1994

Inference for Unreplicated Factorial and Fractional Factorial Designs.

William J. Kasperski
Louisiana State University and Agricultural & Mechanical College

Order Number 9502117

Inference for unreplicated factorial and fractional factorial designs

Kasperski, William J., Ph.D.

The Louisiana State University and Agricultural and Mechanical Col., 1994

U·M·I
300 N. Zeeb Rd.
Ann Arbor, MI 48106

INFERENCE FOR UNREPLICATED FACTORIAL AND FRACTIONAL FACTORIAL DESIGNS

A Dissertation

Submitted to the Graduate Faculty of the
Louisiana State University and
Agricultural and Mechanical College
in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy

in

The Interdepartmental Program in Business Administration

by
William J. Kasperski
B.S., Northern Arizona University 1986
May 1994

ACKNOWLEDGMENTS

I would like to express my appreciation to my major professor, Dr. Helmut Schneider, for his assistance, support, and guidance throughout this research. I look forward to continuing our research efforts in the future.

I would also like to thank the members of my doctoral committee, Dr. Kwei Tang, Dr. Peter Kelle, Dr. Dan Rinks, Dr. Ye-Sho Chen, and Dr. Richard Kazmierczak. Their comments and suggestions helped make this a more thorough research study. Also, I would like to thank Dr. James Pruett for serving on my committee while he was at LSU.

To Dr. Graydon Bell, thanks for getting me on the right track early in my academic career. Your straightforward comments were not fully appreciated for several years, until I realized that they were in my best interest.

To Mrs. Forbes, your assistance in this work was an immense help. I would still be at this if it were not for you. Thanks for everything over the past several years.

To Rob Rhoades, Ken Sosinski, Luciana Reis, Dr. Keith Johnson, Jeff Jonkman, and Phil Wadsworth, thanks for being there over the years and making things interesting.

To my brother and sister, Frank and Mary Ann, thanks for your support over the years and for providing me with vacation possibilities across the U. S. and abroad.

To my father, Franklin, thanks for making all of this possible. Your hard work and assistance has helped immeasurably over the years.

Finally, I would like to dedicate this to the loving memory of my mother, Geraldine.

TABLE OF CONTENTS

Page

LIST OF TABLES

Page

LIST OF FIGURES

ABSTRACT

The ability to determine which factors significantly affect a product or process can help to improve its quality. Usually there are many factors to be considered initially, but a limited amount of time and money, so it is important to screen the numerous factors with a limited number of experimental trials. In this situation, unreplicated factorial and fractional factorial designs are often used, but because these experiments are unreplicated they do not possess a formal estimate of the experimental error variance. Several methods have been proposed by Daniel, Box and Meyer, Benski, Lenth, and Schneider, Kasperski, and Weissfeld to determine the significant effects in these experiments.

This research focuses on an in-depth comparison of the aforementioned methods under a variety of practical situations commonly found in industrial experiments. Each method will be critically evaluated, with the culmination of the work being a recommendation for the use of the various methods.

CHAPTER 1. INTRODUCTION

In today's global economy, increased competition has helped to reinforce the importance of statistical design of experiments and the analysis of the resulting data. Through the use of designed experiments beneficial information for improving both products and processes may be obtained. This research will focus on the analysis of designed experiments, which is but one part of the entire experimental planning procedure.

Montgomery (1991) described a seven-step approach for the entire planning process. The corresponding table is shown below.

Table 1.1.
Steps of Experimentation

1. Recognition of and statement of the problem.
2. Choice of factors and levels.
3. Selection of the response variable.
4. Choice of experimental design.
5. Conduction of the experiment.
6. Data analysis.
7. Conclusions and recommendations.
(Coleman and Montgomery, 1993).

Each of the 7 steps is important in the entire scheme of the experiment. Coleman and Montgomery (1993) focused on the "preexperiment planning phase", which they regard as the first 3 steps in Table 1.1. In the past, several authors have also emphasized the importance of the planning activities that occur before the experiment is performed

in order for a successful outcome. These include Box, Hunter, and Hunter (1978), Hahn (1977, 1984), Natrella (1979), and Montgomery (1991). Without a successful start there is little or no chance of the experiment achieving worthwhile results.

Within the same framework described above, Coleman and Montgomery (1993) illustrate an important reason as to why experiments usually do not go exactly as planned. The people who design the experiments (statisticians) have to bridge a gap in knowledge and experience with the scientists and engineers (experimenters). These gaps, when overlooked, can have serious repercussions for the experiment.

When a statistician lacks knowledge about the domain of the experiment several problems may arise. These include unnecessary assumptions, undesirable combinations of variable levels in the design, violating or not exploiting known physical laws, creating unreasonably large designs, inappropriate confounding, inadequate measurement of factors, and an undesirable run order (Coleman and Montgomery, 1993).

When an experimenter lacks sufficient knowledge of statistics, problems may also arise. There may be inappropriate control-variable settings (range too small to observe an effect or too large so that outside factors influence the response variable), misunderstanding of interaction effects that cause inadequate confounding of effects, experimental design results distorted by measurement errors, inadequate identification of factors that are held constant, and misinterpretation of past experimental results that may affect the chosen factors and their levels (Coleman and Montgomery, 1993).

The ability to bridge this gap is important for the success of the experiment. Gunter (1993) made a statement that illustrates this idea. He said that "All experiments

are designed experiments; the only question is whether well or poorly." When there is a failure to perform adequate planning there is little that can be done to save the experiment and the results will most likely be worthless.

Once the initial planning phase has been performed, the experimental design must be selected. Experimental designs that have been receiving increased emphasis over the past few years is unreplicated factorial and fractional factorials for the improvement of a product or process. Many of today's experiments are unreplicated because of the immense amounts of time and cost involved. In these types of experiments the analyst hopes to determine which factor(s) significantly influence the product or process.

When experiments are conducted to study the effects of several factors on a product's yield or quality, a machine's performance, etc., a factorial design is often employed. This type of design allows the effect of changing one factor to be examined independently of the other factors.

After the product or process gets closer to market conditions, there may be a dramatic increase in the restrictions that are required. The product must be able to withstand a wide range of operating conditions and the analyst must be able to reassure management that the product is safe, effective, and capable of being used in conditions not previously considered when it was initially proposed. An increasing number of factors must be examined because of the multitude of factors that can influence a product (Davies, 1971).

It is in the screening of numerous experimental factors where fractional factorial designs play an important role. A large number of factors can be initially considered, with the belief that only a few of them will have a significant impact on the response variable. A fractional factorial design will allow the estimation of main effects, but there will be confounding of higher-order interactions that will not permit the analyst to separate certain effects from one another. Usually it is assumed that 3 factor and higher-order interactions are not the main interest of the experiment, so that confounding of these effects does not present a major stumbling block in most instances.

1.1. Problem Statement

Whenever an experiment is unreplicated, the analyst must find a way to circumvent the lack of an easily definable estimate of the experimental error variance in order to analyze the data. Several different methods have been proposed to analyze unreplicated factorial and fractional factorial designs in order to determine which factor(s) are indeed statistically significant. While each of the methods have given similar results on a limited number of examples, no in-depth analysis has been performed.

The main objectives of this research are to: (1) evaluate the performance of the existing procedures used in analyzing unreplicated factorial and fractional factorial designs with extensive real-life examples along with a limited simulation study and (2) provide a recommendation as to which methods are useful in a given testing situation.

1.2. Contribution of the Research

The analysis of unreplicated factorial and fractional factorial designs has been examined with respect to determining significant factor effects by several researchers over the years, with an increased emphasis in the past few years. These include Daniel (1959, 1976), Box and Meyer (1986, 1993), Benski (1989), Lenth (1989), and Schneider, Kasperski, and Weissfeld (1993). Within each of these papers, after Daniel's 1976 book, the authors analyzed several examples and compared their procedure to each of the existing techniques. Their intention was to demonstrate the consistency of the results obtained using the newer method with those obtained from the established procedures. Statements concerning the computational speed of the algorithms used were often made, but this is becoming less of an issue with today's supercomputers and faster desktop computers. Cabau and Benski (unpublished) compared four of the techniques (all except Daniel's) in a Monte Carlo simulation study, but no extensive comparison of the five aforementioned procedures has been performed using empirical data.

The contribution of this research to the current knowledge of unreplicated factorial and fractional factorial designs is in the following ways. First, the research will provide a detailed analysis of each method including underlying assumptions, advantages, and disadvantages. Second, by comparing the methods using an extensive list of real-life data sets, this research attempts to provide experimenters with information concerning the performance of each technique in practical situations. The performance evaluation will be based upon several experimental situations including the number of factors in the design, design resolution, degree of fractionization and confounding. The similarities

and differences of the method's results will be analyzed in detail. Finally, the research will attempt to provide a recommendation for which method(s) should be used in a particular experimental situation.

1.3. Organization of the Research

The research is organized in eight chapters. In Chapter 2, pertinent background material on factorial and fractional factorial designs is discussed. This will help provide the groundwork for the research. Chapter 3 involves a discussion of factor sparsity along with a literature review of previous techniques used for analyzing unreplicated factorial and fractional factorial experiments. In Chapter 4, the Schneider, Kasperski, and Weissfeld procedure is introduced and described in detail. Chapter 5 contains numerous two-level design data sets along with the results of analyses using the techniques described in Chapters 3 and 4. Also included in Chapter 5 is a discussion of a previous simulation study used to compare the procedures. In Chapter 6, Daniel's normal probability plotting procedure is revisited and modified using the Schneider, Kasperski, Weissfeld procedure. The effects of violating the underlying assumptions are examined and corrective measures are suggested. The final chapter provides a summary and the conclusions for the research.

CHAPTER 2. FACTORIAL AND FRACTIONAL FACTORIAL DESIGNS

2.1. Factors, Effects, and Contrasts

Experiments performed by analysts are usually for the purpose of determining the effects of a factor(s) on the response variable of interest. Response variables include product yield, quality, performance, etc. The analyst is able to gain an advantage if the experiment is designed in such a way that the effect of changing any one variable can be analyzed independently of the other variables. One way of obtaining this goal is to determine the factors of interest for the response variable being studied, select the values (levels) that the factors will assume, and then conduct the experiment. This type of experimentation is known as a factorial experiment (Davies, 1971).

The factors are features of the experiment that may be deliberately altered between trials. Thus, the factors are variables that the analyst has some control over. Examples of factors include temperature, time, pressure, concentration, etc. There may be two different types of factors in an experiment. A qualitative factor is a variable that cannot have its levels arranged by order of magnitude. For instance, different pieces of material produced in different factories would be qualitative since we cannot place them in a specific, meaningful, numerical order. Quantitative factors, on the other hand, can be arranged based on their numerical values. Examples of quantitative factors would be temperature, pressure, concentration, etc. (Davies, 1971).

The values that a factor assumes during an experiment are known as factor levels. This terminology was first used to describe quantitative factors, where its meaning is easier to understand, but it has also been applied to qualitative factors. The particular

combination of levels for the factors used in a single trial (run) of the experiment is known as the treatment or treatment combination. The treatment combination provides a complete description of the testing conditions for the experimental trial (Davies, 1971).

The effect for a given factor is the change in the response variable caused by a change in the level of the factor. For a two-level experiment, the main effects are defined as the difference between the average response at the two levels of a factor. The two-factor interactions are defined as half the difference between the main effects of one factor at the two levels of a second factor (Mason, Gunst, Hess, 1989).

An important concept, which is often misunderstood, is that of a contrast. By definition, a contrast is a linear combination of k averages

$$a_1\overline{y}_1 + a_2\overline{y}_2 + \ . \ . \ . \ + a_k\overline{y}_k,$$

where $\overline{y}_i$ is the i^{th} average and the a_i are constants, with at least two of the a_i's non-zero, and $\Sigma a_i = 0$ (Mason, Gunst, and Hess; 1989). Thus, each of the effects (main, two-factor interactions, etc.) are indeed contrasts.

The confusion surrounding the concept of an effect lies in the fact that it means something different depending on the statistical setting. In the literature describing two-level factorials, an effect is defined as previously mentioned, the difference between the responses of a factor at the high and low levels. However, in the classical analysis of variance setting an effect is defined as one half the difference between the high mean and the grand mean (Lenth, 1989). Throughout this paper, the term contrast will be used to describe the difference between the high and low levels of a factor and will be used interchangeably with the term effect.

Also, a special class of contrasts used in the analysis of factorial designs are orthogonal contrasts. Two contrasts, $C_1 = \Sigma a_i \overline{y}_i$ and $C_2 = \Sigma b_i \overline{y}_i$, are defined as being orthogonal if the sum of the product of the corresponding coefficients of the two contrasts is zero, i.e., $\Sigma a_i b_i = 0$. Three or more contrasts are mutually orthogonal if all pairs of the contrasts are orthogonal (Mason, Gunst, and Hess; 1989).

To illustrate a factorial experiment consider the following scenario. An analyst wishes to study the strength of a porcelain product (response variable). Three factors are being considered in the experiment; temperature, baking time, and cooling method. For the temperature factor, five different levels are to be used; 250°, 300°, 350°, 400°, and 450°. With respect to the baking time, four different levels will be considered; 1 hour, 1 1/2 hours, 2 hours, and 2 1/2 hours. Finally, there will be two different cooling methods used; a water bath and a nitrogen shower. In order to perform a full factorial experiment the analyst would need to perform 5x4x2 = 40 experimental trials. A specific treatment combination would be (T_2, B_3, C_1) which would correspond to a 300° temperature, 2 hour baking time, and a water bath.

2.2. Randomization

Randomization is an important feature that is useful in all types of experimental designs. In a completely randomized design all of the combinations for the factor levels in the experiment are randomly assigned to experimental units or to the sequence of test runs. This is done in such a way that each factor level combination has an equal chance of being assigned to any test sequence (Mason, Gunst, and Hess; 1989).

The importance of randomization is due to the fact that the analyst cannot always be certain that all of the major influences on the response have been considered. Cochran and Cox (1957) compare randomization to a form of insurance. There may be unanticipated events that may or may not occur and these may or may not be of a serious nature and randomization in all instances, including those in which no major problems are anticipated even without randomizing, is there to protect against unforseen events that can disrupt the analysis. Examples of these unforseen events include drifting instrumentation, malfunctioning equipment, operator errors, etc.

More specifically, if an analyst wants to compare two different types of computers, with respect to average current flow, and has five of each type available for his use, in what order should the 10 computers be tested? If the testing order used is to examine all five units of one type followed by the five units of the other type then problems of accountability may arise. Suppose that the line voltage drifts during the testing procedure. The analyst may ascertain that there is a significant difference between the two types of computers, when what actually accounts for the major portion of the difference is the drifting line voltage. By randomizing the order in which the experiment is performed the effect due to the drifting line voltage will tend to average out over the varying experimental conditions (Hicks, 1973). This relates directly to factorial experiments where the analyst cannot prevent unforseen events, line voltage drifts, but can spread the problem over all of the factor levels, computer types.

2.3. Factorial Example

Davies (1971) demonstrated the advantages of a factorial experiment using the following example. Two factors were considered (temperature and pressure) at two levels each (T_0, T_1, P_0, P_1). The minimum amount of work required to provide information about both factors is three trials, T_0P_0, T_1P_0, T_0P_1. These three trials are shown in the table below as (1), (2), and (3).

Table 2.3.1.
Factorial Experiment Example

Pressure	Temperature	
	T_0	T_1
P_0	(1)	(2)
P_1	(3)	(4)

The effect due to changing temperature is given as (2) - (1), with the effect due to changing pressure given by (3) - (1). The experiment could be replicated and the averages of several runs used to estimate the effects. This type of experimentation is known as one factor at a time, since each factor is studied separately.

Now consider running a fourth trial, (4), where we use T_1P_1. By completing the factorial experiment we can estimate the temperature effect at pressure P_0 by (2) - (1) and at pressure P_1 with (4) - (3). If no interaction existed between pressure and temperature, then the estimates just determined will be different only because of the experimental error. Thus, the average of the estimates will provide the temperature effect with as much precision as replications of (1) and (2). The same criteria applies to the estimate

of the effect due to pressure (3) - (1) and (4) - (2). So when no interaction exists, the four experimental trials of the factorial design provide estimates of the effects that are as precise as the six trials when we replicated the one factor at a time design (Davies, 1971).

Suppose now that the temperature and pressure factors interacted with each other. If the analyst found that T_1P_0 and T_0P_1 provided a better result for the response variable, then he may conclude that T_1P_1 should be the best of them all. This conclusion is based on the assumption of no interaction, which may be seriously inaccurate. The treatment combinations T_1P_0 and T_0P_1 may be about the same as T_0P_0, but with interaction present T_1P_1 may be tremendously better. By using the one factor at a time design the analyst could entirely miss the "best" combination and arrive at a wrong conclusion (Davies, 1971).

2.4. Replication

If possible, repeated test runs should be used in experimental designs. The reason for this is because the variability exhibited by the responses in repeated runs will only be due to uncontrolled sources (e.g., measurement error). The repeated runs are then used to estimate the experimental error variance (Mason, Gunst, and Hess; 1989).

Some experiments, just by the nature of the problem, are too time consuming or costly to consider replicating and therefore are run only one time. An example of this would be an experiment concerning oil refineries where it was necessary to stop production when changing between experimental trials. Also, the pressure exerted by management to complete the experiment as quickly as possible and with the minimum

necessary expenses has led to an increasing number of experimental designs without repeated test runs or replication (Anderson and McLean, 1974).

Another consideration is the number of factors under investigation. Usually there will be more factors to be studied than the time and budget will allow. Instead of duplicating a three factor experiment it will usually be better to conduct an unreplicated four factor experiment, which will provide additional information on another factor. Or, the analyst may perform a fractional factorial experiment considering five factors and expend the same amount of time and money (Box, Hunter, Hunter, 1978).

2.5. Factorial Design Features

In general, the important features of factorial designs include:

1) the main effects of each factor can be estimated independently of the other factors;
2) the interactions between the factors can be estimated;
3) the effects are determined with maximum precision; and
4) they provide an estimate of the experimental error for determining the significance of the factor effects (Davies, 1971).

The fourth feature listed above mainly concerns the designs which include repeated runs. However, as we have just seen, many experiments are conducted in an unreplicated manner for one reason or another. This need not be a concern to the analyst, as various techniques useful in analyzing unreplicated experiments will be discussed in Chapters 3 and 4.

2.6. Two-Level Designs

In the following analyses, the emphasis will be placed on two-level experimental designs. A two-level design is one in which all of the factors occur at only two levels. Box, Hunter, and Hunter (1978) listed the following reasons for the importance of two-level designs.

1. They require a relatively small number of runs per factor and can demonstrate trends to help guide further studies.
2. They can be augmented to create composite designs, as in response surfaces.
3. They form the basis for two-level fractional factorial designs, which are important in the early stages of the investigation process as they allow an analyst to examine many factors superficially.
4. They can be used as building blocks to match the design's degree of complexity with the problem's sophistication.
5. The interpretation of the responses can be done with simple arithmetic and the use of common sense.

For many years, most analysts felt that two-level factorials were useful only as an exploratory aid. In recent years, however, this belief has changed to where many analysts feel it is more valuable to consider many factors, each at a high and low level, rather than arbitrarily selecting a few factors and examining them at several different levels. One reason for this is that two-level factorial experiments will provide an analyst with experimental evidence, to go along with their theoretical evidence, to eliminate

some of the factors from consideration. The combined proof derived from both theory and experimentation is preferred to only having theoretical evidence. The reasoning behind this is that the actual theory being used may be unknown, but is based upon prior experience. Therefore, by using a two-level factorial design it provides the analyst with an efficient design that is statistically more sound than the theoretical-only approach (Anderson and McLean, 1974).

When the number of factors, k, under consideration is small then a complete factorial experiment can be utilized. However, when considering a two-level design, the number of runs required is 2^k, which increases geometrically in k. The analysis can quickly become unwieldy and expensive in terms of cost and time for large values of k. An alternative that has long been used in industry to help alleviate this problem is the fractional factorial design (Tippett, 1934; Fisher, 1966).

2.6.1. Redundancy in Factorial Designs

Consider the following example from Box, Hunter and Hunter (1978). Suppose we have a two-level design with seven variables. Then a full factorial design requires $2^7 = 128$ runs, which can be used to estimate the following effects.

As Box, Hunter, and Hunter (1978) point out, just because we are able to estimate all of these effects it does not mean that they are significant. Also, the main effects tend to be larger, in absolute magnitude, than 2-factor effects which in turn are larger than 3-factor effects and so on as we move from left to right. As can be expected,

Table 2.6.1.
All Possible Effects in a 2^7 Factorial Experiment

Response Variable	Main Effects	Interactions					
		2-factor	3-factor	4-factor	5-factor	6-factor	7-factor
1	7	21	35	35	21	7	1

there comes a point at which the higher-order interactions become negligible and can be disregarded. This would be similar to a Taylor series expansion where the higher-order terms are ignored. Previous analyses have shown that when a large number of variables are used in a design many of them may not have any distinguishable effects at all. Combining this with the assumption of negligible higher-order interactions, we can say that when k becomes large there appears a redundancy in a 2^k design. This redundancy is both in terms of the number of variables studied and the number of interactions effects estimated (Box, Hunter, & Hunter, 1978). This idea becomes readily apparent when we consider the following scenario.

As previously mentioned, as we increase the number of factors under consideration in a two-level experiment the number of experimental runs quickly exceeds feasibility in terms of time and cost. A full 2^8 factorial design requires 256 runs, but often times only the main effects and two-factor interactions are important to the analyst. After we have accounted for the eight main effects and the 28 two-factor interactions, we have 219 remaining degrees of freedom for the higher-order interactions, which are usually deemed as negligible. This means that we can use the 219 degrees of freedom for our error estimate, which can be viewed as overkill. Thus, we have made 256 runs to estimate only 36 effects (John, 1971).

2.7. Estimating Factor Effects in a Two-Level Design

Three main methods are commonly used to determine estimates for the effects. The first procedure utilizes a table of + and − coefficients, which can be expanded to accommodate various designs. The second procedure relies upon symbolic expressions. The third technique is known as Yates' algorithm and involves simple additions, subtractions, and divisions. To illustrate each of these procedures we consider the 2^3 factorial design pilot plant example from Box, Hunter, and Hunter (1978).

The experiment involved the study of the chemical yield as the response variable. The three factors under investigation were temperature, concentration, and type of catalyst. The first two factors were quantitative, with the third being qualitative. The levels of the factors used were:

A: temperature	160° C = low	and	180° C = high
B: concentration	20% = low	and	40% = high
C: catalyst	Type A = low	and	Type B = high

The "low" levels of the factors will be denoted by a (1), subscript 0, or (−), while the "high" levels are given by a lower case letter, subscript 1 or (+). The three types of notation are shown in Table 2.7.1.

2.7.1. Table of + and − Coefficients

To use the table of + and − coefficients technique we begin by listing a column with $2^3 = 8$ + signs along with the heading "mean". Columns A, B, and C from Table 2.7.1. are then used to create the complete table. After the "mean" column we copy the A and B columns. The next column is AB, which we obtain by multiplying the

Table 2.7.1.
Factor Effect Notations for a 2^3 Factorial Design

Run	A	B	C		A	B	C
1	−	−	−	(1)	0	0	0
2	+	−	−	a	1	0	0
3	−	+	−	b	0	1	0
4	+	+	−	ab	1	1	0
5	−	−	+	c	0	0	1
6	+	−	+	ac	1	0	1
7	−	+	+	bc	0	1	1
8	+	+	+	abc	1	1	1

The 2^3 or 8 treatment combinations are listed in the following table along with the yield in grams.

Table 2.7.2.
Empirical Data for the 2^3 Factorial Design Pilot Plant Example

	A_0				A_1			
	B_0		B_1		B_0		B_1	
C_0	(1)	60	b	54	a	72	ab	68
C_1	c	52	bc	45	ac	83	abc	80

coefficients across the rows in the A and B columns. Next, we list column C and obtain columns AC and BC, in the same manner as we determined AB. Finally, we list column ABC which is found by multiplying the coefficients in columns A, B, and C. The entire table is shown below in Table 2.7.3.

Table 2.7.3.
Signs for Calculating Effects from the 2^3 Factorial Design, Pilot Plant Example

Treatment Combination	mean (1)	A (2)	B (3)	AB (4)	C (5)	AC (6)	BC (7)	ABC (8)	Avg. Yield
(1)	+	−	−	+	−	+	+	−	60
a	+	+	−	−	−	−	+	+	72
b	+	−	+	−	−	+	−	+	54
ab	+	+	+	+	−	−	−	−	68
c	+	−	−	+	+	−	−	+	52
ac	+	+	−	−	+	+	−	−	83
bc	+	−	+	−	+	−	+	−	45
abc	+	+	+	+	+	+	+	+	80
divisor	8	4	4	4	4	4	4	4	

The estimate for the mean is obtained using the signs in the first column,

$$\frac{60+72+54+68+52+83+45+80}{8} = \frac{514}{8} = 64.25$$

The contrast associated with the Catalyst (C) is obtained using the signs in the fifth column,

$$\frac{-60-72-54-68+52+83+45+80}{4} = \frac{6}{4} = 1.5$$

and the contrast associated with the three factor interaction is obtained from the signs in the eighth column,

$$\frac{-60+72+54-68+52-83-45+80}{4} = \frac{2}{4} = .5$$

The complete list of all contrast estimates are shown in Table 2.7.4.

Table 2.7.4.
Calculated Contrast Estimates for the Pilot Plant Example

Contrast		Estimate
Average		64.25
Main Effects		
	Temperature A	23.0
	Concentration B	-5.0
	Catalyst C	1.5
Two-Factor Interactions		
	AB	1.5
	AC	10.0
	BC	0.0
Three-Factor Interaction		
	ABC	0.5

2.7.2. Symbolic Expressions

The symbolic expressions shown in Table 2.7.2. can also be used to determine the contrast estimates. For example, to determine the main effect estimate for factor A, we would average the responses that corresponded to the treatment combinations containing a (i.e. factor A at its "high" level), then average all of the treatment combinations that did not contain a (i.e. factor A at its "low" level), and then find the difference. If we refer to Table 2.7.3, we would subtract the average response for (1),

b, c, and bc (indicated by a $-$ in the A column of Table 2.7.3) from the average response from a, ab, ac, and abc (given by a + sign in Table 2.7.3). Thus, we would obtain

Average yield at "high" level of A

$= \quad 1/4\ (72 + 68 + 83 + 80) = 75.75$

Average yield at "low" level of A

$= \quad 1/4\ (60 + 54 + 52 + 45) = 52.75$

Therefore, the contrast estimate for the main effect of A would be given by $75.75 - 52.75 = 23$.

If we use the symbols that represent the various treatment combinations we would have:

Average of A at "high" level $= 1/4\ (a + ab + ac + abc)$

Average of A at "low" level $= 1/4\ ((1) + b + c + bc)$

Main effect of A $= 1/4\ (a + ab + ac + abc) - 1/4\ ((1) + b + c + bc)$

If we then treat (1), a, b, and c as algebraic symbols the main effect of A can be written as follows:

$$A = 1/4\ \{(a-1) + (ab-b) + (ac-c) + (abc-bc)\}$$

$$= 1/4\ (a-1)\ (b+1)\ (c+1)$$

It must be stressed that this expression is only symbolic and cannot be used in its compact form with numbers. It must first be expanded before any numerical results can be substituted into the equations (Davies, 1971).

These results can be expanded to include all of the contrast estimates. Note that for the contrast estimate A only the a term had a - 1, while both b and c were added to 1. This generalization holds true for all of the other effect estimates, where the factor(s) appearing will have a $-$ 1 term, while the other factor(s) have a + 1 term. For instance, the contrast estimate of the AB interaction will be $AB = 1/4\ (a-1)(b-1)(c+1)$. Thus, for a 2^n factorial design we would have:

$$A = (1/2)^{n-1}\ (a-1)(b+1)(c+1)\ldots(n+1)$$

$$AB = (1/2)^{n-1}\ (a-1)(b-1)(c+1)\ldots(n+1)$$

$$ABC\ldots N = (1/2)^{n-1}\ (a-1)(b-1)(c-1)\ldots(n-1).$$

(Davies, 1971).

2.7.3. Yates' Algorithm

In order to apply Yates' algorithm to obtain the factor effect estimates in a two-level design, the observations must be placed in standard order. The first column of the matrix has successive $-$ and + signs, the second column has successive pairs of $-$ and + signs, and so on. In general, the k^{th} column has 2^{k-1} $-$ signs followed by 2^{k-1} + signs (Box, Hunter, and Hunter; 1978). This can be seen in Table 2.7.1. under the A, B, and C headings. The computations for Yates' algorithm are as follows:

1. Generate a new column (1), whose first k/2 entries are the sums of successive pairs of responses and whose next k/2 entries are the differences

   second response $-$ first response

   for the two responses in each pair.

2. Using the newly created column (1), apply the procedure described in step 1 to create a column (2).

3. Using each newly created column, continue the pattern of additions and subtractions of successive pairs of entries until a total of k new columns have been generated.

4. Divide the first entry in column (k) by 2^k, while dividing the remaining entries in the column by 2^{k-1}. This column consists of the constant effect, main effects, and interactions.

5. Identify the factor effects by noting the coded factor levels in the first k columns of the table. The row of $-$ signs in the first k columns denotes the overall constant effect (overall average) of the responses. Each of the next k rows contains a single $+$, indicating the main effect for that factor. The next k(k-1)/2 rows have two factors with $+$ values, indicating a two-factor interaction. Continue in this manner until all main effects and interactions have been identified, Mason, Gunst, and Hess (1989).

The calculations for Yates' Algorithm for the pilot plant example are shown in Table 2.7.5.

2.8. Fractional Factorial Designs

Situations such as the one described in section 2.6.1. have encouraged analysts to examine the use of fractional factorial experiments. In these experiments only a fraction of the total number of runs are performed, which can result in the savings of a great deal of money and time. For example, the cost of conducting an experimental run

Table 2.7.5.
Yates' Algorithm for the Pilot Plant Example

Factors									
A	B	C	Yield	(1)	(2)	(3)	divisor	estimate	identification
−	−	−	60	132	254	514	8	64.25	Average (1)
+	−	−	72	122	260	92	4	23.0	a
−	+	−	54	135	26	-20	4	-5.0	b
+	+	−	68	125	66	6	4	1.5	ab
−	−	+	52	12	-10	6	4	1.5	c
+	−	+	83	14	-10	40	4	10.0	ac
−	+	+	45	31	2	0	4	0.0	bc
+	+	+	80	35	4	2	4	0.5	abc

at an oil refinery may be several thousand dollars, so (in terms of short-term profitability) the fewer required runs the better (John, 1971).

Fractional factorial designs are also used extensively in screening experiments, where many factors are initially considered, but it is believed that relatively few of them will have a significant impact on the response variable. An interesting example described by John (1971) involved a new cake mix. Before sending the cake mix to market, the company must examine the effects of many factors on the final product. Some of the factors to be considered are baking temperature, baking time, and the size of the eggs used in the mix. The screening experiment is performed in hopes of determining which factors affect the cake adversely. The results of the screening experiment can then be used to guide the direction of further study of the factors deemed to be significant.

Daniel (1976) described the advantages and disadvantages of using a fractional factorial design. Advantages included being able to examine the factor effects of interest over a more expansive range of operating conditions and decreasing the number of runs required to study the main effects and two-factor interactions. The main disadvantages were the lack of degrees of freedom for testing for lack of fit and the increased vulnerability to outliers, errors in recording, and problems in testing conditions.

Daniel (1976) also points out an important fact about 2^p designs. Every 2^p factorial design is a fraction of a larger experiment 2^P ($P > p$) where some of the factors were not varied. The problem with this scenario is that some of the factors held constant may not have been included at their best level. In this way, the analyst must be well informed about the experiment in order to decide which factors to include in the study. It may be possible to include some of the assumed insignificant factors and not increase the workload much, if any. Quoting from Daniel (1976) page 198,

> Put still another way, we may be able to broaden the base of our inferences about the effects of the p important factors by varying some other factors which "probably" produce no effects. We do not know that these latter factors are uninfluential; we only hope that they are. If our data show that they are indeed negligible, a point has been gained. If, on the other hand, one or more of them do influence results, an even more important fact has been learned.

2.8.1. Obtaining the Fraction to Analyze

Consider the two-level factorial design with 5 variables (2^5) Reactor Example from Box, Hunter, & Hunter (1978). A complete design would require $2^5 = 32$ runs. However, as mentioned in an earlier section, when k becomes large there becomes a redundancy in the experiment. Suppose also that finances allowed only for a maximum

of 16 runs to be performed. How would we decide which 16 runs from Table 2.8.1. should be used in order to obtain relevant information?

The 16 runs denoted with an asterisk in Table 2.8.1., represent a half-fraction of the complete design. This is usually denoted as a 2^{5-1} fractional factorial design since

$$(1/2)\ 2^5 = 2^{-1}\ 2^5 = 2^5\ 2^{-1} = 2^{5-1}$$

This notation identifies the design as having five variables, each at two levels, but with only $2^{5-1} = 2^4 = 16$ runs used in the analysis.

The question of which 16 runs to choose can be answered as follows. First, we write a full 2^4 design for variables 1, 2, 3, and 4. Second, we write the column of signs for the 1234 interaction, which are used to define the levels of variable 5 = 1234. Applying this procedure to the data in Table 2.8.1. provides us with Table 2.8.2. (Box, Hunter, & Hunter; 1978).

2.8.2. Confounding

A frequently asked question concerning fractional factorials is, "By analyzing only a fraction of the runs has any information been lost?" An examination of Table 2.8.2. shows that we have made 16 runs, which can be used to estimate 16 quantities: the mean, 5 main effects, and 10 two-factor interactions. The quantities (as determined by Yates' algorithm) are shown in Table 2.8.3. (Box, Hunter, & Hunter; 1978). But what has happened to the other 16 effects from the full factorial with 32 runs?

To answer that question we can estimate the three-factor interaction 124. If we multiply columns 1,2, and 4 in Table 2.8.2. we obtain the following (written as a row).

124 = - + + - - + + - + - - + + - - +.

Table 2.8.1.
2^5 Factorial Design, Reactor Example

	Variable					Response
Run	1	2	3	4	5	Y
1	-	-	-	-	-	61
*2	+	-	-	-	-	53
*3	-	+	-	-	-	63
4	+	+	-	-	-	61
*5	-	-	+	-	-	53
6	+	-	+	-	-	56
7	-	+	+	-	-	54
*8	+	+	+	-	-	61
*9	-	-	-	+	-	69
10	+	-	-	+	-	61
11	-	+	-	+	-	94
*12	+	+	-	+	-	93
13	-	-	+	+	-	66
*14	+	-	+	+	-	60
*15	-	+	+	+	-	95
16	+	+	+	+	-	98
*17	-	-	-	-	+	56
18	+	-	-	-	+	63
19	-	+	-	-	+	70
*20	+	+	-	-	+	65
21	-	-	+	-	+	59
*22	+	-	+	-	+	55
*23	-	+	+	-	+	67
24	+	+	+	-	+	65
25	-	-	-	+	+	44
*26	+	-	-	+	+	45
*27	-	+	-	+	+	78
28	+	+	-	+	+	77
*29	-	-	+	+	+	49
30	+	-	+	+	+	42
31	-	+	+	+	+	81
*32	+	+	+	+	+	82

Table 2.8.2.
Half-Fraction of a 2^5 design: A 2^{5-1} Fractional Factorial Design, Reactor Example

	Variable															Response
Run	1	2	3	4	5	12	13	14	15	23	24	25	34	35	45	Y
17	-	-	-	-	+	+	+	+	-	+	+	-	+	-	-	56
2	+	-	-	-	-	-	-	-	-	+	+	+	+	+	+	53
3	-	+	-	-	-	-	+	+	+	-	-	-	+	+	+	63
20	+	+	-	-	+	+	-	-	+	-	-	+	+	-	-	65
5	-	-	+	-	-	+	-	+	+	-	+	+	-	-	+	53
22	+	-	+	-	+	-	+	-	+	-	+	-	-	+	-	55
23	-	+	+	-	+	-	-	+	-	+	-	+	-	+	-	67
8	+	+	+	-	-	+	+	-	-	+	-	-	-	-	+	61
9	-	-	-	+	-	+	+	-	+	+	-	+	-	+	-	69
26	+	-	-	+	+	-	-	+	+	+	-	-	-	-	+	45
27	-	+	-	+	+	-	+	-	-	-	+	+	-	-	+	78
12	+	+	-	+	-	+	-	+	-	-	+	-	-	+	-	93
29	-	-	+	+	+	+	-	-	-	-	-	-	+	+	+	49
14	+	-	+	+	-	-	+	+	-	-	-	+	+	-	-	60
15	-	+	+	+	-	-	-	-	+	+	+	-	+	-	-	95
32	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	82

By examining Table 2.8.2. we see that this is the same as the 35 column. In this way, 124 = 35 and we say that the three-factor interaction 124 is confounded with the two-factor interaction 35. Some authors refer to this as 124 and 35 being aliases of each other (Cochran and Cox, 1957). When we determine the contrast estimate for the 35

Table 2.8.3.
Effect Estimates for the Reactor Example

Mean = 65.25	12 = 1.5
1 = -2.0	13 = 0.5
2 = 20.5	14 = -.75
3 = 0.0	15 = 1.25
4 = 12.25	23 = 1.50
5 = -6.25	24 = 10.75
	25 = 1.25
	34 = 0.25
	35 = 2.25
	45 = -9.50

interaction we will be estimating the sum of the mean values of effects 35 and 124. This can be represented as $E_{35} \rightarrow 35 + 124$. The resulting confounding pattern for the entire reactor example is shown in Table 2.8.4. (Box, Hunter, & Hunter; 1978).

In general, we can characterize a fractional factorial design based on its generator. In the previous example the generator was **5 = 1234**. By definition, any number multiplied by **I** is just that number and any number multiplied by itself is **I**. When we multiply each side by **5** we obtain $\mathbf{5^2 = I = 12345}$. As a measure of checking this we multiply the signs in columns **1,2,3,4, & 5** and obtain a column of (+) signs, which is defined as **I**. The relation **I=12345** is known as the defining relation. If we multiply a column by the defining relation we obtain its alias. For example, if we multiply the defining relation by 13 we obtain 13 = 245. Similarly, we can obtain the remaining relations shown in the first column of Table 2.8.4. (Box, Hunter, & Hunter; 1978). Note that we could have used the complementary half-fraction by setting I=-12345 as our defining relation. If we had used this generator we would have obtained the half-

Table 2.8.4.
Confounding Pattern for 2^{5-1} Reactor Example

Relation	Confounding Pattern	Estimate
1 = 2345	$E_1 \rightarrow 1 + 2345$	E_1 = -2.0
2 = 1345	$E_2 \rightarrow 2 + 1345$	E_2 = 20.5
3 = 1245	$E_3 \rightarrow 3 + 1245$	E_3 = 0.0
4 = 1235	$E_4 \rightarrow 4 + 1235$	E_4 = 12.25
5 = 1234	$E_5 \rightarrow 5 + 1234$	E_5 = -6.25
12 = 345	$E_{12} \rightarrow 12 + 345$	E_{12} = 1.5
13 = 245	$E_{13} \rightarrow 13 + 245$	E_{13} = 0.5
14 = 235	$E_{14} \rightarrow 14 + 235$	E_{14} = -0.75
15 = 234	$E_{15} \rightarrow 15 + 234$	E_{15} = 1.25
23 = 145	$E_{23} \rightarrow 23 + 145$	E_{23} = 1.5
24 = 135	$E_{24} \rightarrow 24 + 135$	E_{24} = 10.75
25 = 134	$E_{25} \rightarrow 25 + 134$	E_{25} = 1.25
34 = 125	$E_{34} \rightarrow 34 + 125$	E_{34} = 0.25
35 = 124	$E_{35} \rightarrow 35 + 124$	E_{35} = 2.25
45 = 123	$E_{45} \rightarrow 45 + 123$	E_{45} = -9.50
I = 12345	Average	Avg. = 65.25

fraction that corresponded to the runs in Table 2.8.1. that are not marked with an asterisk.

Cochran and Cox (1957) stated three rules concerning aliases for two-level designs. These rules were:

1) In a 2^n design, the alias of any factorial effect is its generalized interaction with the defining contrast.

2) In a 2^n design, any two factorial effects may be used as defining contrasts to divide a factorial experiment into quarter fractions. Their generalized interaction also acts as a defining contrast and cannot be estimated from the quarter fraction.

3) In a quarter fraction of a 2^n design, any factorial effect that is not a defining contrast has three aliases. These are its generalized interactions with the three defining contrasts.

These three rules can be illustrated as follows. First, consider a 2^3 factorial with a defining contrast of ABC. Then the alias of B is the generalized interaction ABBC or AB^2C. Since any squared term is equal to I we have $AB^2C = AC$. Second, consider a 2^6 factorial. The generalized interaction of ABCDEF and ABCDE is $A^2B^2C^2D^2E^2F$ or just F. Finally, consider the aliases of A, which are its interactions with ABCDEF, ABCDE, and F, i.e. BCDEF, BCDE, and AF (Cochran & Cox, 1957).

2.8.3. Design Resolution

The design resolution of a given fractional factorial design helps to identify the order of confounding of main effects and interactions. Mason, Gunst, and Hess (1989) define the design resolution as follows: An experimental design is of resolution R if all effects containing s or fewer factors are unconfounded with any effects containing fewer than R-s factors.

Consider the example shown in Table 2.8.4. It is readily apparent that main effects are confounded with four-factor interactions and the two-factor interactions are confounded with three-factor interactions. Therefore we would have a 2^{5-1} design of resolution V.

In general, the resolution of a two-level design is the length of the shortest defining contrast. Also, it has been shown that:

A) A design of resolution III does not confound main effects with one another, but does confound main effects with two-factor interactions.

B) A design of resolution IV does not confound main effects and two-factor interactions, but does not confound two-factor interactions with other two-factor interactions.

C) A design of resolution V does not confound main effects and two-factor interactions with each other, but does confound two-factor interactions with three-factor interactions and so on (Box, Hunter, & Hunter; 1978).

The importance of design resolution is that it provides an analyst with a fast method of determining if a particular experimental design allows all of the important effects to be estimated without confounding them with one another. As previously stated, the important effects of interest to the analyst are assumed to be the main effects and low-order interactions (Mason, Gunst, and Hess; 1989).

2.9. Plackett-Burman Designs

Plackett and Burman (1946) discussed a group a two-level orthogonal designs where the number of runs is always a multiple of 4. Whenever a Plackett-Burman design has its number of runs as a power of 2 (8, 16, 32, ...) it is referred to as geometric and can be converted into a 2^{k-p} design. Box and Meyer (1993) pointed out that we can use the geometric Plackett-Burman design with 2^{k-p} runs to generate a 2^{k-p} design by choosing the appropriate columns from the Plackett-Burman design. However, some of the + and - signs may have to be switched in some columns.

When a Plackett-Burman designs has a number of runs that is not a power of 2 (12,20,24,...) it is known as a non-geometric design. These designs have a more complicated alias structure than do geometric Plackett-Burman designs. Box and Meyer (1993) described the complexity of non-geometric Plackett-Burman designs and noted that a particular alias term may be associated with many different contrasts along with having a fraction as its coefficient.

As an example, consider a 12-run Plackett-Burman design. Then the contrast estimate for the main effect of any factor will have in its alias estimate all of the two-factor interactions not involving that factor, with each of these having a fractional coefficient (Box and Meyer, 1993). The alias pattern generation scheme was provided in Box and Wilson (1951), while Box (1952) described the necessary relationship between the coefficients of the aliases for all orthogonal designs.

CHAPTER 3. COMPARISON OF PREVIOUS TECHNIQUES USED FOR DETERMINING ACTIVE EFFECTS IN AN UNREPLICATED FACTORIAL DESIGN

One of the most important aspects of fractional factorial designs is how to estimate the experimental error variance. There are several techniques available to analysts in this situation. The experimenter may be able to make an educated guess of the value of the error variance, σ^2. The problem with this technique, however, is that the accuracy of these prior estimates tends to differ greatly based upon the experimental circumstances (Davies, 1971).

When the observations are costly, as is the case in many industrial experiments, but the experimental error is small, the designs most often used include a large number of factors that are analyzed at the same time. In these types of experimental designs most of the comparisons among the observations will be used for determining the factor effect estimates. The problem encountered in this situation is that there may only be a few or no comparisons at all that measure the experimental error (Davies, 1971).

When an experiment is genuinely replicated under a certain set of testing conditions, the variation between the associated observations can be used to estimate the variance of a single observation and therefore the variance for the effects. When genuine replicates are used then the variation between runs made at the same testing conditions reflects the total variability for runs that are performed at different testing conditions (Box, Hunter, and Hunter; 1978).

So when we have a fractional factorial design that is replicated we can obtain an estimate of the experimental error variance based on the replicates. But what can be

used as an estimate for the error variance when the experiment is unreplicated? In this chapter, several different methods for analyzing unreplicated experiments will be examined.

3.1. Factor Sparsity

While each of the following methods vary slightly in their underlying theory, they all rely on the assumption of factor sparsity. In most screening designs, the analyst assumes that the large effects that are desired to be detected will actually arise from only a small number of the factors being analyzed. The large effects are referred to as "active" effects, while the other are deemed to be "inert." The assumption of factor sparsity is related to the Pareto principle, which states that only a small percentage of the variables involved in the process will account for a disproportionate amount of the process variation (Box and Meyer, 1986a and 1986b).

3.2. Daniel's Normal Probability Plotting Procedure

The first method useful in determining active effects in an unreplicated experimental design is attributed to Daniel (1959, 1976). The procedure involves a normal probability plot of the effect estimates to determine whether or not an effect is active or inert. Daniel's 1959 paper involved the use of a half-normal plot, but more recently, in his 1976 book, Daniel advocates the use of a complete normal plot. The apparent difference between the two procedures is worth noting.

In his 1976 book, Daniel examined three different 2^5 designs with a half-normal plot and compared the results with his prior analysis. The half-normal plots were found to not reveal any of the peculiarities found in the prior analyses. The reason for this,

Daniel points out, is the fact that the defects found were all very sign dependent and these were obscured by overaggregation in the half-normal plots.

Daniel presented the following sermon, prompted by the failures of half-normal plots. It reads as follows, "Do not ever assume that a statistic aggregated over a whole data set is distributed as required by some unverified assumptions. The homogeneity of the parts of an aggregate can be tested before or after the aggregation, but such testing must be done before conclusions can be drawn from the experiment" (Daniel, 1976). Because of this situation, we will only examine Daniel's use of full-normal probability plots.

3.2.1. Description of Daniel's Technique

When using a normal probability plot, the horizonal axis of the plot consists of the values of the contrast estimates, while the vertical axis lists the expected normal values if the data were actually normally distributed. If the data were from a normal distribution then the normal probability plot should follow a straight line through the origin. Those effects that lie on the line are assumed to be inert and distributed normally with a mean of zero and constant variance. Those points that deviate from the straight line significantly are considered to be associated with active effects.

The procedure for constructing a normal probability plot is as follows:

1) Obtain estimates of the n effects using Yates' algorithm.
2) Order the n effects from smallest to largest, $x_{(1)} \leq x_{(2)} \leq \ldots \leq x_{(n)}$.
3) Compute the i^{th} expected normal value, $y_{(i)}$, using the normal distribution function

$$P[z \le y_{(i)}] = \int_{-\infty}^{y(i)} \frac{1}{\sqrt{2\pi}} e^{-\frac{z^2}{2}} dz = (i-\frac{3}{8})/(n+\frac{1}{4}).$$

The above probability is the probability of observing a value $z \le y_{(i)}$ when we have a standard normal distribution.

4) Plot the data pairs $(x_{(i)}, y_{(i)})$

5) Draw a straight line that passes through the origin, which best describes the data.

6) Assess the fit of the line by determining which factor effect estimates fall far enough off the line to be deemed active.

When drawing the straight line that best fits the data, we must force the line to go through the point (0,0) as stated in step 5. This point corresponds to the 50th percentile of the normal distribution on the y-axis. Daniel (1976) states that the data points near the 16^{th} and 84^{th} percentiles of the normal distribution should be given a larger influence in the determination of the slope of the line.

3.2.2. Variations in Plotting Positions

Nelson (1982) describes the motivation for the determination of the plotting positions y_i. The smallest of the n observations relates to the first (100/n)% of the distribution, which is the amount of the distribution between 0 and (100/n)%. The midpoint for this interval represents the first plotting position that is given by the formula $y_{(1)}$ = [100(1-.5)/n]%. In the same way, the next smallest observation corresponds to the next (100/n)% of the distribution, which is the amount of the distribution between

(100/n)% and [100(2/n)]%. The second plotting point is given by the midpoint of this interval and is given by $y_{(2)}$ = [100(2-.5)/n]%. We would continue in this same manner until we have the n^{th} plotting position $y_{(n)}$ = [100(n-.5)/n]%.

The formula shown in step 3 for the determination of the expected normal value y_i is one of several that have been used to construct normal probability plots. Nelson (1982) notes that different plotting positions have increased over the years, among them the variations proposed by Blom (1958), Harter (1969), and Tukey (1962).

The "mean" plotting position has been used quite extensively and is given by $y_{(i)}$ = i/(n+1). Tables providing the corresponding values to this formula are listed in King (1971). Note that these values are the expected (mean) percentage of the sample below the i^{th} ordered observation. Another plotting position receiving consideration is known as the "median" plotting position. Johnson (1964) described the median plotting positions in detail and provided tables based upon $y_{(i)}$ = (i-.3)/(n+.4), which had been shown to be a good approximation by Bernard and Bos-Levenbach (1953).

With all of the various plotting positions available, an analyst must decide which one to use when creating a normal probability plot. However, as Nelson (1982) reiterated, the choice of the plotting positions did not differ much when compared to the overall randomness of the data. Therefore, when creating a normal probability plot one can choose whichever formula is easier. This is why the formula (i - 3/8) / (n + 1/4) was chosen to be used in step 3. Also, this formula is used in various statistical packages since it is regarded as providing a good estimate for the normal distribution.

3.2.3. Advantages and Disadvantages of Daniel's Procedure

The advantage of the normal probability plot lies in its simplicity. It is straightforward and provides results quickly. As Daniel (1959) suggests, plotting the data can shed light upon possible model inadequacies. Thus, the technique should always be used during the preliminary analysis. However, the major disadvantage of the technique lies in the interpretation of the plot. The analyst's subjectivity plays an important role since the analyst must determine what denotes a significant departure from the straight line in order to deem an effect active. More importantly, the subjectivity associated with determining the slope of the line through the data will affect the decisions made. Given the same normal probability plot, different analysts will most likely draw lines with different slopes. Also, because of the lack of a clear cut decision rule, it is impossible to assess the power of Daniel's procedure.

3.3. Box and Meyer's Bayesian Procedure

Box and Meyer (1986a, 1993) proposed a bayesian approach for determining the significance of factor effects. All effects are assumed to arise from normal distributions with a mean of 0, but the active effects come from a distribution that has a larger variance than the inactive effects. Based upon the prior probability that an effect is active, the posterior probability that the effect comes from the distribution with the larger variance is computed. If the posterior probability of an effect is close to 1, then it is deemed to be active, while if it is close to 0 it is deemed inactive.

3.3.1. Description of Box and Meyer's Technique

Box and Meyer listed several assumptions for their procedure. These included the following:

1) For each effect, c_i, $i = 1,\ldots, n$ the prior probability that it is active is α,

2) The active effects are independent and identically distributed as $N(0, \sigma_c^2)$,

3) The inert effects are distributed as $N(0, \sigma^2)$.

4) The inflation factor of the standard deviation for an active effect is k.

Then by using an orthogonal array to obtain the vector of estimated effects, $\mathbf{C} = (C_1,\ldots, C_n)$, and standardizing them, so that given c_i the estimated effects all have the same, unknown variance. Box and Meyer (1986a) have shown that if we let $k^2 = (\sigma^2 + \sigma_c^2)/\sigma^2$, then the C_i, $i = 1, \ldots, n$ can be viewed as iid from a scale-contaminated normal distribution given by $(1-\alpha)N(0, \sigma^2) + \alpha N(0, k^2\sigma^2)$. Thus, with probability $1-\alpha$ an effect is distributed as $N(0, \sigma^2)$ and with probability α the effect is distributed as a $N(0, k^2\sigma^2)$. The posterior probability, p, that an effect comes from the $N(0, k^2\sigma^2)$ distribution is then computed and when it is close to 1 then the effect is deemed to be active.

3.3.2. Estimation of Parameters in Box and Meyer's Procedure

The values of the parameters used in Box and Meyer's procedure, α, the prior probability that an effect is active and k the inflation factor for the standard deviation of the active effects, are not just merely guesses, but were estimated based on prior information. Box and Meyer studied 10 examples of 16 and 32 run two-level experiments (see Table 1, Box and Meyer 1986a) in order to determine a range of values

for α and k. The range for α was from .13 to .27, with an average of .20, and for k the range was between 2.7 and 18, with an average of 10. This is why they suggest the use of $\alpha = .2$ and $k = 10$.

3.3.3. Advantages and Disadvantages of Box and Meyer's Procedure

When Box and Meyer introduced their procedure in 1986, it was viewed not as an alternative to Daniel's procedure, but rather as an adjunct to plotting. This was indeed an advantage since now there was a procedure that could be combined with the plotting. However, as with Daniel's plotting procedure, Box and Meyer's procedure suffers slightly from its inherent subjectivity on the part of the analyst.

The disadvantages for Box and Meyer's procedure include the following. First, the prior probability that an effect is active, α, must be specified. Second, the inflation factor of the standard deviation, k, for active effects must also be specified. Finally, there is no exact cutoff point for deciding whether or not an effect's posterior probability is close to 1 and can be deemed active. Is .5 close enough or does it need to be at least .75?

3.4. Benski's W'-test and Fourth-Spread Test Procedure

Benski (1989) proposed a 2-step procedure based on similar assumptions as Daniel's and Box and Meyer's methods, which is that the non-significant effects come from a normal distribution with a zero mean and the significant effects are viewed as "contaminants" from this distribution. Benski claims that while both Daniel's Probability Plotting technique and Box and Meyer's technique require subjectivity on the analyst's

behalf, his procedure does not. However, it will be shown that there does exist a small part of subjectivity in the procedure.

The first step of the procedure involves the W'-test of normality proposed by Shapiro and Wilk (1965) but in the modified form of Olsson (1979). The second step of the procedure involves the fourth-spread test, which is used to determine the presence of outliers. The fourth-spread test was chosen because it uses a robust estimate of the spread, which has been shown to be insensitive to many inert effects mixed together with a few active effects (Benski, 1989).

3.4.1. Description of Benski's Technique

Benski's entire procedure is performed as follows:

1) The treatment combinations are arranged in standard order and Yates' algorithm is applied to obtain estimates of the effects. The first element in the last column of Yates' algorithm can be discarded since it is associated with the average.

2) Using the column of data derived for Yates' algorithm, perform Olsson's modification of the W'-test for normality. Determine the significance level of the W'-test, P_1.

3) If P_1 is small, then apply the fourth-spread test for the detection of outliers. Determine if the largest estimated effect (in absolute value) is significant. If it is go to step 5, if not go to step 6.

4) If P_1 is not small then go to step 6.

5) Remove the largest estimated effect (in absolute value) from the column and using the remaining effects, go to step 2.

6) List all of the estimated effects that were found to be significant in step 3 and stop.

Benski (1989) notes that any test of normality could be used in step 2, but chooses Olsson's (1979) modification of the Shapiro and Wilk (1965) W'-test because of its ease of use and omnibus power (see Shapiro (1980) and Lin and Mudholder (1980)). Also, the procedure does not require the use of any tables. The entire procedure is outlined in Appendix A. Note that Olsson's original routine had a few errors, which were later corrected by Olsson (1981).

When P_1 is small, we proceed to the fourth-spread test, as described in step 3. Instead of using the usual estimate of the sample standard deviation, the fourth-spread or d_F is used, which had been described by Hoaglin (1983). Hoaglin showed that the fourth-spread is a robust estimate of the spread of the noise population.

In order to determine the fourth-spread, d_F, the effect estimates must be sorted from smallest to largest. Ranks are then assigned to the data in a similar manner as used in nonparametric procedures. In this way, tied ranks need to be considered. The depth of the median is then defined as its rank in the ordered data set. Once the depth of median has been found, we can use it to determine the depth-of-fourths, where depth-of-fourths=([depth-of-median]+1)/2, and the brackets imply the truncated, integer value of the depth-of-median. Since each of the fourths is halfway between the median and one of the extreme points, we can define the data values that correspond to these

fourths as F_L (lower fourth) and F_U (upper fourth). Note that in some instances the data values that correspond to the upper and lower fourths must be determined by interpolation.

Once the data values of the upper and lower fourths are obtained, we define the fourth-spread as the difference between the upper and lower fourths, $d_F = F_U - F_L$. Using the fourth-spread as a measure of dispersion, the interval $[-2d_F, 2d_F]$ is computed. The largest estimated effect (in absolute value) is then compared to the interval. If the effect lies outside the interval it is deemed to be significant, removed from the column of effects, and the procedure moves back to the W'-test. Otherwise, all remaining effects are deemed to be inert and the procedure stops.

3.4.2. Advantages and Disadvantages of Benski's Procedure

Advantages of Benski's procedure are that this procedure does not require any subjective assessment as to a departure from a straight line, as in Daniel's technique, or with the assignment of prior probabilities, as in Box and Meyer's procedure. Also, no tables are required to determine significance of effects and numerically it is less computationally intensive than the numerical integration of the bayesian procedure. However, there is one oversight on Benski's part.

Nowhere does he define what would be considered a small value of P_1, the significance level of the W'-test. Should we consider a value of $P_1 = .10$ as small? Or is $P_1 \leq .05$ small? A clarification of the significance level used in the procedure should be stated as to avoid any misunderstandings or misinterpretations of the effects.

3.5. Lenth's Pseudo Standard Error Procedure

Lenth (1989) introduced an alternative to Box and Meyer's bayesian procedure, which is based upon the pseudo standard error of the contrast estimates. As with Box and Meyer's procedure, Lenth advocates the plotting of the contrasts. However, the plot is based upon the actual contrast values, which allows the analyst to see the size and "significance" of the contrasts at the same time.

The procedure involves the determination of both inner and outer limits for the plot of the contrasts. A contrast that lies outside the outer limits is deemed to be active, while a contrast that lies within the inner limits is deemed to be inert. A contrast that lies between the inner and outer limits may or may not be active.

3.5.1. Description of Lenth's Technique

The underlying assumptions that Lenth made were similar to the previously described methods of Daniel and Box and Meyer. The contrasts were denoted by k_i, while their estimates were denoted by c_i, for $i = 1,\ldots,m$. The sampling distributions of the c_i are approximately normal, with possible different means, k_i, but with equal variances τ^2. Thus, the c_i can be described in the usual notation by, $c_i \sim N(k_i, \tau^2)$.

Lenth's procedure can be summarized as follows:

1) Determine the contrast estimates using Yates' algorithm.

2) Define $s_o = 1.5 \text{ x median } |c_j|$ and the pseudo standard error

   $PSE = 1.5 \text{ x median } |c_j|$ defined on the set $|c_j| > 2.5 \text{ x } s_o$.

3) Define the margin error $ME = t_{.975;d} \text{ x } PSE$, where $d = m/3$.

4) Define the simultaneous margin of error SME $= t_{1-\alpha/2;d}$ x PSE, where $\alpha = 1\text{-}(.95)^{1/m}$.

5) Construct a plot with lines drawn at $\pm$ ME and $\pm$ SME. Plot the contrast estimates on the graph.

6) Contrasts that lie outside the $\pm$ SME lines are deemed active. Contrasts that lie within the $\pm$ME lines are deemed inert. Contrasts that lie between the ME and SME lines may or may not be active.

Lenth's definition of $s_o = 1.5$ x median $|c_j|$ and the pseudo standard error (PSE) of the contrasts as PSE $= 1.5$ x median $|c_j|$, where $|c_j| < 2.5s_o$ in step 2 illustrates that the only difference between s_o and the PSE is the fact that the median used to define s_o was based on all the observations, while the median used for the PSE is based upon a limited group of the $|c_j|$'s.

The PSE in then used to determine the inner and outer limits. The margin of error (ME) for the contrast estimates is given in step 3 as ME$=t_{.975;d}$ * PSE, where $t_{.975;d}$ is the 97.5 percentile of the t-distribution based on d degrees of freedom. Lenth determined a conservative estimate for d as the number of contrasts divided by 3, m/3, through empirical studies of the distribution of the PSE[2]. A 95% confidence interval for the ith contrast, k_i, could be determined by using $c_i \pm$ ME. The inner limits were thus defined by $\pm$ ME.

Outer limits were determined in step 4 by Lenth because of the following situation. Due to the number of contrasts involved, usually 15 or 31, but possibly more, we will be making many inferences at the same time and we could expect there to be

some false alarms where an inactive contrast will lie outside the $\pm$ ME limits. That is why Lenth computed the simultaneous margin of error (SME), which would account for the many simultaneous inferences. The SME was defined in step 4 as follows: SME $= t_{1-a/2;d}$ x PSE, where $\alpha = 1\text{-}(.95)^{1/m}$. The outer limits were then defined as $\pm$ SME.

In step 5, the contrast estimates can then be shown on a graph similar to a Bayes plot or the analysis-of-means plot proposed by Ott (1967) along with the lines corresponding to the inner limits ($\pm$ ME) and the outer limits ($\pm$ SME). Finally, as given in step 6, if the contrast estimate extends beyond the SME line then it is deemed to be active. Likewise, a contrast estimate that falls within the $\pm$ME lines is deemed to be inert. However, when the contrast lies between the ME and SME lines it is in a grey area of uncertainty. The contrast could be active or maybe it is inert, we do not know for sure.

3.5.2. Advantages and Disadvantages of Lenth's Procedure

Lenth's procedure possesses several advantages, but one main disadvantage. The advantages include the ease of computation, the graphical display of the contrast along with its numerical value, and the distinct cutoffs provided for determining whether or not a contrast is active or inert.

The main disadvantage, as highlighted previously, is the conclusion reached when a contrast lies between the ME and SME lines. As Lenth states, "a good argument can be made both for its being active and for its being a happenstance result of an inactive

contrast." Thus, we cannot reach a firm conclusion for any contrast that lies in this grey area.

3.6. A Published Example Comparing the Four Techniques

The following example was taken from Schneider, Kasperski, and Weissfeld (1993) and was originally presented by Quinlan (1985). It is described in detail in section 4.1.1., so only the table of effect estimates is reproduced here.

Table 3.6.1. Effect Estimates Based on the Log Transform for Quinlan's Example

Estimate of Contrasts	
μ=	-1.430
E1=	0.168
E2=	0.239
E(12)=-E5	0.119
E3=	-0.028
E(13)=-E6	-0.046
E(23)= E8	0.212
E(123)= E11	-0.102
E4=	0.222
E(14)= -E7	0.084
E(24)= -E9	0.882
E(124)= E12	-0.020
E(34)=-E10	0.317
E(134)= E13	0.309
E(234)= E14	-0.604
E(1234)=-E15	0.025

Applying Daniel's normal probability plotting procedure to the effect estimates yields the plot shown in Figure 3.6. As can be seen from the plot only effects 9 and 14 lie well off the line, which would indicate that most likely they are the only significant effects. In 1988, Box applied the bayesian procedure and likewise, concluded that only effects 9 and 14 were significant, with posterior probability of roughly .96 and .67. All of the remaining effect estimate's posterior probabilities were less than .15, which would lead one to believe that they were not significant.

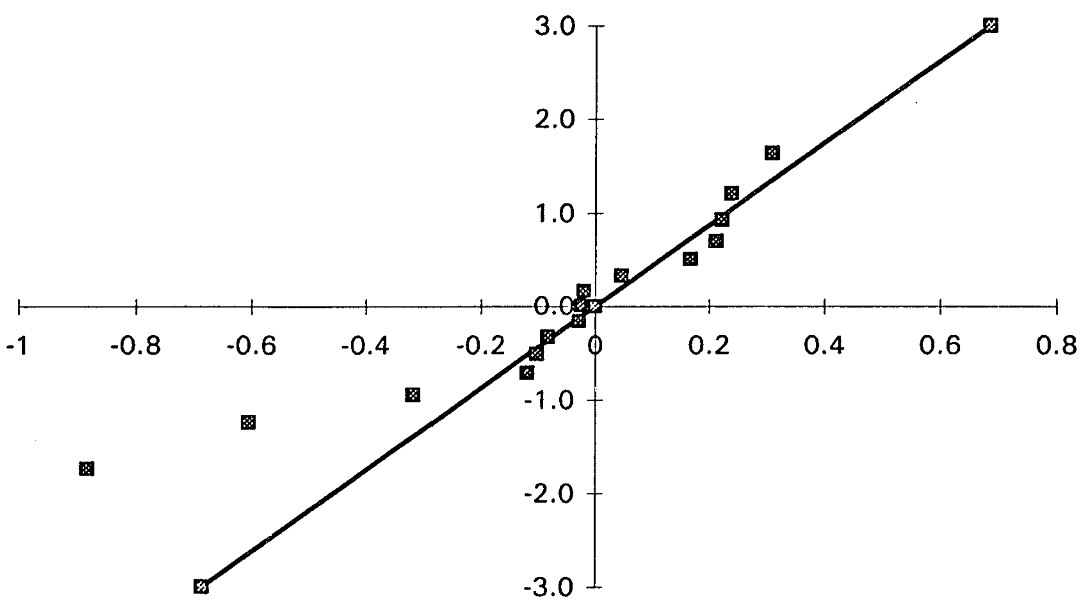

Figure 3.6.
Normal Probability Plot of Effects for Quinlan's Data Set

The application of Lenth's procedure provided inner confidence limits of $\pm$ 0.5532 and outer confidence limits of $\pm$ 1.123. These limits correspond to $\alpha/2 = 0.025$ and $\alpha/2 = 0.0017$. When we compare the effect estimates to these limits both effects 9 and 14 lie in the uncertain zone between the inner and outer limits, while the remaining effects are deemed to be not active.

Benski's procedure yields results that are consistent with the other techniques. Table 3.6.2. (Table 4 from Schneider, Kasperski, and Weissfeld (1993)) shows the results of the Benski procedure. For example, since effect 9 is the largest (in absolute value) we consider it first and perform the W'-test to obtain a significance level of $P_1 = .024$. Since P_1 is small, using .05 as a benchmark, we compute the fourth-spread limits, $\pm\ 2d_F = .509$. Because effect 9 (-.882) lies beyond the limits it is deemed to be significant, so we remove it and repeat the entire process considering the next largest effect, effect 14. Effect 14 is deemed to be significant since it lies outside the fourth-spread limits of $\pm\ 2d_F$ and on the next iteration of the procedure $P_1 > .5$, so the procedure ends.

Table 3.6.2. Benski Procedure Analysis of Quinlan Data

Effect No.	W'	P_1	$\pm 2d_F$	Significant?
9	0.856	0.024	0.509	Yes
14	0.819	0.010	0.500	Yes
10	0.961	>0.500	0.484	No

CHAPTER 4 - SCHNEIDER, KASPERSKI, AND WEISSFELD'S PROCEDURE FOR DETERMINING ACTIVE EFFECTS IN AN UNREPLICATED FACTORIAL DESIGN

The recent procedure from Schneider, Kasperski, and Weissfeld (1993) is based upon the same assumptions as the procedures described in Chapter 3. The inert contrasts are assumed to be distributed normally with a mean of zero and constant variance and factor sparsity is assumed to be in existence. The assumption of factor sparsity is important since most of the contrasts will be inert and can be used in estimating the experimental error (Box & Meyer, 1986).

The procedure involves successive pooling of a specified number of the absolute smallest contrasts in order to estimate the experimental error variance. The contrasts used in this estimation are treated as a Type II right censored sample to help reduce the bias of the variance estimate. T-statistics, based upon the less biased variance estimate, are then calculated and compared to inner and outer limits. A contrast that has a t-value that lies beyond the outer limit is deemed active, while a contrast that has a t-value that lies within the inner limit is deemed inert.

4.1. Previous Related Material

The Schneider, Kasperski, and Weissfeld procedure has its roots in two previously published techniques, but with slight modifications and different underlying assumptions. The first procedure is successive pooling from Quinlan (1985) and the second is the use of the smallest contrasts to estimate the experimental error variance from Wilk, Gnanadesikan, and Freeny (1963). Both of these procedures will be described in further detail in the following sections.

4.1.1. Quinlan's Successive Pooling Procedure

Quinlan (1985) presented his analysis of an experiment at the American Supplier Institute's symposium on Taguchi methods. The experiment was a two-level, 16 run fractional factorial design with 15 factors under consideration (see Table 4.1.1.). In the notation of Box, Hunter, and Hunter (1978) this can be written as 2^{15-11} Resolution III. The objective was to decrease the post-extrusion shrinkage of a speedometer casing.

Table 4.1.1. Factors Used in Speedometer Case Shrinkage Example

Factor	Effect	Name	Factor	Effect	Name
1	E1	Liner tension	8	E(23)	Braiding tension
2	E2	Liner line speed	9	-E(24)	Wire braid type
3	E3	Liner die	10	-E(34)	Liner material
4	E4	Liner OD	11	E(123)	Cooling method
5	-E(12)	Melt temperature	12	E(124)	Screen Pack
6	-E(13)	Coating material	13	E(134)	Coating die type
7	-E(14)	Liner temperature	14	E(234)	Wire diameter
			15	-E(1234)	Line speed

The shrinkage values for four samples taken from a 3000 foot length of product produced at each set of conditions are given in Table 4.1.2. Box (1988) suggested a log transformation for the data and the effect estimates from the averages of the natural logarithms of the data are given in Table 4.1.3. At first glance, one may be tempted to used the standard deviation of the four sample values to estimate the experimental error variance. However, because the sample values are not true replicates, but merely

multiple measurements for one trial, this is not a viable alternative (Schneider, Kasperski, and Weissfeld, 1993).

Table 4.1.2. Speedometer Shrinkage Results (Quinlan's Data)

No	y1	y2	y3	y4
1	0.49	0.54	0.46	0.45
2	0.55	0.60	0.57	0.58
3	0.07	0.09	0.11	0.08
4	0.16	0.16	0.19	0.19
5	0.13	0.22	0.20	0.23
6	0.16	0.17	0.13	0.12
7	0.24	0.22	0.19	0.25
8	0.13	0.19	0.19	0.19
9	0.08	0.10	0.14	0.18
10	0.07	0.04	0.19	0.18
11	0.48	0.49	0.44	0.41
12	0.54	0.53	0.53	0.54
13	0.13	0.17	0.21	0.17
14	0.28	0.26	0.26	0.30
15	0.34	0.32	0.30	0.41
16	0.58	0.62	0.59	0.54

As Box (1988) points out, Quinlan used the following formula to obtain values for use in an ANOVA,

$$SN_S = -10\log_{10}\left(\frac{1}{4}\sum_{i=1}^{4} y_i^2\right).$$

Table 4.1.3. Effect Estimates Based on the Log Transform for Quinlan's Data

Estimate of Contrasts	
μ=	-1.430
E1=	0.168
E2=	0.239
E(12)=-E5	0.119
E3=	-0.028
E(13)=-E6	-0.046
E(23)= E8	0.212
E(123)= E11	-0.102
E4=	0.222
E(14)= -E7	0.084
E(24)= -E9	0.882
E(124)= E12	-0.020
E(34)=-E10	0.317
E(134)= E13	0.309
E(234)= E14	-0.604
E(1234)=-E15	0.025

The mean square error for the ANOVA was obtained by pooling the mean squares for the seven smallest effect estimates.

Based on the ANOVA, Quinlan concluded that all of the remaining eight factor effects were significant, with the Wire Braid Type and Wire Diameter accounting for more than 70% of the experimental variance (Box, 1988).

Box (1988) illustrated the extreme bias induced by pooling effects in this manner through the use of Rand Corporation (1955) tables. Box states that it was due to this type of analysis, which almost guarantees spurious conclusions, that led to Daniel's normal probability plotting procedure. Box and Meyer (1986) and Box (1988) also stated another reason for considering Quinlan's successive pooling unsatisfactory: it systematically underestimates the experimental error variance.

4.1.2. Wilk, Gnanadesikan, and Freeny's Variance Estimation Procedure

Wilk, Gnanadesikan, and Freeny (1963) describe a technique that uses the smallest, ordered, absolute valued contrasts to estimate the experimental error variance. By using the ordered absolute values of the contrasts, Wilk et. al note that there are two sources of bias for the variance. First, the number of contrasts, K, out of the total number, N, that are free from real effects is usually unknown. Second, if we were to include contrasts associated with real effects in the estimation of the error variance it would greatly exaggerate its magnitude. The authors felt that the bias arising from the first source will usually be a less serious problem than the second.

The K contrasts, $K \leq N$, are regarded as a random sample of size K from $N(0,\sigma^2)$. Next, a number M, where $M \leq K$, is selected sufficiently small so that we will be almost certain that the M smallest absolute contrasts do not contain any active effects. These M contrasts are then squared and used to estimate that variance with a maximum likelihood procedure, where it is assumed that these M contrasts are the first M order statistics in a sample of size K from a $\sigma^2 X^2_{(1)}$ distribution.

Wilk et al (1963) illustrated (in their Table 2) the effects of the 2 sources of bias. These values are reproduced in Table 4.1.4. For a given value of K, the variance estimate does not differ much for varying values of M. However, for a given value of M, the estimates greatly differ for various values of K. This means that if, for a given M, we were to choose K too large we would overestimate the variance. Also, if we were to choose M too large, where it may include active effects, it would inflate the estimate even more.

Table 4.1.4. Wilk et. al's (1963) Estimates of the Error Variance

K	M=15	M=20	M=K
20	.0023	.0018	.0018
25	.0041	.0042	.0033
30	.0063	.0071	.0109

4.2. Description of Schneider, Kasperski, and Weissfeld's Technique

The Schneider, Kasperski, and Weissfeld procedure combines the work of Quinlan and Wilk et al, but it is based upon different assumptions and includes extensions to test the significance of factor effects.

As previously stated, successive pooling is considered unsatisfactory since it systematically underestimates the error variance. However, this bias can be reduced by viewing the n smallest selected values as coming from a Type II right censored sample. A Type II right censored sample is one in which only the n smallest values are available (Schneider, 1986). Also, whereas Wilk et al. (1963) used the χ^2 distribution to describe the smallest contrasts, the normal distribution is employed in this case.

The steps for using the Schneider, Kasperski, and Weissfeld technique are as follows:

1) Rank the m contrasts according to their absolute value from smallest to largest.

2) Choose the n = 7 or 8 absolute smallest contrasts to estimate the experimental error variance.

3) For each contrast, c_i, use the MLE, $\hat{\sigma}$, to compute a modified t-statistic $t_{ci} = c_i/\hat{\sigma}$ for i = 1, ..., m. For the calculation of $\hat{\sigma}$ refer to the Appendix of Schneider, Kasperski, and Weissfeld (1993).

4) Compute inner and outer limits, similar to Lenth's (1989) procedure, based upon the t-statistic,

$$t = \sqrt{[z_{\alpha/2}^2 \ / \ (1 - \delta \ z_{\alpha/2}^2 \ /m)]} \, .$$

5) If t_{ci} is within the inner limits defined by $\pm \ \tau_{.025}$, then effect c_i is deemed to be inert. If t_{ci} is outside of the outer limits defined by $\pm \ \tau_{\alpha/2}$, where $\alpha = 1-(.95)^{1/m}$, then the effect c_i is deemed to be clearly active. Effects that lie between the inner and outer limits may possibly be active.

4.2.1. Determination of Critical t-values and Limits

Compared with the modified t-statistic in step 3 of the previous section, two other t-statistics were computed by Schneider, Kasperski, and Weissfeld (1993). However, each of these had serious limitations.

The first t-statistic is given by $t_{mi} = c_i/s_m$, where c_i is the i^{th} contrast and

$$s_m^2 = \frac{1}{m}\sum_{i=1}^{m} c_i^2 .$$

This statistic could be used to test the null hypothesis that all effects are inactive, but was shown to be biased when the alternate hypothesis of at least one active effect was true and therefore may not perform as well as other test statistics (Schneider, Kasperski, and Weissfeld, 1993).

The second t-statistic involves an alternate estimate of the variance given by

$$s_n^2 = \frac{1}{n}\sum_{i=1}^{n} c_i^{\ 2},$$

where the c_i's are the n absolute smallest contrasts. Because large values may be systematically excluded from the error distribution the t-statistic, $t_{ni} = c_i/s_n$, will be biased since the error variance is underestimated. As previously stated, however, the bias of the t-statistic given in step 3, t_{ci}, is reduced by treating the n absolute smallest contrasts as a Type II censored sample (Schneider, Kasperski, and Weissfeld, 1993).

As Schneider (1986) demonstrated, the t_{ci} are asymptotically normally distributed under the null hypothesis of no active effects, but may be less efficient than the t_{mi}'s. However, under the alternate hypothesis the t_{ci}'s are less biased than either the t_{mi}'s or t_{ni}'s since s_m overestimates and s_n underestimates the error standard deviation (Schneider, Kasperski, and Weissfeld, 1993).

Schneider, Kasperski, and Weissfeld (1993) determined the t-statistic used for the inner and outer confidence limits in the following manner. Begin by using the statistic

defined as $T = C_i \pm \tau\hat{\sigma}$. Then its asymptotic expected value and variance are given as

$$E[C_i \pm \tau\hat{\sigma}] = \pm\tau\sigma$$

and

$$Var[C_i \pm \tau\hat{\sigma}] = \sigma^2 + \tau^2\sigma^2\delta/m,$$

where δ is the asymptotic variance factor for a censored sample (see Gupta, 1952, pg. 263). By letting $u = \Phi^{-1}(n/m)$ we obtain

$$\delta = [2(n/m) - u\phi(u) + u^2 m\phi(u)/r - u]^{-1}.$$

where m = number of total contrasts, n = number of contrasts used to estimate the variance, and r = m - n. Using the asymptotic normal distribution of T (Schneider,1986), τ is determined so that

$$Pr(c_i + \tau\hat{\sigma} < 0) = \frac{\alpha}{2}$$

and

$$Pr(c_i - \tau\hat{\sigma} < 0) = \frac{\alpha}{2}$$

The above information is combined to obtain (for large values of m)

$$\phi(-\tau/\sqrt{(1+\tau^2\delta/m)}) = \frac{\alpha}{2}$$

and

$$\tau/\sqrt{(1+\tau^2\delta/m)} = Z_{\frac{\alpha}{2}},$$

where $Z_{\alpha/2}$ is defined to be the 100 x (α/2) percentile of the standard normal function.

Solving the above equation for τ yields

$$\tau=\tau_{\frac{\alpha}{2}}=\sqrt{[Z^2_{\alpha/2}/(1-\delta Z^2_{\alpha/2}/m)]}$$

which is used in step 4 of the Schneider, Kasperski, Weissfeld procedure. Note that when we consider m = 15 factors, the outer limit will be $\pm\ \tau_{.0017}$.

4.2.2. Advantages and Disadvantages of Schneider, Kasperski, Weissfeld's Procedure

There are several advantages to the Schneider, Kasperski, Weissfeld procedure and, as in Lenth's procedure, one main disadvantage. The advantages include the robustness of the procedure when selecting the number of absolute, smallest contrasts to use when estimating the standard deviation, the distinct cutoffs provided for determining whether a contrast is active or inert, and the use of t-statistics for testing the significance of an effect. Since t-statistics are taught in basic statistical courses, advanced statistical training is not necessary to obtain an understanding of the tests.

The main disadvantage, as previously mentioned in section 3.5.2., is when an effect's t-value lies between the inner and outer t-limits. This zone of uncertainty allows one to make a good argument for the effect being either active or inert (Lenth, 1989). However, when an effect lies in this zone it would probably receive further attention in subsequent experimental trials to resolve the ambiguity.

4.3. Published Example of the Schneider, Kasperski, Weissfeld Procedure

Consider the Speedometer casing shrinkage experiment performed by Quinlan (1985) and analyzed in Schneider, Kasperski, and Weissfeld (1993). Using the effect

estimates in Table 4.1.3. and applying the procedure described in section 4.2., we obtain an estimated standard deviation $\hat{\sigma}$= 0.1927, along with the t-values shown in Table 4.3.1. Also shown in Table 4.3.1. are Quinlan's t-values.

Table 4.3.1. Effect Estimates and t-values for Quinlan's Experiment

Effect No	t_n-values (n=7)	adjusted t_c-values (n=7)
E1	2.36	0.87
E2	3.37	1.24
E5=-E(12)*	-1.67	-0.62
E3*	-0.39	-0.14
E6=-E(13)*	0.64	0.24
E8=E(23)	2.99	1.10
E11=E(123)*	-1.43	-0.53
E4	3.13	1.15
E7=-E(14)*	-1.19	-0.44
E9=-E(24)	-12.42	-4.58
E12=E(124)*	0.28	-0.10
E10=-E(34)	-4.46	-1.64
E13=E(134)	4.35	1.60
E14=E(234)	-8.50	-3.14
E15=-E(1234)*	-0.35	-0.13
$\hat{\sigma}$	0.071	0.193

$\hat{\sigma}$: estimated experimental standard deviation using values marked by an asterisk.

The critical values are $\tau_{.025}$ = 2.27 and $\tau_{.0017}$ = 4.573. Comparing the t-values shown

in Table 4.3.1. to these critical values, effect 9 would be deemed active while effect 14 lies in the zone of uncertainty between the two critical values.

CHAPTER 5. EMPIRICAL STUDY AND IN-DEPTH ANALYSIS

In order to compare each of the techniques in further detail, several examples were chosen from various literature sources. The examples are separated into groups based upon the number of runs in each design and whether or not they are a full factorial or a fractional factorial design. Also, to provide another variation in the type of design analyzed, a small group of Plackett-Burman designs were also considered.

In this chapter, the four numerical techniques (Box and Meyer, Benski, Lenth, and Schneider, Kasperski, and Weissfeld) will be compared using the examples listed in tables A1-A6. A total of 56 examples were analyzed, including 18 2^3 designs, 6 2^4 designs, 6 2^5 designs, 7 2^3 fractional factorial designs, 15 2^4 fractional factorial designs, and 4 Plackett-Burman designs. As previously mentioned, these tables have separated the examples into distinct groupings (2^3, 2^4, and 2^5 (full factorials), 2^3 and 2^4 fractional factorials, and Plackett Burman designs).

5.1. Previous Comparisons of the 4 Methods Using a Simulation Study

Cabau and Benski (unpublished) compared the four numerical techniques in a Monte Carlo simulation study. In the study, they examined each technique's ability to distinguish assumed active effects from the inert noise distribution, along with the technique's sensitivity to departures from the underlying assumptions and its computational speed.

The simulations performed included both 16 run and 32 run designs. In each case, 10,000 Monte Carlo trials were performed. The number of assumed active effects, w, varied from 0 to 3 and were taken from a normal distribution with mean 0 and a

standard deviation of 10, N(0,10). It should be noted that the assumption that the "active" effects can be described by a N(0,10) is the exact assumption made by Box and Meyer (1986). Also included in the study was the effect of changing the standard deviation of the assumed active effects to 5 or 15. Meanwhile, the remaining $n-w$ effects were taken from a normal distribution with mean 0 and a standard deviation of 1, N(0,1).

Cabau and Benski classified the outcomes of the simulation analyses in terms of the number of effects found to be active by each technique. This could provide useful information since we would like to find all of the active effects, but not at the expense of classifying a large number of inert effects as active.

The w group consisted of the percentage of outcomes in which the technique was able to correctly identify all of the assumed active effects and only these effects were deemed active. The w+ group was the percentage of outcomes that the technique correctly identified all of the assumed active effects, but also deemed some of the effects associated with noise as active. The $(w-i)$ group was the percentage of outcomes each technique found only assumed active effects, but only $w-i$ of them, while the $(w-i)+$ group was the percentage of outcomes it found only $w-i$ of the assumed active effects along with classifying some of the noise effects as active. The final group, others, was the percentage of times the technique did not deem any of the assumed active effects as active, but did deem some of the noise effects as active (Cabau and Benski, unpublished).

Cabau and Benski's conclusions from the simulation study were that for overall power, Box and Meyer's bayesian technique and Benski's technique seemed to be the

best. Lenth's technique, although being the fastest computationally, performed the worst. The Schneider, Kasperski, and Weissfeld procedure was close to the top two, but Cabau and Benski felt that its power in finding only the assumed active effects diminished too quickly. An examination of Cabau and Benski's Figures 2 and 3 shows a peculiarity in that the Schneider, Kasperski, and Weissfeld procedure actually had the highest percentage of correct classifications in 4 of the 6 situations depicted. It should also be noted that the implementation of the Schneider, Kasperski, and Weissfeld procedure did not correspond to the suggested number of absolute smallest contrasts to be used in estimating the experimental error variance.

5.1.1. Concerns of the Simulation Study

While Cabau and Benski's simulation study is well intended, it does suffer from several serious deficiencies. To begin with, none of the techniques, no matter how powerful, will be able to find one of the assumed active effects if it is imbedded very deep in the ordered set of effects. For instance, suppose there were 3 assumed active effects out of 15 and the third active effect was the sixth largest effect overall. Then the techniques would have to deem 3 inert effects as active in order to locate all of the assumed active effects. In practical situations, the Pareto Principle (factor sparsity) is assumed to apply since there are only a few big factors that greatly affect the outcome of the product or process. Our concern should lie in finding these large influential factors, regardless of whether they arose from the active or inactive distributions. This is why just "counting" the outcomes of a simulated experimental situation does not shed much light upon how the techniques will perform using real data.

Is it beneficial to consider there to be 2 distinct distributions for the effects? Or, would it be more realistic to consider the effects as arising from a single distribution with varying degrees of activity for all of the factors? In this situation, the active effects should be larger in magnitude than the inactive effects and would lie at the ends of the continuum. By viewing the entire situation in this way it would eliminate the imbedding problem described above.

Practically speaking, Cabau and Benski never properly addressed the question of whether a small, active factor is of more importance overall than a larger factor that arose from the noise distribution. In a real world situation, if the noise factors are intermixed with the supposed active factors, then there could be other problems existing in the experiment including scaling, outliers, etc. Also, just because an effect is from a distribution with a larger variance, how can we justify it being active if noise factors have a larger influence on the process? In a real-life experiment, can we say that a particular factor is active when it is imbedded within noise factors? And if we actually could, would that factor truly be more important to us from a financial standpoint than the larger factors that could have arose from the noise distribution? The 2 distribution assumption for the effects may aid an analyst in modelling how a procedure may work, but what is that practical justification for saying that because an effect comes from a distribution with a larger variance that it is important?

5.1.2. Considerations for Comparing the 4 Procedures

In order to properly assess the performance of the 4 procedures, criteria must be established to avoid the problems discussed in the prior section. Justification for the criteria will be described at this time to avoid later confusion.

First, the effects will be viewed as having varying degrees of activity, and not just coming from either an active or inactive distribution. The reasoning for this is that we cannot precisely know before the analysis is performed whether each effect definitely comes from one or the other distribution. In this way, we also avoid the question of, "what if an effect is from the active distribution, but is smaller than some of the noise factors?".

Second, the factor sparsity assumption will be used since it does apply to most practical situations. Most of the effects will be grouped rather closely, but there may be effects that lie at the extremes. These outliers will be the effects under scrutiny since we want to see if they are significantly different than the remaining effects, which can be viewed as noise.

Third, the analysis performed in this chapter will first consider the separate groupings of the examples and then consider all of the examples together. The goal of the analysis is to see what similarities and differences exist between the four numerical methods. Since the normal probability plotting procedure of Daniel does not produce numerical results like the other methods it will be considered separately in the next chapter because it does have useful merits despite its inherent subjectivity.

When analyzing the examples, several standards were maintained in order to compare the results on a somewhat even scale. The legend used in tables A1 - A6 shows whether an effect was deemed significant or active (2), uncertain (1), or inactive (0).

The numerical results for each of the examples in tables A1-A6 are separated for each of the methods. Tables A1.1 - A6.1 list the effect estimates along with the ME and SME cutoffs for Lenth's procedure and the estimate of σ for Schneider, Kasperski, and Weissfeld's procedure. Tables A1.2 - A6.2 list the posterior probabilities from Box and Meyer's bayesian procedure. Tables A1.3 - A6.3 list the results using Benski's procedure. For the Schneider, Kasperski, and Weissfeld procedure, the results are shown in Tables A1.4 - A6.4. Tables A1.5 - A6.5 provide a summary for each method's classification of effects as 0, 1, or 2 for each of the designs. The summary for each method results (0,1,2's) as compared to the other methods are shown in tables A1.6 - A6.6. Tables A1.7-A6.7 provide a closer examination of when a method classified an effect is inactive, while the other method(s) classified the same effect as either uncertain or active. Table A7 lists the results of comparisons of the methods when an effect has a result of uncertain (1) or active (2) for each type of design.

For Box and Meyer's method, listed as "Bayes" in all of the tables, an effect with a posterior probability of .95 or above was deemed active. An effect with a posterior probability between .5 and .94 were deemed to be uncertain, while an effect with a posterior probability less than .5 was deemed to be inactive.

In order to use Benski's method, the significance level of the W'-test for normality was assumed to be .05. Therefore, the fourth-spread test was used only when

the significance level was .05 or less. Also, Benski's procedure is the only method which does not classify an effect as uncertain. An effect is deemed active if it lies outside the cutoffs for the fourth-spread test or inactive if it lies within the cutoffs.

As previously discussed, when using Lenth's method, effects that lie with the $\pm$ ME cutoffs were deemed inactive. Those effects between the ME and SME cutoffs are deemed as uncertain, while effects beyond the $\pm$ SME cutoffs are deemed to be active.

In using the Schneider, Kasperski, and Weissfeld procedure, the number of effects used to estimate the standard deviation was approximately 1/2 of the total number of effects. Therefore, in the 2^3 experiments the 4 absolute, smallest effects were used, while in the 2^4, 2^5, and Plackett-Burman experiments the 8, 16, and 5 absolute, smallest effects were used, respectively.

As can be seen in tables A1-A6 and A1.5 - A6.5 there are relatively few effects that are either uncertain or active (1 or 2) as compared to the number deemed inactive (0). This is consistent with the underlying assumption of factor sparsity. Also, a brief examination of tables A1 - A6 reveals that the 4 methods provide similar results. As previously mentioned, the objective here is to determine when the methods provide similar results and when the results differ.

An interesting observation made by a closer examination of tables A1.5 - A6.5 is that Benski's procedure always classified more effects as 0 (inactive) than any of the other methods. One possible explanation of this is how Benski's procedure classifies effects. If the W'-test is significant and the fourth-spread test shows that the absolute

largest remaining effect lies beyond the cutoff, then the effect is deemed to be active (2). Otherwise, the effect is deemed inactive (0). There is no uncertain area when using Benski's procedure, so although the number of effects deemed active (2) is often comparable to the other methods, the number of effects deemed inactive (0) was always the highest of the four methods (see tables A1.5 - A6.5).

While most of the tables are self-explanatory, tables A1.6 - A6.6, A7, and A9 are to be read as follows. When the method listed in the row across the top is compared with the method in the column on the left side the number of times that the given event occurred is shown. For example, in table A1.6, which represents the 2^3 full factorial designs, the number of times that Box and Meyer's bayesian method classified an effect as inactive (0) and Benski's procedure yielded the same result was 98/99 (98.99%). Because the number of classifications of effects as either uncertain (1) or active (2) was usually quite limited, tables A1.6 - A6.6, A7, and A9 list the classifications as fractions and not as percentages. If the listings were only in percentages, the results could be easily misinterpreted. For instance, in table A2.6 when Box and Meyer's procedure deemed an effect as uncertain, Benski's procedure said it was active 50% of the time. However, there were only two instances when Box and Meyer's procedure produced an uncertain result and Benski's method regarded the same effects as being active once. Thus, listing the results as 1/2 is more meaningful due to the limited number of occurrences. When the comparisons are expressed as a percentage, it will be referred to as the consistency ratio throughout the text.

Although the analyses are divided into sections based upon the type of design, an aggregate of all the designs will be discussed in a similar manner to conclude this chapter. An important point to make before further discussion of the results is how we should interpret and analyze the information in the tables. Remembering that we are looking for the effects that affect our product or process, we could concentrate our efforts on the classifications of effects as uncertain or active. The effects deemed to be uncertain are those that may be significant and therefore may warrant further investigation to see if they are truly influential to the product or process. The consistency of the methods for identifying those probable active effects is an area that will be fully explored in further detail.

5.2. 2^3 Factorial Design Results

An examination of table A1.5 reveals that each of the methods classified almost the same number of effects as active (2), with Box and Meyer's, Lenth's, and Schneider, Kasperski, and Weissfeld's procedures providing consistent results for the 1's and 0's (uncertain and inactive effects). As previously mentioned, Benski's procedure had the highest number of inactive (0) classifications.

In comparing the number of times that a given method classified an effect as inactive (0) and the other methods agreed the consistency ratio ranged from 98/117 (84%) to 99/99 (100%) (see Table A1.6). Due to the fact that Benski's method had 117 0's as compared with 99 for Schneider, Kasperski, and Weissfeld's and Box and Meyer's procedures, the largest possible value for the lower limit of the consistency ratio range

was 99/117 or 85%. Thus, from the possible number of matching results, a high percentage was achieved.

The number of matching results when a given procedure classified an effect as uncertain can also be seen in table A1.6. For the uncertain classifications, the consistency ratio ranged from 53% for the times when the bayesian procedure and Lenth's procedure agreed to 93% when Lenth's method classified an effect as uncertain and Schneider, Kasperski, and Weissfeld's procedure agreed. An important aspect to see in this situation is that although there was not a big difference in the number of times an effect was deemed uncertain, 17 for Box and Meyer's, 14 for Lenth's, and 18 for Schneider, Kasperski, and Weissfeld's, the number of times that the methods reached the same conclusion about an effect varied widely.

When comparing the consistency ratios for when the methods both said an effect was active, we see that the range is from 56% (5/9) for Lenth and Benski to 89% (8/9) for several of the other comparisons. This means that although each of the methods classified either 10 (Box and Meyer's) or 9 (the remaining 3 methods) effects as active, there were several times when the same effect was classified differently. This is why when we want to examine the effects that most likely have a significant impact we should examine Table A7 to gain further insight.

As previously mentioned, Table A7 lists the number of times that the first method classified an effect as either uncertain or active and the second method did likewise. This table, therefore, should provide a clearer picture concerning probable influential effects and the methods' classification consistency.

A closer examination of Table A7 for the 2^3 designs reveals that the consistency ratio ranges from 30% for Box and Meyer/Benski and Schneider, Kasperski, and Weissfeld/Benski to 100% for Lenth/Schneider, Kasperski, and Weissfeld. A reason for the 30% consistency ratio is the fact that Benski's procedure only classified 9 effects as active, while Box and Meyer's and Schneider, Kasperski, and Weissfeld's procedures classified 27 effects as either uncertain or active. However, for the 9 effects that Benski's procedure deemed active, Box and Meyer's results agreed 8 times for a consistency ratio of 89%. A more in-depth comparison of these results is given in the next section.

If we take the complement of the first part of table A1.6 we would obtain the number of times that a given method deemed an effect as inactive, while the other methods deemed the same effect as either uncertain or active. This is shown in the fourth section of table A1.6. In this instance, the consistency ratio should be viewed as 1 minus the fraction shown in the table, since this section lists the number of times that the method listed in the row across the top deemed an effect 0, while the method listed in the column on the side deemed it to be something different.

If we were mainly concerned with the times when a method classified an effect as inactive, but another method deemed that same effect as active we should refer to the sixth section of table A1.6. All of these percentages of differing classifications are low ranging from 0% to 3.4% for Benski/Lenth. Thus, whenever a given method deems an effect to be inactive, the remaining methods do not have a high percentage of active classifications for the same effect.

5.2.1. In-depth Examination of Uncertain and Active Classification Differences for 2^3 Factorial Designs

When considering Benski's procedure and its classification of active effects, each of the remaining 3 methods gave an uncertain or active decision on 8 of the 9 times. The occasions when the methods do not provide a similar result is our topic of interest in this section. When the effects are shown their corresponding estimates are usually given in parentheses.

The one time that the other 3 methods did not agree with Benski's classification of an effect as active (or uncertain) was for effect 1 in Example 18. The effect estimate was -16.5, with the significance level of the W'-test $<.005$, and a fourth-spread test cutoff of $\pm$ 6. For Box and Meyer's procedure that posterior probability was .35, while Lenth's cutoffs were $\pm$ 36.66 and $\pm$ 87.85. The corresponding t-statistic for the Schneider, Kasperski, and Weissfeld procedure was -1.83 with cutoffs at $\pm$ 2.6274 and $\pm$ 6.5253. These results show that the effect was not close to being deemed uncertain for either of the remaining methods, so this is one of the times when a true difference exists.

For the remaining 3 procedures (Box and Meyer's, Lenth's, and Schneider, Kasperski, and Weissfeld's) the results for the differing classifications are shown in Table A1.7. When considering only these 3 methods, the consistency ratio ranged from 74% for Box and Meyer/Lenth to 100% for Lenth/Schneider, Kasperski, and Weissfeld.

When we examine the information shown in table A1.7, one important consideration is how close the other method(s) were to classifying the effect as uncertain

to further warrant investigation. The numbers in bold indicate that the given procedure deemed the effect to be uncertain.

In example 1, both Box and Meyer's and Schneider, Kasperski, and Weissfeld's methods deemed effect 4 (-73.5) uncertain, but Lenth's procedure had a lower limit cutoff of $\pm$ 76.14. For effect number 6, only Box and Meyer's procedure deemed the effect uncertain with a posterior probability of .62. Neither Lenth's nor Schneider, Kasperski, Weissfeld's were close to deeming this effect as uncertain. Thus, although Lenth's method did not find effect 4 to be uncertain, it was extremely close to the point where it would have been called uncertain, so there was no major difference between the 3 methods with respect to this effect.

In example 4, both Lenth's and Schneider, Kasperski and Weissfeld's procedures deemed effects 2 and 4 as uncertain, but Box and Meyer's posterior probabilities were only .25 and .11. Neither of the posterior probabilities are close to the uncertainty cutoff point of .5 nor are the effects or t-statistics close to their respective inner limits. This would represent an instance when a major difference existed between Box and Meyer's and the other 2 methods.

In example 6, the Schneider, Kasperski, Weissfeld procedure deemed effect 5 (7.5) uncertain with a t-value of 2.793, which is just beyond the inner limit of 2.6274. Box and Meyer's procedure nearly deemed it as uncertain with a posterior probability of .49, while it was close to Lenth's inner limit for uncertainty. This effect represents only a minor difference between the methods.

In example 11, Box and Meyer's method deemed both effects 1 (11.3) and 4 (7) uncertain with posterior probabilities of .7 and .53. Lenth's inner limit is $\pm$ 18.05 and neither effect is close to being deemed uncertain. For Schneider, Kasperski, and Weissfeld's procedure, only effect 1 was deemed uncertain with a t-value of 2.838, which is only slightly above the inner limit of 2.6274. Effect 4, meanwhile, had a t-value of 1.758, which is not close to the inner limit. Thus Box and Meyer's and Schneider, Kasperski, and Weissfeld's methods provide similar results, as effect 4's posterior probability of .53 was only slightly above the uncertainty cutoff. However, for Lenth's method the results were quite different in that neither effect was close to being deemed uncertain.

In example 12, both Box and Meyer's and Schneider, Kasperski, and Weissfeld's procedures deemed effect 4(5) as uncertain, while Lenth's inner limit was only .64 above the effect estimate. This would indicate only slight difference, in that the posterior probability was .52 and t-value = 3.406, both barely over their uncertainty classification limit.

In example 14, only Box and Meyer's procedure deemed effect 4 (15.25) as uncertain with a posterior probability of .57. Lenth's inner limit was 26.79, while Schneider, Kasperski, and Weissfeld's t-value was only 2.272. Again, this would represent only a marginal difference in the methods as .57 is close to the limit of .5.

5.3. 2^4 Factorial Design Results

Upon examination of Table A2.5, it is readily apparent that again the classification of effects as inactive, uncertain, and active by each of the four methods is

quite consistent. As previously mentioned, Benski's procedure produced the highest number of inactive classifications.

The consistency ratios shown in Table 2.6 do not reveal any major discrepancies between the 4 methods' classifications. A section which deserves a closer viewing is the fourth section, since it shows the number of times that a given method classified as effect as inactive and another method classified the same effect as either uncertain or active. The biggest difference between the methods was when we compared the times that Benski's procedure classified an effect as inactive and Schneider, Kasperski, and Weissfeld's procedure classified the effect differently. There were only three occurrences of this, out of a total of 73 inactive classifications by Benski's method, so there does not appear to be a dramatic difference.

To evaluate the consistency of the methods in their classification of effects as uncertain or active, we can look at the second section of Table A7. The lowest consistency ratio is for Schneider, Kasperski, and Weissfeld/Benski (17/20 or 85%), while there was 100% agreement between several of the procedures (Bayes/Lenth, Bayes/Schneider, Kasperski, Weissfeld, Benski/Bayes, Benski/Lenth, and Benski/Schneider, Kasperski, Weissfeld).

5.3.1. In-depth Examination of Uncertain and Active Classification Differences for 2^4 Factorial Designs

When comparing the times when Benski's procedure classified an effect as either active or uncertain, there was 100% agreement with the other 3 methods. As seen in Table A7, however, Benski's method classified the fewest number of effects as possibly active.

To examine the differences between the remaining 3 methods classifications of active and uncertain effects, we can view the results shown in Table A2.7. There were only 3 occasions when the methods were not in agreement on an effect's classification.

In example 2, there were 2 effects that had differing results. In both instances, the Schneider, Kasperski, Weissfeld procedure deemed the effects as uncertain, while Box and Meyer's and Lenth's procedures deemed them inactive. For effect 8 (.27), the posterior probability was only .34, but the effect estimate was quite close to Lenth's inner limit of .3084. the t-value for the Schneider, Kasperski, Weissfeld procedure was 2.4885, which was just beyond the inner t-value of 2.2361. For effect 10 (-.25), the posterior probability was only .27, while the Schneider, Kasperski, Weissfeld t-statistic was -2.3041. Again, this effect was just barely deemed uncertain by the Schneider, Kasperski, Weissfeld procedure. Finally, in example 5, effect 5 (1.1875) was deemed uncertain by Lenth's method, which had an inner limit of 1.0119. The effects posterior probability was .41 and its t-statistic was 1.8628.

In each of the 3 cases, the effects deemed uncertain were close to the lower significance limits for at least one of the other procedures. This would lead to a conclusion of only minimal differences in the results.

5.4. 2^5 Factorial Design Results

In Table A3.5, we see that for the 2^5 factorial designs subtle differences begin to appear between the techniques. Again, Benski's method has the highest number of inactive classifications, while the Schneider, Kasperski, Weissfeld procedure has the highest number of uncertain and active classifications combined.

The consistency ratios shown in Table A3.6 provide support for the nearly identical results for Lenth's and Schneider, Kasperski, and Weissfeld's procedures. The Schneider, Kasperski, Weissfeld method classified 1 more effect as uncertain than did Lenth's method, which is the only difference between the 2 techniques. This is definitely the closest match obtained between any of the methods. Since Box and Meyer's procedure only classified 4 effects as uncertain, there was less of a match between it and the other methods. With regards to when a particular method deemed an effect as inactive, while another method deemed it differently, Box and Meyer's method differed from Lenth's in 3.5% of the cases and in 4.1 % of the cases it differed from Schneider, Kasperski, Weissfeld's method.

The third section of Table A7 shows a 100% consistency ratio for Benski's classifications with the other methods and for the Bayes/Lenth, Bayes/Schneider, Kasperski, Weissfeld, and Lenth/Schneider, Kasperski, Weissfeld comparisons. Due to the fact that both Benski's and Box and Meyer's procedures had the lowest number of uncertain and active classifications (10 and 16, respectively) both Lenth's and Schneider, Kasperski, Weissfeld's results agreed with them each time. However, when we compare the number of times that Lenth's (Schneider, Kasperski, Weissfeld's) procedure yielded an uncertain or active result and Benski's procedure agreed, the consistency ratio is only 10/22 = 45% (10/23 = 43%). Similarly, for Box and Meyer's procedure it was only 16/22 = 73% (16/23 = 70%). Therefore, some differences in the classification of effects have appeared.

5.4.1. In-depth Examination of Uncertain and Active Classification Differences for 2^5 Factorial Designs

The differences in the classifications of effects as uncertain or active shown in Table A7, section 3 help to explain the wide ranging consistency ratios. As mentioned in 5.4, whenever Benski's procedure deemed an effect as either uncertain or active, the 3 procedures did the same.

There were a total of 7 times when the remaining 3 procedures had differing results for the effects. Table A3.7 shows the differences between the methods. In example 1, effect 11 (-11.7) was deemed to be uncertain by the Schneider, Kasperski, Weissfeld procedure with t = -2.1436, while the inner t-limit was $\pm$ 2.087. The posterior probability was .46 and the ME was $\pm$ 11.988. Thus, the other 2 methods were close to considering the effect to be uncertain.

In example 3, there were 3 effects deemed uncertain by both Lenth's and Schneider, Kasperski, Weissfeld's procedure. The 3 effects were 5 (71.25) with t = 2.3516, 12 (68.25) with t = 2.2526, and 22 (71.75) with t = 2.3681. The inner limit for Lenth's method was 64.935, so the 3 effects were just beyond the inner limit. The posterior probabilities for the 3 effects were .3, .26, and .31 respectively. Therefore, none of these were close to the .5 cutoff for the uncertain classification.

In example 5, 2 effects were deemed uncertain by both Lenth's and Schneider, Kasperski, Weissfeld's procedure. Effect 1 (2.25) with t = 2.4401 and effect 8 (2.25) with t = 2.4401 both were beyond the inner t limit of 2.087 and the ME = 2.0812. However, in this particular instance, Box and Meyer's procedure had a posterior probability of .47 for each effect, so they were close to being deemed uncertain. This

was the same occurrence in example 6, where effect 5 (1.28) with t = 2.206 was just beyond the inner limits for Lenth' procedure (ME = 1.0408) and Schneider, Kasperski, Weissfeld's, but had a posterior probability of .47. Therefore, although there were differences between Box and Meyer's procedure and the other 3 methods, in 4 of the 7 instances the posterior probability was at least .46.

5.5 2^3 Fractional Factorial Designs Results

The summary of the four techniques' classifications given in Table A4.5 reveals only slight differences between Box and Meyer's, Lenth's, and Schneider, Kasperski, and Weissfeld's techniques. However, a more dramatic difference appears when comparing these 3 methods with Benski's procedure. Each procedure deemed either 1 or 2 effects as clearly active, but because Benski's procedure only has 2 possible classifications it deemed 96% (47/49) of the effects as inactive. Meanwhile, Box and Meyer's and Schneider, Kasperski, and Weissfeld's procedures declared 78% (38/49) of the effects inactive, with 84% (41/49) of the effects deemed inactive by Lenth's procedure.

The consistency ratios shown in Table A4.6 support the similarities between Box and Meyer's and Schneider, Kasperski, and Weissfeld's methods quite clearly. Each of the methods deemed the same 11 effects to be either uncertain or active (as seen in Table A4). Because of its 47 inactive classifications (96%), Benski's consistency ratio of inactive classifications with both the Bayesian and Schneider, Kasperski, and Weissfeld procedures is only 81% and only slightly better with Lenth's procedure, 87%. Also, when comparing Lenth's method to Box and Meyer's and Schneider, Kasperski, and Weissfeld's procedures the consistency ratio was 93%. There were only 3 occasions

when these 3 procedures did not deem the same effect either uncertain or active, which will be explored in further detail in the next section.

As can be seen in Table A4, one of the effects was deemed uncertain by the Box and Meyer, Lenth, and Schneider, Kasperski, and Weissfeld methods (Example 4, effect 2), while the other effect was deemed active (Example 4, effect 3). Thus, although the consistency ratio of possible active effects for Benski's method (see Table A4.6) was 2/11 (18%), 2/8 (25%), and 2/11 (18%) versus the Box and Meyer, Lenth, and Schneider, Kasperski, and Weissfeld techniques respectively, it did provide similar results to the 3 methods for the 2 effects deemed active. Also, whenever Lenth's method declared an effect possibly active, so did the Box and Meyer and Schneider, Kasperski, and Weissfeld procedures. Therefore, other than for Benski's classification of a larger number of effects as inactive, the remaining 3 procedures showed little difference in their classifications.

5.5.1. In-depth Examination of Uncertain and Active Classification Differences for 2^3 Fractional Factorial Designs

As previously mentioned, in Table A7 section 4, the classification of uncertain and active effects for each of the 4 procedures is shown with 100% agreement between several of the methods. However, the intention in this section is to examine the differences between the procedures when an effect was deemed either active or uncertain.

With respect to the Benski procedure results shown in Table A4.3, the same 2 effects deemed probably active by the other 3 procedures in example 4 were clearly beyond the cutoffs for the fourth-spread test. However, for the remaining effects deemed possibly active by the other techniques, Benski's procedure never moved beyond the

W'-test for normality. Therefore, it was not a case of almost deeming the effects active, but clearly declaring them as inactive.

To examine the differences between the other 3 methods, we can view the results shown in Table A4.7. As described in section 5.5, there were only 3 times when the Box and Meyer, Benski, and Schneider, Kasperski, Weissfeld procedures differed on a classification. In each case, Lenth's procedure deemed the effect as inactive, while the Box and Meyer's and Schneider, Kasperski, Weissfeld's methods declared the effect to be uncertain.

In example 3, effect 4 (-16.575) was deemed uncertain with a posterior probability of .53 and a t-value of -3.412. The inner limit for the t-test was $\pm$ 2.6274, so the t-value was clearly beyond this limit, but not close to the outer t-limit of 6.5253. The inner limit for Lenth's procedure was $\pm$ 18.048, which again was near the effect estimate.

In example 5, effect 2 (-3.8) had a posterior probability of .85 and a t-value of -3.463. Lenth's inner limit was $\pm$ 4.512 and again was close to the effect estimate. In this instance, because 2 of the procedures clearly deemed the effect uncertain, while the other method was close, the factor should be further investigated.

With respect to the results shown in Table A4.7, there does not appear to be a major difference between the 3 procedures. In 2 of the 3 instances, the effect was deemed uncertain by a small margin by the Box and Meyer's method. Lenth's procedure was close to deeming the effect uncertain in all 3 of the instances, so a large discrepancy

in the conclusions was not readily apparent. Overall, for the 2^3 fractional factorial designs these 3 procedures were consistent.

5.6. 2^4 Fractional Factorial Design Results

Table A5.5 shows results similar to those for the 2^3 fractional factorial designs. The Box and Meyer's, Lenth's, and Schneider, Kasperski, Weissfeld's procedures were similar with their classification of effects, while Benski's procedure had a larger number of inactive and active classifications. The number of effects deemed either uncertain or active by each method ranged from 23 for Benski's procedure up to 35 for the Lenth and Schneider, Kasperski, Weissfeld methods. However, when considering the total number of effects, 225, this 12 effect difference is only 5.3% of the total.

The consistency ratios in Table A5.6 tell much the same story as described above. While there was not 100% agreement between any of the methods on a large scale, the fourth section of Table A5.6 shows quite consistent results. When comparing the Box and Meyer, Lenth, and Schneider, Kasperski, Weissfeld procedures, the consistency ratios for probable active effects ranged from 97.9% to 99.5%. Even when comparing the Benski procedure to the others, the consistency ratios for probable active effects ranged from 93.6% to 94.6%. This would lead us to believe that on a larger scale, the differences are not as profound.

When we examine the uncertain and active classification consistency ratios in sections 2 and 3 of Table A5.6, the Box and Meyer's procedure had the smallest number of uncertain classifications. The Box and Meyer's and Lenth methods both declared 13 of the same effects as uncertain and 13 active. When compared to the Schneider,

Kasperski, Weissfeld procedure, Box and Meyer's method agreed 12 and 15 times for uncertain and active classifications. For the Lenth and Schneider, Kasperski, Weissfeld procedures, they agreed 18 and 14 times respectively for the uncertain and active effects. While the consistency ratios for these classification ranged from 63.2% to 94.7%, we need to remember that section 4 of Table A5.6 would be the most useful section since it provides information concerning effects that are either active or possibly active and requiring further examination.

5.6.1. In-depth Examination of Uncertain and Active Classification Differences for 2^4 Fractional Factorial Designs

When determining the differences between the procedures when analyzing 2^4 fractional factorial designs, the results shown in Table A5.7 actually show striking similarities between Box and Meyer's, Lenth's, and Schneider, Kasperski, Weissfeld's methods. There were 6 occasions, out of a total of 225, when the 3 methods did not agree on the classification of an effect. For 4 of the 6 effects, 2 of the 3 methods agreed on the uncertain conclusion. On another occasion, a second method was extremely close to deeming the effect uncertain, and for the last effect the one method that deemed the effect uncertain was only .7% beyond its uncertain limit.

For the Benski procedure, its significance level for the W'-test was close to being significant on 3 occasions (Examples 5, 10, and 15) and then proceeding to the fourth-spread test. This could have helped narrow the gap between the number of inactive classifications for the Benski procedure and the other 3 methods.

Upon a more in-depth examination of Table A5.7, the different classifications of the 3 methods for the same effects were not too different. As previously mentioned, in

4 of the 6 instances when the classifications differed, 2 of the 3 methods declared the effect uncertain. This would be an indication of only minor differences in the methods. In each of the 6 instances, none of the methods provided a strong vote for the uncertain classification as the values were usually close to the cutoff values for uncertainty.

In example 1, effect 3 (16.6875) was deemed uncertain by the Schneider, Kasperski, Weissfeld procedure with a t-value of 2.2521, which was just beyond the inner limit of $\pm$ 2.2361. The posterior probability was only .30, while Lenth's inner limit was $\pm$ 19.034. This difference in classification could be viewed as a slight difference in the methods, but since the t-value was only slightly beyond the inner limit it would not be a strong indication of an uncertain classification.

In example 2, effect 8 (100.5) was deemed uncertain by both Lenth's and Schneider, Kasperski, Weissfeld's methods. However, the posterior probability for the effect was .44, which is close to the .5 threshold for deeming an effect as uncertain. In example 10, effect 10 (-1.169) was deemed uncertain with a posterior probability of .68 and an inner limit for Lenth's method of 1.06. The Schneider, Kasperski, Weissfeld procedure deemed the effect as inactive, but the t-value of -2.181 was near the inner limit of $\pm$ 2.2361. This would indicate little difference between the procedures.

In example 11, effect 10 (27.625) was deemed uncertain by both Lenth's and Schneider, Kasperski, Weissfeld's procedures, where the inner limit was 26.021 and t-value was -2.645. The posterior probability for this effect was .46. In example 12, effect 8 (-81.312) was deemed uncertain with a posterior probability of .59. Lenth's inner limit was $\pm$102.109, but the Schneider, Kasperski, Weissfeld t-value was -2.20,

just inside the inner limit of $\pm$ 2.2361. In example 14, effect 3 (-.21) was deemed uncertain by both Lenth's and Schneider, Kasperski, Weissfeld's procedures. The posterior probability was only .38, which would not be an indication of possible uncertainty.

When viewing the overall results for the 2^4 fractional factorial designs, there does not appear to be much difference between Box and Meyer's, Lenth's, and Schneider, Kasperski, and Weissfeld's procedures. The number of possibly active effects (both uncertain and active) only differed by 2, with 2 effects having posterior probabilities of .44 and .46. Thus, for all intents and purposes, there were no dramatic differences between the 3 methods. When comparing the Benski procedure to the other 3, the main difference is again its larger number of inactive effect classifications. However, when considering the total of 225 effect estimates, the difference is not as profound.

5.7. Plackett-Burman Design Results

The summary of the results for the analysis of Plackett-Burman is shown in Table A6.5. As was the case in all of the prior types of designs, Benski's procedure has the highest percentage of inactive classifications 95.5% (42/44). The Lenth and Schneider, Kasperski, Weissfeld methods both deemed 39 effects inactive, while the Bayesian procedure deemed 38 inactive.

The most striking result shown in Table A6 is the perfect match between the Lenth and Schneider, Kasperski, Weissfeld procedures. Both methods came to the same conclusion on all 44 effects. Box and Meyer's procedure was almost the same, except for effect 2 in Example 1, which it deemed uncertain, while Lenth's and Schneider,

Kasperski, Weissfeld's procedure deemed it inactive. Another important result is that only 2 effects were deemed as clearly active (Example 3, effects 7 and 10) and each of the 4 methods provided the same results. These events led to consistency ratios of 1, or very close to 1, for most of the comparisons. However, it must be noted that only 4 Plackett-Burman examples were analyzed and each of these were only 12 run designs.

5.7.1. In-depth Examination of Uncertain and Active Classification Differences for Plackett-Burman Designs

Section 6 of Table A7 shows the uncertain and inactive classification results for the Plackett-Burman designs for each of the 4 procedures. On over half of the comparisons (7/12), the consistency ratios were 100% and on 2 other instances (Bayes vs Lenth and Bayes vs Schneider, Kasperski, Weissfeld) there was a consistency ratio of 5/6. The lowest consistency ratios again involved Benski's method. The consistency ratios ranged from 33% (2/6) for Bayes/Benski to 40% (2/5) for Lenth/Benski and Schneider, Kasperski, Weissfeld/Benski. Again, it must be reiterated that these low ratios may be slightly misleading due to the limited number of examples involved.

As previously mentioned, there was only 1 instance where the Box and Meyer, Lenth, and Schneider, Kasperski, Weissfeld procedures did not agree. In example 1, effect 2 (10.58) had a posterior probability of .59, which is just beyond the lower limit of .5 for the uncertain classification. The t-value for effect 2 was 2.2178, which is not quite at the inner t-limit of 2.5286, and Lenth's inner limit was ± 15.4119, which again is well above the effect value. Because of the closeness in the effect classifications for the Box and Meyer's, Lenth's, and Schneider, Kasperski, Weissfeld's procedures, they could be deemed as similar. Also, there was not a major difference between Benski's

procedure and the remaining 3 methods as there was less than a 10% difference in the number of effects deemed to be possibly active (uncertain or active) by the other methods when Benski's procedure deemed the effect inactive (38/42 or 90.5% to 39/42 or 92.9%).

5.8 Summary of Analyses

Based on the results obtained there do appear to be subtle differences between the methods along with two major differences. The information shown in Tables A8, A9, and A10 provide the support for the differences. An important point to remember is that although there were only 56 examples considered, they provided a total of 720 effects for the analysis.

Table A8 provides an aggregate display of the classification results for each of the four methods, with the percentages shown in parentheses. The most striking element in this table involves the number of cases of inactive classifications, which supports Box's assertion of factor sparsity. While Box and Meyer's, Lenth's, and Schneider, Kasperski, and Weissfeld's procedures all classified approximately 84% of the effects as inactive, Benski's procedure deemed over 91% of the effects as inactive. One possible reason for this difference, as mentioned earlier, is the fact that Benski's procedure only classifies effects as either active or inactive, with no provision for uncertainty. Therefore, the first major difference between the four methods involves the number of inactive classifications.

If we examine the remainder of Table A8 the next noticeable characteristic is the apparent uniformity in the results for Box and Meyer's, Lenth's, and Schneider,

Kasperski, and Weissfeld's procedures across all three classification groupings. If we consider the effects that are possibly active, i.e. uncertain and active combined, the overall classification percentages are 15.4%, 15.6%, and 16.8% for Box and Meyer's, Lenth's, and Schneider, Kasperski, and Weissfeld's respectively. This would indicate only minimal differences between these three procedures. When we compare these percentages to that for Benski's active classifications, 8.75%, there is a dramatic difference. Again, we consider the uncertain and active effects as possibly active due to the fact that a case can be made for further investigation of the uncertain effects to determine if they actually do have a significant impact on the response variable. Another interesting observation is that overall Benski's procedure deemed the largest number of effects as clearly active. So when we look at both ends of the classification spectrum (inactive and active), Benski's procedure has the highest percentages.

The results shown in Table A9 are for the consistency of the methods to deem the same effect as possibly active. As described in the earlier sections of this chapter, the major differences between the methods were between Benski's procedure and the other three methods as a group. If we look down the column for Benski's procedure we see that the remaining three procedures all provided similar classifications 97% of the time. There were only 2 occasions (out of 63) when Benski's procedure deemed an effect as active and the other methods did not classify the same effect as possibly active. However, when we consider the number of times that the other procedures deemed an effect as possibly active and Benski's procedure agreed the consistency ratios range from 50% to 55%. This can be seen by looking across the Benski row of Table A9. The

conclusion drawn from this information is that whenever Benski's procedure declared an active effect the other procedures usually classified it similarly, but the opposite was not true when the other procedures found an effect that was possibly active.

When we further explore the information in Table A9, with regards to Box and Meyer's, Lenth's, and Schneider, Kasperski, and Weissfeld's procedures, we see that the consistency ratios range from 87% to 98%. The agreement between the procedures when Lenth's method deemed an effect as possibly active was the highest of the three. However, the consistency ratios for the three procedures overall indicated quite similar results.

Table A10 provides the classification results for the four methods across the six individual types of designs and overall. The patterns that are most easily observed are as follows. First, Benski's procedure always had the highest number of effects deemed inactive (and therefore the fewest number of probable actives), regardless of the type of design considered. This again probably goes back to the fact that Benski's procedure only classifies effects into two categories, active or inactive, whereas the other procedures all had a zone of uncertainty where an argument could be made for the effect being significant. Second, Benski's procedure also deemed the largest number of effects as active in 5 of the 7 cases. On the two occasions when another procedure declared more actives, Benski's method declared only one or two fewer active effects. Third, the Schneider, Kasperski, and Weissfeld procedure deemed the greatest number of effects as possibly active in all but one instance (Plackett-Burman). This may be due to the number of the smallest, absolute effects chosen to estimate the experimental error

variance, but as can be seen in Table A9 there were only 9 and 10 more possible active classifications than for Box and Meyer's and Lenth's procedures respectively. The robustness of the Schneider, Kasperski, and Weissfeld procedure has been demonstrated in the past and a change in the number of effects used to estimate the experimental error variance does not affect the overall results in a serious manner. Fourth, the Schneider, Kasperski, and Weissfeld procedure declared the most uncertain effects for each of the three groups of full factorial designs. Finally, Lenth's procedure declared the smallest number of active effects for six of the seven groupings. The only time it did not declare the fewest actives it was only one different from the lowest.

While these patterns may not be all of the patterns that exist in Table A10, they do help provide some insight into the similarities and differences between each of the four methods. Overall, the four methods can be used to analyze the same experimental data, but the results can be different depending on the procedure used.

CHAPTER 6. DANIEL'S NORMAL PROBABILITY PLOTTING PROCEDURE REVISITED

In this chapter, we reconsider Daniel's normal probability plotting procedure. Although it suffers from subjectivity on the part of the analyst, it has many beneficial uses, which will be explored in further detail.

The subjectivity of Daniel's normal probability plotting procedure has two main components. The first part involves drawing the straight line through the data points. Due to the fact that there are no strict guidelines as to how to draw this line, different analysts can, and usually do, draw different lines to fit the data. The second part of the subjectivity involves the assessment of whether or not a data point falls far enough off the straight line to be deemed significant. Again, different people can see the same picture quite differently. Of these 2 sources of subjectivity, this chapter will examine a technique that provides an approximate line through the data based on the estimate of the standard deviation from the Schneider, Kasperski, and Weissfeld procedure.

6.1. How to Draw the Line

Daniel's normal probability plotting procedure was previously described in Section 3.2.1. The procedure for constructing a normal probability plot is as follows:

1) Obtain estimates of the n effects using Yates' algorithm.
2) Order the n effects from smallest to largest, $x_{(1)} \le x_{(2)} \le \ldots \le x_{(n)}$.
3) Compute the i^{th} expected normal value, $y_{(i)}$, using the normal distribution function

$$P[z \le y_{(i)}] = \int_{-\infty}^{y(i)} \frac{1}{\sqrt{2\pi}} e^{-\frac{z^2}{2}} dz = (i-\frac{3}{8})/(n+\frac{1}{4}).$$

The above probability is the probability of observing a value $z \le y_{(i)}$ when we have a standard normal distribution.

4) Plot the data pairs $(x_{(i)}, y_{(i)})$

5) Draw a straight line that passes through the origin, which best describes the data.

6) Assess the fit of the line by determining which factor effect estimates fall far enough off the line to be deemed active.

When drawing the straight line that best fits the data, we must force the line to go through the point (0,0) as stated in step 5. This point corresponds to the 50th percentile of the normal distribution on the y-axis. Daniel (1976) states that the data points near the 16th and 84th percentiles of the normal distribution should be given a larger influence in the determination of the slope of the line.

The question that arises most often when creating a normal probability plot is how to draw the line through the data, i.e. what should be the slope of the line? As previously mentioned, different analysts usually visualize different lines to fit the data, which constitutes one of the major criticisms of this technique. If there were some way to eliminate this subjectivity the procedure could be improved. Daniel's suggestion of allowing those points closest to the 16th and 84th percentiles to have a larger influence on determining the slope of the line is a place to start, but it does not totally rectify the situation.

In comparing the other procedures used to evaluate unreplicated designs, only the Schneider, Kasperski, and Weissfeld procedure provides information that can be used to help eliminate some of the inherent subjectivity. Therefore, the Schneider, Kasperski, Weissfeld procedure's standard deviation estimate will be utilized in the next section.

6.2. Using the Schneider, Kasperski, and Weissfeld Procedure with the Normal Probability Plot

Since the Schneider, Kasperski, Weissfeld procedure generates an estimate of the standard deviation, this value can be used to help draw the line through the data. The actual way in which the standard deviation estimate is used in the normal probability plot is given below:

1) Determine the two points $+ 3\ \sigma$ and $- 3\ \sigma$ using the Schneider, Kasperski, Weissfeld estimate of σ,

2) Draw two points at ($x = -3\sigma$, $y = -3$) and ($x = 3\sigma$, $y = +3$) on the normal probability plot,

3) Draw a straight line that passes through the origin and connects the two points defined in step 2.

These 3 steps can be inserted in place of step 5) in Daniel's original procedure. As in Daniel's original procedure, the fit of the line is then assessed to determine which points fall far enough off the line to be deemed active.

The reasoning behind the use of $\pm\ 3\sigma$ is based on the theory of the normal distribution. Under the assumption that the effect estimates follow a normal distribution, approximately 100% of the data should lie within those limits. Thus, the line connecting the 2 points at $\pm\ 3\ \sigma$ should cover the range of the data if the effects do indeed follow

a normal distribution. Significant effects will be those points that fall far enough off the line. It should be noted that any other 2 points (e.g. $\pm\ \sigma$ or $\pm\ 2\sigma$) may be used as well.

An illustration of the procedure is shown in Figure 6.2.1. Figure 6.2.1. is the modified normal probability plot of Example 3 from the 2^4 full factorial designs. The line connecting the two points drawn at $\pm\ 3\ \sigma$ appears to fit the data quite well. Four data points clearly fall far from the line and would be deemed active. This conclusion would be consistent with the results obtained with each of 4 the previous methods as discussed in Chapter 5.

At this point, an analyst may be compelled to use the modified normal probability plotting procedure in hopes of obtaining such "good" results with all unreplicated designs. However, there are times at which the procedure needs to be slightly altered. A more in-depth discussion of the possible problems are described in the following section.

6.3. Problems Encountered with the Modified Plotting Procedure

In order to illustrate one of the possible problems encountered with the modified procedure, consider example 1 from the 2^4 full factorial designs. Figure 6.3.1 shows the modified normal probability plot of the effect estimates along with the line connecting the $+3\sigma$ and -3σ points. As can clearly be seen, the line does not fit the data well, nor does there appear to be any straight line that would pass through the origin and fit the data. In this case, the underlying assumption of normality for the inactive effects is questionable. The results of the four other procedures indicated that the 3 largest effects

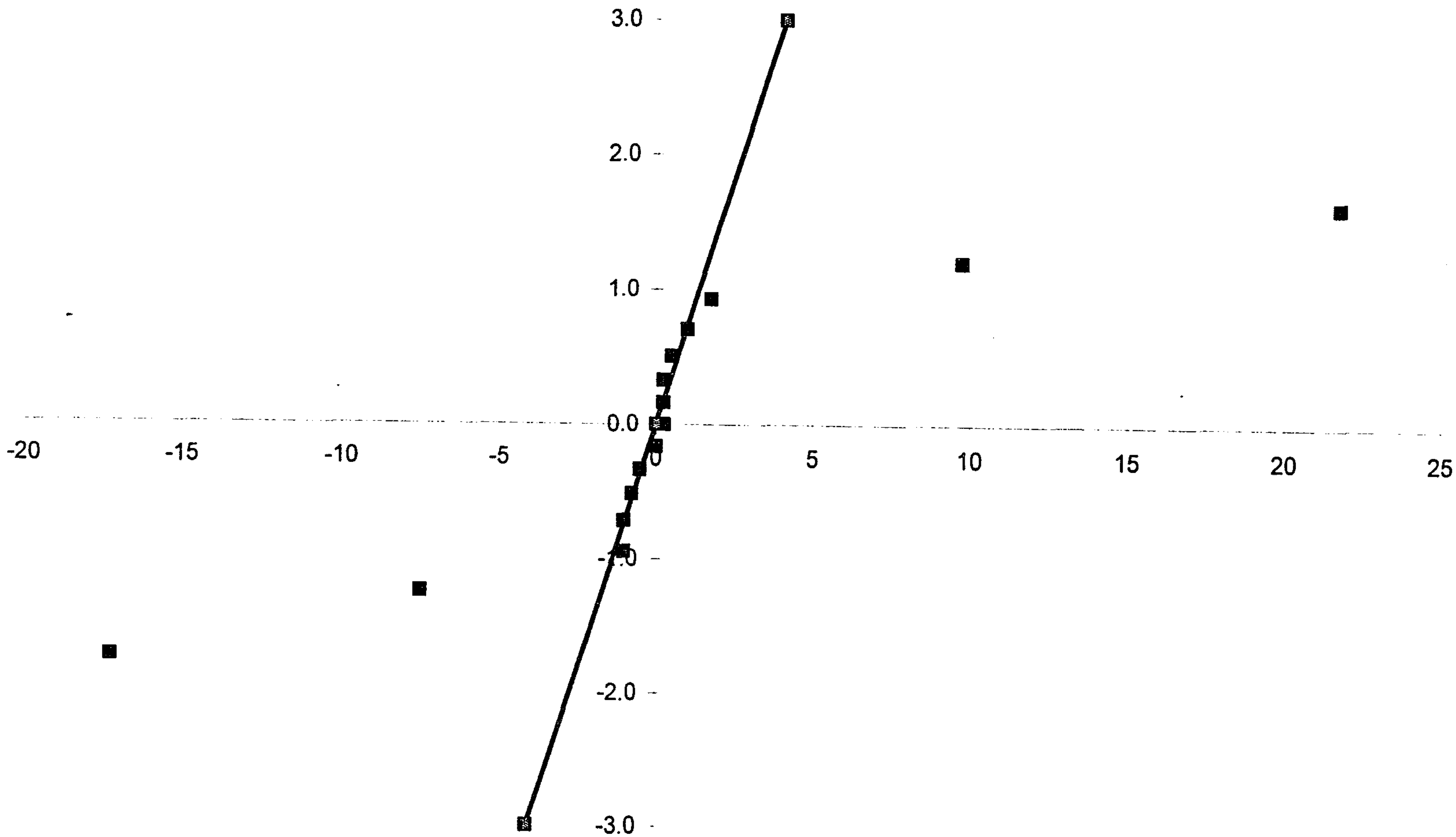

Figure 6.2.1.
Modified Normal Probability Plot of 2^4 Factorial Example 3

were each deemed to be active. However, these results may be misleading due to the probable normality violation.

Thus, in order to solve this problem so that the modified normal probability plotting can be applied something must be done to the data to better satisfy the normality assumption. One of the options would be the use of a data transformation (natural logarithm, square root, etc.) to better achieve normality and then one could apply the modified normal probability plotting technique. This option is explored in further detail in the next section.

The modified normal probability plot for Example 1 from the Plackett-Burman designs is shown in Figure 6.3.2. on the previous page. As Box and Meyer (1993) originally reported, only 1 effect stands out from the rest, that being effect 2 (10.58). A closer examination of the plot shows definite gaps between the groupings of effects that fall along nearly parallel lines. Daniel (1976) and Box and Draper (1987) suggested that this may indicate that the original observations contained outliers.

The possible consequences of outliers have been described by Box (1991), Box and Meyer (1993), and Torres (1993). Box (1991) noted that an unusual data value would cause a split in the middle of the normal probability plot. Box and Meyer (1993) described more fully the effects of an outlier. When the outlier was biased by a positive (negative) amount, then all of the effect estimates that the outlier is involved in positively (negatively) will be shifted to the right. For those effect estimates, where the outlier takes on a positive (negative) value, the effects will be shifted to the left. Therefore, a

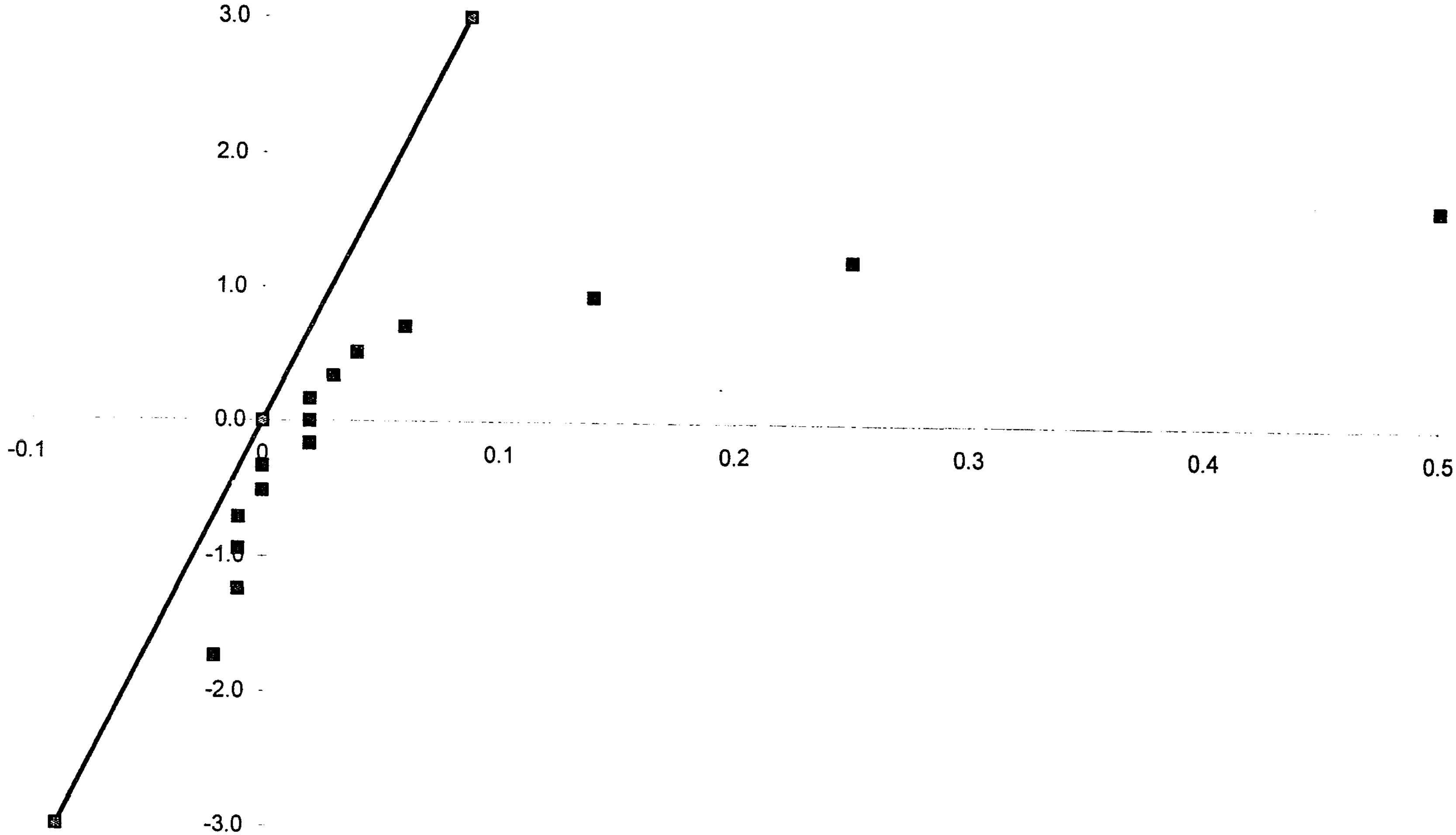

Figure 6.3.1.
Modified Normal Probability Plot of 2^4 Factorial Example 1

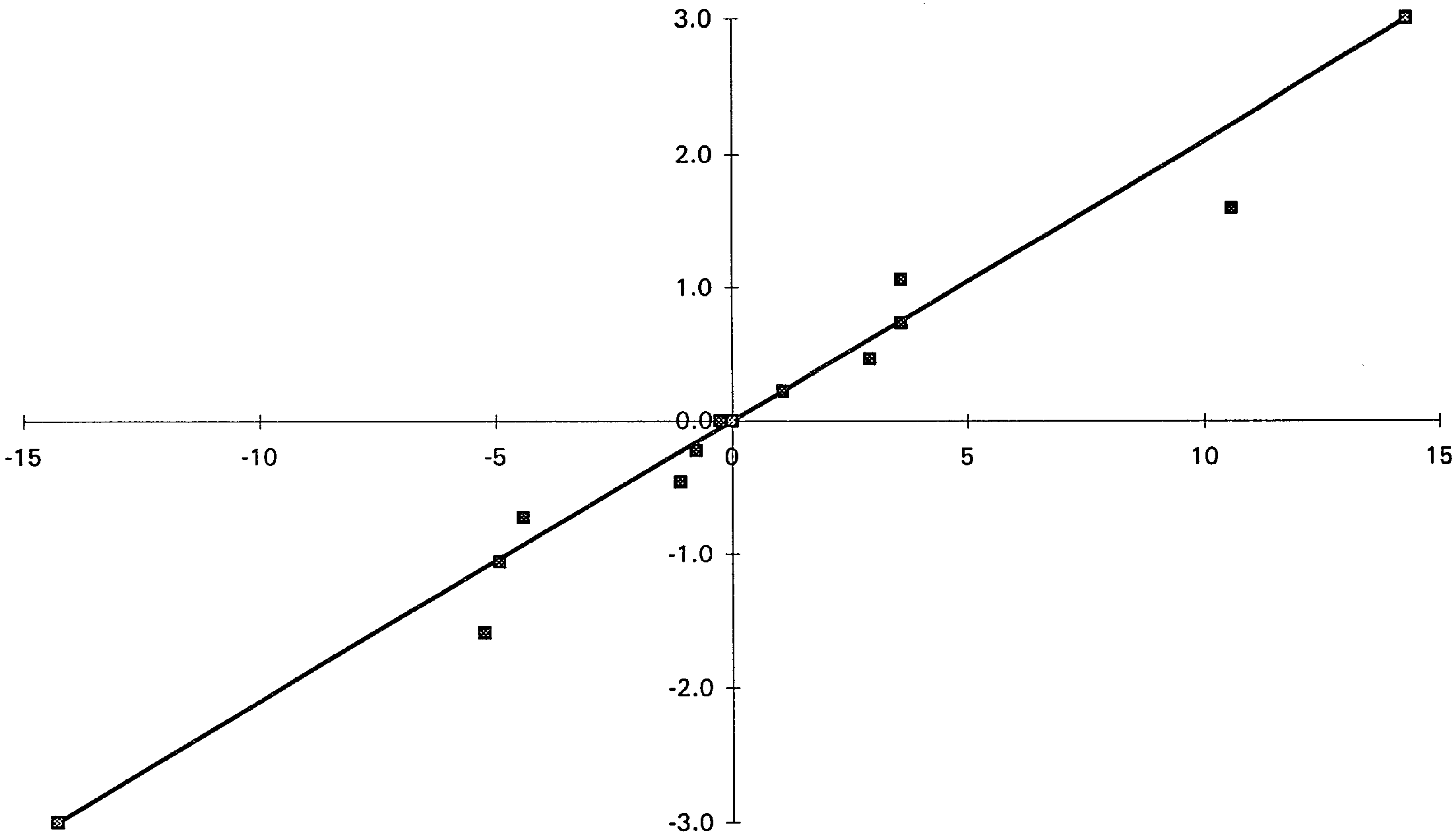

Figure 6.3.2
Modified Normal Probability Plot of Plackett-Burman Example 1

probable outlying value will be one that exists as a positive (negative) value for the positive effect estimates and as a negative (positive) value in the negative contrasts (Box and Meyer, 1993).

If we follow this process for this particular example, none of the observations are diagnosed as a definite outlier. However, if we examine the confounding scheme of this Plackett-Burman design, as shown in Table 6.1 (Box and Meyer's (1993) Table 2), an explanation for the peculiarities in Figure 6.3.2. can be found. As can be seen in Table 6.1, the main effects are confounded with all of the 2-factor interactions that do not involve that given factor. As this problem is a subset of the 2^5 Reactor Example from Box, Hunter, and Hunter (1978), also shown as Example 4 in the 2^5 designs, the BD and DE interactions are known to be large positive and negative effects. When we take these into consideration when examining the alias strings in Table 6.1, it is clear that effects 1 and 9 are shifted over in a positive direction, while effects 8 and 11 are shifted in a negative manner. The remaining effect estimates are not affected as much. Box and Meyer (1993) described the overall effect of these happenings as creating the rather obscure results shown in Figure 6.3.2., as compared to the analysis of the entire 2^5 design. Important factors may not be as readily apparent due to the unusual pattern of the data points in the normal probability plot.

6.4. Data Transformations

When the underlying assumptions of an experiment are violated, the results obtained are often misleading and may lead to inaccurate conclusions. In the context of

Table 6.1. Alias Strings for Plackett-Burman Example 1

Effect	Alias String
1	A + 1/3 (-BC + <u>BD</u> + BE - CD - CE - <u>DE</u>)
2	B + 1/3 (-AC + AD + AE - CD + CE - <u>DE</u>)
3	C + 1/3 (-AB -AD - AE - <u>BD</u> - BE - <u>DE</u>)
4	D + 1/3 (AB - AC - AE -BC - BE - CE)
5	E + 1/3 (AB - AC - AD + BC - <u>BD</u> - CD)
6	1/3 (-AB + AC - AD + AE + BC - <u>BD</u> - BE + CD - CE - <u>DE</u>)
7	1/3 (-AB - AC - AD + AE - BC + <u>BD</u> - BE + CD - CE + <u>DE</u>)
8	1/3 (AB + AC - AD - AE - BC - <u>BD</u> - BE - CD + CE + <u>DE</u>)
9	1/3 (-AB - AC - AD - AE + BC + <u>BD</u> - BE - CD - CE - <u>DE</u>)
10	1/3 (-AB - AC + AD - AE - BC - <u>BD</u> - BE + CD + CE - <u>DE</u>)
11	1/3 (-AB + AC + AD - AE - BC - <u>BD</u> + BE - CD - CE + <u>DE</u>)

unreplicated factorial and fractional factorial designs, this can mean that inactive effects are wrongfully deemed active, while significant effects go undetected. Both of these problems can be serious, so the use of a data transformation may be in order to better evaluate the situation.

Torres (1993) and Bisgaard (1993) both discussed the uses of transformations when analyzing factorial designs. Torres (1993) suggested a Box-Cox transformation to assist the analyst so that inactive effects will not be diagnosed as significant. A disadvantage of this procedure is that it requires a substantial statistical understanding to be used in an effective manner.

Bisgaard (1993) examined the situation through the use of residual analysis. Under the assumption that there are not any "bad" values in the data, a tentative model

is identified that includes those effects deemed to be active. The inverse Yates' algorithm (see Hunter (1966) for further details) is then used to obtain estimates of the residuals.

Once the residuals are obtained, they may be plotted in different ways. A normal probability plot of the residuals may be used to identify problematic data values. Another diagnostic tool is the plot of the residuals versus the predicted values. As Box, Hunter, and Hunter (1978) eloquently described in their book, an examination of the dependence between the mean μ and the standard deviation of the observations σ_y can help identify a transformation to help stabilize the variance. Bisgaard (1993) listed an edited version of an extensive table given by Box, Hunter, and Hunter (1978), p. 234), which is duplicated in Table 6.2. An example of the use of the table was given by Bisgaard (1993). In the problem under consideration, as the predicted values increased, the residuals became more spread out and the resulting residual plot was funnel shaped. This, in turn, was an indication that the error variance was dependent upon the mean and a logarithmic transformation was in order.

Once the data are transformed, a new normal probability plot may be created to assess which effects are significant. However, as Bisgaard (1993) stated "... if the data happened to be recorded on a scale different from the one where the factors naturally act linearly on the response, then the non-linearity in the response surface introduced by going from one scale to another may produce the *appearance* of interaction effects." Thus, we must be careful when we apply any transformation to the data.

Besides the transformations listed in Table 6.2., Torres (1993) introduced the use of a rank transformation of the data to eliminate abnormalities. The original data are ordered from smallest to largest and ranks are assigned to the data. If ties exist between the data points, then a non-parametric procedure is used to assign the average of the ranks to each tied data point. A normal probability plot can then be produced using the ranks for the effect estimates. When the results from the ranks' normal probability plot agrees with the results obtained using the original data then no abnormalities are deemed to be present. In the case of a dramatic difference between the 2 analyses, Torres (1993) states that "abnormalities may exist and the analysis based on ranks is probably more accurate than the analysis on the original data." Using ranks to analyze experiments has been done in the past (see MacDonald (1971), Iman (1974), and Conover and Iman (1976)), but none of these references addressed the situation encountered in unreplicated factorial designs.

6.5. Analysis of 2^4 and Plackett-Burman Designs Using the Modified Normal Probability Plotting Procedure

The modified normal probability plotting procedure described in sections 6.1 - 6.2 was applied to the 2^4 designs (both full and fractional factorials) along with the Plackett-Burman designs to demonstrate its use in identifying active effects. The results are shown in Appendix B, Figures B1 - B 25. Also, Table 6.3 provides a summary of the modified normal probability plotting results.

In the case of the full factorial designs, we see that the lines fit the data reasonable well in Figures B3 - B5. The effects that lie well off the lines would be

Table 6.2. A Range of Useful Variance Stabilizing Transformations

Dependence of σ_y on μ	Variance Stabilizing Transformation	
$\sigma_y \propto \mu^2$	$1/y$	Reciprocal
$\sigma_y \propto \mu^{3/2}$	$1/\sqrt{y}$	Reciprocal square root
$\sigma_y \propto \mu$	ln(y)	Log[a]
$\sigma_y \propto \mu^{1/2}$	$\sqrt{y}$	Square root
$\sigma_y \propto$ constant	y	No transformation

[a]Either the natural log or the base 10 log can be used.

deemed active and these results are consistent with those obtained with the 4 numerical methods as described in Chapter 5. When the line does not fit the data well, as is the case with Figures B1, B2, and B6, then the results are questionable. In these instances, more detailed analyses involving a transformation (rank, square root, log, etc.) should be utilized before assessing the significance of effects.

An interesting observation of the Plackett-Burman plots, in Figures 6.3.2. and B21 - B24, is that none of the lines fit the original data well. All of the plots show rather large gaps in the data, which could lead one to believe that "bad" data values as described by Box (1991) are obscuring the picture too much to correctly assess the situation. Other possible reasons for the lines not fitting the data well are those described earlier in this chapter including outliers and interactions.

Table 6.3. Summary of Modified Normal Probability Plotting Procedure Results

Line Fits Data	Design Type	Line Does Not Fit Data
Ex 3,4,5	2^4 full factorials	Ex 1,2,6
Ex 4,5,8,11,12,13	2^4 fractional factorials	Ex 1,2,3,6,7,9,10,14,15
	Plackett-Burman	Ex 1,2,3,4

CHAPTER 7. SUMMARY AND CONCLUSIONS

The use of designed experiments will continue to play an important role in determining which factors are the most influential on the product or process. The ability to isolate and then control the influence of these factors can help to improve the quality. Within the realm of designed experiments, unreplicated factorial and fractional factorial designs have grown in prominence because of their ability to provide helpful insights with less investment of time, money, and effort than their replicated counterparts. However, the lack of a formal estimate of the experimental error variance has led to several different procedures being proposed for analyzing these experiments. The various methods proposed over the years to analyze these unreplicated designs are from Daniel (1959, 1976), Box and Meyer (1986, 1993), Lenth (1989), Benski (1989), and Schneider, Kasperski, and Weissfeld (1993). In each case, the authors of the later papers claimed that their procedure provided similar results as the earlier procedures, but with more ease of understanding. The analyst was therefore left with the choice 5 procedures that supposedly provided similar results, but these claims were based on only a maximum of 5 published examples, which were considered by Schneider, Kasperski, and Weissfeld.

This research has addressed the issue of evaluating each of these procedures in a variety of commonly found experimental settings to determine what, if any, differences exist between the methods. Based upon the differences between the procedures, an analyst can use this information to employ the "best" procedure in his or her particular experimental setting. Also, this research has examined the use of Daniel's normal probability plot combined with the estimate of the standard deviation from the Schneider,

Kasperski, and Weissfeld procedure in hopes of eliminating part of the inherent subjectivity of the procedure.

The reason for performing an empirical study rather than a simulation study to evaluate the performance of the procedures was outlined in Chapter 5. Within the context of a simulation study, active effects are thought to arise from a normal distribution with an inflated variance, as compared to the supposed inactive effects. A major concern lies in the fact that the simulation program could create active effects that are smaller than the simulated inactive effects. Is it not more important to find the largest effects that may influence our response variable, rather than small active factors that are unimportant when compared to the remaining effects? The simulation program never considered the size of the effect, only whether or not it came from the active or inactive distribution. In real life, however, the size of the effect is important because of its influence on the product or process. Also, before we perform an experiment, we do not know which effects come from the active or inactive distributions. In performing the experiment we want to determine those effects that are the most influential. This is why the analysis in this research was based upon an empirical study and not a simulation study.

All of the methods considered have at least some subjectivity when it comes to determining whether or not a particular factor effect is deemed significant, but Daniel's normal probability plotting procedure is often viewed as the most subjective. This is due to the fact that the procedure is subjective both with respect to drawing the straight line through the data points and with assessing whether or not a point falls far enough from

the line to be deemed significant. Although the line must pass through the origin, there are numerous lines that may be drawn through the graph depending on the analyst's perception of the situation. The work shown in Chapter 6 is an attempt to help eliminate some of this subjectivity. By utilizing the standard deviation estimate from the Schneider, Kasperski, and Weissfeld procedure a useful guide for drawing the line through the data has been devised. As was illustrated in Chapter 6, the line through the data may exhibit a close fit or an extremely poor fit.

The usefulness of the normal probability plot cannot be discredited because of the procedure's subjectivity. As Daniel himself suggested (and others have endorsed), plotting the data can shed light upon possible model inadequacies. One major modeling assumption that may be violated, but overlooked by the analyst, is the normality of the supposed inactive effects. This violation, however, can be revealed through the use of the normal probability plot. The examples shown in Chapter 6 and the Appendix where the line poorly fits the data may be an indication of this violation. The line based upon the Schneider, Kasperski, and Weissfeld standard deviation estimate may not always provide the best fit, but it does give a logical starting point for drawing the line on the graph. An experiment that does exhibit a peculiarity, e.g. a curvilinear in the normal probability plot, should be reassessed before proceeding with any of the quantitative procedures to determine the active effects. A suggested course of action might be a transformation of the data to help achieve normality of the inactive effects and then proceed with one of the quantitative methods.

The subjectivity for Box and Meyer's, Lenth's, and Schneider, Kasperski, and Weissfeld's procedures all involve the zone of uncertainty. For the Lenth and Schneider, Kasperski, and Weissfeld procedures this occurs when the effect estimate or its corresponding t-value lies between the inner and outer limits. As Lenth discussed in his paper, an argument can be made for the effect being deemed significant, but also for the effect being declared inactive. The suggestion made at this point is to further investigate the factor that lies in this uncertain zone.

The zone of uncertainty can also be applied to Box and Meyer's bayesian procedure because active effects will have posterior probabilities close to 1, while inactive effects have posterior probabilities close to 0. The question that arises is, "How close is close to 1 (or 0) respectively?". An allowance can be made in much the same way as in the Lenth's and Schneider, Kasperski, and Weissfeld's procedures. Effects with posterior probabilities of at least .95 were deemed clearly active, while those effects with posterior probabilities between .5 and .94 were considered to be in the uncertain zone. The cutoffs can be arbitrarily chosen by the individual analyst, but these particular values were used to provide a sense of consistency with the other methods.

The subjectivity surrounding Benski's method is minimal with respect to how small the significance level, P_1, of W'-test for normality must be in order to continue with the fourth-spread test. The assumption made in the analyses was that $P_1 \leq .05$ indicated a small enough value to move to the fourth-spread test. The choice of .05 was consistent with that for determining the limits for both Lenth's and Schneider, Kasperski, and Weissfeld's procedures.

The main thrust of this research was to determine what, if any, differences there are between the procedures and the extensive analyses suggest that there do exist differences between the four quantitative methods. Again, because of its non-quantitative results, Daniel's normal probability plot was not considered for these groupings. Two distinct groupings of the methods arose from the analyses. In one group there were Box and Meyer's, Lenth's, and Schneider, Kasperski, and Weissfeld's procedures. The second group consisted solely of Benski's procedure.

The groupings were based primarily upon the consistency of the procedures in classifying the same effects as either inactive or possibly active, which included both uncertain and active. The analyses in Chapter 5 and the results summarized in the Tables in Appendix A support these suggested groupings. As was usually the case, there were only minor differences between the three procedures in the first group, regardless of the type of design under consideration. The situation never arose where one of the three methods declared an effect as active and the others declared the same effect as inactive. In each instance, when an effect was deemed active by one of the procedures the other procedures deemed the same effect as either active or uncertain. Also, on several occasions when an effect was deemed uncertain by one of the methods, the other methods either deemed the effect as uncertain or were extremely close to declaring the effect uncertain. An examination of Tables A1.7 - A6.7 reveals that there were only 29 instances where Box and Meyer's, Lenth's, and Schneider, Kasperski, and Weissfeld's procedures were not in complete agreement on the classification of an effect as possibly

active. When considering the total number of effects analyzed, 720, this amounts to only a 4% (29/720) difference.

Benski's procedure was usually quite different when the number of possibly active effects were compared. As previously mentioned, this is a direct consequence of the procedure not having a zone of uncertainty. Effects fall into either the active group or the inactive group based on Benski's decision rule. This led to Benski's procedure being the most conservative method in terms of declaring effects as possibly active. However, Benski's procedure consistently produced the largest number of active classifications for the effects and on several occasions the procedure declared an effect to be active, while the remaining three procedures deemed the same effect as inactive. If the other methods deemed the effect as uncertain there would not be much cause for concern, but since the other methods found the same effect to be inactive this would be an indication of a definite difference.

At this point, the choice as to which of the procedures to use may come down to the ease of understanding. All of the procedures can easily be implemented on a computer, with FORTRAN programs available for each method. The running time for each procedure is negligible for the most part, except that with larger experiments Box and Meyer's procedure does require a noticeable amount more time because of the numerous calculations involved in computing the posterior probabilities. With respect to the ease of understanding for Box and Meyer's, Lenth's, and Schneider, Kasperski, and Weissfeld's procedures, we need to examine what underlying principles each procedure is based upon. Both Lenth's and Schneider, Kasperski, and Weissfeld's

procedures are based upon t-statistics for determining the significance of an effect. T-statistics are taught in introductory statistics courses, so there is not a high degree of statistical theory proficiency required to have an adequate understanding of how to apply these two procedures. The bayesian procedure of Box and Meyer, on the other hand, is derived from more complicated statistical theory that, while briefly mentioned in introductory statistics courses, is usually only fully explained in advanced statistical classes. From a hands-on approach, where the intended audience does not have a substantial statistical background, t-statistics are usually more easily understood. From this point of view, either Lenth's or Schneider, Kasperski, and Weissfeld's procedures could be employed by the analyst to obtain similar results. For an advanced audience, any of the three procedures would be applicable.

BIBLIOGRAPHY

Anderson, V.L. and McLean, R.A. (1974). Design of Experiments: A Realistic Approach. *Marcel Dekker, Inc.*, New York, New York.

Benski, H.C. (1989). "Use of a Normality Test to Identify Significant Effects in Factorial Designs". *Journal of Quality Technology* 21, pp. 174-178.

Bernard, A. and Box-Levenbach, E.D. (1953), "The Plotting of Observations on Probability Paper". *Statistica Neerlandica* 7, 163-173.

Bisgaard, S. (1993). "Iterative Analysis of Data from Two-Level Factorials". *Quality Engineering* 6(2), pp. 319-330.

Blom, G. (1958). Statistical Estimates and Transformed Beta-Variables. *John Wiley and Sons, Inc.*

Box, G.E.P. (1952). "Multi-factor Designs of First Order". *Biometrika* 39, pp. 49-57.

Box, G.E.P. (1980). "Sampling and Bayes' Inference in Scientific Modelling and Robustness". (With Discussion) *Journal of the Royal Statistical Society* Ser. A, 143, pp. 383-430.

Box, G.E.P. (1988). "Signal-to-Noise Ratios, Performance Criteria, and Box Transformations". *Technometrics* 30, pp. 1-17.

Box, G.E.P., (1991). "Finding Bad Values in Factorial Designs". *Quality Engineering* 3(3), pp. 405-410.

Box, G.E.P., and Cox, D.R. (1964). "An Analysis of Transformations". *Journal of the Royal Statistical Society* Series B, 26, pg. 211.

Box, G.E.P., and Draper, N.R. (1987). Empirical Model-Building and Response Surfaces. *John Wiley*, New York.

Box, G.E.P., Hunter, W.G. and Hunter, J.S. (1978). Statistics for Experiments. *John Wiley*, New York.

Box, G.E.P., and Meyer, R.D. (1993). "Finding the Active Factors in Fractionated Screening Experiments". *Journal of Quality Technology* Vol. 25, No. 2, pp. 94-105.

Box, G.E.P., and Meyer, R.D. (1986a). "An Analysis for Unreplicated Fractional Factorials". *Technometrics* 28, pp. 11-18.

Box, G.E.P., and Meyer, R.D. (1986b). "Dispersion Effects from Fractional Designs". *Technometrics* 28, pp. 19-27.

Box, G.E.P., and Wilson, K.B. (1951). "On the Experiment Attainment of Optimum Conditions". *Journal of the Royal Statistical Society* Series B 13, pp. 1-45.

Bullington, R.G., Lovin, S., Miller, D.M., and Woodall, W.H. (1993). "Improvement of an Industrial Thermostat Using Designed Experiments". *Journal of Quality Technology* Vol. 25, No. 4, pp. 262-270.

Cabau, E. and Benski, C. (unpublished) "Benchmarking Procedures for Detecting Effects in Unreplicated Factorial Designs."

Chang, C.D. and Kononenko, O.K. (1962). *Adhesives Age*, 5, 7, July 1962, pp. 36-40.

Cochran, W.G. and Cox, G.M. (1957). Experimental Designs. *John Wiley and Sons, Inc.*

Coleman, D.E. and Montgomery, D.C. (1993), "A Systematic Approach to Planning for a Designed Industrial Experiment". *Technometrics* Vol. 35, No. 1, pp. 1-12.

Conover, W.J. and Iman, R.L. (1976). "On Some Alternative Procedures Using Rands for the Analysis of Experimental Designs". *Communications in Statistics-Theory and Methods* A5, pp. 1349-1368.

Daniel, C. (1959). "Use of Half-Normal Plots in Interpreting Factorial Two-Level Experiments". *Technometrics* 1, pp. 311-341.

_______ (1976). Applications of Statistics to Industrial Experimentation. *John Wiley*, New York.

Davies, O. L. (ed.) (1971). The Design and Analysis of Industrial Experiments. *Hafner, MacMillan.*

Dey, Aloke (1985). Orthogonal Fractional Factorial Designs. *Halsted Press.*

Federer, Walter T. (1955). Experimental Design: Theory and Application. *The MacMillan Company.*

Fisher, R.A. (1966). The Design of Experiments (8th edition). New York: *Hafner Press.*

Gunter, B.H. (1993) Discussion of "A Systematic Approach to Planning for a Designed Industrial Experiment". By Coleman, D.E. and Montgomery, D.C., *Technometrics* Vol. 35, No. 1, pp. 13-14.

Gupta, A. K. (1952). "Estimation of the Mean and Standard Deviation of a Normal Population from a Censored Sample". *Biometrika* 39, pp. 260-273.

Hahn, G. (1977), "Some Thing Engineers Should Know About Experiment Design". *Journal of Quality Technology* 9, 13-20.

Hahn, G. (1984), "Experimental Design in a Complex World". *Technometrics* 26, 19-31.

Harter, H. (1969). "Order Statistics and Their Use in Testing and Estimation". Vol. 2, *Aerospace Research Laboratories*, pp. 27-28, 456.

Hoaglin, D.C. (1983). "Letter Values: A Set of Selected Order Statistics". *Understanding Robust and Exploratory Data Analysis* (D.C. Hoaglin, F. Mosteller, and J. W. Tukey, Eds.), *John Wiley and Sons*, New York, New York.

Hunter, J.S. (1966). "The Inverse Yates' Algorithm". *Technometrics* 8(1), pp. 177-183.

Hunter, G.B., Hodi, F.S., and Eager, T.W. (1982). "High-Cycle Fatigue of Weld Repaired Cast Ti-6Al-4V". *Metallurgical Transactions* 13A, pp. 1589-1584.

Iman, R.L. (1974). "A Power Study of a Rank Transform for the Two-way Classification Model when Interaction may be Present". *The Canadian Journal of Statistics* 2, pp. 227-239.

Johnson, L.G. (1964), The Statistical Treatment of Fatigue Experiments. *Elsevier*, New York.

Johnson, N.L. and Leone, F.C. (1977), Statistics and Experimental Design in Engineering and the Physical Sciences. Volume 2, 2nd ed., *John Wiley and Sons, Inc.*

Kempthorne, O. (1952). The Design and Analysis of Experiments. *John Wiley and Sons, Inc.*

King, J.R. (1971) "Probability Charts for Decision Making". *Industrial Press*, New York.

Lin, C. and Mudholkar, G.S. (1980), "A Simple Test for Normality Against Asymmetric Alternatives". *Biometrika* 67, pp. 455-461.

Lin, D.K.J. and Draper, N.R. (1992), "Projection Properties of Plackett and Burman Designs". *Technometrics* 34, No. 4, pp. 423-428.

Lenth, R. V. (1989). "Quick and Easy Analysis of Unreplicated Factorials". *Technometrics* 31, pp. 469-473.

MacDonald, P. (1971). "The Analysis of a 2^n Experiment by Means of Ranks". *Applied Statistics* 20, pp. 259-275.

Mason, R. K., Gunst, R. F. and Hess, James L. (1989). Statistical Design and Analysis of Experiments with Applications to Engineering and Science. *John Wiley and Sons, Inc.*

McLean, R.A. and Anderson, V.L. (1984). Applied Factorial and Fractional Designs. *Marcell Dekker, Inc.*

Milton, Roy C. and Hotchkiss, R. (1969). "Computer Evaluation of the Normal and Inverse Normal Distribution Function". *Technometrics* Vol. 11, No. 4, pp. 817-822.

Montgomery, D.C. (1991), Design and Analysis of Experiments. (3rd ed.) New York, *John Wiley and Sons.*

Natrella, M.G. (1979), "Design and Analysis of Experiments". *Quality Control Handbook*, ed. J.M. Juran, New York: McGraw-Hill, pp. 27-35.

Nelson, W. (1982). Applied Life Data Analysis. *John Wiley and Sons, Inc.*

Odeh, R.E. and Evans, J.O. (1974). "The Percentage Points of the Normal Distribution". (Algorithm AS70), *Applied Statistics* Vol. 23, No. 1, pp. 96-97.

Olsson, D. M. (1979). "Analysis of Two-Level Factorial Experiments". *Journal of Quality Technology* 14, pp. 95-98.

Olsson, D.M. (1981). "Corrigenda". *Journal of Quality Technology*, 13, p. 282.

Ott, E.R. (1967). "Analysis of a Means - A Graphical Procedure". *Industrial Quality Control* 24, pp. 101-109.

Peng, K. C. (1967). The Design and Analysis of Scientific Experiments. *Addison-Wesley Publishing Co., Inc.*

Plackett, R.L. and Burman, J.P. (1946). "Design of Optimal Multifactorial Experiments". *Biometrika* 23, pp. 305-325.

Quinlan, J. (1985). "Product Improvement by Application of Taguchi Methods". *American Supplier Institute News* (special symposium ed.), American Supplier Institute, Dearborn, Michigan, pp. 11-16.

Rand Corporation (1955). A Million Random Digits With 100,000 Normal Deviates. Glencoe, IL: Free Press.

Schneider, H. (1986). Truncated and Censored Samples from Normal Populations, Marcel Dekker, New York.

Schneider, H., Kasperski, W. and Weissfeld, L. (1993). "Finding Significant Effects for Unreplicated Fractional Factorials Using the n Smallest Contrasts". *Journal of Quality Technology* 25, pp. 18-27.

Schneider, H., Pruett, J.M. and Magee. L. (1994) "Reduction of Variation in the Gasoline Blending Process - A Case Study". To appear ISA Transactions.

Shapiro, S. S. and Wilk, M. B. (1965). "An Analysis of Variance Test for Normality (Complete Samples)". *Biometrika* 52, pp. 591-611.

Shapiro, S.S. (1980) "How to Test Normality and Other Distributional Assumptions". Vol. 3., ASQC Basic References in Quality Control: Statistical Techniques, *American Society for Quality Control*, Milwaukee, WI.

Stegner, D.A.J., Wu, S.M., and Braton, N.R. (1967), "Prediction of Heat Input for Welding." *Welding Journal Research Supplement*, 1, March 1967.

Taguchi, G., and Wu, Y. (1980). *Introduction to Off-line Quality Control*, Nagoya, Japan: Central Japan Quality Control Association.

Tippett, L.H.C. (1934). "Applications of Statistical Methods to the Control of Quality in Industrial Production". *Journal of the Manchester Statistical Society.*

Torres, V.A. (1993). "A Simple Analysis of Unreplicated Factorials with Possible Abnormalities". *Journal of Quality Technology*, Vol. 25, No. 3, pp. 183-187.

Tukey, J.W. (1962). "The Future of Data Analysis". Ann. Math. Stat., 33, 1-67.

Wilk, M. B., Gnanadesikan, R. and Freeny, A. E. (1963). "Estimation of Error Variance from Smallest Ordered Contrasts". *JASA* 58, pp. 152-160.

Yates, F. (1937). "The Design and Analysis of Factorial Experiments". *Imperial Bureau of Soil Science*, Harpenden, England.

APPENDIX A. TABLES OF ANALYSES

Benski's Procedure, Olsson's Modification

Olsson's procedure can be implemented as follows. Let the y_i be the ordered components of Yates' final column of factor effects. Then we define W′ as

$$W' = \frac{\left(\sum_{i=1}^{n} m_i y_i\right)^2}{\sum_{i=1}^{n} m_i^2 \cdot \sum_{i=1}^{n} (y_i - \bar{y})^2}$$

where $m_i = \Phi^{-1}(p_i)$, Φ^{-1} is the inverse normal distribution, and $p_i = (i-\alpha)/(n-2\alpha+1)$. The value for α is given by the following formulas:

$$\alpha = \begin{cases} 0.275499 + 0.072884*[\ln(n)]^{0.41148} & if\ 1 < i < n \\ 0.205146 + 0.1314965*[\ln(n)]^{0.226701} & if\ i = 1\ or\ i = n. \end{cases}$$

Then the probability of rejecting the normality hypothesis is given by $P_1 = \exp(c)$, for $P_1 > .005$, where

$$C = \frac{(W'-A)/B + 0.0486128}{0.02760309} - \ln(100)$$

$$A = 1.031918 - 0.183573*(0.1*n)^{-0.5447402}$$

$$B = -0.5084706 + 2.076782*(0.1*n)^{0.4905993}.$$

Table A1
Results for 2^3 Factorial Designs

		EFFECTS						
		1	2	3	4	5	6	7
	BAYES	0	1	0	1	0	1	0
EX 1	BENSKI	0	0	0	0	0	0	0
	LENTH	0	1	0	0	0	0	0
	SKW	0	1	0	1	0	0	0
	BAYES	0	2	0	0	0	0	0
EX 2	BENSKI	0	2	0	0	0	0	0
	LENTH	0	1	0	0	0	0	0
	SKW	0	1	0	0	0	0	0
	BAYES	1	1	0	1	0	0	0
EX 3	BENSKI	0	0	0	0	0	0	0
	LENTH	2	2	0	1	0	0	0
	SKW	2	1	0	1	0	0	0
	BAYES	2	0	0	0	0	0	0
EX 4	BENSKI	2	0	0	0	0	0	0
	LENTH	2	1	0	1	0	0	0
	SKW	2	1	0	1	0	0	0
	BAYES	2	0	0	2	0	0	0
EX 5	BENSKI	2	0	0	2	0	0	0
	LENTH	1	0	0	1	0	0	0
	SKW	2	0	0	1	0	0	0
	BAYES	2	0	0	0	0	0	0
EX 6	BENSKI	2	0	0	0	0	0	0
	LENTH	2	0	0	0	0	0	0
	SKW	2	0	0	0	1	0	0

	BAYES	1	0	0	1	0	0	0
EX 7	BENSKI	0	0	0	0	0	0	0
	LENTH	1	0	0	1	0	0	0
	SKW	1	0	0	1	0	0	0
	BAYES	0	1	0	0	0	0	0
EX 8	BENSKI	0	0	0	0	0	0	0
	LENTH	0	1	0	0	0	0	0
	SKW	0	1	0	0	0	0	0
	BAYES	1	0	0	0	0	0	0
EX 9	BENSKI	0	0	0	0	0	0	0
	LENTH	1	0	0	0	0	0	0
	SKW	1	0	0	0	0	0	0
	BAYES	2	1	2	0	0	0	0
EX 10	BENSKI	0	0	0	0	0	0	0
	LENTH	2	1	2	0	0	0	0
	SKW	2	1	2	0	0	0	0
	BAYES	1	0	0	1	0	0	0
EX 11	BENSKI	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0
	SKW	1	0	0	0	0	0	0
	BAYES	0	2	0	1	0	1	0
EX 12	BENSKI	0	2	0	0	0	0	0
	LENTH	0	2	0	0	0	1	0
	SKW	0	2	0	1	0	1	0
	BAYES	0	2	0	0	0	0	0
EX 13	BENSKI	0	2	0	0	0	0	0
	LENTH	0	2	0	0	0	0	0
	SKW	0	2	0	0	0	0	0

	BAYES	0	0	0	1	0	0	0
EX 14	BENSKI	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0
	SKW	0	0	0	0	0	0	0
	BAYES	1	2	0	0	0	0	0
EX 15	BENSKI	0	2	0	0	0	0	0
	LENTH	1	2	0	0	0	0	0
	SKW	1	2	0	0	0	0	0
	BAYES	0	0	0	0	0	0	0
EX 16	BENSKI	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0
	SKW	0	0	0	0	0	0	0
	BAYES	0	0	0	0	0	0	0
EX 17	BENSKI	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0
	SKW	0	0	0	0	0	0	0
	BAYES	0	0	0	0	0	0	0
EX 18	BENSKI	2	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0
	SKW	0	0	0	0	0	0	0

Table A1.1
Effects, ME, SME, and Sigma for 2^3 Factorial Designs

Effect	Estimates	2^3 Designs								
	Effects									SKW
Example	1	2	3	4	5	6	7	ME	SME	SIGMA
Ex 1	15.5	-132.5	13.5	-73.5	1.5	47.5	2.5	76.14	182.45	20.663
Ex 2	1.25	-4.85	-0.6	0.6	0.15	0.45	-0.5	3.1	7.43	0.8363
Ex 3	0.75	-0.59	-0.03	-0.35	-0.03	-0.04	-0.08	0.197	0.473	0.1041
Ex 4	-575	-90	10	-65	15	10	-10	56.4	135.15	20.931
Ex 5	2.675	-0.225	0.075	-1.825	-0.425	-0.13	-0.225	1.269	3.041	0.3162
Ex 6	24.5	-5	1.5	1	7.5	2	-0.5	8.46	20.272	2.6854
Ex 7	5.063	1.0625	0.688	4.813	2.438	0.438	-0.188	3.878	9.292	1.3941
Ex 8	-2.25	3.25	-0.75	-1.75	0.25	-0.25	-0.25	2.82	6.76	0.9576
Ex 9	23	-5	1.5	1.5	10	0	5.5	18.33	43.924	6.2871
Ex 10	11.25	4.75	9.25	-0.25	0.25	0.75	-0.75	2.82	6.76	1.0371
Ex 11	11.3	-3.2	-4.65	7	0.75	0.45	-0.2	18.048	43.248	3.982
Ex 12	1	20	0	5	-1	6	1	5.64	12.515	1.468
Ex 13	-2.25	-43.75	3.75	0.25	0.75	-1.75	-3.25	11.28	27.03	2.9819
Ex 14	-1.25	4.25	4.75	15.25	3.75	5.25	-5.25	26.79	64.2	6.7129
Ex 15	-7.25	-29.25	-2.25	-0.25	-1.25	-1.25	1.75	7.05	16.89	2.3836
Ex 16	-3.55	2.8	-0.1	5.45	-0.35	-2.8	-2.4	15.79	37.84	3.7185
Ex 17	11.5	-9.5	8.5	6	9	1	-7	47.94	114.88	11.7235
Ex 18	-16.5	7	7.5	6.5	4	3.5	5	36.66	87.85	9.0168

Table A1.2
Box and Meyer's Posterior Probabilities for 2^3 Factorial Designs

Example	1	2	3	4	5	6	7
Ex 1	0.08	0.88	0.06	0.74	0.02	0.62	0.02
Ex 2	0.25	0.98	0.04	0.04	0.02	0.03	0.04
Ex 3	0.94	0.92	0.03	0.89	0.03	0.03	0.08
Ex 4	1	0.25	0.02	0.11	0.03	0.02	0.02
Ex 5	0.98	0.04	0.02	0.97	0.16	0.03	0.04
Ex 6	0.98	0.26	0.03	0.03	0.49	0.04	0.02
Ex 7	0.63	0.1	0.04	0.62	0.42	0.03	0.02
Ex 8	0.46	0.59	0.16	0.41	0.03	0.03	0.03
Ex 9	0.88	0.14	0.03	0.03	0.43	0.02	0.16
Ex 10	0.97	0.93	0.96	0.02	0.02	0.04	0.04
Ex 11	0.7	0.37	0.44	0.53	0.03	0.03	0.02
Ex 12	0.03	0.98	0.02	0.52	0.03	0.6	0.03
Ex 13	0.03	1	0.07	0.02	0.02	0.03	0.05
Ex 14	0.02	0.04	0.04	0.57	0.03	0.05	0.05
Ex 15	0.69	1	0.05	0.02	0.03	0.03	0.04
Ex 16	0.1	0.08	0.02	0.22	0.02	0.08	0.07
Ex 17	0.08	0.05	0.05	0.03	0.05	0.02	0.04
Ex 18	0.35	0.05	0.05	0.04	0.03	0.03	0.03

Table A1.3
Benski Results for 2^3 Factorial Designs

Benski Results																			
2^3 Designs																			
		Example																	
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
ITER 1	Effect	-132.5	-4.85	0.75	-575	2.675	24.5	5.063	3.25	23	11.25	11.3	20	-43.75	15.25	-29.25	5.45	11.5	-16.5
	W'	0.84	0.727	0.83	0.588	0.775	0.769	0.882	0.868	0.871	0.854	0.911	0.735	0.56	0.891	0.646	0.921	0.905	0.594
	Prob	0.102	0.013	0.086	<.005	0.031	0.028	0.224	0.173	0.184	0.135	0.384	0.015	<.005	0.266	<.005	0.457	0.34	<.005
	Cutoff		2.15		1.75	0.667	9						10.33	6.5		8.33			6
	Signif?	No	Yes	No	Yes	Yes	Yes	No	No	No	No	No	Yes	Yes	No	Yes	No	No	Yes
ITER 2	Effect		1.25		-90	-1.825	7.5						6	3.75		-7.25			7.5
	W'		0.958		0.831	0.692	0.915						0.886	0.942		0.86			0.943
	Prob		>.50		0.116	0.011	0.48						0.294	>.50		0.189			>.50
	Signif		No		No	Yes	No						No	No		No			No
ITER 3	Effect					-0.425													
	W'					0.944													
	Prob					>.50													
	Cutoff																		
	Signif?					No													

Table A1.4
Schneider, Kasperski, and Weissfeld t-statistics for 2^3 Factorial Designs

	Effects								
EXAMPLE	1	2	3	4	5	6	7	t-inner	t-outer
EX 1	0.7501	-6.412	0.6533	-3.557	0.0726	2.2988	0.121	2.6274	6.5253
EX 2	1.4947	-5.799	-0.717	0.7174	0.1794	0.5381	-0.598		
EX 3	7.2046	-5.668	-0.288	-3.362	-0.288	-0.384	-0.768		
EX 4	-27.47	-4.3	0.4778	-3.105	0.7166	0.4778	-0.478		
EX 5	8.4598	-0.712	0.2372	-5.772	-1.344	-0.395	-0.712		
EX 6	9.1234	-1.862	0.5586	0.3724	2.7929	0.7448	-0.186		
EX 7	3.6314	0.7621	0.4931	3.452	1.7484	0.3138	-0.134		
EX 8	-2.35	3.3939	-0.783	-1.827	0.2611	-0.261	-0.261		
EX 9	3.6583	-0.795	0.2386	0.2386	1.5906	0	0.8748		
EX 10	10.848	4.5801	8.9191	-0.241	0.2411	0.7232	-0.723		
EX 11	2.8378	-0.804	-1.168	1.7579	0.1883	0.113	-0.05		
EX 12	0.6812	13.624	0	3.406	-0.681	4.0872	0.6812		
EX 13	-0.755	-14.67	1.2576	0.0838	0.2515	-0.587	-1.09		
EX 14	-0.186	0.6331	0.7076	2.2717	0.5586	0.7821	-0.782		
EX 15	-3.042	-12.27	-0.944	-0.105	-0.524	-0.524	0.7342		
EX 16	-0.955	0.753	-0.027	1.4656	-0.094	-0.753	-0.645		
EX 17	0.9809	-0.81	0.725	0.5118	0.7677	0.0853	-0.597		
EX 18	-1.83	0.7763	0.8318	0.7209	0.4436	0.3882	0.5545		

Table A1.5
Summary of Classifications for 2^3 Factorial Designs

	Bayes	Benski	Lenth	Schneider, Kasperski and Weissfeld
0's (inactive)	99	117	103	99
1's (uncertain)	17	N/A	14	18
2's (active)	10	9	9	9

Table A1.6
Consistency Ratios for 2^3 Factorial Designs

	Compare				
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	——	98/117	97/103	96/99	O in Column and Row Agrees
Benski	98/99	——	102/103	98/99	
Lenth	97/99	102/117	——	99/99	
Schneider, Kasperski, and Weissfeld	96/99	98/117	99/103	——	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	——	N/A	9/14	13/18	1 in Column and Row Agrees
Benski	N/A	——	N/A	N/A	
Lenth	9/17	N/A	——	13/18	
Schneider, Kasperski, and Weissfeld	13/17	N/A	13/14	——	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	——	8/9	7/9	8/9	2 in Column and Row Agrees
Benski	8/10	——	5/9	6/9	
Lenth	7/10	5/9	——	8/9	
Schneider, Kasperski, and Weissfeld	8/10	6/9	8/9	——	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	——	19/117	6/103	3/99	Col=0 Row>0
Benski	1/99	——	1/103	1/99	
Lenth	2/99	15/117	——	0/99	
Schneider, Kasperski, and Weissfeld	3/99	19/117	4/103	——	

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes		17/117	6/103	3/99	Col=0 Row=1
Benski	N/A		N/A	N/A	
Lenth	2/99	11/117		0/99	
Schneider, Kasperski, and Weissfeld	3/99	16/117	4/103		
	Bayes	**Benski**	**Lenth**	**Schneider, Kasperski, and Weissfeld**	
Bayes		2/117	0/103	0/99	Col=0 Row=2
Benski	1/99		1/103	1/99	
Lenth	0/99	4/117		0/99	
Schneider, Kasperski, and Weissfeld	0/99	3/117	0/103		
	Bayes	**Benski**	**Lenth**	**Schneider, Kasperski, and Weissfeld**	
Bayes		N/A	3/14	2/18	Col=1 Row=2
Benski	0/17		3/14	2/18	
Lenth	2/17	N/A		1/18	
Schneider, Kasperski, and Weissfeld	1/17	N/A	1/14		

Table A1.7
Different Uncertain and Active Classifications for 2^3 Factorial Designs

Example	Effect Number and Estimate	Posterior Probability	ME	SME	t-value	t-inner	t-outer
1	4 = -73.5	.74	±76.14	±182.45	-3.557	±2.6274	±6.5253
1	6 = 47.5	.62	±76.14	±182.45	2.299	±2.6274	±6.5253
4	2 = -90	.25	±56.4	±135.15	-4.3	±2.6274	±6.5253
4	4 = -65	.11	±56.4	±135.15	-3.105	±2.6274	±6.5253
6	5 = 7.5	.49	±8.46	±20.27	2.793	±2.6274	±6.5253
11	1 = 11.3	.7	±18.05	±43.25	2.8383	±2.6274	±6.5253
11	4 = 7	.53	±18.05	±43.25	1.758	±2.6274	±6.5253
12	4 = 5	.52	±5.64	±13.52	3.406	±2.6274	±6.5253
14	4 = 15.25	.57	±26.79	±64.2	2.272	±2.6274	±6.5253

Table A2
Results for 2^4 Factorial Designs

		EFFECTS														
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
	BAYES	0	2	0	2	0	0	0	2	0	0	0	0	0	0	0
EX 1	BENSKI	0	2	0	2	0	0	0	2	0	0	0	0	0	0	0
	LENTH	0	2	0	2	0	0	0	1	0	0	0	0	0	0	0
	SKW	0	2	0	2	0	0	0	2	0	0	0	0	0	0	0
	BAYES	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
EX 2	BENSKI	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	SKW	0	0	0	0	0	0	0	1	0	1	0	0	0	0	0
	BAYES	0	2	0	2	0	2	0	2	0	0	0	0	0	0	0
EX 3	BENSKI	0	2	0	2	0	2	0	2	0	0	0	0	0	0	0
	LENTH	0	2	0	2	0	2	0	2	0	0	0	0	0	0	0
	SKW	0	2	0	2	0	2	0	2	0	0	0	0	0	0	0
	BAYES	2	2	0	0	0	0	0	2	0	1	0	0	0	0	0
EX 4	BENSKI	2	2	0	0	0	0	0	2	0	2	0	0	0	0	0
	LENTH	2	2	0	0	0	0	0	1	0	1	0	0	0	0	0
	SKW	2	2	0	0	0	0	0	2	0	1	0	0	0	0	0
	BAYES	0	2	0	2	0	0	0	2	0	0	0	2	0	0	0
EX 5	BENSKI	0	2	0	2	0	0	0	2	0	0	0	2	0	0	0
	LENTH	0	2	0	2	1	0	0	2	0	0	0	2	0	0	0
	SKW	0	1	0	2	0	0	0	2	0	0	0	2	0	0	0
	BAYES	2	1	0	2	0	0	0	0	0	0	0	0	0	0	0
EX 6	BENSKI	2	0	0	2	0	0	0	0	0	0	0	0	0	0	0
	LENTH	2	1	0	2	0	0	0	0	0	0	0	0	0	0	0
	SKW	2	1	0	2	0	0	0	0	0	0	0	0	0	0	0

Table A2.1
Effects, ME, SME, and Sigma for 2^4 Factorial Designs

Effect	Estimates	2^4 Designs				
	Examples					
Effects	1	2	3	4	5	6
1	0.06	-0.19	0.25	-8	-0.2375	68.375
2	0.25	-0.02	-17.25	24	2.2875	-24.875
3	-0.01	0	0.5	1	-0.2375	4.375
4	0.5	-0.08	9.75	-2.25	-8.2375	-62.375
5	0	0.03	-1	0.75	1.1875	-5.125
6	-0.02	-0.07	-7.5	-1.25	0.8125	-10.875
7	0	0.15	1.75	-0.75	-0.4625	-7.125
8	0.14	0.27	21.75	-5.5	5.1875	-13.875
9	0.03	-0.16	-1	0	0.1125	6.375
10	-0.01	-0.25	1	4.5	-0.2625	-2.875
11	0.02	-0.1	-0.75	0.5	0.2625	-0.125
12	0.04	-0.03	-0.5	-0.25	-3.0875	-0.875
13	0.02	-0.01	0.25	-0.25	-0.5125	0.875
14	-0.01	0.12	0.25	-0.75	-0.1875	-3.875
15	0.02	0.02	0	-.25	.1875	2.375
ME	0.0771	0.3084	1.9275	2.8913	1.0119	15.902
SME	0.1566	0.6264	3.915	5.8725	2.0554	32.299
SIGMA	0.0289	0.1085	1.3597	1.0865	.6375	7.1638

Table A2.2
Box and Meyer's Posterior Probabilities for 2^4 Factorial Designs

	Examples					
Effects	1	2	3	4	5	6
1	0.31	0.14	0.02	0.99	0.03	1
2	1	0.02	1	1	0.95	0.91
3	0.03	0.02	0.03	0.03	0.03	0.03
4	1	0.04	1	0.25	1	1
5	0.02	0.03	0.04	0.03	0.41	0.04
6	0.03	0.04	1	0.05	0.13	0.19
7	0.02	0.08	0.1	0.03	0.04	0.06
8	0.98	0.34	1	0.95	1	0.38
9	0.05	0.1	0.04	0.02	0.02	0.05
10	0.03	0.27	0.04	0.89	0.03	0.03
11	0.03	0.05	0.03	0.03	0.03	0.02
12	0.09	0.03	0.03	0.02	0.99	0.02
13	0.03	0.02	0.02	0.02	0.05	0.02
14	0.03	0.06	0.02	0.03	0.03	0.03
15	0.03	0.02	0.02	0.02	0.03	0.03

Table A2.3
Benski Results for 2^4 Factorial Designs

		EXAMPLE					
		1	2	3	4	5	6
ITER 1	EFFECT	0.5	0.27	21.75	24	-8.238	68.375
	W'	0.611	0.97	0.751	0.611	0.763	0.737
	PROB	<.005	>.50	<.005	<.005	<.005	<.005
	CUTOFF	0.108		3.167	3.417	1.8	21.25
	SIGNIF?	YES	NO	YES	YES	YES	YES
ITER 2	EFFECT	0.25		-17.25	-8	5.188	-62.375
	W'	0.692		0.706	0.84	0.797	0.682
	PROB	<.005		<.005	0.019	0.005	<.005
	CUTOFF	0.092		2.83	3.5	2.15	23.5
	SIGNIF?	YES		YES	YES	YES	YES
ITER 3	EFFECT	0.14		9.75	-5.5	-3.088	-24.875
	W'	0.78		0.707	0.848	0.828	0.91
	PROB	<.005		<.005	0.03	0.017	0.163
	CUTOFF	0.072		2.5	2.83	1.05	
	SIGNIF?	YES		YES	YES	YES	NO
ITER 4	EFFECT	0.06		-7.5	4.5	2.288	
	W'	0.936		0.632	0.81	0.812	
	PROB	0.362		<.005	0.014	0.014	
	CUTOFF			2.54	2.75	1.58	
	SIGNIF?	NO		YES	YES	YES	
ITER 5	EFFECT			1.75	-2.25	1.188	
	W'			0.944	0.95	0.879	
	PROB			0.483	>.50	0.098	
	CUTOFF						
	SIGNIF?			NO	NO	NO	

Table A2.4
Schneider, Kasperski, and Weissfeld t-statistics for 2^4 Factorial Designs

	EXAMPLES					
EFFECTS	1	2	3	4	5	6
1	2.07612	-1.7512	0.18386	-7.3631	-0.3725	9.54452
2	8.65052	-0.1843	-12.687	22.0893	3.58824	-3.4723
3	-0.346	0	0.36773	0.92039	-0.3725	0.61071
4	17.301	-0.7373	7.1707	-2.0709	-12.922	-8.707
5	0	0.2765	-0.7355	0.69029	1.86275	-0.7154
6	-0.692	-0.6452	-5.5159	-1.1505	1.27451	-1.518
7	0	1.38249	1.28705	-0.6903	-0.7255	-0.9946
8	4.84429	2.48848	15.9962	-5.0621	8.13725	-1.9368
9	1.03806	-1.4747	-0.7355	0	0.17647	0.88989
10	-0.346	-2.3041	0.73546	4.14174	-0.4118	-0.4013
11	0.69204	-0.9217	-0.5516	0.46019	0.41176	-0.0174
12	1.38408	-0.2765	-0.3677	-0.2301	-4.8431	-0.1221
13	0.69204	-0.0922	0.18386	-0.2301	-0.8039	0.12214
14	-0.346	1.10599	0.18386	-0.6903	-0.2941	-0.5409
15	0.69204	0.18433	-.18386	-0.2301	0.29412	0.33153
t-inner	2.2361					
t-outer	4.2129					

Table A2.5
Summary of Classifications for 2^4 Factorial Designs

	Bayes	Benski	Lenth	Schneider, Kasperski and Weissfeld
0's (inactive)	72	73	71	70
1's (uncertain)	2	N/A	5	5
2's (active)	16	17	14	15

Table A2.6
Consistency Ratios for 2^4 Factorial Designs

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	---	72/73	71/71	70/70	0 in Column and Row Agrees
Benski	72/72	---	71/71	70/70	
Lenth	71/72	71/73	---	69/70	
Schneider, Kasperski, and Weissfeld	70/72	70/73	69/71	---	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	---	N/A	2/5	2/5	1 in Column and Row Agrees
Benski	N/A	---	N/A	N/A	
Lenth	2/2	N/A	---	2/5	
Schneider, Kasperski, and Weissfeld	2/2	N/A	2/5	---	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	---	16/17	14/14	15/15	2 in Column and Row Agrees
Benski	16/16	---	14/14	15/15	
Lenth	14/16	14/17	---	13/15	
Schneider, Kasperski, and Weissfeld	15/16	15/17	13/14	---	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	---	1/73	0/71	0/70	Col=0 Row>0
Benski	0/72	---	0/71	0/70	
Lenth	1/72	2/73	---	1/70	
Schneider, Kasperski, and Weissfeld	2/72	3/73	2/71	---	

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	1/73	0/71	0/70	Col=0 Row=1
Benski	N/A	—	N/A	N/A	
Lenth	1/72	2/73	—	1/70	
Schneider, Kasperski, and Weissfeld	2/72	3/73	2/71	—	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	0/73	0/71	0/70	Col=0 Row=2
Benski	0/72	—	0/71	0/70	
Lenth	0/72	0/73	—	0/70	
Schneider, Kasperski, and Weissfeld	0/72	0/73	0/71	—	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	N/A	2/5	1/5	Col=1 Row=2
Benski	1/2	—	2/5	2/5	
Lenth	0/2	N/A	—	1/5	
Schneider, Kasperski, and Weissfeld	2/2	N/A	2/5	—	

Table A2.7
Different Uncertain and Active Classifications for 2^4 Factorial Designs

Example	Effect Number and Estimate	Posterior Probability	ME	SME	t-value	t-inner	t-outer
2	8 (.27)	.34	.3084	.6264	**2.4885**	2.2361	4.2129
2	10 (-.25)	.27	.3084	.6264	**-2.3041**	2.2361	4.2129
5	5 (1.1875)	.41	**1.0119**	2.0554	1.8628	2.2361	4.2129

Table A3
Results for 2^5 Factorial Designs

	EX 1				EX 2			
EFFECTS	BAYES	BENSKI	LENTH	SKW	BAYES	BENSKI	LENTH	SKW
1	0	0	0	0	2	2	2	2
2	1	0	1	1	0	0	0	0
3	0	0	0	0	0	0	0	0
4	0	0	0	0	2	0	1	1
5	0	0	0	0	0	0	0	0
6	0	0	0	0	0	0	0	0
7	0	0	0	0	0	0	0	0
8	0	0	0	0	0	0	0	0
9	0	0	0	0	0	0	0	0
10	0	0	0	0	0	0	0	0
11	0	0	0	1	0	0	0	0
12	0	0	0	0	0	0	0	0
13	0	0	0	0	0	0	0	0
14	0	0	0	0	0	0	0	0
15	0	0	0	0	0	0	0	0
16	0	0	0	0	2	2	2	2
17	0	0	0	0	0	0	0	0
18	0	0	0	0	0	0	0	0
19	0	0	0	0	0	0	0	0
20	0	0	0	0	0	0	0	0
21	0	0	0	0	0	0	0	0
22	0	0	0	0	0	0	0	0
23	0	0	0	0	0	0	0	0
24	0	0	0	0	0	0	0	0
25	0	0	0	0	0	0	0	0
26	0	0	0	0	0	0	0	0
27	0	0	0	0	0	0	0	0
28	0	0	0	0	0	0	0	0
29	0	0	0	0	0	0	0	0
30	0	0	0	0	0	0	0	0
31	0	0	0	0	0	0	0	0

	EX 3				EX 4			
EFFECTS	BAYES	BENSKI	LENTH	SKW	BAYES	BENSKI	LENTH	SKW
1	2	2	2	2	0	0	0	0
2	0	0	0	0	2	2	2	2
3	0	0	0	0	0	0	0	0
4	0	0	0	0	0	0	0	0
5	0	0	1	1	0	0	0	0
6	0	0	0	0	0	0	0	0
7	0	0	0	0	0	0	0	0
8	2	0	2	2	2	2	2	2
9	0	0	0	0	0	0	0	0
10	0	0	0	0	2	2	2	2
11	0	0	0	0	0	0	0	0
12	0	0	1	1	0	0	0	0
13	0	0	0	0	0	0	0	0
14	0	0	0	0	0	0	0	0
15	0	0	0	0	0	0	0	0
16	2	2	2	2	2	2	2	2
17	0	0	0	0	0	0	0	0
18	0	0	0	0	0	0	0	0
19	0	0	0	0	0	0	0	0
20	0	0	0	0	0	0	0	0
21	0	0	0	0	0	0	0	0
22	0	0	1	1	0	0	0	0
23	0	0	0	0	0	0	0	0
24	0	0	0	0	2	2	2	2
25	0	0	0	0	0	0	0	0
26	0	0	0	0	0	0	0	0
27	0	0	0	0	0	0	0	0
28	0	0	0	0	0	0	0	0
29	0	0	0	0	0	0	0	0
30	0	0	0	0	0	0	0	0
31	0	0	0	0	0	0	0	0

	EX 5				EX 6			
EFFECTS	BAYES	BENSKI	LENTH	SKW	BAYES	BENSKI	LENTH	SKW
1	0	0	1	1	0	0	0	0
2	0	0	0	0	1	0	1	1
3	0	0	0	0	0	0	0	0
4	0	0	0	0	0	0	0	0
5	0	0	0	0	0	0	1	1
6	0	0	0	0	0	0	0	0
7	0	0	0	0	0	0	0	0
8	0	0	1	1	2	2	2	2
9	0	0	0	0	0	0	0	0
10	0	0	0	0	0	0	0	0
11	0	0	0	0	0	0	0	0
12	0	0	0	0	0	0	0	0
13	0	0	0	0	0	0	0	0
14	0	0	0	0	0	0	0	0
15	0	0	0	0	0	0	0	0
16	1	0	1	1	1	0	1	1
17	0	0	0	0	0	0	0	0
18	0	0	0	0	0	0	0	0
19	0	0	0	0	0	0	0	0
20	0	0	0	0	0	0	0	0
21	0	0	0	0	0	0	0	0
22	0	0	0	0	0	0	0	0
23	0	0	0	0	0	0	0	0
24	0	0	0	0	0	0	0	0
25	0	0	0	0	0	0	0	0
26	0	0	0	0	0	0	0	0
27	0	0	0	0	0	0	0	0
28	0	0	0	0	0	0	0	0
29	0	0	0	0	0	0	0	0
30	0	0	0	0	0	0	0	0
31	0	0	0	0	0	0	0	0

Table A3.1
Effects, ME, SME, and Sigma for 2^5 Factorial Designs

Effect	Estimates	2^5 Designs				
	Examples					
Effects	1	2	3	4	5	6
1	-7.8124	-0.11844	339.25	-1.375	2.25	-0.65625
2	15.7	-0.00356	-25.75	19.5	0.25	1.59375
3	5.0375	-0.04019	12.75	1.375	0.5	1.03125
4	3.25	0.09581	15.75	-0.625	-1.375	0.21875
5	3.3125	-0.03331	71.25	0.75	0.625	1.28125
6	5.15	0.03331	-21.75	0.875	1.125	0.03125
7	1.9875	0.00019	-12.25	1.5	0.875	0.21875
8	-5.5125	0.00581	128.25	10.75	2.25	-3.46875
9	2.95	-0.03406	51.75	0.875	-0.25	0.34375
10	-0.4875	-0.00419	20.75	13.25	-0.25	-0.15625
11	-11.7	-0.02106	0.25	1.375	0.5	0.53125
12	-5.6125	-0.00256	68.25	2.125	-0.625	-0.65625
13	0.9	0.02056	-43.25	-0.75	-0.125	0.90625
14	1.0875	-0.01131	3.75	1.125	-1.125	-0.59375
15	-0.625	0.03631	0.25	0	-0.875	-0.40625
16	7.6	-0.13981	187.25	-6.25	-2.625	-1.71875
17	8.7375	0.00156	-12.25	0.125	-0.625	-0.40625
18	-3.875	0.01819	2.75	2	-0.625	0.09375
19	-1.5125	-0.01394	41.25	-1.875	-0.375	0.28125
20	4.35	-0.05819	-16.75	0.875	1.25	-0.03125
21	-6.6125	-0.03631	54.75	-2.5	-0.25	-0.21875
22	2.25	0.02456	71.75	0.125	-0.75	-0.21875
23	-2.0375	0.01944	-34.75	1.5	0.5	-0.28125
24	-0.4125	0.01906	-35.25	-11	-1.375	0.90625
25	-3.475	0.01319	-11.75	0.625	-1.875	0.46875
26	-1.7375	0.01731	-4.75	-0.25	-0.375	-0.28125
27	-3.725	0.00844	-53.25	0.625	-0.625	0.65625
28	-4.8375	0.00769	3.75	0.125	-0.25	-0.53125
29	-6.35	0.02931	-15.75	1	0.75	0.28125
30	3.1125	0.01019	19.25	-0.625	0.25	0.03125
31	4.775	0.04831	19.75	-0.25	0	-0.03125
ME	11.988	0.0621	64.935	2.9138	2.0812	1.0408
SME	22.788	0.1181	123.435	5.5388	3.9562	1.9784
SIGMA	5.458	0.0281	30.299	1.4618	0.9221	0.5807

Table A3.2
Box and Meyer's Posterior Probabilities for 2^5 Factorial Designs

	Examples					
Effects	1	2	3	4	5	6
1	0.12	1	1	0.05	0.47	0.07
2	0.84	0.02	0.03	1	0.02	0.73
3	0.05	0.12	0.03	0.05	0.03	0.24
4	0.03	0.96	0.03	0.03	0.11	0.03
5	0.03	0.08	0.3	0.03	0.03	0.47
6	0.05	0.08	0.03	0.03	0.06	0.02
7	0.03	0.02	0.03	0.06	0.04	0.03
8	0.05	0.02	0.95	1	0.47	1
9	0.03	0.08	0.11	0.03	0.02	0.03
10	0.02	0.02	0.03	1	0.02	0.02
11	0.46	0.04	0.02	0.05	0.03	0.05
12	0.05	0.02	0.26	0.13	0.03	0.07
13	0.02	0.04	0.07	0.03	0.02	0.16
14	0.02	0.03	0.02	0.04	0.06	0.05
15	0.02	0.09	0.02	0.02	0.04	0.04
16	0.11	1	1	1	0.66	0.81
17	0.17	0.02	0.03	0.02	0.03	0.04
18	0.04	0.03	0.02	0.11	0.03	0.02
19	0.02	0.03	0.06	0.09	0.03	0.03
20	0.04	0.44	0.03	0.03	0.08	0.02
21	0.06	0.09	0.13	0.23	0.02	0.03
22	0.03	0.04	0.31	0.02	0.04	0.03
23	0.03	0.04	0.05	0.06	0.03	0.03
24	0.02	0.04	0.05	1	0.11	0.16
25	0.03	0.03	0.03	0.03	0.28	0.04
26	0.03	0.03	0.02	0.02	0.03	0.03
27	0.03	0.03	0.12	0.03	0.03	0.07
28	0.04	0.02	0.02	0.02	0.02	0.05
29	0.07	0.06	0.03	0.04	0.04	0.03
30	0.03	0.03	0.03	0.03	0.02	0.02
31	0.04	0.23	0.03	0.02	0.02	0.02

Table A3.3
Benski Results for 2^5 Factorial Designs

		EXAMPLE					
		1	2	3	4	5	6
ITER 1	EFFECT	15.7	-0.1398	339.25	19.5	-2.625	-3.4688
	W'	0.971	0.87	0.711	0.63	0.972	0.842
	PROB	0.44	<.005	<.005	<.005	0.462	<.005
	CUTOFF		0.0735	122.17	3.25		1.5
	SIGNIF?	NO	YES	YES	YES	NO	YES
ITER 2	EFFECT		-0.1184	187.25	13.25		-1.7188
	W'		0.904	0.861	0.67		0.962
	PROB		0.015	<.005	<.005		0.292
	CUTOFF		0.067	114	3.27		
	SIGNIF?		YES	YES	YES		NO
ITER 3	EFFECT		0.0958	128.25	-11		
	W'		0.946	0.931	0.711		
	PROB		0.134	0.063	<.005		
	CUTOFF				3.313		
	SIGNIF?		NO	NO	YES		
ITER 4	EFFECT				10.75		
	W'				0.705		
	PROB				<.005		
	CUTOFF				3.292		
	SIGNIF?				YES		
ITER 5	EFFECT				-6.25		
	W'				0.796		
	PROB				<.005		
	CUTOFF				3		
	SIGNIF?				YES		
ITER 6	EFFECT				-2.5		
	W'				0.957		
	PROB				0.282		
	CUTOFF						
	SIGNIF?				NO		

Table 3.4
Schneider, Kasperski, and Weissfeld t-statistics for 2^5 Factorial Designs

	EXAMPLES					
EFFECTS	1	2	3	4	5	6
1	-1.43139	-4.21495	11.19674	-0.94062	2.440082	-1.1301
2	2.876512	-0.12669	-0.84986	13.33972	0.27112	2.744532
3	0.922957	-1.43025	0.420806	0.940621	0.542241	1.775874
4	0.595456	3.409715	0.519819	-0.42756	-1.49116	0.376701
5	0.606907	-1.18541	2.351563	0.513066	0.677801	2.206389
6	0.943569	1.185516	-0.71785	0.598577	1.220041	0.053814
7	0.364144	0.006655	-0.4043	1.026132	0.948921	0.376701
8	-1.00999	0.206833	4.232813	7.353947	2.440082	-5.97339
9	0.540491	-1.2121	1.707977	0.598577	-0.27112	0.591958
10	-0.08932	-0.14911	0.684841	9.064167	-0.27112	-0.26907
11	-2.14364	-0.74947	0.008251	0.940621	0.542241	0.914844
12	-0.94586	-0.0911	2.25255	1.453687	-0.6778	-1.1301
13	0.164896	0.731779	-1.42744	-0.51307	-0.13556	1.560616
14	0.199249	-0.40249	0.123766	0.769599	-1.22004	-1.02247
15	-0.11451	1.292278	0.008251	0	-0.94892	-0.69959
16	1.392451	-4.97544	6.180072	-4.27555	-2.84676	-2.95979
17	1.600861	0.055623	-0.4043	0.085511	-0.6778	-0.69959
18	-0.70997	0.64726	0.090762	1.368176	-0.6778	0.161443
19	-0.27712	-0.49609	1.361431	-1.28267	-0.40668	0.484329
20	0.796995	-2.07082	-0.55282	0.598577	1.355601	-0.05381
21	-1.12908	-1.29217	1.80699	-1.71022	-0.27112	-0.3767
22	0.412239	0.874128	2.368065	0.085511	-0.81336	-0.3767
23	-0.37331	0.691708	-1.1469	1.026132	0.542241	-0.48433
24	-0.07558	0.678399	-1.1634	-7.52497	-1.49116	1.560616
25	-0.63668	0.469324	-0.3878	0.427555	-2.0334	0.807215
26	-0.31834	0.616121	-0.15677	-0.17102	-0.40668	-0.48433
27	-0.68248	0.300249	-1.75748	0.427555	-0.6778	1.130102
28	-0.88631	0.273559	0.123766	0.085511	-0.27112	-0.91484
29	-1.16343	1.043167	-0.51982	0.684088	0.813361	0.484329
30	0.570264	0.362562	0.635334	-0.42756	0.27112	0.053814
31	0.874863	1.719324	0.651837	-0.17102	0	-0.05381
t-inner	2.087					
t-outer	3.772					

Table A3.5
Summary of Classifications for 2^5 Factorial Designs

	Bayes	Benski	Lenth	Schneider, Kasperski and Weissfeld
0's (inactive)	170	176	164	163
1's (uncertain)	4	N/A	11	12
2's (active)	12	10	11	11

Table A3.6
Consistency Ratios for 2^5 Factorial Designs

	Compare				
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	170/176	164/164	163/163	0 in Column and Row Agrees
Benski	170/170	—	164/164	163/163	
Lenth	164/170	164/176	—	163/163	
Schneider, Kasperski, and Weissfeld	163/170	163/176	163/164	—	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	N/A	4/11	4/12	1 in Column and Row Agrees
Benski	N/A	—	N/A	N/A	
Lenth	4/4	N/A	—	11/12	
Schneider, Kasperski, and Weissfeld	4/4	N/A	11/11	—	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	10/10	11/11	11/11	2 in Column and Row Column Agrees
Benski	10/12	—	10/11	10/11	
Lenth	11/12	10/10	—	11/11	
Schneider, Kasperski, and Weissfeld	11/12	10/10	11/11	—	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	6/176	0/164	0/163	Col=0 Row>0
Benski	0/170	—	0/164	0/163	
Lenth	6/170	12/176	—	0/163	
Schneider, Kasperski, and Weissfeld	7/170	13/176	1/164	—	

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	4/176	0/164	0/163	Col=0 Row=1
Benski	N/A	—	N/A	N/A	
Lenth	6/170	11/176	—	0/163	
Schneider, Kasperski, and Weissfeld	7/170	12/176	1/164	—	

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	2/176	0/164	0/163	Col=0 Row=2
Benski	0/170	—	0/164	0/163	
Lenth	0/170	1/176	—	0/163	
Schneider, Kasperski, and Weissfeld	0/170	1/176	0/164	—	

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	N/A	1/11	1/12	Col=1 Row=2
Benski	0/4	—	0/11	0/12	
Lenth	0/4	N/A	—	0/12	
Schneider, Kasperski, and Weissfeld	0/4	N/A	0/11	—	

Table A3.7
Different Uncertain and Active Classifications for 2^5 Factorial Designs

Example	Effect Number and Estimate	Posterior Probability	ME	SME	t-value	t-inner	t-outer
1	11 (-11.7)	.46	11.988	22.788	**-2.1436**	2.087	3.772
3	5 (71.25)	.3	**64.935**	123.435	**2.3516**	2.087	3.772
3	12 (68.25)	.26	**64.935**	123.435	**2.2526**	2.087	3.772
3	22 (71.75)	.31	**64.935**	123.435	**2.3681**	2.087	3.772
5	1 (2.25)	.47	**2.0812**	3.9562	**2.4401**	2.087	3.772
5	8 (2.25)	.47	**2.0812**	3.9562	**2.4401**	2.087	3.772
6	5 (1.28)	.47	**1.0408**	1.9784	**2.206**	2.087	3.772

Table A4
Results for 2^3 Fractional Factorial Designs

		EFFECTS						
		1	2	3	4	5	6	7
	BAYES	1	1	0	0	0	0	0
EX 1	BENSKI	0	0	0	0	0	0	0
	LENTH	1	1	0	0	0	0	0
	SKW	1	2	0	0	0	0	0
	BAYES	0	0	0	1	0	0	0
EX 2	BENSKI	0	0	0	0	0	0	0
	LENTH	0	0	0	1	0	0	0
	SKW	0	0	0	1	0	0	0
	BAYES	0	0	0	1	1	0	0
EX 3	BENSKI	0	0	0	0	0	0	0
	LENTH	0	0	0	0	1	0	0
	SKW	0	0	0	1	1	0	0
	BAYES	0	1	2	0	0	0	0
EX 4	BENSKI	0	2	2	0	0	0	0
	LENTH	0	1	2	0	0	0	0
	SKW	0	1	2	0	0	0	0
	BAYES	1	1	0	0	0	0	0
EX 5	BENSKI	0	0	0	0	0	0	0
	LENTH	1	0	0	0	0	0	0
	SKW	1	1	0	0	0	0	0
	BAYES	0	0	0	1	1	0	0
EX 6	BENSKI	0	0	0	0	0	0	0
	LENTH	0	0	0	0	1	0	0
	SKW	0	0	0	1	1	0	0
	BAYES	0	0	0	0	0	0	0
EX 7	BENSKI	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0
	SKW	0	0	0	0	0	0	0

Table A4.1
Effects, ME, SME, and Sigma for 2^3 Fractional Factorial Designs

	Example						
Effects	1	2	3	4	5	6	7
1	10.5	1.325	-10.875	3.5	-5.8	-10.9	-4.25
2	14	5.925	-2.775	12	-3.8	-2.8	-1.75
3	-0.5	0.175	3.175	22.5	0.2	3.2	-0.25
4	1.5	14.075	-16.575	1	-1.2	-16.6	-0.75
5	3	-0.275	-22.825	0.5	0.8	-22.8	2.75
6	-1.5	2.725	-3.425	1	-0.2	-3.4	2.25
7	-1	-3.525	0.525	2.5	0.8	0.5	2.75
ME	8.46	11.421	17.907	5.64	4.512	18.048	12.69
SME	20.272	27.368	42.91	13.515	10.812	43.248	30.409
SIGMA	2.1302	3.451	4.8578	3.2004	1.0974	4.8397	2.9819

Table A4.2
Box and Meyer's Posterior Probabilities for 2^3 Fractional Factorial Designs

	Example						
Effects	1	2	3	4	5	6	7
1	0.92	0.04	0.42	0.17	0.92	0.42	0.17
2	0.95	0.42	0.04	0.93	0.85	0.04	0.04
3	0.02	0.02	0.04	0.98	0.02	0.04	0.02
4	0.04	0.89	0.53	0.03	0.15	0.53	0.03
5	0.22	0.02	0.64	0.02	0.06	0.64	0.06
6	0.04	0.11	0.05	0.03	0.02	0.05	0.04
7	0.03	0.17	0.02	0.07	0.06	0.02	0.06

Table A4.3
Benski Results for 2^3 Fractional Factorial Designs

		EXAMPLE						
		1	2	3	4	5	6	7
ITER 1	EFFECT	14	14.075	-22.825	22.5	-5.8	-22.8	-4.25
	W'	0.842	0.875	0.942	0.738	0.849	0.942	0.918
	PROB	0.107	0.198	>.50	0.016	0.122	>.50	0.431
	CUTOFF				13.5			
	SIGNIF?	NO	NO	NO	YES	NO	NO	NO
ITER 2	EFFECT				12			
	W'				0.697			
	PROB				0.012			
	CUTOFF				5.33			
	SIGNIF?				YES			
ITER 3	EFFECT				3.5			
	W'				0.89			
	PROB				0.383			
	CUTOFF							
	SIGNIF?				NO			

Table A4.4
Schneider, Kasperski, and Weissfeld t-statistics for 2^3 Fractional Factorial Designs

	Example						
EFFECTS	1	2	3	4	5	6	7
1	4.92911	0.38395	-2.2387	1.09361	-5.2852	-2.2522	-1.4253
2	6.57215	1.71689	-0.5712	3.74953	-3.4627	-0.5785	-0.5869
3	-0.2347	0.05071	0.65359	7.03037	0.18225	0.6612	-0.0838
4	0.70416	4.07853	-3.412	0.31246	-1.0935	-3.43	-0.2515
5	1.40832	-0.0797	-4.6986	0.15623	0.729	-4.711	0.92223
6	-0.7042	0.78963	-0.7051	0.31246	-0.1822	-0.7025	0.75455
7	-0.4694	-1.0214	0.10807	0.78115	0.729	0.10331	0.92223
t-inner	2.6274						
t-outer	6.5253						

Table 4.5
Summary of Classifications 2^3 Fractional Factorial Designs

	Bayes	Benski	Lenth	Schneider, Kasperski and Weissfeld
0's (inactive)	38	47	41	38
1's (uncertain)	10	N/A	7	9
2's (active)	1	2	1	2

Table A4.6
Consistency Ratios for 2^3 Fractional Factorial Designs

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	38/47	38/41	38/38	0 in Column and Row Agrees
Benski	38/38	—	41/41	38/38	
Lenth	38/38	41/47	—	38/38	
Schneider, Kasperski, and Weissfeld	38/38	38/47	38/41	—	
	Bayes	**Benski**	**Lenth**	**Schneider, Kasperski, and Weissfeld**	
Bayes	—	N/A	7/7	9/9	1 in Column and Row Agrees
Benski	N/A	—	N/A	N/A	
Lenth	7/10	N/A	—	6/9	
Schneider, Kasperski, and Weissfeld	9/10	N/A	6/7	—	
	Bayes	**Benski**	**Lenth**	**Schneider, Kasperski, and Weissfeld**	
Bayes	—	1/2	1/1	1/2	2 in Column and Row Agrees
Benski	1/1	—	1/1	1/2	
Lenth	1/1	1/2	—	1/2	
Schneider, Kasperski, and Weissfeld	1/1	1/2	1/1	—	
	Bayes	**Benski**	**Lenth**	**Schneider, Kasperski, and Weissfeld**	
Bayes	—	9/47	3/41	0/38	Col=0 Row>0
Benski	0/38	—	0/41	0/38	
Lenth	0/38	6/47	—	0/38	
Schneider, Kasperski, and Weissfeld	0/38	9/47	3/41	—	

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	9/47	3/41	0/38	Col=0 Row=1
Benski	N/A	—	N/A	N/A	
Lenth	0/38	6/47	—	0/38	
Schneider, Kasperski, and Weissfeld	0/38	8/47	3/41	—	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	0/47	0/41	0/38	Col=0 Row=2
Benski	0/38	—	0/41	0/38	
Lenth	0/38	0/47	—	0/38	
Schneider, Kasperski, and Weissfeld	0/38	1/47	0/41	—	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	N/A	0/7	0/9	Col=1 Row=2
Benski	1/10	—	1/7	1/9	
Lenth	0/10	N/A	—	0/9	
Schneider, Kasperski, and Weissfeld	1/10	N/A	1/7	—	

Table A4.7
Different Uncertain and Active Classifications for 2^3 Fractional Factorial Designs

Example	Effect Number and Estimate	Posterior Probability	ME	SME	t-value	t-inner	t-outer
3	4 (-16.575)	.53	17.907	42.91	-3.412	2.6274	6.5253
5	2 (-3.8)	.85	4.512	10.812	-3.463	2.6274	6.5253
6	4 (-16.6)	.53	18.048	43.248	-3.43	2.6274	6.5253

Table A5
Results for 2^4 Fractional Factorial Designs

		EFFECTS														
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
	BAYES	0	0	0	0	0	0	0	2	0	0	0	0	0	0	0
EX 1	BENSKI	0	0	0	0	0	0	0	2	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0	2	0	0	0	0	0	0	0
	SKW	0	0	1	0	0	0	0	2	0	0	0	0	0	0	0
	BAYES	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
EX 2	BENSKI	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0
	SKW	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0
	BAYES	0	0	0	0	0	0	0	0	0	0	0	0	0	2	2
EX 3	BENSKI	0	0	0	0	0	0	0	0	0	0	0	0	0	2	2
	LENTH	0	0	0	0	0	0	0	0	0	0	0	0	0	2	2
	SKW	0	0	0	0	0	0	0	0	0	0	0	0	0	2	2
	BAYES	1	0	0	1	0	0	0	0	0	0	0	0	0	0	0
EX 4	BENSKI	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	LENTH	1	0	0	1	0	0	0	0	0	0	0	0	0	0	0
	SKW	1	0	0	1	0	0	0	0	0	0	0	0	0	0	0
	BAYES	0	1	0	0	0	0	0	0	0	0	1	2	0	2	0
EX 5	BENSKI	0	0	0	0	0	0	0	0	0	0	0	0	0	2	0
	LENTH	0	1	0	0	0	0	0	0	0	0	1	2	0	2	0
	SKW	0	1	0	0	0	0	0	0	0	0	1	2	0	2	0

	BAYES	0	0	0	0	0	0	2	0	0	0	0	0	0	0	0
EX 6	BENSKI	0	0	0	0	0	0	2	0	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0
	SKW	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0
	BAYES	0	2	0	0	0	0	2	2	0	2	0	0	0	0	2
EX 7	BENSKI	0	2	0	0	0	0	2	2	0	2	0	0	0	0	2
	LENTH	0	2	0	0	0	0	1	2	0	2	0	0	0	0	1
	SKW	0	2	0	0	0	0	2	2	0	2	0	0	0	0	1
	BAYES	0	0	0	2	0	0	0	0	0	0	0	2	2	0	0
EX 8	BENSKI	0	0	0	2	0	0	0	0	0	0	0	2	2	0	0
	LENTH	0	0	0	2	0	0	0	0	0	0	0	2	1	0	0
	SKW	0	0	0	2	0	0	0	0	0	0	0	2	2	0	0
	BAYES	1	1	0	0	0	0	1	1	0	0	0	0	0	1	0
EX 9	BENSKI	2	2	0	0	0	0	2	2	0	2	0	0	0	2	0
	LENTH	2	1	0	0	0	0	1	1	0	0	0	0	0	1	0
	SKW	2	1	0	0	0	0	1	1	0	0	0	0	0	1	0
	BAYES	2	2	0	0	0	0	0	2	0	1	0	0	0	0	0
EX 10	BENSKI	2	2	0	0	0	0	0	2	0	0	0	0	0	0	0
	LENTH	2	2	0	0	0	0	0	2	0	1	0	0	0	0	0
	SKW	2	2	0	0	0	0	0	2	0	0	0	0	0	0	0

	BAYES	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0
EX 11	BENSKI	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0	1	0	1	0	0	0	0	0
	SKW	0	0	0	0	0	0	0	1	0	1	0	0	0	0	0
	BAYES	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0
EX 12	BENSKI	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	SKW	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	BAYES	0	0	0	0	0	0	0	0	0	2	0	0	0	1	0
EX 13	BENSKI	0	0	0	0	0	0	0	0	0	2	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0	0	0	1	0	0	0	1	0
	SKW	0	0	0	0	0	0	0	0	0	1	0	0	0	1	0
	BAYES	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1
EX 14	BENSKI	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	LENTH	0	0	1	0	0	0	0	0	0	0	0	0	0	0	1
	SKW	0	0	1	0	0	0	0	0	0	0	0	0	0	0	1
	BAYES	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0
EX 15	BENSKI	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
	SKW	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0

Table A5.1
Effects, ME, SME, and Sigma for 2^4 Fractional Factorial Designs

Effect	Estimates	2^4 Example						Fractional				Factorial			
	Example														
Effects	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	0.4375	-86.75	0.13	-34.375	65.5	-1.875	-2	-0.6	-3.78	3.004	-16.625	-1.8625	0.168	0.16	0.01
2	-7.5625	37.25	-0.15	11.625	-248.5	-0.875	20.5	-0.4	2.65	3.719	0.375	-14.0125	0.239	0.04	0.05
3	16.6875	19.25	0.3	6.375	-28.5	2.375	1.5	-0.6	0.37	-0.099	-2.625	32.3125	-0.119	-0.21	0.01
4	14.0625	-26.75	0.15	28.375	-38.5	0.875	0	4.6	0.51	0.754	-10.125	37.5875	-0.028	-0.04	-0.02
5	3.0625	-29.75	0.4	-10.875	55.5	-2.375	0.5	0.9	0.37	-0.129	5.875	-12.2375	0.046	0.11	0.02
6	8.3125	23.75	-0.02	-6.375	-72.5	-1.375	1.5	-0.2	0.43	-0.399	-4.125	34.3625	0.212	0.09	0.01
7	0.0625	-40.75	0.38	1.875	-8.5	-7.625	-9.5	-0.3	-1.79	-0.001	12.375	38.2875	-0.102	0.01	-0.02
8	66.6875	100.5	0.4	1.125	-37.5	-2.125	12.25	-1.2	-2.43	3.544	29.875	-81.3125	0.222	0.14	-0.01
9	5.1875	4.5	-0.05	15.875	-3.5	0.625	-0.75	0.7	0.11	-0.319	-18.625	-26.4875	-0.084	-0.06	0.01
10	-3.5625	-17	0.43	3.875	136.5	1.125	10.75	0.1	0.84	-1.169	-27.625	-14.1375	-0.882	0.01	0.02
11	4.6875	51.5	0.13	6.125	300.5	-2.125	2.25	0.3	0.09	-0.231	-7.625	6.9375	-0.02	-0.04	-0.01
12	14.3125	21	0.13	-7.375	-463.5	-2.125	0.25	-5.5	0.34	-0.084	-0.125	-26.2875	-0.317	0.04	0
13	-7.6875	-58.5	-0.38	6.875	-113.5	1.125	1.25	3.8	-0.56	-0.391	-2.125	2.1375	0.309	-0.01	-0.01
14	2.3125	-19.5	2.15	-10.625	656.5	0.625	1.25	0.1	-2.18	-0.176	9.875	-42.7625	-0.604	0.06	0.01
15	-3.9375	21.5	3.1	-14.875	-19.5	-1.125	-6.25	-0.6	0.43	0.656	4.375	-28.0875	-0.025	0.26	-0.02
ME	19.034	97.339	0.578	26.503	148.418	4.819	4.819	1.928	1.658	1.06	26.021	102.109	0.553	0.193	0.038
SME	38.66	197.71	1.174	53.831	301.455	9.788	9.788	3.915	3.367	2.153	52.852	207.397	1.124	0.392	0.078
SIGMA	7.4099	39.861	0.4051	10.9438	90.7854	2.0265	2.173	0.8465	0.745	0.5361	10.4437	36.9536	0.2285	0.0838	0.0162

Table A5.2

Box and Meyer's Posterior Probabilities for 2^4 Fractional Factorial Designs

	Example														
Effects	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	0.02	0.31	0.03	0.7	0.05	0.05	0.08	0.05	0.92	1	0.15	0.02	0.04	0.21	0.03
2	0.04	0.04	0.03	0.05	0.87	0.03	1	0.03	0.84	1	0.02	0.03	0.08	0.03	0.77
3	0.3	0.03	0.05	0.03	0.03	0.08	0.05	0.05	0.04	0.02	0.02	0.05	0.03	0.38	0.03
4	0.18	0.03	0.03	0.52	0.03	0.03	0.02	1	0.05	0.25	0.05	0.07	0.02	0.03	0.07
5	0.03	0.03	0.08	0.05	0.04	0.08	0.03	0.11	0.04	0.03	0.03	0.03	0.02	0.09	0.07
6	0.05	0.03	0.02	0.03	0.06	0.04	0.05	0.03	0.04	0.05	0.03	0.06	0.06	0.06	0.03
7	0.02	0.05	0.07	0.02	0.02	0.97	0.99	0.03	0.76	0.02	0.08	0.07	0.03	0.02	0.07
8	1	0.44	0.08	0.02	0.03	0.06	1	0.28	0.83	1	0.54	0.59	0.06	0.16	0.03
9	0.03	0.02	0.02	0.11	0.02	0.03	0.03	0.06	0.02	0.04	0.2	0.04	0.03	0.04	0.03
10	0.03	0.03	0.09	0.03	0.37	0.03	1	0.02	0.21	0.68	0.46	0.03	0.95	0.02	0.07
11	0.03	0.07	0.03	0.03	0.93	0.06	0.12	0.03	0.02	0.03	0.04	0.02	0.02	0.03	0.03
12	0.19	0.03	0.03	0.03	0.99	0.06	0.02	1	0.03	0.02	0.02	0.04	0.17	0.03	0.02
13	0.04	0.1	0.07	0.03	0.22	0.03	0.04	1	0.06	0.05	0.02	0.02	0.15	0.02	0.03
14	0.02	0.03	1	0.05	1	0.03	0.04	0.02	0.81	0.03	0.05	0.09	0.75	0.04	0.03
15	0.03	0.03	1	0.09	0.02	0.03	0.96	0.05	0.04	0.16	0.03	0.04	0.02	0.58	0.07

Table A5.3
Benski Results for 2^k Fractional Factorial Designs

		EXAMPLE														
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
ITER 1	EFFECT	66.6875	100.5	3.1	-34.4	656.5	-7.63	20.5	-5.5	-3.78	3.719	29.875	-81.31	-0.882	0.26	0.05
	W'	0.657	0.972	0.65	0.95	0.851	0.843	0.85	0.805	0.877	0.755	0.964	0.937	0.863	0.957	0.896
	PROB	<.005	>.50	<.005	0.422	0.021	0.016	0.02	0.005	0.046	<.005	>.5	0.284	0.03	>.50	0.081
	CUTOFF	25.875		0.765		232	5.708	4.75	2.1	3.21	1.96			0.601		
	SIGNIF?	YES	NO	YES	NO	YES	YES	YES	YES	YES	YES	NO	NO	YES	NO	NO
ITER 2	EFFECT	16.6875		2.15		-463.5	2.375	12.25	4.6	2.65	3.544			-0.604		
	W'	0.962		0.651		0.882	0.903	0.865	0.737	0.862	0.726			0.905		
	PROB	>.50		<.005		0.063	0.115	0.039	<.005	0.035	<.005			0.124		
	CUTOFF			0.827				5	2.4	2.03	1.95					
	SIGNIF?	NO		YES		NO	NO	YES	YES	YES	YES			NO		
ITER 3	EFFECT			0.43				10.75	3.8	-2.43	3.034					
	W'			0.926				0.835	0.726	0.777	0.702					
	PROB			0.254				0.02	<.005	<.005	<.005					
	CUTOFF							4.375	1.6	1.95	0.636					
	SIGNIF?			NO				YES	YES	YES	YES					
ITER 4	EFFECT							-9.5	-1.2	-2.18	-1.169					
	W'							0.753	0.965	0.746	0.868					
	PROB							<.005	>.50	<.005	0.061					
	CUTOFF							5.5		1.33						
	SIGNIF?							YES	NO	YES	NO					
ITER 5	EFFECT							-6.25		-1.79						
	W'							0.768		0.714						
	PROB							0.006		<.005						
	CUTOFF							3.5		0.66						
	SIGNIF?							YES		YES						
ITER 6	EFFECT							2.25		0.84						
	W'							0.93		0.836						
	PROB							0.386		0.044						
	CUTOFF									0.693						
	SIGNIF?							NO		YES						
ITER 7	EFFECT									-0.56						
	W'									0.729						
	PROB									0.006						
	CUTOFF									0.61						
	SIGNIF?									NO						

Table A5.4
Schneider, Kasperski, and Weissfeld t-statistics for 2^4 Fractional Factorial Designs

	Example														
EFFECTS	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	0.059	-2.176	0.3209	-3.141	0.7215	-0.925	-0.92	-0.709	-5.074	5.6034	-1.592	-0.05	0.7352	1.9093	0.6173
2	-1.021	0.9345	-0.37	1.0622	-2.737	-0.432	9.434	-0.473	3.557	6.9371	0.0359	-0.379	1.046	0.4773	3.0864
3	2.2521	0.4829	0.7406	0.5825	-0.314	1.172	0.6903	-0.709	0.4966	-0.185	-0.251	0.8744	-0.521	-2.506	0.6173
4	1.8978	-0.671	0.3703	2.5928	-0.424	0.4318	0	5.4341	0.6846	1.4065	-0.969	1.0172	-0.123	-0.477	-1.235
5	0.4133	-0.746	0.9874	-0.994	0.6113	-1.172	0.2301	1.0632	0.4966	-0.241	0.5625	-0.331	0.2013	1.3126	1.2346
6	1.1218	0.5958	-0.049	-0.583	-0.799	-0.679	0.6903	-0.236	0.5772	-0.744	-0.395	0.9299	0.9278	1.074	0.6173
7	0.0084	-1.022	0.938	0.1713	-0.094	-3.763	-4.372	-0.354	-2.403	-0.002	1.1849	1.0361	-0.446	0.1193	-1.235
8	8.9998	2.5213	0.9874	0.1028	-0.413	-1.049	5.6374	-1.418	-3.262	6.6107	2.8606	-2.2	0.9716	1.6706	-0.617
9	0.7001	0.1129	-0.123	1.4506	-0.039	0.3084	-0.345	0.8269	0.1477	-0.595	-1.783	-0.717	-0.368	-0.716	0.6173
10	-0.481	-0.426	1.0615	0.3541	1.5035	0.5551	4.9471	0.1181	1.1275	-2.181	-2.645	-0.383	-3.86	0.1193	1.2346
11	0.6326	1.292	0.3209	0.5597	3.31	-1.049	1.0354	0.3544	0.1208	-0.431	-0.73	0.1877	-0.088	-0.477	-0.617
12	1.9315	0.5268	0.3209	-0.674	-5.105	-1.049	0.115	-6.497	0.4564	-0.157	-0.012	-0.711	-1.387	0.4773	0
13	-1.037	-1.468	-0.938	0.6282	-1.25	0.5551	0.5752	4.4891	-0.752	-0.729	-0.203	0.0578	1.3523	-0.119	-0.617
14	0.3121	-0.489	5.3073	-0.971	7.2313	0.3084	0.5752	0.1181	-2.926	-0.328	0.9455	-1.157	-2.643	0.716	0.6173
15	-0.531	0.5394	7.6524	-1.359	-0.215	-0.555	-2.876	-0.709	0.5772	1.2237	0.4189	-0.76	-0.109	3.1026	-1.235
t-inner	2.2361														
t-outer	4.2129														

Table A5.5
Summary of Classifications for 2^4 Fractional Factorial Designs

	Bayes	Benski	Lenth	Schneider, Kasperski and Weissfeld
0's (inactive)	192	202	190	190
1's (uncertain)	15	N/A	21	19
2's (active)	18	23	14	16

Table A5.6
Consistency Ratios for 2^4 Fractional Factorial Designs

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	---	191/202	189/190	188/190	0 in Column and Row Agrees
Benski	192/192	---	189/190	189/190	
Lenth	190/192	189/202	---	189/190	
Schneider, Kasperski, and Weissfeld	189/192	189/202	189/190	---	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	---	N/A	13/21	12/19	1 in Column and Row Agrees
Benski	N/A	---	N/A	N/A	
Lenth	13/15	N/A	---	18/19	
Schneider, Kasperski, and Weissfeld	12/15	N/A	18/21	---	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	---	17/23	13/14	15/16	2 in Column and Row Agrees
Benski	17/18	---	13/14	15/16	
Lenth	13/18	13/23	---	14/16	
Schneider, Kasperski, and Weissfeld	15/18	15/23	14/14	---	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	---	11/202	1/190	2/190	Col=0 Row>0
Benski	1/192	---	1/190	1/190	
Lenth	3/192	13/202	---	1/190	
Schneider, Kasperski, and Weissfeld	4/192	13/202	1/190	---	

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	10/202	1/190	2/190	Col=0 Row=1
Benski	N/A	—	N/A	N/A	
Lenth	3/192	12/202	—	1/190	
Schneider, Kasperski, and Weissfeld	4/192	12/202	1/190	—	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	1/202	0/190	0/190	Col=0 Row=2
Benski	1/192	—	1/190	1/190	
Lenth	0/192	1/202	—	0/190	
Schneider, Kasperski, and Weissfeld	0/192	1/202	0/190	—	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	N/A	5/21	3/19	Col=1 Row=2
Benski	5/15	—	9/21	7/19	
Lenth	1/15	N/A	—	0/19	
Schneider, Kasperski, and Weissfeld	1/15	N/A	2/21	—	

Table A5.7
Different Uncertain and Active Classifications for 2^4 Fractional Factorial Designs

Example	Effect Number and Estimate	Posterior Probabilities	ME	SME	t-value	t-inner	t-outer
1	3 (16.6875)	.3	19.034	38.66	**2.2521**	2.2361	4.2129
2	8 (100.5)	.44	**97.339**	197.71	**2.5213**	2.2361	4.2129
10	10 (-1.169)	**.68**	**1.06**	2.153	-2.181	2.2361	4.2129
11	10 (27.625)	.46	**26.021**	52.852	**-2.645**	2.2361	4.2129
12	8 (-81.312)	**.59**	102.109	207.397	-2.2	2.2361	4.2129
14	3 (-.21)	.38	**.193**	.392	**2.506**	2.2361	4.2129

Table A6
Results for Plackett-Burman Designs

			EFFECTS									
		1	2	3	4	5	6	7	8	9	10	11
	BAYES	0	1	0	0	0	0	0	0	0	0	0
EX 1	BENSKI	0	0	0	0	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0	0	0	0	0
	SKW	0	0	0	0	0	0	0	0	0	0	0
	BAYES	0	0	0	0	0	0	0	0	0	0	0
EX 2	BENSKI	0	0	0	0	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0	0	0	0	0
	SKW	0	0	0	0	0	0	0	0	0	0	0
	BAYES	1	0	1	0	0	0	2	1	0	2	0
EX 3	BENSKI	0	0	0	0	0	0	2	0	0	2	0
	LENTH	1	0	1	0	0	0	2	1	0	2	0
	SKW	1	0	1	0	0	0	2	1	0	2	0
	BAYES	0	0	0	0	0	0	0	0	0	0	0
EX 4	BENSKI	0	0	0	0	0	0	0	0	0	0	0
	LENTH	0	0	0	0	0	0	0	0	0	0	0
	SKW	0	0	0	0	0	0	0	0	0	0	0

Table A6.1
Effects, ME, SME, and Sigma for Plackett Burman Designs

	Examples			
EFFECTS	1	2	3	4
1	2.92	0.326	16.3	-0.092
2	10.58	0.294	1.1	0.1956
3	-0.75	-0.246	14.3	-0.2962
4	3.58	-0.516	2.6	0.2233
5	-5.25	0.15	4.5	-0.658
6	-1.08	0.915	0.7	0.1995
7	1.08	0.183	32.7	-0.327
8	-4.42	0.446	23	-0.4558
9	3.58	0.453	0.7	-0.3022
10	-0.25	0.081	42.7	-0.0771
11	-4.92	-0.242	2.4	-0.1097
ME	15.4119	1.2657	10.7625	0.9613
SME	33.0792	2.7166	23.1	2.0633
SIGMA	4.7706	0.4347	3.9278	0.3467

Table A6.2
Box and Meyer's Posterior Probabilities for Plackett-Burman Designs

	EXAMPLES			
EFFECTS	1	2	3	4
1	0.04	0.04	0.90	0.03
2	0.59	0.04	0.03	0.03
3	0.02	0.03	0.88	0.04
4	0.05	0.08	0.04	0.03
5	0.1	0.03	0.11	0.33
6	0.03	0.42	0.03	0.03
7	0.03	0.03	.95	0.05
8	0.06	0.06	0.92	0.11
9	0.05	0.06	0.03	0.05
10	0.02	0.02	.97	0.02
11	0.08	0.03	0.04	0.03

Table A6.3
Benski Results for Plackett Burman Designs

		EXAMPLE			
		1	2	3	4
ITER 1	EFFECT	10.58	0.915	42.7	-0.658
	W'	0.921	0.959	0.839	0.948
	PROB	0.278	> .5	< .036	> .5
	CUTOFF			3.58	
	SIGNIF?	NO	NO	YES	NO
ITER 2	EFFECT			32.7	
	W'			0.827	
	PROB			< .035	
	CUTOFF			30.4	
	SIGNIF?			YES	
ITER 3	EFFECT			23.0	
	W'			0.805	
	PROB			0.029	
	CUTOFF			26.4	
	SIGNIF?			NO	

Table A6.4
Schneider, Kasperski, and Weissfeld t-statistics for Plackett-Burman Designs

	Examples			
EFFECTS	1	2	3	4
1	0.61208	0.74994	4.14991	-0.2654
2	2.21775	0.67633	0.28005	0.56418
3	-0.1572	-0.5659	3.64071	-0.8543
4	0.75043	-1.187	0.66195	0.64407
5	-1.1005	0.34507	1.14568	-1.8979
6	-0.2264	2.1049	0.17822	0.57543
7	0.22639	0.42098	8.32527	-0.9432
8	-0.9265	1.02599	5.8557	-1.3147
9	0.75043	1.0421	0.17822	-0.8716
10	-0.0524	0.18634	10.8712	-0.2224
11	-1.0313	-0.5567	0.61103	-0.3164
t-inner	2.5286			
t-outer	6.913			

Table A6.5
Summary of Classifications for Plackett-Burman Designs

	Bayes	Benski	Lenth	Schneider, Kasperski, Weissfeld
0's (inactive)	38	42	39	39
1's (uncertain)	4	N/A	3	3
2's (active)	2	2	2	2

Table A6.6
Consistency Ratios for Plackett-Burman Designs

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	-----	38/42	39/39	39/39	0 in Column and Row Agrees
Benski	39/38	-----	39/39	39/39	
Lenth	38/38	39/42	-----	39/39	
Schneider, Kasperski, and Weissfeld	38/38	39/42	39/39	-----	
	Bayes	**Benski**	**Lenth**	**Schneider, Kasperski, and Weissfeld**	
Bayes	-----	-----	3/3	3/3	1 in Column and Row Agrees
Benski	-----	-----	-----	-----	
Lenth	3/4	-----	-----	3/3	
Schneider, Kasperski, and Weissfeld	3/4	-----	3/3	-----	
	Bayes	**Benski**	**Lenth**	**Schneider, Kasperski, and Weissfeld**	
Bayes	-----	2/2	2/2	2/2	2 in Column and Row Agrees
Benski	2/2	-----	2/2	2/2	
Lenth	2/2	2/2	-----	2/2	
Schneider, Kasperski, and Weissfeld	2/2	2/2	2/2	-----	
	Bayes	**Benski**	**Lenth**	**Schneider, Kasperski, and Weissfeld**	
Bayes	-----	4/42	1/39	1/39	Col = 0 Row > 0
Benski	-----	-----	-----	-----	
Lenth	0/38	3/42	-----	0/39	
Schneider, Kasperski, and Weissfeld	0/38	3/42	0/39		

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	-----	1/42	1/39	1/39	Col = 0 Row = 1
Benski	-----	-----	-----	-----	
Lenth	0/38	1/42	-----	0/39	
Schneider, Kasperski, and Weissfeld	0/38	1/42	0/39	-----	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	-----	0/42	0/39	0/39	Col = 0 Row = 2
Benski	0/38	-----	0/39	0/39	
Lenth	0/38	0/42	-----	0/39	
Schneider, Kasperski, Weissfeld	0/38	0/42	0/39	-----	
	Bayes	Benski	Lenth	Schneider, Kasperski, Weissfeld	
Bayes	-----	-----	2/3	2/3	Col = 1 Row = 2
Benski	0/4	-----	0/3	0/3	
Lenth	0/4	-----	-----	0/3	
Schneider, Kasperski, and Weissfeld	0/4	-----	0/3	-----	

Table A6.7
Different Uncertain and Active Classifications for Plackett-Burman Designs

Example	Effect Number and Estimate	Posterior Probability	ME	SME	t-value	t-inner	t-outer
1	2 (10.58)	.59	15.4119	33.0792	2.2178	2.5286	6.913

Table A7
Consistency Ratios for Uncertain or Active Classifications for Each Design Type

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	8/9	21/23	24/27	2^3 Factorial Designs
Benski	8/27	—	8/23	8/27	
Lenth	21/27	8/9	—	23/27	
Schneider, Kasperski, and Weissfeld	24/27	8/9	23/23	—	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	17/17	18/19	18/20	2^4 Factorial Designs
Benski	17/18	—	17/19	17/20	
Lenth	18/18	17/17	—	18/20	
Schneider, Kasperski, and Weissfeld	18/18	17/17	18/19	—	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	10/10	16/22	16/23	2^5 Factorial Designs
Benski	10/16	—	10/22	10/23	
Lenth	16/16	10/10	—	22/23	
Schneider, Kasperski, and Weissfeld	16/16	10/10	22/22	—	
	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	2/2	8/8	11/11	2^3 Fractional Factorial Designs
Benski	2/11	—	2/8	2/11	
Lenth	8/11	2/2	—	8/11	
Schneider, Kasperski, and Weissfeld	11/11	2/2	8/8	—	

	Bayes	Benski	Lenth	Schneider, Kasperski, and Weissfeld	
Bayes	—	22/23	32/35	31/35	2^4 Fractional Factorial Designs
Benski	22/33	—	22/35	22/35	
Lenth	32/33	22/23	—	34/35	
Schneider, Kasperski, and Weissfeld	31/33	22/23	34/35	—	
	Bayes	**Benski**	**Lenth**	**Schneider, Kasperski, and Weissfeld**	
Bayes	—	2/2	5/5	5/5	Plackett-Burman
Benski	2/6	—	2/5	2/5	
Lenth	5/6	2/2	—	5/5	
Schneider, Kasperski, and Weissfeld	5/6	2/2	5/5	—	

Table A8
Summary of Effect Classifications for All Examples

	Bayes (%)	Benski (%)	Lenth (%)	Schneider, Kasperski, and Weissfeld (%)
0's (inactive)	609 (84.6)	657 (91.25)	608 (84.4)	599 (83.2)
1's (uncertain)	52 (7.2)	—	61 (8.5)	66 (9.2)
2's (active)	59 (8.2)	63 (8.75)	51 (7.1)	55 (7.6)

Table A9
Consistency Ratios for Uncertain or Active Classifications for All Examples

	Bayes (%)	Benski (%)	Lenth (%)	Schneider, Kasperski, and Weissfeld (%)
Bayes	-----	61/63 (97)	100/112 (89)	105/121 (87)
Benski	61/111 (55)	-----	61/112 (54)	61/121 (50)
Lenth	100/111 (90)	61/63 (97)	-----	110/121 (91)
Schneider, Kasperski and Weissfeld	105/111 (95)	61/63 (97)	110/112 (98)	-----

Table A10
Classification Results for the Methods Across the Various Types of Designs

	Full Factorials			Fractional Factorials			
Designs	2^3	2^4	2^5	2^3	2^4	Plackett-Burman	Overall
Classifications							
Most Inactives	Benski	Benski	Benski	Benski	Benski	Benski	Benski
Most Uncertains	SKW**	Lenth/SKW	SKW	Bayes	Lenth	Bayes	SKW
Most Actives	Bayes	Benski	Bayes	Benski/SKW	Benski	All	Benski
Most Probable Actives (Uncertain & Active)	Bayes/SKW	SKW	SKW	Bayes/SKW	Lenth/SKW	Bayes	SKW
Fewest Inactives	Bayes/SKW	SKW	SKW	Bayes/SKW	Lenth/SKW	Bayes	SKW
Fewest Uncertains*	Lenth	Bayes	Bayes	Lenth	Bayes	Lenth/SKW	Bayes
Fewest Actives	Benski/Lenth/SKW	Lenth	Benski	Bayes/Lenth	Lenth	All	Lenth
Fewest Probable Actives (Uncertain & Active)	Benski	Benski	Benski	Benski	Benski	Benski	Benski
Fewest Probable Actives* (Uncertain & Active)	Lenth	Bayes	Bayes	Lenth	Bayes	Lenth/SKW	Bayes

* When Considering only Bayes, Lenth, and Schneider, Kasperski, Weissfeld
**Schneider, Kasperski, Weissfeld

APPENDIX B. MODIFIED NORMAL PROBABILITY PLOTS

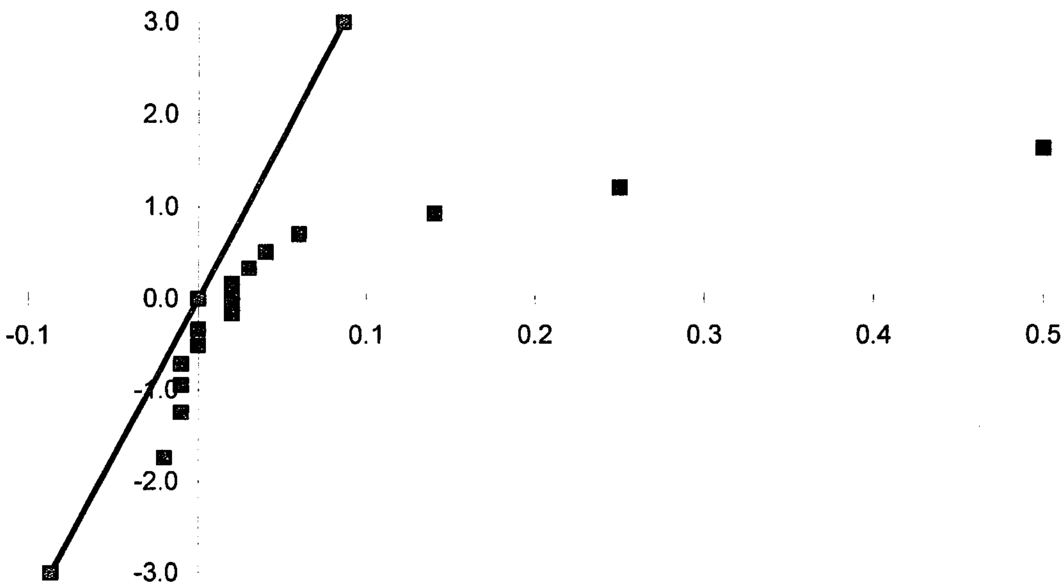

Figure B1
Modified Normal Probability Plot 2^4 Example 1

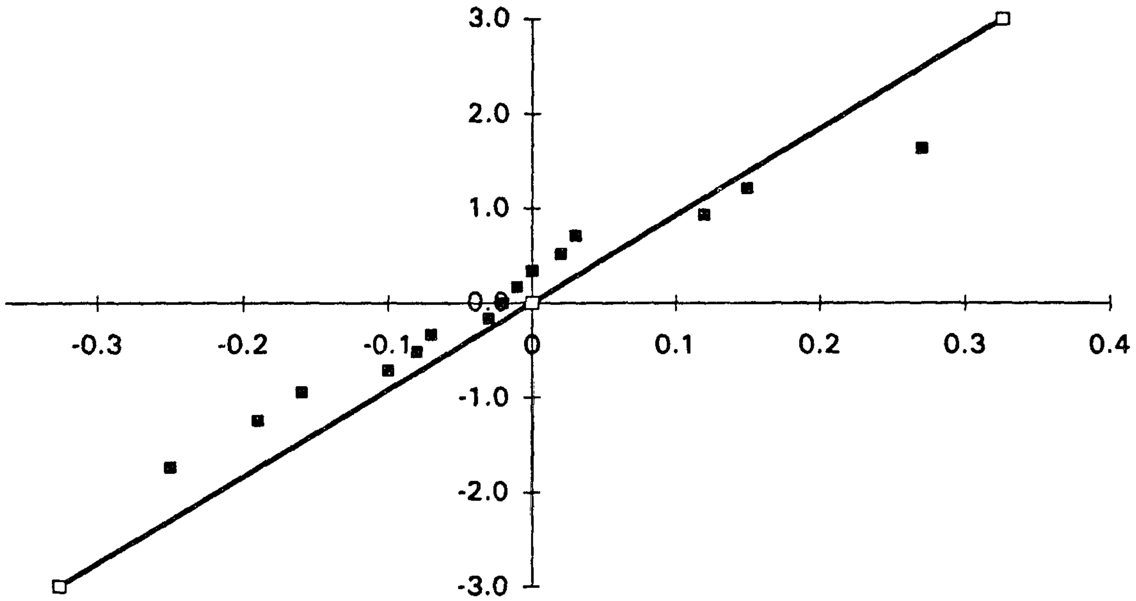

Figure B2
Modified Normal Probability Plot 2^4 Example 2

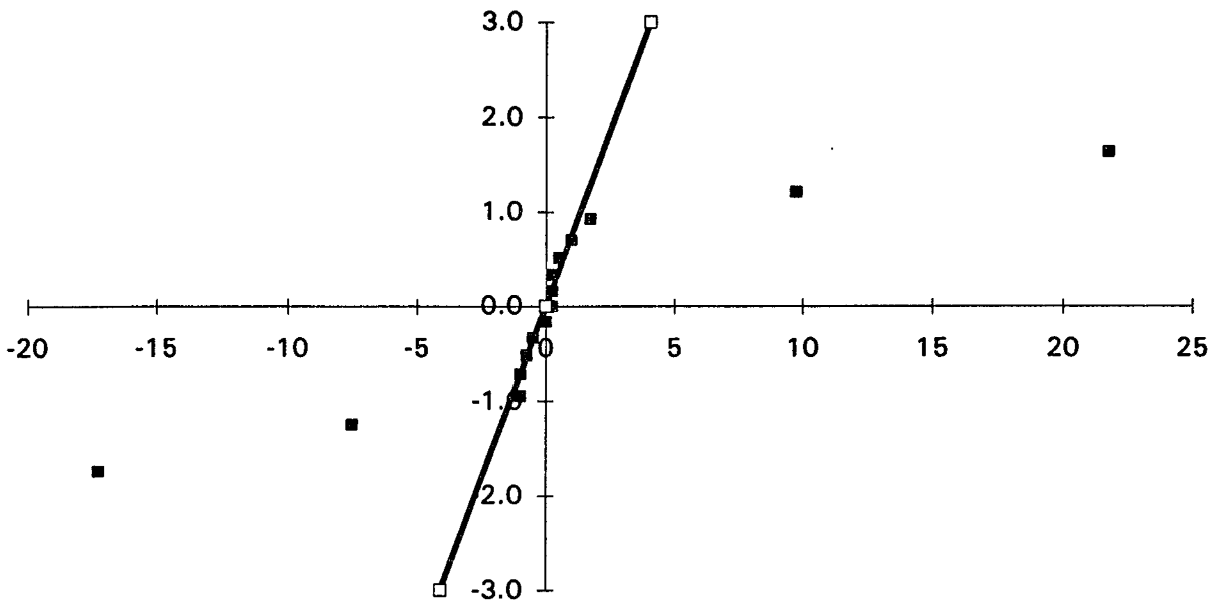

Figure B3
Modified Normal Probability Plot 2^4 Example 3

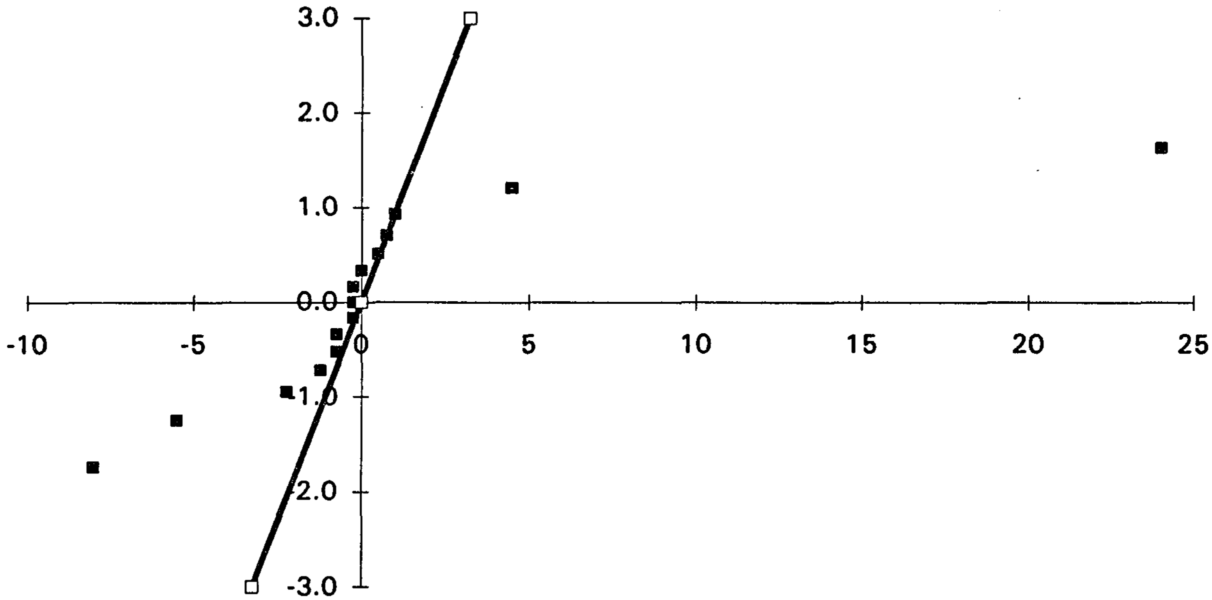

Figure B4
Modified Normal Probability Plot 2^4 Example 4

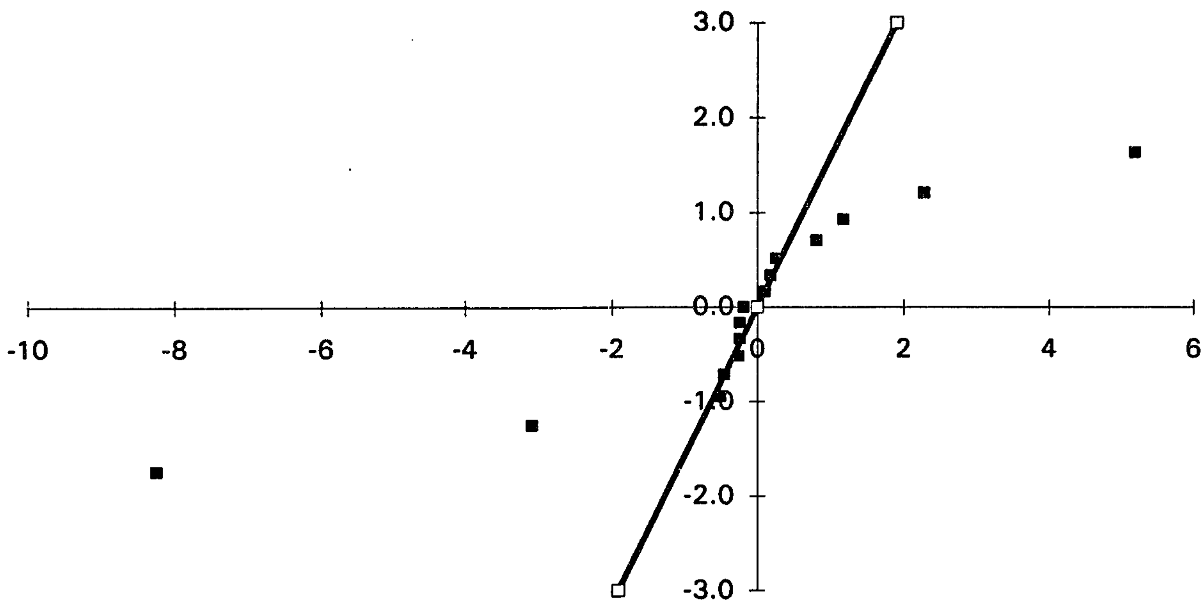

Figure B5
Modified Normal Probability Plot 2^4 Example 5

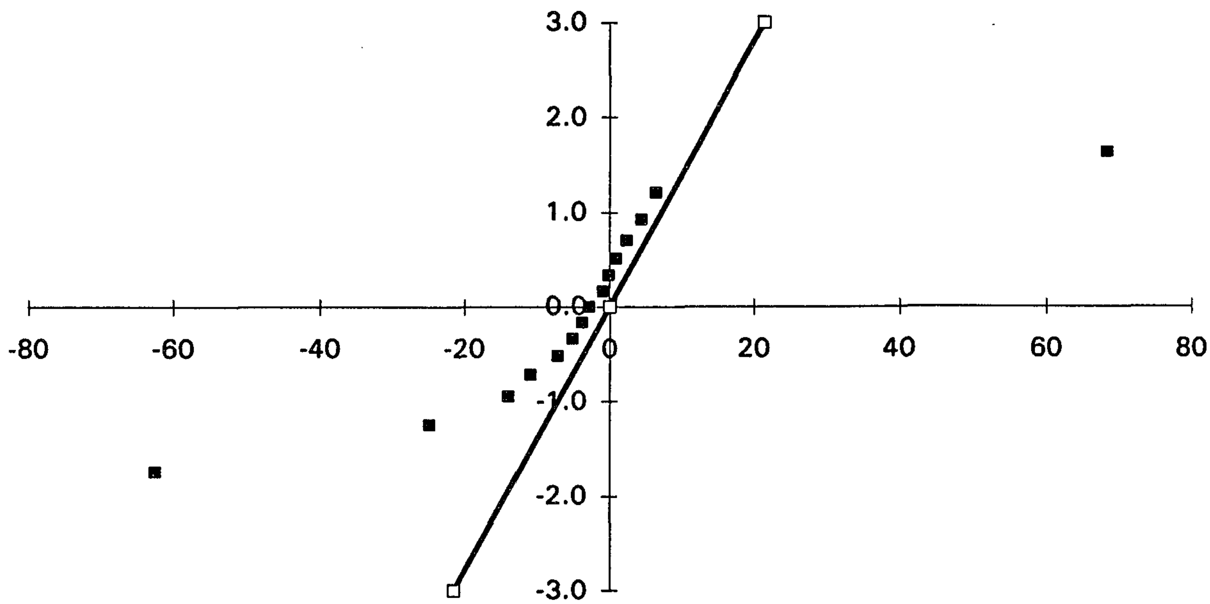

Figure B6
Modified Normal Probability Plot 2^4 Example 6

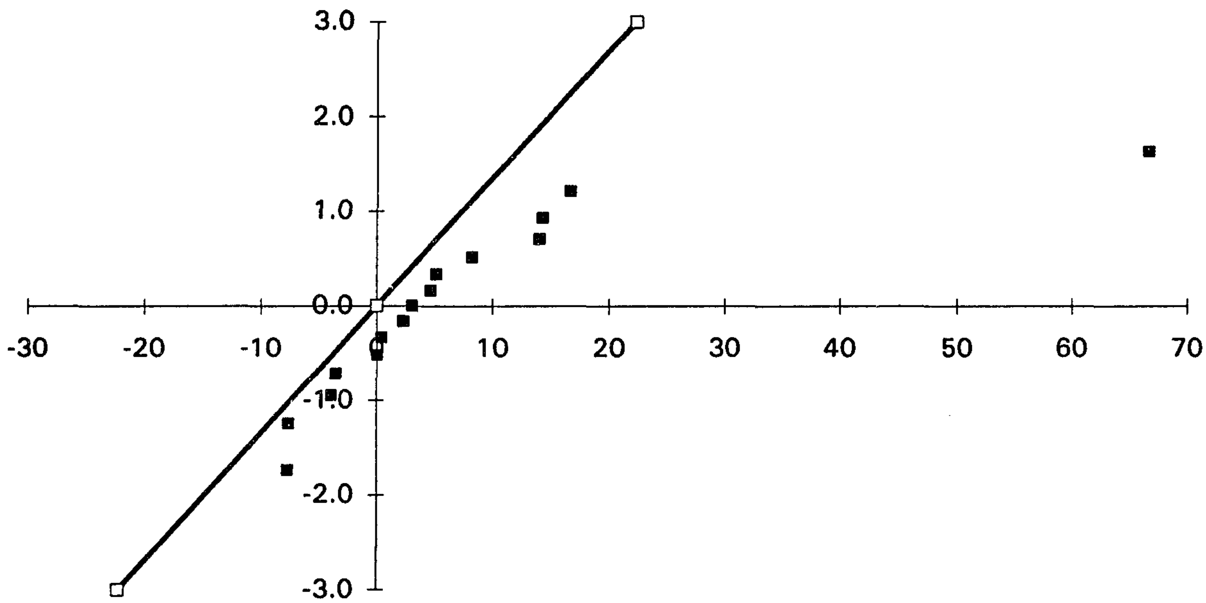

Figure B7
Modified Normal Probability Plot 2^4 Fractional Factorial Example 1

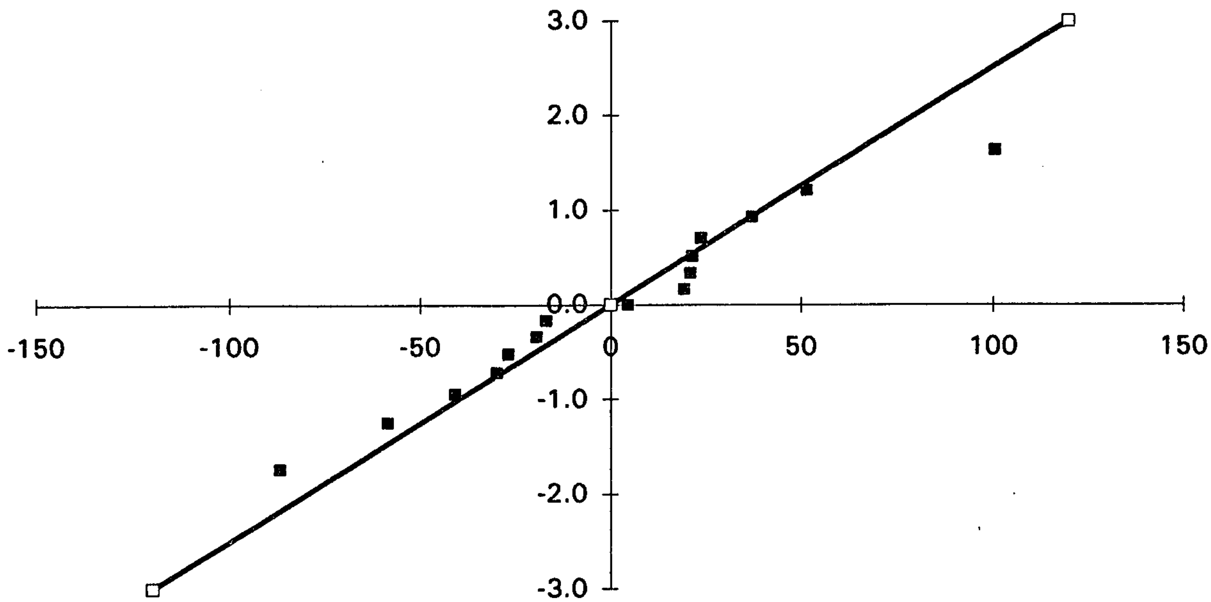

Figure B8
Modified Normal Probability Plot 2^4 Fractional Factorial Example 2

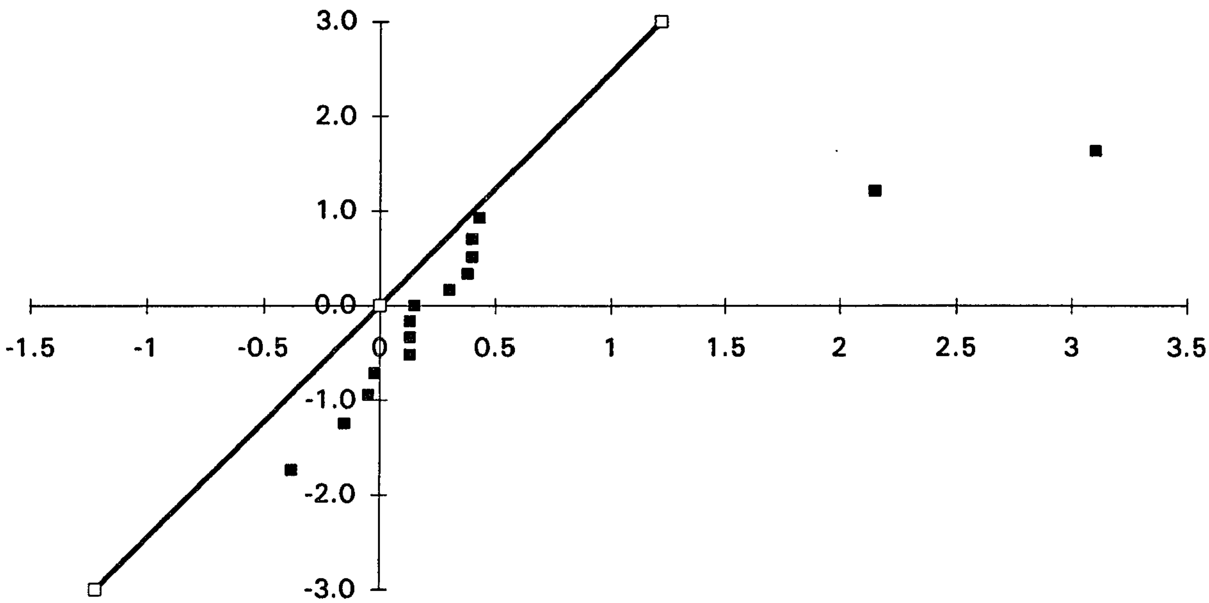

Figure B9
Modified Normal Probability Plot 2^4 Fractional Factorial Example 3

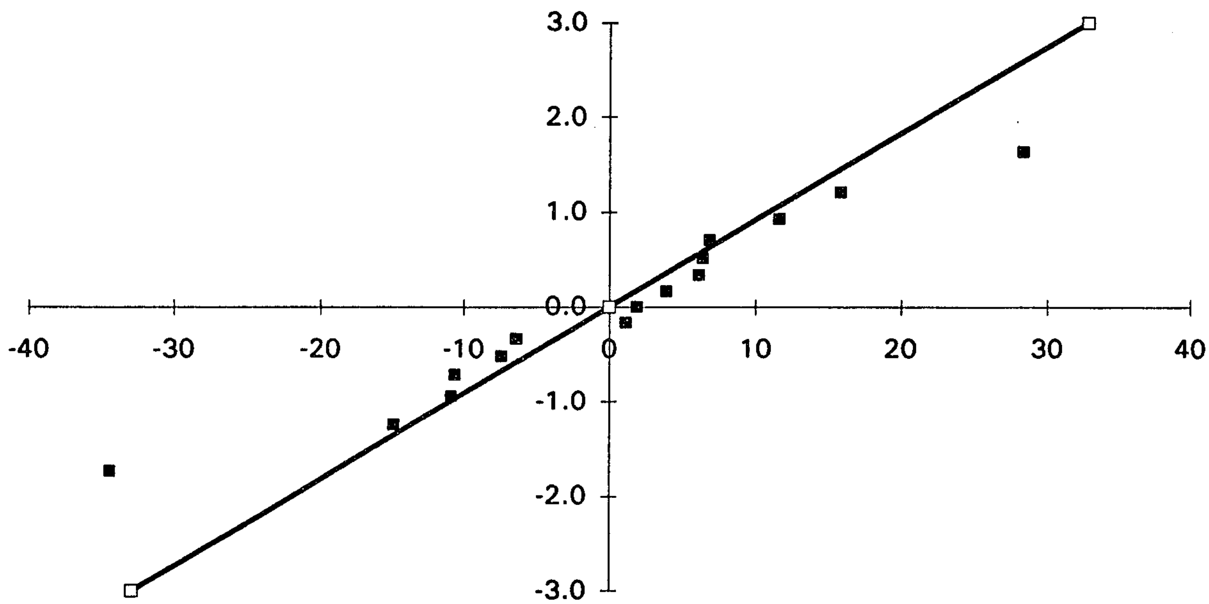

Figure B10
Modified Normal Probability Plot 2^4 Fractional Factorial Example 4

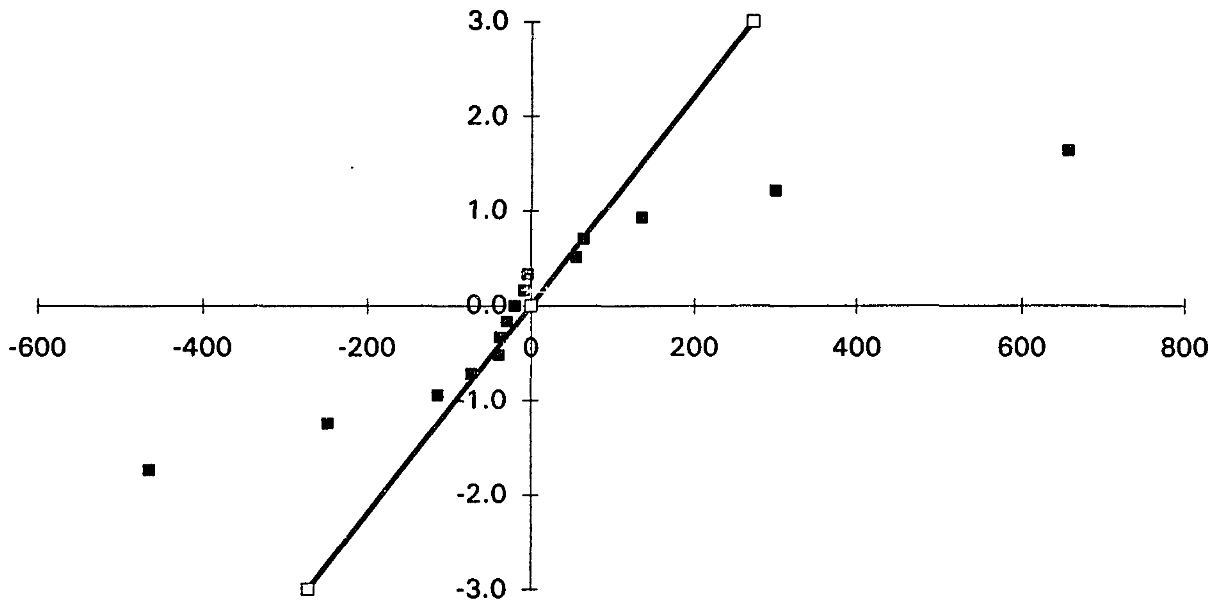

Figure B11
Modified Normal Probability Plot 2^4 Fractional Factorial Example 5

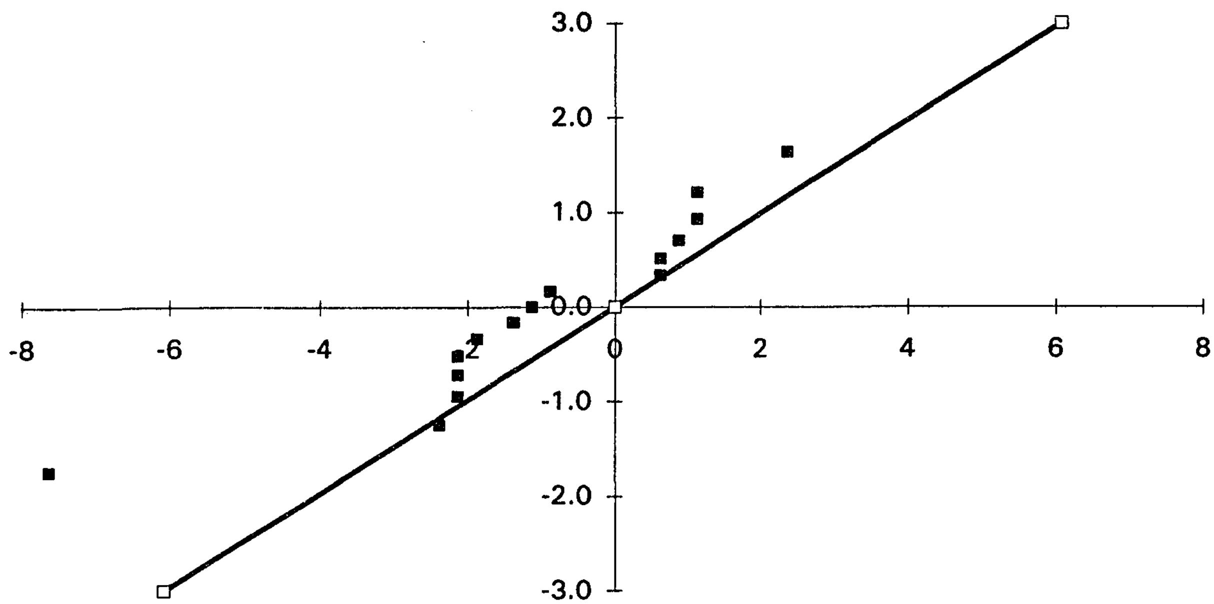

Figure B12
Modified Normal Probability Plot 2^4 Fractional Factorial Example 6

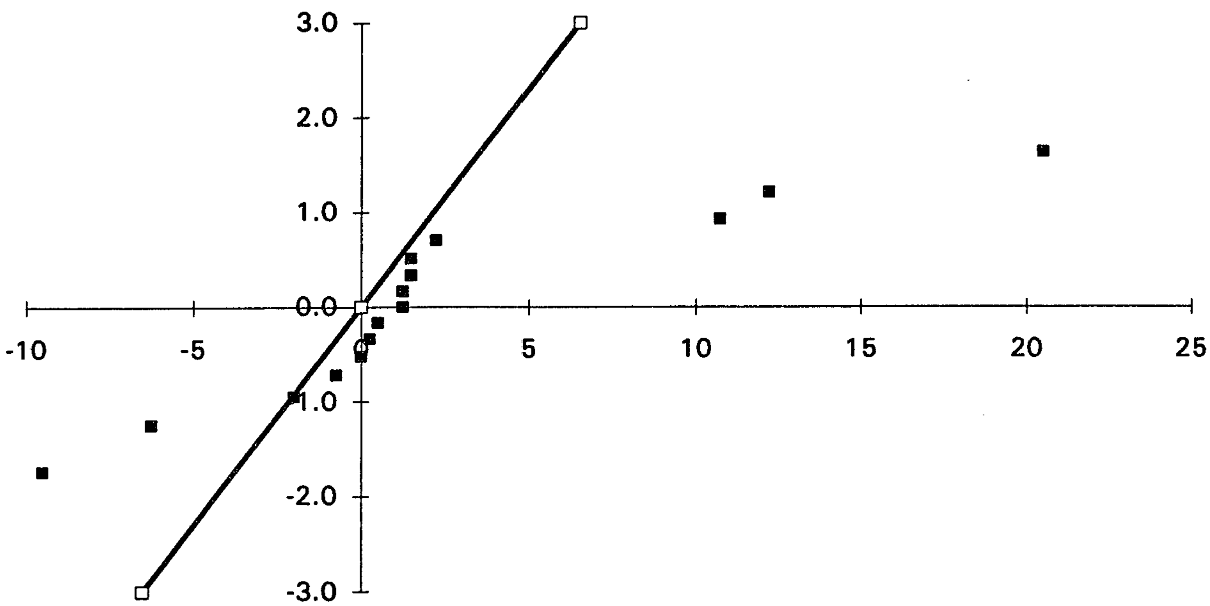

Figure B13
Modified Normal Probability Plot 2^4 Fractional Factorial Example 7

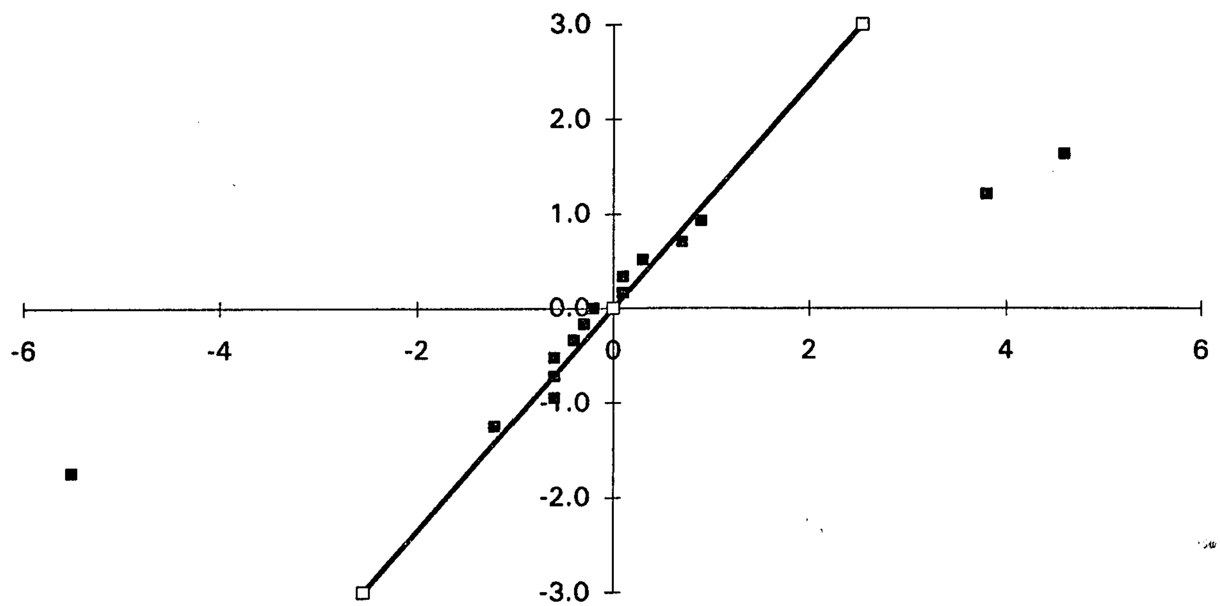

Figure B14
Modified Normal Probability Plot 2^4 Fractional Factorial Example 8

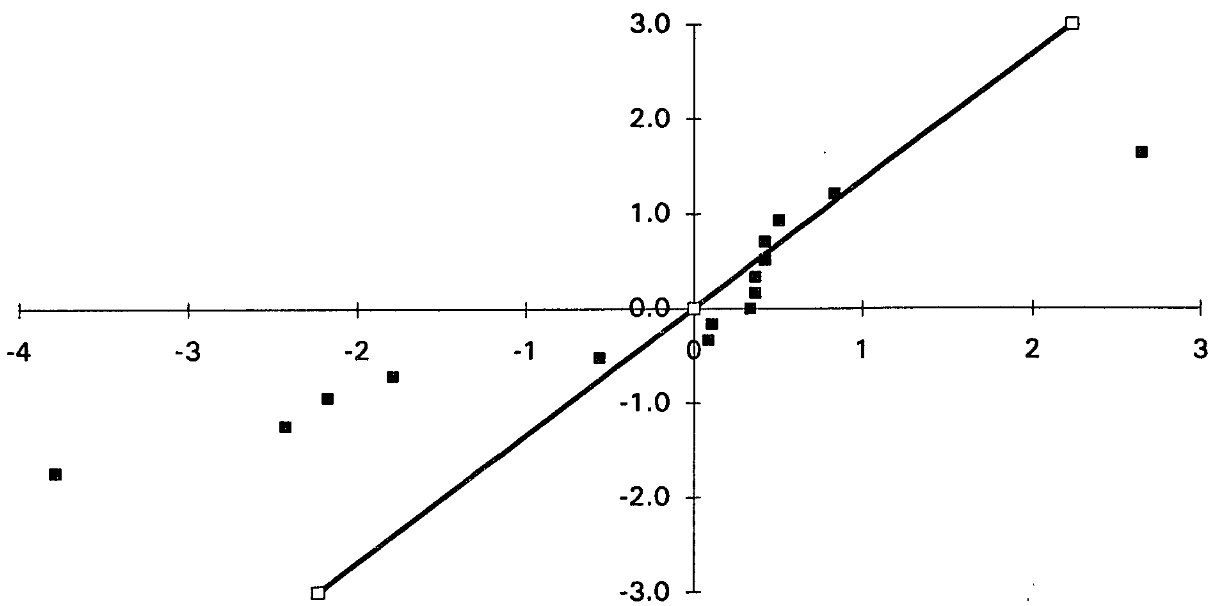

Figure B15
Modified Normal Probability Plot 2^4 Fractional Factorial Example 9

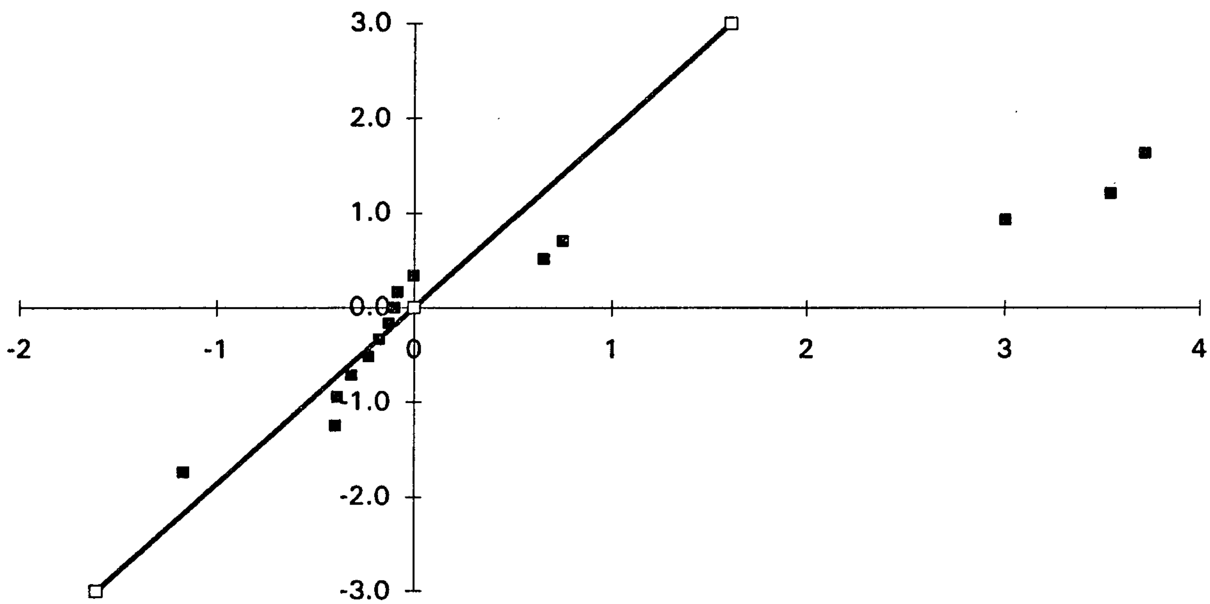

Figure B16
Modified Normal Probability Plot 2^4 Fractional Factorial Example 10

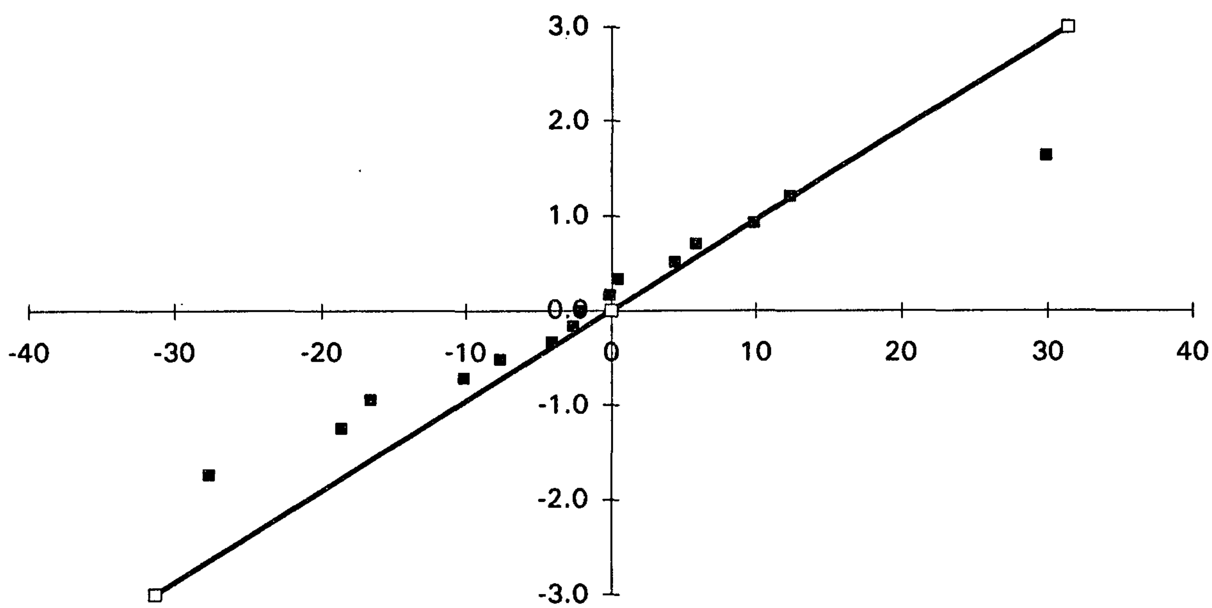

Figure B17
Modified Normal Probability Plot 2^4 Fractional Factorial Example 11

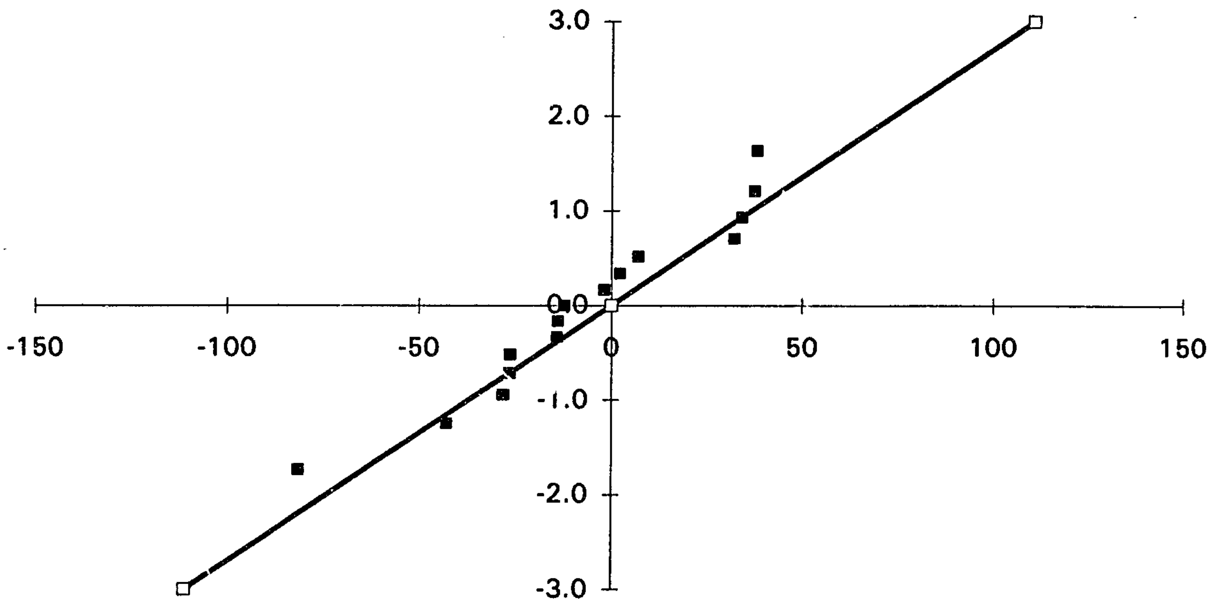

Figure B18
Modified Normal Probability Plot 2^4 Fractional Factorial Example 12

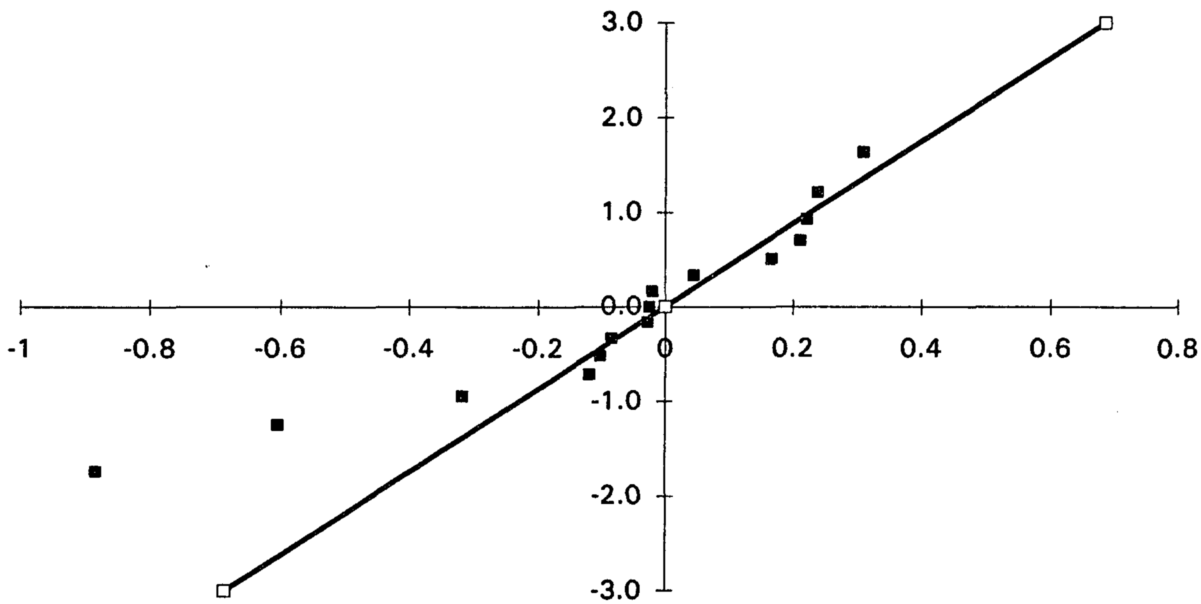

Figure B19
Modified Normal Probability Plot 2^4 Fractional Factorial Example 13

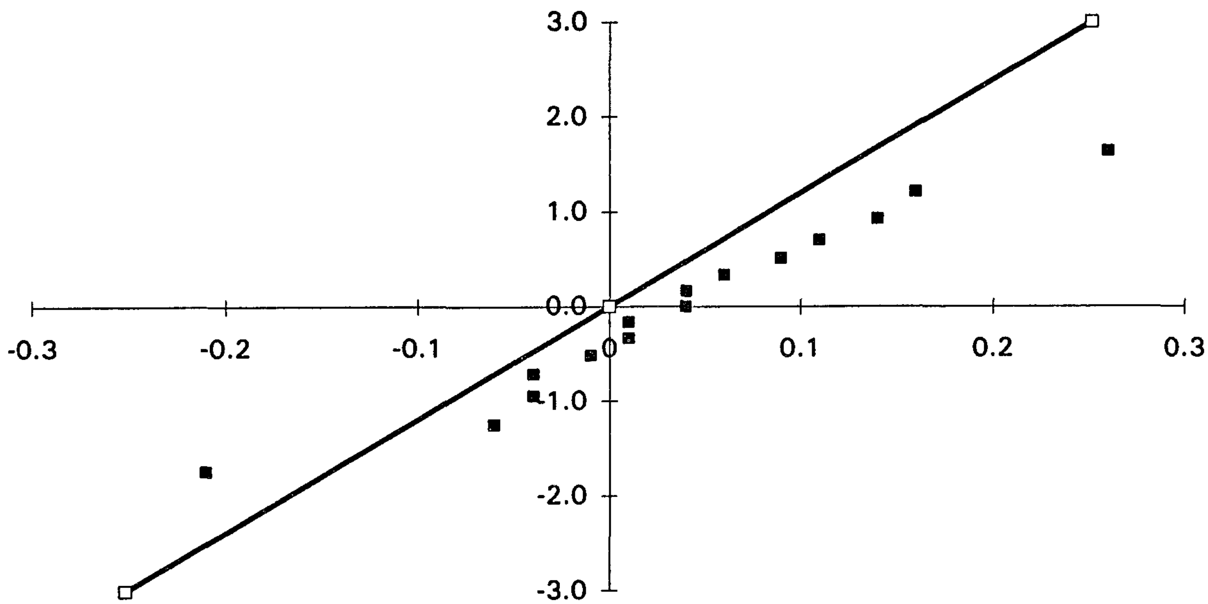

Figure B20
Modified Normal Probability Plot 2^4 Fractional Factorial Example 14

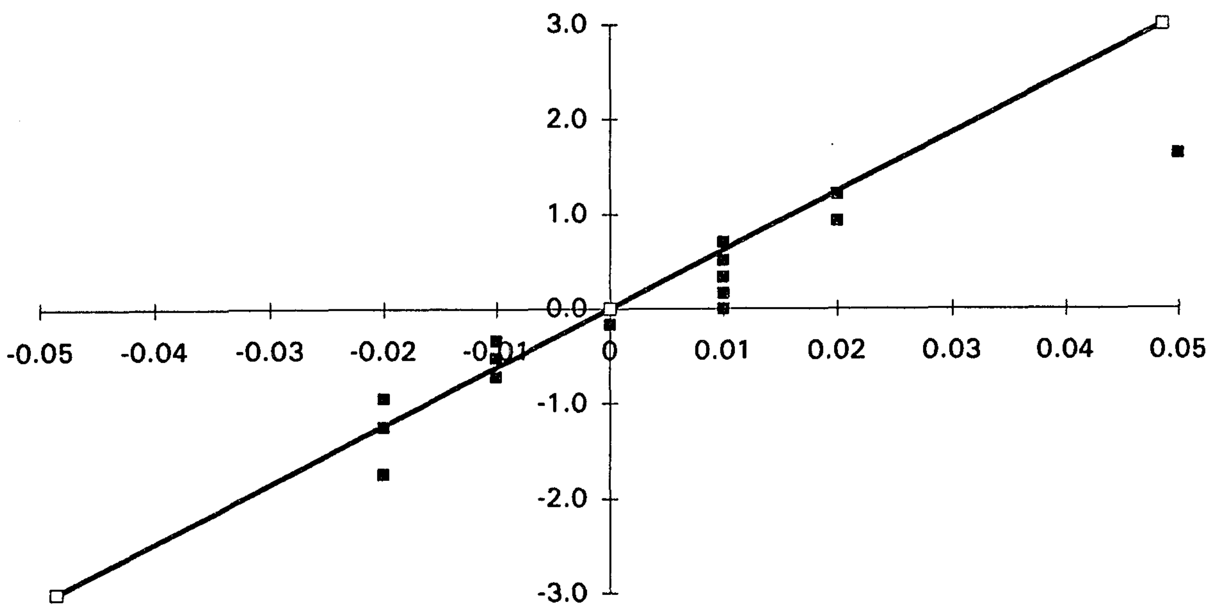

Figure B21
Modified Normal Probability Plot 2^4 Fractional Factorial Example 15

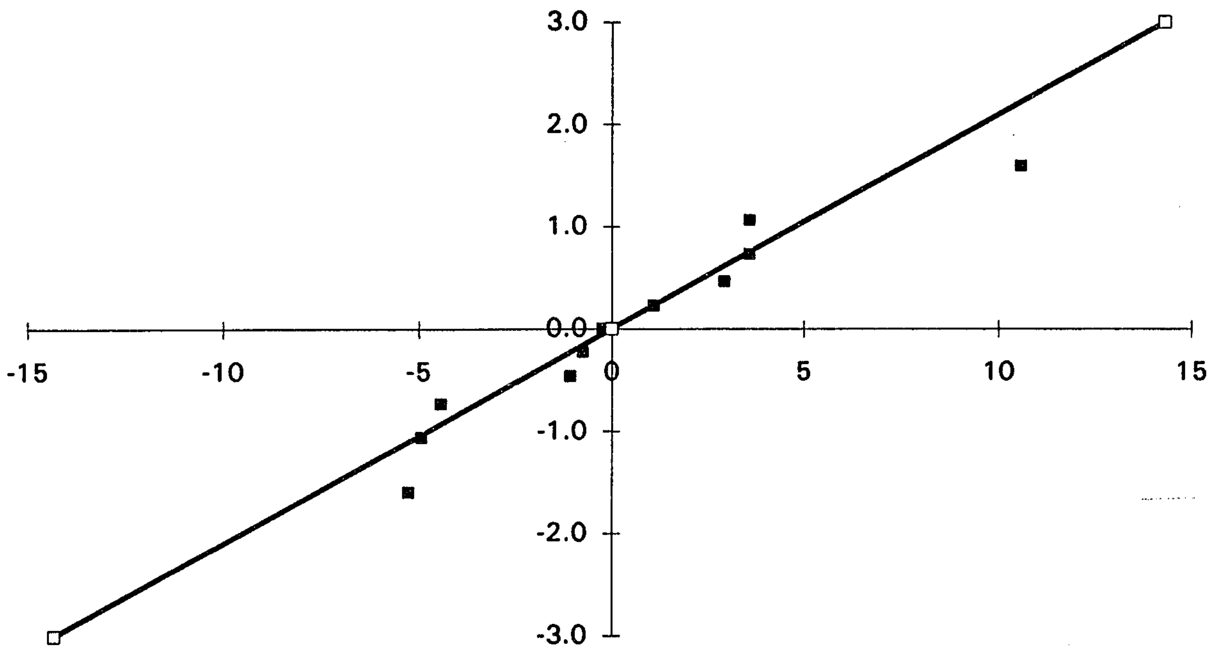

Figure B22
Modified Normal Probability Plot Plackett-Burman Example 1

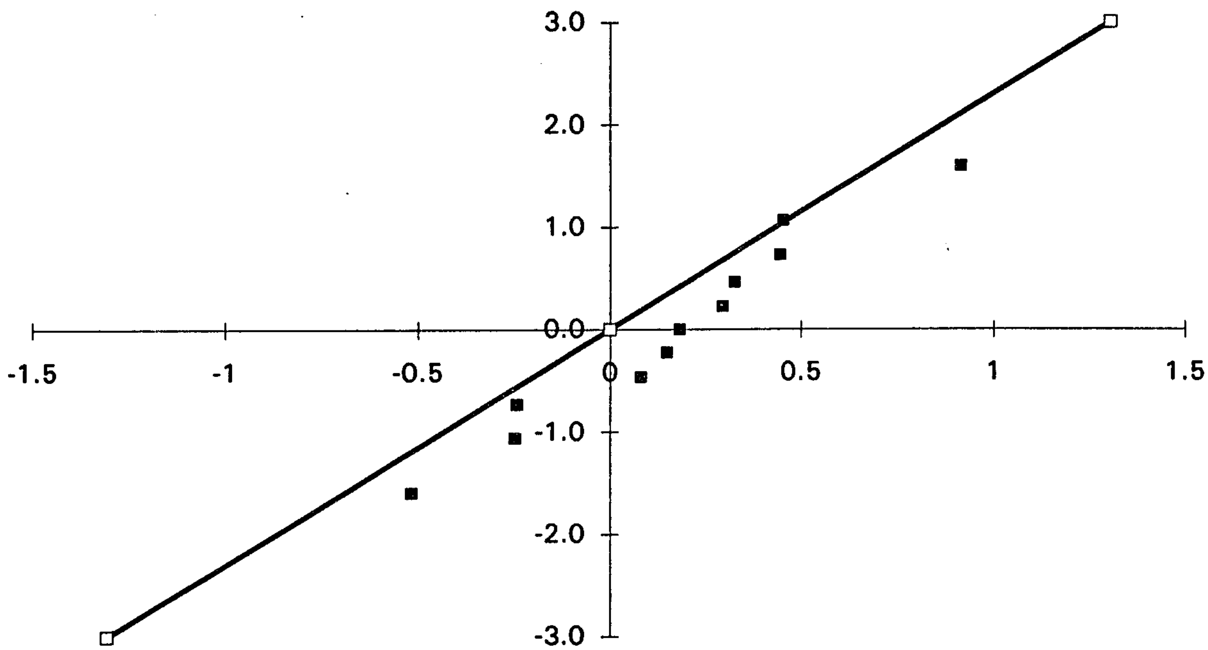

Figure B23
Modified Normal Probability Plot Plackett-Burman Example 2

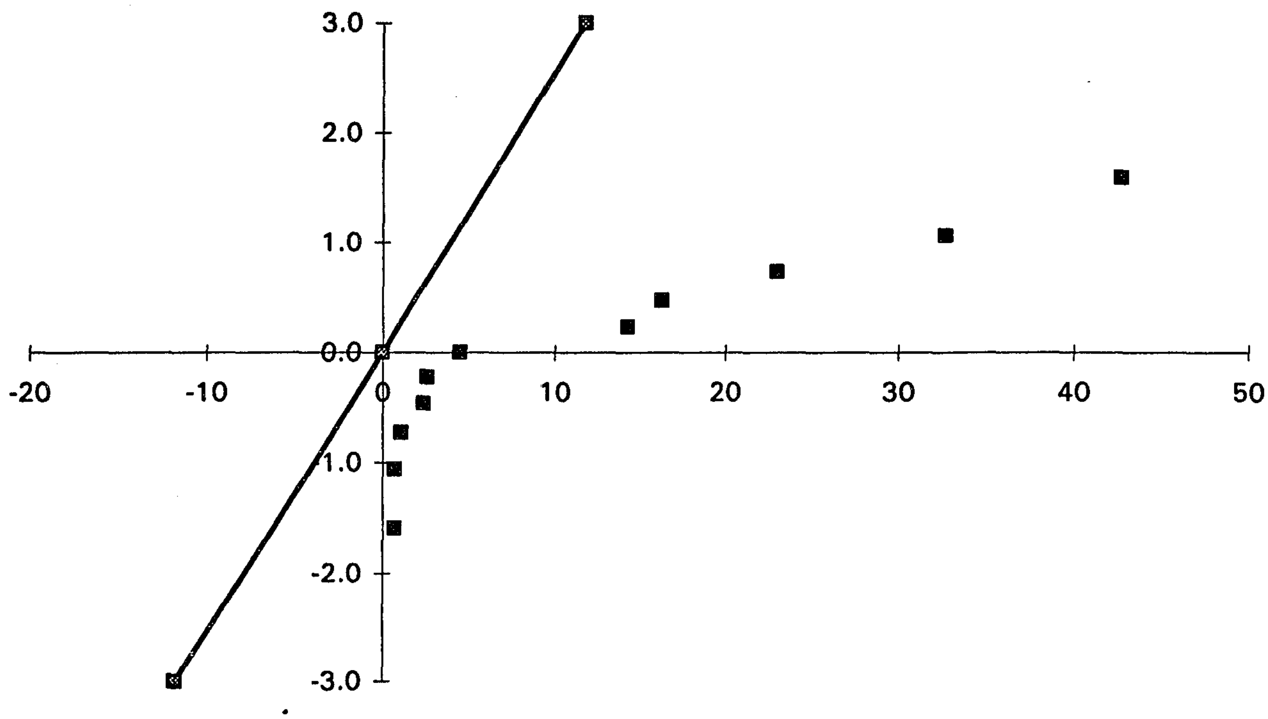

Figure B24
Modified Normal Probability Plot Plackett-Burman Example 3

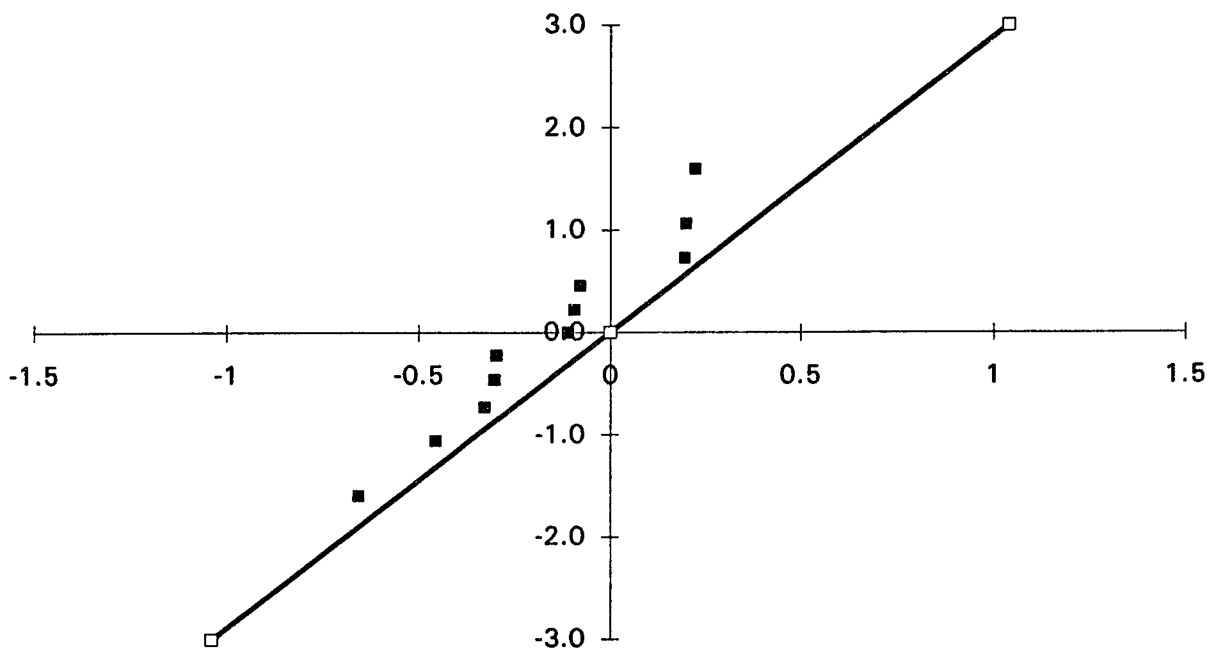

Figure B25
Modified Normal Probability Plot Plackett-Burman Example 4

APPENDIX C. DATA SET DESCRIPTIONS

Table C1.
2^3 Factorial Data Sets

Example	Source	Design	Factors	Response
1	Daniel (1976)	2^3	Time of Stirring (A), Temperature (B), Pressure (C)	Cement Thickening Time
2	Davies (1954)	2^3	Time of addition of nitric acid (A), Time of stirring (B), Heel effect (C)	Nitration Process Yield
3	Box & Draper (1987)	2^3	Specimen Lenth (S), Amplitude (A), Load (L)	Cycles to Failure
4	Box & Draper (1987)	2^3	Percent Carbon (C), Percent Manganese (M) Percent Nickel (N)	Martensite Start Temperature
5	Box & Draper (1987)	2^3	Paper Type (A), Humidity (B), Tear Direction (C)	Tear Resistance
6	Box, Hunter, & Hunter (1978)	2^3	Temperature (T), Concentration (C), Catalyst (K)	Yield
7	Box, Hunter, & Hunter (1978)	2^3	Popcorn Brand (A), Popcorn/Oil Ratio (B), Batch Size (C)	Popcorn Yield
8	Box, Hunter, & Hunter (1978)	2^3	Planting Depth (A), Watering Times (B), Bean Type (C)	Yield
9	Box, Hunter, & Hunter (1978)	2^3	Temperature (A), Concentration (B), Catalyst (C)	Yeild
10	Box, Hunter, & Hunter (1978)	2^3	Temperature (A), pH(B), Agitation Rate (C)	Chemical Yield
11	Box, Hunter, & Hunter (1978)	2^3	Temperature (A), Catalyst (B), pH(C)	Yield
12	Box, Hunter, & Hunter (1978)	2^3	Temperature (A), Concentration (B), Stirring Rate (C)	Percent Yield
13	Box, Hunter, & Hunter (1978)	2^3	Humidity (A), Temperature (B), Pump (C)	Finish Imperfections
14	Box, Hunter, & Hunter (1978)	2^3	Temperature (A), Concentration (B), Time (C)	Weight of Metal Recovered
15	Zelen and Connor (1959)	2^3	Carbon Content (A), Tempering Temperature (B), Method of Cooling (C)	Tensile Strength
16	Johnson & Leone (1977)	2^3	Agitation Speed (A), Concentration (B), Temperature (C)	Percent Yield
17	Johnson & Leone (1977)	2^3	Catalyst (A), Laboratory (B), Pressure (C)	Product Yield
18	Johnson & Leone (1977)	2^3	Catalyst (A), Concentration (B), Reaction Temperature (C)	Degree of Conversion

Table C2.
2^4 Factorial Data Sets

Example	Source	Design	Factors	Response
1	Daniel (1976)	2^4	Load (A), Flow (B), Speed (C), Mud (D)	Log drill advance
2	Davies (1954)	2^4	Acid strength (A), Time (B), Amount of acid (C), Temperature (D)	Yield of isatin
3	Box & Draper (1987)	2^4	Catalyst Concentration (A), NaOH Concentration (B), Agitation Level (C), Temperature (D)	Chemical Yield
4	Box, Hunter, & Hunter (1978)	2^4	Catalyst Charge (A), Temperature (B), Pressure (C), Concentration (D)	Conversion Percentage
5	Close (1967)	2^4	Daily Insolaton (A), Storage Capacity (B), Flow Rate (C), Intermittency (D)	Collection Efficiency
6	Johnson & Leone (1977)	2^4	Propellent Charge (A), Projectile Charge (B), Propellant Web (C), Weapon (D)	Projectile Velocity

Table C3.
2^5 Factorial Data Sets

Example	Source	Design	Factors	Response
1	Yates (1937)	2^5	Spacing of rows (S), Dung (D), Nitrochalk (N), Superphosphate (P), Muriate of Potash (K)	Bean Yield
2	Davies (1971)	2^5	Corn Steep Liquor Concentration (A), Lactose (B), Precursor (C), Sodium Nitrate (D), Glucose (E)	Penicillin Yield
3	Kempthorne (1952)	2^5	Sulfate of Ammonia (S), Superphosphate (P), Muriate of Potash (K), Agricultural salt (N), Dung (D)	Mangold Yield
4	Box, Hunter, & Hunter (1978)	2^5	Feed Rate (A), Catalyst (B), Agitation Rate (C), Temperature (D), Concnetration (E)	Percent Reacted
5	Johnson & Leone (1977)	2^5	Concentration (A), Distillation Rate (B), Solution Volume (C), Stirring Rate (D), Solvent-to-Water Ratio (E)	Residual Acidity
6	Johnson & Leone (1977)	2^5	Heating Time (A), Quenching Time (B), Drawing Time (C), Boss (D), Position of Measurement on the Boss (E)	Casting Hardness

Table C4.
2^3 Fractional Factorial Data Sets

Example	Source	Design	Factors	Response
1	Davies (1954)	2^{4-1}	Concentration of filtered liquor (A), Freshness (B), Butanol (C), Temperature (D)	Purity
2	Davies (1954)	2^{5-2}	Reactant (A), Acid Concentration (B), Acid Amount (C), Reaction Time (D), Reaction Temperature (E)	Percent Yield of Medicine
3	Box & Draper (1987)	2^{7-4}	Foreman Presence (A), Sex of Packer (B), Time of Day (C), Temperature (D), Music (E), Packer Age (F), Factory Location (G)	Packing Time
4	Box, Hunter & Hunter (1978)	2^{7-4}	Seat (A), Dynamo (B), Handlebars (C), Gear (D), Raincoat (E), Breakfast (F), Tires (G)	Time to Climb Hill
5	Box, Hunter & Hunter (1978)	2^{4-1}	Acid Concentration (A), Catalyst Concentration (B), Temperature (C), Monomer Concentration (D)	Stability
6	Box, Hunter & Hunter (1978)	2^{7-4}	Water Supply (A), Raw Material (B), Temperature (C), Recycle (D), Caustic Soda (E), Filter Cloth (F), Holdup Time (G)	Filtration Time
7	Johnson & Leone (1977)	2^{4-1}	Location (A), Mix (B), Cure (C), Test (D)	Rubber Tensile Strength

Table C5.
2^4 Fractional Factorial Data Sets

Example	Source	Design	Factors	Response
1	Davies (1954)	2^{5-1}	Oxidation Temperature (A), Starting Material Quality (B), Reduction Pressure (C), Oven Drying Pressure (D), Vacuum Leak (E)	Quality of Dyestuff
2	Davies (1954)	2^{5-1}	Preparation Corn Steep Liquor Concentration (A), Sugar Amount (B), Sugar Quality (C), Fermentation Corn Steep Liquor Concentration (D), Corn Steep Liquor Quality	Penicillin Yield
3	Taguchi & Wu (1980)	2^{9-5}	Rods (A), Period (B), Material (C), Thickness (D), Angle (E), Opening (F), Current (G), Method (H), Preheating (J)	Tensile strength
4	Chang & Kononenko (1962)	2^{6-2}	Sucrose (S), Paraform (P), NaOH (N), Water (W), Max Temperature (T), Time at Max Temperature (t)	Shear Strength (psi)
5	Mason, Gunst, Hess (1989)	2^{7-3}	Sample Preparation (A), Moisture Measure (B), Speed (C), Mixing Time (D), Equilibrium Time (E), Spindle (F), Lid (G)	Viscosity
6	Mason, Gunst, Hess (1989)	2^{6-2}	Bed Temperature (A), Tube Temperature (B), Particle Size (C), Environment (D), Particle Material (E), Tube Material (F)	Corrosion
7	Box, Hunter & Hunter (1978)	2^{5-1}	Feed Rate (A), Catalyst (B), Agistation Rate (C), Temperature (D), Concentration (E)	Percent Reacted
8	Box, Hunter & Hunter (1978)	2^{8-4}	Temperature (T), Moisture (M), Holding pressure (H), Thickness (V), Booster pressure (B), Cycle Time (C), Gate Size (G), Speed (S)	Shrinkage
9	Box, Hunter & Hunter	2^{8-4}	Number of Lengths of Track (A), Number of Adjacent Sectors (B), Navigation Beacons (C), Jet Mix (D), Aircraft Arrivals (E), Communication Transactions (F), Transaction Length (G), Intertransaction Gap Time (H)	Time
10	Stegner, Wu, Braton (1967)	2^{5-1}	Open-Circuit Voltage (A), Slope (B), Melt-Off Rate (C), Diameter (D), Extension (E)	Voltage
11	Anderson & McLean (1974)	2^{5-1}	Aggregate Gradation (A), Compaction Temperature (B), Asphalt Content (C), Curing Condition (D), Curing Temperature (E)	Index of Goodness
12	Haaland (1989)	2^{6-2}	RadDos (A), Prime1 (B), VolPrs (C), CelNum (D), Growth (E), Prime2 (F)	Antibody Yield

13	Quinlan (1985)	2^{15-11}	Linear Tension (A), Linear Line Speed (B), Linear Die (C), Liner OD (D), Melt Temperature (E), Coating Material (F), Linear Temperature (G), Braiding Tension (H), Wire Braid Type (I), Linear Material (J), Cooling Method (K), Screen Pack (L), Coating Die Type (M), Wire Diameter (N), Line Speed (O)	Speedometer Case Shrinkage
14	Schneider, Pruett, and Magee (1994)	2^{6-2}	Sample Temperature (A), Air Chamber Time (B), Fuel Chamber Temperature (C), Fuel Bottom Filled (D), Air Chamber Saturations (E), Number of Sample Shakes (F)	RVP
15	Schneider, Pruett, and Magee (1994)	2^{6-2}	Bottle Temperature (A), Bottle Bottom Filled (B), Bottle Fill Rate (C), Bottle Fill Level (D), Sample Temperature (E), Cork Type (F)	RVP

VITA

William J. Kasperski completed a Bachelors of Science degree in Mathematics and Statistics at Northern Arizona University in May, 1986. He is currently a Visiting Assistant Professor at Tulane University. He has accepted a position at The University of Nebraska at Omaha after completing graduate studies at Louisiana State University. His research interests include statistical quality control, design of experiments, and total quality management.

DOCTORAL EXAMINATION AND DISSERTATION REPORT

Candidate: William J. Kasperski

Major Field: Business Administration

Title of Dissertation: Inference for Unreplicated Factorial and Fractional Factorial Designs

Approved:

____
Major Professor and Chairman

____
Dean of the Graduate School

EXAMINING COMMITTEE:

____

____

____

____

____

____

____

____

____

Date of Examination:

March 4, 1994