar})\{\varepsilon_{bar}\} = 75{,}000 - 7.5 \times 10^6\ \varepsilon_{bar} \qquad (7\text{-}18)$$

Graphically equate $(P_{bar}/A_{bar})\{\varepsilon_{bar}\}$ and $(P_{spring}/A_{bar})\{\varepsilon_{bar}\}$ by plotting them together in Figure (7-2), and noting that intersections are solutions.

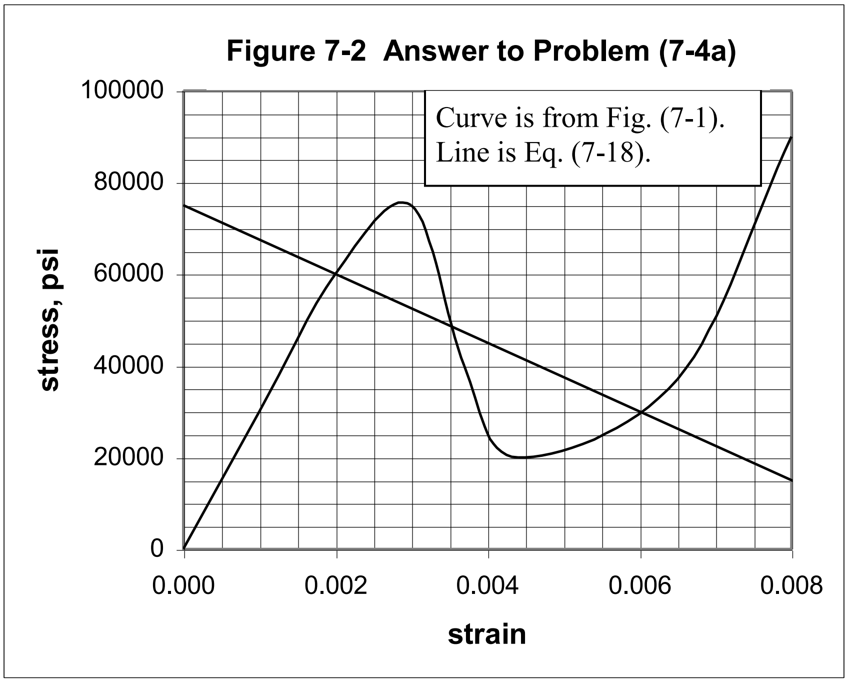
7.11.4 Problem (7-4a) Answer

Figure (7-2) indicates that there are three possible solutions to the problem:

Stress, psi	**Strain**
60,000	.0020
49,000	.0035
30,000	.0060

Inspection of Figure (7-2) indicates that the solution at 49,000 psi is unstable. (See Chapter 9.) Because of the instability, the system will not operate at 49,000 psi, but will shift to the solution at 30,000 psi.

Therefore the steady-state stress and strain values are 60,000 psi at .0020 strain, and 30,000 psi at .0060 strain. The problem statement does not contain sufficient information to determine a unique solution.



7.12 Problem (7-4b)
Repeat Problem (7-4a) using modulus methodology instead of behavior methodology.

7.12.1 Problem (7-4b) Given

- No load length of bar = 4.950 ft.
- Bar cross-section = 4 in^2
- No load length of spring = 2.000 ft.
- Distance between walls = 7.000 ft
- The behavior of the spring is described by Eq. (7-8).
- The behavior of the bar material is described by Figure (7-1M).

7.12.2 Problem (7-4b) Analysis
The reader is encouraged to attempt to solve Problem (7-4b). He will likely find that the analysis using modulus methodology is so difficult he will abandon the attempt long before he obtains the correct answer.

7.13 Discussion of Problems (7-4a) and (7-4b).

- Problem (7-4a) is stated and solved *without E*, demonstrating that *E* is unnecessary.

- The solution of Problem (7-4a) using behavior methodology is simple and direct, even though the problem concerns highly nonlinear behavior, and several components. The solution of Problem (7-4b) using modulus methodology is neither simple nor direct.

- The behavior analysis of Problem (7-4a) (an inelastic problem) is exactly the same as the behavior analysis of Problem (7-2) (an elastic problem). This illustrates that, if behavior methodology is used, elastic problems and inelastic problems are analyzed in the same direct way. On the other hand, if modulus methodology is used, elastic problems can be solved in a direct manner, but inelastic problems must generally be solved in an indirect manner.

7.14 Closing remarks.

The problems in this chapter:

- Illustrate how to use behavior methodology to solve stress/strain problems that involve elastic and/or inelastic behavior. In other words, illustrate how to solve problems with σ and ε *separate* and *explicit—ie without E—ie without* σ/ε.

- Demonstrate that problems that involve elastic behavior and/or inelastic behavior are solved simply and directly using behavior methodology.

- Demonstrate that it is not necessary to solve elastic problems using elastic modulus σ/ε, and inelastic problems using secant modulus σ/ε.

If the reader will attempt to solve Problems (7-3b) and (7-4b) using modulus methodology, he will gain a true appreciation of the simplicity that results from abandoning modulus methodology, and replacing it with behavior methodology.

Chapter 8

Why modulus methodology should be replaced by material behavior methodology.

8 Summary

This chapter explains why modulus methodology should be replaced by behavior methodology—ie why methodology based on σ/ε should be replaced by methodology based on $\sigma\{\varepsilon\}$.

The explanation is given in two ways:

- In a general way by critically examining the nature and application of "modulus".

- In a specific way by comparing the behavior analyses in Chapter 7 with the modulus analyses of the same problems.

The answers strongly support the conclusion that stress/strain "behavior methodology" should replace "modulus methodology".

8.1 The ratio named "modulus".

In conventional engineering, the ratio σ/ε is named "modulus", and is assigned the symbol E. This ratio is used in stress/strain analyses to describe the relationship between σ and ε.

Every modulus value is the result of a test to measure the relationship between stress and strain—ie to measure the function $\sigma\{\varepsilon\}$. Stress/strain data are reduced to several different moduluses. For example,

- $E_{elastic}$ is usually taken to be the measured value of stress divided by the measured value of strain at a strain of .002. It is always a constant.

- $E_{plastic}$ (also known as E_{secant}) is the measured value of stress divided by the measured value of strain at *any* value of strain. It is a constant in the elastic region, and a *variable* in the inelastic region. However, it generally refers to the inelastic region.

- $E_{tangent}$ is the slope of the stress/strain curve. It is a constant in the elastic region, and a variable in the inelastic region.

8.2 The rationale of reducing stress/strain data to modulus.

In modulus methodology:

- The relationship between stress and strain is determined empirically—ie $\sigma\{\varepsilon\}$ is determined empirically.
- Modulus values are determined by transforming $\sigma\{\varepsilon\}$ data to the form $\sigma/\varepsilon\{\varepsilon\}$—ie to the form $E\{\varepsilon\}$.
- $E\{\varepsilon\}$ is used in stress analyses to describe $\sigma\{\varepsilon\}$.

At this point, it is appropriate to ask:

- What useful purpose is served by transforming $\sigma\{\varepsilon\}$ data to the form $(\sigma/\varepsilon)\{\varepsilon\}$—ie the form $E\{\varepsilon\}$?
- Isn't the only purpose of $E\{\varepsilon\}$ to describe $\sigma\{\varepsilon\}$?
- Isn't $\sigma\{\varepsilon\}$ the best way to describe $\sigma\{\varepsilon\}$?
- Since $\sigma\{\varepsilon\}$ is the best way to describe $\sigma\{\varepsilon\}$, wouldn't it be better to use $\sigma\{\varepsilon\}$ in stress/strain analyses, and to abandon $E\{\varepsilon\}$?

The appropriate responses are:

- No useful purpose is served by transforming $\sigma\{\varepsilon\}$ data to the form $(\sigma/\varepsilon)\{\varepsilon\}$—ie the form $E\{\varepsilon\}$.
- $(\sigma/\varepsilon)\{\varepsilon\}$ is an undesirable way to describe $\sigma\{\varepsilon\}$. Using $(\sigma/\varepsilon)\{\varepsilon\}$ to describe $\sigma\{\varepsilon\}$ is like using $(y/x)\{x\}$ to describe $y\{x\}$.
- It is a truism to say that $\sigma\{\varepsilon\}$ is the best way to describe $\sigma\{\varepsilon\}$.
- Yes!

8.3 Stress/strain "behavior".

Stress/strain "behavior" is determined empirically by performing stress/strain tests over the range zero strain to fracture, and then plotting the data in the form $\sigma\{\varepsilon\}$. Empirically determined stress/strain behavior is fundamental in behavior methodology, *and* in modulus methodology.

8.4 Mathematical analogs.

The table below identifies mathematical analogs of stress/strain parameters.

Stress/strain	Mathematical analog
ε	x
σ	y
E	(y/x)
$\sigma\{\varepsilon\}$	y{x}
$E\{\varepsilon\}$	(y/x){x}

Note the following in the table:

- The mathematical analog of E is (y/x). In mathematics, every effort is made to *separate x* and *y*—ie to *eliminate* terms such as (y/x) that *combine* x and y. Yet an analog of (y/x) is the basis for analysis in modulus methodology.

- The mathematical analog of an $E\{\varepsilon\}$ chart such as Figure (7-1M) is a chart in the form (y/x) vs x. This form is not used in mathematics because it largely conceals the relationship between x and y, the relationship (y/x) vs x is intended to reveal.

  If a chart is in the form (y/x) vs x, and x is to be determined from a given value of *y*, an iterative or trial-and-error procedure is required *simply to read the graph* in nonlinear regions, as in Problem (7-4b).

- The mathematical analog of stress/strain behavior $\sigma\{\varepsilon\}$ is $y\{x\}$, the form of choice in mathematics.

In summary, the mathematical analogs in the table above indicate that stress/strain behavior methodology is mathematically desirable, and that modulus methodology is mathematically *un*desirable.

8.5 How modulus methodology complicates the solution of problems that concern inelastic behavior.

Stress/strain modulus is mathematically undesirable because it combines σ and ε in the ratio σ/ε, symbol E. Note the following:

- Elastic problems may be solved in a direct manner using modulus methodology because σ/ε is a *constant* in the elastic region. Inelastic problems must generally be solved in an *indirect* manner if modulus methodology is used because modulus is a *variable* in the inelastic region—an unnecessary *variable* that transforms problems with *two* variables (σ and ε) into problems with *three* variables (σ, ε, and σ/ε), thereby greatly complicating the solution of inelastic problems.

- If σ and ε are separated as in behavior methodology, both elastic *and inelastic* problems can be solved in a direct manner because they both concern only *two* variables.

- Indirect solutions are more difficult, more time-consuming, and much more likely to contain errors than direct solutions.

8.6 The significance of the Problems in Chapter 7

- Problems (7-1) and (7-2) demonstrate that elastic stress/strain problems are readily solved using either modulus methodology or behavior methodology.

- Problems (7-3) and (7-4) demonstrate that stress/strain problems that must be solved in an indirect manner using modulus methodology can be solved in a direct and much simpler manner using behavior methodology. Note that modulus methodology:

    - Requires an iterative procedure simply to read the chart in Figure (7-1M).

    - Requires at least 100 times longer to solve Problem (7-3) or Problem (7-4).

    - Increases the likelihood of error by a factor of 100 or more.

- Note that if Problem (7-3) concerned the determination of stress at a given strain rather than the determination of strain at a given stress, the solution could be determined in a direct manner using modulus methodology.

However, if Problem (7-3b) concerned a *system* of several components including the component in Problem (7-3b), modulus methodology would require an indirect solution whether the problem concerned the determination of stress given strain, or the determination of strain given stress.

- Problem (7-4) is so difficult to solve using modulus methodology that it is unlikely any reader will solve it correctly and completely without using a computer.
- Even if a reader uses a computer, there is a considerable likelihood of error because the reader may not program the computer to find all possible solutions.
- Even if the reader finds all possible solutions, there is a considerable likelihood of error because it may not be readily apparent that one of the solutions is unstable, and is not a steady-state solution. (See Chapter 9.)

8.7 Conclusions

In order that both elastic and inelastic problems may be solved using the same methodology, and in order to greatly simplify the solution of inelastic problems:

- The modulus concept (σ/ε) should be abandoned, and should be replaced by the behavior concept ($\sigma\{\varepsilon\}$).
- Both elastic problems and inelastic problems should be solved using behavior methodology.

Chapter 9

Irreversible stress/strain behavior.

9 Summary

The stress/strain behavior of metals is generally reversible in the elastic region, and irreversible in the inelastic region. In other words, in the inelastic region, stress is not a unique function of strain—stress also depends on work history. Therefore the relationship between stress and strain in the inelastic region is not described by a line on a stress/strain chart, but rather is described by a two-dimensional zone.

In Chapter 7, the subject material was idealized in that it exhibited reversible stress/strain behavior throughout the elastic and inelastic regions. The material was idealized in this way so that the analyses would be closely analogous to analyses of electrical and heat transfer problems in earlier chapters.

In this chapter, the subject material exhibits irreversible stress/strain behavior in the inelastic region. This chapter demonstrates that irreversible stress/strain behavior is dealt with simply and effectively if σ and ε are kept separate—ie if stress/strain behavior methodology is used.

9.1 Reversible and irreversible stress/strain behavior.

If the stress/strain behavior of a material is reversible, the following apply:

- Stress is uniquely determined by strain.

- The material is not subject to permanent strain.

- The relationship between stress and strain is one-dimensional—ie is described by a single line on a stress vs strain chart.

If the stress/strain behavior of a material includes an irreversible region, the following apply in that region:

- Stress is *not* uniquely determined by strain. Stress also depends on work history.

- The material is subject to permanent strain.

- The relationship between stress and strain is two-dimensional—ie is described by an area on a stress vs strain chart.

9.2 A definition of "stress/strain diagram",

The following is a reasonable definition of "stress/strain diagram":

> *The stress/strain diagram is the locus of points that describe the relationship between stress and strain.*

Figure (9-1) is a stress/strain diagram of a more or less typical material in its virgin state—ie with no initial permanent strain. The solid curve in Figure (9-1) would result if the strain were monotonically increased from zero strain to fracture.

Each dashed line indicates how stress would vary if the strain was monotonically increased to the upper limit of the dashed line, and the load was then decreased to zero. Since the dashed lines do not coincide with the solid line, the stress/strain behavior is irreversible, and the result is permanent strain.

The myriad of dashed lines in Figure (9-1) indicate that, since stress/strain behavior is irreversible except in the elastic region, the stress/strain diagram is essentially the area under the curve.

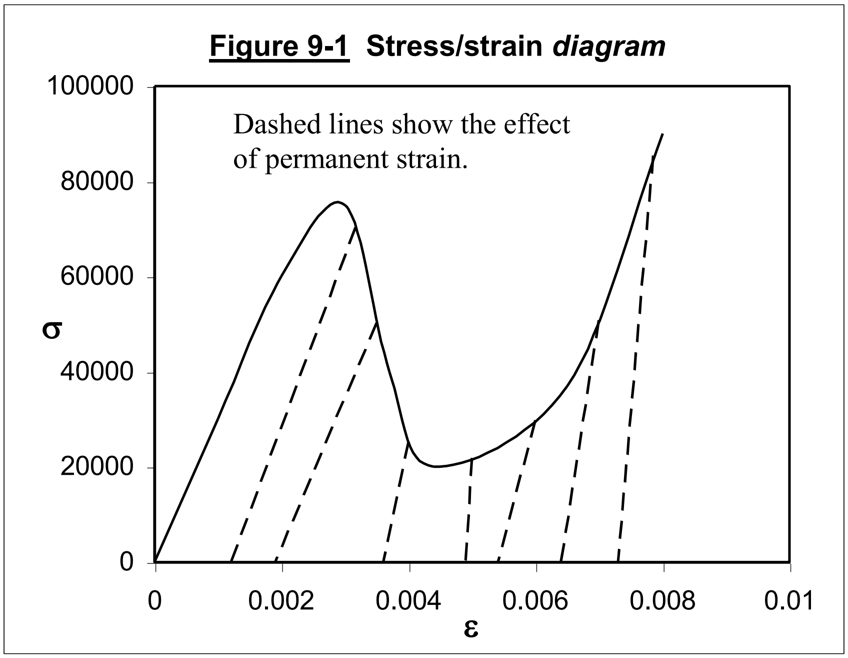


Figure 9-1 Stress/strain diagram
Dashed lines show the effect
of permanent strain.
100000
80000
60000
40000
20000
0
σ
0
0.002
0.004
0.006
0.008
0.01
ε

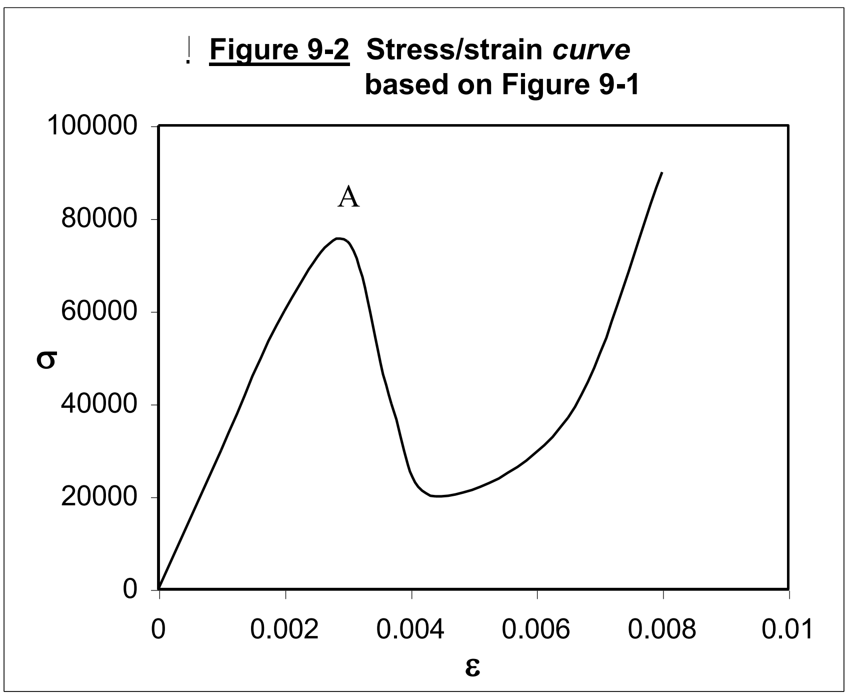


Figure 9-2 Stress/strain curve
based on Figure 9-1
A
100000
80000
60000
40000
20000
0
σ
0
0.002
0.004
0.006
0.008
0.01
ε

9.3 A definition of "stress/strain curve".

The following is a reasonable definition of "stress/strain curve":

> *The stress/strain curve is the upper boundary of the stress/strain diagram.*

Figure (9-2) is the stress/strain curve for the material described in Figure (9-1).

9.4 Measuring the stress/strain curve.

Stress/strain data can be obtained using apparatus in which load is the controlled variable, or strain is the controlled variable. However, if the stress/strain curve contains a maximum, the curve can be measured in its entirety only if strain is the controlled variable, and the strain is monotonically increased from zero to fracture.

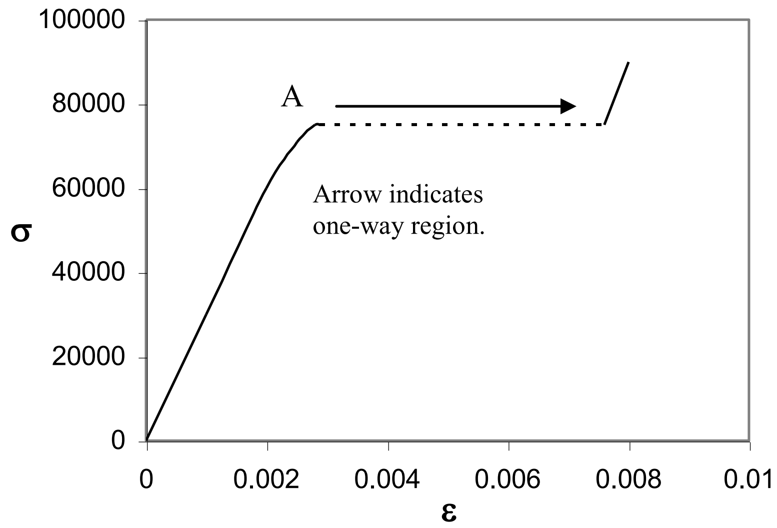
For example, note in Figure (9-3) that, if the *load* is slowly and monotonically increased from zero to fracture, a step increase in strain results when the stress passes through the maximum at Point A—ie there is a step from (.0029, 75000) to (.0076, 75000). Thus the test data will give the result in Figure (9-3) rather than the desired result in Figure (9-2).

If the load is reversed just before the fracture point is reached, and if the load is then monotonically reduced to zero, the test data will give the result shown in Figure (9-4).

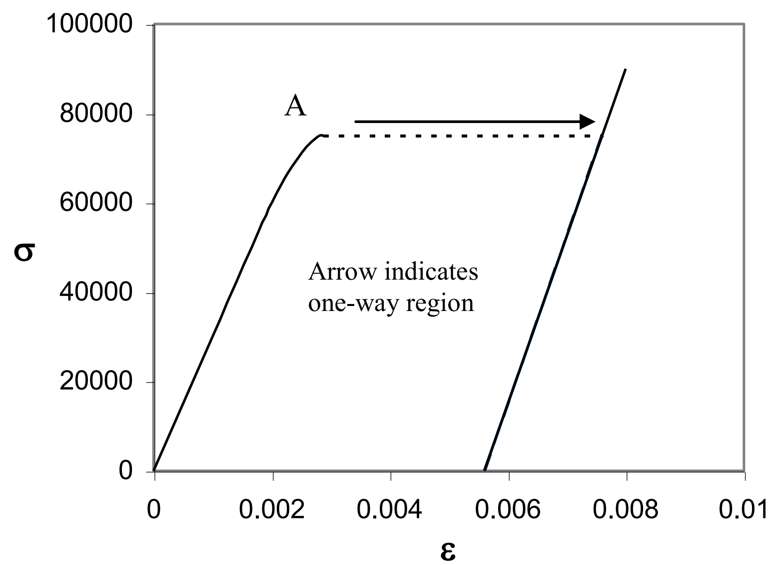
In summary:

- If a strain-controlled apparatus is used, the stress/strain curve can be measured in its entirety, even if $\sigma\{\varepsilon\}$ contains a maximum.
- If a load-controlled apparatus is used, the stress/strain curve can be measured in its entirety *only* if $\sigma\{\varepsilon\}$ does not contain a maximum.

<u>Figure 9-3</u> Load increased monotonically, stress/strain curve from Figure 9-2.



<u>Figure 9-4</u> Load in Figure 9-3 decreased monotonically from σ = 75000 to 0.



9.5 Summary of stress/strain diagram and stress/strain curve.

- The stress/strain diagram is the locus of points that describe the relationship between σ and ε.

- The stress/strain curve is the upper boundary of the stress/strain diagram.

- In elastic regions:
  - Operation is reversible.
  - Permanent strain does not result.
  - The stress/strain diagram is one-dimensional.

- In inelastic regions:
  - Operation is irreversible.
  - Permanent strain results.
  - The stress strain diagram is two-dimensional.

- In order to uniquely determine the stress and strain in a material that is subject to permanent strain, it is necessary to know the work history of the material.

9.6 Impact of permanent strain on Problems (7-1) and (7-2).
Permanent strain has no impact on the solution of Problems (7-1) and (7-2). Those problems concern elastic behavior, and permanent strain is an inelastic phenomenon.

9.7 Impact of permanent strain on Problem (7-3).
In order to revise Problem (7-3) so that it concerns a material that is subject to permanent strain, revise the problem in the following ways:

- State that the stress/strain diagram of the material is described by Figure (9-1) rather than Figure (7-1).

- State that the work history of the material is unknown.

Because the work history of the material is unknown, the amount of permanent strain is unknown. Therefore the solution of the problem must allow for the potential effect of permanent strain. The net result is that a specific value of stress can result in a wide range of strain values.

The solution is obtained by inspecting Figure (9-1) to determine the intersections between a line drawn at stress = 40,000 psi and the myriad of dashed lines that describe behavior in a permanently strained condition. The complete solution of the revised problem is:

> The strain may be any value between .0013 and .0037, and any value between .0066 and .0075.

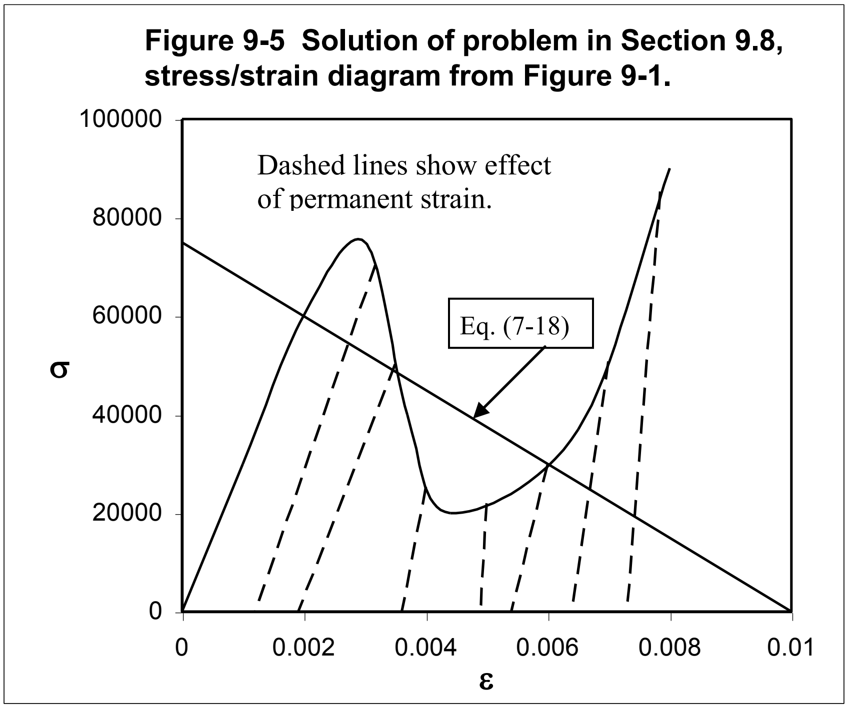
In order to uniquely determine the strain that would result from a stress of 40,000 psi, it would be necessary to know the work history.

9.8 Impact of permanent strain on Problem (7-4)—work history is unknown.

In order to revise Problem (7-4) so that it concerns a material that is subject to permanent strain and has unknown work history, revise the problem in the following ways:

- State that the stress/strain diagram of the material is described by Figure (9-1) rather than Figure (7-1).
- State that the work history of the material is unknown.
- Generate Figure (9-5) by plotting Eq. (7-18) on Figure (9-1).

The solution is obtained by inspecting Figure (9-5) to determine the intersections that occur between Eq. (7-18) and the myriad of dashed lines that describe behavior in the permanently strained condition.



The complete solution of the revised problem is:

> The stress may be any value between 49,000 and 60,000 psi, *and* any value between 20,000 and 30,000 psi. The strain may be any value between.002 and .0035, *and* between .006 and .0075.

9.9 Impact of permanent strain on Problem (7-4)—work history is known.

In order to revise Problem (7-4) so that it concerns a material that is subject to permanent strain and has a known work history, revise the problem in the following ways:

- Revise the given information to state that the stress/strain diagram of the material is described by Figure (9-1) rather than Figure (7-1).
- Revise the given information to state that the work history of the material indicates a lifetime maximum strain of .0032.
- Redraw Figure (9-1) to reflect the work history of the material:

- Delete the stress/strain curve at strain values less than .0032.
- Change the dashed line that intersects the stress/strain curve at $\varepsilon = .0032$ to a solid line.
- Delete the other dashed lines, and note that the result is a stress/strain curve. (The stress/strain *curve* is appropriate because there is no ambiguity about the condition of the material, since the work history is known.).

- Plot Eq. (7-18) on Figure (9-6).

The solution is obtained by inspecting Figure (9-6). The complete solution of the revised problem is:

The stress is 55,000 psi and the strain is .0027.

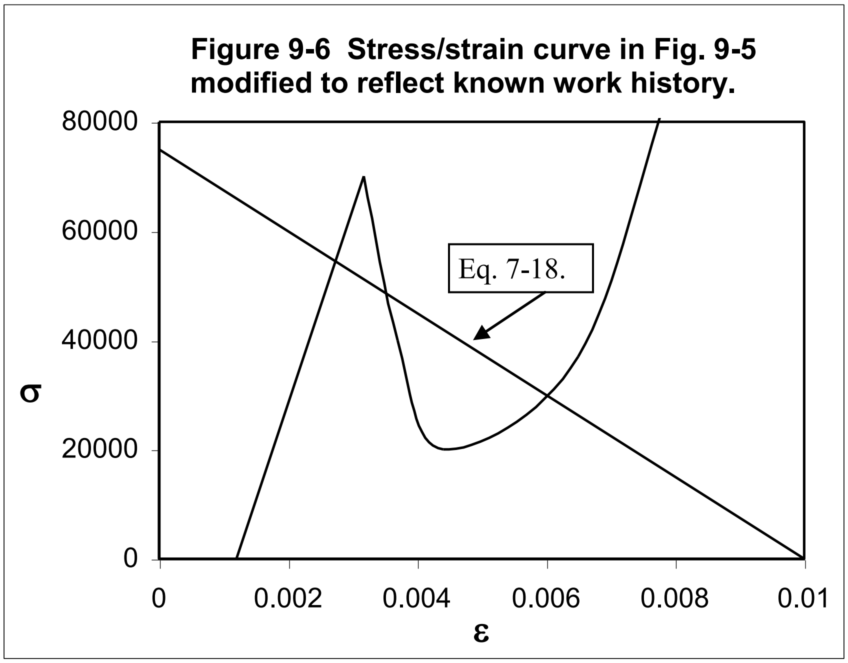
Note that the only solution is in the elastic region. The other two intersections in Figure (9-6) are not solutions because they are at strain values larger than .0032, and the work history indicates that the strain never exceeded .0032.

9.10 Stability at potential stress/strain operating points.

Stability at potential stress/strain operating points is dealt with in the following manner:

- If a stress/strain analysis indicates a triple-valued solution, the middle intersection is unstable and is ignored. If the system somehow arrives at the middle intersection, it simply refuses to remain there, and operation shifts to the solution at greater strain.

- Undamped oscillations do *not* result from unstable operating points because stress/strain curves do not exhibit the type of behavior required to generate undamped oscillations.



9.11 Conclusions

Irreversible stress/strain behavior is dealt with simply and effectively if σ and ε are kept separate—ie if stress/strain behavior methodology is used.

Chapter 10

A critical examination of fluid friction factor.

10 Summary

This chapter critically examines the group parameter known as "fluid friction factor". The examination reveals that "fluid friction factor" combines the primary parameters flow rate W and pressure drop ΔP, thereby making it necessary to solve fluid flow problems with the primary variables *combined*.

Since it is generally easier to solve problems if the variables are kept separated, it is concluded that "fluid friction factor" should be abandoned, and should be replaced by behavior methodology—ie by methodology in which W and ΔP are kept *separated*.

10.1 Problems to be solved by readers who are familiar with Moody charts.

The reader is requested to solve Problems (10-1) and (10-2) in order to better appreciate why the fluid friction factor should be abandoned, and the Moody chart revised.

10.1.1 Problem (10-1)

Moody charts are in the form friction factor versus Reynolds number with relative roughness as a parameter. Describe how to read a Moody chart to find the friction factor if the given information is pressure drop, fluid properties, relative roughness, and flow path geometry.

10.1.2 Problem (10-2)

On Figure (10-1), sketch $\Delta P\{W\}$ for steady, incompressible flow in a smooth, constant area duct. (Notice that Figure (10-1) is a *linear* chart.) In other words, *qualitatively* describe how ΔP increases as W is increased from zero. A Moody chart may be used for reference.

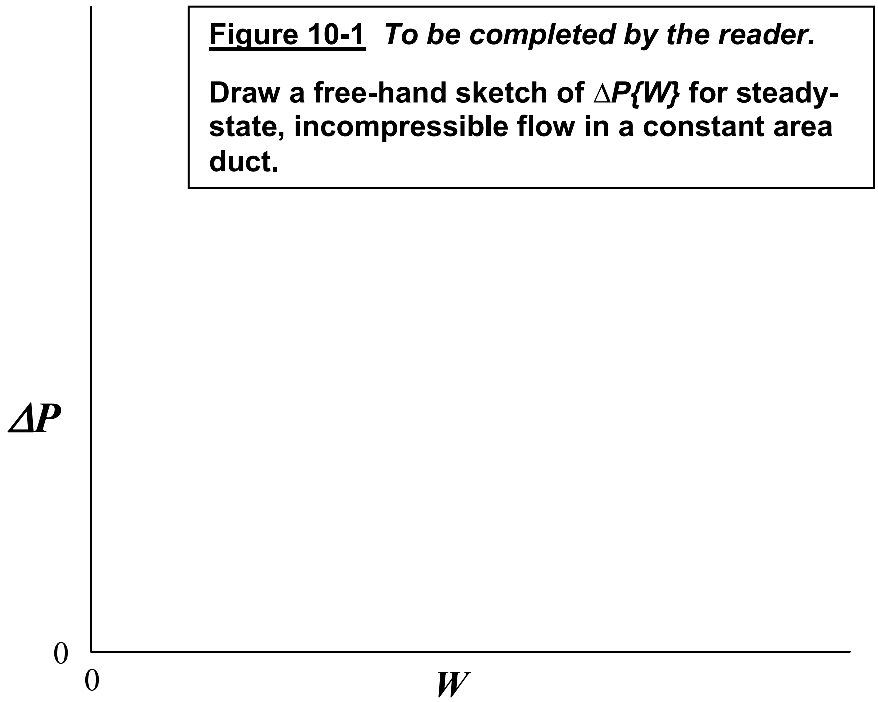


Figure 10-1 ***To be completed by the reader.***

Draw a free-hand sketch of Δ*P{W}* for steady-state, incompressible flow in a constant area duct.

10.2 Friction factors

There are two widely used “friction factors”—the Darcy friction factor and the Fanning friction factor. They are defined as follows:

The Darcy friction factor is $(2gD\rho A^2\Delta P/LW^2)$.

The Fanning friction factor is $(gD\rho A^2\Delta P/2LW^2)$.

Notice that both friction factors combine ΔP and W in parameter groups that are proportional to $\Delta P/W^2$. Therefore ΔP and W can be separated only if friction factor is *not* used.

It is generally agreed that friction factor is related to Reynolds number and relative wall roughness in the following ways:

- In laminar flow, the friction factor is a unique function of Reynolds number $(D/\mu A)W$. The Darcy laminar friction factor is $64/N_{Re}$. The Fanning laminar friction factor is $16/N_{Re}$.

- In turbulent flow, the friction factor is a function of Reynolds number and wall relative roughness ε/D.

- If the flow is turbulent and the wall relative roughness is very large, the friction factor depends primarily on wall relative roughness, and little depends on Reynolds number.

The Moody chart is in the form friction factor vs Reynolds number, with wall relative roughness as parameter.

10.3 The Moody chart as described by Moody.

Moody (1944) describes the purpose of the Moody chart:

The author does not claim to offer anything particularly new or original, his aim merely being to embody the now accepted conclusions in convenient form for engineering use.

Moody explains why the coordinates in the Moody chart are friction factor and Reynolds number, since other coordinates were also in use in 1944:

. . . R. J. S. Pigott (1933) published a chart for the (Darcy friction factor)*, using the same coordinates* (used in the Moody chart). *His chart has proved to be most useful and practical and has been reproduced in a number of texts.*

Moody divided the chart into four Reynolds number zones, and used literature correlations and generally accepted views to generate the curves in each zone. The following describes the four zones and the manner in which Moody determined the relationship between friction factor and Reynolds number in each zone:

- Laminar flow zone: Reynolds number is less than 2000. Line in chart is Hagen-Poiseuille law, $f_{Darcy} = 64/N_{Re}$.

- Critical zone: Reynolds number is 2000 to 4000. Friction factor is an indeterminate value between laminar flow value and turbulent flow value. Zone is shown as a gray area.

- Transition zone: Region between the smooth wall curve and the lower limit of the rough-pipe zone. The smooth wall curve was determined from an equation attributed to von Karman (1930), Prandtl (1933), and Nikuradse (1933). The curves within the transition region were determined from the Colebrook (1938-1939) function. The lower limit of the rough-pipe zone was determined from an equation used by Rouse (1943) to generate a friction factor chart.

- Rough-pipe zone: Lower limit of zone was determined from an equation used by Rouse (1943) to generate a friction factor chart. Lines within the zone were determined from the generally accepted view that, in rough-pipe zone, friction factor is independent of Reynolds number. Zone has no upper limit.

With regard to the accuracy of the Moody chart, Moody states:

It must be recognized that any high degree of accuracy in determining the friction factor is not to be expected.

10.4 Why fluid "friction factor" is undesirable.

Fluid "friction factor" is the parameter group $(gD\rho A^2\Delta P/LW^2)$ multiplied by 2 (to give the Darcy friction factor) or divided by 2 (to give the Fanning friction factor).

Both friction factors are undesirable because they combine the primary variables W and ΔP, thereby making it necessary to solve fluid flow problems with W and ΔP combined, even though it is generally much simpler to solve nonlinear problems if the primary variables are kept separated.

10.5 Mathematical analogs of fluid flow parameters.

The primary parameters in fluid flow engineering are flow rate W and pressure drop ΔP. The mathematical analogs of fluid flow parameters are listed below:

Parameter	Symbolic form	Analog
Fluid flow rate, pps	W	x
Pressure drop, psi	ΔP	y
Reynolds No.	$(D/\mu A)W$	ax
Fanning friction factor	$(gD\rho A^2/2L)(\Delta P/W^2)$	$b(y/x^2)$
Darcy friction factor	$(2gD\rho A^2/L)(\Delta P/W^2)$	$4b(y/x^2)$

10.6 Why the Moody chart is undesirable.

In the Moody chart, the relationship between W and ΔP is described in the form friction factor $(2gD\rho A^2/L)(\Delta P/W^2)$ vs Reynolds number $(D/\mu A)W$. This form is undesirable because:

- If ΔP is given and the problem is to determine W, neither the Reynolds number nor the friction factor can be calculated directly from the given information. Consequently an indirect procedure is required *simply to read the Moody chart*. The Moody chart can be read in a direct manner *only* if W is included in the given information.

- The Moody chart is in the form $a\Delta P/W^2$ vs bW. This form is undesirable because it largely conceals the relationship it is intended to describe—namely, the relationship between ΔP and W.

- The mathematical analog of the Moody chart is a chart of y/x^2 vs x. In pure mathematics, it would be unheard of to describe a highly nonlinear function such as $\Delta P\{W\}$ with a chart in the form y/x^2 vs x. Note that $\Delta P\{W\}$ includes a region in which ΔP is proportional to W, a region in which there is a step change in $\Delta P\{W\}$, and a region in which ΔP is proportional to W raised to a power between 1.8 and 2.

In summary, the Moody chart is undesirable because it is in an inconvenient form for engineering use—so inconvenient that the Moody chart must be read by trial-and-error or iteratively if ΔP is given, and W is to be determined. Yet the Moody chart is widely used in conventional engineering because it is, in Moody's words, *in convenient form for engineering use*.

10.7 The purpose of Problem (10-1).

The purpose of Problem (10-1) is to illustrate that the Moody chart must be read in an indirect and undesirable manner if W is not given. Note that both friction factor and Reynolds number are functions of W, and therefore the value on *neither* axis can be calculated if W is not given. Therefore if ΔP is given and the problem is to determine W, the Moody chart must be read by trial-and-error, or in an iterative manner such as the following:

- Estimate the flow rate.

- Calculate a Reynolds number based on the estimated flow rate.

- Read the Moody chart to determine the friction factor at the above Reynolds number and the given relative roughness.
- Use the above friction factor and the given ΔP to determine a better estimate of the flow rate.
- Repeat the above until the solution converges.
- If the solution diverges, use a different indirect method.

It is important to note that, if the Moody chart is transformed so as to separate W and ΔP by eliminating friction factor, the resultant chart can be read directly if W is given and ΔP is to be determined, and if ΔP is given and W is to be determined.

10.8 The purpose of Problem (10-2).

The purpose of Problem (10-2) is to illustrate that the Moody chart largely conceals the relationship it is intended to describe—namely, the relationship between ΔP and W.

The solution of Problem (10-2) requires the transformation of the Moody chart from the form $a\Delta P/W^2$ vs bW to the form cΔP vs dW. Figure (11-2) is the solution to Problem (10-2).

The Moody chart is in the form y/x^2 vs x. This form would *not* be used in mathematics because it largely conceals the relationship between x and y. Yet y/x^2 vs x is the mathematical analog of the Moody chart, and the Moody chart is widely used in conventional engineering.

10.9 Fluid friction factor in laminar flow.

In conventional engineering, Eq. (10-1) describes the relationship between fluid flow rate and pressure drop. It is used for both laminar flow and turbulent flow.

$$\Delta P_{laminar\ or\ turbulent} = f_{Darcy}\,(L/2gD\rho A^2)(W^2) \qquad (10\text{-}1)$$

Consider the following anomaly:

- Eq. (10-1) *explicitly* states that $\Delta P_{laminar}$ is proportional to W^2.
- Eq. (10-1) *actually* states that $\Delta P_{laminar}$ is proportional to W.

- The discrepancy between appearance and reality results because Eq. (10-1) describes the relationship between ΔP and W explicitly *and* implicitly.

- The relationship between ΔP and W is in part described implicitly because f_{Darcy} is a function of W. For example, in laminar flow, $f_{Darcy} = 64/N_{Re} = 64/(DW/\mu A)$.)

- When both explicit and implicit functionalities are considered, Eq. (10-1) actually states that $\Delta P_{laminar}$ is proportional to W, even though it appears to state that $\Delta P_{laminar}$ is proportional to W^2.

In order to make appearance agree with reality in the laminar region, f_{Darcy} must be eliminated from Eq. (10-1). This is accomplished by substituting $64/(DW/\mu A)$ for $f_{Darcy,laminar}$, resulting in Eq. (10-2). Note that Eq. (10-2) explicitly and correctly indicates that $\Delta P_{laminar}$ is proportional to W. In other words, in Eq. (10-2), appearance agrees with reality, as desired.

$$\Delta P_{laminar} = (32\mu L/gD^2\rho A)W \qquad (10\text{-}2)$$

Also note that ΔP and W are separate and explicit in Eq. (10-2). Therefore Eq. (10-2) is a behavior equation, and is in the form required in the new engineering.

Eq. (10-2) is often used in conventional engineering in place of Eq. (10-1). However, Eq. (10-2) is so superior to Eq. (10-1) that it is surprising that Eq. (10-1) and friction factor are *ever* used to describe or analyze laminar flow.

10.10 Conclusions

- Friction factor is the dimensionless group $(gD\rho A^2\Delta P/LW^2)$ multiplied or divided by 2.

- Friction factor is undesirable because it combines ΔP and W, thereby making it necessary to solve fluid flow problems with the variables combined, even though nonlinear problems are generally much easier to solve if the variables are separated.

- The Moody chart is undesirable because it is in the form friction factor vs Reynolds number. Since the parameters on both axes are functions of W, the chart must be read in an indirect manner when ΔP is given, and W is to be determined.

- The Moody chart is undesirable because it is in a form that largely conceals the relationship between ΔP and W, the relationship it is intended to reveal.

- Even in conventional engineering, friction factor should not be used to describe or analyze laminar flow because it results in an equation that explicitly states that ΔP is proportional to W^2, when in fact the equation states that ΔP is proportional to W.

Chapter 11

Fluid flow behavior methodology.

11 Summary

Chapter 10 demonstrates that "friction factor" and friction factor charts such as the Moody chart do not provide a desirable methodology for dealing with fluid flow. The methodology is undesirable because friction factor combines the primary parameters ΔP and W, thereby making it necessary to solve problems with ΔP and W combined, even though fluid flow problems are generally much easier to solve ΔP and W are kept separated.

This chapter presents fluid flow methodology based on fluid flow "behavior"—ie methodology in which ΔP and W are kept *separate* and *explicit*. In order to separate ΔP and W, friction factor *must* be abandoned, and the Moody chart *must* be transformed.

11.1 The behavior replacements for "friction factor" and the Moody chart.

The Moody chart presents fluid flow information in a form that is inconvenient for engineering use—friction factor $(2Dg\rho A^2/L)(\Delta P/W^2)$ vs Reynolds number $(D/\mu A)W$. The inconvenience results because W appears on both axes, making it necessary to read the chart by trial-and-error or iteratively if ΔP is given, and W is to be determined from the chart.

In order to present the information in the Moody chart in a more convenient form, the chart must be transformed so that W appears on only one axis, and ΔP appears only on the other axis. This transformation requires that friction factor be abandoned because both ΔP and W are implicit in friction factor.

The Moody chart is transformed to behavior form in the following way:

- Note that the Darcy friction factor is $(2Dg\rho A^2/L)(\Delta P/W^2)$ and that the Reynolds number is $(D/\mu A)W$.

- Note that in order to separate ΔP and W, a parameter group is required that is a function of ΔP, but is independent of W.

- Note that if f_{Darcy} is multiplied by $0.5N_{Re}^2$, the resultant parameter group is $(D^3g\rho/L\mu^2)\Delta P$. This parameter group is a function of ΔP, but is independent of W, as desired

- Note that if the y axis is labeled $(D^3g\rho/L\mu^2)\Delta P$, and the x axis is labeled $(D/\mu A)W$, the resultant chart will be in behavior form—ie will be in the form $\Delta P\{W\}$—ie ΔP and W will be *separate* and *explicit*.

In summary:

- In behavior methodology, the dimensionless parameters called "Darcy friction factor" and "Fanning friction factor" are replaced by the dimensionless group parameter $(D^3g\rho/L\mu^2)\Delta P$. Since this group parameter includes the primary parameter ΔP, the new engineering requires that it appear in explicit form, and similarly for the Reynolds number since it includes primary parameter W.

- In behavior methodology, the Moody chart is replaced by the fluid flow "behavior" chart—a chart of $(D^3g\rho/L\mu^2)\Delta P$ vs $(D/\mu A)W$, with ε/D parameter. The axes titles *must* be in explicit form.

- Because ΔP and W are separated in fluid flow behavior charts, the charts can be read directly if W is given and ΔP is sought, *and* if ΔP is given and W is sought.

11.2 Generating the fluid flow behavior chart.
The fluid flow behavior chart is generated by transforming the Moody chart from f_{Darcy} (or $f_{Fanning}$) vs Reynolds number to $(D^3g\rho/L\mu^2)\Delta P$ vs $(D/\mu A)W$. The transformation is accomplished as follows:

- On a spreadsheet, list (f_{Darcy},N_{Re}) or $(f_{Fanning},N_{Re})$ coordinates for each curve in the Moody chart.

- Multiply the f_{Darcy} coordinates by $0.5N_{Re}^2$ (or the $f_{Fanning}$ coordinates by $2N_{Re}^2$), resulting in coordinates of $((D^3g\rho/L\mu^2)\Delta P, (D/\mu A)W)$.

- Plot $(D^3g\rho/L\mu^2)\Delta P$ vs $(D/\mu A)W$.

- Entitle the y-axis "$(D^3g\rho /L\mu^2)\Delta P$", and the x-axis "$(D/\mu A)W$".

- Entitle the chart "Fluid flow behavior chart".

Figure (11-1) is a fluid flow behavior chart. It was obtained by transforming Moody chart curves for a smooth wall, and for a wall of relative roughness $\varepsilon/D = .003$. Notice that Figure (11-1) is on two pages, and that it is incomplete in that it omits many of the ε/D values found in the Moody chart.

11.3 Advantages of fluid flow behavior charts relative to friction factor charts.

Fluid flow behavior charts such as Figure (11-1) have the following advantages relative to friction factor charts such as the Moody chart.

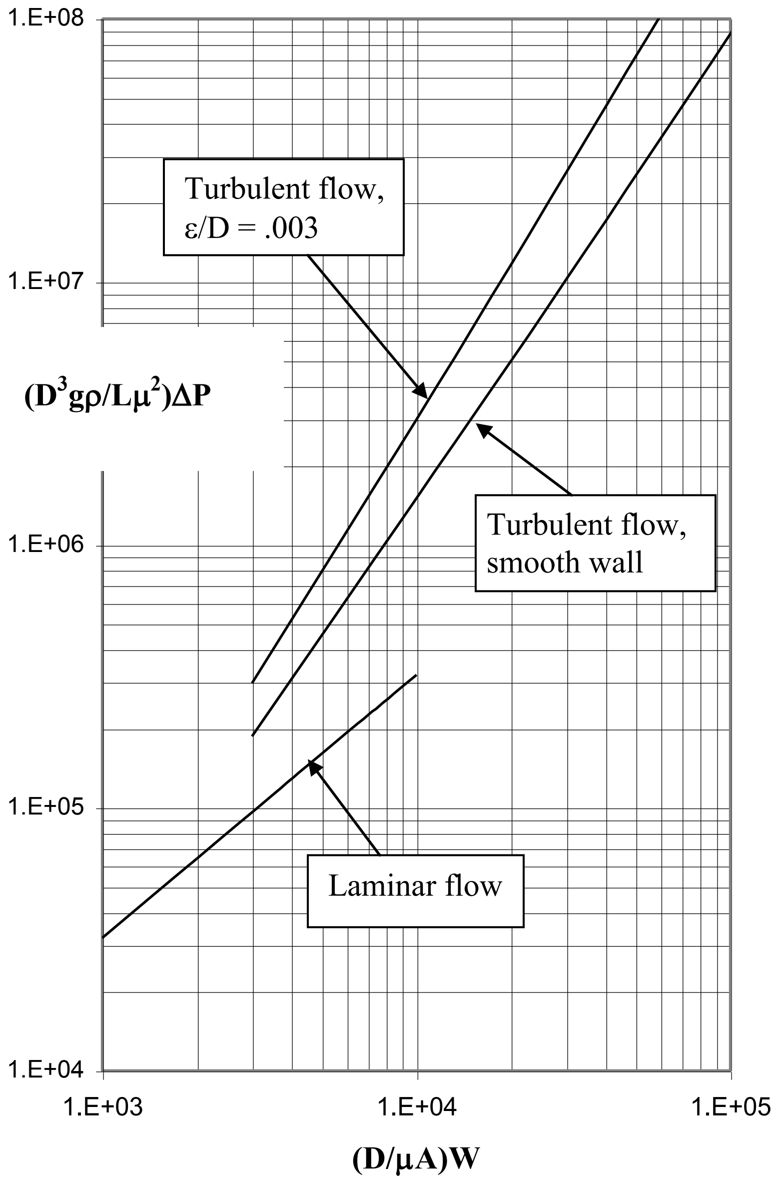
- Fluid flow behavior charts can be read directly if ΔP is given and W is sought, *and* if W is given and ΔP is sought. Friction factor charts such as the Moody chart can be read directly if W is given and ΔP is sought, but must be read in an indirect manner if ΔP is given and W is sought.

- Fluid flow behavior charts readily reveal the qualitative relationship between ΔP and W because they are in the form $a\Delta P$ vs bW. Friction factor charts largely conceal the relationship between ΔP and W because they are in the form $c\Delta P/W^2$ vs bW.

Recall that Moody's stated purpose in preparing the Moody chart was to put accepted knowledge of fluid flow behavior *"in convenient form for engineering use"*, and that he used the same coordinates used by Pigott because Pigott's *"chart has proved to be most useful and practical . . . "*.

On the bases of convenience, usefulness, and practicality, the fluid flow behavior chart should replace the Moody chart because the Moody chart can be read directly only if W is given and ΔP is to be determined, whereas the fluid flow behavior chart can be read directly if W is given and ΔP is to be determined, *and* conversely.

Figure 11-1A Fluid flow behavior chart



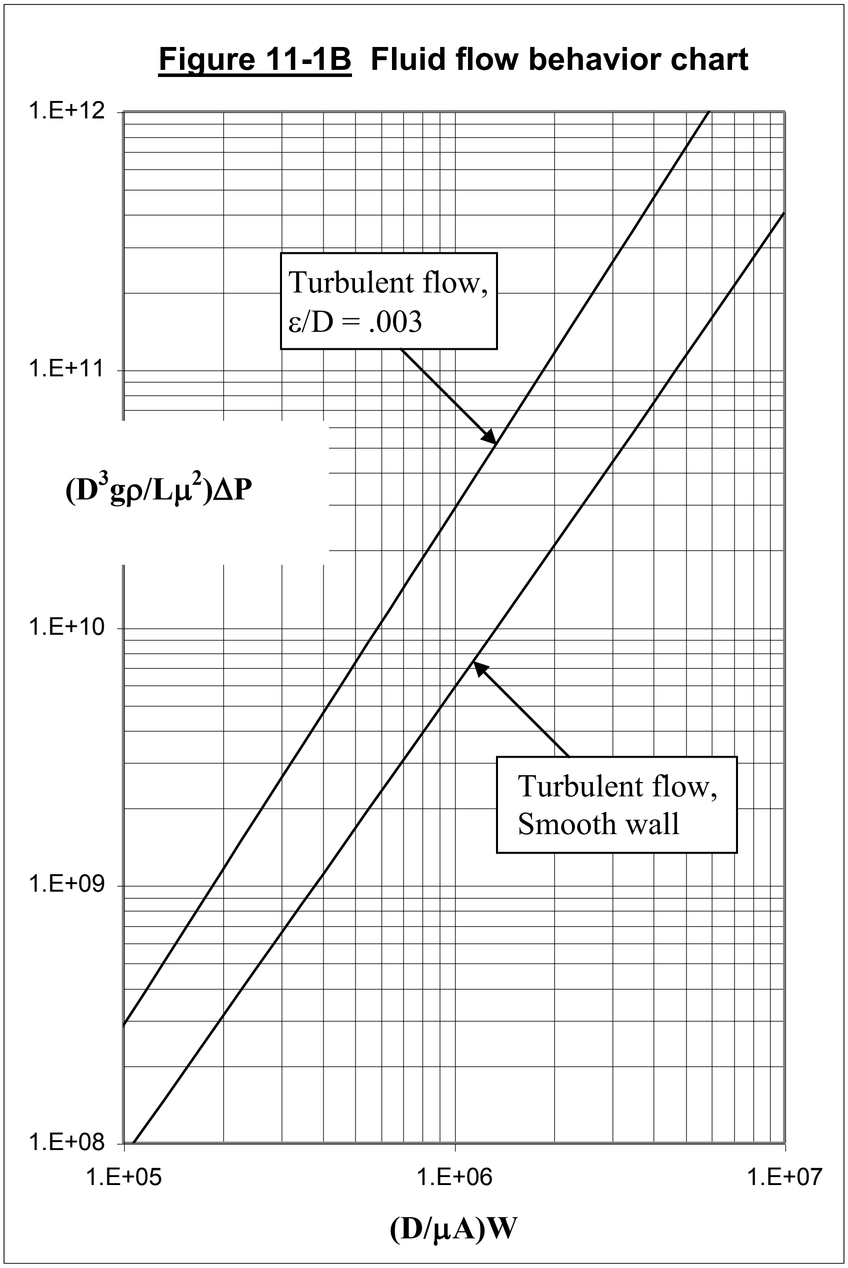


Figure 11-1B Fluid flow behavior chart
1.E+12
1.E+11
1.E+10
1.E+09
1.E+08
1.E+05
1.E+06
1.E+07
Turbulent flow,
ε/D = .003
Turbulent flow,
Smooth wall
(D3gρ/Lμ2)ΔP
(D/μA)W

11.4 Group parameters in fluid flow behavior methodology.
In a previous chapter, it was noted that in the new engineering, group parameters are used in the following manner:

- Group parameters that include both primary parameters are *not* used because combining the primary parameters is mathematically undesirable.

- Group parameters that include one primary parameter are used *only* in explicit form in order that the primary parameter will appear explicitly.

- Group parameters that do not include a primary parameter are used in both explicit and implicit form.

Therefore, in fluid flow behavior methodology,

- Friction factor is *not* used because it combines ΔP and W.

- $(D^3 g\rho/L\mu^2)\Delta P$ is used *only* in explicit form because it includes ΔP.

- Reynolds number is used *only* in explicit form because it includes W. In other words, $(D/\mu A)W$, is used, but the words "Reynolds number" and the symbol N_{Re} are *not*.

11.5 Solution of Problem (10-2).
The solution of Problem (10-2) is readily apparent by noting that the fluid flow behavior chart, Figure (11-1), is a quantitative description of $\Delta P\{W\}$ in logarithmic form. The logarithmic form is transformed to linear form by noting the following in Figure (11-1):

- All the curves in the figure are essentially straight lines, and therefore the curves are essentially of the form $\Delta P = mW^n$.

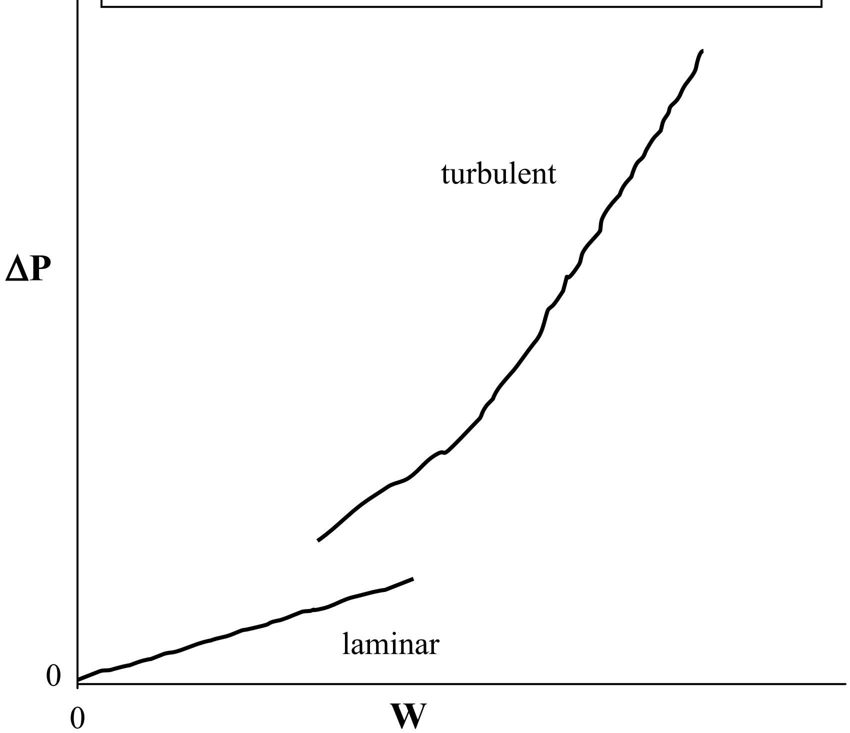
- The exponent in the laminar region is one. Therefore the laminar flow region is described by a straight line that starts at the origin.

- The exponent in the turbulent region is approximately 2. (Note that when W increases a factor of 10, ΔP increases a factor of approximately 100, indicating that the exponent is approximately 2.) Therefore the turbulent region is described by a curve that turns upward.

- The turbulent region line begins near the upper end of the laminar region, and *above* the laminar line.

The solution of Problem (10-2) is shown in Figure (11-2).

Figure 11-2 Solution of Problem (10-2): A qualitative description of ΔP{W}.



11.6 Simple, analytical expressions for fluid flow behavior.
In most practical cases, there is no need to use the fluid flow behavior chart because the curves in the chart are accurately described by simple, easy-to-use equations. In the laminar region, fluid flow behavior is described by Eq. (10-2). In the turbulent region, fluid flow behavior is described by equations in the form of Eq. (11-1) where m and n are constants whose value depends solely on the value of ε/D.

$$(D^3 g\rho/L\mu^2)\Delta P_{turb} = m(DW/\mu A)^n \qquad (11\text{-}1)$$

For example, for smooth wall tubes, $\Delta P\{W\}$ determined from Eq. (11-2) closely agrees with $\Delta P\{W\}$ determined from the Moody chart. For relative wall roughness of .003, $\Delta P\{W\}$ determined from Eq. (11-3) closely agrees with $\Delta P\{W\}$ determined from the Moody chart.

$$(D^3 g\rho/L\mu^2)\Delta P_{turb,smooth} = 0.0755\ (DW/\mu A)^{1.8142} \qquad (11\text{-}2)$$

$$(D^3 g\rho/L\mu^2)\Delta P_{turb,e/D = .003} = 0.0289\ (DW/\mu A)^2 \qquad (11\text{-}3)$$

The close agreement between chart and equations is shown in Figure (11-3). Note that the solid lines obtained by transforming Moody chart curves are barely distinguishable from the dashed lines based on Eqs. (11-2) and (11-3). And recall Moody's statement:

> *It must be recognized that any high degree of accuracy in determining the friction factor is not to be expected.*

11.7 Analytical expressions for turbulent friction factor.

The friction factor curves in the Moody chart may also be described analytically, but the equations for turbulent flow are not easy to use. Moody states that the smooth wall curve was obtained from Eq. (11-4), and the curves in the transition region were obtained from Eq. (11-5).

$$1/(f_{Darcy})^{.5} = 2\ \log_{10}(N_{Re} f_{Darcy}{}^{0.5}/2.51) \qquad (11\text{-}4)$$

$$1/(f_{Darcy})^{.5} = -2\ \log((\varepsilon/3.7D) + 2.51/(N_{Re} f_{Darcy}{}^{0.5})) \qquad (11\text{-}5)$$

Note that neither equation is easy to use.

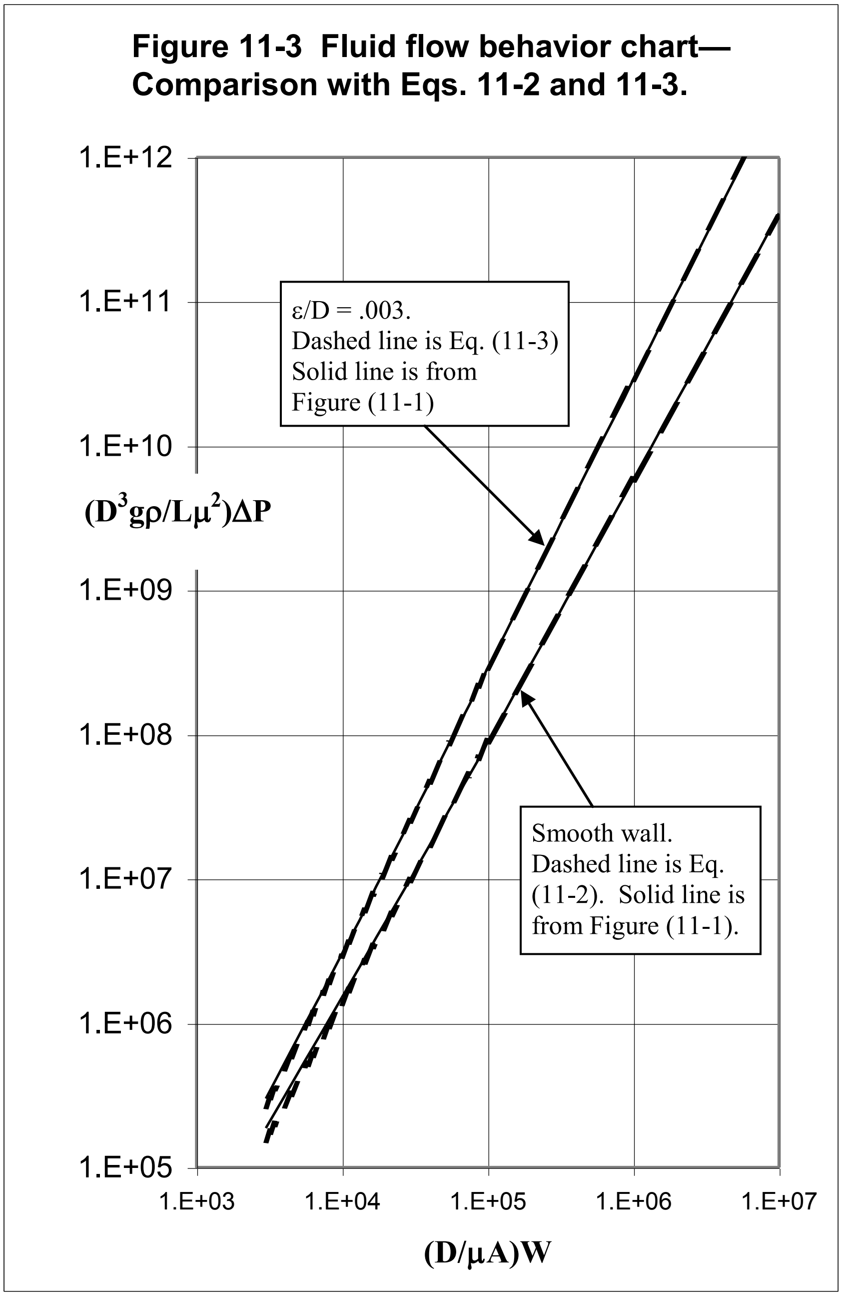


Figure 11-3 Fluid flow behavior chart—
Comparison with Eqs. 11-2 and 11-3.
1.E+12
1.E+11
1.E+10
1.E+09
1.E+08
1.E+07
1.E+06
1.E+05
1.E+03
1.E+04
1.E+05
1.E+06
1.E+07
$(D^3 g\rho/L\mu^2)\Delta P$
$(D/\mu A)W$
ε/D = .003.
Dashed line is Eq. (11-3)
Solid line is from
Figure (11-1)
Smooth wall.
Dashed line is Eq.
(11-2). Solid line is
from Figure (11-1).

11.8 A digital form of the fluid flow behavior chart.

When Moody's article was published, charts were more convenient than computation, as evidenced by the following charts in Moody's article:

- Moody's Figure 1 is the Moody chart.
- Moody's Figure 2 is a chart to determine ε/D, given the pipe diameter and either the value of ε or the pipe material.
- Moody's Figure 3 is a chart to determine Reynolds number for water at 60 F, given the velocity in ft/sec and the pipe diameter in feet or inches.
- Moody's Figure 4 is a chart to determine Reynolds number, given the fluid, the fluid temperature, and the product of velocity in ft/sec and pipe diameter in inches.

Today, because of the widespread use of calculators and computers, simple analytical expressions are much more convenient than charts, and charts are avoided if possible.

The close agreement between the Moody chart curves and the equations in Figure (11-3) suggests that the fluid flow behavior chart should also be presented in digital form. For example, a digital form could be a table that lists ε/D values and the corresponding values of m and n to be used in Eq. (11-1), such as the abbreviated table below.
The complete table would:

- Include all ε/D values in the Moody chart.
- State that laminar flow is described by Eq. (10-2).
- State that the transition from laminar flow to turbulent flow usually occurs at (DW/μA) values in the range 2000 to 4000.
- Include a small, linear chart that qualitatively describes ΔP{W} for (DW/μA) values in the range 0 to 10000.

<u>Digital form of the fluid flow behavior chart</u>

ε/D	**m in Eq. (11-1)**	**n in Eq. (11-1)**
smooth	0.151	1.8142
.003	0.578	2

11.9 Summary

Fluid flow behavior methodology is described by the following:

- Fluid flow phenomena are described and problems are solved with W and ΔP separate and explicit. In order for W and ΔP to be separate, parameter groups that combine them are abandoned—ie parameter groups such as friction factor are abandoned

- The group $(D^3 g\rho/L\mu^2)\Delta P$ replaces both f_{Darcy} and $f_{Fanning}$. This group makes it possible to read the transformed Moody chart directly whether W is given and ΔP is to be determined, and conversely. It also ends the ambiguity and confusion caused by the two different friction factors used in conventional engineering.

- Fluid flow behavior charts replace Moody charts—ie $(D^3 g\rho/L\mu^2)\Delta P$ vs $(D/\mu A)W$ replaces f_{Darcy} (or $f_{Fanning}$) vs N_{Re}. Fluid flow behavior charts can be read in a direct manner if ΔP is given or if W is given, but Moody charts can be read in a direct manner only if W is given.

- Friction factor methodology is replaced by fluid flow behavior methodology. This simplifies the solution of nonlinear fluid flow problems because it allows the primary parameters to be kept separate, whereas the primary parameters are combined in friction factor methodology.

- In the interest of engineering convenience, the fluid flow behavior chart is also presented in the digital form described in Section 11.8.

Chapter 12

How parametric correlations are determined in the new engineering.

12 How parametric correlations are determined in conventional engineering, and why they are not user friendly.

In conventional engineering, parametric correlations generally describe how group parameters are related. The parameters in each group are usually determined *a priori* using dimensional analysis, and the values of coefficients and exponents in correlations are determined by experiment and induction. (Dimensional analysis is discussed in Appendix 2.) For example, some heat transfer correlations describe how group parameters such as Nusselt number and Reynolds number and Prandtl number are related, as in Eq. (12-1).

$$Nu = a\ Re^b Pr^c \tag{12-1}$$

Parametric correlations in terms of group parameters may be convenient for those who *determine* parametric correlations, but they are decidedly *inconvenient* for those who *use* them. Since parametric correlations are of value only if they are used, they should be in the form that is most convenient for the user. Note that:

- Correlations in terms of group parameters *cannot be used* unless group parameters are replaced by *individual* parameters.

- The purpose of parametric equations is to reveal how *individual* parameters are related. Therefore parametric equations should describe how *individual* parameters are related, *not* how *group* parameters are related. It is *impossible* to determine how individual parameters are related unless group parameters are replaced by individual parameters.

  For example, given Eq. (12-1), it is *impossible* to determine how pipe diameter affects the relationship between heat flux and temperature difference unless the groups are replaced by individual parameters.

  Therefore, if a parametric correlation is intended to be used, it should *always* be expressed in terms of *individual parameters*.

- Most parametric correlations in conventional engineering are *fluid generic,* and necessarily contain temperature dependent parameters. Correlations are much more convenient to use if temperature dependent parameters are replaced by temperature, resulting in *fluid specific* correlations that do *not* require reference to property tables.

12.1 The preferred form of parametric correlations.

In the new engineering, the preferred form of parametric correlations is described by the following:

- *Group* parameters are replaced by *individual* parameters.
- *Temperature dependent parameters* are replaced by *temperature*.
- Correlations are fluid *specific* instead of fluid *generic*.
- If a correlation is quantitative, dimension units that underlie parameter symbols *must* be specified in an accompanying nomenclature.

The end result is that parametric correlations are user friendly because they:

- Can be evaluated *without* replacing group parameters.
- Can be evaluated *without* reference to property tables.
- *Reveal* how *individual* parameters are related.

Note that when temperature replaces temperature dependent parameters, correlations contain *only* parameters measured in the underlying experiment. (Conventional engineering correlations include temperature dependent parameters that are *not* measured in the underlying experiment.)

12.2 How parametric correlations used in conventional engineering are transformed for use in the new engineering.

For some time, conventional parametric correlations will be used in the new engineering, but they *must* first be transformed by *separating* primary parameters such as q and ΔT.

They should also be transformed by replacing group parameters with individual parameters, and replacing temperature dependent parameters with temperature, as described in Chapter 13. The end result is parametric correlations in the fluid specific form preferred.

12.3 How parametric correlations are determined in the new engineering.

In the new engineering, the following methodology is used to determine parametric correlations:

- Determine parameter functionality by *induction*.

- Generate correlations that contain *only* parameters measured in the experiment.

- *Assume and verify* that the function of each parameter is independent of the other parameters, as indicated in Eq. (12-2).

  $$y = f_1\{p\}\, f_2\{q\}\, f_3\{r\}\, f_4\{s\} \qquad (12\text{-}2)$$

  It is important to note that the functions in Eq. (12-2) may be in *any form* suggested by the data.

- The assumption (that the function of each parameter is independent of the other parameters) is *experimentally verified* by designing the underlying experiment so that each function is determined at the upper and lower extremes of the other parameters.

- Correlations are *fluid specific*. For example, if the fluid in the underlying experiment is water, the resultant correlation applies only to water.

12.4 An example that demonstrates how parametric correlations are determined in the new engineering.

Problem statement:

Determine a fluid specific correlation for heat transfer to water in turbulent flow in a pipe.

Solution

Assume that the desired correlation is in the form of Eq. (12-3). Note that there are *no* group parameters in Eq. (12-3), and temperature dependent parameters have been replaced by temperature.

$$q = a\, f_1\{D\} f_2\{G\} f_3\{T_{water}\} f_4\{\Delta T\} \qquad (12\text{-}3)$$

Design an experiment to determine each function in Eq. (12-3) at the extreme values of the other parameters.

The experiment design is shown in the table below. The numbers represent relative values of the parameters. Each parameter is to be tested at 5 values while other parameters are at their *lowest* value, and at the same 5 values while other parameters are at their *highest* values. This makes it possible to verify the assumption that each function in Eq. (12-3) is independent of the value of the other parameters.

G	**T_{water}**	**ΔT**
1	**1**	**1-5**
5	**5**	**1-5**
1	**1-5**	**1**
5	**1-5**	**5**
1-5	**1**	**1**
1-5	**5**	**5**

30 data points are required for each pipe diameter tested, 15 to determine each function in Eq. (12-3), and 15 to verify that the effect of each parameter is independent of the value of the other parameters.

Since correlations in conventional engineering generally indicate that q is a weak function of D, the experiment design would reasonably call for only three values of D, and the entire experiment would require 90 data points. ((If it is known *a priori* that q is proportional to ΔT, the entire experiment requires only 60 data points.)

12.5 Advantages of parametric correlations determined using the new engineering.

Correlations determined using conventional engineering methodology are generally fluid generic, and describe how dimensionless group parameters are related. The parameters in each group are usually selected by *a priori* deduction referred to as dimensional analysis[3].

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[3] In conventional engineering, it seems reasonable to accept that the dimensional analysis of parametric equations is rational because parameter symbols in parametric equations represent dimension. In the new engineering, it is *not* reasonable to accept that the dimensional analysis of parametric equations is rational because parameter symbols in rational parametric equations are *dimensionless*. Therefore rational parametric equations contain *no dimensions* to be analyzed.

Correlations determined using new engineering methodology have the following advantages relative to correlations determined using conventional engineering methodology:

- They are more likely to closely agree with data for two reasons:
    - Because parameter functionality is determined by *induction,* rather than by a combination of *a priori deduction* and induction.
    - Because correlations are fluid *specific* rather than fluid *generic*.
- They do *not* require transformation from group parameters to individual parameters.
- They *reveal* how *individual* parameters are related rather than how *group* parameters are related.
- They do *not* require reference to property tables.

Chapter 13

How to transform conventional parametric correlations to the form preferred in the new engineering.

Much of the material in this chapter is from Adiutori (1975, Chapter 7).

13 Parametric correlations in the new engineering.

This chapter demonstrates how to transform the fluid *generic* parametric correlations widely used in conventional engineering to the fluid *specific* form preferred in the new engineering. Two example transformations result in fluid specific correlations for water and air.

13.1 The transformation of a fluid *generic* heat transfer correlation to a fluid *specific* correlation for water.

Eq. (13-1a) is a fluid generic heat transfer correlation in terms of group parameters. It is widely used in conventional engineering, and concerns heat transfer to one phase, forced convection fluids in turbulent flow.

$$Nu = .023\ Re^{.8}\ Pr^{.4} \quad (13\text{-}1a)$$

$$qD/\Delta Tk = .023\ (DG/\mu)^{.8}(C_p\mu/k)^{.4} \quad (13\text{-}1b)$$

$$q = .023\ k^{.6}\ D^{-.2}\ G^{.8}\ \mu^{-.4} C_p^{.4}\ \Delta T \quad (13\text{-}1c)$$

Since k, μ, and C_p are functions of temperature, we may write

$$q = .023\ D^{-.2}\ G^{.8} f\{T\}\Delta T \quad (13\text{-}2)$$

in which $f\{T\}$ is described by Eq. (13-3).

$$f\{T\}_{Eq.\ (13\text{-}3)} = k^{.6}\ \mu^{-.4}\ C_p^{.4} \quad (13\text{-}3)$$

The table below lists coordinates of $f\{T\}_{Eq.\ (13\text{-}3)}$ based on saturated water properties from Holman (1972). The dimension units are Btu, pounds, feet, Fahrenheit, and hours.

T, F	$f\{T\}_{Eq. (13-3)} = k^{.6} C_p^{.4} \mu^{-.4}$
100	.4458
150	.5515
200	.6441
250	.7112
300	.7964
350	.8529
400	.9004
450	.9415
500	.9685

A curve fitted to the above coordinates is described by $f\{T\}_{Eq. (13-4)}$.

$$f\{T\}_{Eq. (13-4)} = .2068 + 2.631x10^{-3}T - 2.218x10^{-6}T^2 \qquad (13\text{-}4)$$

The table below compares $f\{T\}_{Eq.(13-3)}$ and $f\{T\}_{Eq. (13-4)}$.

T, F	$f\{T\}_{Eq. (13-3)}$	$f\{T\}_{Eq. (13-4)}$	% Difference
100	.4458	.4477	0.43
150	.5515	.5515	0.00
200	.6441	.6443	0.03
250	.7112	.7105	0.10
300	.7964	.7965	0.01
350	.8529	.8559	0.35
400	.9004	.9043	0.43
450	.9415	.9416	0.01
500	.9685	.9678	0.07

Combining Eqs. (13-2) and (13-4) results in Eq. (13-5), a *quantitative, dimensionless, homogeneous, fluid specific* correlation for heat transfer to forced convection saturated water in turbulent flow. Because Eq. (13-5) is *quantitative*, the dimension units that underlie parameter symbols *must* be specified, and are Btu, pound, foot, Fahrenheit, and hour.

$$q = .023\, D^{-.2}\, G^{.8} (.2068 + 2.631x10^{-3}T - 2.218x10^{-6}\, T^2)\, \Delta T \qquad (13\text{-}5)$$

Note that Eq. (13-5) *reveals* the relationship between q, D, G, T, and ΔT, whereas Eq. (13-1a) *reveals nothing* about the relationship between q, D, G, T, and ΔT.

The above table indicates that, from 100 F to 500 F, Eq. (13-5) agrees with Eq. (13-1a) within 0.5%.

Note that Eq. (13-5) is much more user friendly than Eq. (13-1a) because:

- In order to use Eq. (13-1a), the user must first transform it by replacing group parameters with individual parameters. There are *no* group parameters in Eq. (13-5).

- In order to use Eq. (13-1a), the user must first determine the values of temperature dependent parameters k, μ, and C_p by referring to material property tables. There are *no* temperature dependent parameters in Eq. (13-5).

13.2 The transformation of a fluid *generic* heat transfer correlation to a fluid *specific* correlation for atmospheric air.

When $f\{T\}_{Eq.\ (13\text{-}3)}$ is evaluated for atmospheric air using properties from Holman (1972), and $f\{T\}_{Eq.\ (13\text{-}6)}$ is used to approximate $f\{T\}_{Eq.\ (13\text{-}3)}$, the results in the table below are obtained with dimension units Btu, pounds, feet, Fahrenheit, and hours.

$$f\{T\}_{Eq.\ (13\text{-}6)} = .1544 + 6.37\text{x}10^{-5}T - 1.59\text{x}10^{-8}T^2 \qquad (13\text{-}6)$$

T, F	$f\{T\}_{Eq.\ (13\text{-}3)}$	$f\{T\}_{Eq.\ (13\text{-}6)}$	% Difference
100	.1603	.1606	0.19
200	.1665	.1665	0.00
300	.1719	.1721	0.12
400	.1772	.1773	0.06
500	.1822	.1823	0.05
600	.1862	.1869	0.38
700	.1908	.1912	0.21
800	.1951	.1952	0.05
900	.1987	.1989	0.10

Combining Eqs. (13-2) and (13-6) results in Eq. (13-7), a *quantitative, dimensionless, homogeneous, fluid specific* correlation for heat transfer to forced convection air in turbulent flow. Because Eq. (13-7) is *quantitative*, the dimension units that underlie parameter symbols *must* be specified, and are Btu, pound, foot, Fahrenheit, and hour.

$$q = .023\ D^{-.2}\ G^{.8}\ (.1544 + 6.371\text{x}10^{-5}T - 1.598\text{x}10^{-8}\ T^2)\ \Delta T \qquad (13\text{-}7)$$

Note that the above table indicates that, from 100 F to 900 F, Eq. (13-7) agrees with conventional Eq. (13-1a) within 0.5%.

Chapter 14

Research methodology, and the problem with *a priori* deduction.

Much of this chapter is excerpted from Adiutori (1975, Chapter 1).

14 The scientific method.

The first step in the scientific method is experimentation. The second step is induction *without* prejudice—induction *without* being influenced by *anything* except data. It is easy to obtain data. The difficult part is induction *without* prejudice.

For example, if a researcher knows from the literature that the phenomenon he is investigating generally describes a particular type of behavior, it is quite likely that he will induce the same type of behavior, even if the data do not support that conclusion.

For almost one hundred years, heat transfer journals and texts have generally agreed that nucleate boiling heat transfer data exhibit highly *nonlinear* behavior, even though nucleate boiling data in the literature exhibit highly *linear* behavior. (The erroneous nonlinear conclusion resulted from induction methodology that is not rigorous.)

(In 1964, my article that showed that nucleate boiling data exhibit highly *linear* behavior was favorably reviewed and was accepted for publication in the *AIChE Journal* by Professor Editor Harding Bliss (Yale).

However, it was *never* published in the *AIChE Journal* because shortly before publication, Professor Editor Bliss received a letter from Professor James W. Westwater (head of the Chemical Engineering Department at University of Illinois) in which he requested that my article *not* be published.

Thirty years later, the article was published in a Japanese journal of mechanical engineering. (See Adiutori (*1994*).) To the best of my knowledge, it has *never* been referenced in an American publication. And 23 years later, the erroneous nonlinear view is *still* presented in American heat transfer texts.)

It is more than likely that the erroneous nonlinear view has prevailed for almost one hundred years because researchers were influenced by the knowledge that earlier researchers generally concluded that nucleate boiling heat transfer data exhibit highly *nonlinear* behavior.

For almost one hundred years, most American heat transfer texts have:

- Credited Newton with Eq. (14-1) and h,
- Referred to Eq. (14-1) as “Newton’s law of cooling”.
- Cited Newton (1701).

But the truth is that Newton’s 1701 article has nothing to do with Eq. (14-1), and nothing to do with h. Newton had *no understanding* of Eq. (14-1) or h.

$$q = h\,\Delta T \qquad (14\text{-}1)$$

Eq. (14-1) and h were in fact conceived by Fourier. Fourier (1822) should be cited, and Eq. (14-1) should be referred to as “Fourier’s law of heat transfer to atmospheric air in forced convection”. (See Adiutori (1974, 1989, 1990, 2005) and Bejan (1993 and 2013).

It is more than likely that this erroneous citation has prevailed for almost one hundred years (and has not yet died) because researchers and authors of heat transfer texts have been influenced by the knowledge that earlier researchers and authors generally credited Newton with Eq. (14-1) and h.

14.1 Dealing with prejudice.

Experimental research articles in engineering journals generally have two things in common:

- They present the results of *a priori* literature searches to determine what work has been done, and what conclusions have been reached in the area of interest.
- They describe *a priori* analyses. For example, if the purpose of the research is to generate a parametric correlation, they describe *a priori* analyses to predict parameter functionality.

After the researcher has steeled himself with a knowledge of conclusions reached by others who performed similar experiments, and by *a priori* analyses to predict functionality, it is fair to ask

Can the researcher analyze the data without prejudice?

My answer to this question is:

NO!

How can one guard against prejudice? It is not easy. There is no foolproof method. Even constant vigilance is sometimes not enough. In order to minimize prejudice, *I follow and recommend* the following guidelines:

- NEVER perform *a priori* deduction to determine *anything* about the proposed correlation. Determine the proposed correlation by *induction*.

- NEVER perform *a priori* literature searches.

- NEVER pay *any* attention to *anyone's* prediction about parameter functionality.

- ALWAYS make a conscious effort to forget or ignore what you think you know about parameter functionality.

- ALWAYS be ready to accept that the data accurately describe functionality, no matter how violently the data disagree with reported results of similar experiments, or with popular views of the day.

It is probably not necessary to point out that these guidelines bear no resemblance to the experimental research methodology used in conventional engineering. These guidelines will likely seem preposterous to anyone familiar with engineering journals,

But it is in fact conventional research methodology that is preposterous. It is preposterous because it largely prevents the researcher from analyzing his data with an open mind, and an open mind is a *cardinal* requirement in science.

Untermeyer (1955) presents the following Wright brothers' quote. Note that they decided to *disregard* the results of their literature searches.

Having set 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. . .

We had taken up aeronautics as a sport. We reluctantly entered upon the scientific side of it, and we soon found the work so endlessly fascinating that we were driven into it deeper and deeper.

Even a brief review of engineering journals reveals that *a priori* analyses and *a priori* literature searches are important parts of the preparation for experiment. These things have nothing to do with experiment in the new engineering. In the new engineering, researchers go to great lengths to avoid methodology that increases the likelihood of prejudice.

Much of my sixty years in engineering has been spent in hands-on experimental research in industry. I always approach an experiment within the guidelines listed above, and with the hope that the data will *not* agree with the generally accepted view of whatever I am investigating.

My mindset is dictated by the knowledge that experimental results that do *not* agree with widely accepted views are the most valuable, and result in the *greatest progress.*

Many years ago, Pasteur said the following about prejudice:

But if we are inclined to believe that it is so because we think it likely, let us remember, before we affirm it, that the greatest disorder of the mind is to allow the will to direct the belief.

14.2 An example that reveals the problem with *a priori* deduction.
The problem with *a priori* deduction is that it can result in correlations that little resemble the data correlated, and that are accepted in spite of the fact that they little resemble the data correlated—as in the following example.

In 1975, it had been widely accepted for decades that, in the "far from slot regime", film cooling effectiveness is described by correlations in the form of Eq. (14-2), a dimensionless correlation in the group parameter form that usually results from dimensional analysis. (See Wieghardt (1946), Hartnett, Birkebak, and Eckert (1961), and Kays (1966).)

$$\eta = a(x/ms)^{-b} \qquad (14\text{-}2)$$

η is the dimensionless parameter "film effectiveness" (η = 0 indicates *no* cooling effect, η = 1.0 indicates *complete* cooling effect), x is the distance downstream from the film coolant slot exit, m is the ratio of coolant mass flow rate to mainstream mass flow rate at the film coolant slot exit, and s is the height of the film coolant slot at the exit.

It is important to note that:

- The exponent in Eq. (14-2) is *negative*. Therefore Eq. (14-2) states that η = *infinity* at x = 0—ie at the film coolant slot exit.
- η *cannot* exceed 1.0 because 1.0 indicates that the temperature of the cooled surface is the same as the temperature of the coolant.
- Correlations in the form of Eq. (14-2) are said to apply only in the "far from slot regime". (In 1975, there were few (if any) conventional engineering correlations for the "near the slot regime", even though the "near the slot regime" is as almost as important to designers and analysts as the "far from slot regime".)
- The location of the "far from slot regime" is *not* established by the detection of a change in the nature of film cooling. The location of the "far from slot regime" is wherever Eq. (14-2) agrees with the data.

  Kays (1966) states that Wieghardt's (1946) Eq. (14-3) agrees with the data if x/s is greater than 100. Therefore the "far from slot regime" is the region downstream of the point at which x/s = 100.

  $$\eta = 21.8\ (x/Ms)^{-0.8} \qquad (14\text{-}3)$$

- Eq. (14-4) is the film cooling correlation presented in Hartnett, Birkebak, and Eckert (1961).

  $$\eta = 16.9\ (x/Ms)^{-0.8} \qquad (14\text{-}4)$$

  The authors state that the form of Eq. (14-4) resulted from *a priori* deduction. Their Fig. 24 compares Eq. (14-4) with their data, and indicates that, downstream of the point at which x/s = 100, Eq. (14-4) agrees with the data.

Adiutori (1974, page 6-31) states:

If the reader will refer to Fig. 24 in the article by Hartnett, Birkebak, and Eckert (1961), he will find that the data near the slot and the data far from the slot are very well correlated by the simple expression

$$\eta = 1/(1 + .0142(x/ms)) \qquad (14\text{-}5)$$

which obviously is not (in the form that usually results from *a priori* deduction). *Eq. (14-5) correlates all the results in Fig. 24 so well that I doubt that any reader will be able to distinguish between Eq. (14-5) and the curve drawn by the authors through their experimental points.*

Note the following:

- I determined Eq. (14-5) by induction, the preferred methodology in the new engineering. Eq. (14-5) closely agrees with the data near the slot, *and* the data far from the slot.

- Hartnett, Birkebak, and Eckert (1961) state that Eq. (14-4) was largely determined by *a priori* deduction. Eq. (14-4) agrees with the data “far from the slot”, but *disagrees* with the data if x/s is less than 100.

- Eq. (14-5) demonstrates that there is only one film cooling regime. In conventional engineering, it was necessary to assume that there are at least two regimes because *a priori* deduction resulted in a correlation that predicts η equals *infinity* at the slot exit.

Together, Eqs. (14-4) and (14-5) demonstrate that accurate parametric correlations are much more likely to result if functionality is determined by *induction* (the research methodology *always* used in the new engineering) rather than a combination of *a priori deduction* and induction, the methodology often used in conventional engineering.

Chapter 15

Why data that underlie journal articles should be published, or stored in documentation institutes open to the public.

Analyzing data is the best way to determine functionality in parametric correlations because:

- *Data is certain. All* else is *uncertain*.

- *Data has permanent value. All* else has *temporary* value.

Analyzing data is the only way to *reliably* determine functionality. *All* other ways result in functionality that maybe/perhaps/probably/possibly describes the behavior of engineering phenomena.

In science, we recognize that even though we may not understand the data, it is Nature's testimony, and She is an infallible witness. She does not wear her secrets on her sleeve, but She does *not* lie.

The primary importance of data in science is a principle of such long standing that many readers will feel it is "obvious", and that I need not have mentioned it. There are two reasons I have stressed the importance of data:

- In many conventional engineering journals, data is treated as though it has secondary or tertiary importance.

- In all new engineering journals, data is treated as though it has *paramount* importance.

Many readers will likely feel that I am mistaken—that data is of paramount importance in engineering journals. But even a cursory examination reveals that many engineering journals contain very little data.

This will undoubtedly be difficult to believe if you do not have frequent occasion to refer to engineering journals. I encourage you to verify it for yourself—examine conventional engineering journals and try to find data—try to find digital values of the parameters measured in experiments.

Readers who examine conventional engineering journals will find very little data. Since data is *not* important enough to require publication, we must conclude that it has very little importance in conventional engineering journals.

Omission of data from engineering journals is not an oversight. It is the result of a conscious effort to prevent the publication of data. Why would anyone want to prevent the publication of data—the publication of Nature's infallible testimony—the publication of the only thing that definitively establishes parameter functionality?

I asked two editors of engineering journals why their journals contain very little data, and they each replied that there is no room for data in his journal. No room for data!!! There is a great deal of room for discussions/analyses/derivations/theories of little or no importance, but no room for data!

To say that there is no room for data in engineering journals is like saying there is no room for fuel in an airplane. And if there is no room for fuel, of what possible use is the airplane?

In new engineering journals, nothing takes precedence over the data. Data is paramount—data has a reserved seat, and no performance begins until data is seated. Since data is of paramount importance, we have every right to *insist* that data that underlie journal articles be included in the articles, or stored in documentation institutes open to the public.

I once suggested to the editor of a Journal that he *require* authors to include underlying data in articles, or to store the data in a documentation institute open to the public. The editor said he would *not* do that because it would be like requiring authors to testify against themselves, in violation of the fifth amendment!!!

I tried to explain that truth is essential in science—that those who are not prepared to tell the truth have no place in engineering journals. The editor dismissed my argument as idealistic prattle.

If engineering journals are to be scientific, data that underlie journal articles *must* be published in the articles, or stored in documentation institutes open to the public. There is no other way.

If there is no data, there is no science.

Chapter 16

The history of dimensional homogeneity.

16 The 2000 year view of dimensional homogeneity.

For 2000 years, scientists such as Euclid, Galileo, and Newton agreed that, with one exception, dimensioned parameters *cannot* rationally be multiplied or divided. (The one exception is that a dimensioned parameter can be divided by the same dimensioned parameter, resulting in a pure number.) The 2000 year view of homogeneity is reflected in the following:

Algebraically, speed is now represented by a "ratio" of space traveled to time elapsed. For Euclid and Galileo, however, no true ratio could exist at all except between two magnitudes of the same kind. Drake (1974)

Newton did not concern himself with dimensions or units; he merely expressed proportionality according to the custom of his days.
Kroon (1971)

For 2000 years, dimensional homogeneity was achieved by requiring that parametric equations consist of ratios in which the numerator and denominator are the *same* parameter. Such equations are homogeneous because *each ratio* is dimensionless.

Because it was agreed that dimensioned parameters cannot be multiplied parametric equations such as "force equals mass times acceleration" were considered irrational.

16.1 Dimensional homogeneity in Galileo's Theorem VI, Prop. VI.

The following verbal equation is Galileo's Theorem VI, Proposition VI:

If two particles are carried at a uniform rate, the ratio of their speeds will be the product of the ratio of the distances traversed by the inverse ratio of the time-intervals occupied. Galileo (1638)

Galileo's equation reflects the 2000 year view of homogeneity. It includes the ratios speed to speed, distance traversed to distance traversed, and time-interval to time-interval. Because the ratios are dimensionless, they can be multiplied and divided. The equation is homogeneous because it is dimensionless.

Galileo saw no conflict between the concept of speed, and the view that distance *cannot* be divided by time. To Galileo, distance and time were necessary to quantify speed, but speed had nothing to do with *dividing* distance by time. In Galileo's view, and in the view of the science community for 2000 years, dividing distance by time is irrational.

16.2 Dimensional homogeneity in Newton's version of the second law of motion.

Equation (16-1) is generally referred to as Newton's second law. Newton's famous treatise *The Principia* (1726*),* is usually cited.

$$f = ma \qquad (16\text{-}1)$$

However, Eq. (16-1) is *not* the second law of motion conceived by Newton and published in *The Principia*. Because of his view of dimensional homogeneity, Newton would have considered Eq. (16-1) irrational.

Newton's version of the second law is described in the following from *The Principia*:

A change in motion is proportional to the motive force impressed and takes place along the straight line in which that force is impressed.

Note that Newton's version of the second law:

- Is *not* an equation. It is a proportion.
- Is *not* quantitative in an absolute way.
- Does *not* include mass.

Newton's version of his second law is a proportion that conforms to the 2000 year view of homogeneity. His version states:

If some force generates any motion, twice the force will generate twice the motion, and three times the force will generate three times the motion. Newton (1726)

In other words,

$$(\text{Force 1}/\text{Force 2}) = (\text{Motion 1}/\text{Motion 2}) \qquad (16\text{-}2)$$

Note that Newton's version of his second law reflects the 2000 year view of dimensional homogeneity because both ratios in Eq. (16-2) contain the same parameter in the numerator and the denominator.

16.3 Dimensional homogeneity in Ohm's version of Ohm's law.
Equation (16-3) is generally referred to as Ohm's law. Ohm's famous treatise, *The Galvanic Circuit Investigated Mathematically* (1827) is usually cited.

$$E = IR \tag{16-3}$$

However, Eq. (16-3) is *not* the law conceived by Ohm and published in *The Galvanic Circuit Investigated Mathematically*. Ohm's version of Ohm's law is described in the following from his treatise:

. . . the current in a galvanic circuit is directly as the sum of all the tensions, and inversely as the entire reduced length of the circuit . . .

$$I = E/L \tag{16-4}$$

L is the "reduced length of the circuit"—ie *L* is the length of a copper wire of a standard diameter that exhibits the same electrical behavior as a given circuit.

Equation (16-4) is obviously *not* homogeneous. Presumably, Ohm saw no reason to comply with the 2000 year view that parametric equations must consist of ratios in which numerator and denominator are the same parameter, or with Fourier's view that each term in a parametric equation must have the same dimension.

Ohm's inhomogeneous Eq. (16-4) was used for decades, and with excellent practical result, as indicated in the following published three decades after the publication of Ohm (1827):

(Reduced length) is a very convenient mode of expressing the resistance to conductability, presented by the whole or by a part of a circuit, to reduce it to that which would be presented by a certain length of wire of a given nature and diameter. De La Rive (1856).

Note that "a certain length of wire of a given nature and diameter" is a more useful measure of electrical resistance than "ohms" because it is *tangible*, whereas "ohms" is *intangible*. Because Eq. (16-4) is in a more useful form than Eq. (16-3), the dimensional homogenization of Eq. (16-4) in the second half of the nineteenth century was a step down in engineering science.

16.4 Fourier's heat transfer laws.
Two centuries ago, Fourier performed experiments to determine the laws of convective and conductive heat transfer. (In Fourier's day, a law was a parametric equation that is *always* obeyed. Today, a law is a parametric equation that is *sometimes* obeyed, such as Ohm's law.)

Fourier recognized that the heat transfer behavior he induced could be described by quantitative, homogeneous equations *only* if dimensions could be multiplied and divided.

Fourier convinced his contemporaries that, for 2000 years, the science community was *wrong* in maintaining that dimensioned parameters cannot rationally be multiplied or divided. He convinced his contemporaries that it is both *rational and necessary* to multiply and divide dimensioned parameters. It is generally agreed that Fourier (1822a) conceived the modern view of homogeneity, although the modern view differs somewhat diff.

Fourier's famous treatise, *The Analytical Theory of Heat* (1822), presents many of Fourier's contributions to engineering science in general, and to heat transfer science in particular. Fourier described the purpose of his heat transfer research in the following:

Primary causes are unknown to us; but are subject to simple and constant laws, which may be discovered by observation . . .

The object of our work is to set forth the mathematical laws which this element (ie heat transfer) *obeys.* Fourier (1822b)

16.5 Fourier's experiments and results.
Fourier performed comprehensive experiments in forced convection heat transfer to atmospheric air, and in conductive heat transfer. From data he had obtained, Fourier concluded that:

- If an object is cooled by forced convection atmospheric air at a constant velocity, convective heat flux q_{conv} is proportional to boundary layer temperature difference ΔT_{BL}.

$$q_{conv} \propto \Delta T_{BL} \qquad (16\text{-}5)$$

- Conductive heat flux q_{cond} is proportional to temperature gradient dT/dx.

$$q_{cond} \propto dT/dx \qquad (16\text{-}6)$$

Presumably, Galileo and Newton would have been satisfied with Proportions (16-5) and (16-6). But Fourier was not satisfied with proportions. He wanted quantitative, *homogeneous* equations that *always* applied, and would be considered *laws*.

The transformation of Proportions (16-5) and (16-6) to equations results in Eqs. (16-7) and (16-8) in which b and c are *constants*.

$$q_{conv} = b\Delta T_{BL} \quad (16\text{-}7)$$

$$q_{cond} = c(dT/dx) \quad (16\text{-}8)$$

Fourier was not satisfied with Eqs. (16-7) and (16-8) because they are *not* dimensionally homogeneous.

16.6 Fourier's revolutionary view of dimensional homogeneity, and his attempt to validate his view.

Fourier described his revolutionary view of dimensional homogeneity in the following:

. .every undetermined magnitude or constant has one dimension proper to itself, and the terms of one and the same equation could not be compared, if they had not the same exponent of dimensions. (This view of homogeneity) *is the equivalent of the fundamental lemmas which the Greeks have left us without proof.* Fourier (1822a)

In other words, parameters *and constants* in equations have dimension, and the terms in an equation cannot be compared unless *all* terms have the same dimension—ie unless the equation is dimensionally homogeneous. The dimension of a term is determined by multiplying and dividing the dimensions of each *undetermined magnitude or constant,* as described in Fourier (1822c).

Note that Fourier attempts to validate his view of homogeneity by claiming that it *"is the equivalent of the fundamental lemmas* (axioms) *which the Greeks have left us without proof"*. Also note that Fourier does *not* include the lemmas in his almost 500 page treatise, *nor* does he cite a reference to them.

In short, Fourier attempts to validate his view of homogeneity by claiming that unspecified lemmas left by the Greeks are the equivalent of his view of homogeneity, and implying that the unspecified lemmas are scientifically correct, even though *"the Greeks have left us without proof"*, and Fourier provides no proof.

16.7 How Fourier transformed Eqs. (16-7) and (16-8) from inhomogeneous to homogeneous.

Equations (16-7) and (16-8) are inhomogeneous because b and c are constants. But recall Fourier's view that even a constant has "one dimension proper to itself".

. .every undetermined magnitude or ***constant*** *has one dimension proper to itself, and the terms of one and the same equation could not be compared, if they had not the same exponent of dimensions.*
Fourier (1822a)

In Fourier's view, the "one dimension proper to" constant b is whatever dimension will make Eq. (16-7) homogeneous, and similarly for constant c and Eq. (16-8). Fourier arbitrarily assigned the dimensions required for homogeneity to constants b and c, and substituted symbols h and k for symbols b and c. The homogeneous Eqs. (16-9) and (16-10) resulted.

$$q_{conv} = h\Delta T_{BL} \qquad (16\text{-}9)$$

$$q_{cond} = k(dT/dx) \qquad (16\text{-}10)$$

Fourier and his contemporaries accepted Eqs. (16-9) and (16-10) as *laws*—ie as *homogeneous* equations that *always* apply. To Fourier and his contemporaries, Eqs. (16-9) and (16-10) state:

- If heat transfer is by forced convection to atmospheric air, q_{conv} is *always* proportional to ΔT, and h is *always* a constant of proportionality.

- q_{cond} is *always* proportional to dT/dx, and k is *always* a constant of proportionality.

16.8 Fourier's valid claim of priority.

Fourier validly claimed priority for h and k, and for Eqs. (16-9) and (16-10), in the following:

I have induced these laws (ie Eqs. (16-9) and (16-10)) *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.* Fourier (1822d)

Most American heat transfer texts incorrectly refer to Eq. (16-9) as "Newton's law of cooling", and credit Newton with Eq. (16-9) and h. See Adiutori (1990).

16.9 Why Fourier's contemporaries accepted his revolutionary view of homogeneity.

Even though Fourier did not prove the validity of his revolutionary view of homogeneity, his contemporaries accepted his view because his treatise demonstrates a *quantitative* understanding of *both* convective and conductive heat transfer, whereas his contemporaries did not have even a *qualitative* understanding of convective *or* conductive heat transfer.

For example, Biot (1804) expresses the following conclusion about conductive heat transfer:

Thus it is physically impossible to heat to one degree the end of an iron bar of two metres or six feet in length by heating the other end, because it would melt before this.

Fourier explains why Biot's conclusion was not correct:

. . . this result depends on the thickness of the (bar) employed. If it had been greater, the heat would have been propagated to a greater distance . . . We can always raise by one degree the temperature of one end of a bar of iron, by heating the solid at the other end; we need only give the radius of the base a sufficient length; which is, we may say, evident, and of which besides a proof will be found in the (quantitative) *solution of the problem* (given below). Fourier (1822e)

Fourier's knowledge of conductive heat transfer was so complete that he calculated the temperature distribution throughout the bar, whereas his contemporaries did not even know which parameters influence conductive heat transfer.

Fourier explains the critical importance of dimensional homogeneity:

If we did not make a complete analysis of the elements of the problem, we should obtain an equation not homogeneous, and, a forteriori, we should not be able to form the equations which express the movement of heat in more complex cases. Fourier (1822e)

A purist might note that, if a complete analysis of the elements of the problems is not made, an equation will be obtained that does not include all of the important parameters, in which case the equation will not express the movement of heat, even if the equation is homogeneous.

Ohm's law is a case in point. The original Ohm's law, Eq. (16-4), is *in*homogeneous, yet it describes the behavior of Ohm's law conductors as accurately as the dimensionally homogenized version.

In short, homogeneity does *not* prove that an equation is correct, and the lack of homogeneity does *not* prove that an equation is *in*correct.

16.10 How dimensional homogeneity is achieved in conventional engineering.

In conventional engineering, dimensional homogeneity is achieved by stipulating that:

- Parameter symbols in equations represent numerical value and dimension.

- Dimensions may be multiplied and divided. They may *not* be added or subtracted.

- Dimensions may *not* be assigned to constants.

- The dimension of a term in an equation is determined by multiplying and dividing the dimensions of the parameters in the term.

16.11 Why laws such as Fourier's laws of heat transfer, Young's law, and Ohm's law should be considered *in*homogeneous.

It is important to note that in Fourier's view of homogeneity, it was rational to assign dimensions to constants. He homogenized his laws of heat transfer by assigning dimensions to *constants* b and c in Eqs. (16-7) and (16-8).

However, in conventional engineering, dimensions may *not* rationally be assigned to constants. Langhaar (1951) states

Dimensions must ***not*** *be assigned to numbers, for then any equation could be regarded as dimensionally homogeneous.*

Fourier's heat transfer laws should now be considered *in*homogeneous because they resulted from assigning dimensions to *constants,* a violation of the modern view of homogeneity.

Because laws such as Ohm's law were also made homogeneous by assigning dimensions to constants, they too should now be considered *in*homogeneous.

16.12 Why the conventional view of homogeneity is irrational.
The conventional view of homogeneity is irrational because it is based on the following irrational premises:

- Dimensions *can* be multiplied, but *cannot* be added or subtracted.

- Dimensions *can* be divided.

Multiplication is repeated addition. "Multiply six times eight" *means* "Add six eights." Because multiplication is repeated addition, things that cannot be added cannot be multiplied. Therefore it is irrational for the modern view of homogeneity to maintain that dimensions *can* be multiplied, but *cannot* be added.

"Multiply six times eight" *means* "add six eights". Therefore "multiply meters times kilograms" *means* "add meter kilograms". Because "add meter kilograms" has no meaning, it is irrational to multiply dimensions.

"Divide twelve by four" *means* "how many fours are in twelve?" Therefore "divide meters by minutes" means "how many minutes are in meters?" Because "how many minutes are in meters?" has no meaning, it is irrational to divide dimensions.

In conventional engineering, the multiplication and division of dimensions are indicated symbolically. But that does not prove that dimensions can actually be multiplied and divided.

16.13 Dimensional homogeneity in the new engineering.
In the new engineering, parametric equations are *inherently* dimensionally homogeneous because parameter symbols in equations represent numerical value, but *not* dimension.

Chapter 17

My 1964 paper that was accepted for publication in the *AIChE Journal,* but *never* published there. It was published in a Japanese journal in *1994*.

This chapter is an abridged version of a narrative on my website. The unabridged version, and all correspondence and publications referenced in this chapter, can be downloaded without charge for personal use at my website, thenewengineering.com.

17 Summary

In January of 1964, I submitted a paper entitled "Nucleate Boiling—the Relationship Between Heat Flux and Thermal Driving Force" to Professor Editor Harding Bliss (Yale) for publication in the *AIChE Journal*.

The paper states that a widely used induction methodology is *not* rigorous, and describes rigorous methodology. It also states (without proof) that rigorous induction methodology indicates that nucleate boiling data exhibit a *highly linear* relationship between heat flux and temperature difference.

In 1964, it had been generally agreed for several decades that nucleate boiling data exhibit a *highly nonlinear* relationship between heat flux and temperature difference. The nonlinear view *still* prevails in 2017, and is presented in most American heat transfer texts and journal articles.

(In 2002, Professor Rohsenow received the ASME Classic Paper Award for Rohsenow (1952), the paper that presents his *highly nonlinear* nucleate boiling correlation. The correlation is a power law in which the exponent on temperature difference is three.)

In a revised version of my article, rigorous methodology is applied to nucleate boiling data in the literature, and demonstrates that nucleate boiling data exhibit a highly *linear* relationship between heat flux and temperature difference.

The article was favorably reviewed and accepted for publication, and the galley proofs were prepared and sent to me for approval. The most favorable reviewer (Professor Charles P. Costello, University of Washington) stated in part:

This is possibly the most stimulating and exciting article I have ever been asked to review. It will be highly controversial, and people with axes to grind will try to disparage it, but the truth of the author's contentions is unquestionable. . . .

The author has done a great service in pointing out the logical errors often brought about by the use of log-log coordinates . . .

I strongly recommend this paper for publication, and the author is to be commended for his fresh approach to boiling heat transfer . . . [4]

Shortly before the intended publication of the article, the editor of the *AIChE Journal* received a letter from a "responsible person" in which he requested that my article *not* be published. Because of the letter, the article was *never* published in the *AIChE Journal*.

However, *thirty years* after my article was accepted and rejected by the *AIChE Journal*, it was published under a different title in a different engineering journal. The article is "A Critical Examination of the View that Nucleate Boiling Heat Transfer Data Exhibit Power Law Behavior", *Japanese Society of Mechanical Engineers International Journal*, Series B, Vol. 37, No. 2, 1994, pp 394-402.

17.1 Widely used induction methodology that is *not* rigorous.
The paper critically appraises the induction methodology widely used in conventional engineering. The methodology is described in the following:

- Plot the data on log log coordinates.
- Draw a straight line through the data.
- Measure the slope of the straight line.
- Conclude that the correlation is a power law in which the exponent is the slope of the straight line.

---

[4] The complete review is on pages 185-186. I was so pleased by the anonymous, highly favorable review that I asked Professor Editor Bliss (Yale) to tell the author I would like to learn his name and address so that we could correspond. Professor Costello and I corresponded until, under pressure from Professor Editor Bliss who was under pressure from "a responsible person", Professor Costello changed his mind, and *voted against publication!*

17.2 Rigorous induction methodology.
Rigorous induction methodology is described by the following:

- Plot the data on *linear* coordinates.

- Draw a smooth line through the data points.

- Select whatever equation most resembles the smooth line.

- The induced correlation is the equation that most resembles the smooth line.

17.3 The main thrust of the article.
The main thrust of the article is that the methodology in which a straight line is drawn through data plotted on log log coordinates is *not rigorous* because it *assumes without verification* that the data describe a power law—ie describe an equation in the form of Eq. (17-1).

$$q = m\Delta T^n \qquad (17\text{-}1)$$

Note that, if n is greater than zero, Eq. (17-1) includes point (0,0). Therefore, *if* it is known that nucleate boiling requires a *finite ΔT*, one may *not* reasonably assume that nucleate boiling data exhibit the behavior described by Eq. (17-1) because Eq. (17-1) indicates that boiling does *not* require a finite ΔT.

If nucleate boiling *requires* a finite ΔT, one may reasonably assume, subject to verification, that nucleate boiling data exhibit the *displaced* power law behavior described by Eq. (17-2).

$$q = m\Delta T^n + b \qquad (17\text{-}2)$$

It is important to note that, if nucleate boiling data exhibit the behavior described by Eq. (17-2), the slope of a straight line drawn through nucleate boiling data plotted on log log coordinates will *not* equal the value of the ΔT exponent in Eq. (17-1) *or* Eq. (17-2). The slope of the line will differ from the true value of the ΔT exponent by a factor that depends on the values of m, n, and b in Eq. (17-2).

17.4 A nucleate boiling requirement.
For approximately 100 years, it has been known that nucleate boiling requires a *finite* ΔT. Nucleate boiling does *not* occur at $\Delta T = 0$.

Therefore it is *not* reasonable to assume that nucleate boiling data exhibit the behavior described by Eq. (17-1), nor is it reasonable to assume that the slope of a straight line drawn through nucleate boiling data plotted on log log paper is equal to the ΔT exponent in a power law.

17.5 The first version of the paper.
The first version of the paper critically appraises the induction methodology in which data are plotted on log log charts. The paper explains why this widely used induction methodology is *not* rigorous, and describes rigorous induction methodology.

The paper also states, without proof, that if rigorous induction methodology is applied to nucleate boiling data in the literature, it is found that the data generally exhibit the behavior described by Eq. (17-2) in which n is 1, and b is finite. No data is analyzed because I did not wish to embarrass the few researchers who had selflessly published their reduced data in digital form, and had erroneously concluded that the data exhibit highly nonlinear power law behavior.

I expected the paper to be favorably reviewed and accepted for publication because:

- The paper's importance was (and still is) reflected in the widespread use of power laws and log log charts to correlate data.
- The rationale is so simple that the paper can be understood by anyone competent in high school mathematics.
- The conclusions reached are obviously correct.

17.6 Reviewers' comments require that the first version of the paper be revised.
The reviewers failed to comprehend the main thrust of the article, and objected to the lack of proof that nucleate boiling data exhibit linear behavior. For example, Professor Rohsenow (a professor at MIT, and a highly regarded expert on nucleate boiling heat transfer) was one of three reviewers. His negative review stated in part:

I would suggest that the author . . . attempt to show us and himself a comparison of plotting accepted boiling data on log-log and linear-linear paper. . . This might show us something interesting.

I was amazed that Professor Rohsenow:

- Had so much confidence in his view that nucleate boiling data exhibit highly nonlinear power law behavior.
- Had so little intellectual curiosity that he did not have one of his graduate students plot *accepted boiling data on log-log and linear-linear paper.*
- Had so underestimated me that he evidently assumed I had not plotted accepted boiling data on linear-linear paper.

It seems more than likely that, until sometime after he wrote the above review, Professor Rohsenow had *never* seen nucleate boiling data plotted on linear-linear paper.

I rewrote the paper and, as suggested by the reviewers, I included evidence that nucleate boiling data in the literature exhibit linear behavior.

17.7 My correspondence with Professor Rohsenow, and the errors in his letter of April 27, 1964.

In addition to rewriting the paper, I also responded to Professor Rohsenow's review of the first version, and addressed his suggestion that I *attempt to show us and himself a comparison of plotting accepted boiling data on log-log and linear-linear paper*. My letter dated March 1, 1964, states in part:

If you will send me the coordinates of five runs of Berenson's which you will select, I will plot them and send you copies that we may reach a rapport in this matter. . .

(I had sent the letter to Professor Editor Bliss, with the request that he forward it to Professor Rohsenow if he felt it was appropriate for me to respond to Professor Rohsenow's review, and to discard the letter if he felt it was inappropriate. Professor Editor Bliss told me that he forwarded my letter. In the above, Berenson refers to Berenson (1960 and 1962).)

When Professor Rohsenow did not respond to my letter of March 1, I sent him a letter dated March 13, 1964, with carbon copy to Professor Editor Bliss. The letter states in part:

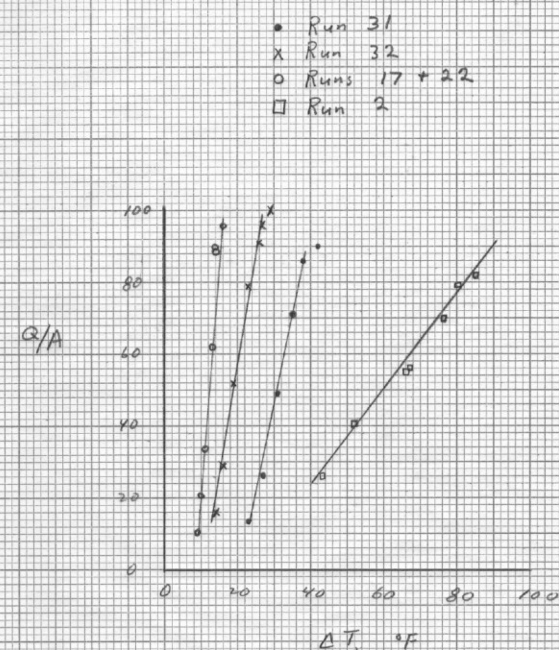
In order that you may be convinced that I used no selection procedure to pick out runs of a certain type, I have plotted the same runs you did on page 119 of your contribution to Modern Developments in Heat transfer by Ibele (1963). *The important thing to note in the enclosed graph is that every point falls within one or two degrees F. of a straight line on linear paper!! Moreover, this one or two degrees is the error Berenson set on the temperature difference alone without counting the error in heat flux!*
(The enclosed graph referred to is on page 172.)

Professor Rohsenow responded in a letter dated April 27, 1964. (The letter and the enclosed graph are on pages 173 and 174.) The letter contains two errors of omission, and one error of commission:

- The letter should have indicated that a carbon copy was sent to Professor Editor Bliss. *No* distribution is indicated.
- The carbon copy that Professor Editor Bliss received should have included a copy of the chart prepared at MIT. It did not.
- The statement *"if you had included all of the points from the graph on page 119 of Modern Developments in Heat Transfer, instead of only those lying in a narrow range"* accuses me of cheating. I did *not* cheat, and I am quite certain that Professor Rohsenow knew I had not cheated.

I had no idea why Professor Rohsenow falsely accused me of cheating, or why he claimed that the chart enclosed with his letter validated his nonlinear view when it validated the linear view.

A letter I received from Professor Editor Bliss dated May 5, 1964 cleared up the matter. The letter stated that he had received a copy of Professor Rohsenow's letter, and chastised me for cheating by selecting data in a narrow range.



Berenson's data; the above runs are plotted on log-log coordinates by Rohsenow on page 119 of Modern Developments in Heat Transfer (1963)

mh 3/13/64

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF MECHANICAL ENGINEERING
CAMBRIDGE 39, MASSACHUSETTS

WARREN M. ROHSENOW
PROFESSOR IN CHARGE
HEAT TRANSFER LABORATORY

April 27, 1964

Mr. Eugene F. Adiutori
Stability Consultants
P. O. Box 18062
Cincinnati, Ohio

Dear Mr. Adiutori:

I am writing in reply to your letter of March 13th and also to that of March 1st, which was forwarded to me by Dr. Bliss.

It was good of you to provide a plot of the points from Dr. Berenson's thesis as I suggested, for now I understand how you were led into error on this question of linearity. It is true, as you say, that the points which you have chosen, although hardly a complete selection of those available to you, do appear to be linear. However, a little thought on the matter discloses the following points. A cubic or even a quadratic equation will appear linear over a short interval, particularly as one gets far from the origin. It is precisely for this reason that log-log paper is employed, i.e., to point out the non-linear relationship even in ranges where it might escape notice on linear paper. Furthermore, if you had included all of the points from the graph on page 119 of Modern Developments in Heat Transfer, instead of only those lying in a narrow range, the non-linear character of these points might have been apparent even on linear paper. I do not feel that I can comment on your "more powerful method" of proving this linearity for I am somewhat vague on just what you mean by an "orthogonal" experiment.

In recommending that your earlier paper not be published by AIChE, I was not objecting to your suggestion that the data be plotted on linear scale, because you are entitled to this opinion and to make this suggestion. Rather, I objected to the form in which you wrote the paper. Your papers tend to be written in a cavalier editorial style. I suggest you shorten your rather lengthy descriptions and use graphs to illustrate your points. Further, you refer to your as yet unpublished works to attempt to convince the reader of certain points.

It is quite likely that many of your observations are worthwhile. I suggest you improve your method of presentation so that your readers can be properly convinced; state your proposition and prove it in a succinct manner without excess verbiage and do avoid gratis comments such as: "Is the boy whistling because he is afraid or ---". These do not add to your logic in any way.

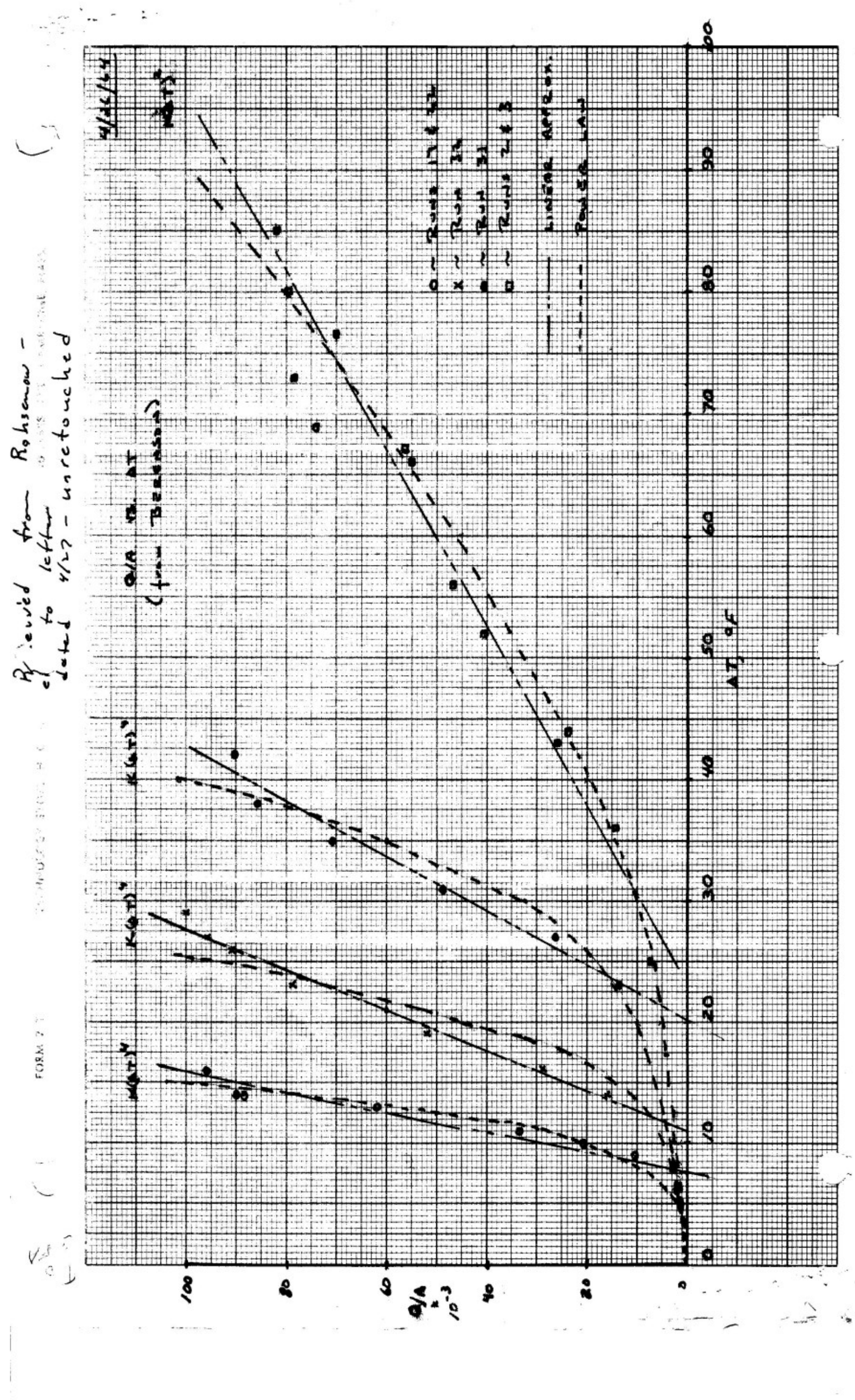
Sincerely,

Warren M. Rohsenow

Warren M. Rohsenow
Professor of
Mechanical Engineering

WMR:hns

P.S. I have enclosed a complete plot of Berenson's data on linear paper and samples of power law curves which are appropriate to the data.



Q/A vs. ΔT
(from Berenson)
O ~ Runs 17 & 32
X ~ Run 24
□ ~ Runs 2 & 3
Linear Approx.
Power Law
ΔT, °F
Q/A x 10^-3

My response to Professor Editor Bliss dated May 7, 1964 (see pages 294-296) states in part:

I feel it was quite unfair of Rohsenow to send you a copy of his letter of April 27th without so indicating on the copy I received. His charge about my selecting the data from a narrow range was without foundation and I was sure he (knew it). . .

I wish to hear no more about the possibility that I judiciously selected the data. You have my graphs, and Berenson's data is clearly presented in the Int Jour Heat etc. I suggest that you have someone check my graphs against Berenson's reported data and let the matter end there. . .

I enclose the graph I got from Rohsenow. (In an earlier telephone call, Professor Editor Bliss confirmed my suspicion that Professor Rohsenow had neglected to send him a copy of the MIT graph.) *I ask you the following question and hope for an answer:*

Can you honestly look at this graph which Rohsenow had made up and not agree that I am correct—that the data do not at all indicate any non-linearity??

Professor Editor Bliss' letter of May 18, 1964 states in part:

I return herewith the graph prepared by Rohsenow, since it is your property. You asked my opinion, and I can state that the points of this graph appear to me better represented by straight lines than by the curved ones.

17.8 The revised version receives two favorable reviews. The article is accepted for publication, and the galley proofs are sent to me for approval.

The revised version was submitted on March 16, 1964. It includes graphs of data presented in Berenson (1962) and in Cichelli and Bonilla (1945). The data by Cichelli and Bonilla was the *same* data Professor Rohsenow used to validate the Rohsenow (1952) correlation for nucleate boiling.

(I had a personal relationship with Professor Bonilla beginning in 1963. When I told him that I had plotted the nucleate boiling data in his 1945 article on linear paper, and the data exhibited highly *linear* behavior, his immediate response was "That's impossible".)

Two reviewers voted for publication, and one voted against publication. Professor Charles P. Costello's (University of Washington) review was the most favorable. It states in part:

This is possibly the most stimulating and exciting article I have ever been asked to review. It will be highly controversial and people with axes to grind will try to disparage it, but the truth of the author's contentions is unquestionable. . .

The author has done a great service in pointing out the logical errors often brought about by the use of log-log coordinates . . .

I firmly recommend this paper for publication, and the author is to be commended for his fresh approach to boiling heat transfer . . .

Professor Seban's (University of California, Berkeley) favorable review states in part:

I support publication because, surprisingly, the work reveals that a Cartesian plot of the results for nucleate boiling does enable a specification of the results by a linear relation that is just as good as the power law representations that are the usual basis for results for boiling of this kind. . .

The third reviewer (anonymous) voted against publication on the amazing grounds that I had presented no experimental data of my own, and because

It is generally accepted that there is no physical reason to assume a linear relationship.

In other words, nucleate boiling data *cannot* describe linear behavior because there is "no physical reason" to *assume* linearity! This convoluted logic concretely demonstrates that this reviewer is simply incompetent.

The fact that I plotted the data on *linear* coordinates did *not* indicate that I *assumed* linearity. I plotted the data on linear coordinates so that the data could describe its own functionality. I was merely reporting the data's description of itself, and explaining why the data had previously been misinterpreted.

In a letter dated April 21, 1964, Professor Editor Bliss accepted my article for publication in the *AIChE Journal*. Several months later, the galley proofs were sent to me for approval.

The title and byline from the galley proofs are on page 187. The body of the galley proofs is on pages 188-192. Note on page 191, just below the Literature Cited section, the statement

Manuscript received March 23, 1964; revision received April 14, 1964, ***paper accepted April 21, 1964. Paper presented at AIChE Boston meeting.***

I had told Professor Editor Bliss that I submitted the paper for presentation at the Boston meeting, and he assumed it would be accepted. It was a reasonable assumption because articles for publication must meet a much higher standard than articles for presentation. It would be unheard of for an article to be accepted for publication, and rejected for presentation.

17.9 I submit the revised version for presentation at an AIChE meeting. In a letter dated August 6, 1964, it is *rejected*.

On April 9, 1964, I submitted the revised version of the paper to Dr. David Miller, an employee of Argonne National Laboratory, for presentation at a symposium on two phase heat transfer to be held at the 57th annual meeting of the AIChE in Boston in December, 1964.

The rejection letter from Dr. Miller is dated August 6, 1964, but I did not receive it until August 18. The letter states:

. . . On the recommendation of three reviewers and my own judgement this paper has been rejected.

It has been difficult to get a thorough review of your paper as several competent reviewers have returned it with the simple, qualitative statement that it was obviously incorrect and should be rejected. However, we bend over backward in the review of a paper which proposes to overthrow any of the universally accepted relationships and I have taken special care in the case of your paper to insure that we are correct in its rejection.

(The data cited in the paper) *represent a narrow range of the temperature difference between the surface and the bulk fluid temperature and, as several reviewers have pointed out, any function can be linearized over a small enough range. However, even the points you have so carefully selected do not prove your point since the data you labled (sic) natural convection can be seen to be boiling data by reference to the original work.*

. . . I suggest you get assistance in preparation of future manuscripts to check the technical content and style before submission.

Any author who attempts to demonstrate incorrectness of well accepted technical relations has a particular obligation to thoroughly document his case and to present it with the maximum of clarity. I suggest you will do more damage than good to your reputation by presenting and publishing work which does not meet high standards of accuracy and of presentation.

Signed by David Miller, Argonne National Laboratory, Papers Chairman

cc R. F Gaertner
Professor Editor Harding Bliss, *AIChE Journal*
Professor Editor S. P. Kezios, *ASME Journal of Heat Transfer*

17.10 My reply to the accusations in Dr. Miller's letter of August 6, 1964.

My letter to Dr. Miller dated August 18, 1964 states:

Thank you for your letter of August 6 in which you express the opinion that I am both incompetent and dishonest. I sense that your mind cannot be changed on this point and that any attempt to do so would prove to be an exercise in futility. . . .

My only standard is simple honesty. If that is not sufficient to place me in good stead, then I prefer to be placed otherwise.

With further regard to your letter, I sense that you have written it for the benefit of Messrs. Bliss and Kezios, and that your advice is intended for their benefit rather than mine. Even so, since I had not requested your advice, it was rather presumptuous of you to proffer it. Therefore, you may forgive me if I choose to disregard your advice and continue publishing my work in an honest and sometimes impolitic manner. . . .

cc R. F. Gaertner
Professor Editor Harding Bliss, *AIChE Journal*
Professor Editor S. P. Kezios, *ASME Journal of Heat Transfer*

17.11 Dr. Miller's holier-than-thou letter dated August 25, 1964.

In a letter dated August 25, 1964, Dr. Miller replied to my letter of August 18, 1964. The letter states in part:

. . . The purpose of informing Professor Editors Bliss (AIChE Journal) and Kezios (ASME Heat Transfer Journal) of our rejection of papers is to minimize the burden on the members of the professional community competent enough and honest enough to review work of others . . .

In other words, he informed the editors of the *AIChE Journal* and the *ASME Heat Transfer Journal* that papers submitted by me should be rejected *without* review in order to "minimize the burden on the members of the professional community competent enough and honest enough to review work of others".

17.12 How Professor Editors Bliss and Kezios replied to Dr. Miller's letter of August 6, 1964.

To the best of my knowledge, neither Professor Editor Bliss nor Professor Editor Kezios ever replied to Dr. Miller's letter of August 6, 1964.

I feel that Professor Editor Bliss should have replied to Dr. Miller's letter. He should have pointed out that the article Dr. Miller rejected had previously received high praise from reviewers for the *AIChE Journal*, and had been accepted for publication.

I also feel that Professor Editor Kezios should have replied to Dr. Miller's letter, and should have said that he neither requested nor desired Dr. Miller's advice with regard to whose work should be rejected without review. He should have said that all papers submitted to his journal would be peer reviewed.

17.13 Professor Editor Bliss receives a negative, unsolicited review from a "responsible person".

The disappointment I felt because of Dr. Miller's rejection letter was nothing compared to the disappointment I was about to feel because of the letter dated August 17, 1964 from Professor Editor Bliss. (See page 297.) That letter gave me an inside view into the manner in which engineering journals are administered. The letter states in part:

I must acquaint you with the fact that I have had a vigorous complaint about my acceptance of (your) *paper. The responsible person making this complaint states that you are wrong . . .*

I have never gone back on my word with regard to an accepted paper . . . However . . . .

The letter explains that Professor Editor Bliss' is going to send the "responsible person's" complaint letter to the highly favorable reviewer, Professor Charles P. Costello, in order to allow him to reconsider his strong recommendation for publication.

Professor Costello's re-review indicated that the "responsible person" was Professor James W. Westwater (head of the chemical engineering department at University of Illinois). It seems to me that, had he truly been a responsible person, he would have included me in the distribution of his letter so that I might address the reasons he felt I was wrong. Professor Editor Bliss never informed me of the reasons cited in Professor Westwater's complaint, perhaps because he recognized that the reasons were not valid.

If the reader will again read Professor Costello's review, I believe he will agree that it has the sound of a freely given, honest, unbiased opinion.

Since he states that "the truth of the author's contentions is unquestionable", and recognizes that "people with axes to grind will try to disparage it", it seems unlikely that a few discouraging words by someone with an axe to grind would cause him to change his vote. I was confident that the paper would eventually be published in the *AIChE Journal*.

17.14 My challenge to Dr. Miller, and his lack of response.

Dr. Miller's letter dated August 6, 1964 found fault with the data points I had "so carefully selected", implying that I had selected the few data points in the literature that indicated linearity. There was no truth in this charge, and I am quite certain he knew it. I had in fact selected the *same* data points that had been used by Professor Rohsenow to validate the widely used nucleate boiling correlation presented in Rohsenow (1952), a nonlinear power law with a temperature difference exponent of three.

I discussed the matter with Professor Editor Bliss, the result of which was his letter of August 24, 1964 to Dr. Miller. The letter states in part:

(Adiutori claims) *that he can prove his point on any data. He volunteers to do this with any data you select. Would you be good enough to pick a reference for him to work with in which tabular data are presented and let him try to prove his point in such data of your selection. It should be remembered that the data Adiutori used to show his linear relationship were exactly those used by Professor Rohsenow to show a non-linear one.*

It is perhaps not necessary to say that Dr. Miller declined the invitation to select the data, perhaps on the grounds that it would serve no useful purpose (for him). At any rate, Professor Editor Bliss never again mentioned the subject of Dr. Miller selecting the data for me to use in another revision of my paper, and he presumably never made the same offer to Professor Westwater.

17.15 Professor Costello's re-review of my paper.

The day of reckoning finally arrived. Professor Costello had completed his re-review of my paper, and had reconsidered his strong recommendation to publish what he described as "one of the most stimulating and exciting articles I have ever been asked to review"—the article in which "the truth of the author's contentions is unquestionable"—the article in which "the author has done a great service"—the article that led to his seemingly unshakeable and unbiased decision:

I firmly recommend this paper for publication, and the author is to be commended for his fresh approach to boiling heat transfer . . .

How did Professor Costello's first thought based on his own experience and judgement compare with his second thought biased by the knowledge that a "responsible person" with "an axe to grind" wanted my article to *not* be published?

Professor Costello's re-review was contained in his letter to Professor Editor Bliss dated August 25, 1964. It stated in part:

Thank you very much for your letter of August 19 and for the opportunity of reappraising the subject paper . . .

I believe Professor Westwater has summarized my feelings best . . .

Needless to say, Professor Editor Bliss apparently felt that the responsible person's request that my article *not* be published, and Professor Costello's negative re-review, were sufficient reason to reject the article he had accepted four months earlier. (The rejection letter is dated 9/7/1964, and is on pages 298 - 299.) I found little solace in Professor Editor Bliss' earlier assurance that "I have never gone back on my word with regard to acceptance of a paper".

17.16 Professor Westwater's motivation to block publication of my article.

The mathematics and rationale in my article are so simple that Professor Westwater *surely* agreed that the induction methodology presented in my paper *is rigorous*, and the nonlinear conclusion is *wrong*.

The article indicates that, over a period of thirty or forty years, Professor Westwater and most researchers reached the erroneous *nonlinear* conclusion because their induction methodology was based on the *assumption* that nucleate boiling data describe power laws. (In conventional engineering, it is tacitly assumed that Nature prefers power laws.)

If they had *not* assumed that the data described power law behavior, the data would likely have been plotted on *linear* coordinates, and it would have been obvious that nucleate boiling data do *not* describe highly *nonlinear* behavior, but describe highly *linear* behavior.

I feel quite certain that Professor Westwater's vigorous complaint, and his unethical request to *not* publish my article, were motivated by the fact that the article would reflect badly on him and the many researchers who reached and promoted the *erroneous* conclusion that nucleate boiling data exhibit highly *nonlinear* behavior.

Although the truth would have been embarrassing, that is not a valid reason for Professor Westwater to block the publication of the truth—not if he had an open mind and a selfless desire to promote progress, traits that should be typical of *all* members of academia.

17.17 My one page ad in *Nucleonics*, and my invited talk given at Professor Wallis' (Dartmouth) 1965 seminar on two phase flow and heat transfer.

I was so disgusted with the acceptance/rejection of my paper, and so anxious to see it published in some manner, that I took out a one page ad for Stability Consultants (my short-lived company) in *Nucleonics.* The ad presented the galley proofs (except for charts) in very tiny letters, and offered to send readers free, complete copies of the galley proofs. Page 187 is the cover on the galley proof copies I mailed to the 26 readers who sent me requests. The galley proofs are on pages 188 to 192.

In 1965, in response to my request, I was invited to speak at a summer seminar on two phase flow and heat transfer given by Professor Wallis (Dartmouth), Dr. Novak Zuber, and Dr. John Collier. I gave a four hour talk on thermal stability and the material in my nucleate boiling article.

17.18 In 1973, I requested that Professor Editor R. C. Reid honor Professor Editor Bliss' 1964 acceptance of my paper.

My letter of October 22, 1973 to Professor Editor R. C. Reid, *AIChE Journal* describes the 1964 acceptance/rejection of my paper, and closes with the following:

My purpose in writing you is to determine whether you would be willing to reopen the question of my accepted but as yet unpublished article. Your editorial in the Jan '73 AIChE Journal seemed to suggest that you would be open-minded with regard to my unique manuscript—particularly your observation that

. . . papers which are truly advances are few and far between. In fact, they are not often even recognized as milestones in the sense that they must, by definition, diverge from the existing mainstream of thought.

Needless to say, Professor Editor R. C. Reid saw no reason to honor Professor Editor Bliss' acceptance of my paper. He suggested that I again submit the paper for review, but I had no interest in another exercise in futility.

17.19 In 1990, I submitted my nucleate boiling article for presentation at the ASME/AIChE National Heat Transfer Conference in 1991. It was rejected.

In 1990, I submitted my accepted/rejected article for presentation at the ASME/AIChe National Heat Transfer Conference in 1991. The rejection letter I received from Professor Ralph Greif (University of California at Berkeley) is dated February 11, 1991. Professor Grief suggested that I revise my article to reflect the comments in the three unfavorable reviews included with his rejection letter.

In my reply letter dated February 27, 1991, I discuss the points made by the three negative reviewers in the following:

- *There is much scatter in boiling data, and a power law is good enough for design.* (Has this reviewer noticed that the standard deviation in the data cited is much less than 1C? Has he actually read the manuscript?)

- *The power law is implied by theoretical models and "fundamental studies" and dimensional analysis.* (Don't these reviewers know that Nature dictates behavior—not models and theories and studies and dimensional analysis?)

- *Demonstrating that the data are linear is not sufficient "to fully refute the power law"*. (This statement is patently ridiculous. Data is both necessary and sufficient to establish behavior.)

- *The linear relationship "may be difficult to generalize in terms of the pressure as the parameter"*. (This would not be difficult, although I can well believe it might be difficult for this reviewer.)

- *The manuscript analyzes the data only over a narrow region, and that is why it can be approximated by a linear relationship*. (This is a polite way of saying I am dishonest. I have in fact analyzed the data over the full nucleate boiling region, but I have not included non-boiling data with boiling data. Surely boiling correlations should describe boiling behavior.)

- In reference to the assumption implicit in drawing straight lines on log log paper, one reviewer states: "*I have never seen anybody apply this constraint"*. This comment demonstrates that this reviewer does not know the meaning of the word "implicit".

The last paragraph of my reply states:

I am sure your advice to revise my nucleate boiling article in line with the reviewers' comments was well intended. But I will not do that because the manuscript in its present form is correct, and I will not make it less than correct. I would rather it went unpublished for another 30 years.

17.20 In 1992, I submitted my nucleate boiling paper to the *Japanese Society of Mechanical Engineers International Journal Series B.* It is accepted, and published!!!

On May 18, 1992, I submitted a paper entitled "A Critical Examination of the View that Nucleate Boiling Data Exhibit Power Law Behavior" to the *Japanese Society of Mechanical Engineers International Journal Series B*. It was essentially the same paper Professor Editor Bliss accepted/rejected for publication in the *AIChE Journal* in1964.

After several revisions, the paper was published. (See Adiutori (1994).) I am reasonably certain that the article has *never* been referenced in an English language periodical. Understandable, in Professor Editor Reid's stated view, because the article *diverges from the existing mainstream of thought.*

Reviews

Review of

NUCLEATE BOILING - THE RELATIONSHIP BETWEEN HEAT FLUX AND THERMAL DRIVING FORCE

by

E. F. Adiutori

This is possibly the most stimulating and exciting article I have ever been asked to review. It will be highly controversial and people with axes to grind will try to disparage it, but the truth of the author's contentions is unquestionable. It is so easy to overlook what the mechanism of plotting points on log-log paper really means that I feel the article should be printed in the *Journal* as a strong reminder to researchers. I can well remember, now that the author suggests it, that most of the data from our laboratory have generated straight lines on linear coordinates and yet my thinking is so accustomed to presenting the curves on log-log paper with slopes close to three that I have been lulled into doing so.

The author has done a great service in pointing out the logical errors often brought about by the use of log-log coordinates.

I think the paper is just about the right length. I feel that it would be helpful if one additional curve were shown, i.e., one which shows the same data plotted on log-log paper but with the additive constant subtracted out. The author's argument on this score is a very strong one and it would help to reinforce it with a graph.

It should not yet be *firmly* concluded that the heat flux is linearly dependent upon ΔT. The data shown certainly indicate that this is so, but since scatter is always present, the true functionality may not be fully revealed. Perhaps the author should use a few more words like "probably" and "apparently" - I have found it has saved me embarrassment at times.

I notice that at the bottom of page 6 the word "figures" appears with the first letter in caps in some places and in lower case in others.

On page 10 it is observed that "... there is only one regime of nucleate boiling ...". Perhaps it should be mentioned that this contention applies only to the heat transfer behavior. Certainly more than a single regime exists as far as the hydrodynamic patterns are concerned and these are also important in some areas (e.g., in asecrtaining density fluctuations in the vicinity of the heater).

I strongly recommend this paper for publication, and the author is to be commended for his fresh approach to boiling heat transfer - a field where the game of "follow-the-leader" is all too often played.

As a matter of policy, I would prefer that my comments should be anonymous.

Nucleate Boiling: The Relationship Between Heat Flux and Thermal Driving Force

EUGENE F. ADIUTORI
Stability Consultant, Cincinnati, Ohio

This article was accepted for publication in the AIChE Journal, but never published. It received the full review procedure, but a complaint from a "responsible person" was responsible for reversing the decision to publish it. This copy was prepared from galley proofs received from the Journal prior to its intended publication.

Notice the following statement on page 4: "*Manuscript received March 23, 1964; revision received April 14, 1964; paper accepted April 21, 1964. Paper presented at AIChE Boston meeting.*" The fact is that neither publication nor presentation were ever permitted.

The primary purpose of this article is to obtain a definitive answer to the following question: how is heat flux related to thermal driving force during nucleate boiling? This question has of course been posed and answered many times in the past and has almost invariably resulted in the conclusion that these two parameters are related in a nonlinear fashion. The relationship most often suggested is that the heat flux is a function of the thermal driving force raised to some power larger than 1. A review of the literature indicates that this result is commonly based on plotting the data on log-log graph paper and noting that the resultant slope is oftentimes a value of 3 or 4. As described below, this approach is somewhat lacking in generality because the use of log-log graph paper demands a tacit assumption about the relationship between the two parameters. Moreover, unless the accuracy of this assumption is verified, the resultant slope may be grossly in error.

The present article takes a more general approach to the question. It analyzes a sample of the data already in the literature and makes no assumptions about the relationship between these two parameters. (It should be noted that the data in the literature have grown to such an extent that it would be impractical to analyze more than a small fraction in a single article. The problem of selecting a sample of data arbitrarily in such a case and demonstrating that the data were not carefully selected is a quite difficult one. In this case, it has been partially solved by analyzing the same data presented by Rohsenow on page 119, Figure 29, of reference *1*. This paper will attempt to determine this relationship by simply plotting the experimental results on linear graph paper and then using the resultant curves to suggest their own relationships.

A more or less general equation relating two parameters is the type given as Equation (1):

$$y = mx^a + b \tag{1}$$

Because of its simplicity the author will first consider an equation of this form where the heat flux is the dependent variable and the thermal driving force is the independent variable as in Equation (2):

$$q/A = M(\Delta T)^a + B \tag{2}$$

If the data suggest an equation of this form, the author will then attempt to simplify this general equation by determining one or all of the constants. However, one may find that one or all of the constants are affected by parameters such as surface condition or pressure and that Equation (2) is already in its most simplified form.

As described below, the analysis results in the realization that the heat flux is essentially linearly related to the thermal driving force during nucleate boiling. This result disagrees not only with the previous data reduction but also disagrees with parts of the present theory of the boiling process. This latter discrepancy will require that certain of the present theory be revised to agree with the experimental evidence.

BACKGROUND

In the past 30 yr. a great amount of research and analysis has been performed on the subject of boiling. A recent summary of this large amount of work is presented by Rohsenow in reference *1*. With regard to the relationship betwen heat flux and thermal driving force during nucleate boiling, he states on page 117:

> Although the slope is predominantly in the neighborhood of 3, observations are available with resulting slopes of as low as unity for contaminated surfaces and as high as approximately 25 for very clean surfaces (*2*).

(In the above quote the word "slope" refers to the slope of q/A vs. ΔT plotted on log-log graph paper.) On this same subject, Rohsenow states on page 125:

> The great increase in heat transfer rate associated with nucleate boiling $(q/A) \sim (\Delta T)^3$ is due to the agitation . . .

In a recent discussion of nucleate boiling, Zuber (*3*) states:

> More than two dozen equations have been heretofore proposed for correlating experimental data . . . . All of them can be put in the form
>
> $$h = \text{const.}\ (\Delta T)^m$$
>
> where the value of the exponent varies between 1 and
> 3 and the . . . .

(It should be noted that if the above expression had been written for q/A rather than h, the value of the exponent would have been 2 and 4.) Zuber also notes that

> . . . variations in the exponent m, ranging from 1 to 25 can be produced by polishing the surface with different grades of emery paper . . .

The above summaries indicate that there is fairly widespread agreement that the heat flux is related to the thermal driving force in a nonlinear fashion in which the exponent on the thermal driving force varies over a very wide range but is usually some value around 3.

In addition to the above empirical correlations, a number of investigators have taken a theoretical approach to determine the relationship between heat flux and thermal driving force. Examples of theoretical treatments are the analyses of Zuber (*3*) and of Levy (*4*). Levy's analysis results in an equation which relates heat flux to the third power of the thermal driving force. Zuber derives an equation relating heat flux to the thermal driving force raised to some power larger than 5/3. (It is not possible to determine the precise value of Zuber's exponent because his expression includes the surface density of nucleating sites as an empirically measured quantity. Since this surface density is positively correlated with the thermal driving force, his derived exponent is always larger than the exponent he derives for the explicit functionality which is 5/3. The fact that part of the functionality between the two parameters is expressed only implicitly of course places a severe limitation on the usefulness of his equation, that is it cannot be used unless one first empirically determines the functionality between this surface density and the other parameters in the system.)

In summary, there has been fairly widespread agreement on both an empirical and theoretical basis that, during nucleate boiling, the heat flux is nonlinearly related to the thermal driving force. This nonlinearity is such that a in Equation (2) would be expected to assume values anywhere from 1 to 25 but would generally be between 2 and 4.

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DATA ANALYSIS

The data analysis begins by simply plotting up some experimental results on linear graph paper such as is shown in Figures 1 through 3. Once the points are plotted and the shape of the curves has become more or less apparent. one must then determine that relationship which seems most strongly suggested by the data. For the ten runs shown in Figures 1 through 3 this is a simple matter because the nucleate boiling portions of the curves strongly suggest that straight lines be drawn through the data. Thus, all ten of the runs suggest that the heat flux is essentially linearly related to the thermal driving force by the relationship

$$q/A = M(\Delta T)^1 + B \tag{3}$$

The fact that the several runs obviously exhibit various values for M and B suggests that those constants are not pure constants and that they are indeed functions of such variables as surface condition, pressure, and the many other variables encountered in boiling systems.

It should be emphasized that the scatter which is always present in experimental data makes it impossible to prove that the relationship is precisely linear; that is one cannot infer that the exponent is 1.000000 simply because the data are well correlated by perfectly straight lines. Rather, the data demonstrates that the exponent is not sufficiently different from unity that the difference can be detected. It therefore seems reasonable to accept the value of 1.0 for the exponent, even though any value between 0.9 and 1.1 would probably fit the data equally well.

Close inspection of Figures 1 through 3 reveals that of the sixty-two nucleate boiling points plotted, virtually every one falls with 2 F. of the straight lines drawn through the data. Moreover, the average deviation from the straight lines is less than 1 F. This amazing precision demonstrates that any nonlinearity in the data is of little practical significance.

It is important to note that the data in Figures 1 through 3 are fairly widespread samples of nucleate boiling data. The data consist of data obtained by two different investigators, in two different laboratories, for two different boiler types, for two different boiler plate materials, for three different fluids, for four different pressures, and for five different surface finishes. The fact that every run exhibits a linear relationship between the variables in spite of the widely different nature of the runs strongly suggests that the author's results are generally applicable.

It may perhaps be more convincing to observe that the data in Figures 1 through 3 would give reasonably straight lines if plotted on log-log paper and that the slopes would indeed be in the neighborhood of 3 or 4. For instance, the same data shown in Figure 3 are also presented by Rohsenow on page 119 of reference *1* as a log-log graph in which straight lines are drawn through the nucleate boiling data points. On this particular graph, the slope of the lines varies from about 2 to 6, seeming to suggest that the relationship is nonlinear in spite of the fact that it is linear as shown in Figure 3. The reason for this seeming anomaly is presented below.

LOG-LOG GRAPH PAPER

It was mentioned above that the use of log-log graph paper involves a tacit assumption about the relationship between the variables of interest. This may be seen by noting that Equation (1) will be a straight line on linear graph paper only if $a = 1.0$ and will be a straight line on log-log graph paper only if $b = 0.0$. Thus, when one attempts to draw a straight line through data on log-log graph paper, one tacitly assumes that b does indeed equal zero. Thus, the result will be quantitatively correct only if one verifies the accuracy of this tacit assumption. This of course is easily done by plotting the data on linear graph paper and extrapolating the data to determine the value of b. If b does not equal zero, the data would indicate the wrong value of the exponent if it were plotted directly on log-log graph paper and the slope were measured. This is why the data in Figure 3 indicate an exponent of 1 on linear graph paper and an exponent of 2 to 6 when plotted on log-log paper. As a result of the finite value of B in Equation (2), the exponents deduced from the slope on log-log paper are incorrect. (Even a cursory glance at Figures 1 through 3 will indicate that B is indeed finite.)

The above is not to say that one is helpless in the face of a finite value of B. In such a case, the variables simply require a minor transformation so that the transformed equation will exhibit a zero value for B. After the data are transformed, the log-log graph would exhibit a slope in agreement with the true value of the exponent. This transformation has been performed on the data in Figure 3, and the results are presented in Figure 4. The transformation has been accomplished by extrapolating the lines in Figure 3 to determine ΔT_o and then subtracting this value from each of the corresponding data points. It should be noted in Figure 4 that the transformed results are satisfactorily correlated by lines drawn with unity slope. Thus, one obtains the same exponent from plotting the data on both linear and log-log graph paper (as desired).

The importance of first performing this transformation is best illustrated by noticing that an exponent of as high as 25 can be deduced from a set of data in which the exponent is actually 1. The safest way to avoid this type of error would seem to be to always plot the data first on linear graph paper.

RESULTS

The obvious result of the above is the realization that heat flux and thermal driving force seem to be linearly related during nucleate boiling and that this result is generally applicable. This will simplify the correlation of this type of data and will somewhat simplify the design of boiling equipment (in the sense that it is more convenient to work with linear correlations than with nonlinear ones).

A less obvious but far more important result is the fact that this linear relationship is in violent disagreement with some of the present theories about the boiling process. For instance, the separate analyses of Zuber and Levy both resulted in a highly nonlinear relationship between heat flux and thermal driving force (as mentioned above). Since this result disagrees with the experimental evidence, it is manifest that the theory on which these analyses were based was incorrect.

It is also interesting to note that Berenson's data is linear with constant slope throughout the entire nucleate boiling region. This strongly suggests that there is only one heat transfer regime of nucleate boiling in this particular data. This therefore tends to disprove the generality of Zuber's (3) theory of bubble interference, since this theory resulted in the conclusion that there were generally two heat transfer regimes in nucleate boiling.

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THEORETICAL DISCUSSION

Up to this point, the discussion has been completely empirical and has been based solely on the measured data. However, it is appropriate to consider whether there is any other basis which would support or explain the unexpected results and perhaps answer the questions: Why is B in Equation (2) finite (that is non-zero)?, and Why is the relationship between heat flux and thermal driving force seemingly linear?

A quite satisfactory answer to the first question can be obtained by first considering the definition of boiling offered by Rohsenow (*1*):

> The process of evaporation . . . results in the conversion of a liquid into a vapor. When this conversion occurs within a liquid, forming vapor bubbles, it is called boiling.

From this definition it follows that the essence of the process called *boiling* is the formation of bubbles within a liquid. Since bubbles in a liquid phase have a natural tendency to collapse owing to surface tension, the boiling process must somehow supply a counterforce of finite magnitude in order to form bubbles against the force of surface tension. It is generally agreed that this counterforce is the result of superheat in the liquid. Therefore, since the counterforce must be of finite magnitude, it follows that the superheat required for boiling must also be of finite magnitude. It is this requirement of finite superheat which causes B in Equation (2) to be of finite magnitude.

In a very real sense, ΔT_o may be considered as the minimum thermal driving force which can sustain boiling. Thus, if one accepts the conclusion that the boiling process requires a finite thermal driving force, one is forced to also conclude that B in Equation (2) is finite. Once one accepts that B in Equation (2) is finite, one is forced to conclude that the slope obtained by plotting the data directly on log-log graph paper is not indicative of the relationship between heat flux and thermal driving force.

A more or less satisfactory answer to the second question can be obtained by first observing that the process of nucleate boiling sharply decreases the resistance to heat transfer. This decrease is generally agreed to be the result of the turbulence created at the boiling interface by the continual formation of bubbles. Therefore one may reasonably conclude that the relationship between heat flux and bubble induced turbulence is what in turn determines the relationship between heat flux and thermal driving force. Therefore one should turn his attention to the following parameters which might be expected to amply define this bubble induced turbulence: specific bubble frequency, that is number of bubbles per unit time per active nucleating site; bubble diameter; and surface density of nucleating sites. By considering the manner in which each of these is affected by heat flux, one will attempt to indirectly infer the relationship between heat flux and thermal driving force.

Perkins and Westwater (*5*) observed that both the bubble frequency and the bubble diameter were essentially independent of average heat flux throughout most of the nucleate boiling region. Thus one must conclude that once an active site begins to nucleate, further increases in average heat flux have no effect on either the specific bubble frequency or the bubble diameter. This absence of effect makes it necessary to conclude that the relationship between heat flux and bubble induced turbulence is determined solely through the surface density of nucleating sites.

Turning now to the surface density of nucleating sites, Adiutori (*6*) demonstrates that the data in the literature generally demonstrates a high degree of linearity in the relationship between heat flux and the surface density of nucleating sites. In addition, it seems reasonable to suppose that this surface density is defined by the thermal driving force and that it is only indirectly related to the heat flux. In other words suppose that

$$\left.\begin{array}{ll} n/A \,\alpha\, \Delta T^{c} & (4) \\ \Delta T \,\alpha\, (q/A)^{d} & (5) \end{array}\right\} \text{transformed to eliminate additive constants}$$

and therefore (by substitution)

$$n/A \,\alpha\, (q/A)^{cd} \tag{6}$$

From the experimental observation that n/A is linearly related to heat flux, one is forced to conclude that

$$cd = 1 \tag{7}$$

Equation (7) therefore confronts one with a choice: either d may assume a wide range of values and the identity is preserved because c always assumes the value $1/d$, or both c and d are generally equal to essentially unity. Since the first choice seems untenable, it would seem that the second choice should be accepted by default. Therefore, since it is concluded that d is generally unity, it is concluded that the heat flux and the thermal driving force should be linearly related. The fact that this conclusion has been reached quite apart from any measurements of thermal driving force tends to support the conclusion and the rational of the linear relation between heat flux and thermal driving force during nucleate boiling.

CONCLUSIONS

The major conclusions resulting from the above analysis are:

1. The nucleate boiling data in the literature do not generally support the contention that the heat flux and the thermal driving force are related in a nonlinear fashion.
2. A reasonably extensive sample of nucleate boiling data taken from the literature suggests that the relationship between heat flux and thermal driving force is essentially linear during nucleate boiling.
3. In general, data should not be plotted on log-log paper unless it is first demonstrated that there is no additive constant such as b in Equation (1).
4. Theoretical considerations suggest that nucleate boiling correlations should include an additive constant in order to account for the fact that boiling requires a thermal driving force of finite magnitude.

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NOTATION

A = area
a, b, c, d = dimensionless constants
B = dimensional constant
h = heat transfer coefficient
m = dimensionless constant
M = dimensional constant
n = number of active nucleating sites
q = heat
T = temperature
ΔT = thermal driving force
ΔT_o = thermal driving force corresponding to zero heat flux (obtained by extrapolating nucleate boiling data)
x, y = unspecified variables

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Fig. 1. Heat flux vs. thermal driving force, *n*-heptane, at several pressures (7).

Fig. 2. Heat flux vs. thermal driving force, ethyl alcohol, at several pressures (7).

Fig. 3. Heat flux vs. thermal driving force, *n*-pentane, for various surface finishes (8).

Fig. 4. Correlation of the data of Figure 3 by first transforming the variables.

MS 6593

Nucleate boiling: the relationship between heat flux and thermal driving force, Adiutori, Eugene F., **A.I.Ch.E. Journal, 10,** No. 5, p. 000 (September, 1964).

Key Words: Boiling-8, Bubble Phenomena-8, Correlating Procedures-8, Heat Transfer-8, Nucleate Boiling-8.

Abstract: It has been widely accepted that during nucleate boiling the heat flux is related to the thermal driving force in a nonlinear fashion. This article examines the experimental evidence which led to this result.

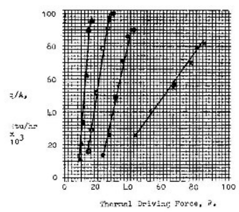
Key:

▲ Run 31: Emery 320

□ Run 32: Emery 60

● Runs 17 &22: Lap E

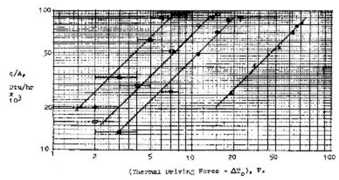
x Run 2: Mirror Finish



(3)

Key: same as Figure 3

Note: All lines drawn with unity slope



(4)

Key:

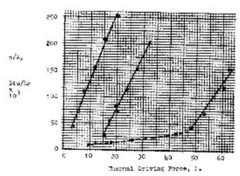
○ 515 psia

x 115 psia

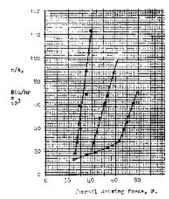
● 14.7 psia

——— Nucleate Boiling

– – – Free Convection



(2)



Key:

○ 50 psia

x 14.7 psia

——— Nucleate Boiling

– – – Free Convection

(1)

Chapter 18

The storm of protest against "New Theory of Thermal Stability in Boiling Systems" published in *Nucleonics* in May of 1964, and my response.

This chapter is an abridged version of a narrative on my website. The unabridged version, and all correspondence and publications referenced in this chapter, can be downloaded without charge for personal use at my website, thenewengineering.com.

18 Summary

"New Theory of Thermal Stability in Boiling Systems" (Adiutori, 1964) was the first published article based on the new engineering. (See pages 285-289.) The article presented the first quantitative understanding of the stability of the heat transfer process—ie thermal stability. It was the first article to present and derive the generic criterion for thermal stability—the first to treat heat transfer as a dynamic process that can and sometimes does result in hysteresis and undamped oscillatory behavior.

Even though the article was at odds with what was generally considered accomplished, scientific fact, I expected heat transfer experts to recognize its validity and first order importance because the rationale was so simple.

My expectation could not have been more wrong. Within a few weeks of publication, the editor of *Nucleonics* received a protest letter signed by *seven* heat transfer experts employed at Argonne National Laboratory.

The protest letter stated in part

"*this article must either be a hoax, or . . . the paper reviewing procedures followed by Nucleonics are in need of reevaluation*".

When the editor surveyed other experts in the field, he found that they

"supported the Argonne criticisms for the most part and took your side only on an occasional point".

The letters section of the December, 1964 issue of *Nucleonics* presented the various criticisms and my reply. My reply should have convinced any competent, unbiased reader that the article was rigorously correct, and that the Argonne group was seriously in error.

The letters section of the October, 1965 issue of *Nucleonics* contained a letter from Professor Graham B. Wallis (Dartmouth College), a recognized expert in the field of two phase heat transfer. The letter was presented under the caption "A Vote for Adiutori".

In his letter, Professor Wallis repeated (without attribution) original and unpublished work I had described in our correspondence, and had presented in a talk I gave at Professor Wallis' summer course on two phase flow and heat transfer.

My article in *Nucleonics* has seldom (if ever) been cited in the literature, and other workers are generally credited with pioneering the original work presented in my article.

However, *in 1989*, an Erratum was published on page 248 of the *ASME Journal of Heat Transfer*, Vol. 111, May, 1989. The Erratum was a communication from Professor John H. Lienhard, and stated *"*

Many of us . . . have credited an important discovery to the wrong authors . . . Stephan, Kovalev, Grassman, and Ziegler. . . To the best of my knowledge, Adiutori (Nucleonics, Vol 22, No. 5, pp 92-101) should be credited . . ."

The erratum has had little impact, and other workers noted in the Erratum are still generally credited with the pioneering work presented in my *Nucleonics* article.

18.1 First contact with *Nucleonics*, and endorsement by Professor Bonilla.

In August, 1963, I went to New York City in the hope of meeting with Detlev J. Raymond, vice president of Pergamon Press, Inc., with whom I had been corresponding with regard to publication of a book on boiling that I proposed to write.

I did not meet with Mr. Raymond, but on August 27, 1963, I met with Jerry Luntz, editor of *Nucleonics*, an international, nuclear energy magazine published by McGraw Hill. I proposed to send *Nucleonics* a short series of articles on the stability of the heat transfer process—a subject I chose to call "thermal stability".

(The term "thermal stability" had not been used in this sense before. It had been used only to describe the stability of materials with regard to temperature. The stability of the heat transfer process was so unheard of that *Nucleonics* suggested that my article include an inset with the title "Thermal stability. What is it?")

I told Editor Luntz that current heat transfer science did not include a quantitative understanding of heat transfer stability, and that heat transfer can and sometimes does result in undamped oscillatory behavior.

Editor Luntz was interested in my proposal, and asked how *Nucleonics* could verify that I was knowledgeable on the subject of stability. I suggested that he phone Professor Charles F. Bonilla, and ask him whether I am competent.

(Professor Bonilla was a department head at Columbia University, and the author of *Nuclear Engineering*, 1957, McGraw Hill. For many years, *Nuclear Engineering* was the "bible" for nuclear engineers. He retired from Columbia University in 1978, and passed away in 1987. Columbia University currently awards the Charles F. Bonilla medal "to that student in the graduating class in the Department of Chemical Engineering who best exemplifies the qualities of Professor Charles F. Bonilla".

Professor Bonilla was familiar with my work because, while I had been working on experimental research on boiling liquid metals in the Space Power and Propulsion Systems (SPPS) department of General Electric, he had been a consultant to that department. We discussed my work on numerous occasions.)

Professor Bonilla later told me that Jerry Luntz called him to ask about me, and he had been happy to tell him that I was quite competent in heat transfer in general and stability in particular.

18.2 Correspondence with *Nucleonics* regarding publication.
The following is a list of the important correspondence regarding publication of my article.

- My letter of August 28, 1963 summarized my proposed series of articles.

- The letter from *Nucleonics* dated August 29, 1963 requested a description of how thermal stability related to oscillations in nuclear reactor coolant flow rate and power level.

- My letter of September 3, 1963 described how thermal stability related to oscillations in nuclear reactor coolant flow rate and power level.

- My letter of September15, 1963 submitted the first of two planned articles.

- The letter from *Nucleonics* dated September 27, 1963 recommended changes to the article submitted on September 15, and suggested that the series of two articles become one article.

- My letter of October 27, 1963 submitted the completed manuscript in the form of a single article.

- The letter from *Nucleonics* dated October 29, 1963 acknowledged receipt of the manuscript, and promised that I would have "full opportunity to review any editing that we do prior to publication".

- The letter from *Nucleonics* dated December 20, 1963 transmitted their edited version of the manuscript.

- My letter to *Nucleonics* dated January 5, 1964 listed my comments on the edited version.

- The letter from *Nucleonics* dated June 17, 1964 indicated that *Nucleonics* had received critical, unspecific comments about my article. It also indicated that *Nucleonics* would not publish the full page ad I had submitted for the July issue until they received specific comments they had solicited from various workers in the field.

- My letter to *Nucleonics* dated August 27, 1964 was a reminder that, in his letter of June 17, the editor of *Nucleonics* had stated "You will be hearing further from me later."

- The letter from *Nucleonics* dated September 9, 1964 transmitted the protest letter signed by seven employees of the Argonne National Laboratory, and the specific comments *Nucleonics* had solicited. The letter from *Nucleonics* stated:

  "Our plan at this point is to publish the Argonne letter in the version we enclose . . ."

- My letter to *Nucleonics* dated September 17, 1964 transmitted my response to the critical comments.

- My letter to *Nucleonics* dated September 25, 1964 was a thank you for publishing both the critical comments and my response.

- The letter from *Nucleonics* dated August 3, 1965 indicated that *Nucleonics* had received a "Letter to the Editor" that praised my article, and stated that the letter would be published in an upcoming issue.

18.3 Unedited version of "New Theory of Thermal Stability in Boiling Systems".

The unedited version of "New Theory of Thermal Stability in Boiling Systems" was an attachment to my letter of October 27, 1963.

18.4 Published version of "New Theory of Thermal Stability in Boiling Systems".

The first paragraph of the published version is written in the first person, whereas the first person is not used in the original version. I felt that *Nucleonics* recommended this change in order to emphasize that the article did not necessarily reflect *Nucleonics'* view. Since such a disclaimer seemed reasonable, I did not object to the change.

18.5 Protest letter from the Argonne seven.

I was notified about the negative response to my article in a letter dated June 17, 1964 from Jerome D. Luntz, editor and publisher of *Nucleonics.* The letter stated:

. . . we have gotten a number of comments from people in the field regarding your article that we carried in the May issue. The letters were critical, but provided no details. I am presently seeking specific comments from the individuals who wrote us.

The "number of comments from people" apparently referred to a protest letter written by seven heat transfer authorities employed at the Argonne National Laboratory.

I received a copy of the protest letter (undated) enclosed with a letter dated September 9, 1964 from Harold Davis, managing editor of *Nucleonics*. The protest letter stated:

Dear (Editor of Nucleonics):
The undersigned, having read "New Theory of Thermal Stability in Boiling Systems" (NUCLEONICS, May 1964, pp. 92-101), ***conclude that this article must be either a hoax, or that the paper reviewing procedures followed by NUCLEONICS are in need of reevaluation****.*

H. K. Fauske, J. B. Heineman, B. M. Hoglund, P. A. Lottes, J. F. Marchaterre, R. R. Rohde, R. P. Stein
ARGONNE NATIONAL LABORATORY

Presumably, the Argonne seven were Ph. D.'s, and therefore were well qualified to conclude that my article was a vast departure from what was the mainstream of thought at that time.

Their unwarranted conclusion that my article "must be either a hoax. . ." reflects bias and/or incompetence, and concretely validates my claim of priority on the subject of what I chose to call thermal stability.

18.6 Purpose of the protest letter.
The real purpose of the protest letter is contained in the phrase

"the paper reviewing procedures followed by Nucleonics are in need of reevaluation".

For the past five decades or so, American engineering journals have largely been under the control of a rather small group of "experts" employed in academia and in national laboratories such as Argonne National Laboratory.

The real purpose of the protest letter was to point out to the editor of *Nucleonics* that only members of the group that controls engineering journals are qualified to determine which technical articles should be published by *Nucleonics*. And to ensure that *Nucleonics* would no longer publish technical articles unless it first received the approval of the group.

18.7 *Nucleonics'* planned response to the protest letter.
In a letter dated September 9, 1964, Harold Davis (the Managing Editor of *Nucleonics*) stated:

Enclosed is a copy of the Argonne letter which you had mentioned hearing about in a recent letter to Jerry Luntz. We have already sent the letter to other people in the field for comment. We also enclose one of the responses—namely, the short discourse by Tong.

We have not included the other responses as we are still checking with the various persons involved to obtain their permission to use their remarks. In sum, however, the half dozen or so people we questioned supported the Argonne criticisms for the most part and took your side only on an occasional point.

Our plan at this point is to publish the Argonne letter in the version we enclose plus Tong's remarks and extremely brief excerpts from the other solicited letters.

We, of course, invite you to add your own comments although I'm sure you understand we must also ask you to be as brief as possible.

Enclosed with the letter from Harold Davis were:

- A copy (undated) of the protest letter from the seven Argonne critics.
- A copy (undated) of a follow up letter from the Argonne group that contained specific comments, but was signed by only four of the Argonne Seven—J. B. Heinman, H. K. Fauske, P. A. Lottes, and B. M. Hoglund.
- A copy of "the short discourse by Tong".

18.8 *Nucleonics'* version of the protest letter.
I was pleased that *Nucleonics* was going to publish the Argonne protest letter "in the version we enclose". Its disdainful tone, and particularly the word "hoax", gave me considerable license to deal with the Argonne critics in a stern manner.

I was dismayed to find that *Nucleonics* did *not* publish the Argonne protest letter "in the version we enclose", and did *not* include the names of all seven authors. Instead, the protest letter and the follow up letter were combined in a single letter signed by only four of the Argonne Seven, and the protest letter portion was revised into the following innocuous statement:

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

The innocuous nature of the published version of the protest letter caused the stern nature of my reply to be inappropriate.

The protest letters, the solicited comments, and my reply, were all published under the title "Thermal Stability View Disputed" in the Letters section of *Nucleonics,* December, 1964. (See pages 290-293.)

18.9 My rejected paper on transition boiling, and my reply in the letters section of the December, 1964 issue of *Nucleonics*.
On March 19, 1964, I submitted "Transition Boiling—the Relationship Between Heat Flux and Thermal Driving Force" to the editor of the *AIChE Journal*, Professor Editor Harding Bliss (Yale). The article is based on the data published in Berenson (1962). (The article is in Appendix 3.)

Berenson (1962) is based on his 1960 Sc. D. thesis at MIT. His experiment was excellent in that he obtained very precise data, and he should be honored for having the courage to publish his data in digital form.

Professor Peter Griffith was Berenson's Sc.D. adviser at MIT, and I naively suggested to Professor Editor Bliss that Professor Griffith be one of the reviewers because he was familiar with Berenson's work. He was in fact one of the reviewers, and he surely recognized the validity and importance of my article. He voted against publication, and the article was rejected.

When *Nucleonics* invited me to respond to the critics of my thermal stability article, I saw a chance to publish a summary of my rejected article. For that reason, my Author's Reply in the "Letters to the Editor" section of the December, 1964 issue of *Nucleonics* includes the following:

. . . Rohsenow states on p. 138 of Modern Developments in Heat Transfer, W. Ibele, ed. (1963*): "With condensing vapor as the heat source on one side of a wall, any point on the entire (pool boiling) curve can be reached under stable conditions."*

My concept of thermal stability demonstrates that this conclusion is wrong and the experiments by Berensen (sic) ("Transition boiling heat transfer from a horizontal surface, Sc.D. thesis, MIT, [1960]; also Int. J. Heat Mass Transfer, 5, 985-999 [1962]) proved it, although apparently neither Berensen nor Rohsenow realized it.

In his experiments, Berensen built just such a boiler as described above, and he reported the results of 20 experiments, each of which purports to cover the entire pool boiling curve.

If the reader will plot Berensen's results on LINEAR graph paper, he will find that there is essentially NO data in the so-called transition region for 17 of the 20 experiments. The reason for this predominant lack of data was that his boiler was usually NOT thermally stable in the transition region. The fact that this was not recognized is doubly important for the following reasons:

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

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

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

Berenson determined the heat flux in the experiment by measuring the temperature gradient in the boiler plate. To improve the accuracy of the measured heat flux, he made the boiler plate very thick, virtually guaranteeing that he would get little data in the transition region.

Berenson obtained boiling curve data for 20 different configurations, and plotted the data on log log paper. He concluded that

The transition-boiling heat-transfer data plotted on log q/A vs log ΔT was found to be correlated by a straight line connecting the maximum and minimum heat flux points.

However, Berenson *should* have concluded that:

- There was essentially *no data* in the transition regions of 17 of the 20 measured boiling curves.

- The reason no data was measured in 17 of the measured boiling curves is because the test boiler was thermally unstable in the transition regions of those boiling curves.

- The reason considerable data was obtained in 3 of the 20 measured boiling curves was because the test boiler was thermally stable in the transition regions of those boiling curves.

- If the transition region data from those 3 boiling curves is plotted on *linear* coordinates, the data from each of the 3 configurations is correlated by a straight line connecting the maximum and minimum heat flux points.

18.10 The originality and importance of my work on thermal stability.

The originality and importance of my work on thermal stability is reflected in the following:

- Professors at MIT, a world class engineering school, did *not* know how to optimize the design of an experimental boiler to operate stably in the transition region.

- Professor Rohsenow (MIT), in *Modern Developments in Heat Transfer,* 1963, W. Ibele ed., page 138, expressed the following *erroneous* view of boiler thermal stability:

  With condensing vapor as the heat source on one side of a wall, any point on the entire (pool boiling) curve can be reached under stable conditions.

- Berenson (1960) and his thesis advisers at MIT did *not* realize that his boiler did *not* able operate stably throughout 17 of the 20 transition regions tested.

- Berenson and his advisers did *not* realize that, in 17 of the 20 pool boiling curves measured, there was virtually *no data* in the transition region because the boiler was not thermally stable.

- On the basis of 17 boiling curves that had virtually *no data* in the transition region, Berenson concluded that, in the transition region, data generally lie on a straight line on *log log* paper.

- Berenson and his advisers did *not* realize that the only boiling curves that had data throughout the transition region were the 3 boiling curves that they disregarded because the data did not describe straight lines on log log paper.

- If Berenson's data had been plotted on *linear* paper, it would have been obvious that:
  - 17 boiling curves had essentially *no* data in the transition region.
  - 3 boiling curves *did* have data throughout the transition region. The data indicated that, in the transition region, the data lie on a straight line on *linear* paper.

One of the reasons functionality in the transition boiling region is of practical importance is because it strongly affects the fuel temperature transient that results from a loss of coolant flow in a liquid cooled nuclear reactor. During the transient, the fuel temperature would increase much more slowly if transition region data describe straight lines on *linear* paper rather than *log log* paper.

18.11 My closing response to the critics of my *Nucleonics* article.
The following is my closing response to the critics of my *Nucleonics* article. It was published in the December, 1964 issue of *Nucleonics* in the letters section under "Author's Reply" (see pp 290-293):

That my concept of thermal stability has not emanated from the universities or the national laboratories is perhaps regrettable. However, it is no less true that it represents new knowledge to the science of heat transfer.

It is now incumbent on the universities and those who control the scientific literature to see to it that this new knowledge is not wasted—and to make it possible for me to publish my work in a less brief and more satisfactory manner. Whether they shall prove equal to the task is a matter for their conscience—not mine.

18.12 "A Vote for Adiutori" in 1965.
The letters section of the October 1965 issue of *Nucleonics* contained a letter from Professor Graham B. Wallis (Dartmouth College), a recognized expert in the field of two phase heat transfer. The letter was presented under the title "A Vote for Adiutori".

In his letter, Professor Wallis should have mentioned that he was present at the 4 hour seminar he invited me to give at his 1965 summer course on two phase flow and heat transfer held at Dartmouth College. In our correspondence before the seminar, and again at the seminar, I presented the stability analysis of Berenson's boiler that Professor Wallis repeated (without attribution) in his letter to *Nucleonics*. (See the letter to Professor Wallis dated June 14, 1965, and also the letter from Professor Wallis dated June 30, 1965.)

18.13 An endorsement in 1989.

My article in *Nucleonics is* seldom (if ever) cited in American engineering journals. However, *in 1989*, an Erratum was published on page 248 of the *ASME Journal of Heat Transfer*, Vol. 111, May, 1989. The Erratum was a communication that recognized my priority with regard to thermal stability. It was from Professor John H. Lienhard (University of Houston), and stated "

Many of us . . . have credited an important discovery to the wrong authors . . . Stephan, Kovalev, Grassman. . To the best of my knowledge, Adiutori (Nucleonics, Vol 22, No. 5, pp 92-101) should be credited …"

The erratum had little impact. Others noted in the Erratum are still generally credited with the pioneering work presented in my *Nucleonics* article.

Chapter 19

The lecture I gave in 1965 at Professor Wallis' Dartmouth College seminar on two phase flow and heat transfer, and the errors and lack of attribution in "Thermal Stability of Surfaces Heated by Convection and Cooled by Boiling", by Hale and Wallis (1972).

This chapter is an abridged version of a narrative on my website. The unabridged version, and all correspondence and publications referenced in this chapter, can be downloaded without charge for personal use at my website, thenewengineering.com.

19 Summary

My correspondence with Professor Wallis began with my letter to him dated October 28, 1963. I had read one of his articles on two phase flow and boiling, and was so impressed by the article that I felt moved to tell him that it was "*by far the most lucid and straightforward article I have encountered* . . ." He replied in a letter dated November 21, 1963, after which we corresponded several times.

In 1965, in response to my request, and on the basis of correspondence in which I presented unpublished, original work on thermal stability, Professor Wallis' letter of June 30, 1965 invited me to lecture at his summer course on two phase heat and fluid flow. The 4 hour lecture took place at Dartmouth on July 9, 1965.

Several weeks after the lecture, Professor Wallis sent a letter to *Nucleonics* in which he repeated (*without attribution*) original, unpublished material from my letters and lecture, and expressed his agreement with my *Nucleonics* article. His letter was published in the October 1965 issue of *Nucleonics* under the title "A Vote for Adiutori".

Professor Wallis' letter of July 8, 1966 enclosed the thermal stability section of his notes for his 1966 summer course on two phase heat and fluid flow. In the notes, he derives a *specific* thermal stability criterion from the *generic* criterion presented in my *Nucleonics* article. I glanced at his notes, then filed them. I did not discover until years later that an error in his derivation resulted in a *specific* criterion that failed to account for the effect of the boiler plate on thermal stability.

In the Hale and Wallis article published in 1972, Wallis' derivation is repeated, the error in the resultant specific criterion is recognized and described, and the error is attributed to a lack of rigor in the generic criterion in my *Nucleonics* article. The article presents a derivation that results in Criterion (24), a *boiler specific* correlation that applies to boilers of the type used by Berenson (1960 and 1962).

Hale and Wallis' Criterion (24):

- Is ***identical*** to Criterion (6) in my letter to Professor Wallis dated June 14, 1965.

- Was presented in my lecture at Professor Wallis' summer course on July 9, 1965, and its application to Berenson's boiler was demonstrated.

- Was the criterion Professor Wallis used to prepare his "A Vote for Adiutori" submitted to *Nucleonics* on or about July 27, 1965, and published in its October, 1965 issue.

To my surprise and dismay, the Hale and Wallis article contained *no attribution* to my letters or my invited lecture at his seminar.

The Hale and Wallis article states

The limitations imposed on Berenson's data by the high wall resistance were first discussed by Wallis in his ("A Vote for Adiutori") *comments on Audiutori's (sic) original paper.*

The above statement is in error. It should state that the limitations imposed on Berenson's data by the high wall resistance were first publicly discussed by me in "Author's reply" published in the December, 1964 issue of *Nucleonics*, and privately in my letter to Professor Wallis dated June 14, 1965, and in my lecture given on July 9, 1965 at Professor Wallis' summer course on two phase flow and heat transfer.

(An article I submitted to the *AIChE Journal* on March 19, 1964 states that thermal stability in the transition region is largely determined by boiler design. The article was rejected, and is in Appendix 3.)

In my letter to the editor of *I & EC Fundamentals* dated August 18, 1972, I requested that he publish my enclosed "letter to the editor" that corrected errors and described the lack of attribution in the article by Hale and Wallis. Predictably, the editor, Professor Pigford (University of California at Berkeley), saw no need to publish my letter.

19.1 Correspondence that resulted in an invitation from Professor Wallis to lecture at his summer course on two phase flow and heat transfer held at Dartmouth College.

In May of 1965, I received a flyer from Professor Wallis announcing a two week summer course on two phase flow and heat transfer to be given at Dartmouth by Professor Wallis, Dr. Novak Zuber, and Dr. John Collier. My letter to Professor Wallis dated May 21, 1965 indicated that I did not wish to enroll in his course, and made the following offer:

Sometime near the end of the two weeks, I would be pleased to present one or two one hour lectures dealing with the results of my conceptual research on two-phase heat and mass transfer. . . . (I) offer to present the lectures without fee and at my own expense.

The reason I offered to lecture "*without fee and at my own expense*" is because I had tried to present and publish the application of the theory presented in my *Nucleonics* article, but had been prevented from doing so by reviewers and editors who (I feel) rejected my work because they correctly sensed that it was revolutionary, and that I was not a member of the club. As a result, I was extremely anxious to present the results of my work to an audience competent in two phase flow and heat transfer, such as persons who completed Professor Wallis' course.

Professor Wallis' reply dated June 9, 1965 stated:

There is a possibility that we might fit you in as a "special topic" on the last day of the course. Before deciding one way or another, I will need to consult with Collier and Zuber about the amount of time they may be able to give up to make room for you.

Also, as I am not familiar with your work, I find it hard to find a basis for a rational decision. ***Perhaps you can supply me with more evidence which will convince me.*** *I will try to be quite objective and not be influenced either by your own opinion of your work or the opinions of those who have resisted its acceptance.*

My letter to Professor Wallis dated June 14, 1965 was my response to "*supply me with more evidence which will convince me*". (See pages 300-303.) The letter presents my view on the relationship between heat flux and interface temperature difference in nucleate boiling, and applies the thermal stability theory presented in my *Nucleonics* article to the boiler used in "Experiments on Pool-Boiling Heat Transfer" by P. J. Berenson, *Int J Heat Mass Transfer*, v 5, pp 985-999, 1962. The letter gives the generic criterion for thermal stability, then states:

For a boiler such as Berenson used for his thesis, this general form may be reworked into the particular form

$$-(1/h_s + t_w/k_w)^{-1} < s \;\; dq/dT_w \qquad (6)$$

where the right hand side of equation (6) of course refers to the slope of the pool boiling curve. From this criterion, it may easily be seen that:

1. The statement that such a boiler is necessarily stable at all points of the pool boiling curve is simply not true.

2. The stability of such a boiler can be improved markedly by simply increasing the steam side coefficient, decreasing the thickness of the wall, or increasing the thermal conductivity of the wall. . . . (Berenson) intentionally made the boiler plate very thick in spite of the fact that he wanted the boiler to be as stable as possible since his thesis subject was transition boiling.

It is important to note that although Criterion (6) is quite simple, it was original. It was the result of the first application of my theory of thermal stability—the first thermal stability criterion derived for a specific type boiler. Nothing like it had ever appeared in the literature, although I had tried unsuccessfully to arrange its publication. (I presented it at an Open Forum held at the 7th National Heat Transfer Conference, August 11, 1964, Cleveland, Ohio.)

Professor Wallis' letter of June 30, 1965 (See page 304.) states:

Thank you for your letter of June 14th.

I would be glad to have you give a lecture on July 9th in the afternoon on a subject of your choice. . . .

. . . I agree with your analysis and comments on Berenson's work which seems quite obvious once you have pointed it out.

(Compare Professor Wallis' "*seems quite obvious once you have pointed it out*" with the Argonne comment "*The most consistent fundamental error throughout the paper treats temperature as an independent variable*". In other words, the Argonne critics considered it irrational to take derivatives with respect to temperature because temperature is not an independent variable. Therefore, in their incompetent and/or dishonest view, the "obvious" Criterion (6) was irrational and of no use.)

Berenson's article was derived from the Sc. D. thesis he submitted in 1960 at MIT. The thesis adviser was Professor Peter Griffith. The first sentence of the thesis abstract states:

An experiment, utilizing a condensing fluid as the heat source, was performed to determine the heat flux vs. temperature difference curve for transition pool boiling from a horizontal surface.

19.2 The lecture I gave on July 9, 1965 at Dartmouth.
My lecture at Dartmouth on July 9th is described in my lecture notes. The first part of the notes is in the form of a manuscript. The second part of the notes is in the form of an outline.

The lecture began with a review of the graphical, conventional view of thermal stability, and a description of what is wrong with the conventional view. The general criterion for thermal stability is given without derivation in the form

$$dQ_{in}/dT_w \ <s \ dQ_{out}/dT_w \qquad (7)$$

in which Q is heat flow rate (not heat flux). (I usually express the general criterion in terms of heat flux rather than heat flow rate. In this case, I used heat flow rate because I feel that the criterion is easier to comprehend if it is first presented in terms of heat flow rate rather than heat flux.)

Criterion (7) must be satisfied at all points within the wall. However, if it is satisfied at the boiling interface, it is satisfied at all points in the wall. The notes state:

The criterion results from idealizing a generalized system and uncoupling it at the boiling interface. The uncoupled pieces are then analyzed to determine whether they would fit together in a stable manner. The derivative on the left hand side . . . answers the question "How is the heat flow into the boiling interface affected by the temperature of the interface?" The right hand side . . . answers the question "How is the heat flow out of the boiling interface affected by the temperature of the interface?"

A boiler of the type used by Berenson is described, and its stability analysis is described analytically and graphically. It is shown that, for such a boiler,

$dQ_{in}/dT_w = -A/(1/h_s + t/k)$ (8)

where A is the area of the boiler plate. Thus the stability criterion for this boiler type is given by Criterion (9). Note that if both sides of Criterion (9) are divided by A, the result is Criterion (6) in my letter to Wallis dated June 14, 1965.

$-A/(1/h_s + t/k) <_s dQ_{out}/dT_w$ (9)

Berenson's transition region article is discussed, and the lack of transition region data noted in the following:

If you will plot up some of (Berenson's) data on linear graph paper, you will find that, in 17 of the 20 experiments, virtually no data was obtained in the transition region, in agreement with the new theory of thermal stability.

With regard to the design of a boiler such as the one used by Berenson, the notes state:

. . . the designer can exert a very strong influence on the thermal stability of the boiler in any of the following simple ways:

1. *Minimize the thickness of the boilerplate*

2. *Maximize the thermal conductivity of the boiler plate.*

3. *Maximize the heat transfer coefficient of the fluid condensing on the heat source side of the boiler plate.*

The phenomenon of burnout (ie a large increase in temperature resulting from an incremental increase in heat flux) in so-called constant heat flux systems such as nuclear reactors and electrically heated boilers was discussed, and it was pointed out that burnout does not necessarily result in such systems.

For example, if the temperature coefficient of reactivity of a nuclear reactor were sufficiently negative, the stability criterion would be satisfied at all values of the boiling interface temperature, and there would be no burnout—ie no step increase in fuel temperature.

The manner in which thermal instability can result in undamped, oscillatory behavior was described.

The so-called dry wall phenomenon was discussed, and it was suggested that it is thermally induced rather than hydraulically induced. Hydraulic stability and its resemblance to thermal stability were discussed.

I thoroughly enjoyed giving the lecture, and sensed that it was well received. I was honored by Dr. John Collier's request for my autograph.

19.3 Professor Wallis' letter to *Nucleonics* published in the October, 1965 issue under the title "A Vote for Adiutori".

Several weeks after my lecture, I received a letter from Professor Wallis dated July 27, 1965 in which was enclosed a copy of a "letter to the editor" he had sent to *Nucleonics.* The "Letter to the Editor" had obviously been prepared using (*without attribution*) original, unpublished material on thermal stability in my letters to Professor Wallis, and in my lecture of July 9, 1965.

The "Letter to the Editor" discussed Berenson's boiler , and expressed agreement with my *Nucleonics* article. The letter was published in the October 1965 issue of *Nucleonics* under the title "A Vote for Adiutori".

I was dismayed that Professor Wallis' "letter to the editor":

- Did *not* state that it was based on the unpublished stability criterion in my letter to Professor Wallis dated June 14, 1965, and in my talk at his summer course on July 9, 1965.

- Did *not* state that he had applied my criterion to Berenson's boiler in the manner I had described in our correspondence, *and* in my talk at his summer course on July 9, 1965.

- Did *not* state that I had shown that Berenson's boiler was poorly designed in that it had a very thick boiler plate, virtually guaranteeing he would obtain little data in the transition region.

However, I was so elated that Professor Wallis had had the courage to publicly endorse my work that I made no effort to have the lack of attribution corrected.

I certainly did not anticipate that Professor Wallis would, years later, present my Criterion (6) as his own original work, or that he would claim to be the first to consider the effect of boiler plate thickness on thermal stability, both of which he did in “Thermal Stability of Surfaces Heated by Convection and Cooled by Boiling” by Hale and Wallis, *I & EC Fundamentals*, Vol. 11, No. 1, 1972.

19.4 The fundamental error in Professor Wallis’ notes for his 1966 summer course on two phase heat and mass transfer.

Professor Wallis’ letter of July 8, 1966 enclosed the section dealing with thermal stability in his lecture notes for his 1966 summer course on two phase heat and mass transfer. Because Professor Wallis did not request that I verify the correctness of his lecture notes, I did not feel the need to carefully examine them. For that reason, I merely glanced at his notes, then put them away.

In preparing this narrative many years later, I examined Wallis’ lecture notes more carefully, and was amazed to find the following in Section 14.6.5 entitled “Pool Boiling From a Surface Which is Heated by Convection (Perhaps Condensation) from a Fluid at Constant Temperature ”, the boiler type used by Berenson.

The stability criterion now becomes

$$-h_1 - m_b < 0 \qquad (14.108)$$

Thus a negative value of m_b can be counteracted by having a suitably large heat transfer coefficient h_1. This was used experimentally by Berenson (15) and Owens (29).

(h_1 is the heat transfer coefficient at the *heated* interface of the boiler plate, and m_b is $dq/d\Delta T$ at the *boiling* interface.)

Criterion (14.108) states that, for the type boiler used by Berenson, the thickness and thermal conductivity of the boiler plate have *no effect* on thermal stability.

This violently disagrees with my “Author’s Reply” in the December, 1964 issue of *Nucleonics*, my letter to Professor Wallis dated June 14, 1965, and my lecture at Professor Wallis’ summer course on July 9,

1965. It also disagrees with Wallis' "A Vote for Adiutori" in which he stated that boiler plate thickness greatly affects thermal stability.

Note that Professor Wallis did *not* use Criterion (14.108) to calculate the results in "A Vote for Adiutori". He used Criterion (6) in my letter to him dated June 14, 1965.

$$-(1/h_s + t_w/k_w)^{-1} < s \ \ dq/dT_w \qquad (6)$$

19.5 The errors and lack of attribution in the article on thermal stability by Hale and Wallis published in *I & EC Fundamentals* in 1972.

The article by Hale and Wallis claims to demonstrate that

$$dq_{in}/dT_w < dq_{out}/dT_w \qquad (1)$$

(the general criterion for thermal stability presented in my *Nucleonics* article) . . . *is valid only if the thermal resistance of the wall is neglected.*

This statement is simply *not true*. What the article by Hale and Wallis actually demonstrates is that if the derivatives in Criterion (1) are not evaluated correctly, the result will be a criterion that is incorrect.

The correct evaluation of the derivatives in Criterion (1) is quite simple. It requires only that both derivatives be evaluated at the boiling interface. (Note that I covered this on page 8 of my lecture notes for Wallis' 1965 summer course.)

Hale and Wallis evaluated one of the derivatives in Criterion (1) at the *heated* interface, and evaluated the other derivative at the *boiling* interface. Of course the result they obtained was incorrect. They erroneously concluded that Criterion (1) is incorrect, but they should have concluded that their application of Criterion (1) was incorrect.

The article by Hale and Wallis *erroneously* alleges that, for a convectively heated wall, Eq. (5) results from application of Criterion (1).

$$h_{in} > - dq_{out}/dT_w \qquad (5)$$

The article also states

Wallis and Collier (1967) applied eq 1 to several different design situations . . .

In other words, in the notes for their 1967 summer course at Stanford, Wallis and Collier appraised the stability of convectively heated boilers using Criterion (5), a criterion that erroneously indicates the boiler plate thickness and thermal conductivity have *no effect* on thermal stability. This same erroneous criterion had previously been presented in Professor Wallis' notes for his 1966 summer course at Dartmouth.

The Hale and Wallis (1972) article indicates that, sometime between 1967 and 1972, Hale and Wallis concluded that Eq. (1) is incorrect. They then used alternative methodology that resulted in their Criterion (24) which correctly states that boiler plate thickness and thermal conductivity have a *strong effect* on stability.

Criterion (24) in Hale and Wallis (1972) is *identical* to Criterion (6) in my letter to Professor Wallis dated June 14, 1965. Criterion (24) is presented as original work in Hale and Wallis (1972), and there is no attribution to my correspondence with Professor Wallis, or to my lecture at Professor Wallis' summer course in 1965.

Amazingly, the Hale and Wallis article also states

Criterion (24) was used by Wallis (1965) in analyzing Berenson's data.

This statement is amazing because it raises the questions:

- How could Professor Wallis apply Criterion (24) in 1965 when he could not deduce it in 1966 or 1967?

- If Criterion (24) cannot be deduced from Criterion (1) as Hale and Wallis (1972) claim, how did Professor Wallis deduce it in 1965?

The Hale and Wallis (1972) article also states:

The limitations imposed on Berenson's data by the high wall resistance were first discussed by Wallis in his comments on Audiutori's (sic) original paper.

The above statement refers to Wallis' comments published under the title "A Vote for Adiutori" in the October, 1965 issue of *Nucleonics*. The statement is *not true*. In fact, the limitations imposed on Berenson's data by "high wall resistance" were discussed in the following, all of which preceded publication of "A Vote for Adiutori".

- My "Letter to the editor" published in *Nucleonics* in December, 1964.

- My letter of June 14, 1965 to Professor Wallis,

- My July 9, 1965 talk at Professor Wallis' summer course at Dartmouth.

19.6 My unsuccessful attempt to correct the errors and lack of proper attribution in Hale and Wallis (1972).

In a letter dated August 18, 1972 to the editor of *I & EC Fundamentals*, I requested that he publish my enclosed "Letter to the Editor" that corrected errors in the Hale and Wallis article, and cited the lack of proper attribution.

Predictably, the editor (Professor Pigford, University of Delaware) saw no reason to publish it.

19.7 Why Hale and Wallis (1972) made me unhappy.

Hale and Wallis (1972) made me very unhappy because:

- The stability criterion that was the main thrust of their article was my original work that I had shared with Professor Wallis and those who attended his two week seminar in 1965.

- I had been unable to arrange the publication of the stability criterion that Hale and Wallis (1972) presented as their original work.

- It was my guess that Hale and Wallis had no difficulty arranging the publication of my original work. It is difficult to arrange the publication of anything that has my name on the byline.

Chapter 20

Writing and marketing the first book about the new engineering.

This chapter is an abridged version of a narrative on my website. The unabridged version, and all correspondence and publications referenced in this chapter, can be downloaded without charge for personal use at my website, thenewengineering.com.

20 Deciding to write a book about the new engineering.

In 1973, I decided to publish abstracts of my many rejected papers in the form of ads for Stability Consultants, an unsuccessful company I started. I contacted the ASME sales department, and they sent a salesman (I am reasonably sure his name was Harry Lenhardt) to see me.

I told Harry I wanted to place ads in *Mechanical Engineering* and *Journal of Heat Transfer*. He said he could sell me space in *Mechanical Engineering*, but he could not sell me space in *Journal of Heat Transfer* because it only accepted ads for books. I told Harry to sell me space in *Journal of Heat Transfer* because I was going to write a book.

I wanted to write a book about the new engineering, but I was reluctant to use the title *The New Engineering* because I was not widely regarded as an expert in any field. My published work consisted of two articles on heat transfer, both of which had been poorly received, and were almost ten years old. It seemed more than likely that a book entitled *The New Engineering* would seem so grandiose that I would be considered a crackpot, and the new engineering would be ignored.

For that reason, I decided that the first book on the new engineering would be *The New Heat Transfer*. Heat transfer is widely regarded as an art rather than a science, and therefore a new heat transfer would seem much less grandiose than a new engineering.

Also, I could better demonstrate the application of the new engineering to heat transfer because of my twenty years of practical experience in high-tech heat transfer in the fields of nuclear reactors, space power, aircraft gas turbines, and fossil fueled power plants.

20.1 Selling *The New Heat Transfer* before writing it.
I had little confidence that I could sell a book about the new heat transfer, particularly a book written by someone as unknown to the public as myself. I was reluctant to write the book if no one was going to buy it, so I decided to test the market before writing the book.

I tested the market by placing ads in engineering journals and trade magazines. The ads stated that I was planning to write *The New Heat Transfer*, and as each chapter was completed, it could be purchased for $1.95. When the book was finished, persons who had purchased all the chapters would receive a complimentary hard copy of the book.

My intent was to decide whether or not to write the book based on the response to the ads. If there were only three or four orders for Chapter 1, I would not write the book, and would simply return the money to those who had ordered Chapter 1, along with an explanatory note and an apology. If there were considerably more than three or four orders, I would write and publish the book.

20.2 The first ad for *The New Heat Transfer.*
In March of 1973, I sent copy for the first ad to ASME's *Journal of Heat Transfer*, *International Journal of Heat and Mass Transfer (IJHMT)*, and AIChE's *Chemical Engineering Progress (CEP)*.

The first ad appeared on page 56 of the July, 1973 issue of *CEP*, and on pages vi and vii of the August, 1973 issue of *IJHMT*. The ad states that heat transfer coefficients are abandoned in the new heat transfer, and briefly described why they should be abandoned. The ad also states that the book is being in written in chapters, chapters can be purchased for $1.95 each, and persons who purchase all the chapters will receive a complimentary bound copy of the book when it becomes available.

20.3 The ASME *Journal of Heat Transfer* would not accept ads for *The New Heat Transfer.*
In 1973, the ASME Heat Transfer Division Executive Committee (ASMEHTDEC) would not allow ads for *The New Heat Transfer* to appear in the *Journal of Heat Transfer*.

Copy for the first ad had been submitted to the ASME in March, but in November, the ASMEHTDEC still would not allow ads for *The New Heat Transfer* to appear in the *Journal of Heat Transfer*. A letter from Professor Sparrow dated November 20, 1973 stated:

In general, the Executive Committee felt that there is a real possibility that an advertisement for your book can ultimately be published in the Journal of Heat Transfer, pending certain matters of clarification and revision.

The letter also noted that the ASMEHTDEC required that several hoops be jumped through, and that it be allowed to revise my ads prior to publication.

My tongue in cheek reply dated November 30, 1973 stated that I would jump through the required hoops. I would also allow the ASMEHTDEC to revise my ads *provided* each ad they edited would state that it had been edited by the ASMEHTDEC, and would also cite a reference where the unedited ad could be found.

I kept negotiating with the ASMEHTDEC in the hope that it would begin to allow ads for *The New Heat Transfer* to appear in the *Journal of Heat Transfer*. The ASMEHTDEC at first allowed only ads that contained no technical content. However, it soon relented, and for many years ending in 1993, ads with technical content appeared on the back cover of every issue of the *Journal of Heat Transfer*. I placed the ads in the *Journal of Heat Transfer* not because they were cost effective, but because they belonged there.

In 1993, it was decided that the ASME *Journal of Heat Transfer* would no longer accept ads from anyone. (Since my ads were essentially the only commercial ads that appeared in the *Journal of Heat Transfer*, this was a clever way of stopping my ads with de facto bias.)

20.4 Response to the first ad for *The New Heat Transfer.*

I was astonished by the first order for Chapter 1. Over the years, I had met such stubborn resistance from professors who controlled the engineering media that I expected to receive no orders from professors.

Amazingly, the first order for Chapter 1 was from a professor—Professor John A. Clark, Chairman of the Mechanical Engineering Department of the University of Michigan!! Note the friendly and encouraging tone of Professor Clark's hand written order dated 7/20/1973. (See page 305.)

(Professor Clark's order initiated our correspondence. See my first letter to Professor Clark dated August 6, 1973, and Professor Clark's first reply dated August 10, 1973. Note that my letter of August 6 states that Chapter 1 will not be completed for four more weeks.)

The response to the first ad was greater than I had expected. By the middle of September, I had received 34 orders for Chapter 1. They are still in my files.

I decided to write *The New Heat Transfer*.

20.5 The advertising plan: 48 pages of ads in *IJHMT*, and an indefinite number in *CEP.*

Because the response to the first ad was so favorable, I decided to advertise the book extensively. Toward that end, I contracted with *IJHMT* for two pages of advertising in each of 12 issues beginning December, 1973. (See my letter to PCI dated August 21, 1973, and PCI's acknowledgement dated August 23, 1973. PCI was the advertising agency for *IJHMT*.)

In my letter to PCI dated November 6, 1973, I contracted for two pages of advertising in another 12 issues, making a total of 48 pages of advertising in 1974 and 1975 issues of *IJHMT*. Note that the copy for an ad that was to appear on the first 2 pages of the March, 1974 issue states:

This is the first in a series of ads describing The New Heat Transfer. The ads will appear on the first two pages of this Journal throughout 1974 and into 1975. The New Heat Transfer may be obtained ($19.95) from: THE VENTUNO PRESS, Box 40321, Cincinnati, Ohio 45240.

I also sent ad copy to *CEP* for the inside back cover of the October, 1973 issue, the second in a series of an indefinite number of ads to run in *CEP*. (The same ad also appeared in the February 18, 1974 issue of *Chemical Engineering*.)

The editors of the *IJHMT* disliked my first ad. The *IJHMT* refunded my money, and refused to honor our contract!

On December 11, 1973, Professor J. P. Hartnett, University of Illinois, informed me by telephone that the entire editorial board of *IJHMT* (most of whom are prestigious professors at world class universities) *unanimously* agreed that the proposed series of ads for *The New Heat Transfer* were "unprofessional" and should be discontinued. (See my letter to Professor Spalding, Imperial College of Science & Technology, dated December 11, 1973.) Professor Hartnett also informed me that the money I had paid for the ad in the August, 1973 issue of *IJHMT* would be refunded!!

Apparently, the editors of *IJHMT* did not see my ad until after it was published. I had submitted copy for the ad to their agent, PCI, in the normal manner. PCI had correctly noted that there was nothing objectionable in the ad, and therefore they had not bothered to have it approved by the editors.

IJHMT refused to honor our contract for additional pages of advertising. And the money I had paid for the first ad was in fact refunded!

However, Professor Spalding's letter of 12/17/73 indicated that *IJHMT* would accept my ads provided they contained no technical content. My letter to Professor Spalding dated 12/26/73 stated that I wished to place such an ad in 6 consecutive issues of *IJHMT*.

(As described below, in 1986, the Editorial Board of *IJHMT* approved a series of ads that contained technical content. For several years, an ad for *The New Heat Transfer* appeared in every issue of *IJHMT*. Then in 1990, the Editorial Board decided to "not accept any additional advertising for *The New Heat Transfer*".)

20.6 Writing *The New Heat Transfer.*

I wrote Volume 1 of *The New Heat Transfer* in 1973. The book is in 3 volumes published in 1974, 1975, and 1976. I wrote the book on what at that time was state of the art equipment—an IBM Selectric typewriter, the first typewriter with erasing capability. It had a tape that lifted characters, and left no trace except for an indentation in the paper.

When I started writing Chapter 1, I was afraid that what I had to say would not fill a book of reasonable size. For that reason, the lines in Chapter 1 are double-spaced. After completing Chapter 1, I was less apprehensive about filling a book, so I changed the line spacing in

Chapter 2 to 1½ spaces. After completing Chapter 2, I was not at all concerned about filling a book, and the lines in the remaining chapters are single-spaced.

The schedule I set and maintained was to complete a chapter every two weeks, including having the chapter printed, and mailing copies to subscribers. If a chapter was completed in less than two weeks, as they generally were, I did not start the next chapter until the two week period was over.

One day while I was writing Chapter 2 or 3, a reader called to ask how many chapters *The New Heat Transfer* would contain. Since I had not made an outline of the book, I had no idea how many chapters the book would contain. But I was embarrassed to admit my error of omission, so I made an instant decision—I told the caller there would be nine chapters. And that is why Volume 1 contains nine chapters.

20.7 What *The New Heat Transfer* is really about.

The real subject of *The New Heat Transfer* is not heat transfer. The real subject is the new engineering. Heat transfer is merely the branch of engineering I used to demonstrate the methodology of the new engineering. Note the following on page 5-5 in Volume 1 of the first edition:

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.

Note on the page facing the title page that *The New Heat Transfer* is the first book in the New Engineering Series. In 1974, it was my intent that books on most branches of engineering would be included in the New Engineering Series. Since the new heat transfer has not yet been widely accepted, engineering branches have not yet been added to the New Engineering Series.

20.8 McGraw-Hill and *The New Heat Transfer*.
After I had written the first two chapters, I began to think seriously about how to publish the book. I decided that it would be best to have the book published by McGraw-Hill. I also decided that if McGraw-Hill did not want to publish it, I would not offer it to a second rate publisher. I would publish it myself.

My letter to McGraw-Hill Book Company dated 10/3/73 offered the publishing rights to *The New Heat Transfer*. Enclosed with the letter were the Table of Contents, the Preface, and Chapters 1 and 2. Note that the letter closes with the following:

I have not offered The New Heat Transfer to any other publisher, nor do I intend to do so. McGraw-Hill has the reputation of being the best publishing house, and only the best will suffice for The New Heat Transfer. If you will not publish it, then I will simply publish it myself.

I received a negative reply in what I judged to be a form letter dated 10/12/73, and signed by Mr. Tyler G. Hicks, Editor-in-Chief, Engineering, Science, and Management.

20.9 Why my publishing company is named "Ventuno Press".
When I started writing *The New Heat Transfer*, the company name I used was Stability Consultants, an engineering company I had unsuccessfully tried to start. (Note that Professor Clark's order cited above is addressed to Stability Consultants.) When I decided to publish the book myself, I abandoned the name "Stability Consultants" in favor of a more appropriate name for a publishing company. I chose the name "Ventuno Press" late in 1973. (It is a very small company. It publishes only material that I write, and I am the only employee.)

Ventuno is twenty-one in Italian. I chose the name Ventuno Press because I felt that the new engineering would gain worldwide acceptance by the beginning of the twenty-first century.

In the last quarter of the twentieth century, worldwide communication was rapid, and national and international engineering conferences were popular and well attended. Therefore twenty-five years seemed a reasonable length of time for worldwide acceptance of the new engineering—reasonable if one makes the naïve assumption that members of academia, and those who control engineering media and societies, have a lack of bias against new ideas, and a selfless interest in progress.

Through what I regard as no fault of mine, worldwide acceptance did not occur by the beginning of the twenty-first century. Perhaps it will occur by the end of the twenty-first century.

20.10 Why my signature is on the cover of *The New Heat Transfer.*

When all nine chapters of ***The New Heat Transfer*** were completed, I contacted Josten's (the company that prints high school yearbooks) and contracted with them to print 5000 copies of the book with a hard cover of my design. As I had promised, complimentary copies were sent to those who had purchased all nine chapters.

I was singularly responsible for the content and design of the book. I typed the masters for the book; I prepared and solved all the problems; I drew every sketch; I prepared every chart; I designed the cover. In short, I did everything except print the book.

Because I had done all the work in the preparation and design of the manuscript and cover, I considered the book a work of art as well as a work of science. And because artists generally sign their work, I thought it appropriate that my signature appear on the cover of *The New Heat Transfer*.

20.11 Mir Publishers (Moscow) published a Russian edition of *The New Heat Transfer.*

A letter dated May 28, 1975 (See pages 306-308.) from the copyright agency of the USSR stated:

The "Mir" Publishers, Moscow are interested in translating and publishing in Russian of "The New Heat Transfer, eugene f. adiutori", 1974.

The book is expected to be published in 1976 in 5,000 copies at the retail price of 1.70 rubles approximately.

Herewith we are enclosing in triplicate the draft agreement for acquisition of rights to translate and publish in Russian the above book. In case you will find the terms and conditions of the draft agreement acceptable, please sign it and return all the copies to us.

Yu. GRADOV, Director, Export & Import Department

It is probably not necessary to say that I was elated!!! Just 14 months after *The New Heat Transfer* was published, the Russians recognized its importance, and wanted to buy the rights to translate and publish a Russian edition!!

My affirmative reply was sent in my letter to Gradov dated June 6, 1975. The contract, signed by both parties, was enclosed with the letter from Gradov dated July 9, 1975.

In 1977, Mir published 5000 copies of *The New Heat Transfer* in soft cover. The price was one ruble, seventy kopeks. (See Russian cover and copyright page.)

(I had heard that the Russians did not pay royalties. The check from Mir publishers dated May 19, 1977 demonstrated that they did in fact pay royalties.)

The Russian edition has a much better appearance than the American edition. In particular, the figures and charts are much more professional. Four persons who contributed to the Russian edition are listed at the end of the book.

20.12 Foreword to the Russian edition.

The foreword to the Russian edition (according to the Berlitz translation) gives an accurate and concise summary of the new heat transfer, then states the following:

Executed by the author the broad study of assets and liabilities of giving up the employment of coefficients of heat exchange and heat conductivity in the analysis of various processes of heat transfer is of undoubted interest. . . The Adiutory's book contains data which allow to appreciate the advantages of giving up this coefficient, and in this way the book contributes to clearing up the matter whether "the game is worth the candle".

This book is . . . the exposition and the defence of the new approach to the analysis of processes of heat transfer which is suggested by the author. The author employs in this book the elementary simple apparatus and the very polemical style of exposition which can irritate a reader. We recommend not to make hasty conclusions.

A review of this book has been published in the "New Books Abroad" (Novye knigi za rubezhom") magazine, Series B.8, page 23, 1976,

Professor I. Aladjev
Doctor of Technical Science

Professor A. Leontjev
Doctor of Technical Science

20.13 Impact of the Russian edition of *The New Heat Transfer*.
It seemed to me that the Russian edition would make it difficult for leaders of the heat transfer world to publicly ignore me, and would make it impossible for them to publicly or even privately portray me as a crackpot.

I was wrong.

20.14 Aftermath of the Russian edition.
In the 1980's, I met Professor Leontjev at an international heat transfer conference. I asked him whether he or Professor Aladjev was mainly responsible for the Russian edition, and he replied that it was Professor Aladjev. (I was dismayed to notice that, in the paper he presented at the conference, Professor Leontjev used heat transfer coefficients. The Russians apparently lost interest in the new heat transfer, perhaps because they were outvoted by their American colleagues.)

In June of 2004, I met Professor Ventsislav Zimparov at an international heat transfer conference in Slovenia. He told me that, many years before, he had read the Russian edition of *The New Heat Transfer!* He is the first and only person I have met who read the Russian edition, and I was very happy to meet him. He took a photo of the two of us, and promised to send me a copy, but he did not.

I did not hear from him again until April of 2005, at which time he was working with Professor Bejan at Duke University. I had been invited to give a dinner talk at North Carolina State University that was scheduled to be held a few weeks after I heard from Professor Zimparov. Since North Carolina State University is very near Duke, I invited him to be my guest at the dinner. He said he would like to come, but he did not.

20.15 Advertising *The New Heat Transfer* in the late 1970's.
George Bernard Shaw observed that:

The spontaneous recognition of really original work begins with a mere handful of people, and propagates itself so slowly that it has become a commonplace to say that genius, demanding bread, is given a stone until after its possessor's death.

The cure for this is sedulous advertisement.

In the late 1970's (and again in the late 1980's), I tried the recommended cure—I placed many different ads for *The New Heat Transfer* in the following journals and magazines:

- *Chemical Engineering Progress*
- *ASME Journal of Heat Transfer*
- *ASME Journal of Engineering for Power*
- *Mechanical Engineering*
- *Nuclear News*
- *Chemical Engineering*
- *International Journal of Heat and Mass Transfer*

The ads from the 1970's were primarily summaries of chapters in the book. Many of the ads included a separate postcard for ordering the book. All of the ads stated that the book would be sent on approval, and the customer would be billed only if the book was not returned in 30 days.

For a time, McGraw-Hill and Ventuno Press jointly marketed *The New Heat Transfer*. McGraw-Hill placed their ads in *Chemical Engineering,* and received orders for the book. (I disliked the McGraw-Hill ads, but did not have editorial approval.) The orders were turned over to and filled by Ventuno Press. The proceeds were split 50-50. (See an example of a McGraw-Hill ad.)

I had thought and hoped that, if I could sell 1000 copies of *The New Heat Transfer*, the book would begin to sell itself, and worldwide acceptance would eventually result. I was wrong. The ads were successful in selling several thousand books, but the book never began to sell itself, and no appreciable acceptance resulted.

Sedulous advertisement may be a cure for the slower-than-death "recognition of really original work", but the cure did not work for *The New Heat Transfer*.

In 1980, I stopped advertising the book.

20.16 Advertising *The New Heat Transfer* in the late 1980's.

In 1986, I decided to again advertise *The New Heat Transfer*, but to change the nature of the ads. Ads from the 1970's were mostly summaries of chapters in the book. Ads from the 1980's were mostly short papers on specific subjects addressed in the book, and were intended to demonstrate the rationale and effectiveness of the new heat transfer.

Many of the ads were condensations of papers I had written in the 1960's and 1970's, and had tried unsuccessfully to present at engineering conferences, or arrange for publication in engineering journals. For example, there were ads with titles such as:

- Who really originated the h concept?

- What's wrong with h?

- Is nucleate boiling really described by $q = a\Delta T^3$?

- Does a straight line on log log paper really describe transition boiling data?

Ads with technical content appeared in the following engineering journals and magazines:

- *International Journal of Heat and Mass Transfer*

- *ASME Journal of Heat Transfer*

- *International Journal of Engineering Science*
- *ASME Gas Turbine and Power*
- *Mechanical Engineering*
- *Chemical Engineering Progress*
- *Numerical Heat Transfer*
- *Heat Transfer Engineering*

In all, there were approximately 100 pages of ads for *The New Heat Transfer*, most of which appeared in the *International Journal of Heat and Mass Transfer*, and in the ASME *Journal of Heat Transfer*.

20.17 The ad series in the *IJHMT* in the 1980's.

As noted above, in the 1970's, the *IJHMT* would accept ads for *The New Heat Transfer* only if they contained no technical content. As in the 1970's, I still wanted to place ads with technical content in the *IJHMT*. In my letter of 6/7/86 to the advertising agent for *IJHMT*, I enclosed ad copy that contained technical content. The cover letter stated:

When you submit the copy for editorial board approval, please indicate to them that I requested that the ad be submitted to them prior to publication.

This time the editorial board graciously approved my ads with technical content. Beginning with the October, 1986 issue, a coordinated series of ads with technical content appeared in every issue of the *IJHMT*, and it was my plan to continue the series indefinitely.

The series of ads continued until I received a letter from Professors Hartnett and Minkowycz, editors of the *IJHMT*, dated 11/19/90. (See pages 309, 310.) The letter states:

*In particular, the Board of Editors (of the IJHMT) is on record as rejecting for publication any advertisement containing technical material which would be rejected for publication when subjected to the normal review procedure. Accordingly, the Editorial Board by **unanimous** vote has recommended to Pergamon Press that they not accept any additional advertising for The New Heat Transfer.*

Note that the editors *unanimously rejected* additional ads—ie ads they had not yet seen—because if the unseen ads were "subjected to the normal review procedure", the editors were *unanimously* agreed that the unseen ads "would be rejected for publication". In other words, *every* editor had the amazing ability to make review decisions on material he had never seen! I believe that is the classic definition of bias.

Clarence Darrow, the well known, highly respected, often maligned lawyer once noted:

There is always one person to state the case for freedom. That's all we need—one!

Darrow was mistaken. There is *not* always one person to state the case for freedom. Sometimes the vote is *unanimous*.

20.18 The ad series in the ASME *Journal of Heat Transfer.*

The ad series that appeared in the *IJHMT* also appeared in the *Journal of Heat Transfer,* beginning with the May, 1986 issue. The ads appeared on the outside of the back cover of every issue, and it was my intent to continue the ads indefinitely.

One day in 1993, I received a canceled invoice for an upcoming ad in the *Journal of Heat Transfer*. On the invoice was a notice that the Editorial Board had decided to discontinue all advertising in the *Journal of Heat Transfer*. (Since my ads were essentially the only ads that appeared there, the decision to discontinue all advertising was a clever way to reject my ads with de facto bias instead of obvious bias.)

The notice gave the name and telephone number of a person who could provide more information. I called that person, but he did not take my call, nor did he return it. I was so annoyed, I threw the invoice away. (That is why there is no link to the canceled invoice.) As noted in my letter dated 10/1/93 to Professor Heggs , the last ad in the series appeared in the August, 1993 issue.

Years later, I noticed an ad in the *Journal of Heat Transfer*, indicating that the Editorial Board had reversed itself, and was again accepting ads. And I again placed an ad in the *Journal of Heat Transfer*, but this time, the ad was for *The New Engineering*.

20.19 The second edition of *The New Heat Transfer.*

In 1989, I wrote, and Ventuno Press published, a second edition of *The New Heat Transfer*. I sent review copies to more than 300 engineering journals and magazines, but again, few reviews were published.

The second edition was not well received.

20.20 Cartoon ad for *The New Heat Transfer.*

In 1990, my sister Josie DeSantis owned a sign company, and she employed "Doc" DeStefano, a competent cartoonist. I showed him a simple sketch of the cartoon I wanted, and it became a cartoon ad that appeared in the December, 1990 issue of *Mechanical Engineering*. (See page 311.)

The cartoon ad conveyed my message—the useful life of heat transfer coefficient is over, and it should be buried alongside other thermal concepts that were buried when they had outlived their usefulness, such as phlogiston and caloric fluid.

I thought the cartoon was well done, but it had no impact.

20.21 Ad offering free copies of the second edition of *The New Heat Transfer.*

In 1993, I decided that, since I could not sell the book, I would give it away. Toward that end, I placed ads that offered free copies of the book to anyone who remitted the required postage, $1.91 within the USA, $3.22 outside the USA. I also sent letters to editors of more than 200 engineering journals and magazines requesting that they mention the free copies, and offering to send them free copies. My letter to Editor Czakainski dated 10/20/93 is typical.

I no longer have a record of how many requests were received, but it was a considerable number.

Chapter 21

Debunking the myth that Newton conceived "Newton's law of cooling" and h.

This chapter is an abridged version of a narrative on my website. The unabridged version, and all correspondence and publications referenced in this chapter can be downloaded without charge for personal use at my website, thenewengineering.com.

21 The myth. And the *denouement*.

For more than 60 years, most American heat transfer texts (for example texts by Jakob (1949), McAdams (1954), Eckert and Drake (1972), Rohsenow and Choi (1973), Rohsenow and Hartnett (1973), Kreith, (1986), Incropera and Dewitt (2007), Holman (2009), Nellis and Klein (2009), Lienhard and Lienhard (2011), and Cengel and Ghajar (2015)) have referred to Eq. (21-1) as "Newton's law of cooling", and credited Newton with Eq. (21-1) and h.

$$q = h\,\Delta T \qquad (21\text{-}1)$$

(q is heat flux, h is heat transfer coefficient, and ΔT is temperature difference between the surface of an object and the ambient fluid in which it is immersed.)

Texts that cite a specific reference generally cite Newton's article, "A Scale of the Degrees of Heat" published in 1701 in the *Proceedings of the Royal Society of London*, Volume 22, page 824. The article was in Latin, and the author was anonymous, although it was no secret that Newton was the author.

Eq. (21-1) *cannot* be "Newton's law of *cooling* because cooling is *transient* behavior, and Eq. (21-1) is a *steady-state* equation.

Eq. (21-1) *cannot* be "Newton's law of cooling" because Newton and his colleagues would have considered Eq. (21-1) *irrational*. In their view of dimensional homogeneity, it is *irrational* to multiply dimensioned parameters such as h and ΔT.

Newton should *not* be credited with Eq. (21-1) or h because Fourier rightly claimed priority for them in *The Analytical Theory of Heat* published in 1822. If his claim had been false, Fourier would surely have been *ridiculed*, and *The Analytical Theory of Heat* would *not* have been widely acclaimed, and published in many editions, and in many languages.

21.1 The first author to credit Newton with h and "Newton's law of cooling".

It is not certain which author first cited Newton's 1701 article as evidence that Newton conceived h and Eq. (1). I feel that a good guess is Professor McAdams, MIT. His *Heat Transmission* (editions published in 1933, 1942, and 1954) is the earliest text I found that credited Newton with Eq. (21-1) and h, and also cited Newton's 1701 article. (Although I no longer have ready access to various editions of *Heat Transmission*, I am quite certain that Newton's article is cited in the third edition, but not in the first edition.)

With regard to the numerous authors who credit Newton with Eq. (21-1) and h, it is quite likely that they simply followed McAdams' lead. Consequently, no significance should be attached to the fact that numerous authors credit Newton with Eq. (21-1) and h unless it can be shown that their agreement with McAdams resulted from informed, independent conclusion, and not from assuming that McAdams correctly cited Newton (1701).

21.2 How I obtained Newton's 1701 article, and what the article is really about.

After I conceived the new engineering, I wanted to determine who conceived conventional engineering science, and why he/she conceived parameters not found in Nature—parameters such as electrical resistance and heat transfer coefficient.

American heat transfer texts led me to believe that Newton conceived Eq. (21-1) and h, and referred to Eq. (21-1) as Newton's law of cooling, suggesting to me that Newton conceived conventional engineering science. To determine whether or not Newton did in fact conceive conventional engineering, I needed to obtain Newton's 1701 article[5].

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[5] The first translation of Newton's article was published in 1749. The complete article is on pages 241-245.

I was daunted by the fact that the article was published almost 300 years earlier, and was afraid only the Library of Congress would have it. I was astonished to discover that the Hamilton County Library in downtown Cincinnati has everything published by the Royal Society since it began meeting in the seventeenth century!

I obtained the volume that contains Newton's article, and was dismayed to find that the article was in Latin, and its title was "Scala graduum Caloris". After spending several hours trying to translate the article with the aid of a Latin/English dictionary, I stopped translating, and began searching the card catalog, hoping to find an anthology that included an English translation of Newton's article.

Fortunately, I found an anthology by Professor I. Bernard Cohen (Harvard) that included an English translation of Newton's article. I was dumbfounded to find that Newton's article had *nothing to do* with the equation generally referred to as "Newton's law of cooling"—*nothing to do* with the concept of heat flux —*nothing to do* with the concept of heat transfer coefficient!

Eq. (21-1) and h were in fact conceived by Fourier (1822). Eq. (21-1) should be referred to as "Fourier's steady-state law of forced convection heat transfer to atmospheric air". [6] As noted above, it is a misnomer to state that Eq. (21-1) is a law of *cooling* because Eq. (21-1) is a *steady-state* equation, and cooling is a *transient* phenomenon.

Newton's article describes a temperature scale in which 0 degrees of "heat" is

The Heat of Winter Air, when Water begins to freeze. This Heat is known by rightly placing the Thermometer in Snow pressed together, at what Time it begins to thaw.

and 12 degrees of "heat" is

The greatest Heat that the Thermometer receives by the Contact of a Human body. This Heat is much the same as that of a Bird sitting upon her Eggs.

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[6] See my article entitled "Origins of the Heat Transfer Coefficient" in the August 1990 issue of *Mechanical Engineering*. The article is an edited version of ASME-89-HT-3, the paper I presented at the 1989 Joint ASME/AIChE National Heat Transfer Conference.

Newton determined the temperature of various phenomena based on his proposed temperature scale, and data he obtained with a linseed oil thermometer. Temperatures beyond the range of his thermometer were determined by extrapolation, using the law of cooling described in the article.

The parametric expression that is in fact Newton's law of cooling, and is referred to by Fourier (1822), states that when an object cools, its rate of temperature change is proportional to the temperature difference between the object and the ambient fluid. Symbolically, the law of cooling given in Newton's article is

$$(dT_{object}/dt) \; \alpha \; -(T_{object} - T_{ambient}) \qquad (21\text{-}2)$$

(Note that Newton's law of cooling is based on the temperature of the object rather than the *surface* temperature of the object.)

In summary, Proportion (21-2) is the *only* parametric expression that is appropriately referred to as "Newton's law of cooling". The *only* parameters in Proportion (21-2) are *temperature* and *time*. Proportion (21-2) has *nothing to do* with heat transfer coefficient—*nothing to do* with heat flux—*nothing to do* with "heat" in the modern sense.

21.3 The modern translation of the word "calor" used in Newton's 1701 article.

When Newton's article was translated in 1749, "calor" was correctly translated to "heat". But in 1749, "heat" was used in place of "temperature". (According to Merriam-Webster, the word "temperature" was not used until 1850.)

If Newton's article were translated today, the correct title would be "A Scale of the Degrees of Temperature", and "temperature" rather than "heat" would appear repeatedly in the body of the article. And it would be readily apparent that Newton did *not* conceive h or Eq. (21-1).

21.4 Fourier's valid claim that he conceived h and "Newton's law of cooling".

American texts usually credit Fourier with the concept of thermal conductivity, and cite his *Analytical Theory of Heat* published in 1822. Fourier's book is easy to obtain, and a delight to read. It can be borrowed from many public libraries, and a Dover edition published in 2003 can be purchased at any bookstore.

(It is a sad commentary on the American education system to note that, in my many engineering courses, I was required to read thousands of pages written by members of academia, but I was *never* required to read even one paragraph written by a great man of science such as Aristotle or Galileo or Kepler or Newton or Fourier or Lavoisier or Maxwell or . . .)

In *The Analytical Theory of Heat*, Fourier refers to Newton's article, but does not credit Newton with the heat transfer coefficient concept. On page 458, Fourier merely observes that:

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 temperature becomes less; it would exactly hold if that difference were infinitely small.

On page 2 of *The Analytical Theory of Heat*, Fourier rightly claims that he conceived the laws of convective and conductive heat transfer. Fourier states:

We have for a long time been in possession of ingenious instruments adapted to measure many of these (heat transfer*) 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 (of convective and conductive heat transfer) 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.

On page 31, Fourier defines the coefficient in his law of convective heat transfer:

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 experiment.

Note that Fourier's h strictly applies *only* if:

- The heat transfer fluid is atmospheric air.
- A surface is cooled by the forced convection of air.
- The velocity of the forced convection air is invariable.

Note that there is a considerable difference between Fourier's h and the current conventional h that applies to:

- Air at *all* pressures,
- *All* fluids.
- *Both* cooling and heating.
- *Both* forced convection and free convection.
- One *and* two phase fluids.

Also note that Fourier's h is *always* a constant of proportionality, whereas conventional h is *sometimes* a constant of proportionality, and *sometimes a variable function of* ΔT.

Fourier's claim to h and Eq. (21-1) is difficult to discredit because, if his claim had been a lie, and Newton had in fact conceived h and Eq. (21-1):

- Fourier would have been severely *ridiculed* by his contemporaries for falsely claiming Newton's contributions as his own.
- Fourier would *not* have received the 1812 prize from the Institut de France for his prize essay, an early version of *The Analytical Theory of Heat*.
- *The Analytical Theory of Heat* would *not* have been widely acclaimed and published in many editions and many languages over a period of two *centuries*.
- It would be necessary to credit Newton, rather than Fourier, with the conventional engineering view of homogeneity.

Newton and his contemporaries held a view of dimensional homogeneity that little resembles the current conventional view generally credited to Fourier. Newton and his contemporaries held that:

- o Dimensioned parameters *cannot* be multiplied and divided, with the *single exception* that a dimensioned parameter may be divided by the same dimensioned parameter, resulting in a pure number.
- o Rational parametric equations are *dimensionless*
- o Dimensioned equations like Eq. (21-1) are *irrational*.

- It would be necessary to credit Newton, rather than Fourier, with the concept of flux in general, and heat flux in particular.

When I became *certain* that Newton did *not* conceive h or Eq. (21-1), and Fourier did, I resolved to debunk the widespread myth that Newton conceived h and "Newton's law of cooling".

21.5 In 1974, *The New Heat Transfer* credited Fourier with h and "Newton's law of cooling", but others refused to accept that conclusion.

The New Heat Transfer, published in 1974, offered what I considered convincing evidence that Newton did not conceive h or "Newton's law of cooling", and that Fourier did. However, it had little impact on those who write American heat transfer texts, either because they did not read *The New Heat Transfer*, or because they found the evidence presented there less than convincing.

For example, "History of Heat Transfer—Essays in Honor of the 50th Anniversary of the ASME Heat Transfer Division", edited by Professors Layton and Lienhard, 1988, contains an essay by Professor Bergles (Rensselaer Polytechnic Institute) that states:

A review of the background of "Newton's law of cooling" leads to the conclusion that it is appropriate to credit Newton with the concept of the convective heat transfer coefficient.

The article indicates that Professor Bergles considered the evidence offered in *The New Heat Transfer*, but found it unconvincing.

Indeed, there is sharp criticism of those who would identify Newton with equation (1), e.g. Adiutori (1974).

21.6 The need for a definitive document to be published in an English language, heat transfer journal.

I consider it unacceptable for American heat transfer texts to claim that Newton conceived h and "Newton's law of cooling" in 1701, when in fact they were conceived by Fourier more than 100 years later.

In order to right this wrong, persons who write American heat transfer texts must somehow be convinced that Fourier rather than Newton conceived h and "Newton's law of cooling".

Since the evidence offered in *The New Heat Transfer* did not convince Professor Bergles, a document was required that would concretely convince authors of American heat transfer texts that Fourier conceived "Newton's law of cooling" and h. And it needed to be published in an English language, scholarly heat transfer journal where it would likely be read by authors of American heat transfer texts.

21.7 The need for an early translation of Newton's article.

I wanted to write the definitive document on the origin of Eq. (21-1) and h, and felt that it should be based on a translation of Newton's article published before 1750 in order to be certain that the person who translated Newton's article did not know more about heat transfer than Newton did. 1750 was the approximate year that Joseph Black is credited with drawing the first clear distinction between heat and temperature.

Encyclopedia Americana (1984) states:

Until the middle of the 18th century little or no distinction was made between heat and temperature. About that time Joseph Black . . . clearly distinguished between quantity of heat and intensity of heat, as temperature was designated.

In Professor Cohen's anthology, no reference was cited for the source of the English translation of Newton's article. In order to determine the source and the date it was translated, I called Professor Cohen at his home. He was both gracious and helpful.

The source was *"The Philosophical Transactions of the Royal Society of London, From Their Commencement, in 1665, to the Year 1800, Abridged"* by C. and R. Baldwin, London, 1809. When I told Professor Cohen that I hoped to obtain an earlier translation, he said that every 20 or 30 years, the Royal Society published a collection of the more important papers, and perhaps one of those contained a translation of Newton's article.

I returned to the Hamilton County Library, and found exactly what I wanted, *The Philosophical Transactions (From the Year 1700, to the Year 1720) Abridged* by Henry Jones, London, 1749. The book's title page and the complete article are on pages 241-245. Note that the title page states "In which the Latin papers are now first translated into English".

THE

PHILOSOPHICAL TRANSACTIONS

(From the Year 1700, *to the Year* 1720.)

ABRIDG'D,

AND

Diſpos'd under GENERAL HEADS.

In Two VOLUMES.

By *HENRY JONES*, M. A. and
Fellow of *King's College* in *CAMBRIDGE*.

VOL. IV. Containing

Part I. The MATHEMATICAL Papers.

Part II. The PHYSIOLOGICAL Papers.

The THIRD EDITION Corrected.

In which the *LATIN* PAPERS are now firſt tranſlated into *ENGLISH*.

LONDON:
Printed for W. INNYS, R. WARE, J. and P. KNAPTON, D. BROWNE,
T. LONGMAN, C. HITCH, J. HODGES, S. AUSTEN, A. MILLAR,
J. and J. RIVINGTON, and J. WARD.
M. DCC. XLIX.

The Philosophical Transactions ABRIDG'D.

PART II.

Containing the

PHYSIOLOGICAL PAPERS.

CHAP. I.

Physiology. Meteorology. Pneumatics.

A Scale of the Degrees of Heat, by . . . n. 270. p. 824.

0		I. THE Heat of Winter Air, when Water begins to freeze. This Heat is known by rightly placing the Thermometer in Snow pressed together, at what Time it begins to thaw.
0, 1, 2.		The Heat of Winter Air.
[illegible] 4.		The Heat of the Air in Spring and Autumn.
4, 5, 6.		The Heat of the Air in Summer.
6.		The Heat of the Air at Noon, about the Month of *July*.
12.	1	The greatest Heat that the Thermometer receives by the Contact of a Human Body. This Heat is much the same as that of a Bird sitting upon her Eggs.
$14\frac{3}{11}$	$1\frac{1}{4}$	The Heat of a Bath, which is almost the greatest that any one can endure long, with his Hand agitated and immersed in it. The same almost is the Heat of Blood just let out.
17	$1\frac{1}{3}$	The greatest Heat of a Bath that any one can endure long, his Hand being immersed and at rest in it.
$20\frac{2}{11}$	$1\frac{1}{2}$	The Heat of a Bath in which Wax swimming and melting, by moving about grows hard and loses its Transparency.
24	2	The Heat of a Bath in which Wax swimming grows liquid by the Heat, and is preserved in continual Flux without Ebullition.
$28\frac{6}{11}$	$2\frac{1}{4}$	The intermediate Heat between the Degrees in which the Wax melts and the Water boils.
34	$2\frac{1}{2}$	The Heat by which Water boils violently, and a Mixture of two Parts of Lead, of three Parts of Pewter, and

		and of five Parts of Bifmuth grows ftiff in cooling. Water begins to boil by a Heat of 33 Parts, and in boiling conceives a Heat of more than $34\frac{1}{2}$ Parts. But Iron with a Heat of 35 or 36 Parts ceafes to excite an Ebullition, when hot Water is dropt upon it; and of 37 Parts, when cold Water does the fame.
$40\frac{4}{11}$	$2\frac{3}{4}$	The leaft Heat by which a Mixture of one Part Lead, of four Parts Pewter, and of five Parts Bifmuth, grows hot and melts, and is preferved in a continual Flux.
48	3	The leaft Heat by which a Mixture of equal Parts of Pewter and Bifmuth melts. This Mixture cools and coagulates by a Heat of 47 Degrees.
57	$3\frac{1}{4}$	A Heat by which a Mixture of two Parts of Pewter, and one Part of Bifmuth is melted, as alfo a Mixture of three Parts of Pewter, and two Parts of Lead. But a Mixture of five Parts of Pewter, and of two Parts of Bifmuth, cools and grows ftiff with this Heat. And a Mixture of equal Parts of Lead and Bifmuth does the fame.
68	$3\frac{1}{2}$	The leaft Heat by which a Mixture of one Part of Bifmuth, and eight Parts of Pewter is melted. Pewter alone is melted with a Heat of 72 Parts, and cools and grows ftiff by a Heat of 70 Parts.
81	$3\frac{3}{4}$	The Heat by which Bifmuth is melted, as alfo a Mixture of four Parts of Lead, and one Part of Pewter. But a Mixture of five Parts of Lead, and one Part of Pewter, grows ftiff when melted, and cools in this Heat.
96	4	The leaft Heat by which Lead is melted. Lead grows hot and melts in a Heat of 96 or 97 Parts, and cools and grows ftiff in a Heat of 95 Parts.
114	$4\frac{1}{4}$	The Heat by which Bodies heated in the Fire by cooling quite leave off to fhine in the Darknefs of the Night, and again by growing hot begin to fhine in the fame Darknefs, but with a very faint Light which can hardly be perceived. In this Heat a Mixture of equal Parts of Pewter and Regulus Martis will melt; but a Mixture of feven Parts of Bifmuth, and four Parts of the fame Regulus Martis, will cool and grow ftiff.
136	$4\frac{1}{2}$	The Heat by which Bodies heated in the Fire grow red hot, but not fo in the Twilight. By this Heat a Mixture of two Parts of Regulus Martis, and of one Part of Bifmuth, as alfo a Mixture of five Parts of Regulus Martis, and one Part of Pewter, by cooling grows ftiff. The Regulus by itfelf grows ftiff with a Heat of 146 Degrees.

161

161	4½	The Heat by which Bodies heated in the Fire plainly grow red hot in the Twilight, juſt before the Riſing or Setting of the Sun, but not ſo in open Day-light, or but very obſcurely.
192	5	The Heat of burning Coals in a ſmall Kitchen Fire, made of bituminous foſſile Coals, and without blowing with Bellows. The ſame is the Heat of Iron in ſuch a Fire, that grows red hot as much as it can. The Heat of a ſmall Culinary Fire made of Wood is ſomething greater, perhaps of 200 or 210 Degrees. But the Heat of a large Fire is ſomething greater ſtill, eſpecially if provoked by the Uſe of Bellows.

In the firſt Column of this Table we have the Degrees of Heat in Arithmetical Progreſſion, beginning the Computation from that Degree in which Water begins to freeze, as it were from the loweſt Degree of Heat, or the common Limit of Heat and Cold, and making the external Heat of a Human Body to be 12 Degrees. In the ſecond Column are had the Degrees of Heat in Geometrical Progreſſion, ſo that the ſecond Degree is as great again as the firſt, the third as great again as the ſecond, and ſo on; and the firſt is the external Heat of the Body of a Man adequate to Senſe. Now it appears from this Table, that the Heat of boiling Water is almoſt three Times greater than the Heat of the Human Body, and that the Heat of melted Pewter is ſix Times greater, and the Heat of melted Lead is eight Times greater, and the Heat of melted Regulus is twelve Times greater, and that the ordinary Heat of a Culinary Fire is 16 or 17 Times greater, than the ſame Heat of a Human Body.

This Table was conſtructed by the help of a Thermometer and a piece of red hot Iron. By the Thermometer I found the Meaſure of all the Degrees of Heat, till I came to the Heat with which Pewter is melted, and by the red hot Iron I found the Meaſure of the reſt. For the Heat which red hot Iron communicates to cold Bodies which are contiguous to it, in a given time, that is, the Heat which the Iron loſes in a given time, is as the whole Heat of the Iron. Therefore if the Times of cooling are taken equal, the Heats will be in a Geometrical Ratio, and therefore are eaſily found by a Table of Logarithms.

Therefore firſt I found, by a Thermometer conſtructed with Linſeed Oyl, that when the Thermometer was put into melting Snow, the Oyl took up a Space of 10000 Parts. The ſame Oyl rarified by a Heat of the firſt Degree, or by that of a human Body, took up the Space 10256; and by the Heat of Water juſt beginning to boil, it took up the Space 10705, and by the Heat of Water boiling vehemently it took up the Space 10725, and by the Heat of melted Pewter cooling, when it began to be ſtiff and put on the Conſiſtence of an Amalgama, it took up the Space 11516, and the Space 11496 when it was quite ſtiff. Therefore the rarified Oyl was to the dilated in the Ratio

Ratio of 40 to 39, by the Heat of the human Body; in the Ratio of 15 to 14 by the Heat of boiling Water; in the Ratio of 15 to 13 by the Heat of cooling Pewter, when it began to grow ſtiff and coagulate; and in the Ratio of 23 to 20 by the Heat by which cooling Pewter grows quite ſtiff. The Rarefaction of Air with equal Heat was ten times greater than the Rarefaction of Oyl, and the Rarefaction of Oyl was about 15 times greater than the Rarefaction of Spirit of Wine. And from what is here found, by ſuppoſing the Heat of the Oyl proportional to its Rarefaction, and for the Heat of the human Body writing 12 Degrees, the Heat of Water when it begins to boil will come out 33 Degrees, and when it boils vehemently 34 Degrees; and the Heat of Pewter either when it melts, or when it begins to cool and becomes of the Conſiſtency of an Amalgama, will be of 72 Degrees, and when it cools and grows hard, of 70 Degrees.

Theſe things being known, that I might find the reſt, I heated a thick piece of Iron till it was red hot, and taking it out of the Fire with a hot pair of Pincers, I immediately put it in a cold Place, where the Wind blew conſtantly; and putting upon it little Particles of different Kinds of Metals, and other Bodies that would melt, I obſerved the Times of Cooling, till all the Particles grow ſtiff and loſt their Fluidity, and the Heat of the Iron was equal to the Heat of the human Body. Then ſuppoſing that the Exceſſes of the Heat of the Iron and the rigid Particles above the Heat of the Atmoſphere found by the Thermometer, are in Geometrical Progreſſion when the Times are in Arithmetical Progreſſion, all the Degrees of Heat became known. I placed the Iron in a Wind blowing uniformly, and not in a quiet Air, that the Air heated by the Iron might always be carry'd away by the Wind, and the cool Air might ſucceed in its Place with an uniform Motion. For thus equal Parts of the Air would be made hot in equal Times, and would conceive a Heat proportional to the Heat of the Iron.

Now the Heats thus found will have the ſame Proportion to one another with the Heats found by the Thermometer, and therefore we have rightly aſſumed, that the Rarefactions of the Oyl are proportional to its Heat.

An Account of Dr. Hook's *Marine Barometer, by Mr.* E. Halley, n. 269. p. 791.

II. Dr. *Hook*, who has made many Attempts to improve the *Barometer*, and to render the minute Diviſions on the Scale thereof more ſenſible, judging that it might be of great Uſe at Sea, contrived ſeveral Ways to make it ſerviceable on Board a Ship; one of which he explained to the *Royal Society* at their weekly Meeting in *Greſham-College*, *January* 2. $166\frac{7}{8}$. ſince which Time he hath further cultivated the Invention, and ſome Years ago produced before the ſaid *Society*, the Inſtrument I am now to deſcribe.

The Mercurial Barometer requiring a perpendicular Poſture, and the Quickſilver vibrating therein with great Violence upon any Agitation, is therefore uncapable of being uſed at Sea, (though it hath lately

21.8 "A New Look at the Origin of the Heat Transfer Coefficient Concept".

In 1985, I wrote what I consider to be the definitive document on the origin of h and Eq. (21-1), and entitled it "A New Look at the Origin of the Heat Transfer Coefficient Concept". It establishes beyond any doubt that Newton did *not* conceive h or Eq. (21-1), and that Fourier did.

One telling argument is that h and Eq. (21-1) could be conceived only by someone who understands the concept of "flux". The concept of flux in general, and heat flux in particular, were conceived by Fourier almost 100 years *after* Newton died. Quoting from Herivel (1975):

(The concept of flux) must be regarded as (Fourier's) most critically important and original single insight into the physical nature of the conduction of heat in solid bodies. . . Fourier's contemporaries (Laplace, Poisson, Biot) found it excessively difficult either to understand or to accept this concept. (And they refused to accept it for more than a decade.)

. . . this is surely another example of one of those apparently simple, almost trivial, concepts in theoretical physics which nevertheless seem to require for their formulation the intervention of a Galileo or a Newton.

21.9 In 1985, *IJHMT* considered publishing "A New Look at the Origin of the Heat Transfer Coefficient Concept", but decided not to.

As noted above, I felt that "A New Look . . ." needed to be published in an English language, scholarly heat transfer journal in order to ensure that it would be read by authors of American heat transfer texts. In my view, the journal of choice was *International Journal of Heat and Mass Transfer (IJHMT)*. My letter dated 8/5/85 to Professor Spalding, Editor, *IJHMT* stated:

. . . I would like to submit a rather unusual manuscript if you agree that the subject matter is appropriate for the Journal. The manuscript deals with the myth that Newton originated the concept of the heat transfer coefficient.

I did not want to submit the paper to *IJHMT* if the editor felt that the subject matter was not suitable for his journal.

Professor Spalding's reply dated 8/14/85 stated:

Thank you for your letter dated August 5th. The subject matter of your proposed paper is certainly an interesting one and I would be very pleased to see the paper.

My letter dated 10/30/85 to Professor Spalding was the cover letter for my manuscript entitled "A New Look . . ." (The manuscript was essentially identical to ASME Paper 89-HT-3 of the same title.) I also included a copy of Newton's article, and explained that it was to be published in tandem with "A New Look . . .".

With regard to "A New Look . . .", Professor Spalding's letter of 11/26/85 stated:

Despite the interest of the facts which (your paper) discloses, however, I do not think that I can publish your article, mainly because it is too long to be fitted in, but also because the reaction of most of your readers would be, I fear, "So what?"

21.10 In 1986, at the 8th International National Heat Transfer Conference, I received 52 requests for copies of "A New Look . . ."

At an Open Forum poster session of the 8th International Heat Transfer Conference, I presented paper OP-54 entitled "What's Wrong with h?" (Papers presented at open forum sessions are not listed in the conference program, and do not appear in the conference proceedings. Because no permanent record is made of papers presented at open forums, the entry standards are low, and virtually all papers are accepted.

At poster sessions, each author is assigned a booth with a backdrop and a table. The author puts up posters of his subject matter on the backdrop, and places display objects on the table. Conference attendees stroll by and peruse the posters and the display objects. Persons with a genuine interest in the subject matter oftentimes discuss it with the author.)

On my table, I placed seven or eight copies of Newton's article "A Scale of the Degrees of Heat", and a like number of "A New Look . . .". It was my intent that persons strolling by would merely peruse them, but I soon noticed they were disappearing. I then labeled the remaining copies "Please do not remove", and placed signup sheets on the table. The sheets stated that free copies of both Newton's paper and "A New Look . . ." would be mailed to persons who left names and addresses.

When I returned home from the conference, I mailed free copies to the 52 persons who had signed up. I was more than happy to do so, as indicated in the cover letter of August 29, 1986 that accompanied the free copies.

21.11 In 1987, IJHMT again considered publishing "A New Look . . ." This time, they lost the manuscript!

On September 3, 1986, I called Professor Spalding to urge him to reconsider his negative decision. He said he would reconsider it, and requested that I resubmit the paper.

My letter dated 9/3/86 to Professor Spalding summarized our telephone conversation, enclosed a copy of the paper I submitted on 10/30/85, mentioned that I had received 52 requests for copies of my paper, and thanked him for reconsidering.

On 1/5/87, I wrote to Professor Spalding to find out what action was being taken on my paper. (The letter is no longer in my files.)

A letter dated 2/19/87 from Professor Spalding's secretary stated:

. . . Professor Spalding sent your paper to Professor Hartnett . . . who is the Coordinating Editor of International Communications in Heat and Mass Transfer. It was decided to publish your paper in the Communications journal and I had assumed that Professor Hartnett would then contact you to this effect.

I suggest that you contact him.

When I learned that the purpose of the Communications journal is to permit rapid publication of preliminary findings, I concluded that I did not want my paper published there. I did not want it published in a journal for preliminary findings. There was nothing preliminary about my findings.

I at once called Professor Hartnett to tell him I did not want the paper published in the Communications journal. Our telephone conversation is summarized in my letter dated 2/26/87 to Professor Spalding's secretary. The letter states:

As you suggested, I contacted Professor Hartnett and learned that he had no recollection of receiving the paper or accepting it for publication. . . . Please note that, if you locate my paper, I prefer that it not be published in the Communications Journal.

In summary, the only result of dealing with *IJHMT* for more than a year was that they somehow managed to lose my manuscript!

21.12 In 1988, "A New Look . . ." was rejected for presentation at the National Heat Transfer Conference.

Since the *IJHMT* would not publish "A New Look . . .", I submitted the paper to Professor Kaviany (University of Michigan) for possible presentation at the 1988 AIChE/ASME National Heat Transfer Conference. I was certain the paper would be accepted for presentation.

I was wrong (again). A letter from Professor Kaviany dated February 9, 1988 stated:

The reviewers have recommended not to accept the paper for presentation (although they find the content interesting).

It is ironic and amusing that, although the editor of *IJHMT* and the several reviewers found the paper interesting, they all felt that others would not want to read it or hear about it!

21.13 In 1988, Ventuno Press printed a monograph that included "A New Look . . ." and "A Scale of the Degrees of Heat". Ads entitled "Demise of a Myth" offered free copies. 154 requests were received from 28 countries.

Since the *IJHMT* was not going to publish the paper, and since the ASME would not accept it for presentation, I decided that Ventuno Press (my company) would print and distribute the paper in a monograph.

The monograph I distributed included Newton's article in its entirety so that the reader could reach an informed, independent conclusion about whether or not Newton should be credited with "Newton's law of cooling" and h.

I recognized that a monograph that is written by someone who is not a member of academia, and is prepared and distributed by a private publishing company, would not have the impact I desired. But I did not want to do nothing.

In 1988, I placed ads entitled "The Demise of a Myth" in *IJHMT*, *Journal of Heat Transfer*, and in other journals and magazines. The ads stated:

There is a widespread myth that Newton conceived the heat transfer coefficient (h) concept in 1701. This myth is historically inaccurate by over one hundred years. The h concept was actually conceived by Fourier in the nineteenth century.

To hasten the demise of this Newtonian myth, Ventuno Press has prepared a monograph entitled "A New Look at the Origin of the Heat Transfer Coefficient Concept" by Eugene F. Adiutori. To obtain a free copy of the monograph, write to (Ventuno Press).

Ventuno Press received 154 requests from readers in 28 countries. (All 154 requests are still in my files.) The monograph was sent to everyone who requested it.

Including the 52 requests for copies I had received at the 1986 International Heat Transfer Conference (mentioned in my letter dated 9/3/86 to Professor Spalding), Ventuno Press mailed 206 free copies of "A New Look . . .". As I had hoped, most of the requests were from professors.

21.14 In 1989, I presented "A New Look . . ." at the National Heat Transfer Conference. Free copies were mailed to all who requested them.

"A New Look . . ." was accepted for presentation at the 1989 ASME/AIChE National Heat Transfer Conference. It was ASME Paper 89-HT-3. (It was the same as the monograph that was sponsored and distributed by Ventuno Press the previous year.)

One thing that was instrumental in my paper's acceptance was that a professor with whom I had been corresponding was one of the reviewers. He had seen the "Demise of a Myth" ad in the ASME *Journal of Heat Transfer*, and requested a copy of "A New Look . . ."

As noted in his letter to Professor Carey dated 1/31/89, the professor and I had discussed "A New Look . . ." in the several months before he was asked to review it, and that fact alone added greatly to my credibility, and to the credibility of my article. (The letter to Professor Carey notes that my article was not the first to "clear up misconceptions" about Newton's law, and cites a 1984 article by Grigull. However, my article was an amplification of the view I expressed in *The New Heat Transfer* published in 1974.)

I presented the paper at a poster session—ie a session where I was given a booth, a poster board, and a table. Attendees walked by and inspected my posters, and perused the documents on my table. Oftentimes they discussed the origin of h with me.

At that time, the ASME printed copies of each conference paper, and sold them to conference attendees for a price that I considered unacceptable—I think the price was $3.00. The conference papers were 8 pages long. Copies were printed on both sides of 2 sheets of 11" x 17" paper, and the 2 sheets were then stapled in the middle to form an eight page pamphlet.

Since each pamphlet consisted of two sheets of paper and two staples, I felt that $.25 was a more reasonable price to charge attendees, particularly since they had already paid a registration fee of $170. Moreover, the specific purpose of these conferences is to promote the flow of information, and charging $3.00 for each paper inhibited, rather than promoted, the flow of information.

By way of silent protest, I told each attendee who expressed an interest in my paper that he should *not* purchase it—that I would send him a free copy if he would write his name and address on one of the signup sheets on my table. I no longer have the signup sheets in my files, but I recall mailing out a large number of copies.

One of the persons who passed by my booth was Dr. Shah who, at that time, was an officer of the ASME. I told him that I considered it unacceptable for the ASME to charge $3.00 for a pamphlet that was merely 2 sheets of paper and 2 staples, and that an acceptable price was $.25. Dr. Shah replied that attendees did not mind paying $3.00 for each paper. But he wrote his name and address on the signup sheet!

21.15 In 1990, *Mechanical Engineering* published "Origins of the Heat Transfer Coefficient Concept", an abridged version of "A New Look . . .".

In the hope of having "A New Look . . ." published in an English language heat transfer journal where it would likely be seen by professors who write heat transfer texts, I submitted the paper to Professor Faeth, Technical Editor, ASME *Journal of Heat Transfer*.

In his letter dated 1/25/90, Professor Faeth stated:

The paper is certainly a scholarly and interesting account of the current notions concerning "Newton's law of cooling", and I enjoyed reading it. However, in order to control our backlog we have not considered papers dealing with historical aspects of heat transfer as a matter of policy.

He also volunteered the following helpful suggestion:

A shorter account could be submitted to Mechanical Engineering, a topical journal of the ASME, where your message would reach most university teachers in heat transfer.

I sensed that Professor Faeth's suggestion was well intended, and followed up on it. My letter dated 2/15/90 to Editor O'Leary, *Mechanical Engineering*, enclosed a copy of "A New Look . . ." and a short summary, then concluded with:

If you would be interested in a shorter, popularized version of the enclosed manuscript for Mechanical Engineering, please let me know what you consider an appropriate length.

To my great surprise and boundless joy, Editor O'Leary's letter dated 5/2/90 stated:

. . . I would be pleased to publish a "popularized" version of your paper, "A New Look at the Origin of the Heat Transfer Coefficient Concept". The revised article would fit very nicely into our August issue, which has heat transfer as one of its themes.

In the event that I would be unable to make the required revisions in time for the August issue deadline, the letter stated:

If you are pressed for time, we would be happy to make these editorial changes and show them to you for your approval before publication.

I would have been happy to make the required revisions, but I did not want to do anything that might delay or jeopardize the publication of "A New Look . . .". I allowed the staff of *Mechanical Engineering* to revise the manuscript with my final approval.

We had no difficulty agreeing on the final version of "A New Look . . . ", and the revised version was published in the August, 1990 issue of *Mechanical engineering* under the title "Origins of the Heat Transfer Coefficient Concept".

When the article was published, I felt that I had done everything reasonably possible to right a wrong.

21.16 Newton's law of cooling in recently published heat transfer texts.

I am dismayed to note that, almost *30 years* after the definitive debunking article was presented at an ASME conference and published in ASME's *Mechanical Engineering*, American heat transfer texts *still* generally and erroneously refer to Eq. (21-1) as "Newton's law of cooling", and claim that Newton conceived Eq. (21-1) and h.

Surely 30 *years* is more than enough time for text books to catch up with the literature.

And I am pleased to note that Professor Adrian Bejan (Duke) credits Fourier with Eq. (21-1) and h in *Heat Transfer* published in 1993, and in *Convection Heat Transfer* published in 2013.

Apparently, Alexander von Humboldt was correct when he noted:

There are three stages of scientific discovery.

- *First people deny it is true.*
- *Then they deny it is important.*
- *Finally they credit the wrong person.*

Appendix 1

Why the manner in which engineering journals are administered *must* be changed.

All of the correspondence and publications referred to in this appendix can be downloaded at my website, thenewengineering.com.

A1 Summary

It is true that all change does not bring progress. But it is equally true that all progress brings change.

It is not possible to be *for* progress, and *against* change.

It is not possible to be *for* science, and *against* change. If science requires anything, it requires the conscious assumption/belief/faith that there is a better way than any known today. It requires only the slightest stretch of imagination to recognize that science is the search for a better way. And a better way can be better only if it is *different*—only if it brings *change*.

Science requires a willingness/eagerness/desire to bring about progress/change. Therefore it is essential that engineering journals be administered in a manner that is not biased *against* the publication of papers that are out of the mainstream of thought—papers that threaten to bring progress/change.

The following narratives reveal that the manner in which engineering journals are presently administered *must* be changed because it results in bias *against* publication of articles that threaten to bring progress/change.

The bias results from the fact that the present method of administering engineering journals places life and death decisions about publication in the hands of a small number of editors and reviewers who feel that self-interest demands that they *preserve* the mainstream of thought, and *oppose* articles that threaten to bring progress/change.

In order to eliminate bias in the administration of engineering journals, editors and reviewers must be *abandoned.* Articles for publication must be *randomly and openly* selected. Peer pressure will ensure the quality of articles selected for publication, and expertise will no longer be judged by the number of articles a person has had published.

A1.1 A twenty-first century example that reveals why the present method of administering peer reviewed engineering journals *must* be changed.

A twenty-first century example that reveals why the present method of administering peer reviewed engineering journals *must* be changed concerns the Moody chart, a chart first published in "Friction Factors for Pipe Flow" by L. F. Moody, *ASME Transactions*, November, 1944. The Moody chart describes the pressure drop characteristics of fluids, and is used to calculate pressure drop, flow rate, or pipe diameter.

Since 1944, the Moody chart has appeared in virtually *every* mechanical and chemical engineering handbook, and *every* mechanical and chemical engineering text that concerns fluid flow. *Every* student in mechanical or chemical engineering is instructed in the use of the Moody chart.

The y axis of the Moody chart is dependent on pressure drop *and* flow rate, and the x axis is dependent on flow rate but not pressure drop. Therefore, if flow rate is given and pressure drop is to be determined, the chart can be read in a simple and direct manner because the value on the x axis can be determined from the given information.

But if pressure drop is given and flow rate is to be determined, the chart *cannot* be read in a simple and direct manner because the value on *neither* axis can be determined unless flow rate is given. The chart must be read in an iterative or trial-and-error manner.

In order that the Moody chart can be read in a simple and direct manner whether the flow rate is given and the pressure drop is to be determined, *and* conversely, the Moody chart must be transformed so that the y axis is dependent on pressure drop but *not* flow rate.

On April 4, 2004, I submitted a draft of *A Transformed Moody Chart That Is Read Without Iterating* for possible presentation at the 2004 ASME International Mechanical Engineering Congress. It was paper number IMECE2004-60213.

The session chairman was an assistant editor of the ASME *Journal of Fluids Engineering* (JFE). Those papers that were accepted for presentation, and were judged sufficiently important to warrant publication, would later be published in the ASME *JFE*.

On July 28, 2004 I withdrew the paper from IMECE consideration before a final decision was reached on acceptance/rejection. I was annoyed because the date for acceptance or rejection had passed, and I had not been informed of the decision.

Later, when the session chairman contacted me, he said the paper was going to be accepted for presentation, and he tried to discourage me from withdrawing the paper. I explained to him that I was withdrawing the paper in part because I was quite certain that, because I was the author, my paper had no chance of being published in the *JFE*.

On August 9, 2004, I received an e-mail from the session chairman. The e-mail stated:

Your IMECE conference paper looks suitable for the JFE and I am encouraging you to submit it to the JFE for consideration.

The session chairman/assistant editor of the *JFE* said he was quite certain that my paper would be seriously considered for publication in the *JFE*.

Because he was an assistant editor of the *JFE*, and because he insisted that my paper would receive serious consideration, I submitted it to the *JFE* in spite of my conviction that it would be judged unworthy of publication because I was the author.

On August 9, 2004, I mailed the paper to Professor Editor Joseph Katz (Johns Hopkins), ASME *Journal of Fluids Engineering*. On November 17, 2004, I again submitted the paper, this time by e-mail. (In response to my query, I had been told that the copy I mailed on August 9 was not received by *JFE*.)

On November 30, 2004, I received an e-mail in response to my e-mail in which I asked whether my paper had in fact been received by the *JFE*. The e-mail was from Laurel Murphy, Editorial Coordinator, *JFE*. The e-mail stated in part:

Yes, we received your manuscript, and Prof. Katz read it. However, he believes that the material does not fall within the scope of JFE. He suggests your manuscript would be better suited to a civil engineering journal with an audience of practicing engineers.

In an e-mail to Lauren Murphy, with a copy to Professor Katz, I responded to the *JFE* rejection. I pointed out that the Moody chart has been an important part of fluids engineering for 60 years, and that it appears in virtually all mechanical and chemical engineering handbooks and fluid flow texts.

I also pointed out that the initial publication of the Moody chart was in an ASME Journal, and the vastly improved form presented in my article would most appropriately be published in the *ASME Journal of Fluids Engineering*.

On 12/2/2004, Professor Katz sent me an e-mail stating that he was certain my article *"doesn't make it as archival material"*. Also, he claimed that I was angry and rude, and that my comment (that a civil engineering journal is not the proper place for my article) was inappropriate.

On 12/2/2004, I sent an e-mail to Professor Katz in which I responded to his e-mail of 12/2/04. I pointed out that my e-mails were frank, not rude. And I was disappointed, not angry. Also, my comment about civil engineering journals was innocuous, not inappropriate. I pointed out that my paper proposes that the friction factor concept be abandoned because it unnecessarily complicates the solution of practical problems.

I had been invited to speak on the new engineering at Howard University on 12/15/2004. (Johns Hopkins University is near Howard University.) I invited Professor Katz to my talk. He did not attend. I was disappointed, not surprised.

A1.2 Forty years of "minimizing the burden".

Professor Katz's response to my paper demonstrates that the manner in which engineering journals are administered was no different in 2004 than it had been in 1964. And by extrapolation, no better today.

Recall the following from David Miller's letter dated August 25, 1964:

. . . The purpose of informing Professor Editors Bliss (AIChE Journal) and Kezios (ASME Heat Transfer Journal) of our rejection of papers is to minimize the burden on the members of the professional community competent enough and honest enough to review work of others . . .

Professor Katz rejected my paper *without having it reviewed*, presumably in order to *minimize the burden on the members of the professional community competent enough and honest enough to review work of others.*

I seriously doubt that Professor Katz read my paper. If he had, it is not possible that he would have concluded that the paper did not belong in the *Journal of Fluids Engineering*. He didn't need to read my paper. He needed only to read as far as the byline in order to conclude that my paper should be rejected without review.

Professor Katz's rejection of my paper demonstrates that he was in fact NOT *competent enough and honest enough to review work of others.*

A1.3 Many engineering journals *reject* ads that propose progress/change.

It has been my experience that many engineering journals categorically *reject* ads that propose progress/change.

The ad on the next page is typical of the several ads I have tried to place in various engineering journals. The technical content in the ad on the next page is so simple, and so obviously correct, that there is no valid reason to reject it. Yet the ad was rejected by more journals than accepted it. For example:

- The ad was submitted to American Institute of Chemical Engineers for placement in *AIChE Journal* and *Chemical Engineering Progress Magazine*. It was REJECTED.

- The ad was submitted to Strojniski vestnik for placement in *Journal of Mechanical Engineering*: It was REJECTED.

- The ad was submitted to American Society of Engineering Education for placement in ASEE *Prism Magazine*. It was REJECTED.

The fact that many engineering journals do *not* accept my ads that promote progress/change, even though they would bring in thousands of dollars per page, concretely validates my conclusion that:

Papers that promote progress/change will have ***no chance*** *of being published in engineering journals until there is a drastic change in the way they are administered.*

thenewengineering.com

Dear Reader,

thenewengineering.com includes my papers and books about a new science of engineering that is conceptually and mathematically much simpler than conventional engineering.

The new engineering is conceptually simpler because it *abandons* all concepts that are ratios of primary parameters—concepts such as:

- "modulus", the ratio of stress to strain.
- "electrical resistance", the ratio of electromotive force to electric current.
- "heat transfer coefficient", the ratio of heat flux to boundary layer temperature difference.

The new engineering is mathematically simpler because, if these ratios are abandoned, problems can be solved with the primary variables kept *separate*, greatly simplifying the solution of problems that concern nonlinear behavior. (Just as in pure mathematics, x and y are kept *separate* in order to greatly simplify the solution of problems that concern nonlinear behavior.)

Note that, if "modulus" is used in the solution of a stress/strain problem, it is *impossible* to keep stress and strain separate because both stress and strain are implicit in "modulus".

In the new engineering, "modulus" is *never* used. Stress/strain problems are *always* solved with stress and strain *separate.* Similarly for "electrical resistance", "heat transfer coefficient", etc.

When concepts such as "modulus" and "electrical resistance" are abandoned, laws such as Young's law and Ohm's law serve no purpose, and are also abandoned.

Everything on thenewengineering.com can be downloaded for personal use without charge, including the book entitled *The New Engineering*.

Hardback copies of *The New Engineering* can be obtained by sending $39.95 (or equivalent) to Ventuno Press, 1094 Sixth Lane N., Naples, FL 34102. Hardback copies can also be obtained at book stores (ISBN 0-9626220-2-8).

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A1.4 Why the present method of administering engineering journals is unacceptable.

The sole purpose of engineering journals is (or should be) to promote progress. Progress *always* brings change. And change *always* means that old and less useful views and methodology must be discarded in favor of new and more useful views and methodology.

Whenever new views and methodology are presented, there are going to be "people with axes to grind" who are against new views and methodologies because they feel that self-interest lies in preserving the current mainstream of thought.

For more than fifty years, I have tried to arrange for the publication of articles that are out of the mainstream of thought. Therefore I am highly qualified to make a judgement about the administration of engineering journals with regard to articles that threaten to bring progress/change.

It is my judgement that the present method of administering engineering journals *must* be changed because the present method ensures that:

- Papers that threaten to bring progress/change will *not* be published if publication is opposed by a "responsible person" such as Professor Westwater (University of Illinois).

- Papers that threaten to bring progress/change will *not* be published if a "responsible person" such as Dr. Miller, (Argonne National Laboratory) is free to intimidate editors and request that they "*minimize the burden on the members of the professional community competent enough and honest enough to review work of others*" by rejecting, *without review*, papers that threaten to bring progress/ change.

- Papers that threaten to bring progress/change will *not* be published if groups of "responsible persons" (such as the Argonne Seven group) are free to intimidate editors and request that they "reevaluate their reviewing procedures" to make certain that no paper will find its way into the literature without first going through a review procedure that is certain to *kill* articles that threaten to bring progress/ change.

  (This refers to the storm of protest that greeted my first published article on the new engineering. The article was published in 1964 in *Nucleonics*, and seven "researchers" at Argonne National laboratory sent a letter to the editor of *Nucleonics* stating that my article "must either be a hoax, or your review procedures need to be changed".)

A1.5 A better way to administer peer reviewed journals.

What is needed is progress/change in the way engineering journals are administered. The new way must increase the likelihood that papers that threaten to bring progress/change will be published. Two changes are required:

- There must be *no* reviewers. If there are no reviewers, they cannot prevent the publication of papers that threaten to bring progress/ change.
- There must be *no* editors. If there are no editors, they cannot prevent the publication of articles that threaten to bring progress/change.

Reviewers and editors can and should be replaced by the *random and open* selection of papers for publication.

It is my serious and firm judgement, based on more than five decades of dealing with engineering journal editors and reviewers, that

The random and open selection of papers for publication will ***greatly*** *increase the likelihood that papers that threaten to bring progress/ change will be published.*

Peers will still have a role to play, but it will not be in deciding which papers are selected for publication. It will be in providing peer pressure so that contributors will be judged by the *quality* of their contributions rather than the *number* of their contributions.

H. G. Wells observed

It is the universal weakness of mankind that what we are given to administer, we presently imagine that we own.

The solution for this universal weakness is:

- Do *not* give the administration of engineering journals to *anyone or any group*.
- Select papers for publication by a random and open process (such as a lottery).
- Post selected papers on internet versions of journals.
- Print and distribute journals as desired.

Peer pressure should result in a vast *reduction* in quantity, and a vast *improvement* in quality. Hopefully the end result will be that, because of a reduction in quantity, no lottery will be required, and all articles that concern data will either include the data, or will specify the public documentation institute that is storing the underlying data.

Appendix 2

Dimensional analysis in conventional engineering, and in the new engineering.

In conventional engineering, dimensional analysis is used to *a priori* deduce which individual parameters should be used to form group parameters in proposed fluid generic correlations. The purpose of using group parameters rather than individual parameters is to minimize the number of parameters in fluid generic correlations, thereby minimizing the amount of data required to generate fluid generic correlations.

Dimensional analysis is described by the following:

- *Assume* it is rational for parameter symbols to represent numerical value *and* dimension.

- *Assume* it is rational to multiply and divide dimensioned parameters. (For 2000 years, scientists such as Euclid, Galileo, and Newton considered it *irrational* to multiply and divide dimensioned parameters.)

- *Assume* each parameter in a group parameter has the same exponent.

- *Assume* engineering phenomena are accurately described by correlations that consist of products of dimensionless groups of parameters raised to exponents.

- *Assume* all exponents in the correlations are *pure numbers*.

In the new engineering, dimensional analysis is *irrational* because there are *no dimensions* to be analyzed in rational parametric equations.

Appendix 3

My article entitled "Transition Boiling—The Relationship Between Heat Flux and Thermal Driving Force" submitted to the *AIChE Journal* on March 19, 1964, but rejected.

On March 19, 1964, I submitted an article entitled "Transition Boiling—The Relationship Between Heat Flux and Thermal Driving Force" to the *AIChE Journal*. The article was highly original and rigorously correct, and lists the following conclusions:

1. During transition boiling, the relationship of heat flux and temperature difference is highly *linear*.

   (The article analyzes data in Berenson (1960 and 1962), a benchmark experiment on transition boiling. Berenson reported data on 20 pool boiling curves. The data were plotted on log log paper, and straight lines drawn through the transition boiling region of 17 of the curves. It was not possible to draw a straight line through the transition boiling region 3 of the curves. Based on the 17 straight lines on log log paper, it was concluded that the relationship of heat flux and temperature difference is highly *nonlinear* in the transition boiling region.

   Due to the vagaries of log log paper, it was not noticed that there was essentially no data in the 17 runs, or that the only curves that had data throughout the transition boiling region were the 3 curves through which a straight line could *not* be drawn. When all 20 curves are plotted on linear paper, it is readily apparent that there is essentially *no data* in the 17 curves, and that the 3 curves that contain data throughout the transition boiling region indicate a highly *linear* relationship.)

2. Contrary to the graphical theory of thermal stability, so-called constant temperature boilers *cannot* necessarily operate at all points of the pool boiling curve. The graphical theory is based on the *erroneous* assumption that a *vertical* line on a graph of $q_{boiling}$ vs $T_{boiling\ interface}$ describes the relationship between heat flux into the boiling interface and the temperature of the boiling interface.

   A vertical line would apply only if the temperature difference between the heat source and the boiling interface were *zero*—ie in conventional engineering terms, only if the heat transfer coefficient on the heated face of the boiler wall were *infinite*, *and* the thermal conductivity of the boiler wall were *infinite*.

Rohsenow (1963) describes the graphical theory in the following:

With condensing vapor as the heat source on one side of a wall, any point on the entire (pool boiling) curve can be reached under stable conditions.

3. The lack of data in the transition boiling region strongly supports the theory of thermal stability formulated by Adiutori. (The reference is to my article in *Nucleonics.*)

4. The design of constant temperature boilers intended to operate in the transition boiling region must consider the requirements of thermal stability.

The article should have been accepted for publication, but it was not.

The letter of transmittal states

The manuscript is a reanalysis of a thesis by Berenson which has gained rather widespread acceptance in the last few years. Since I reach several conclusions which are diametrically opposed to the original conclusions, it would seem advisable to have the manuscript reviewed by either Berenson or someone at MIT where the work was done. Berenson's adviser was Peter Griffith.

The review by Professor Peter Griffith (MIT) is on the next two pages. The article is on pages 257-271.

Review 2

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF MECHANICAL ENGINEERING
CAMBRIDGE 39, MASSACHUSETTS

April 13, 1964

Professor Bliss
A I Ch E. Journal
Yale University
Sterling Chemistry Laboratory - Room 156
225 Prospect Street
New Haven, Connecticut

Dear Professor Bliss:

I believe this man is driving a tack with a sledge hammer. Nothing he says is untrue but I don't think he needs this much paper to say it. A written comment on the paper or a letter to the editor of a journal is sufficient.

In the body of his paper he points out that a combination condenser-boiler such as Berenson used is unstable. This is true but it is fail safe so there is little to worry about. What Berenson did not make clear, and what most of his paper is about, is that with a significant thermal resistance due to the condensing film or the conductor from the condenser to the boiler, there are temperatures differences in the transition boiling regime which are unattainable in a given apparatus.

I hope this comment assists you in evaluating his paper.

Sincerely,

Peter Griffith
Associate Professor of
Mechanical Engineering

PG/C

Dear Professor Bliss,

I believe this man is driving a tack with a sledge hammer. Nothing he says is untrue, but I don't think he needs this much paper to say it. A written comment on the paper or a letter to the editor of a journal is sufficient.

In the body of his paper he points out that a combination condenser-boiler such as Berenson used is unstable. This is true but it is fail safe so there is little to worry about. What Berenson did not make clear, and what most of his paper is about, is that with a significant thermal resistance due to the condensing film or the conductor from the condenser to the boiler, there are temperature differences in the transition boiling regime which are unattainable in a given apparatus.

I hope this comment assists you in evaluating his paper.

6/9/64

March 18, 1964

Transition Boiling--The Relationship Between Heat Flux and Thermal Driving Force

by

Eugene F. Adiutori, President
Stability Consultants
Box 18062
Cincinnati, Ohio 45218

ABSTRACT

The relationship between heat flux and thermal driving force has a very strong influence on the stability of boiling equipment in general and on pool boilers in particular. This article examines some of the pool boiling data reported in the literature and concludes that:

> In the transition boiling region of the pool boiling curve, the relationship between heat flux and thermal driving force is highly linear.

This examination also indicates that so-called constant temperature boilers are not necessarily stable at all points of the pool boiling curve and that special care must be taken in the design of such boilers in order to enhance their stability.

Reviewed by P. Griffith -- see his negative review dated April 15, 1964 -- "temp diffs which are unattainable" - letter is attached to cover letter by Bliss dated 5/5/64

Submitted to H. Bliss on 3/19/64 -- acknowledged by Bliss on 3/24/64 --see also Bliss of 4/3/64 -- negative decision on 5/5/64

see also negative pompous review by F. Kreith -- ~~mixed book~~

INTRODUCTION

The transfer of heat to a pool of boiling liquid is often classified into the following three categories:

1. Nucleate boiling
2. Transition boiling
3. Film boiling

In this article, we will deal with only transition boiling which we will define in the following operational sense:

> Transition boiling is the transfer of heat to a boiling liquid and is characterized by the heat flux being <u>negatively</u> correlated with the thermal driving force. Alternately, transition boiling refers to the boiling process when equation (1) is satisfied:
>
> $$d(q/A)/d\,\Delta T < 0 \qquad (1)$$

The specific purpose of this article is to determine the nature of the relationship between heat flux and thermal driving force during transition boiling. This will be done in an entirely empirical manner--i.e. we will rely only on the experimental evidence quite aside from any theories about the nature of the boiling process. Moreover, this article offers no theory to explain the nature of the relationship indicated by the data.

Transition boiling has been investigated by a number of investigators, the most recent being the experiments by Berenson (1). Because of the excellent precision of Berenson's data and because he controlled so many of the variables which are often uncontrolled, we will deal only with his data. However, because of the depth of his experiments, it seems reasonable to expect the result so obtained to retain a certain degree of generality.

Berenson's data analysis indicated that, during transition boiling, the relationship between heat flux and thermal driving force was highly non-linear. As described below, this result was obtained (in spite of the fact that the data was highly linear) for two reasons:

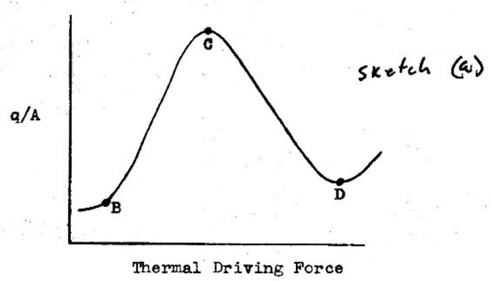
1. At the time of Berenson's experiment, there was a graphical theory of thermal stability which was incorrect. This theory held that so-called constant temperature boilers could necessarily operate stably throughout the transition boiling region. As a result, Berenson concluded that his data defined the shape of the transition region whereas, in point of fact, he had obtained very little data in the transition boiling region. From the manner in which the data was presented, it would seem that Berenson's equipment could not operate stably in most of the transition boiling region.*
2. Due to the vagariousness of log-log graph paper, the lack of data in the transition boiling region was not apparent.

The present analysis of Berenson's data indicates that, in the transition boiling region, the heat flux is linearly related to the thermal driving force.

* A detailed discussion of the stability of Berenson's equipment is beyond the scope of this article. The interested reader is referred to the quantitative theory of thermal stability formulated by Adiutori (2).

BACKGROUND

The shape of the pool boiling curve shown in sketch (a) was first observed by Nukiyama (3).



From the above definition of transition boiling, it can be seen that this type of boiling takes place in region CD of sketch (a). (The precise shape of the curve in region CD is the subject of this article and the above sketch is intended primarily to indicate the presence of the maximum and the minimum in the curve.)

At the time of Berenson's experiment, there was a graphical theory of thermal stability which led to certain conclusions about the stability of pool boilers. With respect to the stability of so-called constant temperature boilers, this theory resulted in the following widely accepted conclusion as stated by Rohsenow (4):

> With condensing vapor as the heat source on one side of a wall, any point on the entire curve (referring to the pool boiling curve as shown in sketch (a)) can be reached under stable conditions.

Since this was precisely the type of boiler used by Berenson, it seemed reasonable to conclude that:

1. It was not necessary to design the boiler with stability in mind since it would be inherently stable.

2. It was not necessary to verify that sufficient data had actually been obtained in the transition boiling region.

As a result of the second conclusion, the fact that many of the runs had virtually no data in the transition region went unnoticed. The fact that the results were plotted on log-log graph paper also contributed to obscuring the lack of data.

ANALYSIS

Berenson's original analysis of his equipment and experiment are best illustrated by his following statements:

> 1. . . . an experiment designed to control the temperature difference allows operation within the transition region, as well as the other two regions, since there is only one value of heat flux associated with each value of temperature difference.
>
> 2. Enough datum points were measured in each run to define the characteristic boiling curve completely.
>
> 3. It was found, with the exception of some of the data presented in Figure 5, that the transition boiling data lie along a straight line connecting the burnout point and the film-boiling minimum point on log-log graph paper. This is also true of the transition boiling data obtained by Braunlich (5) and Kaulakis and Sherman (6).

Berenson's first statement agreed with the existing theory of thermal stability but is disproved by the new theory of thermal stability (2). (The new theory can be used to prove that three values of heat flux can be associated with each value of temperature difference rather than only one value. The key to this seeming riddle is that the operator can control only the overall temperature difference and not the temperature difference corresponding to the thermal driving force between the heat transfer surface and the boiling liquid.)

Berenson's second statement indeed appears to be true when the data is plotted on log-log graph paper (cf. Berenson's report and also page 119 of reference (4)). However, as shown in Figures 1 and 2, some of his runs failed to establish the shape in the transition region. For instance, Figure 1 contains transition boiling data in the range of heat fluxes between 3,000 and 8,000 Btu/hr and no data in the range 8,000 to 90,000 Btu/hr. Thus, in this run, there is no data in over 90% of the transition boiling region! However, as shown in Figures 3 and 4, some of Berenson's runs did indeed establish the complete shape of the pool boiling curve. Thus, Berenson's runs may be classified into two categories:

1. Those runs which had essentially no data in the transition region.
2. Those runs which contained a more or less continuous set of data throughout the transition region.

We have now to determine which type of run was used to obtain the result that

> The transition boiling heat transfer data plotted as log (q/A) vs. log (ΔT) was found to be correlated by a straight line connecting the maximum and minimum heat flux.

Berenson's report contained data for a total of twenty runs. Of these, seventeen could be correlated as Berenson describes above and the remaining three could not be correlated in the above manner--i.e. a straight line could not be drawn through three of the runs when the data were plotted on log-log graph paper. In his third statement above, Berenson is referring to these three runs when he states "with the exception of some of the data . . . ". Thus, Berenson recognized that there were two groups of runs--a large group which could be correlated by straight lines on log log paper and a small group which could not be correlated in this fashion.

Figures 1 and 2 are taken from the large group which seemed to correlate well on log log graph paper and Figures 3 and 4 are taken from the small group which did not correlate. From this, it can be seen that the group that correlated well was the group that contained essentially no data in the transition region! And that the small group which did indeed contain the desired data could not be correlated by a straight line on log-log paper! Moreover, it can be seen from Figures 1 through 4 that:

1. When data was obtained in the transition region, this data was highly linear and would not be expected to exhibit a straight line on log-log graph paper.
2. When data was largely not obtained in the transition region, it is of course not possible to state the relationship between heat flux and thermal driving force. However, Figures 1 and 2 seem to

suggest that the unmeasured data would also have exhibited a linear relationship between the parameters.

In summary, the above analysis leads to the conclusion that, during transition boiling, the heat flux is linearly related to the thermal driving force. It also indicates that so-called constant temperature boilers are not necessarily stable in the transition region.

DISCUSSION

There are a number of reasons why Berenson's transition boiling data are extremely important. One of these is the fact that his data strongly indicated that the graphical theory of thermal instability was incorrect, since he apparently was not able to obtain data at all points of the pool boiling curve. This inability to operate in certain regions was in direct contradiction to the prevalent theory of thermal stability. Moreover, this result of Berenson's serves as virtually indisputable evidence of the rationale of the new theory of thermal stability formulated by Adiutori (2).

(It should be noted that the above states only that Berenson apparently was not able to operate in certain regions of the transition region. This qualification is required as a result of the fact that the data published by Berenson is only part of the data he obtained on the boiler. To state definitively that the absence of data was a result of the inability to operate the equipment in certain regions requires some additional data not included in reference (1). This latter point is somewhat aside from the present discussion and will be taken up in a yet unpublished article on hysteresis by the present author.)

Another important result of Berenson's experiment is the realization that a conscious effort must be made in the design stage in order to result in a highly stable boiler. By assuming that any constant temperature boiler would be stable in the transition region, one runs the danger of overlooking the many various design techniques which can be utilized to improve the stability of even a constant temperature boiler. This point is becoming increasingly important because of the space program and the emphasis being placed on boiling heat transfer to liquid metals. There are several current programs devoted to the construction and operation of pool boilers in the transition region in order to obtain transition boiling data on liquid metals. Unless these boilers are designed specifically to enhance stability, there is the danger that they also will not be able to operate in this region of transition boiling in spite of the fact that they are constant temperature boilers.

The pronounced linearity evident in Figures 3 and 4 is particularly important from the standpoint of stability. As described in reference (2), the stability of a boiler operating in the transition region is determined in large part by the slope of the heat flux vs. thermal driving force curve. Thus, the linearity suggests that all portions of the transition boiling region exhibit approximately the same tendency toward stability or instability. The result obtained by Berenson concluding that the data could be correlated by a straight line on log-log graph paper would suggest a very large, negative slope at the upper end of the transition region. Drawn in this fashion, such a line would overestimate the value of the slope by perhaps an order of magnitude. The end result would have been to indicate the virtual impractability of constructing a boiler which was everywhere stable in the transition region.

A less obvious result of Berenson's experiment is the extreme simplicity of the relationship between heat flux and thermal driving force throughout the nucleate, transition, and film boiling regions. Inspection of Figures 3 and 4 showsthat the shape of the resulting curve is similar to curves experienced in several branches of engineering and that it should be possible to represent the entire curve with a single correlation covering the entire range of boiling (rather than breaking it up into pieces for correlation purposes). However, before this is done, it would seem that more data should be obtained in the transition region in order that the resulting correlation could be more heavily founded on experimental evidence.

CONCLUSIONS

The reexamination of Berenson's data supports the following conclusions:

1. During transition boiling, the relationship of heat flux to thermal driving force is highly linear.
2. Contrary to the graphical theory of thermal stability, so-called constant temperature boilers can not necessarily operate at all points of the pool boiling curve.
3. The lack of data in the transition boiling region strongly supports the theory of thermal stability

formulated by Adiutori (2).

4. The design of constant temperature boilers intended for operation in the transition boiling region must consider the requirements of thermal stability.

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4. Modern Developments in Heat Transfer (1963) by Warren Ibele, Heat Transfer with Boiling by Warren M. Rohsenow
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6. Kaulakis, E. G. and Sherman, L. M., Thesis, Mass. Inst. Technology, (1938)

SYMBOLS

q	heat
A	area
ΔT	thermal driving force

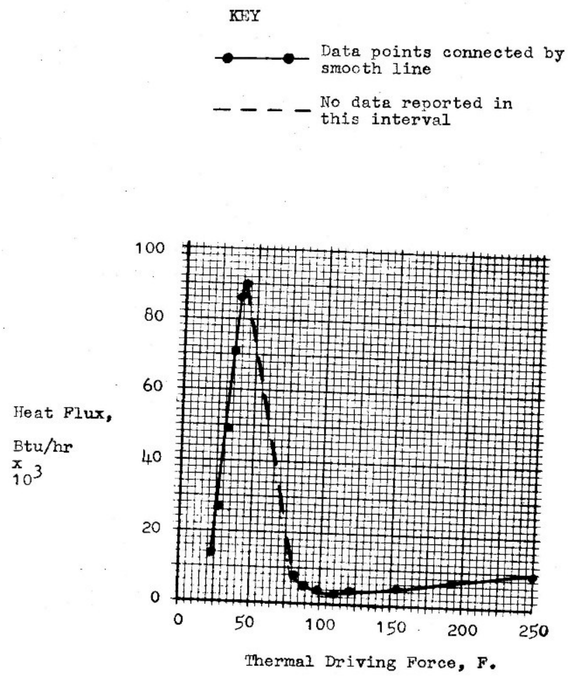


FIGURE 1

Nucleate, Transition, and Film Boiling

(Data by Berenson (1), Run No. 31)

KEY

Data points connected by smooth line

No data reported in this interval

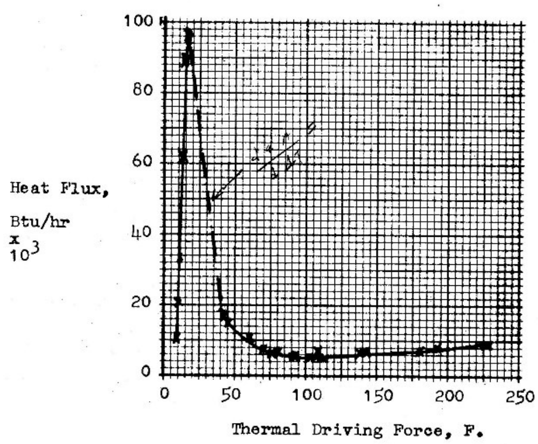


FIGURE 2

Nucleate, Transition, and Film Boiling

(Data by Berenson (1), Runs 17 & 22)

KEY

Data points connected by smooth line

No data reported in this interval

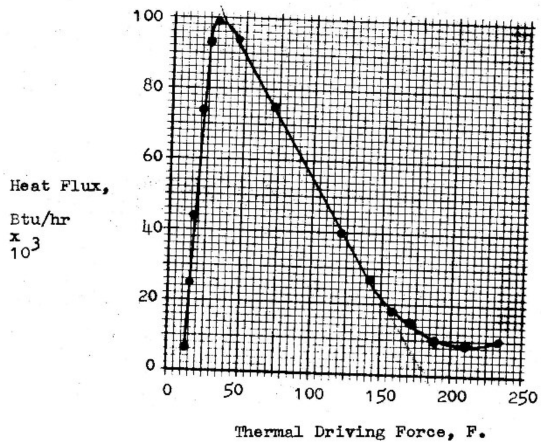


Figure 3

Nucleate, Transition, and Film Boiling

(Data by Berenson (1), Run No. 7)

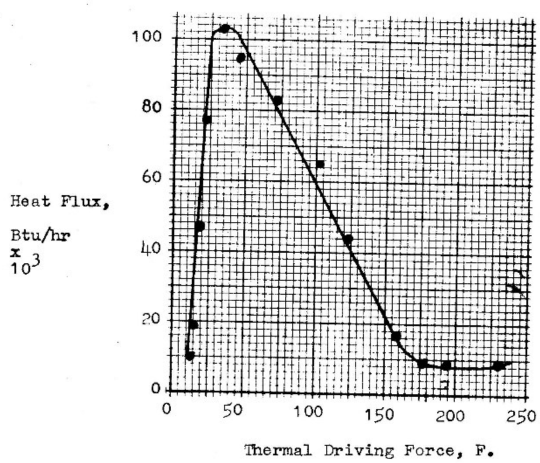


FIGURE 4

Nucleate, Transition, and Film Boiling

(Data by Berenson (1), Run No. 9)

Appendix 4

The purpose of Chapters 17 to 21.

Chapters 17 to 21:

- Validate my conclusion that the manner in which engineering journals are administered *must* be changed because it results in bias *against* publication of articles that threaten to bring progress/change.

- Validate my recommendation that the change in administration include *elimination* of editors and reviewers, and the use of a *random and open procedure* to select articles for publication.

- Serve as a biography of the new engineering. They document the genesis and promotion of the new engineering, and its retarded growth due to the unwarranted resistance of members of academia who unfortunately administer and control American engineering journals.

- Document that, since 1963, I have unceasingly strived to bring about the global acceptance of the new engineering. My failure to accomplish even miniscule acceptance does *not* reflect a lack of effort.

Appendix 5

Documentation

New Theory of Thermal Stability in Boiling Systems

A quantitative derivation of the criterion for thermal stability provides explicit guides to avoid unstable conditions; it also indicates that the traditional graphical analysis may yield erroneous results

by **EUGENE F. ADIUTORI,** *Stability Consultants, Cincinnati, Ohio*

I have derived a quantitative criterion for thermal stability that affords greater insight into the behavior of reactors and boilers than has previously been available with the old graphical method. The new theory presents a quantitative description of the phenomenon known as burnout and promises to be of direct practical significance in the design of both liquid-cooled reactors and boiling systems in the nuclear field. Among other things it leads to the realization that boiling-heat-transfer coefficients are not very helpful in designing boilers and suggests that the boiling data available in the literature be reanalyzed to correlate the heat flux as a function of the thermal driving force. Within the limits of the space available, I will outline the development of the quantitative theory (detailed proofs are omitted at several points) and demonstrate its application to reactor design with emphasis on once-through liquid-metal boilers—a type of boiler that is becoming prominent in the nuclear space-power program and in which the thermal stability problem is especially crucial.

The practical importance of thermal stability (see box) has been recognized since 1934 when Nukiyama (*1*) first observed the "pool boiling curve" shown in Fig. 1a. Since that time, the theory of thermal stability has centered about the graphical demonstration that, if the curve in Fig. 1a is replotted for a monotonically increasing heat flux, the wall temperature will be discontinuous, as shown in Fig. 1b. This discontinuity in the wall temperature is called "burnout" due to the fact that the wall temperature at point C' often exceeds the melting point of the wall material.

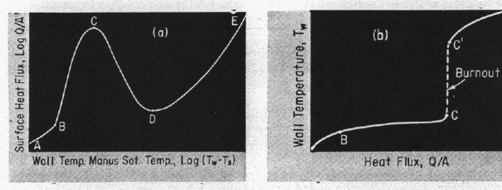


FIG. 1. POOL BOILING CURVE (a) replotted in terms of wall temperature (b) shows discontinuity that in graphical method is assumed to be region of burnout

The above graphical theory does not rigorously apply to real systems and has led to a number of erroneous conclusions about thermal stability. The quantitative theory outlined in this article allows us to see that the following widely accepted conclusions are not always true:

- In reactors and other "fixed heat input" systems, burnout occurs at point C in Fig. 1a.
- Systems in which the temperature of the heat source is controlled necessarily possess thermal stability. In such systems, there is no phenomenon similar to burnout.
- Thermal instability leads to temperature discontinuities but does not lead to oscillatory performance.

These erroneous conclusions have fostered a number of related erroneous conclusions in the fields of reactor design, boiler design, evaporator design, heat transfer to cryogenic and refrigerant fluids, design of heat-transfer test sections and experiments, and correlations of experimental results. Unfortunately, it is not possible to treat each of the above subjects in the space available. Our major emphasis will be on the application of thermal stability to the design and analysis of reactors and reactor plant systems with

Thermal Stability—What Is It?

Heat transfer is a process that involves the flow of heat from a source to a sink. As long as the heat flow leaving the source is just equal, on an instantaneous basis, to the heat flow entering the sink, the system is thermally stable. When this equality is not satisfied, a finite quantity of heat is necessarily stored up in the system or withdrawn from it. This change in heat content can result in either:

- The phenomenon known as burnout in which excess heat builds up to the point where it can cause melting of a reactor fuel element.
- Oscillatory behavior caused by the alternating sign of the heat storage term.

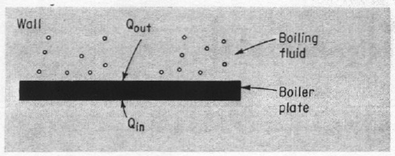


FIG. 2. IDEALIZED thermal system with no net heat flow provides basis for establishing stability criterion of Eq. 9

particular attention to once-through liquid-metal boilers of the kind being considered for use in space.

Definition of Stability

Thermal systems may be classified into two broad categories:

- Systems with no net circulation (such as pool boilers) (Fig. 2)
- Systems with net circulation (such as liquid-metal power plants or forced-convection reactors)

In a practical sense, we are more concerned with the latter group. However, both are thermal systems and because the first type is much simpler, the quantitative derivation will be based on the first type. The results will then be extended to obtain conclusions about the second type of system. In addition, for each of the above systems if the temperature of the heat source is controlled, the system is usually referred to as a "constant temperature" system. If some other attribute of the heat source is controlled, the system is usually referred to as a "fixed heat input" system. Although these conventions will be used throughout this article, it must be emphasized that they are often misleading and are used here only for convenience.

To develop the quantitative theory we will begin by defining thermal stability and then analyze a simple system to determine under what conditions it is not stable. By analyzing a simple system, it is possible to obtain a simple stability criterion and determine the salient features of thermal stability without the cumbersome complication required when treating a complex system. Through an understanding of the salient features of thermal stability, the theory can easily be extended to complex systems. Moreover, this understanding leads to the realization that the designer, through the use of simple techniques, can vastly improve the thermal stability of heat-transfer equipment such as reactors and boilers.

An adequate definition of thermal stability would seem to be the following: A heat source (such as the boiler plate in Fig. 2) is thermally stable at a particular temperature $T(0)$ provided that, when $T(0)$ is perturbed to $T(0) + \Delta T$, the following relationship is satisfied:

$$T(t = \infty) = T(0) \tag{1}$$

If the result of the perturbation does not satisfy Eq. 1, the heat source is thermally unstable.

The above definition strongly suggests that stability be appraised by perturbing the temperature of the heat source and observing its response.

If the temperature of the boiler plate in Fig. 2 is perturbed from some initial value, it may respond in any of the ways shown in Fig. 3. If the system being analyzed behaves like Curves d, c, and e in Fig. 3 (i.e., dT/dt does not change sign anywhere in the interval $0 < t < \infty$), the stability can be easily appraised by determining the sign of dT/dt. For these idealized cases, the stability criteria become:

- the system is stable if dT/dt is opposite in sign to ΔT
- the system is unstable if dT/dt is of the same sign as ΔT
- the system is unstable if $dT/dt = 0$.

It is unfortunate that real systems do not necessarily behave like Curves d and e. However, instability can be most easily analyzed by idealizing the real system in such a way that it behaves like Curves d and e. After the idealized problem is solved, it becomes an easy matter to remove the idealized aspects and solve the real, physical problem.

It will be noted that the above approach is followed throughout this article. Without this simplification, we would almost certainly become bogged down with the period and amplitude of the oscillations which occur in the interval $0 < t \infty$ and which would not help us appreciate or identify the real problem.

Derivation of Criterion

The above discussion was partly intended to illustrate that thermal stability criteria can be simply stated only for simple systems. Toward this end,

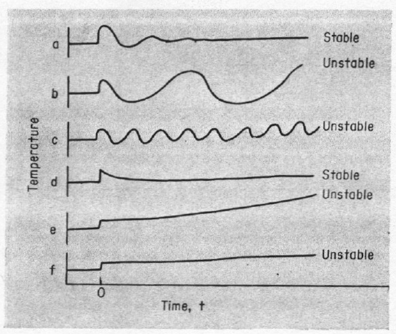


FIG. 3. MODES OF TRANSIENT RESPONSE for idealized system shown in Fig. 2. All are unstable except Modes a and d

FIG. 4. UNSTABLE REGIONS of hypothetical pool boiling curve (below) as defined by criterion of Eq. 9 when Eq. 10 is satisfied

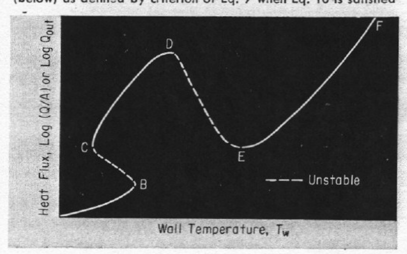
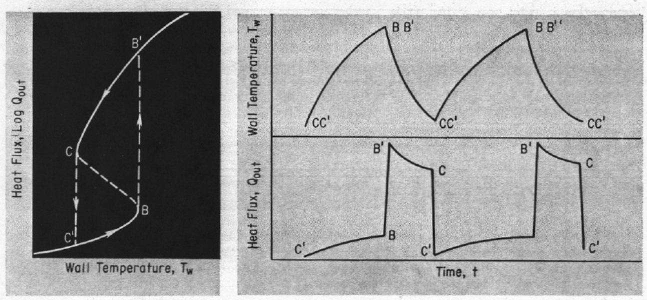


FIG. 5. OSCILLATIONS. Slight perturbation of system operation on curve in Fig. 4 can yield boiling curve at right and resulting system oscillations (far right)



let us now analyze a very simple system which is amenable to hand calculation. However, we must bear in mind that the resultant criterion will be applicable only to systems that closely resemble the idealized system. The system to be analyzed is the pool boiler shown in Fig. 2 and it is idealized by assuming that:

- the temperature of the boiling fluid is constant—i.e., is unaffected by Q_{out}
- the thermal conductivity of the boiler plate is infinite
- the heat capacity of the boiler plate is finite.

In the above idealized system, the parameter we wish to investigate is the temperature of the boiler plate. As suggested by the definition of stability, we shall perturb the temperature and determine under what conditions the temperature is stable. The analysis is based on noting that

$$Q_{in}(t) - Q_{out}(t) = Q_{stored}(t) \quad (2)$$
$$Q_{stored}(t = 0) = 0 \quad (3)$$

When the boiler plate temperature T_w is perturbed a differential amount from some initial value at which Eq. 3 is satisfied,

$$Q_{out}(t) = Q_{out}(\theta) + (dQ_{out}/dT_w)[\Delta T_w(t)] \quad (4)$$
$$Q_{in}(t) = Q_{in}(\theta) + (dQ_{in}/dT_w)[\Delta T_w(t)] \quad (5)$$

Therefore, from Eqs. 2, 3, 4, and 5,

$$Q_{stored}(t) = [\Delta T_w(t)](dQ_{in}/dT_w - dW_{out}/dT_w) \quad (6)$$

Now

$$Q_{stored}(t) = (dT_w/dt)(C_{bp}) \quad (7)$$

where C_{bp} is the total heat capacity of the boiler plate.

From Eqs. 6 and 7

$$dT_w/dt = (1/C_{bp})(dQ_{in}/dT_w - dQ_{out}/dT_w)[\Delta T_w(t)] \quad (8)$$

From Eq. 8, it can be seen that $T_w(0)$ is stable *only* if

$$(dQ_{in}/dT_w - dQ_{out}/dT_w) \underset{s}{<} 0 \quad (9)$$

since this is the only condition under which $T_w(t)$ will tend toward $T_w(0)$, thus satisfying the definition of stability. Equation 9 also demonstrates that the graphical theory is quantitatively correct only in those rare cases in which

$$dQ_{in}/dT_w = 0 \quad (10)$$

Equation 9 is the stability criterion for the idealized system. For the case where Eq. 10 is satisfied, Eq. 9 indicates that the boiler plate temperature would be unstable everywhere in intervals BC and DE of Fig. 4.

Fig. 4 shows that, if the system is initially operating in regionDE, a slight perturbation will cause it to translate to either region CD or EF where it will operate in a stable manner. If the system is initially operating in region BC, a slight perturbation will cause it to translate and it will begin to oscillate (because it cannot "find" a stable condition) as shown in Fig. 5.

Equation 9 applies equally to both "fixed heat input" systems and "constant temperature" systems. The difference in stability between the two types of systems is only one of degree—dQ_{in}/dT_w is generally more negative in constant temperature systems, resulting in greater thermal stability as may be seen from Eq. 9. Thus, burnout does indeed occur in both types of systems and the only significant difference between the two types is that, at burnout the temperature cannot increase without limit in a constant temperature system.

To date, burnout has occurred in constant-temperature pool boilers but seems to have gone unnoticed. However, it has often been observed in forced convection systems, but is usually referred to as a "dry-wall" phenomenon caused by a change in flow regime. Equation 9 demonstrates that this dry-wall phenomenon is simply burnout in a constant-temperature system, and is not caused by a change in flow regime. The fact that a change in flow regime has been visually observed to occur in coincidence with the dry-wall phenomenon would seem to indicate that the heat transfer is affecting the flow regime, rather than the converse.

Applications to Design

Liquid-cooled reactors are normally designed to avoid burnout for obvious reasons. This is accomplished by empirically correlating the results of burnout experiments in which the heat source is electrical rather than nuclear. In the early days of burnout experiments, the burnout point was detected by the physical destruction of the test section. Later, "burnout detectors" were utilized to sense the changing temperature of the heat source and then quickly reduce the power to preserve the test section. The difficulty with both of these methods was that they *assumed* that the burnout heat flux in a reactor would be the same as that in an electrically heated test section. Inspection of Eq. 9 indicates that this assumption is valid only if Eq. 11 is satisfied:

$$(dQ_{in}/dT_w)_{test\ section} = (dQ_{in}/dT_w)_{reactor} \quad (11)$$

The left hand side of Eq. 11 is primarily a function of the temperature coefficient of electrical resistivity of the test section material, while the right hand side is a function of the temperature coefficient of reactivity of reactor fuel and moderator; hence it seems highly unlikely that Eq. 11 would ever be satisfied.

The latest design concept replaces burnout with DNB (Departure from Nucleate Boiling). DNB occurs before burnout and is detected by observing the decrease in dQ_{out}/dT_w which occurs prior to burnout. By using this conservative approach in the experiments, the effective result is the same as assuming that the right hand side of Eq. 11 has a large, positive value. In most water cooled reactors, one could expect the right hand side of Eq. 11 to have a negative value, leading to the conclusion that the DNB design concept is indeed conservative. Moreover, by determining the magnitude of dQ_{in}/dT_w for a given reactor one could appraise the degree of conservatism inherent in all the above methods.

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 in Fig. 2 it would be impossible to avoid the temperature discontinuity shown in Fig. 1b. Equation 9 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. The required value would be whatever is necessary to satisfy Eq. 9 at all points of the pool boiling curve. A number of other equally simple techniques can be used to improve the stability of reactors and other types of heat transfer equipment.

Reactor instability. As we have seen above, under certain conditions thermal instability can result in the oscillatory performance of a pool boiler. If these same conditions exist in a reactor, the reactor also can be expected to operate in an oscillatory manner through the following reasoning:

- If the subcooled region of a reactor channel exhibited the behavior shown in region BC of Fig. 4, the heat flux into the coolant would be expected to oscillate as shown in Fig. 5.
- As a result of the oscillations in the heat flux, the enthalpy of the coolant leaving the channel would oscillate.
- If the coolant leaving the channel is only slightly subcooled, the oscillations in enthalpy would cause the void fraction in the channel to oscillate.
- The oscillation in void fraction would cause the hydraulic resistance of the channel to oscillate, thus resulting in an oscillatory channel flow rate. The amplitude of such oscillations might go unnoticed in a forced convection reactor operating at fluid velocities as high as 15–30 ft/sec. However, in a free convection reactor operating at 1–3 ft/sec, the relative amplitude would be much greater and the oscillations easily detected.
- If a sizable fraction of the channels operate near zero subcooling, they would soon begin to oscillate together by virtue of the fact that they are coupled through the reactor kinetics and the reactor flow rate. Moreover, this sizable fraction would cause the reactor power to oscillate in response to the oscillating void fraction in the reactor core. In the same manner, the reactor flow rate might begin to oscillate.

A region such as BC of Fig. 4 would be expected to occur near the inception of boiling, if at all. Now, for water-cooled systems, there is very little evidence in the literature to suggest a region such as BC. However, as pointed out by Bergles and Rohsenow (*2*),

> Investigators frequently neglect to include data for the transition region, or knee of the boiling curve, which is always present between incipient boiling and fully developed boiling.

Moreover, Ref. *2* presents data for a pressurized water system in which the curve of heat flux vs. wall temperature is exactly like region ABCD of Fig. 4. In conjunction with this data (Fig. 8 in Ref. 2), the authors state

> It appears to be no coincidence that the wall temperatures behave strangely when the first bubbles grow.

as predicted by the above theory of thermal stability.

The data of Corty and Foust (*3*) also indicate a region similar to BC of Fig. 4. They found that, in non-aqueous systems, regions similar to BC would occur at the inception of boiling if

> all active (nucleating) centers are thoroughly snuffed out before heat transfer is increased again.

Upon increasing the heat flux from the above condition, they found that

> Superheats of 40–50°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.

The results of Corty and Foust thus demonstrate the existence of a BC region for non-aqueous pool boilers, but indicate that the region exists only for those cases in which the active centers are initially snuffed out. It seems reasonable to expect that a forced convection system would supply this snuffing action and that region BC would exist in the steady-state, as suggested by the results of Ref. *2*.

In summary, there is a small amount of data that suggests reactor power oscillations may be thermally induced in the manner described by the above theory. Whether this is the major contributor to reactor instability cannot be determined without extensive further analysis. The thermal basis

Comparison of Burnout in Constant-Temperature and Fixed-Heat-Input Systems

	Burnout result
Fixed-heat-input system	Step increase in surface temperature accompanied by change in heat flux
Constant-temperature system	Step decrease in surface heat flux accompanied by increase in surface temperature

for reactor instability is at least suggested by the following excerpt from Ref. *8* by Anderson and Lottes

> Borax IV was different from the other Borax reactors in its fuel assembly material and design. Whereas the first three reactors had used aluminum-uranium alloy fuel elements with a corresponding large thermal conductivity, Borax IV used fuel pellets of mixed uranium oxide and thorium oxide having a low thermal conductivity. Borax IV, with these fuel elements, exhibited a greater degree of stability under similar conditions than did the other Borax reactors. These elements can apparently contribute to reactor stability under certain conditions.

Liquid-metal boilers. Liquid metal boilers (with the exception of pool boilers) are extremely rare. To properly design a once-through liquid-metal boiler to operate at high exit quality, it is essential to gain a thorough understanding of thermal stability. Such a boiler is unique in two aspects, both of which are intimately related to thermal stability

- Their boilers have high exit quality—usually avoided in ordinary boilers.
- Boiling liquid-metal systems exhibit the features of thermal instability to a larger degree than any other class of systems.

The liquid-metal boilers for space will probably be of the constant-temperature type with the heat being supplied by a primary fluid that circulates through a reactor. The high exit quality proposed for the boiler will distinguish it from most common boilers, which are usually designed to operate at low quality followed by a drying stage. By operating in the low quality region, it has been possible to avoid the "dry-wall" phenomenon mentioned briefly above. This phenomenon is the direct result of thermal instability rather than the result of a change in flow regime. Moreover, it is the direct parallel of burnout in fixed heat input systems as shown in the table.

The step decrease in surface heat flux referred to in the table is well demonstrated in the results of Berensen (*4*) presented in Ref. *5*. In Berensen's constant-temperature system, burnout was accompanied by a decrease in surface heat flux from an initial value of 100,000 Btu/hr/ft^2 to values ranging from 4,000 to 15,000 Btu/hr/ft^2 for different surface finishes.

Probably the major reason why the true nature of the dry-wall phenomenon has not been generally recognized to date is the widespread acceptance of correlating heat-transfer coefficients as a function of vapor quality in forced convection systems. With such a correlation, it is extremely difficult to appraise the thermal stability of the system because dQ_{out}/dT_w cannot be easily determined from a correlation which describes the relationship between heat-transfer coefficient and vapor quality. The use of heat-transfer coefficients does no particular harm if the boiler is being designed to operate in a region remote from thermal instability (such as at low vapor quality). However, in a once-through boiler operating at very high exit quality, the use of heat-transfer coefficients masks the most important effect—thermal stability—and such a boiler should be designed on the basis of heat flux rather than heat-transfer coefficients. Indeed, for similar reasons, all forced convection boiling data should be correlated on the basis of heat flux and thermal driving force, and all boilers should be designed on these bases also.

This problem of burnout in the boiler is one of the major problems confronting the design of the liquid-metal boiler. Heretofore, the problem has simply been circumvented by the use of a drying stage. Indeed, the problem of burnout in a constant temperature boiler has previously had so little practical importance that most experimenters correlate their results only in the region removed from burnout and there seem to be no correlations which apply to the dry-wall phenomenon. (Indeed, it would be virtually impossible to obtain such a correlation by correlating heat-transfer coefficient as a function of quality.) However, several experimenters are investigating the use of swirl devices to improve the thermal stability of the high quality region of the boiler. The results obtained by Gambill et al (*6*) on the use of such devices indicate that they will indeed improve thermal stability.

The conclusion that boiling liquid-metal systems exhibit the requirements for thermal instability to a very high degree is obtained through inductive reasoning as follows:

- Liquid-metal pool boilers have often been observed to operate in an oscillatory manner.
- If such a boiler operates in an oscillatory manner, it seems reasonable to conclude that the oscillations are thermally induced.
- If the oscillations are thermally induced, then it is probably true that $dQ_{out}/dT_w < 0$.

Thus, it seems reasonable to conclude that the pool boiling curves for liquid metals are often similar to the shape of the curve in Fig. 4. The final verification of this shape must of course await the design and construction of a boiler which is stable in region BC.

A continuation of the above reasoning leads to the conclusion that a forced-convection liquid metal boiler might oscillate in the manner described above for reactors. Such oscillations have indeed been reported by Smith, Tang, and Ross (*7*).

If one applies the concepts of thermal stability to improve the design of once-through liquid metal boilers one is led to the rather surprising conclusion that such boilers should be concurrent heat exchangers rather than the normal countercurrent heat exchangers.

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Eugene F. Adiutori is president of Stability Consultants where he does research on stability problems in nuclear reactors, power plants, and liquid-fueled rockets. For the nine years previous to last year he worked on naval reactors at the Knolls Atomic Power Lab.

LETTERS

Thermal Stability View Disputed

. . . Argonne Comments

DEAR SIR:

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Nucleonics

Vol. 22, No. 12 - December, 1964

. . . Westinghouse Comments

DEAR SIR:

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

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

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

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. . . Other Comments

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

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

—GEORGE LEPPERT
Stanford Univ., Stanford, Calif.

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

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

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

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

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

—MARIO SILVESTRI
CESNEF, Milan, Italy

. . . Author's Reply

Dear Sir:

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

Argonne critics:

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

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

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

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

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

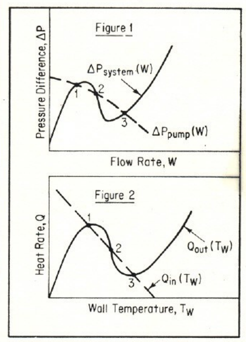
1. *Both* figures apply only to highly idealized systems (although this fact has apparently not been realized in the case of Fig. 1).

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

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

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

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



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

Westinghouse critics:

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

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

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

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

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

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

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

—E. F. ADIUTORI
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December, 1964

May 7, 1964

Dr. Harding Bliss
Chemical Engineering Dept.
Yale University
New Haven, Conn.

Dear Dr. Bliss:

There are several points I would like to make about your letter of May 5 as well as your rejection of my manuscript on transition boiling:

1. I feel it was quite unfair of Rohsenow to send you a copy of his letter of April 27th without so indicating on the copy I received. His charge about my selecting the data from a narrow range was without foundation and I was sure he was as well aware of that as I was. Therefore, in my reply, I did not even bother to discuss that rough graph other than to say that in the final version I had indeed covered the full range reported in Berenson's article. Since there was no distribution indicated on his letter, I did not feel moved to discuss the matter in detail with him. Moreover, since I had two months previously offerred to analyze whatever data he should select, I would think he would be wise enough not to make such a charge. I wish to hear no more about the possibility that I judiciously selected the data. You have my graphs and Berenson's data is clearly presented in the Int Jour Heat etc. I suggest that you have someone check my graphs against Berenson's reported data and let the matter end there one way or the other.

2. I admit that my presentation on the first version of my manuscript on nucleate boiling was distasteful. However, it is my feeling that I made that mistake only once and the manuscript on transition boiling was not at all in bad taste.

3. I am not at all given to exaggeration. I will admit that I sometimes do not accurately gauge the vision of the reader, but I can prove everything I say. If it is ever your feeling that I say something without proof and which is not obvious to you, you have only to so indicate and I will enlarge on the proof.

4. I enclose the graph I got from Rohsenow. I ask you the following question and hope for an answer:

   Can you honestly look at this graph which Rohsenow had made up and not agree that I am correct--that the data do not at all indicate any non-linearity??

5. I want to say very clearly that, in my opinion, your rejection of my manuscript on transition boiling is tantamount to avoiding your function as editor. You are rejecting my manuscript on the basis of two reviews which are not at all scientific. Kreith says I am vague on the stability--if he had read the footnote, he would have noticed that I clearly indicate that a discussion of the stability was beyond the scope of the paper. The clarity of the rest of the paper is beyond question (in my opinion). Griffith says it is unimportant. Now, I would like to observe that they are both wrong, and I would bet that you privately agree with me. Further, it is my feeling that both of these reviews should have been rejected--certainly the function of the editor is to assure that the reviews have substance. There has never been any manuscript written for which someone could not <u>charge</u> vagueness--or unimportance for that matter. Charges are very easy--easy to come by and easy to forget. I would like you to clearly tell me that these reviews comply with whatever standards you have set for your reviewers. How do you determine whether the reviewers are doing their job?? And what do you feel their job really is--to approve only what everyone knows or to be also searching for the new and unknown????

6. Can you read my letter to Griffith and deny that it establishes the importance of my manuscript which Griffith pretends not to see???

7. It is my frank and honest opinion that, by accepting reviews which are based on nothing more than prejudice, you impede the progress of science. Do you think that a reviewer would have chosen oxygen to phlogiston in Lavoisier's day? Kreith would have said that that oxygen thing is vague and Griffith would have said that oxygen is just another name for phlogiston and the equipment is fail safe so there is nothing to worry about.

8. I want you to know that I am well aware that our association must be painful to you. I would also like you to know that you have only to say the word and you can put an end to it.

9. I fail to see any reason why the simple fact that I have used Berenson's data rather than my own should compel me to publish in the same magazine he did or why that fact should relegate my article to one of secondary importance. For any sensible person, the fact that the data was obtained by some person other than my self and over whom I had no control should add significance to my results. Instead, you seem to have sort of rule which seems to more or less imply that the data belongs to whomever gets it.

10. You have erroneously concluded that I am criticizing Berenson. I am not. I use his data to establish the relationship between his variables and I indicate the manner in which he ended up with the wrong answer in direct contradiction of the data. It is my feeling that it is important to publish the reasons he made this mistake in order that, hopefully, these mistakes will not be made by others.

11. I have tried very hard to play the game your way. To me, it is a very sad commentary on the game you are playing that I should have so much trouble getting my work published. The fact that I should be more successful at getting my work published at an industrial magazine like Nucleonics speaks very poorly of the society journals.

12. I am getting so tired of this entire business that I am considering putting my work in advertisements in Nucleonics Magazine where I will not have to put up with the petty prejudices of the reviewers. It is indeed strange that, here in the twentieth century, there should be such an aversion to progress.

13. I note that you have often sent my work to well known workers in the field of heat transfer and I acknowledge my indebtedness for this favor.

I have little doubt that it is to no avail, but I urge you to reconsider your decision about my manuscript. You have reached the wrong decision. I know that the world will not stop as a result, but it is a wrong decision nonetheless.

Sincerely yours,

Eugene F. Adiutori

A. I. Ch. E. Journal
EDITOR
Harding Bliss

YALE UNIVERSITY, 225 PROSPECT STREET, NEW HAVEN, CONNECTICUT 06520

August 17, 1964

Mr. Eugene F. Adiutori
Stability Consultants
P. O. Box 18062
Cincinnati, Ohio

Re: #6593 - "Nucleate Boiling -- The Relationship Between Heat Flux and Thermal Driving Force"

Dear Adiutori:

I must acquaint you with the fact that I have had a vigorous complaint about my acceptance of the above paper. The responsible person making this complaint states that you are wrong in your contentions. He is going to send me his documented case, and I think I should check this with the reviewer who was so favorable.

I have never gone back on my word with regard to an accepted paper, and I do not do so now, at least. However, if you are wrong, then this would be adequate reason for such a reversal.

All I am going to do at the moment is write you again, enclosing the complaints when received, contact the favorable reviewer, and put a <u>temporary</u> stop on publishing the paper. I will write you again when I've heard from the favorable reviewer.

I am sorry about this, but my job as editor is a tough one. Let us see what the favorable reviewer says.

Sincerely yours,

Harding Bliss

Harding Bliss

HB:jo

A. I. Ch. E. Journal
EDITOR
Harding Bliss
YALE UNIVERSITY, 225 PROSPECT STREET, NEW HAVEN, CONNECTICUT 06520

September 7, 1964

Mr. Eugene F. Adiutori
Stability Consultants
P. O. Box 18062
Cincinnati, Ohio 45218

Re: #6593 - "Nucleate Boiling -- The Relationship Between Heat Flux and Thermal Driving Force"

Dear Adiutori:

I enclose a copy of a letter received from the original very favorable reviewer of subject paper, which letter was sent after a re-examination of the revised copy of the paper, and of the reviewers of the paper for a presentation at a meeting. You will note that this further consideration has distinctly reduced his enthusiasm for this paper. He joins in the observation of the other reviewers that your case is not proved. He recommends strongly that your major point about the use of log-log paper is a sound basis for a communication.

I have reviewed this whole matter, and I would like to make these observations:

1. I incline to agree with the almost universal opinion that you have not proved your case.

2. You have answered in a way which is roughly adequate the criticism made by Hsu about the border of free convection with the data of Bonilla. This is still not adequate for a published paper, however, because it seems to me that the written communication from Bonilla agreeing with this reversal of his published statement would be essential.

3. The two marked paragraphs on page 2 of the enclosed review are extremely important. I am particularly depressed by his statement that your material added to give a reason for the linearity proposed is virtually worthless. I had had certain reservations about this addition myself, but I had allowed it to go through on the basis of the limited time at my disposal. It seems to me that you are not quite fair with me here. You should not have added anything which a favorable reviewer would describe as worthless.

Mr. E. F. Adiutori -2- September 7, 1964

In view of all these points I am going to reverse my decision accepting this paper. I know that this will be a disappointment to you, and it is a disappointment to me. I have, however, a responsibility of the Journal as high as I can, and this responsibility must prevail over all other considerations. I think I was wrong in making that acceptance, and no one can enjoy writing that statement.

to keep the quality

The original favorable reviewer suggested that you write a communication about this matter of the log-log paper. I would welcome receiving such a communication from you. Let me say again that I am sorry about this whole matter, but I believe that no reasonable alternative exists.

With best regards,

Sincerely yours,

Harding Bliss

Harding Bliss

HB:jo
Enclosure

June 14, 1965

Dr. Graham B. Wallis
Thayer School of Engineering
Dartmouth College
Hanover, New Hampshire

Dear Dr. Wallis:

It was gratifying to read your letter of the 9th and to find that you do not altogether rule out the possibility of my presenting a lecture at your summer course.

Turning then to your objectivity, I shall try to convince you that two widely accepted conclusions with regard to two phase heat and mass transfer are simply incorrect. While these two conclusions are quite elementary and seem virtually self evident, it is no less true that they are incorrect and that a real understanding of the boiling phenomenon can be reached only by rejecting them. While the difference between the right answers and the wrong answers may seem to be of a secondary nature, this seemingly small difference leads to very great differences when we apply the right answers to the problem at hand--namely, what is the nature of the boiling phenomenon? And in particular, how can we better design equipment in order to deal with the boiling phenomenon in an optimum manner? So much for my opinion.

The first "widely accepted conclusion" which I disprove is the following: During nucleate boiling, the heat flux is related to the thermal driving force in a non-linear manner in which the heat flux is proportional to the thermal driving force raised to some power between 1 and 25 but which is usually some value like 3 or 4. With regard to this conclusion, my own research has led me to the following conclusions:

1. The data in the literature do not generally support the contention that the exponent is usually some value like 3 or 4.

2. The data in the literature do generally support the contention that, if nucleate boiling data be plotted on log log graph paper and one then draws the best straight line through the data, the resultant slope will often be approximately 3 or 4.

3. The procedure described in item 2 would result in the correct exponent only for a very special case. To illustrate, the general equation which one is really using in this case is

$$y = mx^n + b \qquad (1)$$

Now, when one draws a straight line on log log graph paper, he is unconsciously assuming that b in eq. (1) is identically equal to zero, since the equation of a straight line on log log paper is simply

$$y = mx^n \qquad (2)$$

Thus, the slope measured on log log paper will equal the true value of "n" only for the special case where b = 0. We must therefore determine either empirically or theoretically whether the value of "b" is or ought to be zero.

4. We can at once determine that "b" is finite simply on a theoretical basis quite apart from any empiricism. We have only to consider that the boiling process requires the growth of bubbles against a force of finite magnitude. If we accept that the required counterforce is somehow derived from the wall superheat, we are then forced to conclude that the finite counterforce can be supplied only by a superheat of finite magnitude. In this way, we have concluded that "b" cannot be zero because "b" is simply $h\ T_o$ in equation (3):

$$q = h(T^n - T_o) = h\ T^n - h\ T_o \qquad (3)$$

While the inclusion of the additive term in equation (3) may be aesthetically dissatisfying, to me it seems infinitely better than trying to pretend that boiling does not require a driving force of finite magnitude when in point of fact it does.

We must bear in mind that the purpose of performing experiments is to determine the functionality between variables and that we can accomplish this much better by "listening" to the data than by strict adherence to a preconceived notion as to the correct "form" of engineering correlations.

Up to this point, I feel the objective reader should be convinced of the following:

a. The procedure which has heretofore been used to determine the exponent is incorrect.

b. As a result of a., the widely accepted value of 3 or 4 is open to question.

c. Nucleate boiling correlations should contain an additive constant in order to account for the fact that boiling requires a thermal driving force of finite magnitude.

If you can accept the fact that I might not be an incorrigible liar and if you read the enclosed manuscript, perhaps you will also agree with my contention that the exponent is generally unity--which is certainly what the data in the literature says!

Let me now suppose that I have convinced you of the above, but that you are inclined to say "So what--surely there can be little practical or theoretical significance to this seemingly small difference". Let me therefore skip over the reasoning and lead you to a few of the results which derive directly from the above small difference:

1. If one accepts the substance of the above, one must also reject the present shape of the so-called "pool boiling curve" and instead assign to it a vastly different shape.

2. If you look closely at equation (3), you will sense that "h" is a completely new kind of heat transfer coefficient from what is generally defined to be "h"--the difference arising from the fact that the equation makes real sense only if "h" is defined in a derivative sense--i.e. if "h" is defined by

$$h \quad dq/d \; T \qquad (4)$$

rather than the normal definition. A close analogy to the above is the electrical engineers' concept of the "dynamic electrical resistance" which is defined in a similar manner and used to supplement the classical resistance which may also be considered as the "static" resistance. The particular advantage of both of these concepts is that they allow a better understanding of the dynamic behavior of systems and thereby make it possible to design equipment which is more nearly optimum.

Turning briefly to a second widely accepted conclusion, it is generally accepted that a pool boiler in which the heat source is a condensing fluid can operate stably at all points of the pool boiling curve. This statement is simply not true as I point out in the May, 1964 issue of Nucleonics and again in the letters section of the Dec., 1964 issue of Nucleonics. In point of fact, it is true only if the criterion I derive for thermal stability is true at all points of the pool boiling curve in question. The general form for this criterion is

$$dQ_{in}/dT_W - dQ_{out}/dT_W \leq_s 0 \qquad (5)$$

For a boiler such as Berenson used for his thesis, this general form may be reworked into the particular form

$$-\frac{1}{1/h_s + t_W/k_W} \leq_s dq/dT_W \qquad (6)$$

where the right hand side of equation (6) of course refers to the slope of the pool boiling curve. From this criterion, it may easily be seen that:

1. The statement that such a boiler is necessarily stable at all points of the pool boiling curve is simply not true.

2. The stability of such a boiler can be improved markedly by simply increasing the steam side coefficient, decreasing the thickness of the wall, or increasing the thermal conductivity of the wall wall. (Put another way, we can improve the stability of the boiler by simply increasing the coupling between the primary heat source and the primary heat sink in the boiler.) If you will refer to Berenson's article in the Int Jour Ht Mass Trsfr, you will find that he <u>intentionally</u> made the boiler plate very <u>thick</u> in spite of the fact that he wanted the boiler to be as stable as possible since his thesis subject was transition boiling.

For more on the above, I refer you to the aforementioned articles.

Lastly, let me point out that the above constitutes only an infinitesimal fraction of the work I have completed to date which, in point of fact, fills several volumes. Given free use of an hour, I would spend about 45 minutes discussing the meaning and implications of the above two erroneous conclusions and the balance of the time discussing what I feel will be the future of heat and mass transfer. I hope you will take me up on my previous offer and I look forward to meeting someone who lays claim to the highest possible scientific virtue--objectivity!

Sincerely yours,

Eugene F. Adiutori
President

EA/mh

The Thayer School of Engineering

DARTMOUTH COLLEGE · HANOVER · NEW HAMPSHIRE · 03755

June 30, 1965

Mr. Eugene F. Adiutori
President
Stability Consultants
P. O. Box 18962
Cincinnati, Ohio 45218

Dear Mr. Adiutori:

Thank you for your letter of June 14th.

I would be glad to have you give a lecture on July 9th in the afternoon on a subject of your choice. It will be of interest to see how your ideas fit in with the rest of the course material. If it fits in with your plans, I should also like to have lunch with you so that we can discuss informally outside the course framework.

The thermal stability which you mention in your letter is what I would call "first-order thermal instability" and is exactly analogous to the similar situation which arises with pressure drop when one couples a pump to a boiler channel or puts an orifice at the inlet to a heated channel. I agree with your analysis and comments on Berenson's work which seems quite obvious once you have pointed it out.

As far as nucleate boiling is concerned I don't think that any simple equation gives all the answers since one can get almost any possible results by varying the surface characteristics. For some applications your equation may be more useful whereas for others the ΔT^n formulation may be better.

I look forward to meeting you and hope that you will be able to come.

Yours sincerely,

G. B. Wallis

G. B. Wallis
Associate Professor of
Engineering

GBW/jes

UNIVERSITY OF MICHIGAN
Department of Mechanical Engineering
225 West Engineering Building
Ann Arbor, Michigan 48104

To:
Professor Stability Consultants
Professor 618 Waycross Road
Professor Cincinnati, Ohio, 45240
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I'm curious! Enclosed is my check for $1.95. Please send Chap 1 of the 'New Heat Transfer'.

Surely, h must go as it is quite well known that it is a "definition" for operation (like "Fourier's 'law'", which you did not mention). However, I have always thought the route of its exit was well known.

Good luck!

7-20-73

Date

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The "Mir" Publishers, Moscow are interested in translating and publishing in Russian of "The New Heat Transfer, eugene f.adiutori", 1974.

The book is expected to be published in 1976 in 5,000 copies at the retail price of 1.70 roubles approximately.

Herewith we are enclosing in triplicate the draft agreement for acquisition of rights to translate and publish in Russian the above book.

In case you will find the terms and conditions of the draft agreement acceptable, please sign it and return all the copies to us.

Sincerely yours,

Yu. GRADOV
Director, Export & Import
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A G R E E M E N T

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November 19, 1990

Mr. Eugene F. Adiutori
Ventuno Press
6792 Timberwood Drive
West Chester, OH 45069

Dear Mr. Adiutori:

This is to inform you of the decision of the Board of Editors of the *International Journal of Heat and Mass Transfer* relative to accepting advertisements for your book entitled *The New Heat Transfer*. By way of background you should know that the Board of Editors has reserved the right to accept or reject textbook advertising. In particular, the Board is on record as rejecting for publication any advertisement containing technical material which would be rejected for publication when subjected to the normal review procedure. Accordingly, the Editorial Board by unanimous vote has recommended to Pergamon Press that they not accept any additional advertising for *The New Heat Transfer*.

Very truly yours,

JPHartnett
WJMinkowycz

J.P. Hartnett
W.J. Minkowycz
Coordinating Editors
International Journal of Heat and Mass Transfer

JPH/WJM:rms

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