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EXPERIMENTATION, ERRORS, AND UNCERTAINTY

When the word *experimentation* is encountered, most of us immediately envision someone in a laboratory “taking data.” This idea has been fostered over many decades by portrayals in periodicals, television shows, and movies of an engineer or scientist in a white lab coat writing on a clipboard while surrounded by the piping and gauges in a refinery or by an impressive complexity of laboratory glassware. In recent years, the location is often a control room filled with computerized data acquisition equipment with lights blinking on the racks and panels. To some extent, the manner in which laboratory classes are typically implemented in university curricula also reinforces this idea. Students often encounter most instruction in experimentation as demonstration experiments that are already set up when the students walk into the laboratory. Data are often taken under the pressure of time, and much of the interpretation of the data and the reporting of results is spent on trying to rationalize what went wrong and what the results “would have shown if . . .”

Experimentation is not just data taking. Any engineer or scientist who subscribes to the widely held but erroneous belief that experimentation is making measurements in the laboratory will be a failure as an experimentalist. The actual data-taking portion of a well-run experimental program generally constitutes a small percentage of the total time and effort expended. In this book we examine and discuss the steps and techniques involved in a logical, thorough approach to the subject of experimentation.

1-1 EXPERIMENTATION

1-1.1 Why Is Experimentation Necessary?

Why are experiments necessary? Why do we need to study the subject of experimentation? The experiments run in science and engineering courses demonstrate physical principles and processes, but once these demonstrations are made and their lessons taken to heart, why bother with experiments? With the laws of physics we know, with the sophisticated analytical solution methods we study, with the increasing knowledge of numerical solution techniques, and with the awesome computing power available, is there any longer a need for experimentation in the real world?

These are fair questions to ask. To address them, it is instructive to consider Figure 1.1, which illustrates a typical *analytical* approach to finding a solution to a physical problem. Experimental information is almost always required at one or more stages of the solution process, even when an analytical approach is used. Sometimes experimental results are necessary before realistic assumptions and idealizations can be made so that a mathematical model of the real-world process can be formulated using the basic laws of physics. In addition, experimentally determined information is generally present in the form of physical property values and the auxiliary equations (e.g., equations of state) necessary for obtaining a solution. So we see that even in situations in which the solution approach is analytical (or numerical) information from experiments is included in the solution process.

From a more general perspective, experimentation lies at the very foundations of science and engineering. *Webster's* [1] defines science as “systematized

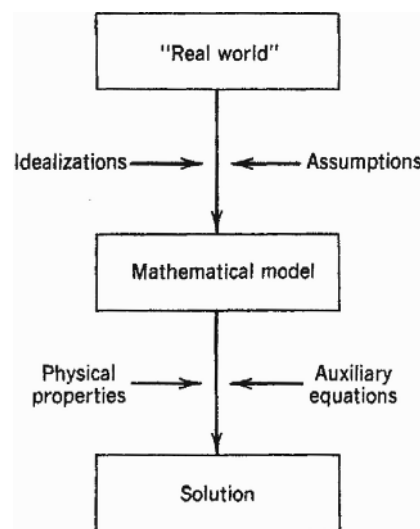


Figure 1.1 Analytical approach to solution of a problem.



knowledge derived from observation, study, and experimentation carried on in order to determine the nature or principles of what is being studied.” In discussing the *scientific method*, Shortley and Williams [2, p. 2] state: “The scientific method is the systematic attempt to construct theories that correlate wide groups of observed facts and are capable of predicting the results of future observations. Such theories are tested by controlled experimentation and are accepted only so long as they are consistent with all observed facts.”

In many systems and processes of scientific and engineering interest, the geometry, boundary conditions, and physical phenomena are so complex that it is beyond our present technical capability to formulate satisfactory analytical or numerical models and approaches. In these cases, experimentation is necessary to define the behavior of the systems and/or processes (i.e., to find a solution to the problem).

1-1.2 Degree of Goodness and Uncertainty Analysis

If we are using property data or other experimentally determined information in an analytical solution, we should certainly consider how “good” the experimental information is. Similarly, anyone comparing results of a mathematical model with experimental data (and perhaps also with the results of other mathematical models) should certainly consider the *degree of goodness* of the data when drawing conclusions based on the comparisons. This situation is illustrated in Figure 1.2. In Figure 1.2a the results of two different mathematical models are compared with each other and with a set of experimental data. The authors of the two models might have a fine time arguing over which model compares better with the data. In Figure 1.2b, the same information is presented, but a range representing the uncertainty (likely amount of error) in the experimental value of Y has been plotted for each data point. It is immediately obvious that once the degree of goodness of the Y value is taken into consideration it is fruitless to argue for the validity of one model over another based only on how well the model results match the data. The “noise level” established by the data uncertainty effectively sets the resolution at which such comparisons can be made.

We will discuss such “validation” comparisons between simulation results and experimental results in considerable detail as we proceed. At this point, we will note that the experimental values of X will also contain errors, and so an uncertainty should also be associated with X . In addition, the simulation result also has uncertainty arising from modeling errors, errors in the inputs to the model, and possibly errors from the algorithms used to numerically solve the simulation equations.

From this example, one might conclude that even a person with no ambition to become an experimentalist needs an appreciation of the experimental process and the factors that influence the degree of goodness of experimental data and results from simulations.

Whenever the experimental approach is to be used to answer a question or to find the solution to a problem, the question of how good the results will be



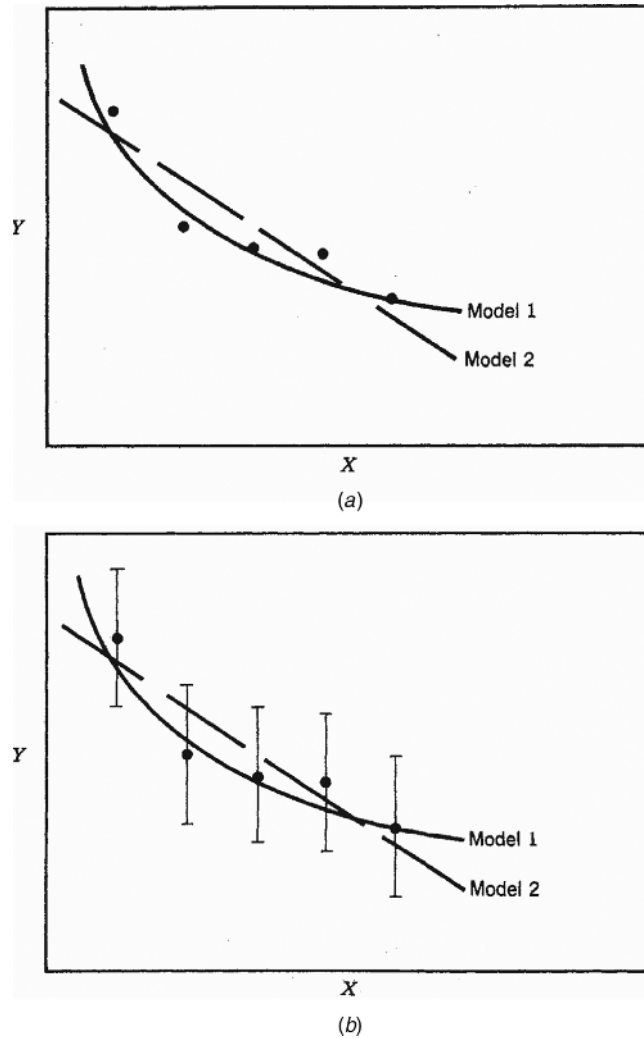


Figure 1.2 Comparison of model results with experimental data (a) without and (b) with consideration of uncertainty in Y .

should be considered long before an experimental apparatus is constructed and data are taken. If the answer or solution must be known within, say, 5% for it to be useful to us, it would make no sense to spend the time and money to perform the experiment only to find that the probable amount of error in the results was considerably more than 5%.

In this book we use the concept of *uncertainty* to describe the degree of goodness of a measurement, experimental result, or analytical (simulation) result. Schenck [3, p.7] quotes S. J. Kline as defining an experimental uncertainty as “what we think the error would be if we could and did measure it by calibration.”

An error δ is a quantity that has a particular sign and magnitude, and a specific error δ_i is the difference caused by error source i between a quantity (measured or simulated) and its true value. As we will discuss in detail later, it is generally assumed that each error whose sign and magnitude are known has been removed by correction. Any remaining error is thus of unknown sign and magnitude,¹ and an uncertainty u is estimated with the idea that $\pm u$ characterizes the range containing δ .

Uncertainty u is thus an estimate: a $\pm u$ interval is an estimate of a range within which we believe the actual (but unknown) value of an error δ lies. This is illustrated in Figure 1.3, which shows an uncertainty interval $\pm u_d$ that contains the error δ_d whose actual sign and magnitude are unknown.

Uncertainty analysis (the analysis of the uncertainties in experimental measurements and in experimental and simulation results) is a powerful tool. This is particularly true when it is used in the planning and design of experiments. As we will see in Chapter 4, there are realistic, practical cases in which all the measurements in an experiment can be made with 1% uncertainty yet the uncertainty in the final experimental result will be greater than 50%. Uncertainty analysis, when used in the initial planning phase of an experiment, can identify such situations and save the experimentalist much time, money, and embarrassment.

1-1.3 Experimentation and Validation of Simulations

Over the past several decades, advances in computing power, modeling approaches, and numerical solution algorithms have increased the ability of the scientific and engineering community to simulate real-world processes to the point that it is realistic for predictions from surprisingly detailed simulations to be used to replace much of the experimentation that was previously necessary to develop designs for new systems and bring them to the market. The new systems to which we refer cover the gamut from simple mechanical and structural devices to rocket engine injectors to commercial aircraft to military weapons systems to nuclear power systems.

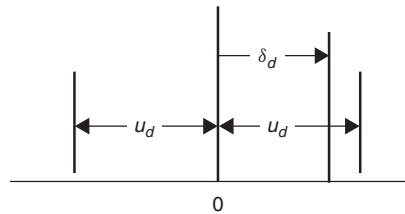


Figure 1.3 An uncertainty u defines an interval that is estimated to contain the actual value of an error of unknown sign and magnitude.

¹There are asymmetric errors that are more likely to (or are certain to) have one sign rather than the other. Treatment of these by either “zero centering” or estimating asymmetric uncertainties is discussed in Chapter 5 and Appendix E.



In the past, it was necessary to test (experimentally determine) subsystem and system performance at numerous set points covering the expected domain of operation of the system. For large, complex systems the required testing program can be prohibitively expensive, if not outright impossible, with available finite resources. The current approach seeks to replace some or much of the experimentation with (cheaper) simulation results that have been validated with experimental results at selected set points—but to do this with confidence one must know “how good” the predictions are at the selected set points. This has led to the emergence of the field called verification and validation (V&V) of simulations (e.g., models, codes).

The verification part refers to application of approaches to determine that the algorithms solve the equations in the model correctly and to estimate the numerical uncertainty if the equations are discretized as, for example, in the finite-difference, finite-element, and finite-volume approaches used in computational mechanics. *Verification* addresses the question of whether the equations are solved correctly but does not address the question of how well the equations represent the real world. *Validation* is the process of determining the degree to which a model is an accurate representation of the real world—it addresses the question of how good the predictions are.

Verification is a necessary component of the validation process and will be described briefly with references cited to guide the reader who desires more detail, but more than that is beyond the scope of what we want to cover in this book. Since experimentation and the uncertainties in experimental results and in simulation results are central issues in validation, the details of validation *are* covered in this book. Basic ideas and concepts are developed as they arise naturally in the discussion of experimental uncertainty analysis—for example, estimating the uncertainty in the simulation result due to the uncertainties in the simulation inputs is discussed in Section 4-8. The application of the ideas and concepts in validation are covered in Chapter 6 with detailed discussion and examples.

1-2 EXPERIMENTAL APPROACH

1-2.1 Questions to Be Considered

When an experimental approach is to be used to find a solution to a problem, many questions must be considered. Among these are the following:

1. What question are we trying to answer? (What is the problem?)
2. How accurately do we need to know the answer? (How is the answer to be used?)
3. What physical principles are involved? (What physical laws govern the situation?)
4. What experiment or set of experiments might provide the answer?



5. What variables must be controlled? How well?
6. What quantities must be measured? How accurately?
7. What instrumentation is to be used?
8. How are the data to be acquired, conditioned, and stored?
9. How many data points must be taken? In what order?
10. Can the requirements be satisfied within the budget and time constraints?
11. What techniques of data analysis should be used?
12. What is the most effective and revealing way to present the data?
13. What unanticipated questions are raised by the data?
14. In what manner should the data and results be reported?

Although by no means all-inclusive, this list does indicate the range of factors that must be considered by the experimentalist. This might seem to be a discouraging and somewhat overwhelming list, but it need not be. With the aid of uncertainty analysis and a logical, thorough approach in each phase of an experimental program, the apparent complexities often can be reduced and the chances of achieving a successful conclusion enhanced.

A key point is to avoid becoming so immersed in the many details that must be considered that the overall objective of the experiment is forgotten. This statement may sound trite, but it is true nonetheless. We perform an experiment to find the answer to a question. We need to know the answer within some uncertainty, the magnitude of which is usually determined by the intended use of the answer. Uncertainty analysis is a tool that we use to make decisions in each phase of an experiment, always keeping in mind the desired result and uncertainty. Properly applied, this approach will guide us past the pitfalls that are usually not at all obvious and will enable us to obtain an answer with an acceptable uncertainty.

1-2.2 Phases of Experimental Program

There are numerous ways that a general experimental program can be divided into different components or phases. For our discussions in this book, we consider the experimental phases as planning, design, construction, debugging, execution, data analysis, and reporting of results. There are not sharp divisions between these phases—in fact, there is generally overlap and sometimes several phases will be ongoing simultaneously (as when something discovered during debugging leads to a design change and additional construction on the apparatus).

In the *planning phase* we consider and evaluate the various approaches that might be used to find an answer to the question being addressed. This is sometimes referred to as the *preliminary design phase*.

In the *design phase* we use the information found in the planning phase to specify the instrumentation needed and the details of the configuration of the experimental apparatus. The test plan is identified and decisions made on the ranges of conditions to be run, the data to be taken, the order in which the runs will be made, and so on.

During the *construction phase*, the individual components are assembled into the overall experimental apparatus, and necessary instrument calibrations are performed.

In the *debugging phase*, the initial runs using the apparatus are made and the unanticipated problems (which must always be expected!) are addressed. Often, results obtained in the debugging phase will lead to some redesign and changes in the construction and/or operation of the experimental apparatus. At the completion of the debugging phase, the experimentalist should be confident that the operation of the apparatus and the factors influencing the uncertainty in the results are well understood.

During the *execution phase*, the experimental runs are made and the data are acquired, recorded, and stored. Often, the operation of the apparatus is monitored using checks that were designed into the system to guard against unnoticed and unwanted changes in the apparatus or operating conditions.

During the *data analysis phase*, the data are analyzed to determine the answer to the original question or the solution to the problem being investigated.

In the *reporting phase*, the data and conclusions should be presented in a form that will maximize the usefulness of the experimental results.

In the chapters that follow we discuss a logical approach for each of these phases. We will find that the use of uncertainty analysis and related techniques (e.g., balance checks) will help to ensure a maximum return for the time, effort, and financial resources invested.

1-3 BASIC CONCEPTS AND DEFINITIONS

There is no such thing as a perfect measurement. All measurements of a variable contain inaccuracies. Because it is important to have an understanding of these inaccuracies if we are to perform experiments (use the experimental approach to answer a question) or if we are simply to use values that have been determined experimentally, we must carefully define the concepts involved.

1-3.1 Errors and Uncertainties

Consider a variable X in a process that is considered to be steady so that its true value (X_{true}) is constant. Measurements of the variable are influenced by a number of elemental error sources—such as the errors in the standard used for calibration and from an imperfect calibration process; errors caused by variations in ambient temperature, humidity, pressure, vibrations, electromagnetic influences; unsteadiness in the “steady-state” phenomenon being measured; errors due to undesired interactions of the transducer with the environment; errors due to imperfect installation of the transducer; and others. As an example, suppose that a measurement system is used to make N successive measurements of X and that the measurements are influenced by five significant error sources, as shown in Figure 1.4.

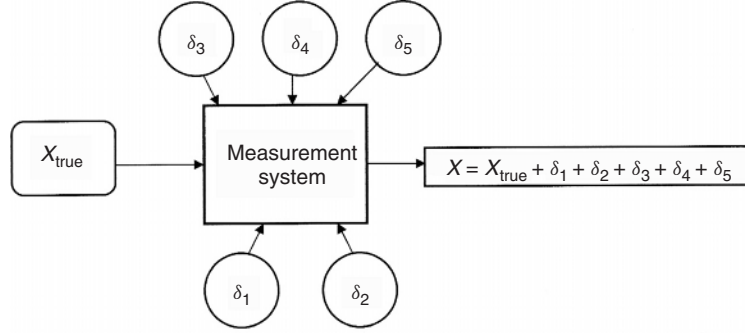


Figure 1.4 Measurement of a variable influenced by five error sources.

The first three of those measurements are given by

$$\begin{aligned}
 X_1 &= X_{\text{true}} + (\delta_1)_1 + (\delta_2)_1 + (\delta_3)_1 + (\delta_4)_1 + (\delta_5)_1 \\
 X_2 &= X_{\text{true}} + (\delta_1)_2 + (\delta_2)_2 + (\delta_3)_2 + (\delta_4)_2 + (\delta_5)_2 \\
 X_3 &= X_{\text{true}} + (\delta_1)_3 + (\delta_2)_3 + (\delta_3)_3 + (\delta_4)_3 + (\delta_5)_3
 \end{aligned} \tag{1.1}$$

where δ_1 is the value of the error from the first source, δ_2 the value of the error from the second source, and so on. Each of the measurements X_1 , X_2 , and X_3 has a different value since errors from some of the sources vary during the period when measurements are taken and so are different for each measurement while others do not vary and so are the same for each measurement. Using traditional nomenclature, we assign the symbol β (beta) to designate an error that does not vary during the measurement period and the symbol ϵ (epsilon) to designate an error that does vary during the measurement period. For this example, we will assume that the errors from sources 1 and 2 do not vary and the errors from sources 3, 4, and 5 do vary, so that Eqs. (1.1) can be written

$$\begin{aligned}
 X_1 &= X_{\text{true}} + \beta_1 + \beta_2 + (\epsilon_3)_1 + (\epsilon_4)_1 + (\epsilon_5)_1 \\
 X_2 &= X_{\text{true}} + \beta_1 + \beta_2 + (\epsilon_3)_2 + (\epsilon_4)_2 + (\epsilon_5)_2 \\
 X_3 &= X_{\text{true}} + \beta_1 + \beta_2 + (\epsilon_3)_3 + (\epsilon_4)_3 + (\epsilon_5)_3
 \end{aligned} \tag{1.2}$$

Since just by looking at the measured values we cannot distinguish between β_1 and β_2 or among ϵ_1 , ϵ_2 , and ϵ_3 , Eq. (1.3) describes what we actually have,

$$X_1 = X_{\text{true}} + \beta + (\epsilon)_1 \quad X_2 = X_{\text{true}} + \beta + (\epsilon)_2 \quad X_3 = X_{\text{true}} + \beta + (\epsilon)_3 \tag{1.3}$$

where now

$$\beta = \beta_1 + \beta_2 \quad \epsilon = \epsilon_3 + \epsilon_4 + \epsilon_5 \tag{1.4}$$

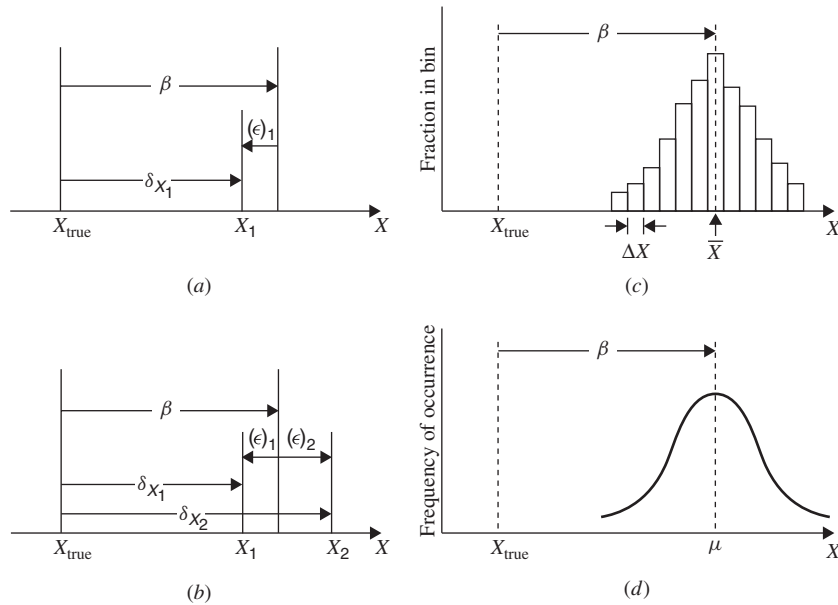


Figure 1.5 Effect of errors on successive measurements of a variable X .

This process of making successive measurements of X is shown schematically in Figure 1.5. In Figure 1.5a, the first measurement X_1 is shown. The difference between the measured value and the true value is the total error δ_{X_1} , which is the sum of the systematic error β (the combination of all of the errors from the systematic elemental error sources) and the random error $(\epsilon)_1$ (the combination at the time X_1 is measured of all of the errors from the error sources that vary during the period that our N measurements are taken). In Figure 1.5b the second measurement is also shown, and of course the total error δ_{X_2} differs from δ_{X_1} because the random error ϵ is different for each measurement.

If we continued to acquire additional measurements, we could plot a *histogram*, which presents the fraction of the N total measurements with values between X and $X + \Delta X$, $X + \Delta X$ and $X + 2\Delta X$, $X + 2\Delta X$ and $X + 3\Delta X$, and so on, versus X , where ΔX is the *bin width*. This is shown schematically in Figure 1.5c and allows us to view the *distribution* of the total of N measured values. This distribution of the *sample population* of N measurements tends to have a larger number of the measured values near the mean of the sample and a decreasing number of measured values as one moves away from the mean. As will be discussed in detail in Chapter 2, a *mean value* \bar{X} can be calculated, as can a *standard deviation* s , which is an indicator of the width of the distribution of the X values (the amount of “scatter” of the measurements caused by the errors from the elemental sources that varied during the measurement period).

As the number of measurements approached infinity, the *parent population distribution* would likely appear as shown in Figure 1.5d, with the mean μ

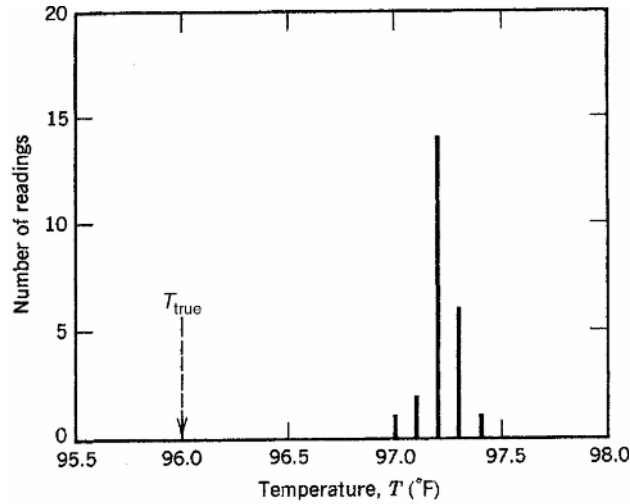


Figure 1.6 Histogram of temperatures read from a thermometer by 24 students.

offset from X_{true} by β , the combination of all of the systematic errors. Of course, we never have an infinite number of measurements, but conceptually the idea of the parent population distribution is very useful to us. The statistics of sample and parent distributions are discussed in Chapter 2.

An example of this behavior exhibited by a real set of measurements is shown in Figure 1.6. A thermometer immersed in an insulated container of water was read independently by 24 of our students to the nearest tenth of a degree Fahrenheit. Unknown to the students, the thermometer was biased (“read high”) by a little over a degree, and the “true” temperature of the water was about 96.0°F . The temperatures read by the students are distributed around an average value of about 97.2°F and are biased (offset) from the true value of 96.0°F .

With such a data sample, what we would like to do is use information from the sample to specify some range ($X_{\text{best}} \pm u_X$) within which we think X_{true} falls. Generally we take X_{best} to be equal to the average value of the N measurements (or to the single measured value X if $N = 1$). The uncertainty u_X is an estimate of the interval ($\pm u_X$) that likely contains the magnitude of the combination of all of the errors affecting the measured value X . Look back at the first measurement of X illustrated in Figure 1.5a and imagine that it is influenced by five significant error sources as in Figure 1.4. Then, recalling Eq. (1.2), the expression for the first measured value X_1 is given by

$$X_1 = X_{\text{true}} + \beta_1 + \beta_2 + (\epsilon_3)_1 + (\epsilon_4)_1 + (\epsilon_5)_1 \quad (1.5)$$

To associate an uncertainty with a measured X value, we need to have elemental uncertainty estimates for all of the elemental error sources. That is, u_1 is an uncertainty that defines an interval ($\pm u_1$) within which we think the value of

β_1 falls, while u_3 is an uncertainty that defines an interval ($\pm u_3$) within which we think the value of ϵ_3 falls.

Using the concepts and procedures in the ISO *Guide to the Expression of Uncertainty in Measurement* [4], a *standard uncertainty* (u) is defined as an estimate of the standard deviation of the parent population from which a particular elemental error originates. Then u_X is found from the combination of all of the elemental standard uncertainties as

$$u_X = (u_1^2 + u_2^2 + u_3^2 + u_4^2 + u_5^2)^{1/2} \quad (1.6)$$

For N measurements of X , the standard deviation s_X of the sample distribution shown in Figure 1.5c can be calculated as

$$s_X = \left[\frac{1}{N-1} \sum_{i=1}^N (X_i - \bar{X})^2 \right]^{1/2} \quad (1.7)$$

where the mean value of X is calculated from

$$\bar{X} = \frac{1}{N} \sum_{i=1}^N X_i \quad (1.8)$$

How can we determine which significant error sources' influences are included in s_X and which ones are not? There is only one apparent answer—we must determine which of the elemental error sources did not vary during the measurement period and thus produced errors that were the same in each measurement. These are the *systematic* error sources, and their influence is not included in s_X . The influences of all of the elemental error sources that varied during the measurement period (whether one knows the number of them or not) are included in s_X .

To understand and to take into account the effects of all of the significant error sources, then, we must identify two categories—the *systematic* sources whose effects are not included in s_X and the so-called *random* sources that varied during the measurement period and whose effects are included in s_X . (Assignment of an error source to the random category does not imply that the variation in the errors from that source is truly random. As will be discussed later, the *combination* of the errors from the different “random” sources tends to have a random behavior.)

In the case being considered, s_X reflects the contributions from the variable errors from sources 3, 4, and 5 and is related to the standard uncertainties from those elemental sources through

$$s_X = (u_3^2 + u_4^2 + u_5^2)^{1/2} \quad (1.9)$$

and so Eq. (1.6) becomes

$$u_X = (u_1^2 + u_2^2 + s_X^2)^{1/2} \quad (1.10)$$

This leaves the standard uncertainties for the systematic error sources to be estimated before we can determine the standard uncertainty u_X to associate with the measured variable X . In this book, as in the American National Standards Institute/American Society of Mechanical Engineers (ANSI/ASME) standard PTC19.1-2005, *Test Uncertainty* [5], the systematic standard uncertainties are designated with the symbol b_i , which is understood to be an estimate of the standard deviation of the distribution of the parent population from which a particular systematic error β_i originates. (This b nomenclature has its origin in the decades-past use of the word *bias*, which has been replaced in current usage with the word *systematic*.)

Upon first encountering the idea of a “parent population” for a systematic error, the idea itself might seem to be self-contradictory. A simple example serves to put the meaning into perspective, however. Imagine that we plan to use a Coleman–Steele model 22 voltmeter to record a measurement system output of 4.360 V and postulate that there are no sources of random error or unsteadiness. We enter a room in which there are 1000 model 22 voltmeters, and we choose the 14th one. In a perfect calibration apparatus we input 4.360 V into the voltmeter and its output is 4.363 V—it “reads high” by +0.003 V (which is the value of its systematic error, β_{14}). Next we choose the 29th model 22 voltmeter, repeat the calibration process, and find that it “reads low” by -0.010 V (which is the value of its systematic error, β_{29}). We repeat this for the 45th, 73rd, 102nd, . . . voltmeters and end up with a *distribution* of β 's from which we can calculate a standard deviation b . This value b would be the systematic uncertainty that we would associate with the voltage measurement of approximately 4.360 V from the model 22 voltmeter used in our measurement system, *because we do not know what the actual β is for that particular meter that we happened to choose from the available population of model 22 voltmeters.*

The systematic standard uncertainties for the elemental sources are estimated in a variety of ways that are discussed and illustrated in later sections. Among the ways used to obtain estimates are use of previous experience, manufacturer's specifications, calibration data, results from specially designed “side” experiments, results from analytical models, and others. Using this nomenclature, Eq. (1.10) is then written as

$$u_X = (b_1^2 + b_2^2 + s_X^2)^{1/2} \quad (1.11)$$

The ISO guide [4, p. 2] recommends designation of the standard uncertainties for the elemental sources by the way in which they are estimated. A *type A* evaluation of uncertainty is defined as a “method of evaluation of uncertainty by the statistical analysis of series of observations,” and the symbol s is used. A *type B* evaluation of uncertainty is defined as a “method of evaluation of uncertainty by means other than the statistical analysis of series of observations,” and the generic symbol u is used. The recommendations in the guide [4] and their use and implementation in this text and in Ref. 5 are discussed in detail later.

If in the case under discussion b_1 was estimated by a statistical evaluation using calibration data, it would be a *type A standard uncertainty* and would be designated $b_{1,A}$. If b_2 was estimated using an analytical model of the transducer and its boundary conditions, it would be a *type B standard uncertainty* and would be designated $b_{2,B}$. If s_X was calculated statistically as described, it would be a *type A standard uncertainty* and Eq. (1.11) would become

$$u_X = (b_{1,A}^2 + b_{2,B}^2 + s_{X,A}^2)^{1/2} \quad (1.12)$$

In summary, an *error* is a quantity with a given sign and magnitude. We presume that corrections will be or have been made for errors of *known* sign and magnitude (more discussion on this later). So in the remainder of this book, unless specifically stated, when we refer to an error, it is of *unknown* sign and magnitude and presumed equally probable to be positive or negative. An *uncertainty* u is an estimate of an interval ($\pm u$) that likely contains the magnitude of the error. Since the standard uncertainty defined in the ISO guide [4] and used in Ref. 5 and this book requires no assumption about the form of the parent population error distribution, the probability that the magnitude of the error falls within $\pm u$ is not known. This is discussed in the following section.

1-3.2 Degree of Confidence and Uncertainty Intervals

We stated earlier that we would like to take the information from a data sample to specify some range ($X_{\text{best}} \pm u_X$) within which we think X_{true} lies. The problem, as just noted, with using the standard uncertainty u_X for this range is that no probability can be associated with it. What is needed is an expanded uncertainty estimate, U_X , so that we can say that we are C percent confident that the true value of X lies within the interval

$$X_{\text{best}} \pm U_X$$

We usually assume X_{best} to be the mean value of the N measurements we have made (or if $N = 1$, *the* value of the single measurement), and U_X is the uncertainty in X that corresponds to our estimate with C percent confidence of the effects of the combination of the systematic and random errors. That is, $\pm U_X$ is the range within which we estimate δ (the value of the total error) lies C percent of the time. If, for example, we make a 95% confidence estimate of U_X , we would expect that X_{true} would be in the interval $X_{\text{best}} \pm U_X$ about 95 times out of 100. The methodology for obtaining the expanded uncertainty U_X from the standard uncertainty u_X is described in Chapter 2.

The confidence specification is necessary because we have made an estimate. We can always be 100% confident that the true value of some quantity will lie between plus and minus infinity, but specifying U_X as infinite provides no useful information to anyone. It is not necessary to perform an experiment to find that result!

The idea of degree of confidence in an uncertainty specification is vividly and humorously illustrated in an anecdote reported by Abernethy et al. [6, p. 162]:

In the 1930's, P. H. Myers at NBS and his colleagues were studying the specific heat of ammonia. After several years of hard work, they finally arrived at a value and reported the result in a paper. Toward the end of the paper, Myers declared: "We think our reported value is good to one part in 10,000; we are willing to bet our own money at even odds that it is correct to two parts in 10,000; furthermore, if by any chance our value is shown to be in error by more than one part in 1000, we are prepared to eat our apparatus and drink the ammonia!"

1-3.3 Expansion of Concept from "Measurement Uncertainty" to "Experimental Uncertainty"

Sometimes the uncertainty specification must correspond to more than an estimate of the "goodness" with which we can measure something. This is true for cases in which the quantity of interest has a variability unrelated to the errors inherent in the measurement system. When discussing this, it is helpful to consider three broad categories of experiments: timewise experiments at a (supposed) steady-state condition, sample-to-sample experiments, and transient experiments (in which there is a time-varying process).

In a typical *timewise experiment* we might want to measure a fluid flow rate in a system operating at a steady-state condition. Although the system might be in "steady" operation, there inevitably will be some time variations of flow rate that will appear as random errors in a series of flow rate measurements taken over a period of time. In addition, the inability to reset the system at exactly the same operating condition from trial to trial will cause additional data scatter.

In *sample-to-sample experiments*, measurements are made on multiple samples so that in a sense sample identity corresponds to the passage of time in timewise experiments. In sample-to-sample experiments, the variability inherent in the samples themselves causes variations in measured values in addition to the random errors in the measurement system.

As a practical example of such a case, suppose that we have been retained to determine the heating value of lignite in a particular geographic region. Lignite is a soft, brown coal with relatively low heating value and high ash and moisture content. However, a utility company is interested in the possibility of locating a new lignite-fueled power plant in the midst of a region (shown in Figure 1.7) that contains several large deposits of lignite. The savings in coal transportation costs might offset the negative effects of the low heating value of lignite and make such a power plant economically feasible. A decision on the economic feasibility depends critically, of course, on the lignite heating value used in the calculations.

The heating value of a 1-g sample of lignite can be determined with an uncertainty of less than 2% in the laboratory using a bomb calorimeter. However, because of large variations in the composition of the lignite within a single deposit and from deposit to deposit, the heating values determined for samples

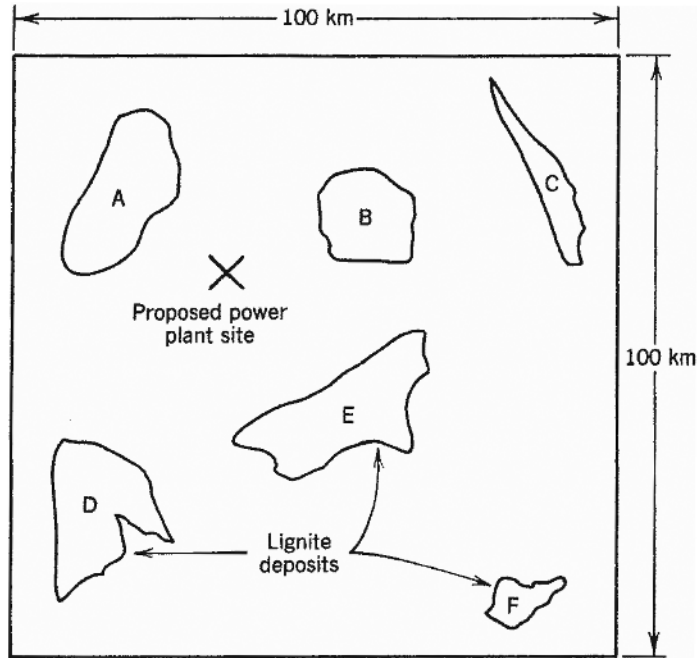


Figure 1.7 Region containing lignite deposits and potential power plant site.

from the northern and southern portions of deposit *C*, for instance, might differ by 20% or more. Even larger differences might very well be observed when results are compared for samples from entirely different deposits.

In this situation, the variation within a set of individual measurements of lignite heating value is not due primarily to errors in the measurements. It is due to the natural variation of the physical variable that is being measured. The amount of the variation within a set of measurements will differ depending on whether the set is taken from within a small area of a single deposit, from the entire area of a single deposit, or from different deposits. We might conclude from this example that the answer to the question, “What is the uncertainty in the lignite heating value?” depends on what is meant when one says, “the lignite.” The answer will be one thing for a single 1-g sample and quite another thing for deposits *A*, *B*, and *E* considered together. Thus we can conclude that the uncertainty estimate depends on what the question is.

In *transient experiments*, the process of interest varies with time and there are some unique circumstances involved in obtaining multiple measurements of a variable at a given condition. As an example of one type of transient test, consider measuring the thrust of a rocket engine with a single load cell from the instant of ignition ($t = 0$) until the time when the desired “steady” operating condition is reached. Only one measurement of thrust can be made at $t = 0.3078$ s in a given test. To obtain a sample population of measurements of thrust at

$t = 0.3078$ s, the engine must be tested again, and again, and again. This type of test has obvious similarities to the sample-to-sample category, and even more so if multiple engines of the same design are considered as part of the sample population of measurements.

A second type of transient test is when the process is periodic. Consider measuring the temperature at a point within a cylinder of an internal combustion engine operating at a steady condition. This type of test has some obvious similarities to the timewise steady category discussed earlier. Multiple measurements of temperature at the “same” operating point can be obtained by making a measurement at 30° before top dead center, for example, over a number of repetitions of the periodic process.

Measurements in a transient test typically have elemental error sources in addition to those normally present in a timewise steady test. Imperfect dynamic response of the measurement system can result in errors in the magnitude and phase of the measured quantity. An introduction to this subject is presented in Appendix F.

1-3.4 Elemental Systematic Errors and Effects of Calibration

Over the years, the misconception has persisted that the effects of systematic errors can be removed by calibration. In fact, systematic errors have often been summarily dismissed in books and articles on error analysis or uncertainty analysis by a simple statement that “all bias errors have been eliminated by calibration.” In real testing situations, this is never the case.

Consider as an example the measurement of a temperature using a thermocouple (tc) system as shown in Figure 1.8a. The tc is connected to a data acquisition system (das) consisting of an electronic reference junction, signal conditioning, an analog-to-digital converter, and a digital voltmeter. The tc is exposed to some temperature T_{true} that one wishes to “measure” and the system output is a voltage E .

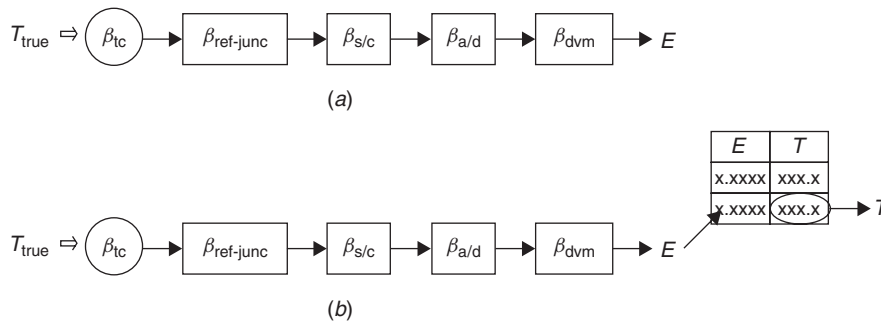


Figure 1.8 (a) Thermocouple system with its output voltage E . (b) No individual calibration—used with generic temperature–voltage table to “measure” a temperature.

Suppose that the thermocouple is used as supplied and is not individually calibrated. As shown in Figure 1.8*b*, the voltage E is used in a generic T -versus- E table for the particular type of thermocouple and the corresponding temperature T is found—this is the temperature that is said to be the measured value of T_{true} . The uncertainty in this value includes contributions from the elemental systematic errors:

- β_{tc} , the amount this tc differs from the generic tc in the table
- $\beta_{\text{ref-junc}}$, the error from the electronic reference junction
- $\beta_{\text{s/c}}$, the error from the signal conditioner
- $\beta_{\text{a/d}}$, the error from the analog to digital converter
- β_{dvm} , the error from the digital voltmeter

Now suppose the thermocouple is calibrated as shown in Figure 1.9*a*. The thermocouple and the temperature standard are both exposed to the same temperature T_{true} , and the voltage E output by the thermocouple system (tc + das)

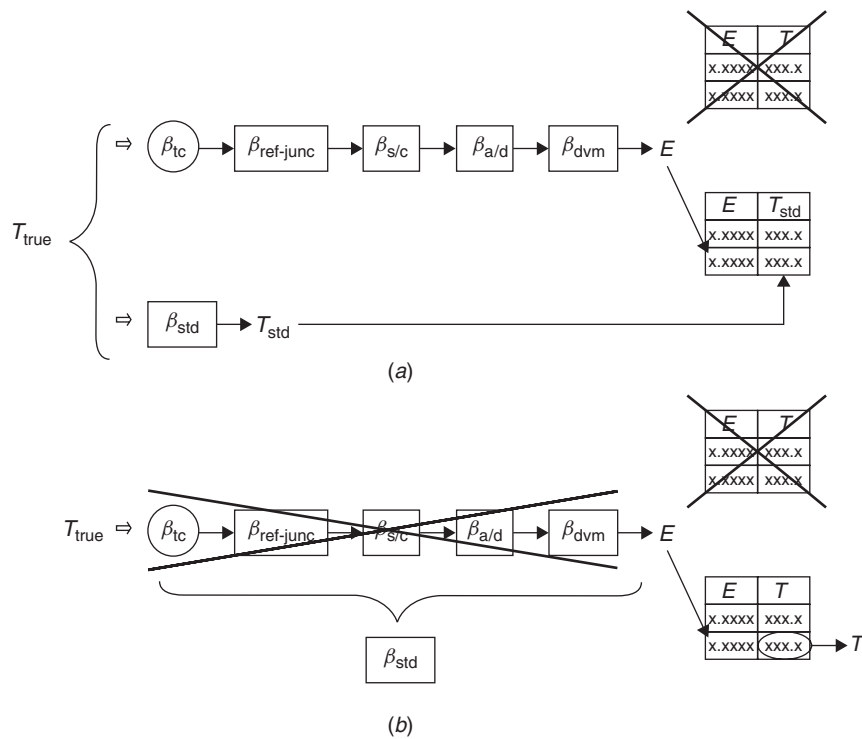


Figure 1.9 (a) Calibration of tc system of Figure 1.8 using a temperature standard. (b) Use of calibrated system to measure a temperature.

and the temperature T_{std} indicated by the standard are entered as a data pair in the calibration table, which now replaces the generic table used previously.

When the tc system is now used to measure a value T_{true} , as shown in Figure 1.9b, the voltage output E is used to enter the calibration table and the corresponding temperature retrieved is effectively that which would be indicated by the standard. The uncertainty in the resulting temperature T now includes the contribution from the systematic error β_{std} (the amount T_{std} differs from T_{true}), which *replaces* the contributions from the errors β_{tc} , $\beta_{\text{ref-junc}}$, $\beta_{\text{s/c}}$, $\beta_{\text{a/d}}$, and β_{dvm} .

So we see that some systematic errors can be replaced by (hopefully!) smaller ones by application of the calibration process. In this example, we have not considered other systematic error sources that might be present in the test (such as installation effects, interaction of the tc with the environment, and others that we will discuss in detail in later chapters) that are not present in the well-controlled calibration process.

It is also critical to realize just what is being calibrated. If the tc and data acquisition system that are calibrated together are used in the test, then the error situation is as described above. However, if the data acquisition system used in the calibration (das_{cal}) is replaced by another (das_{test}) for the test, the systematic error from the standard β_{std} then *only* replaces the error contribution β_{tc} . The uncertainty in the resulting temperature T now includes contributions from (1) β_{std} ; (2) $\beta_{\text{ref-junc}}$, $\beta_{\text{s/c}}$, $\beta_{\text{a/d}}$, and β_{dvm} from das_{cal} ; and (3) $\beta_{\text{ref-junc}}$, $\beta_{\text{s/c}}$, $\beta_{\text{a/d}}$, and β_{dvm} from das_{test} .

1-3.5 Repetition and Replication

In discussing errors and uncertainties in measurements, it is useful to draw a careful distinction between the meanings of the words *repetition* and *replication*. We will use repetition in its common sense, that is, to mean that something is repeated. When we use the word replication, we will be specifying that the repetition is carried out in a very specific manner. The reason for doing this is that different factors influence the errors in a series of measurements, depending on how the repetition is done. The idea of considering uncertainties at different orders of replication level was suggested by Moffat [7–9] for the timewise category of experiments, and we will also find it useful for sample-to-sample experiments. As did Moffat, we will find it convenient to define three different levels of replication: zeroth order, first order, and N th order.

At the *zeroth-order replication level* in a timewise experiment, we hypothesize that time is “frozen” so that the process being measured is absolutely steady. This allows only the variations inherent in the measuring system itself to contribute to random errors. In a sample-to-sample type of experiment, this corresponds to consideration of a single fixed sample.

If we make a zeroth-order estimate of the systematic and random uncertainties associated with an instrument, this is the “best” we would ever be able to achieve if we used that instrument. If this zeroth-order estimate shows that the



uncertainties are larger than those that are allowable in our experiment, it is obvious that we should not perform the experiment using that instrument.

In considering uncertainties at the *first-order replication level* in a timewise experiment, we hypothesize that time runs but all instrument identities are fixed. When estimating uncertainties at this level of replication, the variability of the experimental measurement is influenced by all of the factors that contribute to unsteadiness during repeated trials with the experimental apparatus. Depending on the length of time covered by a replication at the first-order level, different factors such as variations of humidity, barometric pressure, ambient temperature, and the like can influence the random portion of the experimental uncertainty. In addition, our inability to reproduce a set point exactly with the experimental apparatus influences the variations in experimental results.

A first-order estimate of the random uncertainty is indicative of the *scatter* that we would expect to observe during the course of a timewise experiment. If on repeated trials over the course of time we observe a scatter significantly larger than that given by the first-order estimate, it is very likely that there is some effect that has not been taken into account and that we should probably investigate further. This use of first-order random uncertainty estimates in debugging timewise experiments is discussed in detail in Chapter 5.

In considering uncertainties at the first-order replication level in a sample-to-sample experiment, we imagine that all instruments remain the same as sample after sample is tested. Additional variations observed above the level of random errors at the zeroth order are indicative of the variations in the samples themselves.

When considering what uncertainties should be specified when we speak of where the true value lies relative to our measurements, we make estimates at the *Nth-order replication level*. Such an *Nth-order* estimate includes the first-order replication level estimate of random uncertainty combined with a systematic uncertainty estimate that considers all of the systematic error sources that influence our measurements. Using Moffat's concept for timewise steady experiments, at the *Nth-order* replication level both time and instrument identities are considered to vary. At this level, we hypothesize that after each measurement each instrument is considered to have been replaced by another of the same type, model number, and so on.

What this essentially means is that the systematic error associated with a particular instrument becomes a random variable when the instrument identity is allowed to vary. To see this, consider that a particular model of pressure gauge might be specified by the manufacturer as "accurate within $\pm 1\%$ of full scale." The particular gauge we use might actually read high by 0.75% of full scale (a systematic error). If we replace that gauge by an "identical" gauge, it might very well read low by 0.37% of full scale. Thus, when we consider replacing each instrument with an identical instrument for each reading, the systematic error associated with the instrument becomes a random error at the *Nth-order* level of replication. Using an *Nth-order* model for the systematic error, then, we can take the view that a particular systematic error is a single realization drawn from



some parent population of possible systematic errors. This concept is often useful when formulating a systematic uncertainty estimate.

From the preceding discussion it should be evident that the reason for considering different levels of replication is that the factors that are considered to influence the uncertainty in the experiment differ with the level of replication. We shall make use of this point as we consider the analysis and design of experiments in later chapters.

1-4 EXPERIMENTAL RESULTS DETERMINED FROM MULTIPLE MEASURED VARIABLES

In many, if not most, experiments or testing programs the experimental result is not directly measured but is determined by combining multiple measured variables in a *data reduction equation* (DRE). Examples are the dimensionless groups, such as drag coefficient, Nusselt number, Reynolds number, and Mach number, that are often used to present the results of a test. Other examples are the density of a gas (the result) determined using the ideal gas expression

$$\rho = \frac{P}{RT} \quad (1.13)$$

and measurements of pressure P and temperature T , or a heat flux (result) on the inside of a wall using an inverse heat conduction method and measurements of temperature versus time on the outside of the wall.

When a DRE is used, we must consider how the systematic and random uncertainties in the measured variables *propagate* through the DRE to produce the systematic and random uncertainties associated with the result. This is discussed in Chapter 3. There are two propagation approaches in common use today—the Taylor Series Method (TSM) and the Monte Carlo Method (MCM). Both are described and their applications illustrated in the following chapters.

In some experiments, the objective is to determine the relationship among several results. An example is determining the resistance characteristics of a rough-walled pipe in a flow system, as shown schematically in Figure 1.10.

A classic experiment of this kind was executed and reported by Nikuradse [10], who investigated the effect of wall roughness on fully developed flow in circular pipes that had been roughened by gluing a specific size of sand onto the interior wall of a given pipe test section. The results of that experimental investigation were reported with friction factor λ as a function of Reynolds number Re and “relative sand-grain roughness” r/k , as shown in Figure 1.11.

Using modern nomenclature friction factor is defined as

$$f = \frac{\pi^2 d^5 (\Delta P)}{8 \rho Q^2 L} \quad (1.14)$$

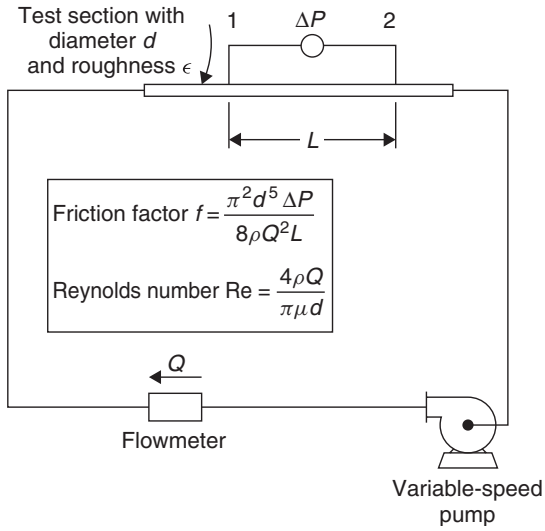


Figure 1.10 Experimental determination of resistance characteristics of rough-walled pipe.

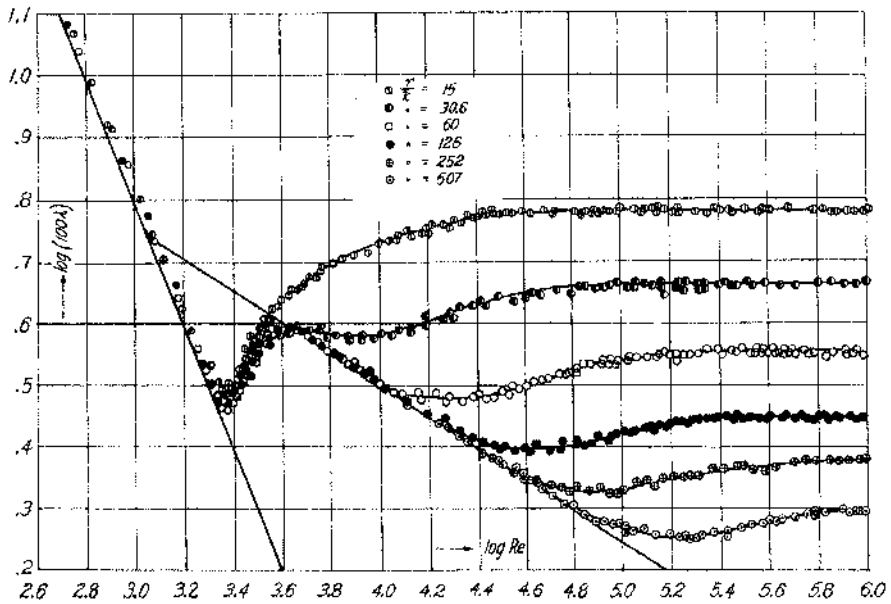


Figure 1.11 Rough-walled pipe results of Nikuradse [10].

and the Reynolds number as

$$\text{Re} = \frac{4\rho Q}{\pi\mu d} \quad (1.15)$$

and the third nondimensional group is ϵ/d , where ϵ in this example is taken as the “equivalent sand-grain roughness” (which is a single length scale descriptor of the roughness). In such an experiment, values for the following variables are found by measurement or from reference sources (property tables, dimensioned drawings):

- d , the pipe diameter
- ϵ , the roughness “size”
- L , the distance between pressure taps
- ΔP , the directly measured pressure drop over distance L
- Q , the volumetric flow rate of the fluid
- ρ , the density of the fluid
- μ , the dynamic viscosity of the fluid

and the experimental results f , Re , and ϵ/d are determined from DREs.

If one were going to test a pipe with a new kind of wall roughness and results such as those shown in Figure 1.11 were not yet known, in the planning phase of the experiment one would use *general uncertainty analysis* (discussed in Chapter 4) to investigate the overall uncertainty behavior of the DREs—Eqs. (1.14) and (1.15) and ϵ/d . Once satisfied that the results (f , Re , ϵ/d) could be obtained with an acceptable uncertainty, then in the postplanning phases described in Section 1-2.2 one would investigate the detailed behavior of the systematic and random uncertainties using *detailed uncertainty analysis* (discussed in Chapter 5).

At the completion of the experimental program, one might want to show the relationship among the results (in addition to a plot similar to Figure 1.11) by using a regression equation such as that [11] which represents the famous Moody diagram:

$$\frac{1}{f^{0.5}} = -1.8 \log_{10} \left[\frac{6.9}{\text{Re}_D} + \left(\frac{\epsilon/D}{3.7} \right)^{1.11} \right] \quad (1.16)$$

When such an expression is used to obtain a value of the result—in this case friction factor f —the uncertainty which should be associated with that value should take into account the uncertainties in the data points used to obtain the regression. The approach to do this is discussed in detail in Chapter 7.

1-5 GUIDES AND STANDARDS

1-5.1 Experimental Uncertainty Analysis

In 1993, the *Guide to the Expression of Uncertainty in Measurement* was published by the International Organization for Standardization (ISO) in its name and those of six other international organizations.² According to the foreword in the ISO guide, “In 1977, recognizing the lack of international consensus on the expression of uncertainty in measurement, the world’s highest authority in metrology, the Comité International des Poids et Mesures (CIPM), requested the Bureau International des Poids et Mesures (BIPM) to address the problem in conjunction with the national standards laboratories and to make a recommendation.” After several years of effort, this led to the assignment of responsibility to the ISO Technical Advisory Group on Metrology, TAG 4, which then established Working Group 3 to develop a guidance document. This ultimately culminated in the publication of the ISO guide, which has been accepted as the de facto international standard for the expression of uncertainty in measurement (and is commonly referred to as the GUM).

As discussed previously in this chapter, the GUM recommends categorizing uncertainties as type A or B depending on the method of evaluation [statistical (A) or otherwise (B)]. The GUM discourages the use of systematic and random but then also states that sometimes dividing uncertainties into those that contribute to variability and those that do not can be useful. In fact, as discussed previously (Section 1-3.1), we cannot envision any way to identify all of the elemental error sources other than to divide them into categories that (1) vary and contribute to the standard deviation s (random standard uncertainty) and those that (2) do not vary and thus must be estimated with a systematic standard uncertainty b . In this book, we use this nomenclature, as does the ASME standard PTC 19.1-2005 [5].

The approach in the GUM is based on a Taylor Series Method (TSM) to model the propagation of uncertainties, and we will use TSM to denote this approach in this book. This is to distinguish that propagation approach from the Monte Carlo Method (MCM) of propagation presented in the recently published [12] Joint Committee for Guides in Metrology (JCGM) supplement.

From the foreword to that document:

In 1997 a Joint Committee for Guides in Metrology (JCGM), chaired by the Director of the Bureau International des Poids et Mesures (BIPM), was created by the seven international organizations that had originally in 1993 prepared the “Guide to the expression of uncertainty in measurement” (GUM) and the “International vocabulary of basic and general terms in metrology” (VIM). The JCGM assumed responsibility for those two documents from the ISO Technical Advisory Group 4 (TAG4) JCGM has two Working Groups. Working Group 1, “Expression of

²Bureau International des Poids et Mesures (BIPM), International Electrotechnical Commission (IEC), International Federation of Clinical Chemistry (IFCC), International Union of Pure and Applied Chemistry (IUPAC), International Union of Pure and Applied Physics (IUPAP), and International Organization of Legal Metrology (OIML).



uncertainty in measurement”, has the task to promote the use of the GUM and to prepare Supplements and other documents for its broad application . . . Supplements such as this one are intended to give added value to the GUM by providing guidance on aspects of uncertainty evaluation that are not explicitly treated in the GUM. The guidance will, however, be as consistent as possible with the general probabilistic basis of the GUM.

The MCM requires estimation of probability distributions rather than simply standard uncertainties (standard deviations) as in the TSM. This, of course, is thoroughly discussed as the method is described and implemented in this book.

1-5.2 Validation of Simulations

The approach to validation of simulations described in this book is that in the recently issued [13] ASME standard V&V20-2009, *Standard for Verification and Validation in Computational Fluid Dynamics and Heat Transfer*. The approach is based on the concepts from experimental uncertainty analysis as presented in this book, the GUM, and JCGM 101:2008 and uses both the TSM and MCM techniques. From the foreword:

The objective of this Standard is the specification of a verification and validation approach that quantifies the degree of accuracy inferred from the comparison of solution and data for a specified variable at a specified validation point. The scope of this Standard is the quantification of the degree of accuracy for cases in which the conditions of the actual experiment are simulated. Consideration of the accuracy of simulation results at points within a domain other than the validation points, for example interpolation/extrapolation in a domain of validation, is a matter of engineering judgment specific to each family of problems and is beyond the scope of this Standard. ASME PTC 19.1-2005 “Test Uncertainty” is considered a companion document to this Standard.

Previously both the American Institute of Aeronautics and Astronautics (AIAA) [14] and the ASME [15] published V&V guides that present the philosophy and procedures for establishing a comprehensive validation program, but neither provides approaches to quantitative evaluations of the comparison of the validation variables predicted by simulation and determined by experiment.

V&V is an area of current research, and there is (and likely will never be) no universally accepted single method for use in simulations in all disciplines ranging from computational mechanics to war gaming. Organizations other than the AIAA and ASME [e.g., 16] are currently in the process of developing V&V guides and standards.

1-6 A NOTE ON NOMENCLATURE

It is worthwhile at this point to emphasize several points on the nomenclature used in experimental uncertainty analysis and the way it has developed over the past



several decades. In Appendix B, the authors outline the historical development of uncertainty analysis from the 1950s through the 1990s, and the reader is referred to it as a fairly concise overview.

For those familiar with the nomenclature used in books and the literature in the recent past (particularly in the United States), perhaps the most prominent change is that the symbols B and P are no longer used. Originally B was termed the “bias limit” and later called the “systematic uncertainty,” and P was termed the “precision limit” and later called the “random uncertainty.” These are no longer needed, as the concepts of u , the standard uncertainty; b , the systematic standard uncertainty; and s , the random standard uncertainty are unambiguous in their definitions and in their use in both the TSM and MCM for uncertainty propagation.

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16. NASA, "Standard for Models and Simulations," NASA-STD-7009, Draft, 2008.

PROBLEMS

- 1.1 Consider the lignite heating value example discussed in Section 1-3.3. A salesman drops by your office and says that he can supply a calorimetry system that can determine the heating value of a 1-g sample of lignite with an uncertainty of about 1% as opposed to the 2% obtained using the present system. Is this idea worth considering if you need to find the heating value of the lignite in deposit E?
- 1.2 A Pitot-static probe is mounted to monitor the exit velocity of air exhausting from a duct in a process unit. The differential pressure output from the probe is applied to an analog pressure gauge, and a technician reads the gauge and notes the reading every hour. The operating condition of the process unit is held as steady as possible by its control system. If the gauge is located in a room with a conditioned environment, list the possible factors involved in causing scatter in the readings. What additional factors are involved if the gauge is located in a room where the temperature and humidity are uncontrolled? If the process unit is shut down and then restarted and reset to the same operating condition? If the gauge is replaced by another of the same model number from the same manufacturer?

