PREFACE

The theme of this second TNHT volume was accurately stated by Mark Twain:

> Supposing is good, but
> finding out is better.

The research/development methods of the old heat transfer are based on supposing. The new methods are based on finding out. I would like to add that the new ways are simpler than the old ways, but this would not be true. It is in fact much simpler to get the wrong answer by supposing than to get the right answer by finding out. The new way is better for only one reason--because it leads to right answers--it leads to answers which agree with real world behavior.

This second book of TNHT begins with the heat flow skeleton presented in Book 1 and develops it in the area of research/development. It describes in detail the new way to design and perform experiments, the new way to analyze and correlate experimental results, the new way to decide what should and should not be published, the new way to administer the engineering Journals. But I have not written this book only for researchers and developers--I have written it equally for designers and analysts. The research/development output is the design/analysis input--we can deal with this interface in an effective way ONLY if the workers at one end of the spectrum understand and appreciate the tasks and methods of the workers at the other end. Every designer/analyst who is going to apply the new heat flow science should read Book 2. Every researcher/developer who is going to apply the new science should read Book 3 which will deal with equipment design/analysis.

In this second book, I have again presented my work in an honest and sometimes impolitic manner. But I am not naive. I am not blind to the fact that honesty is publicly proclaimed and privately condemned. I know that by speaking openly and honestly I increase the likelihood that the new heat flow and the new engineering will be widely used only by generations yet unborn. But is this any reason to abandon simple honesty? Is this any reason to mix "truth and error" in my work? I think not.

I do not know whether Truth is "operative" in the twentieth century. But I do know that Truth is the only sound basis for science. I do know that only Truth has value in science. I do know that the new heat flow is a science, and that is why the 3 volumes of TNHT deal only with Truth.

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FOR DONNA

CHAPTER 1 THE NEW FOUNDATION

WHY BOOK 2?

Book 1 of The New Heat Transfer attempts to answer the question

> What happens to the old heat transfer if we replace the proportional concepts of the heat transfer coefficient, thermal conductivity, and radiative emissivity with the nonlinear, behavioral concepts of the new heat flow?

Book 2 attempts to answer the question

> What happens to heat flow research and development if we accept the nonlinear concepts of Book 1 and strive to construct the new heat flow on a scientific foundation?

The answer to this question is that many fundamental changes take place in the area of heat flow research and development. The more important changes are in the following areas:

> The role of a priori deduction.
>
> The manner in which experiments are designed.
>
> The manner in which experiments are performed.
>
> The role of the data.
>
> The manner in which the data are reduced.
>
> The manner in which the data are correlated.
>
> The form of the correlations.

The changes in all these areas have the same objective--to ensure as positively as we can that the heat flow correlations we generate will agree with Nature's behavior as closely as possible.

In Book 1, we discussed the manner in which old way correlations could be transformed to the form of the new heat flow: substitute $q/\Delta T$ for h and then separate q and ΔT; substitute $qD/\Delta Tk$ for Nu and then separate q and ΔT; substitute $q/(dT/dx)$ for k and then separate q and dT/dx; substitute q/T^4 for ε and then separate q and T.

In Book 2, we are concerned not with transforming old way correlations, but with generating correlations within the framework of the new heat flow. In other words, we are concerned here with discovering Nature's heat flow behavior in terms of the nonlinear concepts described in Book 1, always bearing in mind that the new heat flow is a science and that we wish to use scientific methods whenever possible.

NATURE'S TESTIMONY

Perhaps the most fundamental change between new and old research and development lies in the role we assign to the data. Let us forget heat flow for the moment and consider what experiments are, why we perform them, and what we should strive to learn from them.

When we perform experiments, Nature speaks to us in the marvelous language of DATA. If we would KNOW Her behavior, we have only to listen to Her testimony. In other words, if we wish to be absolutely certain of the manner in which Nature's parameters affect each other, we MUST experiment because experiment is the only certain path to a knowledge of Nature's behavior. All other paths lead to speculation--to maybe/perhaps/probably/possibly. In science, only the data is certain--all else is uncertain. In science, only the data is permanent--all else is temporary. In science, we recognize that even though we do not understand the data, it is Nature's testimony and She is an infallible witness--She does not lie.

The primary importance of the data to 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 why I have stressed the importance of the data:

> In the old heat transfer, the data is dealt with as though it possessed only secondary importance.
>
> In the new heat flow, the data is elevated to its rightful role of paramount importance, and this elevation results in numerous changes and improvements.

Many proponents of the old heat transfer will claim that I am mistaken--that the data is a very important part of the old heat transfer. The undisputable proof that the data is of only secondary importance in the old heat transfer is provided by the literature. Even a cursory review of the literature on the old heat transfer will reveal that it contains virtually no data. This will undoubtedly seem unbelievable to those readers who do not have frequent occasion to refer to the literature. I encourage them to verify it for themselves--look through the literature and try to find data--try to find the measured values of the parameters actually observed in the experiments which the articles purport to describe. Any reader who goes to the trouble of investigating this for himself will conclude with me that there is virtually no data in the literature and therefore that the data is not of first order importance in the old heat transfer since it is not important enough to require publication.

This omission of the data from the old heat transfer literature is not simply an oversight--it is the result of the conscious effort made to PREVENT the publication of the data! WHY? Why would anyone want to prevent the publication of the data--the publication of Nature's infallible testimony--the publication of the only thing in science which has permanent value? The reason given by several Journal editors is that there is no room in the literature for the data. No room!!! There is a great deal of room in the literature for material of much lesser importance, but no room for the data. To say that there is no room for the data is very much like saying that an airplane contains so much baggage there is no room for the fuel. And if there is no room for the fuel, of what possible use is the airplane?

In the new heat flow, NOTHING takes precedence over the data. In the new heat flow, Nature's testimony is paramount--it has a reserved seat in the literature--no performance begins until Her testimony is seated. In terms of the literature, this means we have every right--every reason--to expect the applicable data to be reported in the literature, even when this means that material of lesser importance must be omitted.

In the new heat flow, there is no effort made to prevent the publication of the data. On the contrary, every effort is made to absolutely REQUIRE the publication of the data. This will be accomplished by having all Journals adopt a uniform editorial policy of REQUIRING the publication of the data in digital form in all articles which purport to describe/interpret/correlate/explain experimentally observed behavior.

(An editor of a Journal once told me that it would not be democratic to require researchers to publish their data. He went on to explain that this would be like requiring a person to testify against himself which of course would violate 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 science or in the scientific literature. The editor dismissed my argument as idealistic prattle.)

The old heat transfer has little regard for the data--so little that no room can be found for the data in the literature. In the new heat flow, this situation is reversed. If there is no room for the data in the literature--no room for Nature's infallible testimony--then there is no room for anything in the literature.

If the literature is to be a part of science, it MUST contain data--the ONE thing which distinguishes science from speculation.

SCIENCE OR SPECULATION?

In the old heat transfer, correlations are regarded as more or less scientific depending on the degree of empiricism involved. It is generally felt that there is an inverse relationship between empiricism and science. If a correlation has no theoretical basis--if it has nothing to recommend it other than the fact that it agrees with the data--it is regarded as purely "empirical" and altogether "unscientific". So-called scientific correlations are those obtained largely by analysis and which therefore rest primarily on theory (speculation) rather than experimental evidence.

In the old heat transfer, scientific correlations are obtained largely by an analytical process called "dimensional analysis". This analytical technique has nothing to do with experimental evidence--it is a purely intellectual process whereby one consciously or often unconsciously

> Theorizes/assumes/guesses that Nature insists on so-called power laws.
>
> Theorizes/assumes/guesses that Nature does not permit Her parameters to interact.
>
> Theorizes/assumes/guesses that Nature works in a way which is "dimensionally consistent".

Based on these interesting theories/assumptions/wild guesses, one then proceeds to unravel Nature's secrets NOT by analyzing the data, but by analyzing the things we call "dimensions".

And precisely what are "dimensions"? Dimensions are nothing more than products of the intellect--they are inventions which are useful only because they simplify the bookkeeping aspect of dealing with Natural phenomena. And since they are nothing more than inventions of the intellect, there is no sound reason to suppose they could reveal anything about Nature's behavior. (We will return to the subject of the true character of dimensions in a later chapter.)

In the old heat transfer, the analysis of dimensions as a means to deduce Nature's behavior is regarded as a "scientific" procedure. The analysis of data in order to induce Natures's behavior is regarded as "unscientific" empiricism! In the old heat transfer, empirical correlations are temporary expedients which are abandoned as soon as correlations are available which have a "rational" (ie speculative) basis. In the old heat transfer, the speculation called dimensional analysis is in fact the foundation for most of the popular correlations used to deal with heat transfer phenomena.

It is perhaps not necessary to state that I consider empiricism to be more scientific than and far superior to dimensional analysis. But if we reject dimensional analysis because it is uncalled for speculation and if we reject empiricism because it is not highly scientific, how will we build the new science of heat flow? How will we generate correlations which accurately describe Nature's heat flow behavior and which will enable us to deal simply and effectively with heat flow phenomena?

Fortunately, we are not the first to ask this type of question--it is the very same type of question which has been asked since science began several thousand years ago.

Simply stated, the question is

> How can we discover the manner in which Nature behaves?

And since this question has been asked and successfully answered many times in the world of science, it will not be necessary that we pioneer the answer. We have only to consider the method successfully used in the past and to adapt this same method to the science of heat flow.

A SOUND BASIS FOR SCIENCE

Before we attempt to answer the question of how best to discover Nature's secrets, let us consider the method whereby we assure ourselves that the secrets discovered in the past are indeed correct--that our modern science has a sound basis in fact. A good place to start is with one of the keystones of modern science,

$$F = ma \qquad (1)$$

The question we wish to answer is

> How do we KNOW that eq 1 is correct?

It is not enough to answer that we know eq 1 is correct because Newton said so. It is not enough to answer that we know eq 1 is correct because it has been used for hundreds of years and therefore it must be correct. It is not enough to answer that we know it is correct because it is suggested by some theory--some clever speculation. There must be a sound basis for eq 1--a basis so sound that it is not subject to question. And whatever this basis is, our goal is to find it and to construct the correlations of the new heat flow on this same sound basis.

WHY is it that we accept eq 1 without hesitation? How do we KNOW eq 1 is correct? Every reader knows the answer.

> We KNOW that eq 1 is correct because it agrees with the experimental evidence--it agrees with Nature's testimony about herself. And that is the ONLY reason we are certain of eq 1.

To some readers, it will seem I am suggesting that the foundation for eq 1 is the experimental evidence and thus that eq 1 is an empirical correlation--and that is exactly what I am suggesting. It is very important to note and to understand that

> EQUATION 1 IS AN EMPIRICAL CORRELATION.

We KNOW it is correct only because Nature says so--and She does not lie. The fact that eq 1 is an empirical correlation does not make it less "scientific". Experiment is the only truly reliable way of verifying anything in science, whether it be eq 1 or any other scientific correlation or principle. And thus we conclude that experiment is a very sound basis for science.

In the new heat flow, we reject the dimensionless correlations of the old heat transfer. We recognize that Nature does not give Her testimony in terms of dimensionless groups--in terms of Nusselt numbers and Reynolds numbers. Note that eq 1 is a DIMENSIONAL correlation--anathema in the old heat transfer--unscientific in the old heat transfer. But eq 1 is welcome in the new heat flow which views dimensional equations as quite satisfactory. Dimensional correlations are written in the language of physical parameters--in the language of Nature's own testimony. In the new heat flow, we do not merely accept the language of physical parameters--we INSIST on it because we recognize that any other language brings with it the strong likelihood that much has been lost in the translation. In the new heat flow, we insist on correlations written in Nature's own tongue--in the real, dimensional language of diameters and flow rates and temperatures--not in the artificial, dimensionless language of Nusselt numbers, Reynolds numbers, etc.

In the new heat flow, those correlations which depend largely on theory/analysis/assumption/guesswork are viewed with suspicion and are regarded as unreliable and unscientific--precisely the opposite view from that which prevails in the old heat transfer! And those correlations which (like eq 1) are firmly rooted in the data--those correlations which (like eq 1) are suggested by Nature's own testimony--those correlations which (like eq 1) are written in Her language of physical parameters--those are regarded as reliable and scientific--and they provide the sound basis we require for the new science of heat flow.

DISCOVERING NATURE'S SECRETS

The above discussion leads to the conclusion that experiment is a very sound basis for science--a very sound way to prove that our correlations correctly describe Nature's behavior. But we still have the problem of how to go about discovering Nature's secrets. What is the best way--the simplest way--the most successful way--to discover Nature's secrets, the correlations which describe the manner in which Her parameters are related to each other?

Once more let us turn to eq 1 and this time let us attempt to answer the question

> How did Newton determine eq 1?

Does it seem likely that eq 1 was the pure product of Newton's intellect? Is it reasonable to suppose that Newton divined eq 1 independent of experimental evidence and that the related experiments came only AFTER this triumph of the intellect? Or does it seem more likely that Newton first performed experiments and that his analysis of the data--his study of Nature's testimony--

suggested eq 1 to him? Of course we are not going to obtain a definitive answer to this question, but let us pursue it for a moment.

It is quite likely that many readers will prefer to think that eq 1 was the unadulterated product of Newton's intellect--that he did not require the aid or the inspiration of experimental evidence. But in my view, this attitude is at odds with the super intelligence normally attributed to Newton. How is it possible to regard Newton as a science superstar and at the same time suppose that he failed to understand and appreciate the value of experiment? Is it possible that he was unfamiliar with Galileo's work and his contribution of the scientific method which is based on experiment FIRST, induction SECOND, and deduction LAST? How is it possible to regard Newton as a science superstar and at the same time suppose that he did not know that Nature speaks in Her infallible language of data and that She is anxious to reveal Her secrets to anyone who will pay attention to Her testimony?

Doesn't it seem much more likely that Newton performed experiments related to eq 1 and that his posteriori analysis of the data suggested that the parameters were related as described in eq 1? Doesn't it seem much more likely that Newton was familiar with the work of Galileo? Doesn't it seem much more likely that Newton discovered eq 1 by utilizing Galileo's scientific method rather than by a completely intellectual effort independent of experiment?

Does anyone really suppose that Hooke's Law was the pure product of Hooke's intellect and not primarily the product of his experimenting/measuring/observing/correlating? Does anyone really suppose that Ohm's Law was the pure product of Ohm's intellect and not primarily the product of his careful attention to Nature's testimony? Does anyone really suppose that Fourier's contributions to heat transfer were the pure product of his intellect?

To help us answer these questions, we have Newton's first person testimony (Bk 1, pg 1-12) that he performed experiments. The experiment he describes in Bk 1 is considerably more complex than the experiments that would have been required to suggest eq 1. If Newton was the type of individual who could conceive and execute the ingenious experiment he describes in Bk 1, doesn't it seem likely that he also conceived and executed the simple experiments which would have led him to eq 1?

We also have Fourier's first person testimony (Bk 1, pg 1-14) that he conducted exhaustive experiments in the field of heat transfer. His testimony bears repeating:

> I have deduced these laws (dealing with the effects of heat) 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.

Many readers have expressed surprise at learning from Bk 1 that Newton and Fourier were experimenters, having always supposed they were "pure" theoreticians. But wouldn't it have been more surprising to find that they did not pay close attention to Nature's testimony? Wouldn't it have been more surprising to find that these science superstars preferred speculation to experimentation? Wouldn't it have been more surprising to find that the source of their inspiration was intellectual and not experimental?

Fourier tells us the source of his inspiration--the method he used to deal with the unknown--the method he used to discover Nature's secrets:

> Profound study of Nature is the most fertile source of mathematical discoveries.

Shouldn't we conclude with Fourier that the best method--the simplest method--the most reliable method--of discovering Nature's secrets is to FIRST perform experiments and to then analyze and correlate the data--to FIRST listen to and then learn from Nature's testimony? Shouldn't this same method--the method of Galileo--the method of Newton--the method of Fourier--the so-called scientific method--shouldn't this same method be the method of the new science of heat flow?

This discussion of the scientific method has probably seemed elementary and unnecessary to many readers who regard the superiority of the scientific method as intuitively obvious. But there is one thing about this discussion which is far from obvious--and that is the important realization that the scientific method is NOT the method of the old heat transfer. The method of the old heat transfer is essentially the REVERSE of the scientific method. In the old heat transfer, the experiment does not come first--it comes last. Theory comes first--speculation comes first--dimensional analysis comes first. And this backwards order, by generating preconceived ideas in the mind of the researcher, virtually guarantees that he will largely ignore Nature's testimony and that he will not benefit from it.

In the old heat transfer, dimensional analysis is used to a priori determine how Nature's parameters affect each other. Once this has been determined, experiments are permitted--not to determine how the parameters are related to each other--not even to verify the functionality predicted by the dimensional analysis--but solely to evaluate the constants in the dimensionless correlation which is accepted "without hesitation". The result of this backwards procedure of the old heat transfer is that the correlations used to describe heat transfer phenomena are largely the result of intellectual speculation and little depend on the observed behavior of heat transfer phenomena. And that is why, in the old heat transfer, there is so little resemblance between the correlations used to deal with heat flow phenomena and the real world behavior of heat flow phenomena.

In the new heat flow, we ensure that our correlations closely resemble Nature's behavior because we formulate them in such a way that they depend almost entirely on Nature's testimony and little depend on intellectual speculation. In other words, the new heat flow utilizes the scientific method to discover Nature's secrets--and this improvement in methodology brings with it a number of profound changes, all of which contribute toward establishing the new heat flow as a science rather than an art.

HOW AND WHY

It is not my intent to suggest that theory has no place in science. On the contrary, I recognize that theory has a very special place in science. The reader should recall that a considerable fraction of Book 1 deals with theory--ie attempts to answer the question WHY. WHY must the proportional concept of resistance be abandoned? WHY do h, k, and ε prevent us from understanding what we most need to understand? WHY do pool boilers sometimes operate in an unstable manner? WHY is there hysteresis in the performance characteristics of some equipment? WHY is dimensional analysis unreliable?

Science requires both experiment and theory--both empiricism and speculation. But we must recognize that experiment and theory have different roles to play in science and we must bear in mind that they are not interchangeable. In particular, we must be very careful not to use theory where experiment is more reliable--more useful. With experiment, we find out for certain. With theory, we only suppose. Mark Twain was a close observer of Nature and he had an excellent appreciation of the relative merits of theory and experiment. He phrased the difference this way:

Supposing is good, but
finding out is better.

The old heat transfer is largely the result of supposing. The new heat flow is largely the result of finding out.

The purpose of science is to answer two questions:

> HOW? HOW are Nature's parameters related to each other? HOW do they affect each other? HOW are F, m, and a related to each other?
>
> WHY? WHY does Nature behave the way She does? WHY does F = ma ?

The proper role of experiment in science is to answer HOW questions. The proper role of theory in science is to answer WHY questions and to predict answers to those HOW questions which have not yet been answered by experiment. Theory can provide answers to HOW questions, but they are not reliable answers. Only Nature can give reliable answers to HOW questions. On the other hand, Nature refuses to answer WHY questions. WHY questions can be answered only by the intellect--by theory--by speculation--by guesswork.

It is very much as though Nature is in the witness box and the researcher is examining Her. She is a cooperative witness in that She is anxious to answer HOW questions, but She is a hostile witness in that She altogether refuses to answer WHY questions. The examiner must address WHY questions to a less reliable witness--to the intellect. And from the intellect, he must expect testimony which is generally unreliable and unsound. Certainly the history of science tells us that the intellect has always been an unreliable witness--that scientific theories have always been incorrect and that the study of Nature continually leads to the revision/rejection/replacement of scientific theories.

The point of this discussion is to emphasize that experiment is the ONLY reliable way to obtain answers to HOW questions. We use theory to answer WHY questions for only one reason--because there is no alternative. Theory is an altogether unreliable way to answer anything. Theory possesses only one virtue--it is better than nothing.

It is not my intent to disparage theory, but only to assign it to its proper place--and in the old heat transfer, it is in the WRONG place. In the old heat transfer, HOW questions are not answered by experiment--they are answered primarily by theory, by a priori analysis. Consider the manner in which the old heat transfer determines HOW Nature's parameters are related in one phase, forced convection heat transfer. It is certainly not experiment which leads to the expression

$$Nu = a\,Re^{b}\,Pr^{c} \qquad (2)$$

Nature's parameters do not include Nu, Re, Pr--these parameters have nothing to do with Nature--they are to be found only in the intellect. Equation 2 is the pure product of the intellect--the pure product of the speculation called dimensional analysis. Equation 2 is deduced BEFORE the experiment and thus has little to do with experimental evidence--with Nature's testimony.

It is only AFTER eq 2 is deduced that the experiments are performed--and the sole purpose of the experiments is to determine the "best" values of a, b, c. We do not require the experiments in order to learn HOW "h" is related to diameter, to flow rate, to temperature. We know HOW these parameters are related because dimensional analysis tells us that. We require the experiments merely because dimensional analysis is not powerful enough to define the precise values of a, b, c.

Is it possible that, through some fortunate accident, the functionality expressed by eq 2 accords with Nature's testimony? Perhaps. Maybe. Possibly. We can not tell. Why not? Because the data required to answer this question is not in the literature--the data was not important enough to require publication--not in the old heat transfer.

Does it seem likely that eq 2 agrees with Nature's testimony? Not to me it doesn't! Why not? Because I have many times checked correlations from the old heat transfer against Nature's testimony that I recorded myself--and I have NEVER found one of these old way correlations which agreed with Nature's testimony. Book 1 contains three such examples--the film cooling example in Ch 6, the nucleate boiling example in Ch 7, the transition boiling example in Ch 7. These examples eloquently illustrate how little resemblance can result between experimental conclusion and reality when one uses the methods of the old heat transfer. Behind each of these examples is my first hand experience obtaining and analyzing data with as little prejudice as possible. And only AFTER listening to and studying Nature's testimony independently did I search the literature--and I found that the published results did not at all accord with Nature's testimony.

Why didn't the results in the literature agree with Nature's clear testimony? Because the results in the literature were mainly products of the intellect and not products of experiment. And that is why all the correlations of the old heat transfer are suspect and why they do not generally agree with Nature's testimony.

In the new heat flow, we recognize that reliable answers to HOW questions can be provided ONLY by experiment. We regard the proper role of theory as twofold: to provide tentative, doubtful answers to WHY questions; to provide tentative, doubtful, stopgap answers to HOW questions only until the definitive experiments can be performed. To illustrate, the Moon's surface was analyzed for the first time a few years ago. Since that time, all theories having to do with what the Moon's surface should be like have been irrelevant. Theory was fine when it was the best thing available. As soon as something better came along, we abandoned theory. As soon as we were able to find out, we stopped supposing. It is the same in the new heat flow--we find out, and then we stop supposing.

PREJUDICE AND SCIENCE

It is generally agreed that prejudice has no place in science--prejudice is a closed mind and science requires an open mind. This is particularly true in the application of the scientific method. The first step in the scientific method is experiment; the second step is analysis of the data WITHOUT prejudice. This means that one must listen to Nature's testimony without outside disturbances and with a willingness to believe Her testimony. It is easy to believe Nature's testimony because we know She does not lie. The difficult part is to isolate oneself from outside disturbances--to divorce oneself from all prejudice--to listen to Nature's testimony without being influenced by preconceived ideas formed prior to and independent of the experiment.

How do we guard against prejudice in the old heat transfer? How do we ensure an open ear to Nature's testimony? Let us search the literature for the answer to this question. The articles in the old heat transfer literature generally have two things in common:

> The articles usually describe the analysis which was performed before the experiment. The purpose of this analysis is generally to determine how the parameters in the experiment should be related to each other. (The dimensional analysis which leads to eq 2 is an excellent example.)

> The articles usually describe the literature search which was performed before the experiment. The purpose of this search is usually to determine what work has already been done and what results have already been obtained in the area of interest.

AFTER the researcher has steeled himself with these two sets of prejudices, he performs the experiments and then "analyzes" the data, hopefully without prejudice. But with the prejudice of a priori analysis shouting in his left ear and with the prejudice of known precedent shouting in his right ear, it is fair to ask "Can the researcher possibly hear Nature's testimony?" My answer to this question is "NO! He can't hear Nature's testimony. Nature gives Her testimony in a whisper--She does not strain Herself to drown out prejudice--She simply keeps repeating Her testimony in Her calm fashion until finally a jury is seated which quietly listens to Her."

How can we guard against these built in prejudices of the old heat flow? It is not easy. There is no foolproof method--even constant vigilance is oftentimes not enough. In order to minimize prejudice, I follow and recommend the following guidelines:

> NEVER perform a priori analysis which is intended to predict the functionality among the parameters in the experiment.
>
> NEVER perform a literature search as part of the preparation for an experiment. Make a conscious effort to ignore what you think you know about the results which were obtained in other similar experiments.
>
> NEVER pay the least bit of attention to anyone's predictions of how the experiment should turn out.
>
> ALWAYS be ready to accept the experimental results--to believe Nature's testimony--no matter how violently Her testimony seems to disagree with the popular theories of the day.
>
> ALWAYS recognize that Nature does not care whether or not She is believed or understood. Her only concern is with the Truth.

It is probably not necessary to point out that these guidelines bear little resemblance to the methods of the old heat transfer. To those familiar with the old literature, these guidelines will probably seem nothing short of preposterous. But the fact of the matter is that it is the methods of the old heat transfer which are preposterous--it is only because the old methods have been universally used for decades that they now seem reasonable. The truth is that the old methods are not reasonable because they largely prevent the researcher from approaching his experimental evidence with an open mind.

Even a brief review of the literature will reveal that prejudice--preconceived ideas--a priori analysis--known precedent--are an important part of the old heat transfer preparation for experimental studies. These things have little to do with preparing for experimental studies in the new heat flow. The new heat flow has no room for prejudice--no place for preconceived ideas--no faith in a priori analysis--no interest in known precedent. In the new heat flow, we go to great lengths to avoid prejudice because the new heat flow is a science, and prejudice is the antithesis of science.

The proper role of prejudice in science was beautifully stated by Pasteur many years ago:

> 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.

CONCLUSIONS

The new foundation is experiment/data.

In the new heat flow, we determine parametric functionality by observing/correlating/finding out. In the old heat transfer, we determine parametric functionality largely by speculating/theorizing/supposing.

The new foundation will transform the old art of heat transfer into the new science which is appropriately called heat flow.

CHAPTER 2 DIMENSIONAL CONSISTENCY AND CORRELATING PARAMETERS

INTRODUCTION

Dimensions and dimensional consistency play an altogether different role in the new heat flow. In this new role, the concept of dimensions is viewed as an intellectual invention and we recognize that dimensions have no fundamental character. We utilize dimensions simply because they serve a useful purpose as a bookkeeping device and NOT because they represent something which is found in Nature. Since dimensions are not an inherent part of Nature, there is NO sound reason to suppose that Nature insists on "dimensional consistency". Or to suppose that anything can be learned about Natural phenomena by analyzing dimensions. And that is why the new heat flow abandons the thing called "dimensional analysis".

The correlating parameters of the old heat transfer are primarily dimensionless parameters. These invented "parameters" have nothing to do with Nature, are not actually measured in experiments, and are not the parameters of real concern to designers/analysts who use correlations. The correlating parameters of the new heat flow are dimensional parameters. These are the parameters found in Nature,the parameters actually measured in experiments, the parameters of real concern to designers/analysts. The conversion from invented correlating parameters to Natural correlating parameters brings about a great change in the appearance of correlations used to design/analyze equipment. The benefits which result from this conversion are that the correlations of the new heat flow are much more convenient to use and are more likely to be in agreement with the behavior of Natural phenomena.

DIMENSIONS AND DIMENSIONAL CONSISTENCY

The purpose of dimensions is to enable us to deal in a quantitative way with Natural phenomena. For example, if we touch a hot dish, we sense that it is "hot". But exactly how "hot" is it? In order to answer this question, we invent a temperature scale so that we can quantitatively describe the dish temperature and this in turn tells us how "hot" the dish is--for example, 197 F. The point is that dimensions are not found in Nature and therefore they have no fundamental character. Dimensions are a clever invention which enable us to deal effectively with Natural phenomena.

We use dimensions in the new heat flow, but we do not insist on dimensional consistency because there is no evidence that Nature insists on dimensional consistency. More often than not, the correlations of the new heat flow are "dimensionally inconsistent". They would appropriately be called "dimensional correlations" and would therefore be considered "unscientific", empirical relations in the old heat transfer. However, it is important to recognize that any correlation can be made dimensionally consistent simply by assigning the proper dimensions to the constants in the correlation. In the new heat flow, we view this process of assigning dimensions to what are in fact pure constants as a useless exercise and we avoid it.

Let us develop the new attitude toward dimensions and dimensional consistency by considering a very simple experiment. Suppose we immerse an electrically heated plate in a constant temperature bath and we keep it there for a long time. When the system reaches steady-state, what heat transfer phenomena will be taking place? We know that the plate surface will be hotter than the bath fluid and that heat will be flowing from the plate to the bath. In other words, there will be a temperature difference across the plate/bath interface and, as a result of this temperature difference, heat will be flowing across the interface. Expressed in mathematical symbolism, we might write

$$\Delta T_{\text{interface}} \rightarrow q_{\text{interface}} \tag{1}$$

where the arrow in relation 1 is intended to suggest "gives rise to" or "results in". Relation 1 would seem to be an excellent way to qualitatively describe what is actually learned from the experiment. The experiment tells us that a temperature difference gives rise to a heat flow--and that is precisely what relation 1 states. Relation 1 is not an equation--we can not write it with an equals sign because a temperature difference certainly does not "equal" a heat flow.

Now let us turn our attention to a quantitative description of the same phenomenon. Suppose we perform a quantitative experiment with this same apparatus and obtain the following data:

$\Delta T_{\text{interface}}$, F	$q_{\text{interface}}$, B/hr ft^2
10	1500
20	3000
30	4500
40	6000
50	7500
60	9000
70	10500

How should we express these experimental results? How should we express the experimentally observed fact that the heat flow in B/hr ft^2 is 150 times the ΔT in degrees F? Should we write this quantitative result in the same manner we wrote the qualitative result? Should we write

$$150\ \Delta T_{\text{interface}} \rightarrow q_{\text{interface}} \tag{2}$$

and tell the prospective user that relation 2 is a numerical identity provided that it is used with the dimensions F, B, hr, ft? Or should we write

$$150\ \Delta T_{interface} = q_{interface} \tag{3}$$

and tell the user that eq 3 is a "dimensional correlation" which must be used with the dimensions F, B, hr, ft? Or should we write

$$150\ \Delta T_{interface} = q_{interface} \tag{4}$$

and tell the user that eq 4 is "dimensionally consistent" because the 150 in eq 4 is a "dimensional constant" which has the dimensions B/hr ft^2 F? Or should we take the final step and assign the pure constant a name as well as dimensions? Should we name this pure constant the "heat transfer coefficient" and assign it the symbol h? Should we then write the experimental results in the form

$$h\ \Delta T_{interface} = q_{interface} \tag{5}$$

and tell the user that "h" is 150 B/hr ft^2 F?

In order to determine which of the above relations best describes the experimental results, let us reflect on precisely what was learned from the experiment. What was Nature's testimony? Precisely what did the data tell us about the heat flow behavior at the interface? The data told us that a temperature difference across the interface gives rise to a heat flow across the interface and that the relationship between temperature difference and heat flow was such that multiplying the temperature difference expressed in degrees F by 150 would be numerically equal to the heat flow expressed

in B/hr ft^2. This is PRECISELY what we learned from the experiment and now our problem is to determine which of the above relations most closely describes this result. The answer is obviously that relation 2 most closely describes this result. In fact, relation 2 agrees EXACTLY with the results of the experiment. The data in the above table and relation 2 say EXACTLY the same thing!

It is very important to note that there is NO dimensional consistency in relation 2 in spite of the fact that it agrees EXACTLY with the data. And the reason there is no dimensional consistency in relation 2 is that there is NO dimensional consistency in the data. Nature does not concern Herself with dimensional consistency. A temperature difference can not "equal" a heat flow. The data table does not say that a temperature difference of 10 F "equals" a heat flow of 1500 B/hr ft^2. The data table says that a temperature difference of 10 F gives rise to a heat flow of 1500 B/hr ft^2.

If Nature does not insist on dimensional consistency, it seems reasonable to ask

> How is dimensional consistency obtained in the old heat transfer?

The answer is that it is artificially contrived. We invent the requirement of dimensional consistency and then we enforce it by assigning dimensions to what are in fact pure constants. But if we didn't invent it in the first place, it would be unnecessary to enforce it in the second place. In short, the invention and enforcement of dimensional consistency is an exercise in futility.

It is certainly true that this exercise of assigning dimensions to pure constants is not harmful even though it is useless. The harm enters when we assume that

Nature behaves in a way which is dimensionally consistent and thereby conclude that dimensional analysis is a highly rational analytical tool. Nature does NOT behave in a dimensionally consistent way and therefore dimensional analysis is NOT a highly rational analytical tool.

The artificial, contrived character of dimensional consistency can perhaps be better seen in the following example. Suppose the experiment results had suggested relation 6 rather than relation 2:

$$60\ \Delta T^{1.3} \rightarrow q_{\text{interface}} \qquad (6)$$

Rewriting relation 6 in the form of eq 4, we obtain

$$60\ \Delta T^{1.3} = q \qquad (7)$$

Is eq 7 "dimensionally consistent"? Of course it is. We have merely to describe the constant 60 in eq 7 as a "dimensional constant" and to assign it the dimensions

$$\text{B/hr ft}^2\ \text{F}^{1.3}$$

in order to preserve dimensional consistency. And what does this exercise serve to accomplish? Absolutely nothing.

In the new heat flow, we have a strong preference for the form of relation 2 over the form of eqs 3-5 in spite of the fact that relation 2 is dimensionally inconsistent. In spite of the fact that relation 2 has no equals sign. Why do we prefer relation 2? Because relation 2 is written in Nature's own language which includes neither dimensional consistency nor the concept of "equals". Nature does not tell us that a temperature difference "equals" a heat flow. Nature tells us that heat flow RESPONDS to a temperature difference driving force. She does not concern herself with the thought that things must be "equal"--that heat flow behavior must be described by an "equation" which necessarily indicates BOTH numerical identity AND dimensional identity.

The concept of "equals" is an intellectual invention which is misleading when applied to Natural phenomena. To digress for a moment, the symbolism used in chemistry avoids the use of the concept of "equals" when dealing with chemical reactions. For instance, we do not write

$$2\ H_2 + O_2 = 2\ H_2O \qquad (8)$$

because certainly two different gases do not "equal" one gas/liquid. Instead, we write

$$2\ H_2 + O_2 \rightarrow 2\ H_2O \qquad (9)$$

to denote that these two different gases can combine to form one gas/liquid.

In the old heat transfer, there is a great deal of dimensional consistency. NOT because Nature planned things that way, but because the old heat transfer was made from a mold labeled "dimensionally consistent". Nature speaks in the language of relation 2 and then, in the old heat transfer, a fudge factor is introduced. This fudge factor is given the name "h" and it allows us to replace the arrow in relation 2 with an equals sign because it brings with it the dimensions which make relation 2 "dimensionally consistent". In other words, when we introduce "h" into relation 2, it becomes a dimensional identity as well as a numerical identity. Since this gives us both types of identity, we can replace the arrow of relation 2 with an equals sign. And we can replace the dimensionally inconsistent relation 2 with the dimensionally consistent

$$q = h \Delta T \tag{10}$$

But it is fair to ask how the dimensions of this thing called "h" were discovered. Since "h" is not to be found in Nature, how were its dimensions determined? The answer is that the dimensions were determined for h in the same way that all fudge factors are determined and that is simply by inventing whatever is required to fill the gap. In this case, we must invent some parameter (some fudge factor) which, when introduced into relation 2, will cause it to become a dimensional identity. We can accomplish this by assigning the name "h" to the constant in relation 2 and by inventing whatever dimensions are required of this "h" in order that relation 2 will become a dimensional identity. And that is the ONLY reason h has the units B/hr ft^2 F. And the ONLY reason eq 10 is dimensionally consistent. We invent the requirement of dimensional consistency and then we enforce it by inventing fudge factors with dimensions. In the new heat flow, we do not invent the requirement of dimensional consistency and therefore we need not invent dimensional fudge factors to enforce it.

The point of this discussion is not to suggest that we do away with dimensions in the new heat flow. Dimensions serve a very useful purpose--they simplify the bookkeeping involved in dealing with Natural phenomena. The point of this discussion is to demonstrate that dimensions and dimensional consistency have nothing to do with Nature--they are merely intellectual inventions which were arbitrarily built into the old heat transfer. And that is why the analysis of dimensions--"dimensional analysis"--can tell us nothing about Nature--can reveal nothing about the relationships among Nature's parameters. And that is why we are little concerned with dimensional consistency in the new heat flow.

What reader has ever heard of an electrical resistor which had a resistance of 10 volts per amp? Every reader knows that the dimensions of electrical resistance are ohms. Why ohms and not volts/amp? Since Ohm's Law is

$$V = I R \tag{11}$$

we must conclude either that the units of electrical resistance are not really ohms or that Ohm's Law has NEVER been dimensionally consistent!!! Either we get rid of ohms or we admit that Ohm's Law--this pillar of twentieth century engineering--is dimensionally INconsistent!!!

(There is of course a devious way around this difficulty and that is to say that "ohms" is a short hand way of saying "volts per amp". But I dismiss that argument as clever nonsense. There is a much greater need for a short hand way of saying "British thermal units per hour per foot squared per degree Fahrenheit", but no one has yet invented it. And hopefully it will be ignored if ever it is invented. But perhaps I am wrong. The popular abandonment of "cycles per second" in favor of "Hertz" would seem to prove I am wrong. However, I view the invention of "Hertz" with optimism and the hope that it will meet an early and sudden death. I have little doubt that, if Hertz knew his name had replaced cycles per second, he would vomit.)

The point is that Ohm's Law is not now and never was dimensionally consistent. But this lack of dimensional consistency has not caused the slightest difficulty for anyone. It would be quite simple to make Ohm's Law dimensionally consistent--simply throw out ohms and replace it with volts/amp. But since there is nothing dimensionally consistent about electrical flow behavior, there is not the slightest need to describe electrical flow phenomena with correlations which are dimensionally consistent. So what would be accomplished by converting Ohm's Law to dimensional consistency? Absolutely nothing.

And since electrical engineering science has progressed quite nicely using correlations which are dimensionally INconsistent, there is not the slightest reason to feel that the lack of dimensional consistency in the new heat flow will cause any difficulty.

In the new engineering, we abandon the requirement of dimensional consistency. Therefore we have no need of the dimensional fudge factors of the old engineering and so we abandon them also. Since the concept of "equals" seems to demand the dimensional identity which we lack in the new engineering, we also abandon the equals sign of the old engineering. And thus we write

$$f\{\Delta T\}\ g\{\text{system props.}\} \rightarrow q \tag{12}$$

$$f\{V\} \rightarrow I \tag{13}$$

$$f\{\sigma\} \rightarrow \varepsilon \tag{14}$$

in place of "Newton's Law", in place of Ohm's Law, in place of Hooke's Law. Against the background of the old engineering, relations 12-14 will seem to be a step backward unless one consciously recognizes that they are written in PRECISELY the language of Nature's testimony. And since their sole purpose is to describe Natural phenomena, it is manifest that Her language is the best language.

CORRELATING PARAMETERS--NEW WAY

In the new heat flow, our experimental objective is to obtain the data which will reveal the functionality among the process parameters and so will enable us to induce a simple correlation which closely approximates the process under investigation. This brings up the very important question

> What parameters should appear in the correlation?
> How should we select the correlating parameters?

Should we use the parameters which were measured in the experiment? Or should we correlate with parameters which were not measured in the experiment? This question should seem trivial. It should seem "obvious" that we should correlate with the parameters measured in the experiment rather than the parameters not measured in the experiment. If we measure volts and amps in the experiment, then we should attempt to correlate with volts and amps. And if we measure stress and strain in the experiment, then stress and strain should be among the correlating parameters. "Obviously", we should correlate what we measure. So why are we discussing something which is so "obvious"? Because this "obvious" answer is not the answer of the old heat transfer. In the old heat transfer, we often neglect the parameters which were actually measured and instead correlate with parameters which were not measured.

Let me illustrate how easy it is to correlate with parameters which were not measured--how easy it is to correlate with data which does not even exist. A few years ago, one of the wire services carried a story about some researchers at a medical school who had disproved a widely held notion about coffee and alcohol. The researchers had conducted an exhaustive experiment and from their data concluded that a cup of hot, black coffee would NOT have a sobering effect on persons under the influence of alcohol! The thing I found most surprising about this story was not the experimental conclusion but rather the experimental technique of the researchers. This wire service story prompted me to ask a number of persons the following question:

> If you were to perform such an experiment, what would you consider ABSOLUTELY ESSENTIAL for the experiment?

And every time I have asked this question, the answer has included the two ingredients

COFFEE

ALCOHOL

And I heartily agree that coffee and alcohol are absolutely essential for this experiment. Yet the wire service story indicated that the medical school researchers had performed the entire experiment with only ONE of these ingredients. Which one? Alcohol. The entire experiment was based on not one single drop of coffee! A considerable quantity of alcohol was consumed (by a number of very willing volunteers), but not one single cup of coffee was brewed--not a single drop of coffee violated the lips of a single volunteer! And since coffee was not involved in the experiment, it follows that no data was obtained which had anything to do with coffee. And since no coffee data was obtained, it follows that the data could not have indicated anything about the effect of coffee on anything or anyone. And if the data did not indicate anything about the effect of coffee, how were the researchers able to conclude from their experiment that coffee did not have a sobering effect?

The answer to this riddle is that the researchers' conclusion rests largely on theory--largely on guesswork and assumption and speculation. The researchers did indeed perform an experiment, but their conclusion little depends on the experimental results because their conclusion has to do with parameters which were not even investigated in the experiment. How did the researchers reach their conclusion without investigating anything about coffee? It was quite simple--it required merely that they make a few assumptions--a few wild guesses. And after they made these assumptions, it was not necessary to verify them--these assumptions were accepted "without hesitation". Let us take a moment to consider these unquestionable assumptions.

Drinking a cup of hot, black coffee has many varied effects on the human body. It stimulates the sense of smell, the sense of touch, the sense of taste. It affects the body's temperature control mechanism. It affects the body's chemistry by the addition of the caffein, the water, and whatever else is normally found in a cup of hot, black coffee. And therefore we can perform the subject experiment without any coffee provided we are willing to assume the following:

That stimulating the sense of smell does not have a sobering effect;

That stimulating the sense of touch does not have a sobering effect;

That stimulating the sense of taste does not have a sobering effect;

That altering the body's temperature equilibrium and stimulating its temperature control mechanism does not have a sobering effect;

That the addition of hot water to the body's digestive system does not have a sobering effect.

In short, if we are willing to assume that there is nothing about drinking a cup of coffee which could have a sobering effect EXCEPT the caffein in the coffee, then we have no need of coffee. We can run the experiment with caffein pills--and, according to the wire service story, that is PRECISELY what the researchers did. They had the volunteers consume varying amounts of alcohol in order to then determine whether caffein pills would have a sobering effect on the volunteers. And thus their experiment concerned itself with the sobering effect of caffein pills. And therefore their experimental conclusion should have concerned itself with the sobering effect of caffein pills rather than the sobering effect of hot, black coffee.

I have never heard of anyone taking a caffein pill in order to sober up. I have never heard anyone say that caffein pills have a sobering effect. It does not surprise me at all that caffein pills do not have a sobering effect. And what did the above experiment prove about the sobering effect of hot, black coffee? Absolutely nothing.

If I wished to sober up, would I take a caffein pill? No. Would I drink a cup of hot black coffee in spite of the researchers' conclusion that it would not have a sobering effect? I certainly would.

There is an old saying which goes

> If you can't prove what you want to prove,
> prove something else. And then pretend it's
> the same thing.

The experimenters wanted to prove something about hot black coffee, but instead they proved something about caffein pills. And they assumed/guessed/theorized that they were the same thing. They theorized that hot black coffee could have a sobering effect ONLY because of the caffein in coffee and therefore they performed the experiment with caffein pills rather than coffee. Their "experimental" conclusion has little to do with the experimental results and much to do with the theory that the sobering effect of hot black coffee is the same as the sobering effect of caffein pills. Although this theory may seem rational and unquestionable to the researchers, it does not seem so to me and so I reject the claim that these researchers "disproved" the widely held notion that drinking a cup of hot black coffee has a sobering effect. Their conclusion may well be correct, but they certainly did not "prove" it. Not to me or to anyone else who insists that proof be based on experiment and not assumption, on data and not theory, on science and not speculation.

Now let us turn our attention to the correlation of one phase, forced convection heat transfer data. Should we correlate with parameters measured in the experiment? Or with parameters NOT measured in the experiment? In order to answer this question, consider the parameters which are normally measured:

$$q,\ D,\ W,\ T_{coolant},\ \Delta T_{DF},\ P$$

Should we correlate with these measured parameters? Or with the correlating parameters of the old heat transfer which of course are

$$q,\ D,\ W,\ C,\ \mu,\ k,\ \Delta T_{DF}$$

Notice that, in the old heat transfer, we measure T and P but we do not correlate with them. We forget about them and instead we correlate with C, μ, and k. In this way, we obtain one phase, forced convection heat transfer correlations such as

$$q/\Delta T = h = .023\ D^{-.2}\ (W/A)^{.8}\ C^{.4}\ \mu^{-.4}\ k^{.6} \qquad (15)$$

which, in the dimensionless and mysterious language of the old heat transfer, is also given by

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

It is very important to recognize how, in the old heat transfer, we make this transition from the measured parameters T and P to the UNmeasured parameters C, μ, and k. We assume/deduce that, if T and P are important, it MUST be through their influence on C, μ, and k. Therefore we forget about T and P and focus our attention on C, μ, and k. (We forget about hot, black coffee and focus our attention on caffein pills.)

In the new heat flow, we reject C, μ, k as correlating parameters for one phase, forced convection heat transfer and replace them with T and P. The principal reasons for this decision are:

1. We experimentally determine the effect of T and P on one phase, forced convection heat flow and therefore we FIND OUT about the effect of T and P. We can replace the correlating parameters T and P only by SUPPOSING. We can SUPPOSE that T and P act only through their influence on C, μ, k and this supposition then allows us to replace T and P with C, μ, k. But after we have found out, there is no point in supposing.

2. For a given fluid, the parameters C, μ, k are uniquely determined by the fluid temperature and pressure. Since pressure little affects the C, μ, k of liquids or the C, μ, k of gases at low to moderate pressures, we can say with reasonable accuracy that C, μ, k are determined by temperature. This is very important because it means we can replace the correlating parameters C, μ, k of the old heat transfer with the new correlating parameter T--and obviously it would be desirable to replace three parameters with one parameter. In other words, no matter how C, μ, k affect one phase, forced convection heat transfer, it MUST be possible to correlate the data in terms of T and P because these two parameters uniquely determine ALL the fluid properties. Moreover, it seems quite likely that good correlation can be obtained by neglecting the effect of P and dealing only with T. This greatly simplifies the correlating problem because it means that we can certainly reduce the number of parameters by one and probably by two.

3. Suppose we are interested in water as a heat transfer medium and we perform exhaustive one phase, forced convection heat transfer experiments with water. It would be IDENTICALLY IMPOSSIBLE to separate the individual effects of C, μ, k because their effects are "hopelessly confounded"--ie there is no way that the effect of μ can be separated from the effects of k and C because μ can be varied only by varying T and this in turn causes k and C to vary. If we were to reject dimensional analysis but retain the correlating parameters C, μ, k, we could not correlate the data from our water experiments because there would be no way of telling whether the observed effect of T was the result of its influence on C, μ, or k. We would have to conclude the experiment with the explanation that the data could not be correlated until more experiments were performed with other fluids!

   In the old heat transfer, dimensional analysis separates the effects of C, μ, k. Thus the old heat transfer allows us (or rather forces us) to generate correlations which apply to all fluids even though we have performed experiments with only one fluid. In the new heat flow, we reject the speculative results of dimensional analysis and therefore it is not possible to determine the heat transfer behavior of all fluids by performing experiments with a single fluid. At first glance, this may seem to be a disadvantage of the new way, but actually it is a decided advantage because it forces us to distinguish between what we know and what we suppose.

4. Correlations in the form q{G, D, T, ΔT} are much more convenient to use because they eliminate the need to determine μ{T}, k{T}, and C{T}. In other words, it is not necessary to determine these fluid properties from graphs or tables because the effects of C, μ, k are implicit in the T effect on q. If the correlation is part of a computer program, the

new form saves computer space and time because it eliminates the need for subroutines which determine C, μ, and k.

In the old heat transfer, the effect of T is expressed in terms of C, μ, k functions. For example, eqs 15 and 16 indicate that the temperature dependence for ALL fluids is given by

$$\frac{C^{.4}\, k^{.6}}{\mu^{.4}}$$

(Of course we recognize that "ALL fluids" means all fluids EXCEPT those which do not resemble eqs 15 and 16. And just how are we to determine which fluids do and which fluids do not resemble eqs 15 and 16? By experiment!)

In the new heat flow, the effect of T is expressed in terms of experimentally determined functions of T and we expect these T functions to differ from fluid to fluid, although the same basic function might apply to a group of fluids. For instance, experiments with a certain class of heat transfer fluids might indicate that the temperature dependence is closely approximated by the simple T function

$$q\{T\} \rightarrow a + bT + cT^2 \qquad (17)$$

for constant D, G, ΔT

and that only the values of a, b, c vary from fluid to fluid. Substituting this T function into eq 15 and separating q and ΔT, we obtain

$$q \rightarrow D^{-.2}\, G^{.8} (a + bT + cT^2)\ \Delta T \qquad (18)$$

where the arrow indicates that we are dealing with numerical identity and that we are not concerned with dimensional identity.

It must be emphasized that the temperature dependence in relation 18 is presented here only for the sake of illustration and is not intended to describe the true functionality between q and T. The important thing to note about relation 18 is that it is in the form

$$q \rightarrow f_1\{D\}\ f_2\{G\}\ f_3\{T\}\ f_4\{\Delta T\} \qquad (19)$$

Notice that relation 19 IN NO WAY depends on the complex functions C{T}, μ{T}, k{T} whereas both eqs 15 and 16 depend on all three of these functions. Relation 19 is based on the parameter T which is of direct importance to designers/analysts rather than the temperature-related functions C, μ, k which are of only indirect importance to designers/analysts. Relation 19 is based on one T function (which may turn out to be well described by a simple, analytical function) in place of three T dependent functions which are so complex they are usually presented only in tabular form. The end result is that relation 19 has the following important advantages over eqs 15 and 16:

> Relation 19 better describes the functionality between q and T.
>
> Relation 19 can be solved more quickly.
>
> Relation 19 can be solved more reliably.

Correlations 15, 16, and 19 all have the same, single purpose--to enable engineers to effectively and reliably deal with one phase, forced convection heat transfer. This highly practical purpose means that whichever correlation yields the most reliable answers in the quickest, easiest way is necessarily the best correlation. In other words, the aesthetic value of these individual correlations is of no importance because we are dealing here with a problem of a singularly practical nature. When we rule out nebulous, aesthetic values and make our choice based on concrete, practical values, it is obvious that the form of relation 19 is many times better than the form of equations 15 and 16. And that is why we abandon eqs 15 and 16 in the new heat flow.

Relation 19 is a new way correlation only in the sense that it is based on the correlating parameters used in the new heat flow to describe one phase, forced convection heat transfer. Relation 19 is NOT a new way correlation in the sense that it is based on the unverified assumption that one phase, forced convection heat transfer can be accurately described by the product of four functions, each of which depends on only one parameter. We certainly hope that relation 19 is true because, as we shall see later, relation 19 is much, much easier to deal with than the more general relation

$$q \rightarrow f\{D, G, T, \Delta T\} \qquad (20)$$

Much of what we do in the next few chapters is based on relation 19, but not in the sense that we simply assume it is valid. Rather, we use relation 19 as a guide to simplify the problem, recognizing that we are on shaky ground until we EXPERIMENTALLY VERIFY that relation 19 applies to the problem at hand. And should we find that relation 19 does NOT apply to the problem at hand, then we must be ready to conclude that we have oversimplified the problem and that we must then consider the more general form of relation 20.

The conversion from the C, μ, k of the old heat transfer to the T of the new heat flow represents a very fundamental change. To many, this change will likely seem

a step backward--a step away from science and toward art. But perhaps the following questions will make the conversion seem more rational:

> Isn't it better to correlate with parameters which are measured rather than with parameters which are not measured?
>
> Isn't it better to correlate in a way which requires a great deal of finding out rather than a great deal of supposing?
>
> Isn't it better to correlate with parameters which are important to designers/analysts rather than with parameters which are unimportant to them?
>
> Isn't it better to develop correlations which are simply and rapidly solved and do not require the use of complex, tabular functions?

In the new heat flow, we correlate with parameters which are normally measured in heat flow experiments. These parameters are the same ones which are important to designers/analysts and therefore the correlations of the new heat flow are more convenient to use for design and analysis.

ONE PHASE, FORCED CONVECTION HEAT FLOW

In this chapter, we have spent a good deal of effort on one phase, forced convection. In the next several chapters, we will do likewise. Not because the new heat flow is primarily concerned with heat flow during one phase, forced convection, but only because it is a good subject on which to demonstrate the methods of the new heat flow. Every reader is surrounded by one phase, forced convection heat flow. Almost every reader is familiar with the dimensional analysis which is the basis for eq 16, a pillar of the old heat transfer. Almost every reader has performed an experiment dealing with one phase, forced convection heat flow. Almost every reader has had to correlate one phase, forced convection heat flow data. It is because almost every reader can readily relate to one phase, forced convection heat flow that we continually apply the methods of the new heat flow to this same phenomenon.

But the real subject of this and the next several chapters has nothing to do with one phase, forced convection heat flow. The real subject is the METHODS of the new heat flow. For this reason, the reader should focus his attention on the methods and should more or less ignore the fact that we keep applying them to the same simple phenomenon.

CHAPTER 3 EXPERIMENTATION AND A PRIORI DEDUCTION

INTRODUCTION

Experimentation and data play only a minor role in the old heat transfer. The major roles are played by a priori deduction in general and dimensional analysis in particular. This casting has many consequences in the old heat transfer, the principal of which are:

1. The functionality among the important parameters is determined not by experiment, but largely by a priori deduction such as dimensional analysis.
2. Only few experimental points are required.
3. Experiments can be performed in whatever manner is most convenient. In other words, there is little need to "design" experiments.
4. There is little need to correlate data. Data correlation involves little more than plotting the results on log log graph paper and drawing a straight line through some portion of the results--some invented "regime".
5. Many of the correlations of the old heat transfer bear little resemblance to the data--little resemblance to real world behavior.

In the new heat flow, experimentation and data play the major roles and a priori deduction has only a very minor role. This new casting has the following consequences in the new heat flow:

1. The functionality among the important parameters is determined by experiment and NOT by a priori deduction such as dimensional analysis.

2. A great many experimental points are required.

3. Experiments must be planned and performed in a systematic way and with a specific purpose in mind. In other words, there is a great need to "design" experiments.

4. There is a great need to correlate data--to induce the parametric functionality directly from the data. This inductive process is best accomplished by plotting the data on linear graph paper and inferring the functionality by inspection. (It is virtually impossible to correctly infer functionality from the log log graph paper which is the mainstay of the old heat transfer. For this reason, log log graphs are very seldom used in the new heat flow.)

5. Highly nonlinear phenomena are seldom broken up into more or less linear "regimes". If the observed behavior is so highly nonlinear that it can not be described by a simple analytical expression, then the behavior is usually described by a graphical expression.

6. The analytical and graphical correlations of the new heat flow are derived from the experimental data and NOT from the intellect. It therefore follows that these correlations closely resemble the data--closely resemble real world behavior--and therefore are more accurate and more reliable.

It would be possible to demonstrate the above in a general, abstract way. However, it would be simpler to have the demonstration center around a specific, real world example, and this is the method we will follow. In this and the next few chapters, we will deal with one phase, forced convection heat flow using the methods of the old heat transfer and also the methods of the new heat flow. In this way, we will first consider the shortcomings of the old heat transfer in order that we will be able to decide whether the methods of the new heat flow suffer from these same shortcomings.

The principal objective of this and the next several chapters is to demonstrate the methods of the new heat flow in general by applying them in particular to one phase, forced convection heat flow. However, it will be obvious that these same methods apply equally to other types of heat transfer phenomena and to Natural phenomena in general.

It is not my intent to suggest that the experimental methods described here are original in an absolute sense. They are original only in the sense that they represent a radical departure from the methods of the old heat transfer. The essence of this departure is that the new methods rely heavily on experiment and data whereas the old methods rely heavily on a priori deduction such as dimensional analysis. It is my contention that the experimental methods described here represent a return to the method of Galileo, of Newton, of Fourier--a return to the scientific method of experiment FIRST, induction SECOND, deduction LAST.

A PRIORI DEDUCTION--OLD WAY

Now let us consider a priori deduction in the old heat transfer handling of one phase, forced convection heat transfer. In order to accurately establish the validity of the old methods, we must be very careful to explicitly state all the assumptions involved in the analysis. Several of these assumptions may be new to the reader, not because I have imagined them, but because it is not widely recognized that they are an integral part of dimensional analysis. However, it is very important to note that dimensional analysis is based on just such assumptions in spite of the fact that they are more often made unconsciously than consciously.

Using the methods of the old heat transfer, we begin with the following assumptions:

ASSUMPTION 1: ASSUME that one phase, forced convection heat transfer behavior is defined by the following parameters.:

$$q,\ D,\ V,\ \rho,\ \mu,\ C,\ k,\ \Delta T$$

(Of course in the old heat transfer, h would take the place of q and ΔT. It should also be noted that the above assumption overlooks the so-called L/D effect. We will make this same oversight when we repeat this problem using the new heat flow. We might also have stated that we restrict the investigation to large values of L/D or to fully developed thermal and hydraulic profiles which is the same thing as overlooking the L/D or entrance effect.)

Expressed in mathematical notation, we write this assumption in the form

$$q = f\{D,\ V,\ \rho,\ \mu,\ C,\ k,\ \Delta T\} \tag{1}$$

ASSUMPTION 2: ASSUME that eq 1 can be rewritten in the simple form

$$q = f_1\{D\} \cdot f_2\{V\} \cdot f_3\{\rho\} \cdot f_4\{\mu\} \cdot f_5\{C\} \cdot f_6\{k\} \cdot f_7\{\Delta T\} \tag{2}$$

ASSUMPTION 3: ASSUME that each of the functions in eq 2 is a so-called power law--ie assume that Nature always requires Her parameters to express themselves in terms of exponential functions--ie assume that eq 2 can be rewritten in the form

$$q = a\ D^b\ V^c\ \rho^d\ \mu^e\ C^f\ k^g\ \Delta T^h \tag{3}$$

ASSUMPTION 4: ASSUME that, in one phase, forced convection heat transfer, the heat flow is proportional to the driving force temperature difference--ie assume that the ΔT exponent is unity--ie assume that eq 3 can be rewritten in the form

$$q = a\ D^b\ V^c\ \rho^d\ \mu^e\ C^f\ k^g\ \Delta T \tag{4}$$

ASSUMPTION 5: ASSUME that eq 4 must be "dimensionally consistent"--ie assume that the dimensions on the left side of eq 4 must equal the dimensions on the right side of eq 4.

Assumptions 3 and 5 are the keystones of dimensional analysis. They make it possible for us to deduce a great deal about the exponents in eq 4 without having to perform a single experiment! Based only on the intellect, we can "determine" (and without hesitation) that

$$d = b - 1 \tag{5a}$$

$$e = b \tag{5b}$$

$$f = c - b \tag{5c}$$

$$g = 1 - c \tag{5d}$$

In other words, we can eliminate four of the exponents in eq 4 without performing any experiment! We can thus reduce the six unknown exponents of eq 4 to only two unknown exponents. We can "simplify" eq 4 and rewrite it in the form

$$q = a\ D^{b-1}\ V^b\ \rho^b\ C^c\ \mu^{c-b}\ k^{1-c}\ \Delta T \tag{6}$$

Notice particularly that there are EIGHT unknowns in eq 3 and only THREE unknowns in eq 6. Assumptions 3, 4, and 5 allow us to determine five of the eight unknowns in eq 3. These three assumptions allow us to complete more than 50 per cent of our work withOUT performing a single experiment--withOUT obtaining a single measurement--withOUT examining one bit of data!

Using the methods of the old heat transfer, we would not be happy with the form of eq 6 and we would insist on rewriting it in the "convenient" form

$$(qD/\Delta Tk) = a(DV\rho/\mu)^b (C\mu/k)^c \qquad (7)$$

But we would still not be happy with the form of eq 7 as it is written. Instead, we would insist on hiding eq 7 behind the iron mask of dimensionless parameters and so we would finally obtain the result of our a priori deduction in the old heat transfer

$$Nu = a\ Re^b\ Pr^c \qquad (8)$$

This dimensionless "correlation" is the basis for one phase, forced convection heat transfer design and analysis in the old heat transfer. And we have deduced it without the aid of a single experiment. We have deduced it with the aid of nothing more than some very simple analysis based on FIVE highly questionable assumptions which are normally accepted "without hesitation" in the old heat transfer.

The formulation of eq 8 concludes the a priori deduction part of our investigation and brings us at last to the problem of defining the experiment objectives--ie of describing just what it is we hope/expect to learn from the experiment.

EXPERIMENT OBJECTIVES--OLD WAY

In the old heat transfer, experiments have two general purposes:

1. Experiments provide the data required in order to "verify" the results of a priori deduction such as dimensional analysis. In other words, experiments serve as empirical demonstrations of the soundness/correctness/accuracy of theoretically deduced correlations.

2. Experiments provide the data necessary to establish the value of the constants in theoretical correlations deduced a priori.

With regard to the problem at hand, "verification" is required because eq 8 has nothing to do with real world experience--it relates only to the intellectual world. And since eq 8 has no real world foundation, there is every likelihood that it does not resemble real world behavior--there is every likelihood that it is unsound/incorrect/inaccurate. The only way to find out whether or not eq 8 resembles real world behavior is by performing real world experiments.

Experiments are also required because there is no way to deduce the values of a, b, c in eq 8.

A PRIORI DEDUCTION--NEW WAY

Now let us deal with one phase, forced convection heat flow using the methods of the new heat flow. The first step is to decide what parameters will be measured in the experiment because, as we discussed in Ch 3, the measured parameters are the correlating parameters in the new heat flow. Normally, q, D, W, T, P, and ΔT are measured in one phase, forced convection experiments. The one change we decide to make is to correlate not with W but with W/A, since otherwise D would seem to have a very large effect. The end result is that we decide to deal with one phase, forced convection heat flow using the following correlating parameters:

$$q, D, G, T, P, \text{ and } \Delta T$$

Now the question arises: "Is this an assumption?" The answer is no. It is not an assumption because we have merely decided to consider all the data obtained in the experiment.

(Of course we must recognize that there are many other parameters in the experiment which may or may not be controlled such as water chemistry, surface condition, barometric pressure. But if we were to control and measure these other parameters, we would merely include them in the above list of correlating parameters and there would be no fundamental change in the method.)

Expressed in mathematical notation, we have decided to deal with one phase, forced convection heat flow in the form

$$q \rightarrow f\{D, G, T, P, \Delta T\} \qquad (9)$$

because these are all the parameters which are controlled and measured in the experiment. In other words, we have decided to consider all the data obtained in the experiment and so we have not actually assumed anything as yet.

In the new heat flow, we are willing to base our conclusions on only two types of assumptions:

> Assumptions which are not open to question. (Such as the assumption that things flow downhill or that nothing disappears.)

> Assumptions which are subject to experimental verification. (Such as assuming that, in relation 9, q is essentially independent of P which of course would be quite simple to prove or disprove experimentally.)

Relation 9 would be quite difficult to deal with in a general way, and so what we require at this point is a simplifying assumption which is subject to verification. We want to make a very tentative assumption which we will later verify, and the purpose of this assumption is to simplify the problem at hand in a way which seems likely to be true or which at least seems reasonable. (There would be little point in making an unreasonable simplifying assumption because, should the assumption fail to verify, we will be returning to the form of relation 9 anyway.)

A simplifying assumption which seems reasonable and which, if true, would greatly simplify the problem at hand is the following:

TENTATIVE ASSUMPTION 1: Tentatively assume that rel 9 can be rewritten in the form

$$q \rightarrow f_1\{D\}\ f_2\{G\}\ f_3\{T\}\ f_4\{P\}\ f_5\{\Delta T\} \qquad (10)$$

It is very important to emphasize that we do not know that this assumption will apply with reasonable accuracy to the problem at hand or to other problems in heat flow. However, it seems likely that this assumption applies to many Natural phenomena--at least that is what most of the correlations of the old engineering would have us believe. In any event, if this assumption does not apply to the problem at hand, we will know it because it is a simple assumption to test (as we shall see in one of the later chapters).

This assumption is very convenient--it greatly simplifies the task of designing the experiment and analyzing the results. And it does not compromise any conclusions we may reach because we will experimentally verify its accuracy BEFORE we reach any experimental conclusions. Tentative assumption 1 is essentially the same as Assumption 2 on page 4-4, the only difference being that we make this assumption consciously, tentatively, recognizing that it may not apply to the problem at hand and therefore that it will require concrete verification.

What reader has ever seen Assumption 2 on page 4-4 stated explicitly, tentatively, and with the conscious recognition that it may not be valid? More often than not, the assumption on page 4-4 is not even stated. And it is almost never verified in the true sense of the word.

Notice that the methods of the old heat transfer are based on FIVE questionable assumptions. The methods of the new heat flow are based on only ONE questionable assumption which is made tentatively, subject to experimental verification.

The formulation of relation 10 completes the a priori deduction phase of our investigation using the methods of the new heat flow. We are now ready to decide just what it is we wish to learn from the experiment.

EXPERIMENT OBJECTIVES--NEW WAY

In the new heat flow, the overall purpose of experiment is altogether different than in the old heat transfer. The overall purpose of experiment in the new heat flow is to provide the data from which the functionality among the process parameters can be induced. We use experiments to "paint" a picture of the relationships among the parameters which largely determine the process behavior. In the production of this "picture", the experimental apparatus is the paint brush, the data is the paint, linear graph paper is the canvas, Nature is the artist. And from this "picture", we hope to induce a simple analytical correlation which can be used at will to faithfully reproduce the original.

With regard to the problem at hand, the experiment has two specific objectives:

1. To provide the data necessary to prove or disprove that tentative assumption 1 accurately describes one phase, forced convection heat flow.
2. To provide the data necessary to establish the five unknown functions in relation 10.

In order to accomplish these objectives, it is necessary to "design" the experiment as described in the next chapter.

OLD WAY VS NEW WAY

It is my contention that the new way deduction presented here is many times better than the old way deduction. Why? Because the new way deduction is virtually no deduction at all--and the absolute optimum a priori deduction is no deduction at all. The new way "deduction" actually says the following:

> We had better consider the effect of all the parameters we control and measure in the experiment.
>
> We had better deal with the parameters we control and measure in the experiment and not try to divine a related set of parameters which would replace those involved in the experiment.

> We had better try to determine the behavior of the fluid we are actually testing and forget about trying to determine the behavior of all fluids by testing only one fluid.
>
> We had better verify any simplifying assumptions we make or else our conclusions will be without adequate foundation and may little resemble real world behavior.

Can this be regarded as deduction? The purpose of deduction is to decrease the alternatives--to simplify the problem by narrowing down the possibilities. What alternatives are decreased in the above way? None!

Now consider the old way deduction and its effect on the alternatives. The old way deduction eliminates virtually all the alternatives. In one motion, the old way eliminates every possible function EXCEPT so-called power laws.

> Where is it written across the sky that Nature deals only in power laws?

The assumption that Nature deals only in power laws is a straitjacket which eliminates every alternative save one. The old way deduction is not a preparation for experiment--it is a substitute for experiment--an altogether inadequate substitute.

In their effect on the experimental alternatives, the difference between the old way and the new way deduction is the difference between a bathtub and an ocean. And that is why the new way deduction is many times better than the old way deduction.

CHAPTER 4 EXPERIMENT DESIGN AND PERFORMANCE

INTRODUCTION

Experiment design refers to the game plan for the experiment. In particular, it is concerned with the precise manner in which the experiment will be performed--what parameters will be varied, what parameters will be held fixed, at what parametric values will data be obtained, etc.

In the old heat transfer, it is not necessary to "design" experiments because the experiment and the data are relatively unimportant. Using one phase, forced convection heat transfer as an example, the experiment is used not to determine functionality but merely to evaluate three constants which can not be deduced a priori. For this minor role, the experiment design makes little difference--the only design requirements are that we investigate at least two values of Re, two values of Pr, and that we obtain at least three data points. Three data points to find out all about one phase, forced convection heat transfer for all liquids, all gases, all geometries, all temperatures, all flow rates. And these three data points can be obtained at whatever values of Re and Pr are convenient. Small wonder that it is not necessary to design experiments in the old heat transfer.

In the new heat transfer, it is essential to design experiments because the experiment and the data are so important. They are used to determine the functionality among the process parameters--they serve to suggest the correlations of the new heat flow. And this requires the collection of a considerable amount of data--enough to establish the shape of the interdependence among the various parameters in the experiment.

It is quite simple to perform undesigned experiments. The haphazard experiment designs of the old heat transfer require only that the equipment be made to operate at whatever steady-state conditions are readily obtained. They are also simple in that they usually call for only a few different test conditions and very little data must therefore be taken.

It is considerably more difficult to perform the carefully designed experiments of the new heat flow. These experiments must be performed in the prescribed manner and they call for many different test conditions and a great deal of data--as we shall see in the latter part of this chapter.

EXPERIMENT DESIGN--A SIMPLE EXAMPLE

The best way to describe "experiment design" is with a simple example. Suppose we know that y is a function of x and we wish to determine this function by performing y{x} experiments. In the old heat transfer, given that y and x are real world parameters, we would a priori deduce that the y{x} function is of the form

$$y = a x^{b} \qquad (1)$$

(I know that many readers will balk at this step, feeling that it is so unreasonable that it would NOT be the first step even in the old heat transfer. Those who balk at this step should review the nucleate boiling discussion in Bk 1, Ch 7. Replace the q and ΔT of that discussion with the y and x of this discussion and you will be forced to agree that eq 1 would indeed be the first step in the old heat transfer. Certainly it was the first step in the old way treatment of nucleate boiling for a period of thirty or forty years.)

Equation 1 indicates that we have only to investigate two values of x in order to completely determine the function y{x}. Equation 1 also indicates that we should plot the data on log log coordinates since the deduced function is a straight line on log log coordinates. In summary, the old way experiment design calls for investigating two or more values of x, plotting the y-x data points on log log graph paper, and drawing the best straight line through the data points. Precisely what values of x should be investigated in the experiment? Whatever values are most convenient. It makes little difference as long as there are at least two different values of x in the experiment.

In the new heat flow, our attitude is altogether different. We approach the experiment with an attitude of total ignorance and complete confidence. Total ignorance of the function y{x} and complete confidence that we can determine this function from the experiment alone--ie without the "aid" of a priori deduction. This requires that we design the experiment to obtain whatever data is required in order to clearly define the shape of y{x} when the y-x data points are plotted on linear graph paper. This means simply that we must investigate the complete spectrum of x values that can be obtained in the experimental facility and also that many, closely spaced values of x must be investigated in order to obtain a continuous picture of the unknown function. Figures 1 and 2 illustrate the difference between old way and new way results:

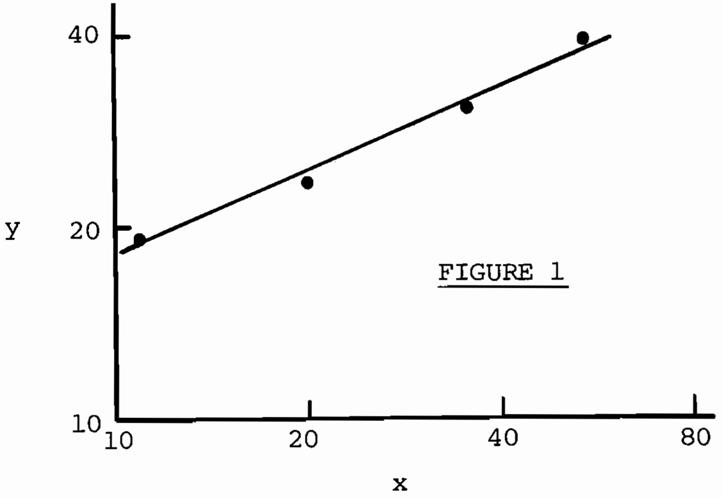


FIGURE 1

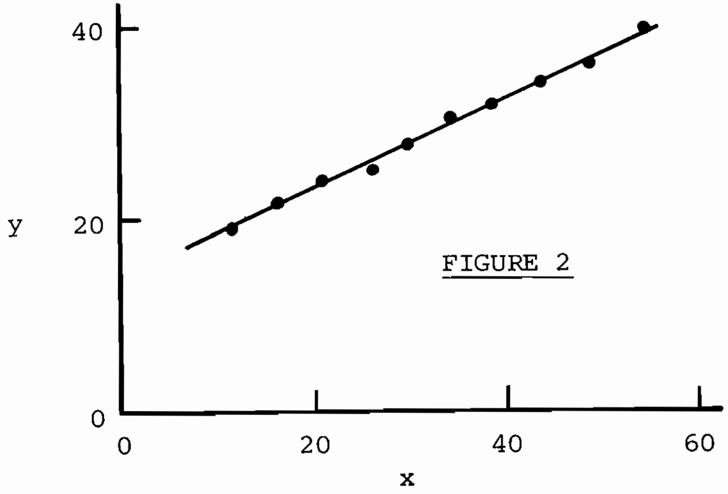


FIGURE 2

Figure 1 and its straight line are adequate in the old heat transfer because we deduced (perhaps by dimensional analysis) that the unknown function would have the form of eq 1 which obviously is a straight line on the log log coordinates of Fig 1. Therefore the problem in Fig 1 is NOT to determine whether the data suggest a straight line and thus verify the deduction. The problem in Fig 1 is to determine the slope of the best straight line that can be drawn through the y-x data points. In the old heat transfer, we have so much confidence in our deductions that we do not ask

> Were we correct in our deduction that y{x} would be a power law?
>
> Is there anything in the data which tends to verify our deduction that the unknown function would in fact be a power law?

These questions would be highly appropriate, but instead we ask merely

> What is the slope of the best fit straight line that can be drawn through the data points in Figure 1?

And so we measure the slope of the straight line in Fig 1 and then we use this slope to evaluate b and then a and then finally we conclude that our "experiment" has "verified" the results of our deduction. And yet the true function of the real world may not at all resemble a power law--may not at all resemble the deduced eq 1. The true function might very well be linear. The true x exponent might very well be one in spite of the fact that our "experiment" and our "analysis" of the data "indicate" a value of three or four!!!

(Those who are inclined to doubt that the methods of the old heat transfer are so ineffective that they can lead to a "measured" exponent of 3 or 4 even though the true exponent is 1 should review the nucleate boiling discussion in Bk 1, Ch 7. In the old heat transfer investigation of nucleate boiling, the ΔT exponent was "measured" repeatedly and found to be "3 or 4" in spite of the fact that the data generally indicated that the true exponent was 1. In other words, a highly linear function was investigated and the "results" indicated that it was a highly nonlinear function. In a very real sense, the methods of the old heat transfer led to the conclusion that a straight line was a highly curved line. This anomalous result was brought about by the a priori deduction of a function like eq 1 followed by experiments and data reduction identical to that suggested by Fig 1.)

In the new heat transfer, the experiment design task is to design the experiment in such a way that we can readily infer the functionality directly from the data. When only two parameters are involved such as in the y{x} experiment, the experiment design is more or less obvious. The design of the y{x} experiment calls for small, incremental changes in x with data taken at each value of x. The data are then plotted as in Fig 2 and from this figure we attempt to induce the simplest y{x} function which closely approximates the plotted results.

In summary, experiment design is largely unnecessary in the old heat transfer because it relies primarily on deduction rather than experiment to determine the functional relationships among the process parameters. On the other hand, experiment design is required in the new heat flow because we rely solely on the experiment and its data to suggest the functional relationships among the process parameters. The real purpose of experiment design is to simplify the problem of inducing correlations from the data. With good experiment design, it is a simple matter to induce correlations from the data. With the haphazard experiment design of the old heat transfer, it is virtually impossible. This creates no difficulty in the old heat transfer because the induction of correlations plays such a small role in the old heat transfer. The induction of correlations from data plays a leading role in the new heat flow and that is why it is so important to design experiments and to perform them in accordance with the design.

EXPERIMENT DESIGN--OLD WAY

In the old heat transfer, the experiment design problem is NOT to design an experiment which will tell us how q, D, G, C, μ, k, ΔT are related to each other. That we determine by the analysis and assumptions which lead us to conclude that

$$Nu = a\ Re^{b}\ Pr^{c} \qquad (2)$$

The design problem is to design an experiment which will "verify" eq 2 and will also serve to provide the data necessary to establish the values of a, b, c. However, as in the previous example and in the nucleate boiling example in Bk 1, Ch 7, only lip service is paid to the need to verify eq 2. In practice, the experiment is used only to establish the values of a, b, c. And of course, this is a very trivial problem.

To determine the values of a, b, c in eq 2, the experiment design must include at least two different values of Re, at least two different values of Pr, and a total of at least three data points. Moreover, the different values of Re and Pr can be attained in whatever manner is most convenient. For example, the different values of Re could be attained by testing two different values of D, but this would involve a considerable effort because it would require that we build and install two different test sections. It would be much simpler to accomplish the same end by testing two different values of G since this would involve nothing more than a valve adjustment. Similarly, the different values of Pr could be attained by testing two different fluids, but it would be much simpler to test only one fluid at two different temperatures.

As we "know" from eq 2, the effect on Nu is PRECISELY the same whether we double the flow rate or whether we double the diameter. Eq 2 tells us that it is neither the flow rate nor the diameter which is important--it is the diameter times the flow rate divided by the viscosity which is important--it is the Re which is important. The physical parameters are not important--it is their dimensionless product which is the important "parameter". That is why we concern ourselves with the "parameters" Nu, Re, and Pr. That is why we design the experiment around the "parameters" Nu, Re, and Pr. That is why the experiment is not concerned with unimportant things like q and G and D and ΔT.

(Those readers who doubt that I am faithfully describing the methods of the old heat transfer should refer to the film cooling discussion in Bk 1, Ch 6. In the investigation by Hartnett, Birkebak, and Eckert, the film cooling correlation obtained by the authors contained

the important parameter x/Ms

We should note that this important parameter is of course dimensionless and that M is actually G_s/G_m. Thus the parameter x/Ms is also given by

$$xG_m/sG_s$$

In their investigation, the authors varied the important parameter xG_m/sG_s over a considerable range and then obtained a more or less general correlation. But it is fair to ask "Precisely how was this parameter varied in the experiment?" "Was the parameter G_m varied?" And the correct answer to this latter question is no, G_m was not varied--the entire experiment and the general correlation are based on only one value of G_m. "Was the parameter s varied?" The correct answer is again no, s was not varied--the entire experiment and the general correlation are based on only one value of s. "Was the parameter G_s varied?" The correct answer is again no, G_s was not varied--the entire experiment and the general correlation are based on only one value of G_s. Using the methods of the old heat transfer, it was not necessary to be concerned about the physical parameters--the value of G_m is not important, the value of s is not important, the value of G_s is not important. What is important?

the important parameter x/Ms

How important is it? Important enough that we can excuse ourselves from dealing with the individual, physical parameters and can focus our attention on the group, dimensionless parameter.

And of course the point of this reference to the experiment by Hartnett, Birkebak, and Eckert is not to suggest that it is an isolated example nor to suggest that it is universally and without exception the only method used in the old heat transfer. The point of this reference is to suggest that it is indeed the reference method of the old heat transfer--that it is indeed the method widely used in the old heat transfer. And it is certainly a method which has no place in the new heat flow.)

In summary, the methods of the old heat transfer lead to an experiment design which calls for:

Two or more values of Re obtained by varying the flow rate.

Two or more values of Pr obtained by varying the temperature of a single test fluid.

Three or more data points (because the experiment is intended to establish the value of the three constants in eq 2).

Using the methods of the old heat transfer, this data will serve to describe one phase, forced convection heat transfer for all liquids, all gases, all flow rates, all geometries, all temperature differences.

PERFORMING THE EXPERIMENT--OLD WAY

Using the methods of the old heat transfer, the experiment is performed in whatever manner is most convenient. The equipment is made to operate at whatever conditions are readily obtained and data is obtained at those essentially unplanned conditions. In the old way performance of experiments, the key words are convenient and haphazard.

DESIGNING THE EXPERIMENT--NEW WAY

Now let us design an experiment based on the new attitude toward dimensions, dimensional consistency, and correlating parameters. Suppose we are interested in using the methods of the new heat flow to investigate the following heat flow behavior:

Type of heat flow: One phase, forced convection

Heat flow fluid: Water

Geometry: Inner surface of pipes; diameters ranging from 0.5 in to 4.0 in

Fluid flow rate: 1 to 20 ft/sec

Fluid temperature: 100 to 500 F

ΔT_{DF}: 0 to 150 F

The experimental facility and its instrumentation are described by:

Type of facility: Closed, pressurized loop with circulating pump, control valve, auxiliary heaters, and cooler. The facility will accomodate test sections of varying length and diameters of 0.5, 1.0, 2.0, and 4.0 in.

Type of heat input: Electrical heat generation in the walls of the pipe test section. To eliminate hot spots, the wall thickness is very uniform and the wall is made of a material which exhibits a zero temperature coefficient of electrical resistivity.

Facility instrumentation: Flowmeter, surface thermocouples on inner surface of pipe test section, thermocouples to measure coolant inlet and outlet temperatures, test section volts and amps, and a system pressure gauge.

In other words, the facility and its instrumentation are adequate to determine how q, D, T (referring to the bulk temperature of the water at the point of interest), G, P, and ΔT_{DF} relate to each other.

The experimental facility is such that only one pipe diameter can be tested at a given time. Therefore, when we install the first pipe test section, the problem will be simply to experimentally determine the function

$$q \rightarrow f\{G, P, T, \Delta T_{DF}\}_{D\,=\,\text{constant}} \tag{3}$$

(The reader has probably noticed before now that I use an arrow instead of an equals sign. This is my personal preference and serves as a reminder that it is not necessary to enforce dimensional identity because dimensional consistency is not found in Nature. It is not my contention that the arrow itself brings about a great improvement in engineering analysis and those readers who dislike the arrow should mentally replace it with an equals sign.)

Now let us tentatively assume (as we did on page 3-9) that relation 3 can be written in the simpler form

$$q \rightarrow f_1\{G\}\ f_2\{P\}\ f_3\{T\}\ f_4\{\Delta T_{DF}\}_{\text{constant } D} \qquad (4)$$

The reason relation 4 is so simple is that it tells us that the effect of each parameter can be separated from the effect of all the other parameters. Each function depends on only ONE parameter--f_1 depends only on G, f_2 depends only on P, etc. And this is very important to us because it means we can experimentally determine the individual functions in a very simple way. To determine f_1, we have merely to vary G in small increments and observe the effect on q while we maintain constant values of P, T, ΔT_{DF}. Then we have merely to plot this two-dimensional q{G} data on linear graph paper and the data will provide a graphical description of the f_1 function! And then the only problem left is to induce an analytical function which closely approximates the graphical function described by the q{G} data.

The real value of relation 4 is that, IF it is true, it allows us to simplify the very difficult problem represented by relation 3 by replacing it with four very simple problems represented by

$$q \rightarrow a f_1\{G\} \qquad P,\ T,\ \Delta T_{DF},\ \&\ D \text{ constant} \qquad (5)$$

$$q \rightarrow b f_2\{P\} \qquad G,\ T,\ \Delta T_{DF},\ \&\ D \text{ constant} \qquad (6)$$

$$q \rightarrow c f_3\{T\} \qquad G,\ P,\ \Delta T_{DF},\ \&\ D \text{ constant} \qquad (7)$$

$$q \rightarrow d f_4\{\Delta T_{DF}\} \qquad G,\ P,\ T,\ \&\ D \text{ constant} \qquad (8)$$

In a very real sense, the value of relation 4 is that it allows us to replace a five-dimensional problem with four two-dimensional problems and this greatly simplifies the problems of design and correlation.

Given only relation 3, it is an extremely difficult problem to correctly determine the functionality among the parameters, even with good experiment design. On the other hand, given relations 5-8 and four sets of data obtained in the new heat flow manner described above, it is no problem at all to correctly determine the functionality among q, G, P, T, and ΔT_{DF}. For example, given q{G} data obtained at constant P, T, ΔT_{DF}, and D, we induce f_1 by

1. Plotting the q{G} data points on LINEAR graph paper.

2. Fairing a curve through the data points. (If the experiment was properly designed, there should be many closely spaced data points and it should require absolutely no imagination or understanding in order to fair the curve. Moreover, if the researcher used good experimental technique, there should be little spread in the data.)

3. Inspecting the curve and attempting to answer the question: What simple analytical function is suggested by the curve? What simple analytical function closely approximates the curve?

4. If step 3 does not result in a reasonably simple analytical function, we conclude that a graphical function would be more useful and so we settle on the graphical function we drew in step 2 using the measured q{G} coordinates.

Step 3 requires a certain amount of imagination--it requires that we mentally consider all the simple analytical functions we know in order to find the one which is most strongly suggested by the data--the one which most closely resembles the data. We consider proportional functions, linear functions, logarithmic functions, trigonometric functions, polynomials, power laws--any simple function would be acceptable.

(It is instructive to compare these four steps of the new heat flow with their counterparts in the old heat transfer. In the old heat transfer, the "data" would be in the form Nu{Re} and we would "induce" the functionality between the "parameters" Nu and Re by

1. Plotting Nu{Re} on LOG LOG graph paper.

2. Drawing the best straight line through the data points. If the data did not at all resemble a straight line, we would draw the line through some portion of the data points and we would call this portion a "regime" and this regime would be given a name of its own such as the bubbly flow regime or the mist flow regime or the slug flow regime.

3. This step is unnecessary in the old heat transfer which rests largely on the assumption that Nature deals exclusively in so-called power laws. This outlandish assumption is the basis for plotting the data on log log coordinates, for drawing straight lines through the data plotted on log log graph paper, and for concluding that pronounced nonlinearity on log log coordinates is substantive evidence of a change in "regime". The instant we abandon this strange assumption, there is absolutely no rationale behind the largely unquestioning use of log log graphs in the old heat transfer.

4. This step is unnecessary in the old heat transfer because no function is so nonlinear that a straight line can not be drawn through some tiny region of the data--some invented "regime". By inventing regimes, we can conveniently ignore the overall problem which should be our real concern and instead we can focus attention on whatever small part of the problem can be handled with our ineffective methods. In this way, we not only excuse ourselves from dealing with the real problem, but we also "create" a number of other small problems (regimes), all of which must be dealt with by any designer/analyst who is confronted with the real problem in the real world and who is not free to focus his attention on the regime of his choice. The casual invention of regimes in the old heat transfer results in the expenditure of much time, money, and talent and artificially creates a great deal of confusion and complexity.

The above procedure is PRECISELY the method used in the old heat transfer to "determine" the functionality among q, D, and G for one phase, forced convection heat transfer. And the reader should take particular notice of the fact that this ENTIRE procedure does NOT require that we examine the data!

It has been my first hand experience that, in the old heat transfer, it sometimes happens that the results are correlated and the report published by persons who have NEVER SEEN the data! What sometimes happens is that a technician performs the experiment and obtains the data. Then an assistant transforms the real data to non-data--ie he transforms flow rates and diameters to Nusselt Numbers and Reynolds Numbers. And finally, when the data has become non-data, it is given to the person who is charged with analyzing and correlating it and preparing the report and presenting the talk. Without ever having seen the data.

In the new heat flow, it is not possible to avoid the data because the data is already in the same language that is used to correlate Natural phenomena. In the new heat flow, we study the data, we correlate the data, we publish the data. That is why the correlations of the new heat flow are so much better than those of the old heat transfer. And it is also the reason the literature of the new heat flow will be so much better and more useful than that of the old heat transfer.)

Returning to the new heat flow, f_2, f_3, and f_4 are determined in the same way we determined f_1. We determine f_2 by induction using the q{P} data obtained at constant G, T, ΔT_{DF}, and D; f_3 by induction using the q{T} data obtained at constant G, P, ΔT_{DF}, and D; f_4 by induction using the q{ΔT_{DF}} data obtained at constant G, P, T, and D.

Once we have determined these four functions, the last step is to determine how D affects one phase, forced convection heat flow. As before, we tentatively assume

$$q \rightarrow f_1\{G\}\ f_2\{P\}\ f_3\{T\}\ f_4\{\Delta T_{DF}\}\ f_5\{D\} \qquad (9)$$

Therefore the determination of f_5 requires merely that we select convenient values of G, P, T, and ΔT_{DF} and that these values be tested with each of the several pipe diameters involved in the investigation. In other words, relation 9 allows us to determine f_5 using the same simple method we used to determine f_1, f_2, etc.

Up to this point, our discussion has centered around how simple the overall problem would be if only the assumption inherent in relation 9 were true. Now it is time to ask

> How do we know that the assumption inherent in relation 9 is true? How do we know that relation 9 applies to the problem at hand?

There is only one correct answer to this question, and that answer is simple and direct:

> We DON'T know that the assumption inherent in relation 9 is true. We DON'T know that relation 9 applies to the problem at hand.

But since we DON'T know whether the relation 9 assumption is true, why have we spent several pages on a discussion of the simple methods which can be used ONLY when it is true?

The reason we have discussed experiment design for the case where the relation 9 assumption is true is that Nature SOMETIMES works in this fashion. And when She does, we want to be certain to recognize it and to take advantage of it in order to greatly simplify the overall problem.

How can we find out about the relation 9 assumption before performing the experiment? We can't. We can speculate/theorize/assume/suppose about this assumption before the experiment, but the only way to actually find out is to perform the experiment and study the data.

What is the answer? We need to know about the relation 9 assumption in order to design an optimum experiment, but the only way to find out about this assumption is to perform the experiment. What is the answer to this chicken and egg riddle? The answer is to design the experiment as though the relation 9 assumption were indeed true and to include in the experiment design whatever tests are necessary to demonstrate that it is true. Then, when the experiment is performed, we will be assured of obtaining whatever data is required to prove/disprove that the relation 9 assumption applies to the problem at hand.

If the experimental results indicate that the relation 9 assumption was true, then the experimental phase of the investigation is complete. On the other hand, if we find that the relation 9 assumption does not apply to the problem at hand, we will have to continue the experimental phase with a more powerful experiment design. This second design would be based largely on the results of the first set of experiments. (We will not discuss this second design here except to note that it requires more imagination than the first.

The first design is simple because the relation 9 assumption excuses us from considering interactions among the parameters. The second design must be more complex because we no longer have this excuse.)

How often will we find that the relation 9 assumption applies to the problem at hand? How often will we find that the simple design is adequate? I don't know. But I do know that

> THE SIMPLE DESIGN IS MORE GENERALLY
> APPLICABLE THAN DIMENSIONAL ANALYSIS.

How do I know that? Because I know that the adequacy of the simple design depends only on the relation 9 assumption whereas the adequacy of dimensional analysis depends on this SAME assumption AND four others. And certainly it is more likely that the one common assumption will apply to the problem at hand than that the common assumption and four others will apply.

How must we design the experiment in order to prove/disprove that relation 9 applies? Relation 9 states that each parametric function is unaffected by the value of the other parameters. For example, the G function is obviously unaffected by the values of P, T, ΔT_{DF}, or D; the P function is obviously unaffected by the values of G, T, ΔT_{DF}, or D; and similarly for the ΔT_{DF} and D functions. Therefore we can prove that relation 9 applies by demonstrating experimentally that the G function is not affected by parameters other than G, that the P function is not affected by parameters other than P, etc. Conversely, if the G function is affected by parameters other than G, it will prove that relation 9 does not apply, and similarly for the P, T, ΔT_{DF}, and D functions.

The demonstration that the G function in no way depends on the value of P, T, ΔT_{DF}, or D requires that we experimentally determine the following:

$$q\{G\} \quad \text{at} \quad P_1,\ T_1,\ \Delta T_{DF1},\ D_1 \qquad (10)$$

$$q\{G\} \quad \text{at} \quad P_2,\ T_1,\ \Delta T_{DF1},\ D_1 \qquad (11)$$

$$q\{G\} \quad \text{at} \quad P_1,\ T_2,\ \Delta T_{DF1},\ D_1 \qquad (12)$$

$$q\{G\} \quad \text{at} \quad P_1,\ T_1,\ \Delta T_{DF2},\ D_1 \qquad (13)$$

$$q\{G\} \quad \text{at} \quad P_1,\ T_1,\ \Delta T_{DF1},\ D_2 \qquad (14)$$

where the subscripts 1 and 2 refer to different values of the parameters. These five functions must be measured in order to demonstrate with certainty that the G function does not depend on the value of the other parameters. However, as a practical matter, little risk is involved in replacing this design with one which calls for the experimental determination of

$$q\{G\} \quad \text{at} \quad P_1,\ T_1,\ \Delta T_{DF1},\ D_1 \qquad (15)$$

$$q\{G\} \quad \text{at} \quad P_2,\ T_2,\ \Delta T_{DF2},\ D_2 \qquad (16)$$

This abbreviated form will seldom lead to an error about the G function and it will probably find more application than the complete form above.

Now let us summarize the results of our experiment design. If we could be absolutely certain that relation 9 were applicable, the experiment design could be limited to the measurement of

$$q\{G\} \text{ at } P_1, T_1, \Delta T_{DF1}, D_1 \qquad (17)$$

$$q\{P\} \text{ at } G_1, T_1, \Delta T_{DF1}, D_1 \qquad (18)$$

$$q\{T\} \text{ at } G_1, P_1, \Delta T_{DF1}, D_1 \qquad (19)$$

$$q\{\Delta T_{DF}\} \text{ at } G_1, P_1, T_1, D_1 \qquad (20)$$

$$q\{D\} \text{ at } G_1, P_1, T_1, \Delta T_{DF1} \qquad (21)$$

If we are not absolutely certain that relation 9 is applicable, the experiment must be designed to convincingly demonstrate that relation 9 is indeed applicable. This means that the design must call for the determination of the functions in 17-21 and also the five functions

$$q\{G\} \text{ at } P_2, T_2, \Delta T_{DF2}, D_2 \qquad (22)$$

$$q\{P\} \text{ at } G_2, T_2, \Delta T_{DF2}, D_2 \qquad (23)$$

$$q\{T\} \text{ at } G_2, P_2, \Delta T_{DF2}, D_2 \qquad (24)$$

$$q\{\Delta T_{DF}\} \text{ at } G_2, P_2, T_2, D_2 \qquad (25)$$

$$q\{D\} \text{ at } G_2, P_2, T_2, \Delta T_{DF2} \qquad (26)$$

If it is important to demonstrate with absolute certainty that relation 9 applies, the abbreviated design described by the functions in 17-26 will not be adequate. In such cases, the design must be expanded in the manner suggested by the functions in 10-14.

The functions in 17-26 prescribe the precise manner in which the experiment is to be performed--ie they are the "experiment design". The functions in 17-26 have no parallel in the old heat transfer because it is not necessary to design experiments using the old ways. And the reason it is not necessary is because the old heat transfer has little use for experiment and instead relies mainly on a priori deduction to determine the functionality among the parameters. Since the functionality is largely determined BEFORE the experiment, the experiment is used merely to establish the value of the few constants which can not be "determined" by a priori deduction. And for this simple purpose, any haphazard experiment design is adequate.

In the new heat flow, we rely almost entirely on the experiment to suggest the parametric functionality in the process under investigation. That is why it is so important to design experiments in the new heat flow.

It is certainly true that the new way requires a great deal more effort than the old way. But that is only because it is always more difficult to find out than to suppose.

PERFORMING THE EXPERIMENT--NEW WAY

It is a good deal easier to design experiments than to perform them. It is a good deal easier to write

> Measure q{G} at $T = 300$ F, $\Delta T_{DF} = 75$ F,
> $L = .909\ L_p$, $P = 400$ psia, $D = 0.5$ in.

than to operate the equipment and successfully obtain the desired data. It is a good deal easier to perform the generally haphazard experiments of the old heat transfer than the carefully designed experiments of the new heat flow. But the successful performance of carefully designed experiments is well within the realm of possibility.

Suppose we are given the experimental facility described on pages 4-10 and 4-11. Further suppose that the experiment objective is to

> Measure q{G} at $T = 300$ F, $\Delta T_{DF} = 75$ F,
> $L = .909\ L_p$, $P = 400$ psia, $D = 0.5$ in.
> (The active length of the pipe test secion is L_p. There is a surface thermocouple at the point of interest, $.909\ L_p$.)

The difficulty with performing this experiment is that we are required to control the equipment in such a way that the values of T and ΔT_{DF} will remain at their prescribed values as we incrementally increase or decrease the system flow rate. In other words, we are going to change the flow rate and then somehow adjust the system controls (which may be manual or automatic) in order to force T and ΔT_{DF} back to their prescribed values. This would be a simple matter if the system contained instruments which directly indicated T and ΔT_{DF} because then we could simply adjust the test section power level and the system temperature controls until the indicated values were the same as the desired values. (There is the additional difficulty that power level adjustments affect T because of the coolant temperature rise through the test section--ie power level and coolant temperature within the test section "interact". The end result of this interaction is that a certain amount of trial and error would be required in order to obtain the desired values of T and ΔT_{DF}, even if we had direct indicators.)

Since we do not have instruments which give a direct indication of the desired parameters, we must use those instruments which indirectly indicate the parameters of interest. We can do this by noting that, if $T = 300$F and $\Delta T_{DF} = 75$ F, then it must be true that

$$T_{surface} = 375 \text{ F} \qquad (27)$$

$$T_{.909L_p} = 300 = T_{in} + .909(T_{out} - T_{in}) \qquad (28)$$

Rearranging eq 28 in order to put it in a more convenient form gives

$$T_{out} = 330 - .10\ T_{in} \qquad (29)$$

Equations 27 and 29 are the operational guides which tell us how to adjust the test section power and the system temperature in order to maintain $T = 300$ F and $\Delta T_{DF} = 75$ F at a point which is 90.9% of the way from the test section inlet to the test section outlet. In other words, we can perform the desired experiment by setting the flow rate at a number of different values and, at each flow rate, adjusting the test section power level and the system temperature controls in order to satisfy eqs 27 and 29. In this way, the successful performance of the above carefully designed experiment is readily accomplished.

Suppose the experiment objective is to

> Measure q{T} at $G = 1.2 \times 10$ lbs/hr ft^2, $P = 400$ psia, $\Delta T_{DF} = 75$ F, $D = 0.5$ in, and $L = .909\ L_p$.

At these conditions, it must be true that

$$W_{water} = 27.3 \text{ lbs/min} \tag{30}$$

$$T_{surface} = T_{in} + .909(T_{out} - T_{in}) + 75 \tag{31}$$

$$T_{surface} = .909\ T_{out} + .091\ T_{in} + 75 \tag{32}$$

Therefore we can measure q{T} in the desired manner by changing T_{in} in small increments and then adjusting the water flow rate and the test section power as required in order to satisfy eqs 30 and 32.

Suppose the experiment objective is to

> Measure q{ΔT_{DF}} at $G = 1.2 \times 10$ lbs/hr ft^2, $P = 400$ psia, $T = 300$ F, $D = 0.5$ in, and $L = .909\ L_p$.

At these conditions, it must be true that

$$W_{water} = 27.3 \text{ lbs/min} \tag{33}$$

$$T_{out} = 330 - .10\ T_{in} \tag{34}$$

Therefore we can measure q{ΔT_{DF}} in the desired manner by changing the test section power level in small increments and then adjusting the water flow rate and the system temperature to satisfy eqs 33 and 34.

The above examples illustrate that it is readily possible to perform the designed experiments of the new heat flow provided that the operation of the equipment is well understood and the operational details are carefully planned in advance.

EXPERIMENT DESIGN--THE GENERAL METHOD

So far in this chapter we have applied the methods of "experiment design" only to the particular problem of one phase, forced convection heat flow. Now let us turn our attention to the application of "experiment design" to Natural phenomena in general.

The "experiment design" of the new heat flow proceeds in two phases:

Phase 1 is based on assuming and then demonstrating that, given a certain Natural phenomenon such that the parameter y depends on the parameters u, v, w, x, and z, the parametric functionality is described by a function of the form

$$y \rightarrow f_1\{u\}\ f_2\{v\}\ f_3\{w\}\ f_4\{x\}\ f_5\{z\} \tag{35}$$

The parameters in relation 35 MUST be those which can be measured and independently controlled in the experiment, either directly or indirectly. The essential method of Phase 1 is: a) experimentally determine the relation 35 functions one at a time by varying only one parameter while holding the others at fixed values; b) verify that relation 35 applies to the problem at hand by experimentally demonstrating that each function in relation 35 is unaffected by the values of the other parameters.

Expressed analytically, we note that for fixed values of v, w, x, z, relation 35 yields

$$y_i \rightarrow a_i f_1\{u\} \tag{36}$$

where the value of a_i depends ONLY on the values of v, w, x, z. Relation 36 states that we can determine f_1 by simply plotting y{u} data points obtained at fixed values of v, w, x, z. The graph obtained in this way would in fact be the graphical expression of $a_i f_1$ and it would be a simple matter to induce the function f_1 from such a graph.

PHASE 2 is required ONLY when the Phase 1 data indicate that relation 35 does NOT apply to the phenomenon being investigated. In Phase 2, the experiment design is based on the results of Phase 1 and for that reason it is not possible to describe a Phase 2 design "recipe" as we did for Phase 1.

It is not possible to know a priori whether Phase 2 will or will not be required. However, the results of the old heat flow indicate that Phase 2 will not often be required.

EXPERIMENT DESIGN--THE REAL PURPOSE

All experimental investigations have the same fundamental purpose and it is to FIND OUT about the functionality among the parameters which influence the phenomenon being investigated. The reason we want to find out about this functionality is generally so that we can, in the future, deal effectively and reliably with the phenomenon of concern. Once we accept that the real purpose of experiment is to FIND OUT, it becomes obvious that

> The purpose of experiment design is to SIMPLIFY the problem of FINDING OUT about parametric functionality. And it simplifies this problem whether we are dealing with heat flow, fluid flow, biology, horticulture, or whatever.

It will surprise some readers to discover that the purpose of experiment in the old heat transfer is NOT to find out about functionality--NOT to find out about real world behavior. The purpose of experiment in the old heat transfer is to "verify" a priori deduction--to confirm by experiment that a great deal was known about functionality before even the first experiment was performed--to demonstrate that there is that nebulous thing called "satisfactory agreement" between the artificial world of a priori deduction and the real world of experiment.

If the purpose of experiment in the old heat transfer had been to find out about real world behavior, it would not have been widely agreed for a period of thirty or forty years that nucleate boiling is described by

$$q \propto \Delta T^n \tag{36}$$

where n is usually about "3 or 4". The data of the real world indicate that nucleate boiling is usually described by a highly LINEAR relationship between q and ΔT and therefore that the ΔT exponent in the real world is usually 1 and NOT "3 or 4".

If the purpose of experiment in the old heat transfer had been to find out about real world behavior, it would not have been possible for three separate investigations performed at one of the greatest scientific institutes in the world to have agreed that transition boiling is a highly nonlinear phenomenon. It would not have been possible for the third investigator to conclude

> It was found, with the exception of some of the data presented in Fig 5, that the transition-boiling data lie along a straight line connecting the burnout point and the film-boiling minimum point on log-log graph paper. This is also true of the transition boiling data obtained by Braunlich and Kaulakis and Sherman.

The data obtained by the third investigator indicated that this relationship was highly LINEAR and in fact did NOT "lie along a straight line . . . on log-log graph paper". The reason the data seemed to lie along a straight line on log-log graph paper was because there generally was NO data obtained in the desired region. The data that was obtained indicated a high degree of linearity and this data is referred to in the above quote in the phrase "with the exception of some of the data . . .".

If the purpose of experiment in the old heat transfer had been to find out about real world behavior, it would not have been possible to reach the widespread and long lasting agreement that film cooling is largely determined by

the important parameter x/Ms

when in fact the real world translation of the "parameter" x/Ms indicates that film cooling behavior IN NO WAY depends on either M or s!!'

In the field of engineering science, what is it that designers/analysts MOST NEED to know in order to effectively and reliably design/analyze equipment? They most need to know about functionality--they most need to know how the process parameters relate to each other in the real world. In the new heat flow, we discover this real world behavior by experiment. In the new heat flow, experiment design simplifies the task of discovering this real world behavior, thereby increasing the likelihood that designers and analysts will have available what they most need--correlations which accurately describe real world behavior. And THAT is the real purpose of experiment design in the new heat flow.

EXPERIMENT DESIGN--THE SHORTCOMINGS

In the old heat transfer, dimensional analysis serves a number of purposes, the principal of which are

> To simplify the task of finding out about parametric functionality. Dimensional analysis accomplishes this end by substituting deduction for experiment. The end result of this process is that the functionality is determined largely by analysis and only very little is left over to be determined by experiment.
>
> To suppose/theorize/speculate about Natural phenomena which have never been experimentally investigated.
>
> To generalize the results obtained from several different but related experimental investigations.

On the other hand, the experimental design method of the new heat flow has a very singular purpose--to simplify the task of finding out about parametric functionality. Compared to dimensional analysis, experiment design has several "shortcomings". It is of no use in supposing/theorizing/speculating. It is of no use in generalizing the results of several different investigations.

Some readers will find fault with the method of experiment design because it has these shortcomings--because it is of no use for theorizing or generalizing. But what of that? A rifle is of no use for milking cows, but that does not make it less useful for shooting deer!

Experiment design will not entirely supplant dimensional analysis because it can not fill all the roles played by dimensional analysis in the old heat transfer. But experiment design WILL assume the role played by dimensional analysis in simplifying the task of finding out about parametric functionality. And it will take over this role because dimensional analysis does not really fill it. To simplify a problem means to help solve the problem by reducing its inherent complexity. Dimensional analysis does NOT do this--it does NOT help solve the problem--dimensional analysis makes it possible to AVOID the problem. It avoids the real problem of finding out by replacing it with the much simpler problem of supposing. And this is NO help.

It is true that experiment design will replace only one of the roles played by dimensional analysis in the old heat transfer. But it is equally true that this is no shortcoming.

EXPERIMENT DESIGN VS DIMENSIONAL ANALYSIS

This chapter asks the reader to decide whether experiment design or dimensional analysis should be the reference method for simplifying the task of finding out about parametric functionality. To help make this decision, let us consider a side-by-side comparison of the two methods.

1. Both methods take an incredibly difficult problem dealing with Natural behavior and attempt to simplify it to the point where it can be readily understood and solved. In the old way, this simplification is accomplished by analyzing the parameter dimensions in order to group the parameters and so invent a fewer number of dimensionless parameters which replace the physical parameters which are important in the process. In the new way, this simplification is accomplished by careful design and performance of the experiment in order to allow the experimental facility to describe the desired functionality in the language of data.

2. Both methods are based on assumption. The old way is based on FIVE questionable assumptions, some of which would be very difficult to truly verify and most of which are seldom stated or verified. The new way is based on only ONE questionable assumption and this one assumption is easily verified. Moreover, this same assumption is also made in the old way.

3. In both methods, it is necessary to select precisely which correlating parameters will be used. Using the old way, this selection is based primarily on assumption/deduction/speculation. Using the new way, this selection is based on the observation/truism that it is far better to correlate with parameters which were actually measured in the experiment.

4. In this chapter, we applied both methods to the problem of one phase, forced convection heat flow. Using the old way, we simplified the problem from the seven real world parameters q, D, G, C, μ, k, ΔT_{DF} to the THREE IMAGINARY WORLD parameters Nu, Re, and Pr. Using the new way, we simplified the same problem into several small problems, each involving only TWO REAL WORLD parameters.

5. The old way forces us to solve problems in a general way even when the experiment has very limited scope. For instance, it forced us to solve the problem of one phase, forced convection heat flow for all fluids even though we planned to test only one fluid. The new way allowed us to solve the problem for just the fluid we tested. In other words, the old way forces us to generalize even when we have no business doing so--ie the old way causes "truth and error (to be) so intimately mixed as to be undistinguishable". The new way permits us to distinguish between what we know and what we suppose--ie it permits us to separate "truth and error".

CONCLUSIONS

The conversion from the old dimensional analysis to the new experiment design will result in a considerable change not only in engineering research/development, but also in engineering design/analysis. No longer will it be necessary to deal with dimensionless parameters such as Nu, Re, Pr--no longer will it be necessary to deal with correlations based only on power laws--no longer will it be necessary to design/analyze equipment using correlations based largely on supposition rather than Natural behavior--no longer will it be necessary to design/analyze equipment using the expensive trial-and-error method which results from correlations which bear little or no resemblance to reality.

In a very real sense, the difference between the old dimensional analysis and the new experiment design is the difference between supposing and finding out. And the reason we abandon the old for the new is because finding out is better than supposing.

CHAPTER 5 DATA ANALYSIS AND CORRELATION

INTRODUCTION

In this chapter, we continue to discuss the experimental methods of the new heat flow using the vehicle of one phase, forced convection heat flow. For the sake of illustration, we imagine that we have performed the experiments in accordance with the Phase 1 design (see page 4-20, experiments 17-26) and we make up a more or less appropriate set of "data". We use this imaginary "data" to answer two questions:

1. Does the data indicate that relation 4-9 (Ch 4, relation 9) accurately applies to the phenomenon being investigated?
2. What parametric functionality is suggested by the data? What engineering correlation does the data suggest would best describe/interpret/resemble/agree with the phenomenon being investigated?

It is very important to note that

THIS CHAPTER USES IMAGINARY DATA

The sole purpose of this IMAGINARY data is to illustrate the methods of the new heat flow. The "conclusions" based on the "data" are only for the sake of illustrating the manner in which data can be made to reveal what we want to know. The data in this chapter is only the result of SUPPOSING and therefore we will truly find out NOTHING about the real world behavior of one phase, forced convection heat flow. However, we WILL find out how to apply the methods of the new heat flow to the problem of data analysis and correlation in general by dealing with one phase, forced convection heat flow in particular.

THE IMAGINARY DATA

As described in Chapter 4, the abbreviated form of the Phase 1 experiment design called for performing 10 small experiments described by

$$q\{G\} \quad \text{at } P_1,\ T_1,\ \Delta T_{DF1},\ D_1 \tag{1}$$

$$q\{G\} \quad \text{at } P_2,\ T_2,\ \Delta T_{DF2},\ D_2 \tag{2}$$

$$q\{P\} \quad \text{at } G_1,\ T_1,\ \Delta T_{DF1},\ D_1 \tag{3}$$

$$q\{P\} \quad \text{at } G_2,\ T_2,\ \Delta T_{DF2},\ D_2 \tag{4}$$

and similarly for $q\{T\}$, $q\{\Delta T_{DF}\}$, and $q\{D\}$. The subscript 1 and 2 nominal values in the experiments were

	Nominal Value, subscript 1	Nominal Value, subscript 2
T, F	150	450
P, psia	125	2000
G, lbs/hr ft^2	$.40 \times 10^6$	3.0×10^6
ΔT_{DF}, F	40	150
D, in	.5	2.0

To simplify the discussion in the remainder of this chapter, we will use the shorthand notation

$$q\{G\}_1$$

to indicate the function $q\{G\}$ with the parameters P, T, ΔT_{DF}, and D maintained at their subscript 1 values. Thus the Phase 1 design calls for us to measure $q\{G\}_1$,

$q\{G\}_2$, $q\{P\}_1$, $q\{P\}_2$, etc.

The IMAGINARY data obtained by performing the Phase 1 experiment is presented in Tables 1-5, pages 5-4 to 5-8. These tables contain a great deal of "data", but even more data would be desirable. (For instance, it would be desirable to have the experiments performed in even smaller increments than those shown in the tables. And we would almost certainly perform the same experiments with the 1.0 and 4.0 inch diameters as we did with the 0.5 and 2.0 inch diameters.)

Using the haphazard experiment designs of the old heat transfer, large quantities of data can be confusing. On the other hand, the experiment design method of the new heat flow reduces the overall problem to several small, 2-dimensional problems and, since we then must cope with only two dimensions, large quantities of data are not at all confusing--they are enlightening.

(I have heard more than one practitioner of the old heat transfer express the view that it is better to restrict the investigation to a few data points because more than that would only serve to confuse the results. The film cooling experiment discussed in Bk 1, Ch 6 is a case in point. The data reported by Hartnett, Birkebak, and Eckert was very consistent and nothing about it was confusing. This "consistency" resulted from the fact that virtually nothing was varied in the investigation--in essence, there was only ONE data point! Recall that the ENTIRE investigation and the generalized conclusion were based on only ONE mainstream flow rate, only ONE coolant flow rate, only ONE slot size, only ONE slot geometry.

Samples of one ALWAYS show EXCELLENT consistency. No sample of one has EVER been found which disagreed with itself. Samples of one are of ABSOLUTELY NO USE for finding out anything about functionality. And for that reason, samples of one have no value in the new heat flow.)

TABLE 1

PHASE 1 q{G} DATA

IMAGINARY DATA

q B/hr ft^2 x 10^3	G #/hr ft^2 x 10^6	P psia	T F	ΔT_{DF} F	D in	Line #
16.8	.20	125	148	40	0.5	1
29.6	.40	130	151	39	"	2
41.2	.61	120	147	41	"	3
55.7	.90	120	149	39	"	4
71.2	1.21	125	153	40	"	5
89	1.62	125	148	40	"	6
107	1.99	130	150	41	"	7
125	2.53	120	151	39	"	8
148	3.02	120	147	40	"	9
168	3.48	125	149	41	"	10
183	3.97	120	148	40	"	11
82	.20	2000	447	150	2.0	12
143	.40	1975	452	152	"	13
194	.59	1975	448	151	"	14
273	.88	2025	452	148	"	15
344	1.21	2000	451	149	"	16
425	1.59	2000	447	150	"	17
515	2.01	1975	448	150	"	18
607	2.53	2025	451	152	"	19
701	2.99	2000	450	151	"	20
810	3.51	2000	451	148	"	21
890	3.98	2025	450	151	"	22

IMAGINARY DATA

TABLE 2

PHASE 1 q{P} DATA

IMAGINARY DATA

q B/hr ft^2 x 10^3	P psia	G #/hr ft^2 x 10^6	T F	ΔT_{DF} F	D in	Line #
29.2	60	.40	151	40	0.5	1
29.4	125	.41	148	39	"	2
29.2	200	.39	149	41	"	3
29.3	420	.42	147	41	"	4
29.6	610	.41	151	40	"	5
29.5	790	.39	152	40	"	6
29.1	980	.39	148	39	"	7
29.2	1300	.40	149	41	"	8
29.8	1620	.41	150	40	"	9
29.5	2025	.38	150	41	"	10
704	725	3.01	349	149	2.0	11
712	850	3.00	351	151	"	12
710	975	3.02	348	150	"	13
702	1150	2.98	352	150	"	14
705	1300	2.98	349	151	"	15
711	1425	3.00	348	149	"	16
710	1575	3.02	351	149	"	17
707	1750	3.03	353	151	"	18
713	1900	2.99	352	149	"	19
703	2025	2.97	348	150	"	20

IMAGINARY DATA

Note that it was necessary to reduce T_2 in order to avoid boiling at the interface.

TABLE 3

PHASE 1 q{T} DATA

IMAGINARY DATA

q B/hr ft^2 x 10^3	T F	G #/hr ft^2 x 10^6	P psia	ΔT_{DF} F	D in	Line #
23.5	102	.40	1100	39	0.5	1
29.8	151	.41	1125	39	"	2
34.3	202	.39	1075	40	"	3
39.8	248	.39	1100	39	"	4
42.5	301	.41	1100	41	"	5
46.2	350	.41	1125	41	"	6
48.4	398	.40	1125	40	"	7
50.6	452	.39	1075	40	"	8
52.1	501	.39	1075	41	"	9
336	99	3.0	2000	148	2.0	10
413	151	3.0	1975	151	"	11
490	198	3.1	1975	150	"	12
545	251	2.9	2000	151	"	13
607	302	2.9	2000	148	"	14
639	348	3.0	1975	149	"	15
683	401	3.1	2025	148	"	16
710	450	3.1	2025	153	"	17
725	480	2.9	1975	152	"	18

IMAGINARY DATA

Note that it was necessary to increase P_1 in order to avoid boiling at the interface.

TABLE 4

PHASE 1 q{ΔT_{DF}} DATA

IMAGINARY DATA

q B/hr ft^2 x 10^3	ΔT_{DF} F	G #/hr ft^2 x 10^6	P psia	T F	D in	Line #
7.1	10	.40	125	149	0.5	1
15.4	21	.40	130	150	"	2
21.5	30	.39	125	152	"	3
29.0	40	.39	130	150	"	4
36.8	51	.41	120	148	"	5
49.6	69	.39	120	149	"	6
65.0	89	.41	130	148	"	7
79.6	110	.40	125	149	"	8
95	131	.40	120	150	"	9
109	149	.41	125	151	"	10
125	169	.39	130	151	"	11
42.2	9	.39	2000	450	2.0	12
98	21	.39	1975	449	"	13
134	29	.40	1975	451	"	14
188	40	.39	2025	448	"	15
233	51	.40	2000	450	"	16
326	70	.41	2000	451	"	17
420	91	.41	2025	451	"	18
522	112	.39	1975	449	"	19
603	129	.40	1975	450	"	20
702	150	.40	2000	448	"	21
797	170	.41	2025	449	"	22

IMAGINARY DATA

TABLE 5

PHASE 1 q{D} DATA

IMAGINARY DATA

q B/hr ft^2 x 10^3	D in	G #/hr ft^2 x 10^6	P psia	T F	ΔT_{DF} F	Line #
30.1	0.5	.40	125	149	40	1
26.2	1.0	.41	120	151	40	2
22.8	2.0	.39	120	151	40	3
19.9	4.0	.40	130	150	40	4
943	0.5	3.0	2000	450	150	5
821	1.0	3.0	1975	451	150	6
715	2.0	3.1	1975	451	150	7
622	4.0	2.9	2025	449	150	8

IMAGINARY DATA

Note that the tables are arranged so that the first two columns are the important ones. Each table contains the results of a 2-dimensional experiment and the tables are arranged to emphasize the two dimensions of importance. For example, the q{G} table is arranged so that the q column is first and the G column second; the q{P} table is arranged so that the q column is first and the P column second. Columns 3 to 6 in each table are of only secondary importance in that they contain the parameters which were held at fixed nominal values during the experiment. These other columns are reported for two reasons:

1. To demonstrate that the parameters in these columns were indeed maintained at values near the desired nominal values.

2. To provide the data necessary to make second order corrections required because the parameters in these columns were not maintained at precisely the desired values.

Notice that the performance of designed experiments does NOT require that the controlled parameters be held at precisely their desired values. As we shall see in this chapter, the analysis of the data is simplified if the parameters are closely controlled within a narrow tolerance of their intended values. However, as we decrease the allowable tolerance in order to simplify the data analysis, we also increase the operational difficulty involved in successfully performing the experiment. In selecting the allowable control tolerance, the experiment designer must consider its effect on operational difficulty. The data in the tables is intended to suggest that the control tolerances were selected so as to make the experiments operationally difficult but not impossible.

It should also be noted that the "data" in the tables is not raw data but has been considerably reduced from the millivolts and pressure drops which are the language of raw data.

STEADY-STATE CRITERIA

The data in the tables is steady-state data. But precisely what is meant by "steady-state data"? How did the operators determine when "steady-state" set in? Was it when the temperatures were changing at less than 1 F per hour? (A maximum rate of temperature change of 1 F per hour is sometimes used as a steady-state criterion in the old heat transfer.) Was it when several repeat readings gave essentially the same results? (This criterion is also used in the old heat transfer.) Or did they use some altogether different criteria in order to determine when "steady-state data" could be obtained?

The methods of the old heat transfer do not lend themselves to the collection of large quantities of steady-state data. The old heat transfer generally requires that the experimental equipment be painfully steady-state if one intends to obtain reliable steady-state data. This means that a good deal of time is required to obtain each data point and the time required to obtain a large number of data points would generally be prohibitive.

The new heat flow contends that reliable steady-state data can be obtained from equipment which is NOT painfully steady-state. It requires only that the equipment be ESSENTIALLY steady-state--ie it insists only that the transient effects be negligible compared to the steady-state conditions. For example:

> Suppose we are obtaining data from a heat exchanger whose wet heat capacity is 150 B/F. At this test operating condition, the data indicates that Q = 600,000 B/hr and that the system temperature is changing at a rate of 20 F/hr. Does this constitute "essential steady-state"? If we read the data at this condition, would we be obtaining "steady-state data"? Would the 20 F/hr temperature change significantly affect the data so that it would differ significantly from truly steady-state data?
>
> The answer is obtained quite simply by noting that
>
> $$Q_{total} = Q_{flowing} + Q_{stored} \qquad (5)$$
>
> Using the wet heat capacity (a very conservative approach), we estimate the transient effect to be
>
> $$Q_{stored} \simeq 150 \times 20 = 3000 \text{ B/hr} \qquad (6)$$
>
> which is indeed small and negligible compared to 600,000 B/hr. We therefore conclude that, for our purposes, the heat exchanger is at essential steady-state and therefore that the above is a satisfactory operating condition at which to take steady-state data.

There is another aspect which must be considered before concluding that steady-state data can be obtained from equipment which is only at essential steady-state. We must consider the time required to obtain a full set of data--the elapsed time involved in reading all the instruments either manually or automatically. The time required must be small enough that there would be little change in the system parameters during this time. For example:

> Suppose the time required to read out all the data were 15 minutes. If the system temperature were changing at a rate of 20 F/hr, the system temperature would change
>
> $$20 \times (15/60) = 5 \text{ F} \qquad (7)$$

during the time required to take a set of readings. We would probably conclude that this large a change would be unacceptable and we might therefore set up the operational requirement that the system temperature be changing less than 4 F/hr when "steady-state data" is being taken (in which case the system temperature would change less than 1 F during a set of readings). Or we might decide to install whatever readout equipment is required in order to read the data in one or two minutes, in which case a temperature ramp of 20 F/hr would be acceptable. Or we might decide to decrease the data taking time by having a large number of data takers.

It will oftentimes be desirable to demonstrate that satisfactory steady-state data were obtained even though the data were obtained under conditions which were not truly steady-state. This is best accomplished by repeating several experimental points with the equipment highly steady-state and demonstrating that the same data was obtained in this condition as in the test condition.

In summary then, two criteria must be satisfied in order to obtain "steady-state data":

1. Transient effects must be negligible.
2. The data taking time must be short enough that no parameter would change significantly while the data is being taken.

Any doubt about what constitutes essential steady-state should be removed by experiment. (For completeness, tables containing "steady-state data" should include a description of the criteria used to determine that the equipment was operating in an essentially steady-state condition.)

PLOTTING THE DATA

In the new heat flow, we have a very specific reason for plotting the data. We consider that the overall purpose of experiment is to determine parametric functionality and we recognize that a graph of the data is actually a "picture" of the desired functionality. We have a strong interest in making this picture as clear and concise as possible because we want it to suggest an analytical function which will closely resemble the picture. Before actually plotting the data, we must make a decision:

> What coordinate system will result in the clearest picture? What type of graph paper should we use? Linear, semilog, log log? What graph paper would least distort our experimentally determined "picture" of the desired function?

If this seems like a trivial decision, bear in mind that the overall purpose of our investigation is the determination of an analytical function which will accurately describe the phenomenon being studied. If we are to "see" this analytical function in the graphical picture, it is essential that we eliminate any distortion in the picture because otherwise we might be unable to recognize even the simplest function.

The nucleate boiling discussion in Bk 1, Ch 7 is an excellent example of how distortion can make it virtually impossible to "see" the correct functionality. For two or three decades, saturated pool nucleate boiling data was plotted on log log graph paper because this is the preferred coordinate system of the old heat transfer. Now of course there can be no harm in plotting data on whatever graph paper one prefers. The harm comes in AFTER we have plotted the data. The harm comes in when we attempt to induce the functionality from the graph--when we attempt to "see" the analytical function by studying the function's picture. In the nucleate boiling example, the picture on log log graph paper suggested a power law to many researchers, even though their data did not at all resemble a power law. Why? Why was it so hard to "see" that the data did not suggest a power law? Because log log coordinates DISTORT and make it VIRTUALLY IMPOSSIBLE to induce the proper functionality. It is so difficult to correctly induce functionality from data plotted on log log coordinates that 20 or 30 years was not long enough to determine that saturated pool nucleate boiling data generally suggest a function of the simple form

$$y = mx + b \tag{8}$$

Yet, when this same data is plotted on linear coordinates, it is immediately obvious that the data strongly suggest that the functionality is of the form of eq 8. (I do not mean to suggest that EVERY researcher concluded that the nucleate boiling function was a power law. What I mean to suggest is that, in the old heat transfer, there was very widespread agreement that pool nucleate boiling behavior generally described a power law. This agreement covered a time span of at least 20 or 30 years and even today (1975) is quite widespread.)

There is a myth in the old heat transfer that plotting the data on log log graph paper will make it appear to be more consistent--will make the scatter seem less significant. I offer without proof that this myth is simply not true. It has no more foundation in fact than the myth that Newton conceived of the "heat transfer coefficient" of the old heat transfer.

There is only ONE coordinate system which does not distort and it is perhaps not necessary to say that that one system is linear coordinates. And that is why linear graph paper is the mainstay of the new heat flow. And why the data from Tables 1-5 are plotted on LINEAR coordinates in Figs 1-5, pages 5-23 to 5-27.

CORRECTING THE DATA FOR PARAMETER DEVIATIONS

It is obvious from Tables 1-5 that the parameters which were to be held at fixed values during each experiment were allowed to deviate somewhat from their desired values. For example, the experiment design called for measuring q{G} with T, P, ΔT_{DF}, and D held at fixed values. However, it was not operationally feasible to hold T, P, and ΔT_{DF} at precisely their desired values as G was varied independently and q dependently. To make the performance of the experiment operationally feasible, it was necessary to permit certain operational tolerances within which T, P, and ΔT_{DF} were to be held. The end result of these tolerances is that the measured q{G} is not precisely the desired function. However, we can quite accurately estimate the desired function by making whatever second order corrections are required in order to compensate for the operational deviations from the desired parametric values.

The simplest way to make these second order corrections is as follows:

1. From Figs 1-5, determine the value of

$$\left(\frac{dq}{q\ d\text{Parameter}}\right)$$

   for the parameters P, T, G, and ΔT_{DF} at the subscript 1 and subscript 2 values.

2. For each parameter which deviates from the desired value, determine the q correction from

$$\Delta q_{corr} = -q_{meas}(\Delta\text{Parameter})\left(\frac{dq}{q\ d\text{Parameter}}\right) \tag{8}$$

The procedure is illustrated in the following example:

Suppose we wish to correct line 3 of Table 1:

Actual Value	Desired Value	Δ = Actual-Desired
$q = 41.2 \times 10^3$ B/hr ft^2	NA	NA
$G = .61 \times 10^6$ #/hr ft^2	NA	NA
$P = 120$ psia	125	-5 psia
$T = 147$ F	150	-3 F
$\Delta T_{DF} = 41$ F	40	+1 F
$D = .5$ in	.5	0

Determine $\left(\frac{dq}{q\,dP}\right)$ from Fig 2 by noting that

$$\frac{dq}{dP} = 0 \qquad (9)$$

$$\left(\frac{dq}{q\,dP}\right) = 0 \qquad (10)$$

Determine $\left(\frac{dq}{q\,dT}\right)_{T=150}$ from Fig 3 by noting

$$\left(\frac{dq}{dT}\right)_{T=150} = 110 \text{ B/hr ft}^2 \text{ F} \qquad (11)$$

$$q_{T=150} = 29{,}600 \text{ B/hr ft}^2 \qquad (12)$$

and therefore that

$$\left(\frac{dq}{q\,dT}\right)_{T=150} = .0037 \text{ F}^{-1} \qquad (13)$$

Similarly, from Fig 4 we obtain

$$\left(\frac{dq}{q\,d\Delta T_{DF}}\right)_{\Delta T_{DF}=40} = .025 \text{ F}^{-1} \qquad (14)$$

Therefore we correct the measured value of q from line 3, Table 1 by noting

$$q_{corr} = q_{meas}(1 + 5(0) + 3(.0037) - 1(.025)) \qquad (15)$$

$$q_{corr} = 41{,}200\ (.9861) = 40{,}600 \text{ B/hr ft}^2 \qquad (16)$$

All the q values in Tables 1-5 would normally be corrected in the manner described above and then the corrected versions of Figs 1-5 would probably be prepared. It would be quite tedious to make all these corrections by hand, but of course it would be no problem at all if a computer were available. (Most desk top computers would be adequate to handle even very large quantities of data.)

The above method of correcting the data is obviously based on linearization. This method is quite satisfactory for small corrections but unsatisfactory for large corrections. For this reason, the experiment should be designed and performed with the goal of keeping the ultimate corrections as small as possible. A reasonable objective is to keep the corrections less than 10% (total).

We noted on page 5-9 that the data analysis would be considerably simplified if the fixed parameters were held within a very narrow tolerance. The intent of that statement was that the above corrections would be unnecessary if the tolerances were small enough to have a negligible effect on q.

VERIFYING THE ASSUMPTION

As we discussed before, Phase 1 is based on assuming that one phase, forced convection heat flow is described by a function of the form

$$q \rightarrow f_1\{G\}\, f_2\{P\}\, f_3\{T\}\, f_4\{\Delta T_{DF}\}\, f_5\{D\} \qquad (17)$$

This assumption is questionable in that there is no good reason to suppose that it is universally applicable. For this reason we must verify relation 17 by experiment. If relation 17 is true, then it must also be true that

$$q\{G\}_i \rightarrow a_i f_1\{G\} \qquad (18)$$

$$q\{P\}_i \rightarrow b_i f_2\{P\} \qquad (19)$$

$$q\{T\}_i \rightarrow c_i f_3\{T\} \qquad (20)$$

$$q\{\Delta T_{DF}\}_i \rightarrow d_i f_4\{\Delta T_{DF}\} \qquad (21)$$

$$q\{D\}_i \rightarrow e_i f_5\{D\} \qquad (22)$$

where a, b, c, d, e are scale factors whose values depend only on the values of the "other" parameters which do not explicitly appear in each equation. We can show in a convincing way that relation 17 is true by demonstrating that relations 18-22 are true. If relations 18-22 are true, then it must be true that

$$q\{G\}_i \propto a_i \qquad (23)$$

$$q\{P\}_i \propto b_i \qquad (24)$$

and similarly for $q\{T\}$, $q\{\Delta T_{DF}\}$, and $q\{D\}$. To demonstrate in a convincing way that 23 is true, we must design an experiment which includes at least two considerably different values of a_i which in turn must include considerably different values of P, T, ΔT_{DF}, and D. (This is the reason we had the subscript 1 and subscript 2 values in the experiment.) If 23 is true, then the experiment will indicate that

$$\frac{q\{G\}_{i=1}}{q\{G\}_{i=2}} = \frac{a_1}{a_2} = \text{constant} \qquad (25)$$

and this is a rigorous proof that 23 is true. (The notation in eq 25 is rather troublesome. The intent of eq 25 is to indicate that, if the q functions are evaluated at the same value of G, then the ratio of the functions is independent of G as required by both 18 and 23.) If we obtain a result similar to eq 25 for P, T, ΔT_{DF}, and D, this constitutes a rigorous proof that relation 17 is true.

To illustrate the above by an example, note that Table 1 contains the results of an experiment designed to have two considerably different scale factors which we denote by subscripts 1 and 2. The subscript 1 results are in the upper half of the table and the subscript 2 results are in the lower half. Also note that each subscript 1 data point has a counterpart in the subscript 2 region of the table--ie the subscript 2 experiment was performed at essentially the same G values used in the subscript 1 experiment. Also note that the table is constructed in such a way that

$$G_{\text{line } i} \simeq G_{\text{line } (i+11)} \qquad (26)$$

$$(0 < i < 12)$$

If eq 25 is true, then eq 26 tells us that the Table 1 results are so arranged that

$$\frac{q_{\text{line } i}}{q_{\text{line } (i+11)}} = \text{constant IF eq 25 is true} \qquad (27)$$

$$\frac{q_{\text{line } 1}}{q_{\text{line } 12}} = \frac{q_{\text{line } 2}}{q_{\text{line } 13}} = \frac{q_{\text{line } 3}}{q_{\text{line } 14}} \text{ etc.} \qquad (28)$$

Solving eq 27 for Table 1 we obtain:

i	q_i/q_{i+11}
1	.205
2	.207
3	.212
4	.204
5	.207

etc.

(Note that we are using the uncorrected values of q--ie we are not going to correct the data for the effects of operational tolerances. We are going to deal with the data as though the effects of the tolerances were negligible, even though this is not strictly true in the Tables 1-5 results.)

The complete solution of eq 27 for Tables 1-5 is given in Table 6, next page. (Note that eq 27 is a conditional equation and we are trying to determine whether or not the condition is satisfied in order that we may conclude whether or not the assumption inherent in relation 17 accurately applies to our problem. Also note that the constant 11 in eq 27 varies from table to table since it is actually the number of experiments per subscript.)

TABLE 6

VERIFYING THE ASSUMPTION BY SOLVING EQ 27

i	q_i/q_{i+11} from Table 1, q{G}	q_i/q_{i+10} from Table 2, q{P}	q_i/q_{i+9} from Table 3, q{T}	q_i/q_{i+11} from Table 4, $q\{\Delta T_{DF}\}$	q_i/q_{i+4} from Table 5, q{D}
1	.205	.0415	.0700	.168	.0319
2	.207	.0413	.0722	.157	.0319
3	.212	.0411	.0700	.160	.0319
4	.204	.0417	.0730	.154	.0320
5	.207	.0420	.0700	.158	
6	.209	.0415	.0723	.152	
7	.208	.0410	.0709	.155	
8	.206	.0413	.0713	.152	
9	.211	.0418		.158	
10	.207	.0420		.155	
11	.206			.157	

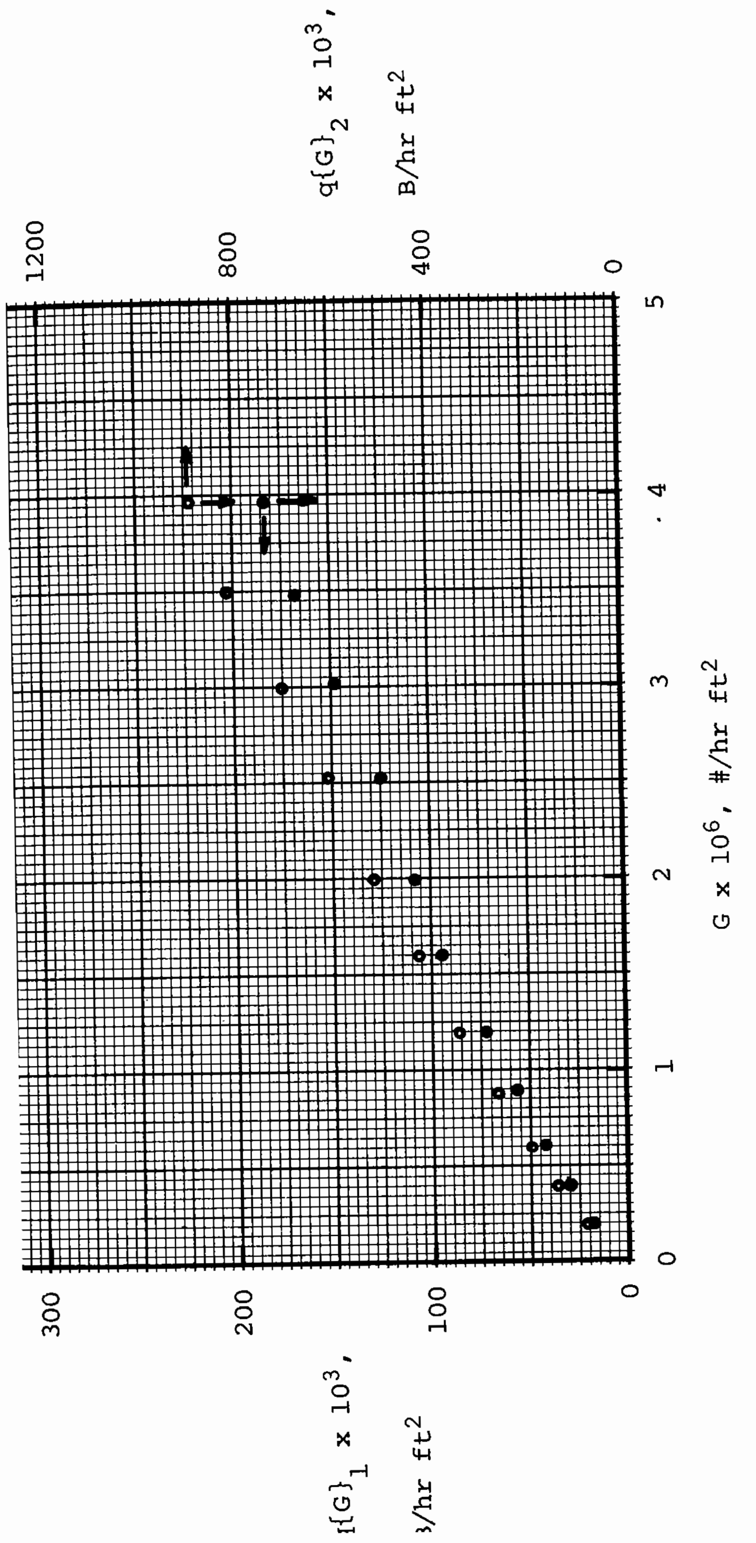
Table 6 is what we have been striving for--a concise summary of the experiment results which can be used to prove/disprove that relation 17 accurately applies to our problem. If relation 17 applies, then

> The values WITHIN EACH COLUMN must show no pattern or trend. The values within each column must look more or less like a collection of disordered numbers with small range.

Conversely, if any column in Table 6 indicates a trend (such as a steady increase or decrease within the column), then we must conclude that relation 17 does NOT apply to the problem at hand and we must begin to plan the Phase 2 experiment.

Inspection of Table 6 shows that there is indeed no trend in any column. We therefore conclude that Table 6 verifies the tentative assumption that relation 17 applies with reasonable accuracy to one phase, forced convection heat flow to water flowing in pipes.

Now that we have verified the assumption inherent in relation 17, we can take advantage of the great simplification afforded by relation 17. We can now forget about the fact that we are dealing with a problem which in fact has SIX dimensions and instead we can deal with several small problems, each of which has only TWO dimensions. And now we shall solve these small problems one at a time by looking at 2-dimensional "pictures" and trying to induce a simple, analytical function from each "picture". These pictures are presented on the next five pages as Figures 1-5. The figures are of course the graphical illustrations of the functions q{G}, q{P}, q{T}, q{ΔT_{DF}}, and q{D} as described by the data.



<u>FIGURE 1</u> q{G}$_1$ and q{G}$_2$ data from Table 1

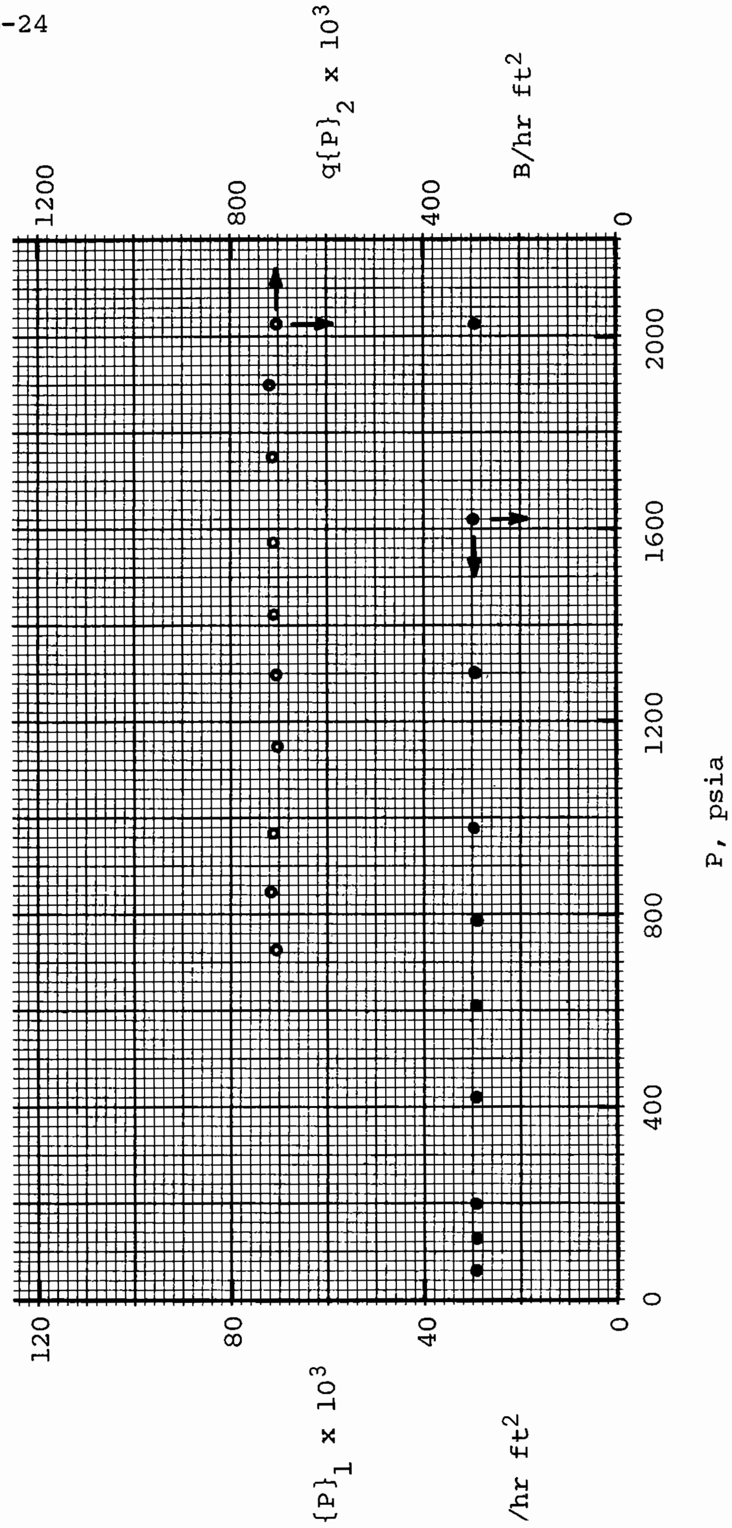


FIGURE 2 q{P}$_1$ and q{P}$_2$ data from Table 2

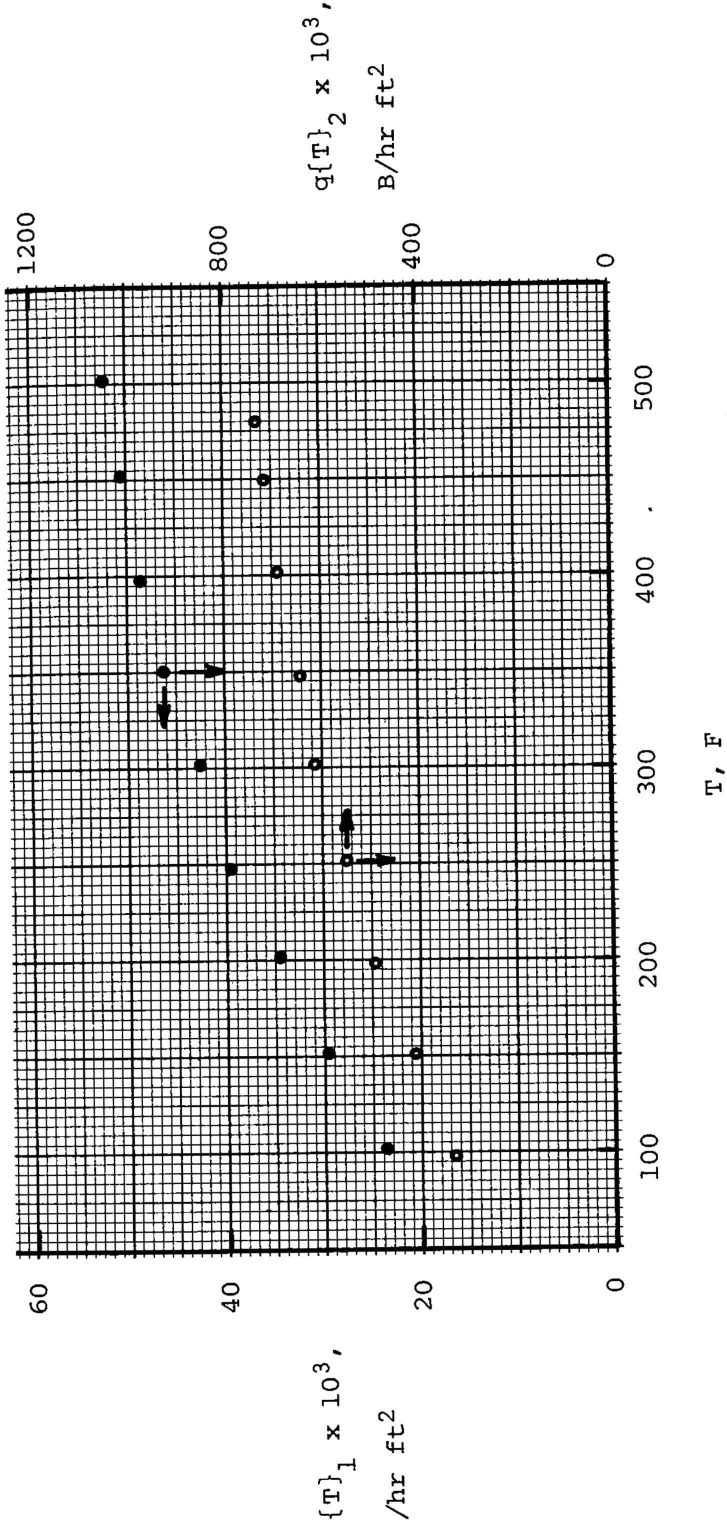


FIGURE 3 q{T}$_1$ and q{T}$_2$ data from Table 3

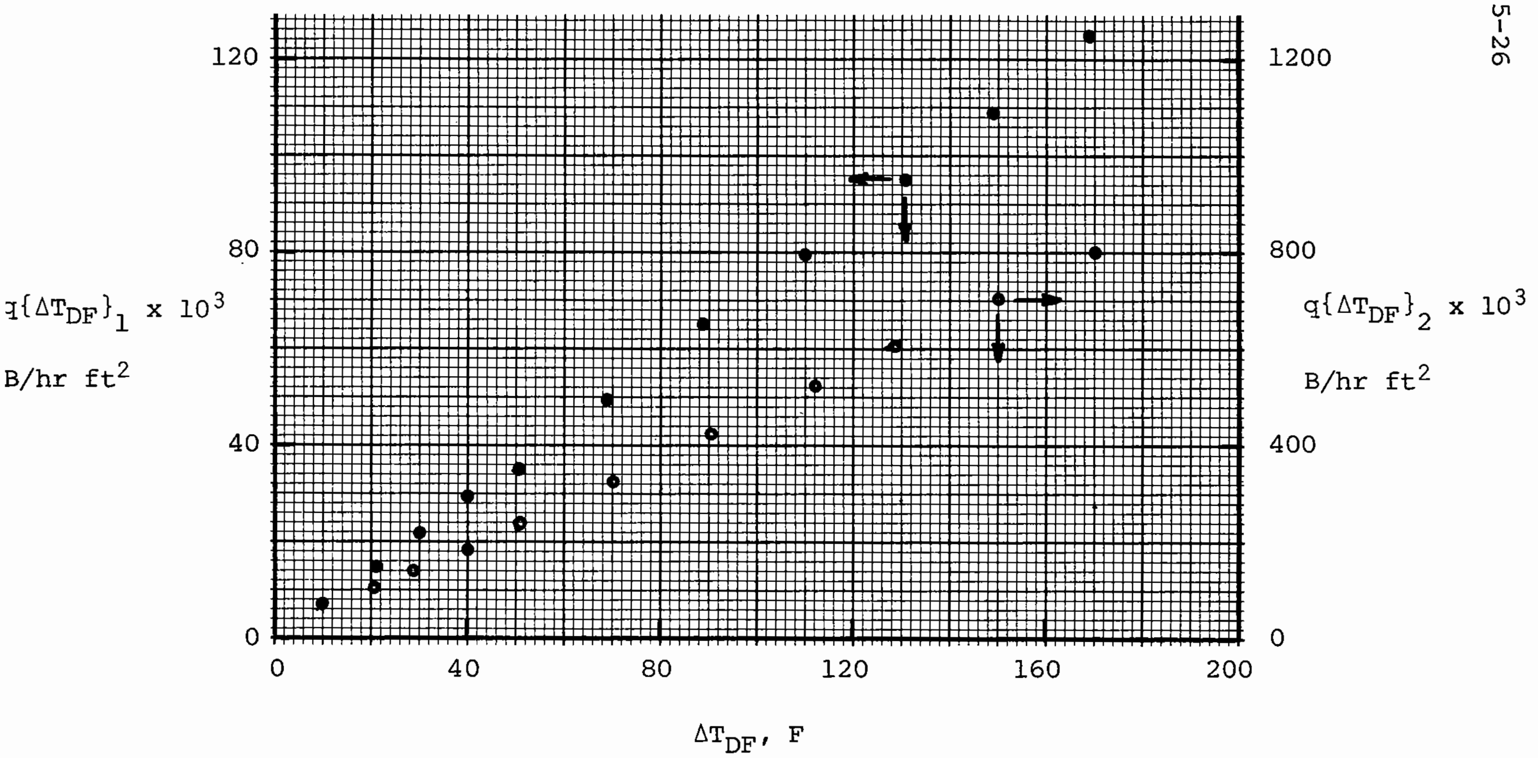


FIGURE 4 $q\{\Delta T_{DF}\}_1$ and $q\{\Delta T_{DF}\}_2$ data from Table 4

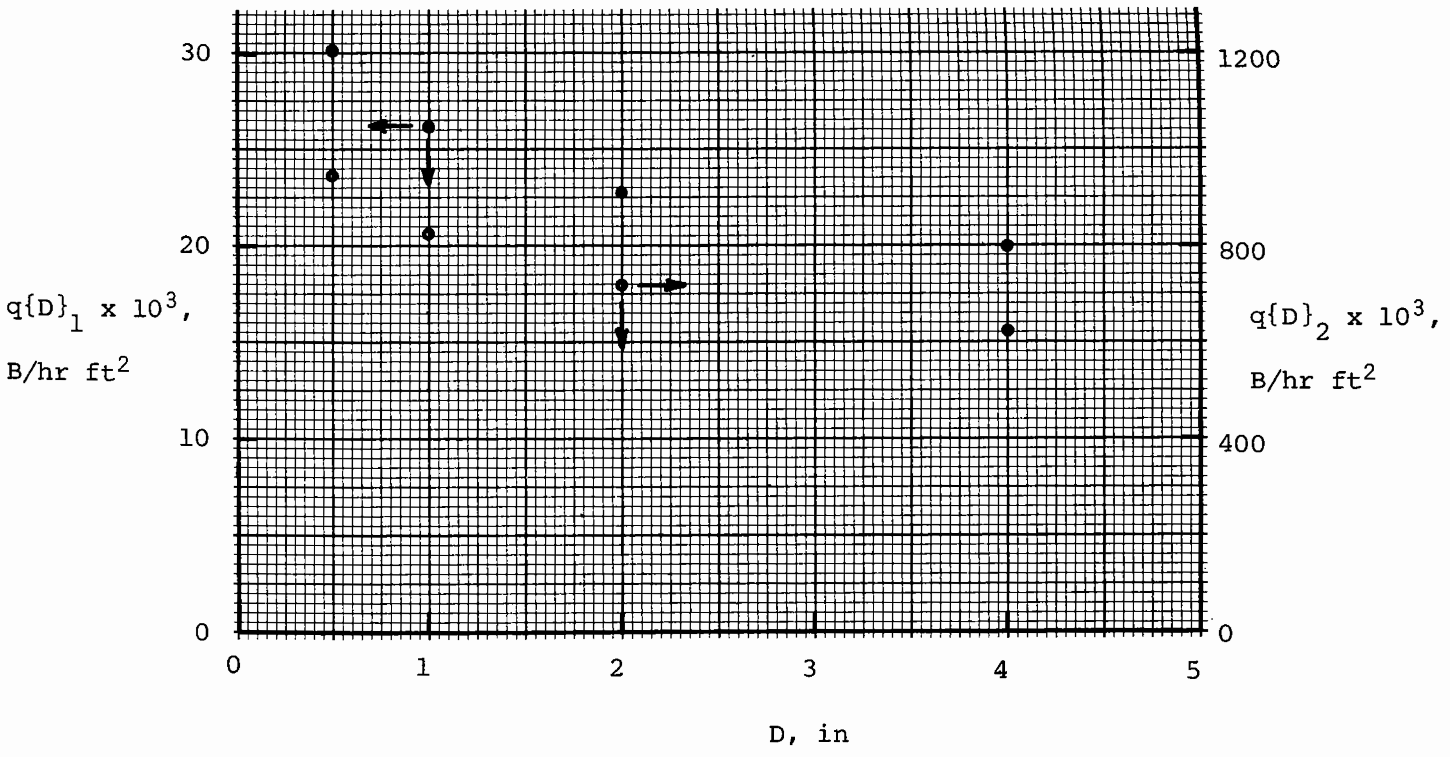


FIGURE 5 $q\{D\}_1$ and $q\{D\}_2$ data from Table 5

INDUCING THE q{G} FUNCTION

The first two columns in Table 1 present a tabular description of $q\{G\}_1$ and $q\{G\}_2$; Figure 1 presents a "picture" of these two functions. Given Figure 1 and Table 1, our task is twofold:

1. Use Fig 1 to "see" what simple analytical function is suggested for q{G}.
2. Use the Table 1 data in order to determine the "best" values of the constants in the function suggested by Fig 1.

Step 1 is normally accomplished by a combination of inspection/imagination/trial-and-error. Step 2 is accomplished either by hand or by computer.

The data in Fig 1 suggests that the curve passes through the point 0,0 and the curvature is roughly indicative of a power law. (Power laws MUST pass through 0,0 if the exponent is positive and through 0,∞ if the exponent is negative. In order to apply power laws to functions which have positive exponents but do not pass through 0,0, it is necessary to first transform the variables in order that the transformed function will pass through 0,0. For example, if the curve in Fig 1 passed through the point $G = .7 \times 10^6$, $q = 0$, we would have to transform G by writing

$$G_{trans} = G_{actual} - .7 \times 10^6 \qquad (29)$$

and then we could use the power law

$$q \rightarrow a\, G^n_{trans} \qquad (30)$$

assuming that the curve was indeed a displaced power law. It is obvious from Fig 1 that our function passes very near the point 0,0 and therefore that it will not be necessary to transform either parameter in order to use power laws.)

In the nucleate boiling example in Bk 1, Ch 7, the boiling theory and the boiling data BOTH indicated that the q function did NOT pass through 0,0. This should have suggested that, if indeed nucleate boiling were described by a power law, it would have to be a displaced power law--ie it would have to be of the form

$$q \propto \Delta T^n_{trans} \qquad (31)$$

$$\Delta T_{trans} = \Delta T_{actual} + \text{best fit correction} \qquad (32)$$

rather than the old heat transfer form

$$q \propto \Delta T^n_{actual} \qquad (33)$$

If the true function were a displaced power law--ie if the true function did not pass through 0,0--then there was no reason to expect the function to be a straight line on log log graph paper. The transformed function in eq 31 would be a straight line on log log paper, but NOT the function in eq 33. To my knowledge, no nucleate boiling investigation in the old heat transfer investigated the need to transform the variables before plotting the data on log log paper and drawing straight lines in order to measure the slope and thus determine the value of n.

By properly transforming the nucleate boiling data before plotting it on log log paper, it should be possible to determine the analytical functionality which agrees with the data--ie it should be possible to obtain the same correct answer whether one uses linear graph paper or log log graph paper. To test this, I some time ago transformed the nucleate boiling data reported by several researchers in the old heat transfer literature. I transformed the ΔT parameter in the precise manner described above and then plotted the transformed data on log log coordinates. The slope of the curve on log log coordinates indicated an exponent of essentially unity--precisely the SAME answer I had earlier obtained using linear graph paper. The only difference was that it was a great deal more difficult to obtain the correct answer using log log graph paper rather than linear graph paper.

(I attempted to publish the results of my nucleate boiling data analysis in the old heat transfer literature. Miraculously, the article was accepted for publication in an American Journal on the basis of three separate reviews--one highly favorable, one lukewarm, and one highly negative. However, it was a hollow victory. The article has not yet been published even though it was accepted for publication

ELEVEN YEARS AGO!!!

We will come back to that article in a later chapter.)

The failure to transform the data before plotting it on log log graph paper is the principal reason that the old heat transfer settled on a ΔT exponent of "3 or 4" when in fact the exponent suggested by the data is 1. And that is another advantage of using linear graph paper--we can forget about the need for transformations of any kind. Linear graph paper describes the function with no distortion--and that is exactly what we want. And as noted by Winston Churchill

> When you have got something where you want it, it is a good thing to leave it where it is.

Returning to the problem at hand, Fig 1 suggests that the curves pass through 0,0 and that a power law function might closely approximate the two curves. Therefore let us determine how well the function

$$q\{G\}_i \rightarrow a_i \, G^n \qquad (34)$$

can be made to fit the q{G} data in Table 1. (In actual practice, we would probably determine the best fit values of a_i and n using regression analysis and a computer. However, here we will simply look for a reasonable fit rather than the best fit.)

There are actually two experiments tabulated in Table 1 and we know from our discussion of relation 17 that the q{G} function is the same in both experiments with the single exception of the scale factor. With regard to eq 34, this means that if the true function is a power law, then the value of n indicated by the data should be the same for both experiments and the only indicated difference should be the scale factor a_i. This means that we must determine three constants from the data in Table 1: a_1, a_2, and n. An estimate of these three constants can be obtained quite simply by selecting three sets of G,q coordinates and solving for the three constants. For the sake of illustration, suppose we solve for the three constants in this manner and obtain the following values:

$$a_1 = 0.963$$

$$a_2 = 4.67$$

$$n_1 = n_2 = 0.8$$

In order to determine whether or not these constants together with relation 34 would closely resemble the real world behavior of q{G}, we perform the simple analysis indicated in Table 7, next page.

TABLE 7

COMPARING THE q{G} DATA IN TABLE 1 WITH A POWER LAW WHERE

$a_1 = .963,\ a_2 = 4.67,\ n_1 = n_2 = 0.8$

G_{actual} #/hr ft^2	q_{actual} B/hr ft^2	$q_{calc} = .963G^{.8}$ or $4.67G^{.8}$ B/hr ft^2
$.20 \times 10^6$	16.8×10^3	16.8×10^3
.40	29.6	29.2
.61	41.2	40.9
.90	55.7	55.8
1.21	71.2	70.8
1.62	89	89
1.99	107	105
2.53	125	128
3.02	148	147
3.48	168	165
3.97	183	183
.20	82	81
.40	143	142
.59	194	193
.88	273	266
1.21	344	343
1.59	425	427
2.01	515	515
2.53	607	619
2.99	701	708
3.51	810	805
3.98	890	890

Table 7 indicates a very close resemblance between the measured values of q{G} and the calculated values. We therefore conclude that f_1 in relation 17 is given by

$$f_1\{G\} = G^{.8} \tag{35}$$

and now we can turn our attention to another parameter.

(Again I would emphasize that all the conclusions in this chapter are based on IMAGINARY DATA and therefore that we are learning absolutely nothing about the real world behavior of anything. The sole purpose of this chapter is to illustrate the manner in which data are analyzed and correlated in the new heat flow. The purpose of the above q{G} example is to illustrate how to deal with power laws withOUT using log log graph paper. The use of log log graph paper has a natural tendency to promote the use of power laws whether or not they are suggested by the data. The use of linear graph paper has no similar prejudicial effect because linear graph paper presents the function without distortion in any direction.)

INDUCING THE q{P} FUNCTION

Figure 2 indicates that P has little if any effect on q. We therefore conclude that q is essentially independent of P and that f_2 in relation 17 is given by

$$f_2\{P\} = 1.0 \tag{36}$$

Now we can forget about P and turn our attention to the next parameter.

INDUCING THE q{T} FUNCTION

Inspection of Figure 3 indicates that the curvature is more or less constant--ie that

$$\frac{d^2q}{dT^2} \simeq \text{constant} \qquad (37)$$

and this in turn suggests that we try the function

$$q\{T\}_i \rightarrow a_i(b + cT + dT^2) \qquad (38)$$

We estimate the constants in relation 38 by selecting several q{T} coordinates from Table 3 and in this way suppose we estimate these constants to be

a_1 = 53,700

a_2 = 753,000

b = .2068

c = .002631

d = -2.218×10^{-6}

In order to determine whether or not these constants together with relation 38 would closely resemble the q{T} data in Table 3, we perform the simple analysis indicated in Table 8, next page.

(It would of course be very easy to start with an analytical function and then generate imaginary data which closely resembled the function. In the q{G} example, this is precisely what I did. However, I wanted to demonstrate that it is quite possible to start with data and induce an analytical function, since this is a much more difficult problem and since it is the problem facing us in the real world. In this q{T} example, I started with the data points and

TABLE 8

COMPARING THE q{T} DATA IN TABLE 3
WITH RELATION 38

T_{actual} F	q_{actual} B/hr ft^2	$q^*_{calc,\ rel\ 38}$ B/hr ft^2
102	23.5×10^3	24.3×10^3
151	29.8	29.7
202	34.3	34.8
248	39.8	38.8
301	42.5	42.8
350	46.2	46.0
398	48.4	48.5
452	50.6	50.6
501	52.1	52.0
99	336	335
151	413	417
198	490	483
251	545	548
302	607	602
348	639	643
401	683	682
450	710	709
480	725	722

*Based on the constants:

a_1 = 53,700

a_2 = 753,000

b = .2068

c = 2.631×10^{-3}

d = -2.218×10^{-6}

induced relation 38 and the above constants from the data points. This is not to say that the data points or relation 38 have any real world significance, but merely to point out that I have not taken the easy way in this q{T} example. The degree of difficulty in this q{T} example was the same for me as it would have been for the reader. What I am trying to say is that I have not "faked" this q{T} example, although of course I faked the q{G} and the q{ΔT_{DF}} examples by actually starting with the analytical functions rather than the data points. We will come back to this q{T} example in a later chapter.)

Table 8 indicates very close agreement between the measured values of q{T} and those calculated from relation 38 using the constants on page 5-34. We therefore conclude that f_3 in relation 17 is given by

$$f_3\{T\} = .2068 + .002631\ T - 2.218 \times 10^{-6}\ T^2 \qquad (39)$$

and that concludes our correlation of the T data.

INDUCING THE q{ΔT_{DF}} FUNCTION

Inspection of Fig 4 very quickly reveals a proportional relationship between q and ΔT_{DF} and therefore we conclude that the q function is given by

$$q\{\Delta T_{DF}\}_i \rightarrow a_i(\Delta T_{DF}) \qquad (40)$$

(This is the type of data correlation performed by Fourier who was concerned about the functionality between q and ΔT. He experimentally investigated this functionality and his data correlation repeatedly told him that there was an essentially proportional relationship between q and ΔT. Since this relationship was

generally proportional and since it obviously contained a proportionality constant, Fourier thought it reasonable and fitting that this proportionality constant should have a name and a symbol. It seemed fitting that this proportionality constant by given the name

heat transfer coefficient

and the symbol "h". In this way, the proportional behavior he observed in graphs similar to Fig 4 did not merely suggest a proportional function which was only sometimes applicable--it suggested a Law of Nature described by

$$q = h\ \Delta T \qquad (41)$$

And since Fourier viewed dimensional consistency as a fundamental requirement, it seemed self-evident to him and to his contemporaries that the dimensions of this thing called "h" were B/hr ft^2 F, since these are the dimensions required in order that "equation" 41 be a dimensional identity.

In the new heat flow, we recognize that there is NO POINT in assigning a name or a symbol or dimensions to the PURE CONSTANT in relation 40. In the new heat flow, we recognize that a constant is a constant is a constant. And that a constant by any other name is still nothing more or less than a constant.)

Again we estimate appropriate values for a_i by selecting several data points and solving for the constants. We obtain

$$a_1 = 730$$

$$a_2 = 4680$$

These constants with relation 40 are evaluated against the Table 4 data in Table 9, next page.

TABLE 9

COMPARING THE $q\{\Delta T_{DF}\}$ DATA IN TABLE 4 WITH RELATION 40

ΔT_{DF}, actual F	q_{actual} B/hr ft^2	$q^*_{calc, rel 40}$ B/hr ft^2
10	7.1×10^3	7.3×10^3
21	15.4	15.3
30	21.5	21.9
40	29.0	29.2
51	36.8	37.2
69	49.6	50.4
89	65.0	65.0
110	79.6	80.3
131	95	95.6
149	109	108.8
169	125	123.7
9	42	42.1
21	98	98
29	134	136
40	188	187
51	233	239
70	326	328
91	420	426
112	522	524
129	603	604
150	702	702
170	797	796

*Based on the constants:

$a_1 = 730$

$a_2 = 4680$

Table 9 indicates that relation 40 with the above constants closely resembles the $q\{\Delta T_{DF}\}$ data in Table 4. We therefore conclude that f_4 in relation 17 is given by

$$f_4\{\Delta T_{DF}\} = \Delta T_{DF} \qquad (42)$$

(In this example, we have "found" that heat flow to water flowing in pipes is essentially proportional. This of course does not give us the right to suppose that all forms of heat flow are proportional. Fourier recognized that some forms of heat flow were not precisely proportional, but the highly nonlinear forms of heat flow such as boiling were only beginning to become important in Fourier's time. To Fourier, it seemed that the important heat flow phenomena were essentially proportional and therefore that "h" could be quite accurately treated as a constant independent of ΔT. And that is why Fourier wrote

> . . . in the application to the natural problems which interest us most, we may assign to these coefficients (C, h, k) values sensibly constant.

I have not the slightest doubt that, if Fourier were brought back to life and were told that there are today many heat flow processes in which the relationship between q and ΔT is highly nonlinear, Fourier would respond with

> If that is so, then why have you retained the concept of the heat transfer coefficient which is only useful for proportional behavior?

And of course the answer is that, through long usage, the old heat transfer came to view the heat transfer coefficient NOT as a proportional behavior concept, but rather as a parameter of Nature which had its own fundamental character.

INDUCING THE q{D} FUNCTION

The first impression given by Fig 5 is that there are not enough data points--the points are too far apart to establish by inspection what the function "looks" like. The data points suggest a moderate degree of nonlinearity, but a straight line could be drawn which would come within about 5% of each data point. A power law with negative exponent would yield the same type curvature suggested by the data points. In short, we can not decide in a definitive way just what function is suggested by the data in Fig 5. In such cases, we would normally select the "best" function by evaluating all the possible functions against the data. In the case of Fig 5, we would certainly evaluate a linear relationship and a power law and whatever other simple functions we felt might closely resemble the data in Fig 5.

By the above type of reasoning, we evaluate several simple functions against the Table 5 data and the best fit is obtained from the power law

$$q\{D\}_i \rightarrow a_i D^n \qquad (43)$$

with the constants

$a_1 = 15{,}900$

$a_2 = 499{,}000$

$n = -.2$

Relation 43 is evaluated against the Table 5 data in Table 10, next page.

TABLE 10

COMPARING THE q{D} DATA IN TABLE 5 WITH A POWER LAW WHERE

$a_1 = 15{,}900, \quad a_2 = 499{,}000, \quad n_1 = n_2 = -.2$

D_{actual} ft	q_{actual} B/hr ft^2	q_{calc} B/hr ft^2 = $15900D^{-.2}$ or $499000D^{-.2}$
.0417	30.1×10^3	30.0×10^3
.0833	26.2	26.1
.1667	22.8	22.8
.3333	19.9	19.8
.0417	943	942
.0833	821	820
.1667	715	714
.3333	622	622

Table 10 indicates that relation 43 with the above constants closely resembles the Table 5 data and therefore that f_5 in relation 17 is given by

$$f_5 = D^{-.2} \qquad (44)$$

Equation 44 completes the induction phase of our data correlation.

Again I would emphasize that we are dealing with IMAGINARY DATA and that we are learning nothing about real world behavior. The reason we "found" that q{G} and q{D} were well described by power laws is only because the data were planned that way. The nature of these functions in the real world remains to be seen.

THE COMPLETE CORRELATION

Assuming and verifying relation 17 made it possible for us to simplify the 6-dimensional problem of determining $q\{G, P, T, \Delta T_{DF}, D\}$ by replacing it with five 2-dimensional problems involving the determination of $q\{G\}$, $q\{P\}$, $q\{T\}$, etc. The solution of these five 2-dimensional problems led to the functions described in equations 35, 36, 39, 42, and 44. Combining these equations with relation 17 gives

$$q \rightarrow a\, G^{.8}(b + cT + dT^2)(\Delta T_{DF})D^{-.2} \qquad (45)$$

where $b = .2068$, $c = .002631$, and $d = -2.218 \times 10^{-6}$. The problem now is to determine the "best" value of a in relation 45. This involves determining the value of a indicated by each data point and averaging all these "a" values in order to use all the information we have. For each data point, the indicated value of "a" is obtained from

$$a \rightarrow \frac{q_{meas}}{G^{.8}(b + cT + dT^2)(\Delta T_{DF})D^{-.2}} \qquad (46)$$

Using the dimensions lbs, hr, ft, F, B, the first few lines of Table 1 indicate the following values of "a":

Line #	a from relation 46
1	.0233
2	.0240
3	.0230

and therefore the complete correlation is

$$q \rightarrow G^{.8}\, D^{-.2}(e + fT + gT^2)\ \Delta T_{DF} \qquad (47)$$

where the dimensions are lbs, hr, ft, F, B and the values of the constants are $e = .00484$, $f = 6.16 \times 10^{-5}$, $g = -5.19 \times 10^{-8}$. (Note that we have combined a with b, c, d.)

Relation 47 is the principal result of our IMAGINARY investigation. It represents the successful accomplishment of the goal we set at the beginning of the investigation--the determination of a simple, easy to use analytical function which accurately describes the experimentally observed behavior of heat flow to liquid water flowing in pipes.

THE NEW HEAT FLOW REPLACEMENT FOR DIMENSIONAL ANALYSIS

Relation 47 is based on IMAGINARY DATA and therefore has no real world value. Relation 47 reveals nothing about anything. There is only one reason we have been working toward the formulation of relation 47 and that is to illustrate the research and development methods of the new heat flow. These methods include

1. careful experiment design
2. experiment performance in accordance with the design
3. explicit statement and experimental verification of all questionable assumptions
4. functionality established by induction from the data and NOT by intellectual speculation

We have applied these methods to a problem dealing with heat flow to water flowing in pipes. But our concern is not with the problem. The problem is only a vehicle. Our real concern is with the methods themselves.

The methods of the new heat flow are rigorous in that they rely on NO unverified assumptions, accurate in that they necessarily lead to correlations which closely resemble the data--closely resemble real world behavior. On the other hand, the methods of the old heat transfer are neither rigorous nor accurate. The methods of the old heat transfer are essentially the methods of dimensional analysis. And dimensional analysis generally involves a number of UNverified assumptions--dimensional analysis generally involves largely ignoring the data and this in turn often leads to correlations which little resemble the data--little resemble real world behavior.

These methods of the new heat flow require the expenditure of a great deal more effort than the methods of the old heat transfer. That is because it ALWAYS requires more effort to find out than to suppose. The methods of the new heat flow are based on finding out. The methods of the old heat transfer are based on supposing. Finding out is infinitely better than supposing. And THAT is why these methods of the new heat flow will largely replace the dimensional analysis of the old heat transfer.

CHAPTER 6 EXPERIMENTAL TECHNIQUE

INTRODUCTION

The Wright brothers are justly famous the world over for solving the puzzle of flight--for discovering those secrets of Nature which made possible the Age of Flight. In view of their success and fame, it is not surprising that science historians have extensively studied the methods and apparatus used by the Wright brothers. But the Wright brothers themselves proved to be a puzzle--neither of them graduated from high school--neither of them had money to spend on equipment!

In view of their limited scientific backgrounds and their poor facilities, wasn't it presumptuous of the Wright brothers to feel they could successfully compete with the greatest scientific establishments the world had ever known? What gave them the boundless confidence required to risk time, money, and life itself in the pursuit of a dream which had eluded man since Time began? How did the Wright brothers use their limited facilities to obtain experimental results so accurate they rivaled the results obtained in the best equipped laboratories in the world?

Einstein explains the presumption of the Wright brothers:

> No one who does not appreciate the terrific exertions, the devotion, without which pioneer creation in scientific thought cannot come into being can judge the strength of the feeling out of which alone such work . . . can grow.

The Wright brothers explain the basis of their confidence:

> We saw that the calculations upon which flying machines had been based were unreliable, and

> that all were simply groping in the dark. 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.

The Wright brothers' confidence was based not on faith but on understanding--not on theory but on an intimate knowledge of Nature's behavior--not on hearsay but on the first hand knowledge which can come from only one source--experiment. Experiment was the source of the Wright brothers' confidence--they had listened carefully to Nature's testimony and they knew that the calculations and correlations being used by others to design and build flying machines were not in accord with Nature's testimony. They knew that their own calculations and correlations were infinitely better suited for success because they accorded with Nature's testimony. And that is why the Wright brothers were confident of their ultimate success in spite of what others might regard as their "limited" scientific backgrounds and their "poor" facilities.

But there is another part of the Wright brothers' puzzle which interests us here, and that is their experimental technique. How did they utilize their limited facilities--their crude instruments--to obtain experimental results of surprising accuracy? What special knowledge/attitude/temperament did the Wright brothers possess which enabled them to obtain accurate measurements from highly inaccurate instruments? In this chapter, we attempt to answer this question in a general rather than a specific way. We consider what is generally required for good experimental technique and then we ask whether it is reasonable to suppose that the Wright brothers possessed these requirements.

Most of our discussion centers around two ingredients essential to good experimental technique:

> An understanding of instrument fundamentals.
>
> A willingness to undertake the extra effort which makes the difference between a mediocre experiment and one which is outstanding.

The objective of our discussion is the realization that accurate results are no accident--they are the product of no magic--they require no special intuition. Accurate results are within the reach of every experimenter who understands instrument behavior and who sets for himself the following objective:

> To obtain the most accurate and reliable results possible with the given apparatus.

As we shall see in this chapter, the fulfillment of this objective requires little more than an understanding of instrument behavior and the expenditure of a moderate amount of extra effort.

ACCURACY AND PRECISION

It is not possible to discuss experimental technique in a meaningful way unless the reader has a firm grasp of the meaning of the words "accuracy" and "precision":

> ACCURACY refers to the agreement between measured value and true value. For example, suppose we obtain the 1 lb. standard from the National Bureau of Standards and we weigh it on our scale. Further suppose that our measured value is 1.030 lbs. In this case, the inaccuracy in the measured value is 0.030 lbs.
>
> PRECISION refers to the agreement between repeat measurements of the same true value. Suppose we reweigh the 1 lb. standard on the same scale

> and this time the scale indicates 1.032 lbs. instead of 1.030 lbs. In this case, the imprecision is 0.002 lbs.

Everyone who really understands the meaning of the words accuracy and precision will agree with the following statement:

> EVERY instrument is more precise than it is accurate. EVERY measurement is more precise than it is accurate.

The proof that this statement is true requires merely the recognition that inaccuracy includes imprecision and that the whole of anything is larger than any single part. This statement is particularly important because it contains the basis for achieving the best possible experimental technique.

A CONCISE GUIDE TO GOOD EXPERIMENTAL TECHNIQUE

One reason we have stressed the meaning of accuracy and precision is because there is a concise guide which leads to good experimental technique and it is impossible to understand this guide unless one understands accuracy and precision. Simply stated, the guide is:

> Good experimental technique and accurate results require that experiments be designed and data analyzed in such a way that the ACCURACY of the results depends ONLY on the PRECISION of the instruments. In other words, our objective is to make the accuracy of the result IN NO WAY depend on the accuracy of the instruments. This can usually be accomplished by designing the experiment and analyzing the data in such a way that the precision of the instruments uniquely determines the accuracy of the results.

The difference between good experimental technique and

poor experimental technique--between the effect of instrument imprecision and the effect of instrument inaccuracy--is usually about a factor of ten difference in the accuracy of the results.

INSTRUMENT FUNDAMENTALS

Most common instruments are essentially linear--ie they respond to variations in the true value of the measured parameter in an essentially linear manner. (The most notable exception is flow rate instrumentation which often has a square root characteristic.) Therefore, let us restrict our discussion to linear and proportional instruments. For ease of discussion, let us consider that we are dealing with instruments which respond with the linear deflection of a needle or a dial.

A proportional instrument exhibits straight line response which passes through zero--ie it behaves in such a way that the relationship between deflection and true value is given by

$$\text{Deflection} \propto \text{True Value} \qquad (1)$$

The proportionality constant between deflection and true value is often called the GAIN and so we rewrite eq 1 in the form

$$\text{Deflection} = \text{Gain} \times \text{True Value} \qquad (2)$$

A linear instrument exhibits straight line response which does not necessarily pass through zero--ie it behaves in such a way that

$$\text{Deflection} = \text{Gain} \times \text{TV} + \text{constant} \qquad (3)$$

The constant in eq 3 is often called the "zero error" and so we rewrite eq 3 in the form

$$\text{Deflection} = \text{Gain} \times \text{TV} + \text{Zero Error} \qquad (4)$$

It should be noted that the gain in eqs 3 and 4 is not the proportionality constant between deflection and true value as it was in eq 2, but rather is given by

$$\text{Gain} = \frac{d(\text{deflection})}{d(\text{true value})} \qquad (5)$$

In order to discuss eqs 2 and 4 in the most meaningful way and to illustrate that results can be obtained which in no way depend on the accuracy of the instruments, let us turn our attention to a linear instrument which everyone has used--the bathroom scale.

BATHROOM SCALES

Everyone has weighed himself on a bathroom scale. Bathroom scales are linear instruments--ie their response is essentially straight line and they often indicate a finite weight even with nothing on the scale--ie they are linear because their gain is constant and their zero error is finite. Their dials are calibrated to read directly in pounds and so we rewrite eq 4 as

$$W_{ind} = G\, W_{true} + E_o \qquad (6)$$

where the gain (G) in eq 6 is given by

$$G = \frac{d(\text{indicated weight})}{d(\text{true weight})} \qquad (7)$$

The manufacturer's design intent is of course to have G in eqs 6 and 7 equal unity.

Now let us rearrange eq 6 in order to get it in a more useful form:

$$W_{true} = (W_{ind} - E_o)/G \qquad (8)$$

When we use the bathroom scale, we do not obtain the precise values of W_{ind}, G, or E_o--we only obtain estimates of these values and therefore it is not possible to determine the precise value of W_{true}. However, eq 8 is still useful because it states that the best estimate of W_{true} is obtained from

$$W_{best\ est} = (W_{ind} - E_o)/G \qquad (9)$$

Eq 9 is the form we will generally use to reduce the data. It states that there are three sources of error in our best estimate of W_{true}:

> The error caused by our inability to read precisely the value indicated by the scale.
>
> The error caused by the fact that we do not know precisely the value of the gain.
>
> The error caused by the fact that we do not know precisely the value of the zero error.

The aim of good experimental technique is to drive down the effect of each of these errors on our best estimate of W_{true}.

Now let us weigh someone on the scale, first using poor experimental technique and then repeating the experiment using better experimental technique. To simplify the discussion, let us ignore the error in determining W_{ind}--ie let us pretend that we can exactly determine the value indicated by the scale. (Later we will come back to the error in determining W_{ind}.)

POOR TECHNIQUE

1. Step on scale. Read and record. 194 lbs
2. Assume E_o is zero, gain is unity.
3. Calculate $W_{best\ est}$ from eq 9:

$$W_{best\ est} = (194 - 0)/1 = 194 \text{ lbs}$$

BETTER TECHNIQUE

1. With nothing on scale, read and record. 8 lbs
2. Step on scale. Read and record. 194 lbs
3. Assume gain is unity.
4. Calculate $W_{best\ est}$ from eq 9, noting that E_o from step 1 is 8 lbs:

$$W_{best\ est} = (194 - 8)/1 = 186 \text{ lbs}$$

GOOD TECHNIQUE

1. With nothing on scale, read and record. 8 lbs

2. Measure gain by weighing known weight. (For illustration, pretend known weight is exactly 177 lbs.) Place known weight on scale.

   Read and record. 189 lbs
3. Step on scale. Read and record. 194 lbs
4. Calculate gain using data from steps 1 and 2:

$$\text{Gain} = \frac{\Delta \text{ indicated weight}}{\Delta \text{ true weight}}$$

$$\text{Gain} = \frac{(189 - 8)}{177} = 1.0226$$

5. Calculate $W_{best\ est}$ from eq 9 using E_o from step 1 and Gain from step 4:

$$W_{best\ est} = (194 - 8)/1.0226 = 182 \text{ lbs}$$

In the above example, the use of good experimental technique improved the accuracy of the result by 12 lbs. Good experimental technique made it possible to obtain a quite accurate result from a very inaccurate instrument. It is important to note that even our best result--182 lbs--is not the true value of the person's weight. Even our best result is affected by the following sources of error:

The imprecision in W_{ind}

The imprecision in E_o

The imprecision in Gain

But these are things we can do nothing about. We can do nothing about the fact that W_{ind} will not exactly repeat each time--E_o will not exactly repeat each time--Gain will not exactly repeat each time. We can do nothing about the repeatability--the precision--inherent in the instrument. But it is very important to note that we have satisfied our objective which was to obtain a result which IN NO WAY depended on the accuracy of the instrument--a result whose accuracy was affected ONLY by the imprecision in the instrument--and that is precisely what we have obtained. The only sources of error in the 182 lb result are due to imprecisions and are NOT due to inaccuracies in the instrument. And thus we have satisfied the objective of good experimental technique which is to obtain the most accurate and reliable result possible with the given apparatus.

This discussion centering around bathroom scales is intended to illustrate the basic requirements of good experimental technique--an understanding of instrument behavior and a willingness to undertake the moderate amount of extra effort which makes the difference between good and bad experimental technique. It also illustrates how simple it is to obtain accurate results from inaccurate instruments and why good experimental technique is such a valuable asset in research and development work.

TECHNIQUE GUIDELINES

Now let us turn away from bathroom scales and discuss equipment and instrumentation in a general way. There are no widely accepted guidelines for good experimental technique, but I have found the following to be quite useful:

1. If at all possible, calibrate instruments in place--ie calibrate them in their normal position in the apparatus and do not disturb them between the time of calibration and the performance of the experiment. Do not remove the instruments and send them to a calibration laboratory unless absolutely necessary. (It is seldom necessary.)

2. If an instrument has recently been calibrated in a calibration laboratory, the gain results will usually be acceptable. However, do not rely on the zero error measured in the calibration. Zero errors are subject to relatively fast changes. (Recall that the zero error on bathroom scales varies considerably from day to day.) ALWAYS try to determine the zero error as part of the experiment.

3. The effect of zero errors on the experimental results can usually be eliminated by calibrating the instruments against each other whenever the desired result is obtained by subtracting one measurement from another.

4. In place calibration should be part of the experimental procedure--ie there should be little difference in time between the calibration and the performance of the experiment.

5. The data reduction must be designed to take advantage of the calibration results.

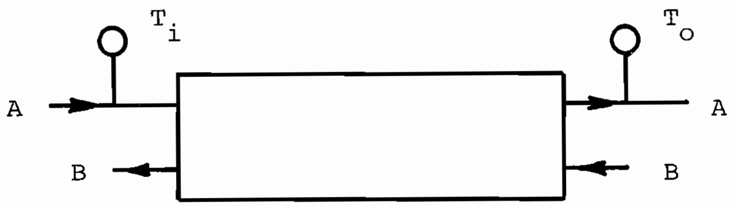
6. All the various methods which might be used to obtain the desired measurement should be considered before the method(s) is selected. Oftentimes an indirect method will give a more accurate result than a direct method.

As a practical matter, it will oftentimes not be possible to make an in place calibration of instrument gain. In such cases, it is useful to have one instrument calibrated in a laboratory and this instrument is then used as the standard for calibrating other similar instruments in the facility. For example, we may be dealing with a facility which has 24 thermocouples. If we had one of them calibrated, we could use it as the gain standard for calibrating the gain of the other 23 thermocouples in the facility. Or we might note that all 24 thermocouples read within a few degrees of each other over a range of several hundred degrees and we could reasonably conclude from this that the gain errors are only a per cent or so. However, even in this case, we would calibrate the zero errors because a few degrees in zero error can be very important. For instance, if we are measuring a temperature rise of 15F, an error of only 3F would be 20%.

Rather than discuss the application of these guidelines in an abstract way, let us apply them to situations which might be encountered in performing real experiments.

EXPERIMENT 1

Fluid A is being heated in the steady-state system shown in Sketch 1. Design an experiment to measure the steady-state temperature rise in Fluid A. First use poor experimental technique and then repeat using good experimental technique.



Sketch 1

T_i and T_o are locations where removable thermometers can be temporarily installed.

EXPERIMENT 1 POOR TECHNIQUE

1. Obtain two thermometers. Recent calibration showed that the gain errors were less than 1%.
2. Install the thermometers at T_i and T_o.
3. Read and record. T_i 70 F

   T_o 95 F
4. Conclude that temperature rise of Fluid A is given by

$$\Delta T_A = 95 - 70 = 25F$$

EXPERIMENT 1 GOOD TECHNIQUE

1. Obtain two thermometers, T_1 and T_2. Recent calibration showed that the gain errors were less than 1%.
2. Install T_1 at T_i and T_2 at T_o.
3. Read and record. $T_{1,i}$ 70 F

   $T_{2,o}$ 95 F
4. Reverse thermometer locations.
5. Read and record. $T_{1,o}$ 92 F

   $T_{2,i}$ 73 F
6. Calculate temperature rise of Fluid A from

$$\Delta T_{A,1} = T_{1,o} - T_{1,i} = 92 - 70 = 22F$$

$$\Delta T_{A,2} = T_{2,o} - T_{2,i} = 95 - 73 = 22F$$

EXPERIMENT 1 DISCUSSION

This first experiment illustrates one very effective way of eliminating the effect of zero errors. In the good technique part of experiment 1, we used each thermometer as though the other did not exist. The advantage in doing this is that the zero error cancels and thus we obtain a result which is independent of the zero error in the instrument. This can be seen by noting that, if we neglect gain error, we may write

$$T_{ind} = T_{true} + E_o \tag{10}$$

and therefore ΔT based on the use of a single thermometer is given by

$$\Delta T = (T_{o,ind} - T_{i,ind}) \tag{11}$$

$$\Delta T = (T_{o,true} + E_o) - (T_{i,true} + E_o) \tag{12}$$

The important thing to note is that E_o cancels in eq 12 ONLY if we are using a single thermometer. If we are using two different thermometers, E_o will not cancel because there is no reason to assume that the E_o for one thermometer will equal the E_o for the other thermometer.

In the poor technique part of experiment 1, the zero error resulted in an error of 14% in the measured value of ΔT_A. In the good technique part, the zero error had no effect on the measured value of ΔT_A. By the expenditure of a small amount of extra effort, we have vastly improved the accuracy of the measured result.

Before leaving experiment 1, it should be noted that this method of eliminating the effect of zero errors has one major disadvantage. It requires that both inlet and outlet condition be measured by the same instrument whereas oftentimes this is not practical.

EXPERIMENT 2

Fluid A is being heated in the steady-state system shown in Sketch 1 (pg 6-12). Design an experiment to measure the steady-state temperature rise in Fluid A. First use poor experimental technique and then repeat using good experimental technique.

Note: T_i and T_o refer to the location of permanently installed thermocouples. The facility would have to be shut down and drained in order to remove the thermocouples.

Note: The T_i thermocouple was calibrated a short time ago. The calibration indicated that the gain was accurate to within 0.5%. The T_o thermocouple has never been calibrated. Both thermocouples are read out on the same millivoltmeter to the nearest tenth of a degree.

EXPERIMENT 2 POOR TECHNIQUE

1. Read and record. T_i 76.3 F

   T_o 109.5 F

2. Conclude that temperature rise of Fluid A is given by

$$T_A = 109.5 - 76.3 = 33.2 \text{ F}$$

EXPERIMENT 2 GOOD TECHNIQUE

1. Shut off flow of Fluid B; increase flow of Fluid A to maximum. When temperatures level off, read and record.

2. With Fluid B flow shut off and with Fluid A flow at maximum, raise temperature of Fluid A. When temperatures level off, read and record.

3. Return equipment to normal lineup. When temperatures level off, read and record.

4. Data:

	T_i	T_o
Step 1	73.7	68.1
Step 2	113.2	108.9
Step 3	76.3	109.5

5. Data analysis:

$$\frac{\text{Gain } T_o}{\text{Gain } T_i} = \frac{(108.9 - 68.1)}{(113.2 - 73.7)} = 1.0329$$

$$T_{o,\text{best est}} = 73.7 + \frac{(109.5 - 68.1)}{1.0329} = 113.8 \text{ F}$$

$$\Delta T_{A,\text{ best est}} = 113.8 - 76.3 = 37.5 \text{ F}$$

EXPERIMENT 2 DISCUSSION

This experiment illustrates the calibration of one instrument against another in order to evaluate both the zero error and the gain error. In this experiment, the T_i thermocouple was treated as the standard because it had recently been calibrated, whereas the T_o thermocouple had never been calibrated.

Note that we did not rely on the zero error which resulted from the instrument lab calibration. Instead, we eliminated the effect of the zero error by calibrating the T_o thermocouple using the T_i thermocouple as the reference standard. That was the sole purpose of steps 1 and 2. In step 1, we stopped the Fluid B flow and increased the Fluid A flow in order to make the Fluid A system as isothermal as possible. This enabled us to conclude that any indicated temperature difference between T_i and T_o was the result of zero error and not the result of a difference in the true temperature at these locations. In step 2, we again made the Fluid A system as isothermal as possible, but this time at a different temperature level in order to obtain the information required to determine the gain of T_o relative to the gain of T_i. (It should be noted that the true temperatures at T_i and T_o were not exactly equal in steps 1 and 2 because there would have been some heat flow to or from the ambient. We could either correct for the effect of this heat flow on T_o or demonstrate by analysis that it had little effect on the value of T_o.)

Together, steps 1 and 2 provide all the information necessary to calibrate T_o using T_i as the reference standard. The principal result of steps 1 and 2 is the equation

$$T_{o,\text{best est}} = 73.7 + (T_{o,\text{ind}} - 68.1)\frac{(113.2 - 73.7)}{(108.9 - 68.1)}$$

and this is the equation we used in the data analysis in order to obtain the best possible estimate of the true temperature at the T_o location.

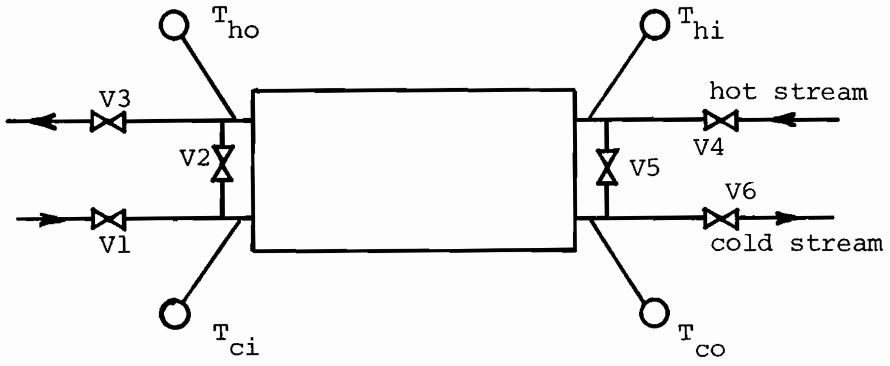
Using poor technique, the error in T_o relative to T_i resulted in about 12% error in the measured value of ΔT_A. Using good technique, the error in T_o relative to T_i had no effect on the measured value of T_A.

By the expenditure of a small amount of extra effort, we have vastly improved the accuracy of the measured result.

EXPERIMENT 3

A water/water heat exchanger is shown in Sketch 2. Design an experiment to measure the temperature change in both fluid streams and also the thermal driving force at each end of the exchanger. (The latter is required in order to determine ΔT_{LM} from the experimental results.) First use poor experimental technique and then repeat using good experimental technique.

Note: T_{hi} was recently calibrated and its gain was accurate to within 0.5%. The other thermocouples in Sketch 2 have never been calibrated.



Sketch 2

Note: Both streams discharge to vented tanks.

EXPERIMENT 3 POOR TECHNIQUE

1. With system operating in normal mode, read and record.

2. Data:

T_{hi}	T_{ho}	T_{ci}	T_{co}
187	142	68	132

3. Analyze data:

$$\Delta T_{\text{hot stream}} = 142 - 187 = -45 \text{ F}$$

$$\Delta T_{\text{cold stream}} = 132 - 68 = 64 \text{ F}$$

$$\Delta T_{DF,1} = T_{hi} - T_{co} = 187 - 132 = 55 \text{ F}$$

$$\Delta T_{DF,2} = T_{ho} - T_{ci} = 142 - 68 = 74 \text{ F}$$

EXPERIMENT 3 GOOD TECHNIQUE

1. Shut V2, V6, and V4. Open V1, V5, and V3. Read and record.

2. Shut V5, V3, V1. Open V6, V4, V2. Read and record.

3. Return system to normal operating mode. Read and record.

4. Data:

	T_{hi}	T_{ho}	T_{ci}	T_{co}
Step 1	66	74	68	71
Step 2	187	188	184	190
Step 3	187	142	68	132

5. Analyze calibration data, steps 1 and 2:

$$T_{ho,best\ est} = 66 + (T_{ho,ind} - 74)\frac{(187 - 66)}{(188 - 74)}$$

$$T_{ci,best\ est} = 66 + (T_{ci,ind} - 68)\frac{(187 - 66)}{(184 - 68)}$$

$$T_{co,best\ est} = 66 + (T_{co,ind} - 71)\frac{(187 - 66)}{(190 - 71)}$$

6. Using calibration results in step 5, determine best estimate values from the data obtained in step 3, normal lineup:

$T_{ho,best\ est,\ step\ 3} = 138$ F

$T_{hi,best\ est,\ step\ 3} = 187$ F

$T_{co,\ best\ est,step\ 3} = 128$ F

$T_{ci,best\ est,\ step\ 3} = 66$ F

7. What are the best estimates of the temperature change in each fluid stream and the thermal driving force at each end of the exchanger during normal operation?

$\Delta T_{hot\ stream} = 138 - 187 = -49$ F

$\Delta T_{cold\ stream} = 128 - 66 = 62$ F

$\Delta T_{DF,1} = T_{hi} - T_{co} = 187 - 128 = 49$ F

$\Delta T_{DF,2} = T_{ho} - T_{ci} = 138 - 66 = 72$ F

EXPERIMENT 3 DISCUSSION

The purpose of this experiment is to illustrate the in place calibration of a number of instruments against one instrument selected as the reference standard. This experiment has dealt with thermocouples, but it should be obvious that the same technique applies also to other types of instruments.

The ho, ci, and co thermocouples were calibrated against the hi thermocouple and significant differences were observed in gain values and zero errors. The differences in gain values are illustrated by the fact that an increase of 121 F in T_{hi} resulted in increases of 114, 116, and 119 F in T_{ho}, T_{ci}, and T_{co} respectively. As a practical matter, the gain values of thermocouples are usually quite accurate and we would be surprised to find differences this large. Normally, the gain values are in good agreement and no gain correction is required. This is not to say that one should merely assume that the gain values are accurate. "Supposing is good, but finding out is better."

As in Experiment 2, steps 1 and 2 are the calibration steps. In these steps, we attempt to make the system essentially isothermal so that we can attribute indicated differences in temperature to instrument error. The "best estimate" equations in step 5 are obtained in the same manner as in Experiment 2. In step 6, we substitute the data from step 3 into the step 5 equations and so obtain our best estimate of the true temperature profile in the equipment during step 3, the normal operating mode. In step 7, we use this best estimate temperature profile in order to answer the original question.

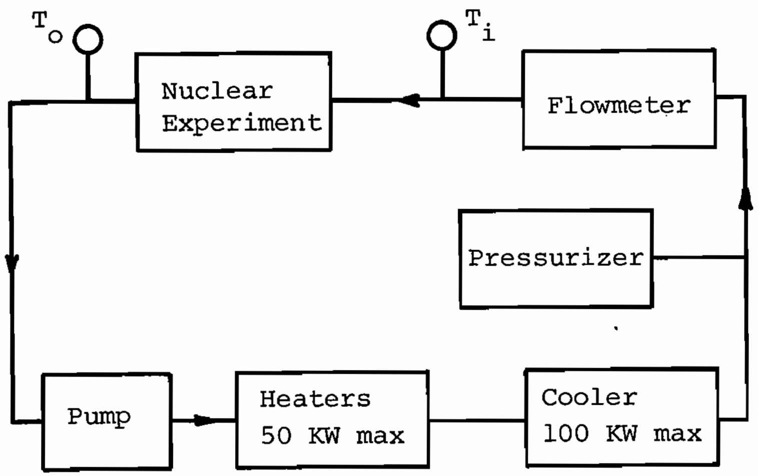
It is important to understand the reason we have gone to the trouble of changing the normal valve lineup in steps 1 and 2. We have done this because our original objective was to measure not only the temperature change in each fluid, but also to measure the driving force temperature difference at each end of the heat exchanger. This requires that we calibrate the thermocouples in one stream against the thermocouples in the other stream. Note that if we had used the Experiment 2 procedure in each stream, we could have accurately determined the temperature change in each stream, but NOT the thermal driving force at the ends of the exchanger. In order to accomplish all our objectives, it was necessary to make both streams isothermal at the same temperature and that is why Experiment 3 differs from Experiment 2.

Before leaving Experiment 3, there is a legitimate question which should be answered and that is "What would we have done if the apparatus had not contained the V2 and V5 bypass legs?" The answer is very simple. We would have had them installed because that is what the best possible answer requires.

EXPERIMENT 4

Sketch 3 describes an experimental facility used to irradiate nuclear fuel element designs being considered for future application. The facility is a closed loop system and contains a region which is routed through a high neutron flux zone of a test reactor. At the next scheduled reactor shutdown, a new set of experimental fuel elements will be placed in the high neutron flux region and they will generate an unknown amount of heat. We would like to measure the heat generated by the fuel in order to accurately determine the irradiation time required to attain 50% fuel burnup. Analysis indicates that the heat generated will be more than 10 KW but less than 30 KW.

Design an experiment to measure the heat generated by the fuel elements. First use poor experimental technique and then repeat using good experimental technique.



Sketch 3

Normal Operating Mode:

Pressure: 2000 psia

Temperature: 550 F

Flow: 200 gpm

Temperature control: heaters set to maintain temperature at 550F; cooler set to maintain temperature at 500F with heaters at max setting and reactor shut down

Note:

T_i and T_o read out to the nearest degree F.

The flowmeter has never been calibrated.

EXPERIMENT 4 POOR TECHNIQUE

With really poor technique, we would install the fuel in the facility and then measure the flow, T_i and T_o, and attempt to calculate the heat generated from the equation

$$Q = W C_p (T_o - T_i) \tag{13}$$

With somewhat better technique, we would make a preliminary estimate of the temperature rise $(T_o - T_i)$ and would note that even if the fuel generates the maximum expected value of 30 KW, the temperature rise would be only about 1 F. Since T_i and T_o read out to the nearest degree, there is no calibration which would allow us to accurately measure a temperature rise of about 1 F or less.

The result of our poor technique would then be one of two alternatives: we could conclude that it cannot be done with the available instruments and that an accurate ΔT instrument must be obtained which has an accuracy of less than one tenth of one degree F; we could conclude that making a heat balance across the fuel elements is not a very good way of measuring the heat generated by the fuel elements and that we would do well to look for some other, less direct way of measuring the heat generated.

At this point, I would encourage the reader to go back to page 2-23 and study the facility in order to design his own experiment intended to result in an accurate measurement of the heat generated by the nuclear fuel. By way of a hint, the facility contains everything that is required in order to accurately measure the heat generated. The right calibration is all that is required.

EXPERIMENT 4 GOOD TECHNIQUE

With good technique, we conclude that the 1 F temperature rise across the fuel elements is too small to be the basis of an accurate measurement of the heat generated in the fuel. We therefore look for some other, less obvious way to measure the heat generated.

The heat generated by the fuel does two things: it causes T_o to be higher than T_i; it heats up the facility, causing T_o and T_i to be higher than T_{amb}. We could use this latter effect to measure the heat generated by noting that

$$Q_{loss} = \text{function of } (T_o - T_{amb}) \tag{14}$$

and performing an experiment to measure the function in this equation. Or we might note that T_{amb} is always maintained at about 70 F, in which case we can rewrite eq 14 in the form

$$Q_{loss} = \text{function of } T_o \tag{15}$$

The main advantage of eqs 14 and 15 over eq 13 is that the effect of Q on T_o (or equally T_i) is many times greater than the effect on $(T_o - T_i)$. This greater effect means that we could obtain a quite accurate measurement of Q even though we lacked a highly accurate measurement of T_o. A lesser advantage of eqs 14 and 15 is that they do not contain W and therefore they obviate the need to obtain a measurement of the flow rate.

The facility information on page 6-23 tells us that, in the normal operating mode, the system is adjusted so that about 50 KW is required from the two heat sources (the auxiliary heaters and the fuel elements) in order to maintain the system temperature at 550 F. Assuming that the system temperature would be about 100 F if there were no heat input from these two sources, the average increase in system temperature per KW heat input is given by

$$\frac{dT_{system}}{dQ_{input}} \simeq \frac{(550 - 100)}{50} = 9 \text{ F/KW} \qquad (16)$$

Compare this value with the effect of Q on $(T_o - T_i)$ which we noted earlier was 1 F increase per 30 KW heat input or equally

$$\frac{d(T_o - T_i)}{dQ_{fuel}} = \frac{1}{30} = 0.033 \text{ F/KW} \qquad (17)$$

Eqs 16 and 17 tell us that Q has 270 times greater effect on T_o than on $(T_o - T_i)$. They also tell us that the accuracy of the T_o measurement is 270 times less important in the method of eq 15 than in the method of eq 13. For example, a T_o error of 1 F results in an error of 1/9 = .11 KW in $Q_{fuel,meas}$ using eq 15. On the other hand, this same T_o error results in an error of 1/.033 = 30 KW in $Q_{fuel,meas}$ using the method of eq 13. Eq 15 has the additional advantage that errors in W and T_i do not contribute to the error in $Q_{fuel,meas}$ whereas they are an important source of error in the method of eq 13.

The method of eq 15 requires that we calibrate the heat loss characteristics of the system. We must determine how T_o responds to Q and this is readily accomplished by a procedure such as

the following:

1. Adjust cooler for normal operating mode: a) with reactor shut down, place auxiliary heaters in manual mode; b) set heaters at maximum setting, 50 KW; c) adjust blower and dampers on cooler so that T_o levels off at about 550 F. Read and record.

   Q __50__ KW

   T_o __536__ F

2. Set auxiliary heaters at 40 KW. When T_o levels off, read and record. Repeat at 30, 20, 10, and 0 KW.

Q, KW	40	30	20	10	0
T_o, F	465	399	334	267	194

   (Note that even with 0 heat input from the heaters, T_o is considerably above T_{amb} due to the heat input from the pump. Thus the derivative in eq 16 is not correct and a more accurate value is 7 F/KW.)

3. Plot T_o vs $Q_{heaters}$. See Fig 1, next page.

Figure 1 is the calibration curve we require in order to accurately measure the heat generated in the fuel. In order to measure the amount of heat generated in the fuel, we have only to secure the auxiliary heaters, observe the value of T_o at steady-state, and then use Fig 1 to determine the value of Q that corresponds to the measured value of T_o.

It should be mentioned that some heat will be generated in the test section housing even though there are no fuel elements installed. This heat will result whenever the reactor is on line and is usually referred to as "gamma heating". In order to determine what heat is generated in the fuel elements alone, it is necessary to determine the amount of gamma heating so that it may be subtracted from the total heat generated. The amount of gamma heating is determined by securing the auxiliary heaters with the reactor on line and with no fuel elements installed in the facility. The amount of heat generated in the fuel alone is then determined from

$$Q_{\text{fuel}} = Q_{\text{meas w fuel}} - Q_{\text{meas w/o fuel}} \qquad (18)$$

where the values on the right hand side are obtained from Fig 1.

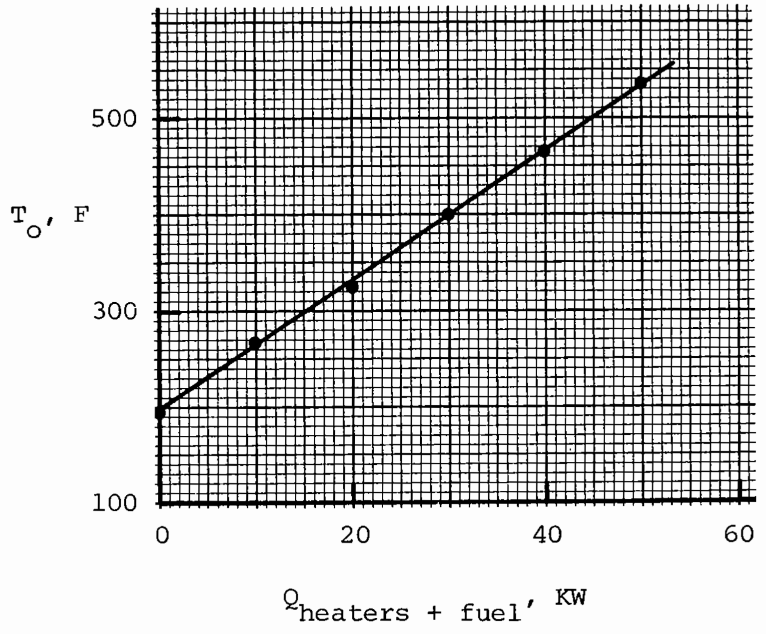


FIGURE 1

The following example illustrates the use of Fig 1:

a) With the reactor on line and with no fuel in the facility, T_o leveled off at 257 F.

b) With the reactor on line and with the new fuel in place in the facility, T_o leveled off at 428 F.

c) From Fig 1, $Q\{T_o = 257\} = 9$ KW and $Q\{T_o = 428\} = 34$ KW.

d) From eq 18,

$$Q_{\text{fuel}} = 34 - 9 = 25 \text{ KW}$$

The above method of measuring Q_{fuel} is quite satisfactory, but there is another, even simpler method of measuring Q_{fuel}. This alternate method requires that we note that, in the normal operating mode,

$$Q_{\text{fuel}} + Q_{\text{gamma}} + Q_{\text{heaters}} = \text{constant} \qquad (19)$$

Eq 19 results from the fact that the facility will remain at a nominal 550 F in spite of the maximum variation in Q_{fuel} and Q_{gamma} because any change in these heat sources is automatically offset by a corresponding change in Q_{heaters}. Therefore we have only to monitor the heaters in order to accurately determine Q_{fuel}. The method of eq 19 is illustrated in the following example:

a) With the reactor on line, the facility in the normal operating mode, and with no fuel elements in the facility, $Q_{\text{heaters}} = 41$ KW

b) With the reactor on line, the facility in the normal operating mode, and with fuel elements in the facility, $Q_{heaters} = 16$ KW.

c) Using eq 19, it can be seen that Q_{fuel} is given by

$$Q_{fuel} = 41 - 16 = 25 \text{ KW}$$

EXPERIMENT 4 DISCUSSION

Experiment 4 demonstrates the importance of studying the equipment and considering all the possible ways of obtaining the desired measurment. The most direct method of measuring Q_{fuel} would be to make a heat balance across the fuel, but this method is unsatisfactory because of the very small temperature rise across the fuel. A brief study of the equipment is all that is required to determine several less direct but quite satisfactory methods of measuring Q_{fuel}.

FLOW MEASUREMENT

The above experiments avoided dealing with flow measurement for two reasons: most flow instruments are not linear; accurate flow measurement is so very simple. The accurate flow measurement of liquids requires little more than a stop watch, a bucket, and a "no springs" scale. The accurate flow measurement of gases requires little more than a stop watch, a tank of known volume, and a pressure gauge. This method of measuring gas flow is not widely known, but it works just as well as the corresponding method for liquids. Its application is based on the fact that, given the tank volume and temperature, the time rate of pressure decrease uniquely determines the flow rate from the tank. In a very real sense, the pressure gauge on the tank tells us the weight of the gas in the tank and the rate at which the pressure decreases tells us how this stored weight is changing as the result of the outflow from the tank. Expressed mathematically,

$$P_{tank} \propto W_{gas\ in\ tank} \qquad (20)$$

$$\frac{dP_{tank}}{dt} \propto \frac{dW_{gas\ in\ tank}}{dt} = \text{flow rate from tank} \qquad (21)$$

The constant of proportionality in eqs 20 and 21 is defined by the molecular weight of the gas, the volume of the tank, and the temperature of the gas.

THE INTANGIBLES

The history of science contains many puzzling stories like that of the Wright brothers--Jenner and his single-handed discovery of a smallpox vaccine--Pasteur and his single-handed conquest of rabies, anthrax, etc--Madame Curie and her single-handed separation of a speck of radium from tons of pitchblende--Mendel and his single-handed discovery of the science of genetics.

It seems unquestionable that each of these scientists used good experimental technique. But there must also have been something else in their technique which allowed them to hear Nature's testimony with such clarity. This something else is described in the following story told by Dr. Albert Schweitzer.

Dr. Schweitzer was an enemy alien during both World Wars and for that reason was required to leave Africa in order to take up residence in European concentration camps for enemy aliens. He reports that at one camp, the food was terrible--so terrible that a group of prisoners complained to the camp commander. The commander replied that the cooks were professionals who, during peacetime, had worked in the best hotels in Paris. However, to placate the prisoners, the commander turned the kitchen over to them for a trial period of two weeks. To Dr. Schweitzer's amazement, there was a great improvement in the quality of the food as soon as the amateurs replaced the professionals. To satisfy his curiosity, Dr. Schweitzer asked one of the amateurs how they had been able to do so well when the professionals had done so poorly. The kitchen 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.

CLOSING REMARKS

As shown in this chapter, good experimental technique is very easy to acquire. Its only requirements are an understanding of instrument fundamentals, an understanding of the experimental equipment, and the willingness to undertake a moderate amount of extra effort in the performance of the experiment.

The examples in this chapter deal primarily with temperature measurement, but it should be obvious that the methods employed in the examples apply equally to other types of instrumentation. The examples in this chapter are intended to illustrate the methods of good experimental technique in general by illustrating them in particular--it is the methods which are important and which are the real subject of this chapter.

Experiment is a very important part of the new heat transfer and for that reason, good experimental technique is very valuable--it allows us to hear Nature's testimony with the highest possible fidelity.

CHAPTER 7 HYSTERESIS, REPLICATION, AND FIT

INTRODUCTION

It is important to design and perform experiments in such a way that hysteresis effects are easy to detect. In other words, it is important to detect whether the behavior is uniquely determined by the values of the controlled parameters or whether the behavior is also affected by the prior operating history. This is especially important when dealing with highly nonlinear behavior, but it is also important even in the case of very simple behavior.

Experiments must also be designed and performed in such a way that an adequate number of "replicates" (ie repeated experiments) are obtained. The value of replicates is that they quantitatively describe the effect of the uncontrolled parameters in the experiment and the imprecision of the instruments. In this way, they enable us to estimate how good a "fit" might be obtained for the given data from the best possible correlation.

HYSTERESIS CAUSED BY SHORT TERM AGING

Suppose that, during the performance of our $q\{\Delta T_{DF}\}_1$ experiments in Ch 5, the inner surface of the test section was rapidly corroding and this corrosion fouled the inner surface, affecting $q\{\Delta T_{DF}\}$ in such a way that it decreased the value of q which would result from a given value of ΔT_{DF}. Under these conditions, if the experiment were performed by montonically increasing ΔT_{DF} in small increments, we would NOT obtain the "correct" answer that $q\{\Delta T_{DF}\}$ was a proportional function. Instead, we would find that $q\{\Delta T_{DF}\}$ was nonlinear and resembled the function in Fig 1 below:

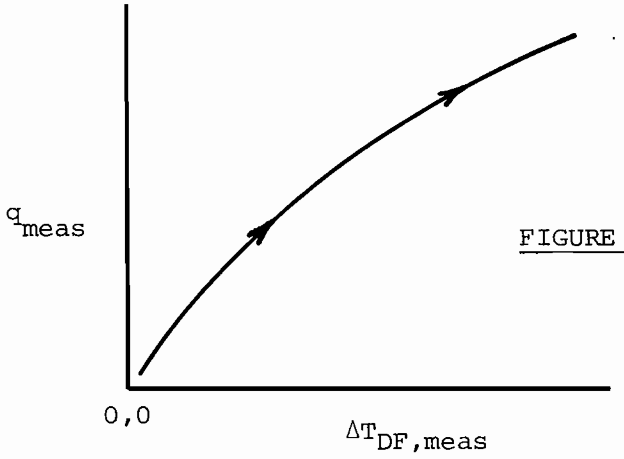


FIGURE 1

On the other hand, if the experiment were performed by monotonically decreasing ΔT_{DF}, we would find that $q\{\Delta T_{DF}\}_1$ resembled the function shown in Fig 2:

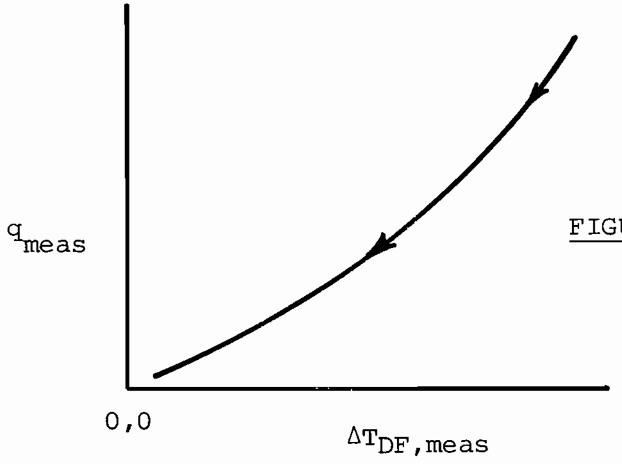


FIGURE 2

There are several ways to ensure that, if such a complication were to occur, we would detect it. One way is to design the experiment so that it calls for monotonically increasing and then decreasing the independently controlled parameter. In the above example, this type of experiment design would give the results indicated in Fig 3:

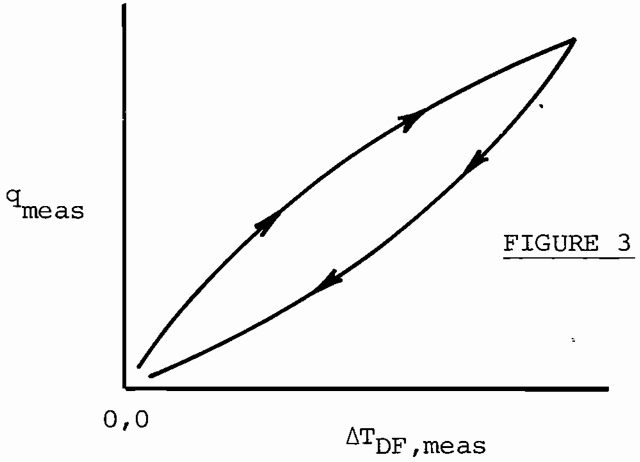


FIGURE 3

The pronounced hysteresis in Fig 3 at once tells us that the experiment is "out of control"--ie that there is an uncontrolled parameter which should be brought under control because its effect is too large to be neglected.

(It would also have been possible to detect the above short term aging effect by holding the equipment at a given point in the experiment design and noting that q had to be steadily decreased with time in order to maintain the desired value of ΔT_{DF}. The disadvantage of this method is that it consumes a great deal of time while resulting in only a minor simplification in the required data analysis.)

HYSTERESIS CAUSED BY NONLINEAR OPERATING CHARACTERISTICS

Oftentimes the parameters of concern in the experiment must be controlled indirectly. For example, in the pool boiling experiment by Berenson discussed in Bk 1, Chs 7, 8, 9, the temperature of the boiling interface was the parameter of concern. However, it was not possible to directly control this temperature. Instead, the temperature of the boiling interface had to be controlled indirectly by controlling the steam temperature in the steam chest below the boiler plate. As described in Bk 1, page 9-20, Berenson looked for and failed to find any evidence of hysteresis in the behavior of $q\{T_{bi}\}$. However, his data strongly suggests that there was very pronounced hysteresis in the behavior of $q\{T_{sc}\}$. Had he looked for and found this hysteresis, it would have been a very clear indicator that the desired data was NOT being obtained and that there was something very wrong about the widely held view that

> With a condensing vapor as the heat source on one side of a wall, any point on the entire pool boiling curve can be reached under stable conditions.

The point of this discussion is that, in performing experiments, the experimenter should not restrict his attention to the parameters of immediate concern, but should also pay close attention to the parameters which reveal the behavior of the experimental equipment. A real understanding of the experiment results is only possible if one has a good understanding of the equipment behavior.

REPLICATION

There are several reasons why it is essential to include replicates in the experiment design, the principal of which are:

1. Replicates describe whether or not the experiment is under control--ie whether or not the effect of uncontrolled parameters is sufficiently small that they can be permitted to remain uncontrolled.

2. Replicates describe the inherent scatter in the experiment results--ie they describe the effect of the uncontrolled parameters and the instrument imprecisions on the randomness of the data. If we plan to correlate the experiment results, it is very important to have a quantitative estimate of the inherent scatter in the data in order that we may have some idea of how well it should be possible to correlate. The scatter about any correlation must necessarily be equal to or larger than the inherent scatter in the data. For example, if the replicate standard deviation were found to be 3%, then the standard deviation about the best possible correlation for the given data would have to be at least 3%. Conversely, if the replicate standard deviation were 3%, we should hardly be satisfied with a correlation which resulted in a standard deviation of 10% between measured and predicted values.

In order to obtain replicates, we must repeat specific data points called for in the experiment design. For example, in Ch 5 we measured $q\{T\}_1$ at essentially 50 F intervals and reported the data in Table 3. Supposing that the Table 3 results were obtained by monotonically increasing T, we could combine the search for hysteresis and the measurement of replicates by then monotonically decreasing T over the same interval and measuring $q\{T\}_1$ at the same values of T employed during the increase. Or, if we felt it was not necessary to replicate each point, we might decide to measure $q\{T\}_1$ at every second or third value of T. These replicates are not "complete" replicates because they do not require us to replicate everything in the experiment--for instance, they do not require us to perform the repeat experiment with a different test section. These "partial" replicates describe the variability only with respect to the given test section. They tell us nothing about the variability which would result if the replication included different test sections of the same nominal diameter (although of course we recognize that complete replicates are necessarily more variable than partial replicates).

LONG TERM AGING EFFECTS

Suppose it took several weeks to perform the Ch 5 experiments on the 0.5" D test section. If some aging effect had been taking place during those tests, it would be essential that we detect it. This detection is best accomplished by periodically performing the same experiment--ie by including a "control point" in the experiment design--ie by periodically replicating the same data point in order to determine whether or not the experiment is under control. These replicates are included in the Ch 5 experiment design in that Tables 1-5 all call for the determination of the data point $q\{G_1, T_1, P_1, \Delta T_{DF1}\}$. Therefore, if some significant aging effect were occurring over the course of the 0.5" D experiment, it would be apparent because the control point would not replicate well from Table to Table--ie the replication "between" Tables would be much worse than the replication "within" Tables.

EXPERIMENTING WITHOUT REPLICATES

The experiment design in Ch 5 includes a number of replicates "within" test sections, but no replicates "between" test sections--ie the design calls for only ONE test section of each diameter. Therefore, when we complete the Phase 1 experiment, we will have NO good estimate of the variability which would have resulted if the experiment had included different test sections of the same nominal diameter. Without this estimate, we run two risks in our effort to correlate the data:

> We may accept a correlation which does not correlate the data well enough--ie the scatter about the correlation we accept may be considerably larger than the inherent scatter in the data.
>
> We may waste time and money looking for a better correlation long after we have found one which is truly adequate. In other words, if the scatter about the correlation were only slightly larger than the scatter inherent in the data, then it would be physically impossible to obtain a significantly better correlation FOR THE GIVEN DATA. (This should be stated as a statistical inference in order to be absolutely precise. I avoid statistical inference here because, although it would be more precise, it would also be less generally intelligible.)

Correlations such as relation 5-47 are generally intended to be used by equipment designers/analysts. Those who use such correlations generally want to know how well they can be expected to predict real world behavior. In the case of the Ch 5 experiment, we actually tested 4 different pipes and thus the correlation variability included variability between pipes. Therefore we could reasonably estimate that the user should expect the accuracy of the correlation to be roughly that indicated by the measured scatter about the correlation. (At the same time, we should recognize that a sample of only four is much too small to yield a reliable estimate of variability. We should also recognize that the scatter about the correlation is biased on the low side because it is determined largely by measurement imprecision rather than measurement inaccuracy.)

Now suppose that the Ch 5 experiment were concerned only with heat flow behavior to pipes of 0.5" D and that the experiment was performed with only one such test section. In this case, we would have NO data on variability between pipes and therefore we would have no reliable way of estimating the accuracy the user should expect from applying the correlation to many 0.5" D pipes. We could tell him that the accuracy will certainly be WORSE than the within pipes variability measured in our experiment, but the user is not interested in that. He wants to know what the accuracy will be BETTER than--and we have no good way of telling him that because we failed to obtain complete replicates.

CONCLUSIONS

This chapter indicates that the Ch 5 experiment design was not a really good design because it did not contain a sufficient number of replicates, particularly complete replicates.

The real point of this chapter is to point out that replicates MUST be included in experiment designs. Experimenting without replicates is like flying in a fog without instruments. It can be done, but there is no telling where you are or where you're going.

CHAPTER 8 PHASE 2

INTRODUCTION

Phase 1 of "experiment design" is based on the questionable assumption that, given a phenomenon in which y is some unknown function of t, u, v, w, x, z, the unknown function may be written in the form

$$y \rightarrow f_1\{t\}\ f_2\{u\}\ f_3\{v\}\ f_4\{w\}\ f_5\{x\}\ f_6\{z\} \qquad (1)$$

We know that this assumption is more generally applicable than the dimensional analysis of the old heat transfer because dimensional analysis is itself based on this same assumption PLUS several others which are also questionable. Even so, we have no basis on which to conclude that the assumption inherent in relation 1 is universally applicable. For that reason, it is ALWAYS necessary to verify this assumption and we must expect that it will SOMETIMES fail to verify. In those cases where the Phase 1 assumption fails to verify, the investigation must be carried onto "Phase 2".

The experimental and analytical methods of Phase 2 are the subject of this chapter. We will discuss these methods in some detail, but it must be noted that the precise nature of Phase 2 investigations will in each real world case depend on the results of Phase 1. This of course opens up an infinite variety of Phase 2 possibilities and prevents us from preparing a singular Phase 2 "recipe" similar to the one we prepared for Phase 1. However, we are able to prepare a very general Phase 2 recipe and it should be adequate to cope with real world problems.

REASONS BEHIND THE FAILURE TO VERIFY

If the assumption inherent in relation 1 fails to verify, the failure must have been caused by one of the following reasons:

1. An experimental or analytical error was made somewhere in the verification part of Phase 1.

2. The "true" functions do not multiply together as in relation 1. For instance, if the true overall function were given by

$$y \rightarrow f_1\{t\}\ f_2\{u\} + f_3\{v\}\ f_4\{w\}\ f_5\{x\}\ f_6\{z\} \qquad (2)$$

   then the verification test with the parameter t would indicate that

$$y\{t_i\}_2 \rightarrow a\ y\{t_i\}_1 + \text{constant} \qquad (3)$$

   and the Phase 1 assumption would fail to verify because, if rel 3 is true, then it must also be true that

$$\frac{y\{t_i\}_2}{y\{t_i\}_1} \neq \text{constant} \qquad (4)$$

3. The parameters are not separable in the manner suggested by relation 1. For example, if the true function could only be expressed in the form

$$y \rightarrow f_1\{t, u\}\ f_3\{v\}\ f_4\{w\}\ f_5\{x\}\ f_6\{z\} \qquad (5)$$

   then the assumption would again fail to verify with the parameter t and we would again obtain the inequality in relation 4.

ANALYZING THE PHASE 1 VERIFICATION RESULTS

The first step in Phase 2 is to consider the possibility that the failure to verify the assumption inherent in relation 1 was the result of an error, either experimental or analytical. This means we should study the verification data and review the data reduction and analysis in order to find the error or to rule out the possibility of error.

The second step is to analyze the Phase 1 verification results in order to determine as much as possible about the nature of the true function. For instance, we could conclude that the functions did not all multiply (ie that the true function resembled relation 2 in that it contained a plus sign) if the verification results indicated that:

1. None of the parameters verified--ie that

$$\frac{y\{u_i\}_2}{y\{u_i\}_1} \neq \text{constant} \qquad (6)$$

$$\frac{y\{v_i\}_2}{y\{v_i\}_1} \neq \text{constant} \qquad (7)$$

   and similarly for t, w, x, z.

2. The experiment results were in the form

$$y\{t_i\}_2 \rightarrow a\, y\{t_i\}_1 + A \qquad (8)$$

$$y\{u_i\}_2 \rightarrow b\, y\{u_i\}_1 + B \qquad (9)$$

$$y\{v_i\}_2 \rightarrow c\, y\{v_i\}_1 + C \qquad (10)$$

and similarly for w, x, z.

Moreover, if both the above are true, then the constants A, B, C, D, E, F suggested by relations 8-10 tell us a good deal about the nature of the true function. They tell us which parameters multiply together. For instance, if we found that

$$A = B \qquad (11)$$

$$C = D = E = F \qquad (12)$$

then we could conclude that the true function was in the form of relation 2 because this is the only form which would give the results described in 1 and 2 above and would also result in eqs 11 and 12.

In summary, if the assumption fails to verify because the functions do not all multiply together, then results 1 and 2 above will agree with the experiment results--ie the results will resemble relations 6-10. In such cases, the constants suggested by relations 8-10 will identify which functions multiply together.

On the other hand, if the assumption failed to verify because not all the parameters could be separated as in relation 1, this would be indicated by the following results:

1. The verification tests failed when tried with those parameters which could not be individually separated as required by relation 1. For instance, if the true function were relation 5, the results would have indicated that

$$\frac{y\{t_i\}_2}{y\{t_i\}_1} \neq \text{constant} \qquad (13)$$

$$\frac{y\{u_i\}_2}{y\{u_i\}_1} \neq \text{constant} \qquad (14)$$

2. The verification tests passed when tried with those parameters which could be individually separated as required by relation 1. For instance, if the true function were relation 5, the results would have indicated that

$$\frac{y\{v_i\}_2}{y\{v_i\}_1} = \text{constant} \qquad (15)$$

and similarly for w, x, z.

In summary, if the assumption fails to verify because some of the functions can not be written in terms of only one parameter, then some of the verification tests should fail and some should pass. The tests which pass indicate that those parameters can be individually separated as required by relation 1. The tests which fail indicate that those parameters are involved in multi-parameter functions.

DESIGNING THE PHASE 2 EXPERIMENT

The Phase 2 design must be based on the results of the verification testing in Phase 1. For example, if our analysis of the Phase 1 verification tests indicates that the overall function is the sum of two product groups such as in relation 2, then the design intent should be to hold one of the groups constant while we experiment with the other group. For instance, if our analysis of the Phase 1 results indicates that the true function has the form of relation 2, then our Phase 2 design should call for the following experiments:

$y\{t\}$ at u_1, v_1, w_1, x_1, z_1 (16)

$y\{t\}$ u_2, v_1, w_1, x_1, z_1 (17)

$y\{u\}$ t_1, v_1, w_1, x_1, z_1 (18)

$y\{u\}$ t_2, v_1, w_1, x_1, z_1 (19)

$y\{v\}$ t_1, u_1, w_1, x_1, z_1 (20)

$y\{v\}$ t_1, u_1, w_2, x_2, z_2 (21)

$y\{w\}$ t_1, u_1, v_1, x_1, z_1 (22)

$y\{w\}$ t_1, u_1, v_2, x_2, z_2 (23)

$y\{x\}$ t_1, u_1, v_1, w_1, z_1 (24)

$y\{x\}$ t_1, u_1, v_2, w_2, z_2 (25)

$y\{z\}$ t_1, u_1, v_1, w_1, x_1 (26)

$y\{z\}$ t_1, u_1, v_2, w_2, x_2 (27)

Note that the v, w, x, z group was held constant during experiments 16-19 while we experimented with the t, u group and that the t, u group was held constant during experiments 20-27 while we experimented with the v, w, x, z group. The experiment design described in 16-27 is a satisfactory design for the problem at hand, but there are many variations of this design which would be equally satisfactory.

Now suppose that our analysis of the Phase 1 verification results indicated that the assumption failed to verify because one or more of the functions could not be written in terms of a single parameter. In that case, the design intent would be to design a multi-dimensional experiment dealing only with those parameters which did not verify. For instance, if our analysis indicated that relation 5 was the true function, the Phase 2 design would consist of the following:

$y\{t\}$ at u_1, v_1, w_1, x_1, z_1 (28)

$y\{t\}$ u_2, v_1, w_1, x_1, z_1 (29)

$y\{t\}$ u_3, v_1, w_1, x_1, z_1 (30)

$y\{t\}$ u_4, v_1, w_1, x_1, z_1 (31)

$y\{t\}$ u_5, v_1, w_1, x_1, z_1 (32)

Note that experiments 28-32 are concerned only with the three parameters y, t, u. In other words, this design is a "3-dimensional" design whereas heretofore we have dealt with only 2-dimensional experiments. It is perhaps not necessary to say that the induction of a 3-dimensional function is a good deal more difficult than that of a 2-dimensional function.

ANALYZING THE PHASE 2 EXPERIMENT RESULTS

The experiment described in 16-27 makes the data analysis very simple because it provides the data in the convenient form

$$y_{16} \rightarrow a\ f_1\{t\} + b \tag{33}$$

$$y_{17} \rightarrow c\ f_1\{t\} + b \tag{34}$$

This convenient form results because b in rel 33 is uniquely determined by the values of v, w, x, z. The constant b reappears in rel 34 because v, w, x, z are maintained at the same values in experiments 16 and 17. The constant "a" in rel 33 assumes a different value in rel 34 because a and c are determined by the value of u which is held at different values in experiments 16 and 17. (This is all very obvious if you compare relations 33 and 34 with rel 2. The two groups on the right side of 33 and 34 correspond to the two groups on the right side of rel 2. That is why b is uniquely determined by the values of v, w, x, z and why a and c are determined by the value of u.)

Because the y{t} data is in the convenient form of relations 33 and 34, we can very simply determine $f_1\{t\}$ in the following way:

1. Determine the value of b which is required in order to satisfy eq 35:

$$\frac{y_{16}\{t_i\} - b}{y_{17}\{t_i\} - b} = \text{constant} \tag{35}$$

2. Note from 33 and 34 that $f_1\{t\}$ is proportional to $(y_{16}\{t_i\} - b)$ and to $(y_{17}\{t_i\} - b)$. This means that we can determine $f_1\{t\}$ by plotting $(y_{16}\{t_i\} - b)$ vs t.

The experiment design described in 28-32 gives us 3-dimensional data which we can plot in the form y{t} with u as a parameter as shown in Fig 1:

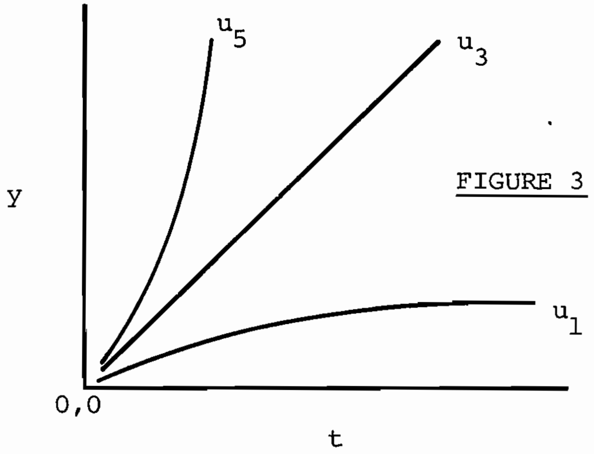


FIGURE 3

From this 3-dimensional plot, we must induce the function y{t,u}. The induction of this 3-dimensional function is not a simple task, but this is as simple as we can make it.

CONCLUSIONS

Phase 2 of experiment design deals with phenomena which are too complex to be handled by Phase 1. However, it is conceivable that there will be little need for Phase 2. It should be recalled that Phase 1 is much more widely applicable than dimensional analysis, the reason being that dimensional analysis is based on five questionable assumptions whereas Phase 1 is based on only one of the five. In the old heat transfer, dimensional analysis was for many years regarded as a generally applicable analytical tool. Since Phase 1 is much more widely applicable than dimensional analysis, it is conceivable that Phase 1 will usually be sufficient and that there will be little need for Phase 2. That is why we have only briefly discussed Phase 2 here, the major intent being to point out that there is still another powerful method of solution should one find in some particular case that Phase 1 is not adequate.

CHAPTER 9 WATER AND AIR

INTRODUCTION

In the engineering world, water and air are the primary heat flow fluids. These two fluids find such universal application that it is surprising they have not yet warranted their own individual correlations. There is not yet a widely accepted and easily used correlation for heat flow to either water or air flowing in pipes. In the old heat transfer, the heat flow behavior to water or air flowing in pipes is usually described by a generalized equation which "applies" to a large number of fluids besides water and air. This generalization supposedly brings a great "unification" to one phase, forced convection heat flow since it purports to describe the heat flow behavior of a great many different fluids. However, the use of this generalized correlation has a very serious drawback which is not widely recognized in the old heat transfer--it largely prevents us from having and using the simplest possible correlations. It would be possible to replace the generalized correlation with a specific correlation for each fluid and in such a way that the specific correlations were much simpler to use than the general correlation.

In this chapter, we ask and answer the question:

> Assuming that the generalized one phase, forced convection heat flow correlation of the old heat transfer accurately applies to water and air, would it be possible to replace the general correlation with a specific correlation which applied only to water and would be much simpler to use than the generalized correlation? Would it be possible to obtain a similar correlation for air?

We answer this question by assuming that the generalized correlation from the old heat transfer,

$$Nu = .023\ Re^{.8}\ Pr^{.4} \qquad (1)$$

accurately applies to one phase, forced convection heat flow to water and also to air. We obtain a specific correlation for water by the suitable transformation of eq 1 and then we repeat the analysis for air. It will be seen that the correlations we obtain in this manner are indeed much simpler to use than eq 1.

Of course eq 1 is not truly difficult to use--it does not defy understanding. But anyone who has actually used eq 1 (or other correlations like it) to solve real world problems knows that it is very cumbersome. The use of eq 1 also involves the use of several functions which are so complex they are usually described graphically or in tables rather than analytically. As we shall see, by replacing eq 1 with specific correlations, we can obtain correlations which are completely analytical and thus do not require the use of additional graphs or tables.

It must be emphasized that, throughout this chapter, we are ASSUMING that eq 1 accurately applies to water and air and therefore we will learn NOTHING here about the real world behavior of water or air. We will simply transform a cumbersome, generalized correlation to two highly specific, easy to use correlations for water and for air. These two transformed correlations are intended to accomplish two ends:

1. Illustrate that it is possible to obtain useful correlations using the new heat flow method of correlating what we measure rather than correlating what we do not measure--of finding out rather than supposing.

2. Demonstrate that the new heat flow method of correlating what we measure leads to correlations which are more useful than those which result from the methods of the old heat transfer.

Since the transformed correlations are derived from eq 1 which is largely the result of supposing, it follows that the transformed correlations are also the result of supposing. In other words, the transformed correlations are necessarily as inaccurate as eq 1 because they are derived from it. The transformed correlations are intended to be used ONLY until water and air correlations are obtained using the "finding out" methods of the new heat flow which are described in the earlier chapters.

The method we use in this chapter to transform eq 1 is of course generally applicable. The reader may wish to transform eq 1 for those fluids which are of particular interest to him. This transformed correlation would be used only until a specific correlation were obtained within the framework of the new heat flow.

WHAT DOES EQUATION 1 REALLY SAY?

Equation 1 is written in the imaginary world language of dimensional analysis. It is this artificial language which makes eq 1 seem to be simple, analytical, and convenient when the truth is it is NOT simple, it is NOT analytical, it is NOT convenient--not in the real world. It will probably surprise some readers to learn that eq 1 is not really an analytical correlation. It is essentially a GRAPHICAL correlation because, hidden behind the iron mask of dimensional analysis are three complex functions which are normally described graphically (or in tables). In order to see this, we must first rewrite eq 1 in the language of the real world:

$$\frac{q\ D}{\Delta T\ k} = .023(DG/\mu)^{.8}(C\mu/k)^{.4} \qquad (2)$$

$$q = .023\ D^{-.2}\ G^{.8}(k\{T\})^{.6}(C\{T\})^{.4}(\mu\{T\})^{-.4}\ \Delta T \qquad (3)$$

(Note that eq 3 neglects the very weak effect that pressure has on k, C, μ.) And now that we have written eq 1 in the real world language of eq 3, it is time to ask

> What is the nature of these functions k{T}, C{T}, μ{T}? Are these functions simple so that we may correctly claim that eq 1 is in fact simple? Are these functions usually given analytically so that we may correctly claim that eq 1 is in fact an analytical correlation? Are these functions described in a convenient way so that we may correctly claim that eq 1 is in fact convenient to use?

And of course the answer is that these functions are not simple--in fact, they are so complex that they are usually expressed graphically. And this complexity means that it is not accurate to claim that eq 1 is simple, analytical, or convenient--not in the real world. Eq 1 is simple, analytical, and convenient only if one remains in the imaginary world. But as soon as we transform it to the real world, it is obvious that it is none of these things.

DERIVING SPECIFIC CORRELATIONS FROM EQUATION 1

Equation 3 brings up the interesting question:

> If a designer/analyst works with the same heat flow fluid from day to day (and many designers/analysts do), why would he want to use eqs 1 or 3 since they contain THREE complex functions of the SAME parameter?

And of course there is no good answer to this question. Obviously a designer/analyst who works with the same fluid from day to day would rather have a correlation which contains one rather than three complex functions. Since the above three complex functions are all uniquely determined by the same parameter, it follows that it must be possible to replace them with a single function whose exact nature varies from fluid to fluid. In other words, for any fluid, there must be some function F{T} which satisfies the relation

$$(k\{T\})^{.6}(C\{T\})^{.4}(\mu\{T\})^{-.4} \rightarrow F\{T\} \qquad (4)$$

where the exact nature of F{T} varies from fluid to fluid. Combining 3 and 4, we obtain a new generalized correlation described by

$$q \rightarrow .023\ D^{-.2}\ G^{.8}\ F\{T\}\ \Delta T \qquad (5)$$

Note that this new generalized correlation is much more useful than eq 3 because it replaces three complex, temperature dependent functions with one complex, temperature dependent function. If a designer/analyst wished to replace the three functions of eq 1 with a single function, he would merely have to solve rel 4 for the fluid which concerned him. For example, if he were concerned with water, he might prepare an F{T} graph by plotting the left side of rel 4 against T on linear graph paper, using whatever water property tables he preferred. Then, when he wished to calculate q given the values of D, G, T, and ΔT, he would need only the F{T} graph and relation 5 rather than the k{T}, C{T}, and μ{T} graphs and equation 1.

Moreover, if F{T} turned out to be relatively simple, it MIGHT be possible to express F{T} analytically, in which case there would be no need for graphs or tables in order to deal with the fluid of concern.

DERIVING A SPECIFIC CORRELATION FOR WATER FROM EQ 1

In order to transform eq 1 to a specific correlation for water, we must first select the water properties k{T}, C{T}, μ{T}. Let us agree on the water properties given in Table 1, next page. Using these water properties, we determine $F\{T\}_{water}$ from relation 4:

T	$F\{T\}_{water} \rightarrow k^{.6}\ C^{.4}\ \mu^{-.4}$
F	(dimensions are B, #, ft, F, hr)
100	.4458
150	.5515
200	.6441
240	.7112
300	.7964
350	.8529
400	.9004
450	.9415
500	.9685

The next step is to plot the above coordinates on a LINEAR graph as shown in Fig 1, page 9-8. The curve in Fig 1 is the "picture" of the function $F\{T\}_{water}$ which results from assuming that eq 1 accurately describes the heat flow behavior to water.

Once we have prepared Fig 1, we are confronted with a choice. Should we be satisfied with the graphical expression of $F\{T\}_{water}$ or should we search for a simple analytical expression which would closely approximate the graphical expression? Let us choose the latter alternative and begin our search by noticing in Fig 1 that the curvature is more or less constant--ie the curve in Fig 1 suggests that

TABLE 1

PHYSICAL PROPERTIES OF WATER AND AIR

(Dimensions are B, #, ft, F, hr)

T	k	C	μ	fluid
100	.364	.997	1.65	water
150	.381	.999	1.04	"
200	.392	1.004	.74	"
240	.396	1.010	.59	"
300	.395	1.026	.45	"
350	.391	1.044	.38	"
400	.384	1.067	.33	"
450	.373	1.095	.29	"
500	.356	1.130	.26	"
100	.0157	.240	.0459	air
200	.0181	.241	.0519	"
300	.0203	.243	.0574	"
400	.0225	.245	.0626	"
500	.0246	.248	.0675	"
600	.0265	.250	.0721	"
700	.0284	.254	.0765	"
800	.0303	.257	.0806	"
900	.0320	.260	.0846	"

(From J. P. Holman, "Heat Transfer", 3rd edition, McGraw-Hill Book Company, New York, 1972; water properties are for saturated liquid; air properties are at atmospheric pressure.)

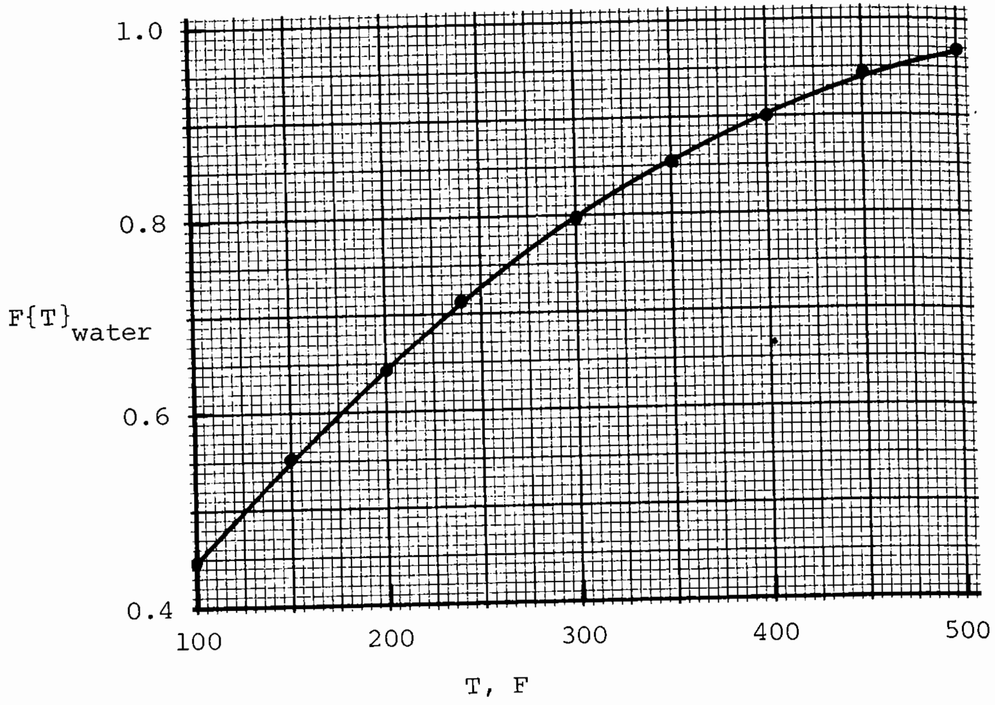


FIGURE 1 $F\{T\}_{water}$ based on relation 4

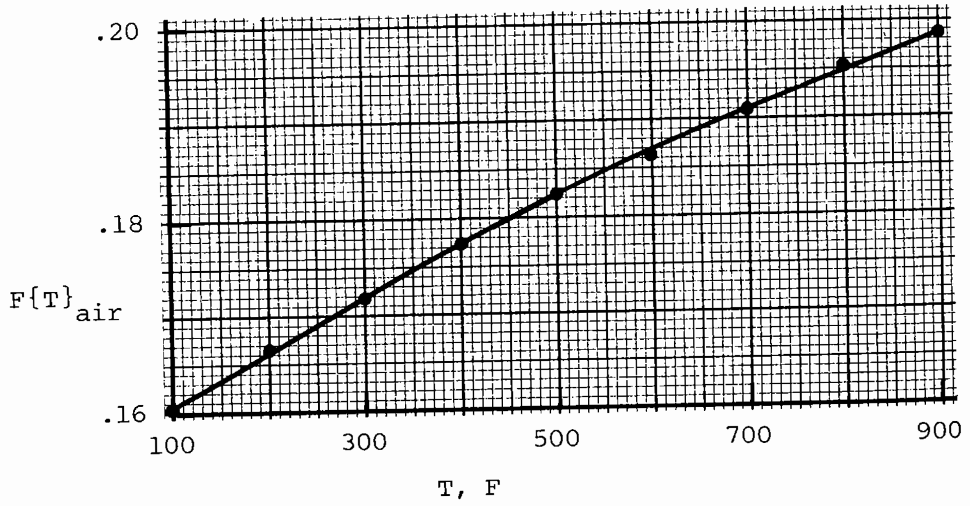


FIGURE 2 $F\{T\}_{air}$ based on relation 4

$$\frac{d^2F}{dT^2} \approx \text{constant} \tag{6}$$

Equation 6 suggests that we approximate F{T} in Fig 1 with a function of the form

$$F\{T\} = a + bT + cT^2 \tag{7}$$

Combining 5 and 7, we obtain the new form

$$q \rightarrow .023\ D^{-.2}\ G^{.8}(a + bT + cT^2)\ \Delta T \tag{8}$$

which is suggested by the physical properties of water but which may also agree with the properties of other fluids, in which case relation 8 would be more or less general and only the values of a, b, c would vary from fluid to fluid.

Now let us determine the values of a, b, c which will cause F{T} in eq 7 to closely agree with F{T} in Fig 1. Again, we will not try to determine the "best fit" values but will be satisfied with "reasonable fit" values for a, b, c. We can determine reasonable fit values by selecting three F{T} coordinates from those listed on page 9-6, substituting these into eq 7, and solving the resultant three equations for a, b, c. Selecting the coordinates (150, .5515), (300, .7964), and 450, .9415), we obtain the equations

$$.5515 = a + 150b + 150^2c \tag{9}$$

$$.7964 = a + 300b + 300^2c \tag{10}$$

$$.9415 = a + 450b + 450^2c \tag{11}$$

The solution of eqs 9 to 11 yields

$$a = .2068$$

$$b = 2.631 \times 10^{-3}$$

$$c = -2.218 \times 10^{-6}$$

With these constants, the F{T} approximation from eq 7 closely agrees with F{T} in Fig 1 as shown in the following comparison:

T_{water}	$F\{T\}_{Fig\ 1}$	$F\{T\}_{eq\ 7}$	Difference, %
100	.4458	.4477	0.43
150	.5515	.5515	0.00
200	.6441	.6443	0.03
240	.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

$F\{T\}_{Fig\ 1} = k^{.6}\ C^{.4}\ \mu^{-.4}$ evaluated at saturated liquid; see page 9-7

$F\{T\}_{eq\ 7} = a + bT + cT^2$ using a, b, c values at top of this page

Dimensions are B, #, ft, F, hr

As indicated above, the eq 7 approximation for the "true" F{T} of eq 1 is accurate to within a fraction of a per cent. This in turn means that relation 8 agrees with eq 1 within a fraction of a per cent when water is the heat flow fluid, provided that relation 8 is used with the above values of a, b, c. In other words, eq 1 and rel 8 give essentially the same answers for water, the only difference being that rel 8 is much more convenient to use because it is a COMPLETELY ANALYTICAL correlation whereas eq 1 is actually a disguised GRAPHICAL or TABULAR correlation.

In summary, we transformed the generalized and graphical eq 1 to the analytical rel 8 which we have so far shown applies only to saturated water. We did this by first rewriting eq 1 in the form of rel 5 in order to separate out the temperature dependent functions in eq 1. We grouped these functions in the new function $F\{T\}_{water}$ and evaluated this function for saturated water with the results shown in the table on page 9-6 and the graph on page 9-8. We approximated this $F\{T\}_{water}$ function with a simple analytical expression (eq 7) which agreed with the function within a fraction of a per cent. We substituted this analytical expression into rel 5 and this completed the transformation of eq 1. The "error of transformation" in this case was only a fraction of a per cent--a small price to pay for converting the cumbersome, graphical eq 1 to the convenient, analytical rel 8.

DERIVING A SPECIFIC CORRELATION FOR AIR FROM EQ 1

A specific correlation for air is derived from eq 1 in the same manner we used for water, the only difference being that we start with a different set of fluid properties. Using the air properties in Table 1, page 9-7, we determine $F\{T\}_{air}$:

T F	$F\{T\}_{air} \to k^{.6}\ c^{.4}\ \mu^{-.4}$ (dimensions are B, #, ft, F, hr)
100	.1603
200	.1665
300	.1719
400	.1772
500	.1822
600	.1862
700	.1908
800	.1951
900	.1987

Plotting $F\{T\}_{air}$, we obtain Fig 2 on page 9-8. Noting from Fig 2 that the curvature is more or less constant, we again approximate the curve with the expression in eq 7. Using the coordinates (200, .1665), (500, .1822), and (800, .1987), we obtain the values

$$a = .1544$$

$$b = 6.37 \times 10^{-5}$$

$$c = -1.59 \times 10^{-8}$$

which, together with relation 8, describe the heat flow behavior of forced convection air. The close resemblance between eq 1 and rel 8 for heat flow to air is shown in the following comparison:

T_{air}	$F\{T\}_{Fig\ 2}$	$F\{T\}_{eq\ 7}$	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

$F\{T\}_{Fig\ 2} = k^{.6}\ c^{.4}\ \mu^{-.4}$ evaluated at atmospheric pressure; see page 9-7

$F\{T\}_{eq\ 7} = a + bT + cT^2$ using a, b, c values from page 9-12

Dimensions are B, #, ft, F, hr

As in the water example, eq 1 and rel 8 give essentially the same answers for air, the only difference being that rel 8 is many times more convenient to use than eq 1.

F{T} IN THE REAL WORLD

In this chapter, we have determined F{T} for water and air by making the questionable assumption that eq 1 accurately applies to one phase, forced convection heat flow to water and air flowing in pipes. Of course this is only a temporary expedient which we will abandon as soon as possible. In the new heat flow, we will in fact determine F{T} by "finding out" in precisely the manner described in Chapter 5. We will determine

$F\{T\}_{water}$ by performing carefully designed experiments with WATER; we will determine $F\{T\}_{air}$ by performing carefully designed experiments with AIR. We will NOT suppose that water experiments establish the heat flow behavior to air.

If it is true that eq 1 accurately describes heat flow behavior to water, then when we actually measure $F\{T\}_{water}$, it should closely resemble Fig 1. Will I be surprised if the measured function does NOT closely resemble Fig 1? No! Why not? Because I have performed many heat flow experiments and I have very seldom found any agreement between the correlations of the old heat transfer and the functionality of the real world.

The simplest possible correlation problems are those which involve only two parameters. It is a very minor task to experimentally determine functionality when the problem consists of only two parameters. The nucleate boiling problem I have discussed several times in Books 1 and 2 is a problem dealing with only 2 parameters, q and ΔT. The problem is to determine the functionality between q and ΔT. And the point I have made several times is that the methods of the old heat transfer are not powerful enough--are not rigorous enough--to solve even this very simple, 2-parameter problem. And the proof of this is that the methods of the old heat transfer led to the widely held conclusion that the functionality was described by

$$q \rightarrow a\ \Delta T^{n} \tag{12}$$

where n is "3 or 4" when in fact the data indicated that the functionality was actually described by

$$q \rightarrow a\ \Delta T + b \tag{13}$$

And the reason I have stressed this point several times is because I have been leading up to the question:

> If the methods of the old heat transfer are so powerless they can NOT successfully cope with simple 2-parameter problems, what reason is there to suppose that they can successfully cope with complex, 6-parameter problems such as one phase, forced convection heat flow?

And this rhetorical question is of course the answer to why I am never surprised to find that the correlations of the old heat transfer bear no resemblance to real world behavior, no matter how simple the problem.

F{T} AND CHAPTER 5

In Chapter 5, we dealt with imaginary data. However, as the reader may have noticed, $f_3\{T\}$ in Chapter 5 was essentially the same as $F\{T\}_{water}$ in this chapter. In Chapter 5, I indicated that I had not faked the example by starting with the analytical form of $f_3\{T\}$, but rather had started with the imaginary data and had in fact induced the analytical function from this "data". The q{T} data points in Ch 5, Table 3, pg 5-6 were determined from the relation

$$q\{T\} \propto k^{.6}\ C^{.4}\ \mu^{-.4} \tag{14}$$

These data points were then plotted as shown in Fig 3, Ch 5 and the analytical function, $f_3\{T\}$, was in fact determined by induction. If the reader will compare the q{T} data points from Table 3, Ch 5 with the $F\{T\}_{water}$ points in the table on page 9-6, he will "find out" that the $f_3\{T\}$ example was not faked.

DISCUSSION

Since $(a + bT + cT^2)$ closely resembles $(k^{.6}C^{.4}\mu^{-.4})$ for two fluids as different as water and air, it seems reasonable to suppose that the same would be true for a large number of fluids. Whether or not this same simple polynomial also describes real world behavior will not be known until experiments are performed within the framework of the new heat flow.

To persons actively engaged in research/development rather than in design/analysis, it may not be obvious that rel 8 is many times more convenient to use than eq 1. But note that, for any fluid, eq 1 requires the additional support of 3 graphs or tables whereas rel 8 requires only the additional support of the 3 constants in rel 8. For instance, if you decide to use eq 1 in the design of water/air heat exchangers, you are actually deciding to use eq 1 AND SIX GRAPHS OR TABLES:

$k_{water}\{T\}$, $C_{water}\{T\}$, $\mu_{water}\{T\}$

$k_{air}\{T\}$, $C_{air}\{T\}$, $\mu_{air}\{T\}$

On the other hand, if you decide to use rel 8, you are actually deciding to use rel 8 and SIX CONSTANTS:

a_{water}, b_{water}, c_{water}

a_{air}, b_{air}, c_{air}

In other words, six new way constants replace six old way graphs or tables. And it goes without saying that it is much more convenient to work with six constants than to work with six graphs or tables.

It is unfortunate that $(k^{.6}C^{.4}\mu^{-.4})$ suggests the polynomial $(a + bT + cT^2)$ for water and for air. Polynomials are a favorite form with empiricists and the reader may conclude from this that the new heat flow rests on an empirical foundation rather than a scientific foundation. Nothing could be farther from the truth. Science is listening to and learning from Nature--it is taking specific examples from Nature and using them to learn about Her behavior. And that is precisely what we do in the new heat flow. We take specific data from Nature and we use them to learn how the parameters are related to each other--and we do this with NO preconceived ideas about whether these relationships should be expressed only in terms of power laws or polynomials or whatever.

It is only in the old heat transfer that power laws are viewed as "scientific" and polynomials are viewed as "empirical". In the new heat flow, all functions are considered to possess the same level of science--the same level of empiricism. In the new heat flow, all forms are equally acceptable provided only that they closely resemble real world behavior. It is only in the old heat transfer that power laws are the preferred language of "science". It is only in the old heat transfer that it makes little difference whether or not the correlating functions closely resemble real world behavior.

There is a very fundamental difference between the form of eq 1 and the form of rel 8. But this difference is only a preview of what is to come. When eq 1 is written in the form of eq 3, it is readily apparent that eq 1 actually consists of six power laws--one each for D, G, k, C, μ, and ΔT. Relation 8 contains only three power laws--one each for D, G, and ΔT. But the only reason relation 8 contains three power laws is because everything in this chapter is based on the questionable assumption that eq 1 accurately describes heat flow behavior to water and air. But we are going to abandon this questionable assumption as soon as possible--as soon as we perform experiments within the framework of the new heat flow. And what will we find then? Will the data suggest that q{D} is best described by a power law? I doubt it. Will the data suggest that q{G} is best described by a power law? I doubt it. Will the data suggest that q{ΔT} is best described by a power law where the exponent is unity--ie will the data suggest that q is indeed proportional to ΔT? Certainly for some fluids and for small values of ΔT, we will expect to find that q is proportional to ΔT. But for many other fluids and for large values of ΔT, I seriously doubt that we will find that q is proportional to ΔT. What will we find? No one knows. What should we be willing to accept? Whatever Nature tells us--whatever the data seems to suggest--whatever closely resembles REAL WORLD BEHAVIOR. Why should we be willing to accept anything the data tells us? Because that is science.

CONCLUSIONS

Until heat flow correlations are obtained within the framework of the new heat flow, it would be very convenient to replace the graphical eq 1 with the analytical rel 8. This will require that values of a, b, c be calculated for those fluids whose behavior is currently dealt with using eq 1.

CHAPTER 10 HIGHLY NONLINEAR PHENOMENA

INTRODUCTION

The researcher who undertakes the investigation of a highly nonlinear phenomenon will inevitably be confronted with many choices, the principal of which is: Should he study the phenomenon as a whole or should he restrict the investigation to some narrow aspect of the problem? Should he invent some narrow "regime" and then carefully avoid investigating/reporting anything outside this regime? Or, if he is particularly concerned with some aspect of the problem, should he first investigate the phenomenon as a whole in order to determine how this aspect fits into the overall problem? In other words, should the researcher study the whole elephant or should he overlook the elephant and restrict his investigation to the tail? And if he is particularly interested in the tail, should he first examine the elephant in order to determine where the tail is located?

In the old heat transfer, it is commonplace to deal with highly nonlinear phenomena in a very piecemeal fashion by inventing numerous "regimes". Many of these "regimes" have nothing to do with fundamental process changes (for example, the fundamental change involved in the transition from the laminar flow regime to the turbulent flow regime), but are brought about by the "need" to fragment the phenomenon into pieces small enough to be analytically described by power laws (for example, the "away from slot regime" described in Bk 1, Ch 6).

In the new heat flow, it is commonplace to deal with highly nonlinear phenomena in an overall and therefore highly useful way. Regime-oriented studies are permitted, but only in conjunction with a study of the overall phenomenon. Regimes are permitted only when there is overwhelming evidence of a fundamental change in the nature of the process (such as the change from laminar to turbulent flow). Regimes are NOT permitted to be invented simply because the data does not lend itself to correlation with power laws or because the data "fail" to agree with speculative results obtained by a priori deduction.

In this chapter, we discuss highly nonlinear phenomena in general by discussing film cooling in particular. We deal with film cooling using the methods generally used in the old heat transfer and the methods of the new heat flow. But it is very important to note that we are not concerned here with film cooling--our real concern is with the METHODS of dealing with highly nonlinear phenomena in general. For our purpose, film cooling is nothing more than a convenient vehicle which simplifies the discussion and enforces the argument by supplying us with a real world example.

(The "old" methods described in this chapter are indeed those generally used in the old heat transfer. However, it is not my contention that these "old" methods are used without exception in the old heat transfer. The literature does contain a few excellent studies in which highly nonlinear phenomena are dealt with in an overall and useful way. But they are rare exceptions and they are seldom referenced in the literature or in the popular texts on the old heat transfer.)

THE REAL WORLD PROBLEM

The phenomenon known as film-cooling has a number of real world applications. As noted by Hartnett, Birkebak, and Eckert (1),

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

Since I have some real world experience in the research/development/design/analysis/invention/testing/data reduction phases of gas turbine engine components, let us discuss film cooling against the real world background of gas turbine engines. In this application, as in many others, the real world problem for the thermal designer is:

> Obtain the required degree of cooling with the MINIMUM amount of coolant.

In aircraft gas turbines, the coolant air is generally bled from the compressor. This bleed air has a negative effect on cycle efficiency and for that reason there is a great deal of pressure on the thermal designer to use the absolute minimum amount of coolant flow. With regard to film cooling systems, this means that the real world design objective is:

> Design the film cooling system in such a way that the required cooling will be obtained with the MINIMUM amount of coolant.

In order to design this optimum system, we must know the functionality among the parameters which influence and largely determine film cooling behavior. In other words, we must have a correlation which describes the parametric functionality, and this correlation may be either graphical or analytical. If this correlation closely describes the real world behavior of film cooling, then the design of an optimum film cooling system will be a simple matter. On the other hand, if the correlation does NOT resemble real world behavior, then it is quite likely that we will either overcool or undercool the structure. If we overcool, it is quite likely that no one will ever know about it and the design will be considered "adequate". If we undercool and the structure fails, then everyone will know about it and the design will rightly be considered inadequate. In the former case, we waste coolant and penalize the overall performance of the equipment. In the latter case, we end up with a truly faulty design which must be corrected, usually by trial-and-error. In any event, the real world requirement that we minimize the amount of coolant flow means that we must run two risks--the risk of wasting precious coolant and the risk of damaging precious parts. There is only one way to minimize both risks, and that is to obtain a correlation which ACCURATELY describes real world behavior. And, for the researcher/developer, the real world problem is the determination of correlations which ACCURATELY describe real world behavior. For the designer/analyst, the real world problem is the selection and utilization of accurate correlations in order to optimize equipment design. For the thermal designer, this optimization will oftentimes involve designing for the minimum possible coolant flow and this is indeed the case for most film cooling systems in aircraft gas turbines.

SOLVING THE REAL WORLD PROBLEM USING $\eta\{x/Ms\}$

Reference 1 states:

> . . . a reasonable estimate of the effectiveness for practical applications can be obtained from Fig 26.

Fig 26 is a graphical correlation in the form $\eta\{x/Ms\}$. The curve in Fig 26 is drawn through the authors' data obtained at many different values of x/Ms. Fig 26 also contains data points obtained by several other investigators and the scatter of all the data is, according to the authors, about plus or minus 40%. As the authors point out, their data in Fig 26

> are correlated for x/Ms > 60 by the relation

$$\eta = 16.9(x/Ms)^{-.8} \qquad (1)$$

Let us take the authors' recommendation and use eq 1 to solve the practical, real world problem of designing a film cooling system which is optimum in the sense that it uses the minimum amount of coolant flow.

In order to optimize the system design, we must somehow determine the optimum values of those parameters which can be controlled by the thermal designer. Rewriting eq 1 in order to eliminate the dimensionless group M, we obtain

$$\eta = 16.9(xG_m/G_s s)^{-.8} \qquad (2)$$

Of the four designer-controlled parameters in eq 2, G_s and s are usually controlled by the thermal designer and so the real world problem is to determine the optimum values of these two parameters--ie we wish to determine the value of G_s and the value of s which will give the highest effectiveness η FOR A GIVEN COOLANT FLOW RATE and for fixed values of G_m and x. We can determine these optimum values by formulating and studying the functions

$$\eta\{G_s\}_{\text{fixed } W_s, G_m, x} \qquad (3)$$

$$\eta\{s\}_{\text{fixed } W_s, G_m, x} \qquad (4)$$

To convert eq 2 to the form of functions 3 and 4, we note that

$$G_s = W_s/s \qquad (5)$$

Combining 2 and 5, we obtain

$$\eta = 16.9(xG_m/W_s)^{-.8} \qquad (6)$$

Eq 6 states that η is COMPLETELY DEFINED by the values of x, G_m, W_s. Therefore, if the values of x, G_m, W_s are fixed as in 3 and 4, then η IN NO WAY depends on G_s or s or anything else and the functions in 3 and 4 are described by

$$\eta\{G_s\}_{\text{fixed } W_s, G_m, x} = \text{constant} \qquad (7)$$

$$\eta\{s\}_{\text{fixed } W_s, G_m, x} = \text{constant} \qquad (8)$$

Equations 6 to 8 state that, in this real world example, the thermal designer can NOT influence film cooling behavior by attempting to optimize the values of G_s and s because film cooling behavior is NOT affected by G_s or s in spite of the fact that eq 2 seems to suggest that they have a first order effect! Given the values of x and G_m, eq 6 states that the thermal designer can influence film cooling in only one way--by increasing or decreasing W_s. Equation 6 also states that this $\eta\{W_s\}$ function is very well behaved and is given by

$$\eta\{W_s\}_{\text{fixed } G_m, x} \propto W_s^{.8} \qquad (9)$$

In summary, eq 1 indicates that our real world film cooling system will be optimum for all values of G_s and all values of s and therefore the system can be designed around any convenient values of G_s and s. Equation 1 also says that $\eta\{W_s\}$ is a very well behaved function and that increasing the coolant flow will increase the effectiveness in an almost linear manner. We therefore conclude that the design of an optimum film cooling system is a very simple matter IF eq 1 accurately describes real world behavior.

THE REAL WORLD BEHAVIOR OF FILM COOLING

As we noted above, eq 1 indicates that $\eta\{W_s\}$ is the very simple function described by relation 9 which says that η is uniquely determined by W_s and that increases in W_s always result in increases in η. Graphically, the function described by eq 1 has the shape

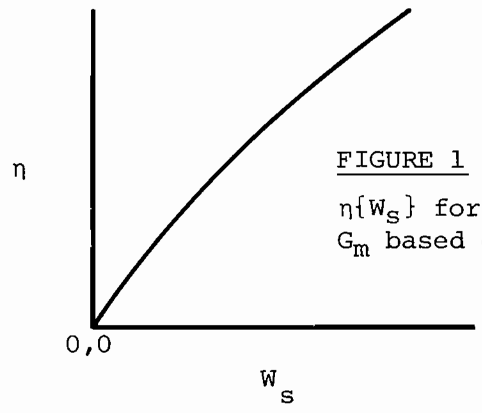


FIGURE 1

$\eta\{W_s\}$ for fixed x and G_m based on eq 1

The question now arises:

> Does film cooling behavior in the real world closely resemble the behavior described by Fig 1?

The answer is no. The function $\eta\{W_s\}$ is NOT a simple function--it is NOT a well-behaved function. For given values of G_m and x, the value of η is NOT uniquely determined by the value of W_s. Film cooling behavior in the real world is oftentimes so complex that it resembles the highly nonlinear behavior described in Fig 2 (page 10-8). Film cooling behavior in the real world is strongly influenced by G_s because the maximum in Fig 2 is influenced by G_s in that this maximum is often observed at values of G_s/G_m about equal to one. Film cooling behavior in the real world is strongly influenced by s because, for a given flow rate, s determines the value of G_s. Figure 2 states that $\eta\{G_s\}$ and $\eta\{s\}$ are NOT described by constants and that they in fact resemble the functions illustrated in Figures 3 and 4 (page 10-8).

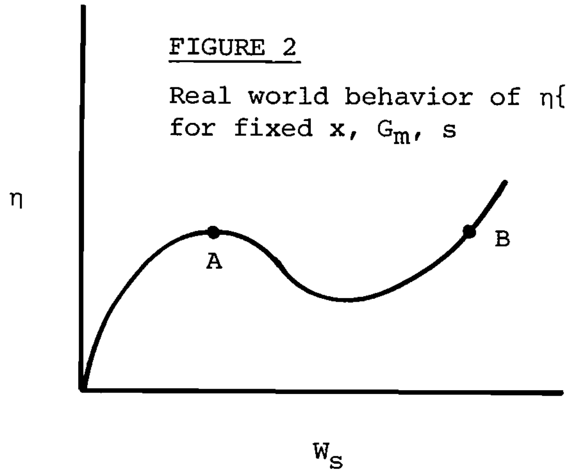


FIGURE 2

Real world behavior of $\eta\{W_s\}$ for fixed x, G_m, s

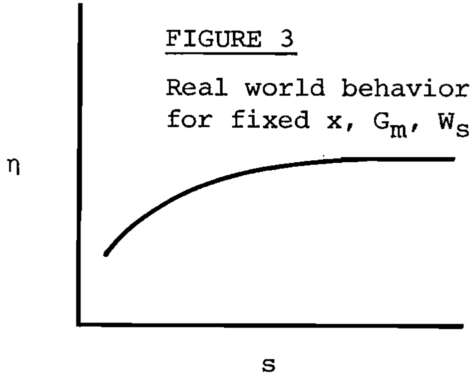


FIGURE 3

Real world behavior of $\eta\{s\}$ for fixed x, G_m, W_s

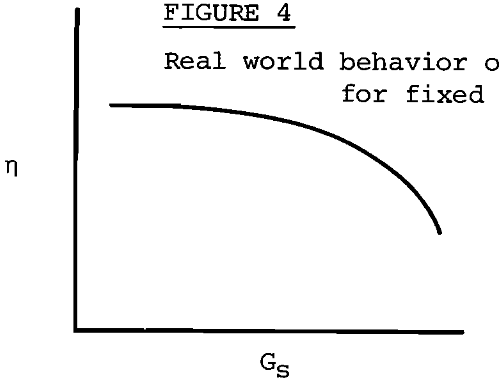


FIGURE 4

Real world behavior of $\eta\{G_s\}$ for fixed x, G_m, W_s

Figures 2 to 4 tell the designer what he needs to know to optimize the film cooling system design. They indicate that an optimum design requires a small value of G_s/G_m--a value something less than unity. In other words, for a given flow rate, the slot opening must be large enough to result in a value of G_s which will be less than or about equal to G_m. And if the slot is made smaller than this, the designer runs the risk of wasting a great deal of coolant because the system may be far from optimum. Note that points A and B in Fig 2 are at the same value of effectiveness but that point B requires about 3 TIMES AS MUCH COOLANT FLOW as point A.

Now let us look again at ref 1. Is there any mention of the fact that film cooling is often a highly nonlinear phenomenon? No. Is there any mention of the fact that film cooling behavior is well-behaved only at small values of "M"? No. Is there any mention of the fact that the dimensionless group (x/Ms) IN NO WAY depends on either M or s? No. Is there any mention of the fact that eq 1 actually says that M and s have NO effect on film cooling even though it seems to say that they have a very strong effect? No. Is there any mention of the fact that M and s DO influence film cooling behavior even though eq 1 says they do NOT? No. What does ref 1 say about the thermal design/ analysis of film cooling systems? It says

> In conclusion, it is possible at the present time to make reasonable estimates of the heat transfer performance with film-cooling . . . in that Fig 26 yields a good approximate value (of effectiveness).

And of course the point of this discussion is to illustrate that this old way of dealing with nonlinear phenomena leads first to correlations which bear no resemblance to reality and second to equipment designs which are far from optimum in the real world.

COPING WITH REGIMES

Correlations such as eq 1 are usually qualified by statements which restrict their use to M values less than about unity. (The authors of ref 1 do not qualify their analytical correlation, eq 1, or their graphical correlation, Fig 26. Perhaps this was the result of their having investigated/reported only the single value, M = .28.) For instance, Kays (2) presents Wieghardt's correlation

$$\eta = 21.8(x/Ms)^{-.8} \tag{10}$$

qualified by the requirements that "0.22 < M < 0.74 and x/s > 100". However, even when qualified in this way, the information is not altogether useful because it raises two questions in the mind of the designer/analyst:

1. Does the qualification merely describe the range of the investigation so that one might reasonably suppose it would apply over a wider range than indicated? Or was the investigation in fact of much wider scope and the indicated range was selected because, outside this narrow "regime", there was absolutely NO resemblance between correlation and reality?

2. If the correlation were used outside the indicated range, would the behavior be better or worse than predicted by the correlation? In other words, is the optimum design to be found inside or outside the regime?

Suppose a designer were confronted with a system in which it would be most convenient to design for a value of M = 1.6. What should he do? Should he use eq 10 even though this value is outside the indicated range/regime? Or should he do whatever is required in order to make M less than 0.74 and thus bring it into the proper regime, even though this would require the expenditure of considerable time/effort/money? Even though this entire effort might be for nothing in that the system might perform better at M = 1.6 than at M < 0.74? (Note that eq 10 and its qualifying statement give no hint whether the performance would be better or worse outside the indicated "regime".) In such a case, doesn't it seem likely that the designer/analyst would design for M = 1.6 and would still use eq 10, hoping that the as-built performance would be even better than that predicted by eq 10 because M was outside the indicated regime?

The point of this discussion is to state that it is better to qualify than not to qualify, but it is infinitely better to reveal than to conceal. When the range is stated in order to reveal the scope of the investigation, that is highly desirable. But when the range is carefully selected in order to create a "regime" which can be "correlated", that is highly undesirable because it conceals the fact that the correlation bears little or no resemblance to reality outside the regime. The invention of narrow regimes does NOT help reveal behavior--it CONCEALS behavior. And since the purpose of correlations is to reveal behavior, we must conclude that the casual invention of regimes does not serve a useful purpose--and must be abandoned in the new heat flow.

THE REAL WORLD USEFULNESS OF EQUATION 1

Reference 1 was presented at an ASME heat transfer conference in 1960. Following the presentation, S. Stephen Papell offered the following comment:

> I would like to add a word of caution concerning application of the correlation presented in this paper. The correlation obtained using low temperature and low velocity data is strictly empirical and, as such, its use cannot be extrapolated to higher temperatures and velocities.

In other words, even if the reference 1 investigation had dealt with film cooling over a wide range of coolant flow rates instead of only one flow rate, the results would not have been useful because the investigation was conducted at such "low temperature and low velocity" that the results "cannot be extrapolated to (the) higher temperatures and velocities" which are of concern in the real world of gas turbines and rocket nozzles. In other words, the correlation would even then have been of little use in designing/analyzing systems "for protecting a surface exposed to a high-temperature environment" such as are found "in gas turbines and in rocket nozzles . . . (and in the) leading edges of hypersonic aircraft".

In response to Papell's accurate comment, the reference 1 authors replied:

> The experimental research on film cooling carried out at this laboratory is INTENTIONALLY CONFINED to temperature differences which are sufficiently small so that the influence of property variations can be neglected.

In other words, they designed their experiments so that the influence of property variations could be neglected, although they fail to explain why this should be a more important consideration than obtaining results which have real world usefulness.

In summary, eq 1 is not useful at low temperatures and low velocities because it is based on an investigation which altogether avoided the highly nonlinear relationship between effectiveness and coolant flow rate. It is even less useful at real world conditions because it is based on an investigation at conditions which bear little resemblance to the conditions of concern in the real world. We must therefore conclude that eq 1 has little if any usefulness in dealing with real world problems.

THE REAL WORLD USEFULNESS OF THE EFFECTIVENESS CONCEPT

The effectiveness concept is a proportional concept in that its usefulness depends on proportional behavior. When one writes eq 1, he is really writing

$$(T_m - T_{aw}) = 16.9(x/Ms)^{-.8}(T_m - T_s) \qquad (11)$$

which of course says that

$$(T_m - T_{aw}) \propto (T_m - T_s) \qquad (12)$$

But if the proportionality in relation 12 does not hold, of what use is η? None. For the same reason that the h concept is of no use unless $q \propto \Delta T$, the η concept is of no use unless relation 12 holds. And so the question arises:

> Does relation 12 accurately describe the real world behavior of film cooling?

If the correct answer is "yes", then we may consider retaining the η concept. But if the answer is "no", then of course we will want to abandon the η concept.

The authors of ref 1 investigated different values of $(T_s - T_m)$ from about 10F to about 150F. In their Fig 24, they demonstrated that $\eta\{x\}$ for fixed values of M and s was essentially independent of $(T_s - T_m)$. This observed independence constitutes an indirect but rigorous demonstration that relation 12 applied to their experiment in the range 10 to 150 F. However, temperatures in gas turbines and rocket nozzles are such that the value of $(T_m - T_s)$ often exceeds 1000 F and thus the demonstrated proportionality from 0 to 150 F has little bearing on the real world problem and we must conclude that relation 12 may or may not apply to real world problems.

The authors of ref 1 present an indirect proof that rel 12 does NOT accurately describe film cooling behavior in the range of practical interest. In their Fig 26, they include data points obtained by Papell et al (3). With regard to these data, the authors provide the following explanatory note:

> Papell's data are for a large temperature difference between the free stream and injected air and have been corrected by $(T_s/T_m)^{.5}$ as suggested by Papell to agree with present results.

This explanation is the same as saying that η is NOT a useful concept. Since it was necessary to "correct" Papell's data because of the large values of $(T_m - T_s)$ employed in that investigation, the correction indicates that relation 12 does NOT apply at the values investigated by Papell. (If η were a useful concept, then relation 12 should also have applied to Papell's data and it should not have been necessary to "correct" his data in order to make them "agree" with the reference 1 results.)

THE REAL WORLD DESIGN/ANALYSIS OF FILM COOLING SYSTEMS

Whenever I point out that a particular correlation or a group of correlations bears no resemblance to real world behavior, I am usually met with the rebuttal:

> Yes, but we used those correlations to design the hardware and the hardware works just fine.

to which I generally reply that I have considerable real world experience and I know that most thermal hardware is designed by art and NOT by science. The first prototype of a new design might well be designed on the basis of eq 1, but as soon as the prototype fails in test, eq 1 is largely forgotten. If some film-cooled surface burned through, eq 1 would not be necessary to determine that better cooling is required. The problem is obviously that better cooling must be provided, but how should the hardware be modified in order to obtain the improved cooling in the most effective way? Should we increase the coolant flow by increasing the coolant supply pressure which in turn would increase G_s? Or should we also open up the slots in order to increase the coolant flow without increasing G_s (ie without increasing "M")? Eq 1 says that both methods would give the same result because the degree of improvement depends only on the amount of additional coolant and NOT on the manner in which the coolant flow rate is increased. Therefore there would be little point in opening up the slots and the designer/analyst would probably retain the same slot dimension used in the original design.

But suppose the surface that burned through was at point A in Fig 2 and the fix that was decided on was to increase W_s 50% by increasing the supply pressure and retaining the same slot geometry. How well would this fix work? It wouldn't work at all--it would be a disaster because Fig 2 indicates it would result in even poorer cooling and thus this second prototype would burn through even faster than the first! And suppose we decided to double the coolant flow rate in a third prototype, again without opening up the slots. How well would this third prototype work? Fig 2 indicates that it would burn through even faster than prototypes 1 and 2!!!

This trial-and-error method of design is simple to discuss, but when this method involves real people and real hardware and real time and real effort and real money, it is far from simple. And this trial-and-error method of equipment design/analysis is not something I have invented for the sake of argument. It is a method which is widely used in the real world. And this ineffective method is not the fault of poor design--it is not the fault of poor analysis. It is the fault of correlations which bear no resemblance to reality--it is the fault of power laws which conceal rather than reveal behavior--it is the fault of regimes which conceal rather than reveal behavior--it is the fault of dimensional groups which conceal rather than reveal behavior--it is the fault of dimensionless groups which conceal rather than reveal behavior--it is the fault of supposing rather than finding out.

And as soon as we abandon all these old way concepts which conceal rather than reveal behavior--as soon as we find out that film cooling behavior often resembles Fig 2--as soon as we find out that η and W_s are often negatively correlated at G_s/G_m values in excess of 1 as indicated in Fig 2, then it becomes a simple matter to cope with film cooling in an effective way. It becomes obvious that the above fix should have involved opening up the slots and increasing the coolant flow rate without increasing G_s and then the fix would have had the desired effect. The fix would have resulted in a cooler surface whereas the fix based on eq 1 resulted in an even hotter surface--an even poorer design in the real world.

BOILING

In the engineering world, the most important form of highly nonlinear heat flow is boiling. In Bk 1, we discussed the highly nonlinear character of $q\{\Delta T\}$ in pool boiling, but we largely avoided forced convection boiling. In reference 4, I discuss the highly nonlinear character of $q\{\Delta T\}$ in forced convection boiling. I have not included a similar discussion here because I would like the reader to refer to that article published more than a decade ago. That article is the seed for the new heat flow--the seed for the new engineering. Perhaps that article will cause the reader to wonder why I have neglected that seed for over a decade--why I have not continued to develop that seed in the literature. It is a point we will discuss in the next chapter.

DISCUSSION

The difference between the old and the new methods of dealing with highly nonlinear phenomena is accurately summarized in the following observations:

> The old methods are based on avoiding the real problem.
>
> The new methods are based on dealing with the real problem.

If the reader doubts that the old methods are based on avoiding the real problem, perhaps the following questions will remove the doubt:

> Doesn't the unquestioned use of power laws avoid the real problem of determining the functionality suggested by the data?
>
> Doesn't the invention of regimes, by fragmenting Natural phenomena into more or less linear segments, avoid the real problem of dealing with the nonlinear character of the phenomenon?
>
> Doesn't the invention of proportional concepts such as the effectiveness η together with the insistence on investigations which are "intentionally confined to temperature differences which are sufficiently small so that the influence of property variations can be neglected"--don't these two together avoid the real problem of dealing with the nonlinear character of the phenomenon?

In the new heat flow, we do NOT accept power laws unless they are suggested by the data--we do NOT accept the casual invention of regimes--we do NOT accept the invention of proportional concepts--we do NOT accept the assumption that proportional over a narrow range means proportional over a wide range. In the new heat flow, we deal with highly nonlinear phenomena by first abandoning all these old way methods which conceal rather than reveal Natural behavior. And as soon as we abandon these old way methods, it becomes a simple matter to deal with highly nonlinear phenomena in a highly effective way.

CONCLUSIONS

The old way methods of dealing with highly nonlinear phenomena must be abandoned. We must begin to actually deal with rather than avoid nonlinear behavior. This is the only path to an understanding of nonlinear phenomena--the only path to the effective design/analysis/construction/operation of equipment to handle nonlinear phenomena.

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CHAPTER 11 THE ENGINEERING LITERATURE

INTRODUCTION

There are a great many things wrong with today's (1975) engineering literature, but the most glaring error is its preoccupation with intellectual speculation and its almost total disregard for engineering science. Speculation has become so popular that there is hardly any room left over in the literature for engineering science. It requires only a brief encounter with today's engineering literature to recognize that it lacks an awareness of its real world purpose. It would seem that the purpose of the engineering literature is to dwell on intellectual speculation which defies comprehension and which holds little or no promise of any present or future practical or theoretical value. It would indeed seem that the purpose of the engineering literature has little or nothing to do with discovering and describing Natural phenomena--little or nothing to do with engineering science.

It is not my intent in this chapter to explain the topsy-turvy condition of today's engineering literature. But it is my purpose here to recommend several cures. Unfortunately, it seems more than likely that the small group which now controls the literature will not appreciate my recommended cures. Their opposition will of course make it quite difficult to bring about these improvements. But the improvements must come. The direct and indirect costs of a speculative engineering literature stagger the imagination. The engineering literature must not be permitted to continue in its present course--a course which has given us a literature whose main thrust is speculation and not useful engineering science--a course which has given us a literature in which, as noted by the Wright brothers,

> Truth and error (are) everywhere so intimately mixed as to undistinguishable.

MAKE ROOM FOR THE DATA

The first lesson one learns in any worthwhile undergraduate engineering laboratory is the importance of the data--the raw data--the numbers recorded in the laboratory while the experiment was actually in progress. Every undergraduate engineer learns that the data is the prime mover--the data is the thing which is more important than everything else together--the data holds everything together--the data is timeless--the data must be preserved. And every good undergraduate lab instructor makes it an ABSOLUTE REQUIREMENT that the raw data be included in the lab report--even if the raw data is recorded on a sheet of paper which looks like it has been sat on, stepped on, sprinkled with coffee, and used as an ash tray.

But as soon as we leave the background of the undergraduate laboratory--as soon as we become sophisticated--as soon as we enter the lofty world of doctoral theses and post doctoral theses and literature publications, we leave the data behind as though it were something which mattered only to undergraduates. As we have discussed before, the excellent undergraduate practice of stressing the importance of the data has not found its way into the engineering literature even though the data must be regarded as an ABSOLUTELY ESSENTIAL part of science.

If the literature is to begin concerning itself with engineering science, the data MUST become an integral part of the literature. There is no other way. It is a contradiction in terms to speak of science without data. There is no such thing.

There will of course be those who will resist the inclusion of the data in the literature on the basis that there is no room or that it would compromise the literature and cause it to become little more than an empirical handbook or that it is difficult to define precisely what is meant by the word "data" or that reporting the data would "waste" a great deal of space in the literature. But let us ignore these arguments on the grounds that they have no real substance and instead let us concern ourselves with a simple, effective way of bringing the data into the literature where it rightfully belongs. This highly desirable end can be accomplished quite simply by setting up and observing the following editorial requirements:

> The authors of all articles which purport to describe experimentally observed Natural phenomena MUST make the raw data available through a documentation institute prior to submitting the article for publication.
>
> All articles which purport to explain/describe/interpret/correlate experimentally observed Natural phenomena MUST include the reduced data in digital form unless the reduced data has already been published in digital form in a generally available reference.

I have discussed these requirements with several Journal editors who, without exception, failed to recognize that the above requirements/improvements were either necessary or desirable. Their response was generally that there is no room for the data in the literature. To which I generally replied that the data is of such paramount importance in science that it is necessary to make room for the data. Even if that involves throwing out something of no value.

There is only one way to bring about an engineering literature which deals with engineering science. That one way is by making room for the data in the literature.

Amen.

MAKE ROOM FOR PROGRESS/CHANGE

It is a truism that

All progress brings change.

It is not possible to be FOR progress and AGAINST change. In the same vein, 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 the imagination to recognize that science is the search for a better way. And this better way can be better ONLY if it is DIFFERENT--only if it brings change.

Science requires flexibility. Science requires a willingness/eagerness/desire to bring about progress/change. Therefore it is essential that the engineering science literature be administered in a way which ENcourages rather than DIScourages progress/change. This will require a great change because the present administrative methods are cleverly designed to guard against progress/change. In order to prove this, I am going to relate a true story which will provide the real world "data" in support of my contention. From this data, we will then try to determine the cure--ie the administrative changes which must be made in order to make room for progress/change in the literature.

By way of introduction to the story, the reader should turn to page 7-19 in Bk 1 and study Fig 8 for a moment. It is my contention that the data in that figure do NOT support the contention that q{ΔT} is highly nonlinear and therefore I accept the contention that q{ΔT} is essentially linear. If any reader concludes from Fig 8 that q{ΔT} is in fact highly nonlinear, he would do well to avoid the rest of this chapter because we lack a basis on which to communicate.

About a decade ago, I prepared an article dealing with the correct/incorrect usage of log log graph paper. The thrust of the article was that drawing straight

lines through data plotted on log log graph paper and measuring their slopes involves a number of unconscious assumptions, the principal of which is that the function passes through the point (0,0) if the line has positive slope. The difficulty is that, if the function does NOT pass through (0,0), then the analytic functionality inferred from the slope of the line will be in error. Since the article had general application, I wrote it in a general way and submitted it for publication in an engineering journal, confident that it would be published because its importance was well established by the widespread use of log log graphs in correlating experimental results. The editor of the journal--let us call him Editor B.--sent the manuscript to 3 so-called reviewers for a publication decision. (The name "reviewers" is a misnomer. They should be called by the name "previewers" because they actually preview articles prior to publication/rejection.)

The reviewers seemed not to understand the point of the article and objected to its general rather than specific treatment of the subject. For instance, Professor R. stated in his review dated 2/19/64:

> I would suggest that the author eliminate 99% of his present collection of work and 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.

Editor B. agreed with Professor R. that the article should be highly specific, so I rewrote it and used the data by Berenson (1) and the data by Cichelli and Bonilla (2). This time, the reviews were more favorable. Professor C. stated in his review of 4/64:

> 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 <u>Journal</u> 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 firmly recommend this paper for publication, and the author is to be commended for his fresh approach to boiling heat transfer . . . . .

Professor S. stated in his review of 4/2/64:

> 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. . . . .

A 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." (He altogether missed the point. I was not assuming a linear relationship--the data was describing itself as linear. I was merely reporting what the data said. And explaining why the data had been misinterpreted in the first place.)

On the basis of the favorable reviews, Editor B. accepted the article for publication. But this is not the end of the story--it is only the beginning. I submitted the article for presentation along with the information that it had already been accepted for publication in the journal edited by Editor B. (I felt that the article would more likely be accepted for presentation if it was known that it had already been accepted for publication.) To my surprise and chagrin, the paper was rejected and I was not to be allowed to present it at the technical meeting. The rejection notice came in the form of a letter from the Papers Chairman, a Dr. M. whose address was listed as Argonne National Laboratory!!! In his letter of 8/6/64, Dr. M. stated:

> . . . 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 . . . and I have taken special care in the case of your paper to insure that we are correct in its rejection. . . (The) points you have so carefully selected do not prove your point since . . . . Although the paper can be rejected on purely technical grounds the incoherence of the arguments presented and the overly wordy and personal style would be additional grounds. I suggest you get assistance in preparation of future manuscripts. . . .
>
> 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.
>
> copies to Journal Editor B.
> Journal Editor K.

The disappointment I felt at the above rejection was nothing compared to the one I was about to receive from Editor B. and which was to give me an inside view of the manner in which the literature is administered. In his letter of 8/17/64, Editor B. stated:

> 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, . . .

Editor B. explained that his "plan" was to send the negative comments to the highly favorable reviewer (Professor C.) in order to give him a second chance to review my article. Now if the reader will again read Professor C's. review, I think he will agree that it has the sound of a freely given, honest, unbiased opinion. Moreover, since it mentions his own first hand experience with the subject, it does not sound as though a few discouraging words would change his mind. Therefore, I was confident that the article would indeed be published in the end.

While Editor B. was waiting for Professor C's. reply, we discussed the article by phone and our conversation led to the following letter from Editor B. to Dr. M.

> (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) R. to show a non-linear one.

It is perhaps not necessary to say that Dr. M. declined the invitation to select the data, perhaps on the grounds that it would serve no useful purpose (for him). At any rate, Editor B. never mentioned this subject again. Perhaps Dr. M. knew I had analyzed all the digital data I could find in the literature.

While we were waiting for Professor C's. re-review, I sent the following letter to Dr. M. in reply to his earlier letter:

> Thank you for your letter of August 6 in which you express the opinion that I am both incompetent and dishonest. . . I sense that you have written your letter for the benefit of (Editors) B. and K. 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.

To which Dr. M. replied in his letter of 8/25/64:

> . . . The purpose of informing Professors B. and K. (editors B. and K. were also Professors) 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. . .

I believe what Dr. M. actually meant was that all the "competent" members of the professional community were employed at Argonne National Laboratory and that they were getting tired of reviewing my articles. And if the reader doubts that this was his meaning, the following letter written by The Argonne Seven to the editor of Nucleonics in response to my first published article should dispel any doubt:

> 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 either be a hoax, or that the paper reviewing procedures followed by NUCLEONICS are in need of reevaluation.
>
> by F/H/H/L/M/R/S
> (The Argonne Seven)

The Managing Editor of Nucleonics sent me a copy of this letter/petition/complaint along with his letter dated 9/9/64 which stated: "Our plan at this point is to publish the Argonne letter in the version we enclose. ." Apparently their plans changed because the published version of the Argonne letter was quite innocuous and made no mention of hoax or reviewing procedures. Since I was not informed of the change in plan, I replied to the word hoax and thus my reply seemed unreasonable. In any event, the Argonne letter to which I replied has now been published and the circle has been closed.

Returning to the main story, the day of reckoning finally arrived--Professor C's. reappraisal was completed--he had now formulated his second "thoughts" on the desirability of publishing my "most stimulating and exciting article"--my article in which "the truth of the author's contentions is unquestionable"--my article in which "the author has done a great service"--my article which had prompted his first thought:

> 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 C's. first thought compare with his second thought? How did his first thought based on his own experience compare with his second thought based on someone else's opinion? How did his first unbiased thought compare with his second thought which was biased by the knowledge that a "responsible person" did not want to see my article published? Professor C's. re-review holds the answer. His re-review was contained in a letter to Editor B. dated 8/25/64 which stated:

> Thank you very much for your letter of August 19 and for the OPPORTUNITY of REAPPRAISING the subject paper. . .
>
> I believe Professor W. has summarized my feelings best. (Editor B. had led me to believe that Professor W. was the "responsible person" who had complained. Note that Professor C. is now in complete agreement with Professor W. and that Professor W. has the amazing ability to describe Professor C's. feelings even better than Professor C feels he can.) . . .
>
> One of my graduate students made an excellent suggestion in that a note would be quite appropriate to point out the rather small bit of technical information contained (in the article). This may be an excellent way out for you in that you are in an excellent position to suggest to Adiutori that he should revise his paper to form a communication for the Journal. The burden would then be upon him; and if he choses to ignore your recommendation, you would be at liberty to dismiss the paper.

I will not comment on the above re-review. Just reading it over again makes me vomit. In any event, the article which was accepted for publication ELEVEN YEARS AGO has NOT YET been published in the Journal. But when I become a "responsible person", I am going to try again.

I have related the above true story for only one reason. To establish the manner in which the engineering literature is administered in the real world. This story is not an isolated instance--it is merely one of many examples I have accumulated over the past decade. These examples are the proof--the real world data that provides the real world demonstration that

> The present methods of administering the literature are cleverly designed to PREVENT progress/change.

And since the true purpose of the literature--the true purpose of science--is to bring about progress/change, we must abandon the present methods in order to make possible the desired progress/change. Progress/change ALWAYS means that we must discard something old and less useful in favor of something new and more useful. And that is where the difficulty comes in. Whenever we discard something old, we are going to find "people with axes to grind" who are anxious to retain the old way and who have little interest in a new and better way because it means that THEIR way must be abandoned. The present methods of administering the literature ensure that nothing will be abandoned which has the backing of a "responsible person". Not as long as other "responsible persons" administer the literature. Not as long as "the members of the professional community competent and honest enough to review the work of others" are permitted to preview every article before it is published, making certain that it does not contain any progress/change which might offend some "responsible person". Not as long as we continue to support a system which permits/encourages the Argonne Sevens and the Dr. M's. and the "responsible persons" to intimidate editors and force them to "reevaluate" their "paper reviewing procedures" in order to make certain that no progress/change can find its way into the literature without their knowledge/approval/blessing/condemnation.

And so we come at last to the cure--to the new method of administering the engineering literature in a way which will make room for progress/change even against the opposition of a "responsible person":

> The new method must do away with the need for editors. If there are no editors, then they can not be intimidated--they can not be prevented from publishing articles which contain progress/change which would/might offend a "responsible person".
>
> The new method must do away with the need for reviewers/previewers. If there are no reviewers/previewers, then they can not prevent the publication of articles which contain progress/change which would/might offend a "responsible person".

This new method is very simple. It requires merely that articles be selected for publication by a RANDOM SELECTION PROCEDURE--RANDOM AND OPEN. It is my conclusion, based on a decade of research into the present methods of administering the literature, that a random selection of articles together with the abandonment of editors and reviewers would be infinitely better than the present methods. Random selection would make room for progress/change and it would have several very important side effects. It would reduce the immense volume of "literature" which is published each year and it would improve the quality of the literature as a whole. No longer would it be an honor to have hundreds of articles published. No longer would it "benefit" researchers to publish their results one data point per article in order to wring the maximum number of articles from a given experimental apparatus.

With random selection, the honor will lie in quality rather than number. Peer pressure will guard against the repeated publication of essentially the same work. (The present methods place the burden of policing for repetition on the reviewer and therefore make possible the repeated publication of the same work.) Peer pressure will guard against publishing experimental results one data point at a time. Peer pressure will ensure that what is published is useful in either a practical or a theoretical way. Peer pressure will lead to a literature which deals with engineering science and which has real world value.

The present methods of administering the literature are absurd--absurd because they place life-and-death decisions on publication in the hands of a very small group of "responsible persons" who are in fact responsible to no one--whose primary interest in the literature has nothing to do with promoting progress/change--nothing to do with science. Peer pressure is much more likely to favor progress/change--much more likely to favor science. That is why peer pressure will be much more effective than the pressure of a few "responsible persons". And even if I am wrong and if peer pressure should prove not to be a good substitute for the pressure of "responsible persons", one thing is certain:

> Peer pressure can't possibly do a worse job than has been done by the pressure of the "responsible persons".

Edward Jenner accomplished the greatest scientific achievement the world has ever seen. He single-handedly developed the vaccine for smallpox at a time when medicine could hardly be considered a science and his methods were later used by Pasteur and other vaccine developers. During the decade or so that Jenner developed the vaccine, he was a respected member of the Royal Society of London and his work dealing with the behavior of cuckoo birds had been published in the Transactions of the Royal Society. However, when he attempted to publish his work dealing with the vaccine for smallpox, he found that his work was not "suitable" for publication in the Transactions. Why not? Because the "responsible persons" were offended by his results which suggested there was a parallel between cow sickness and human sickness--which suggested that cows could be useful in the real world prevention of disease and death. And so Jenner could not arrange the normal publication of his work--the "responsible persons" would not permit it. And so Jenner gave up trying to deal with the "responsible persons". He went to London and paid to have 500 of his manuscripts printed and then he distributed them without the help of the Royal Society--in spite of the "responsible persons". And, as we all know, this is a real world story which has a happy ending because the "irresponsible persons" immediately grasped the significance of his results and were more than happy to receive the vaccine--and of course they paid no more attention to the objections of the "responsible persons" than Jenner had--and for that reason many of us are alive today who would never have seen our first sunrise.

It is only by making room for the data and by making room for progress/change that the literature will begin to approach its potential. And that is why we must adopt the "new" method of administering the literature.

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APPENDIX 1

SYMBOLS

a	acceleration
C	heat capacity
D	diameter
E_o	zero error
F	force
G	mass flow rate or gain
h	heat transfer coefficient
I	current
k	thermal conductivity
L	length
m	mass
M	G_s/G_m
Nu	Nusselt number
P	pressure
Pr	Prandtl number
q	heat flow rate per unit area
Q	heat flow rate
R	resistance
Re	Reynolds number
s	slot height
t	time
T	temperature
V	velocity or volts
W	weight or weight flow rate
x	distance

SYMBOLS cont.

Δ	difference between values
ε	emissivity
η	film cooling effectiveness
μ	viscosity
ρ	density

SUBSCRIPTS

amb	ambient
ci	cold stream in
co	cold stream out
corr	corrected or correction
DF	driving force
est	estimate
hi	hot stream in
ho	hot stream out
i	in
ind	indicated
m	mainstream
o	out
p	pressure
s	slot
trans	transformed