

Design of Experiments With Multiple Independent Variables: A Resource Management Perspective on Complete and Reduced Factorial Designs

Linda M. Collins, John J. Dziak, and Runze Li
Pennsylvania State University

An investigator who plans to conduct an experiment with multiple independent variables must decide whether to use a complete or reduced factorial design. This article advocates a resource management perspective on making this decision, in which the investigator seeks a strategic balance between service to scientific objectives and economy. Considerations in making design decisions include whether research questions are framed as main effects or simple effects; whether and which effects are aliased (confounded) in a particular design; the number of experimental conditions that must be implemented in a particular design and the number of experimental subjects the design requires to maintain the desired level of statistical power; and the costs associated with implementing experimental conditions and obtaining experimental subjects. In this article 4 design options are compared: complete factorial, individual experiments, single factor, and fractional factorial. Complete and fractional factorial designs and single-factor designs are generally more economical than conducting individual experiments on each factor. Although relatively unfamiliar to behavioral scientists, fractional factorial designs merit serious consideration because of their economy and versatility.

Keywords: experimental design, fractional factorial designs, factorial designs, reduced designs, resource management

Suppose a scientist is interested in investigating the effects of k independent variables, where $k > 1$. For example, Bolger and Amarel (2007) investigated the hypothesis that the effect of peer social support on performance stress can be positive or negative, depending on whether the way the peer social support is given enhances or degrades self-efficacy. Their experiment could be characterized as involving four factors: support offered (yes or no), nature of

support (visible or indirect), message from a confederate that recipient of support is unable to handle the task alone (yes or no), and message that a confederate would be unable to handle the task (yes or no).

One design possibility when $k > 1$ independent variables are to be examined is a factorial experiment. In factorial research designs, experimental conditions are formed by systematically varying the levels of two or more independent variables or factors. For example, in the classic 2×2 factorial design, there are two factors each with two levels. The two factors are crossed—that is, all combinations of levels of the two factors are formed—to create a design with four experimental conditions. More generally, factorial designs can include $k \geq 2$ factors and can incorporate two or more levels per factor. With four two-level variables, such as in Bolger and Amarel (2007), a complete factorial experiment would involve $2 \times 2 \times 2 \times 2 = 16$ experimental conditions. One advantage of factorial designs, compared with simpler experiments that manipulate only a single factor at a time, is the ability to examine interactions between factors. A second advantage of factorial designs is their efficiency with respect to use of experimental subjects; factorial designs require fewer experimental

Linda M. Collins, The Methodology Center and Department of Human Development and Family Studies, Pennsylvania State University; John J. Dziak, The Methodology Center, Pennsylvania State University; Runze Li, The Methodology Center and Department of Statistics, Pennsylvania State University.

This research was supported by National Institute on Drug Abuse Grants P50 DA10075 and K05 DA018206. We would like to thank Bethany C. Bray, Michael J. Cleveland, Donna L. Coffman, Mark Feinberg, Brian R. Flay, John W. Graham, Susan A. Murphy, Megan E. Patrick, Brittany Rhoades, and David Rindskopf for comments on an early draft of this article.

Correspondence concerning this article should be addressed to Linda M. Collins, The Methodology Center, Penn State, 204 East Calder Way, Suite 400, State College, PA 16801. E-mail: lmcollins@psu.edu

subjects than comparable alternative designs to maintain the same level of statistical power (e.g., Wu & Hamada, 2000).

However, a complete factorial experiment is not always an option. In some cases there may be combinations of levels of the factors that would create a nonsensical, toxic, logistically impractical, or otherwise undesirable experimental condition. For example, Bolger and Amarel (2007) could not have conducted a complete factorial experiment because some of the combinations of levels of the factors would have been illogical (e.g., no support offered, but support was direct). But even when all combinations of factors are reasonable, resource limitations may make implementation of a complete factorial experiment impossible. As the number of factors and levels of factors under consideration increases, the number of experimental conditions that must be implemented in a complete factorial design increases rapidly. The accompanying logistical difficulty and expense may exceed available resources, prompting investigators to seek alternative experimental designs that require fewer experimental conditions.

In this article the term *reduced design* is used to refer generally to any design approach that involves experimental manipulation of all k independent variables but includes fewer experimental conditions than a complete factorial design with the same k variables. Reduced designs are often necessary to make simultaneous investigation of multiple independent variables feasible. However, any removal of experimental conditions to form a reduced design has important scientific consequences. The number of effects that can be estimated in an experimental design is limited to one fewer than the number of experimental conditions represented in the design. Therefore, when experimental conditions are removed from a design, some effects are combined so that their sum only, not the individual effects, can be estimated. Another way to think of this is that two or more interpretational labels (e.g., main effect of Factor A, interaction between Factor A and Factor B) can be applied to the same source of variation. This phenomenon is known as aliasing (sometimes referred to as confounding, or as collinearity in the regression framework).

Any investigator who wants or needs to examine multiple independent variables is faced with deciding whether to use a complete factorial or a reduced experimental design. The best choice is one that strikes a careful and strategic balance between service to scientific objectives and economy. Weighing a variety of considerations to achieve such a balance, including the exact research questions of interest, the potential impact of aliasing on interpretation of results, and the costs associated with each design option, is the topic of this article.

This article has two objectives. The first objective is to propose that a resource management perspective may be helpful to investigators who are choosing a design for an

experiment that will involve several independent variables. The resource management perspective assumes that an experiment is motivated by a finite set of research questions and that these questions can be prioritized for decision-making purposes. Then according to this perspective, the preferred experimental design is the one that, in relation to the resource requirements of the design, offers the greatest potential to advance the scientific agenda motivating the experiment. Four general design alternatives are considered from a resource management perspective: complete factorial designs and three types of reduced designs. One of the reduced designs, the fractional factorial, is used routinely in engineering but is currently unfamiliar to many social and behavioral scientists. In our view, fractional factorial designs merit consideration by social and behavioral scientists alongside other more commonly used reduced designs. Accordingly, a second objective of this article is to offer a brief introductory tutorial on fractional factorial designs, in the hope of assisting investigators who wish to evaluate whether these designs might be of use in their research.

Overview of Four Design Alternatives

Throughout this article, it is assumed that an investigator is interested in examining the effects of k independent variables, each of which could correspond to a factor in a factorial experiment. It is not necessarily a foregone conclusion that the k independent variables must be examined in a single experiment; they may represent a set of questions composing a program of research or a set of features or components composing a behavioral intervention program. It is assumed that the k factors can be independently manipulated and that no possible combination of the factors would create an experimental condition that cannot or should not be implemented. For the sake of simplicity, it is also assumed that each of the k factors has only two levels, such as On and Off or Yes and No. Factorial and fractional factorial designs can be done with factors having any number of levels, but two-level factors allow the most straightforward interpretation and largest statistical power, especially for interactions.

In this section we introduce the four design alternatives considered in this article using a hypothetical example based on the following scenario: An investigator is to conduct a study on anxiety related to public speaking (this example is modeled very loosely on Bolger and Amarel, 2007). There are three factors of theoretical interest to the investigator, each with two levels, On and Off. The factors are whether or not (a) the subject is allowed to choose a topic for the presentation (Choose), (b) the subject is taught a deep-breathing relaxation exercise to perform just before giving the presentation (Breath), and (c) the subject is provided with extra time to prepare for the speech (Prep). This small hypothetical example will be useful in illustrat-

ing some initial key points of comparison among the design alternatives. Later in the article, the hypothetical example is extended to include more factors so that some additional points can be illustrated.

The first alternative considered here is a complete factorial design. The remaining alternatives considered are reduced designs, each of which can be viewed as a subset of the complete factorial.

Complete Factorial Designs

Factorial designs may be denoted with the exponential notation 2^k , which compactly expresses that k factors with two levels each are crossed, resulting in 2^k experimental conditions (sometimes called cells). Each experimental condition represents a unique combination of levels of the k factors. In the hypothetical example, a complete factorial design would be expressed as 2^3 (or equivalently $2 \times 2 \times 2$) and would involve eight experimental conditions.

Table 1 shows these eight experimental conditions along with effect coding. The design enables estimation of seven effects: three main effects, three two-way interactions, and a single three-way interaction. Table 1 illustrates one feature of complete factorial designs in which an equal number of subjects is assigned to each experimental condition, namely the balance property. A design is balanced if each level of each factor appears in the design the same number of times and is assigned to the same number of subjects (Hays, 1994; Wu & Hamada, 2000). In a balanced design, the main effects and interactions are orthogonal, so that each one is estimated and tested as if it were the only one under consideration, with very little loss of efficiency owing to the presence of other factors.¹ (Effects may still be orthogonal even in unbalanced designs if certain proportionality conditions are met; see, e.g., Hays, 1994, p. 475.) The balance property is evident in Table 1; each level of each factor appears exactly four times.

Individual Experiments

The individual-experiments approach requires conducting a two-condition experiment for each independent variable, that is, k separate experiments. In the example this would require conducting three experiments, involving a total of six experimental conditions. In one experiment, a condition in which subjects are allowed to choose the topic of the presentation would be compared with one in which subjects are assigned a topic; in a second experiment, a condition in which subjects are taught a relaxation exercise would be compared with one in which no relaxation exercise is taught; in a third experiment, a condition in which subjects are given ample time to prepare in advance would be compared with one in which subjects are given little preparation time. The subset of experimental conditions from the complete three-factor factorial experiment in Table 1

that would be implemented in the individual-experiments approach is depicted in the first section of Table 2. This design, considered as a whole, is not balanced. Each of the independent variables is set to On once and set to Off five times.

Single-Factor Designs in Which the Factor Has Many Levels

In the single-factor approach, a single experiment is performed in which various combinations of levels of the independent variables are selected to form one nominal or ordinal categorical factor with several qualitatively distinct levels. West and colleagues (West & Aiken, 1997; West, Aiken, & Todd, 1993) reviewed three variations of the single-factor design that are used frequently, particularly in research on behavioral interventions for prevention and treatment. In the comparative treatment design, there are $k + 1$ experimental conditions: k experimental conditions in which one independent variable is set to On and all the others to Off, plus a single control condition in which all independent variables are set to Off. This approach is similar to conducting separate individual experiments, except that a shared control group is used for all factors. The second section of Table 2 shows the four experimental conditions that would constitute a comparative treatment design in the hypothetical example. These are the same experimental conditions that appear in the individual-experiments design.

By contrast, for the constructive treatment design, an intervention is built by combining successive features. For example, an investigator interested in developing a treatment to reduce anxiety might want to assess the effect of allowing the subject to choose a topic, then the incremental effect of also teaching a relaxation exercise, then the incremental effect of allowing extra preparation time. The third section of Table 2 shows the subset of experimental conditions from the complete factorial shown in Table 1 that would be implemented in a three-factor constructive treatment experiment in which first Choose is added, followed by Breath and then Prep. The constructive treatment strategy typically has $k + 1$ experimental conditions but may have fewer or more. The dismantling design, in which the objective is to determine the effect of removing one or more features of an intervention, and other single-factor designs are based on similar logic.

Table 2 shows that both the comparative treatment design and the constructive treatment design are unbalanced. In the

¹ If we assume that orthogonality is maintained, adding a factor to a factorial experiment does not change estimates of main effects and interactions. However, the addition of a factor does change estimates of error terms, so hypothesis tests can be slightly different.

Table 1
Effect Coding for Complete Factorial Design With Three Two-Level Factors

Experimental condition	Factor 1 Choose	Factor 2 Breath	Factor 3 Prep	Main effects			Interactions			
				1	2	3	1 × 2	1 × 3	2 × 3	1 × 2 × 3
1	Off	Off	Off	-1	-1	-1	1	1	1	-1
2	Off	Off	On	-1	-1	1	1	-1	-1	1
3	Off	On	Off	-1	1	-1	-1	1	-1	1
4	Off	On	On	-1	1	1	-1	-1	1	-1
5	On	Off	Off	1	-1	-1	-1	-1	1	1
6	On	Off	On	1	-1	1	-1	1	-1	-1
7	On	On	Off	1	1	-1	1	-1	-1	-1
8	On	On	On	1	1	1	1	1	1	1

comparative treatment design, each factor is set to On once and set to Off three times. In the constructive treatment design, Choose is set to Off once and to On three times, and Prep is set to On once and to Off three times. Other single-factor designs are similarly unbalanced.

Fractional Factorial Designs

The fourth alternative considered in this article is to use a design from the family of fractional factorial designs. A fractional factorial design involves a special, carefully chosen subset, or fraction, of the experimental conditions in a complete factorial design. The bottom section of Table 2 shows a subset of experimental conditions from the complete three-factor factorial design that constitute a fractional factorial design. The experimental conditions in fractional factorial designs are selected so as to preserve the balance property.² As Table 2 shows, each level of each factor appears in the design exactly twice.

Fractional factorial designs are represented with an exponential notation based on that used for complete factorial designs. The fractional factorial design in Table 2 would be expressed as 2^{3-1} . This notation contains the following information: (a) The corresponding complete factorial design is 2^3 —in other words, it involves three factors, each of which has two levels, for a total of 8 experimental conditions—(b) the fractional factorial design involves $2^{3-1} = 2^2 = 4$ experimental conditions, and (c) this fractional factorial design is a $2^{-1} = \frac{1}{2}$ fraction of the complete factorial. Many fractional factorial designs, particularly those with many factors, involve even smaller fractions of the complete factorial.

Aliasing in the Individual-Experiments, Single-Factor, and Fractional Factorial Designs

As mentioned above, reduced designs involve aliasing of effects. A design's aliasing is evident in its effect coding. When effects are aliased, their effect coding is perfectly correlated (whether positively or negatively). Aliasing in the individual-experiments approach can be seen by examining the first section of Table 2. In the experiment exam-

ining Choose, the effect codes are identical for the main effect of Choose and the Choose × Breath × Prep interaction (-1 for Experimental Condition 1 and 1 for Experimental Condition 4), and these are perfectly negatively correlated with the effect codes for the Choose × Breath and Choose × Prep interactions. Thus, these effects are aliased; the effect estimated by this experiment is an aggregate of the main effect of Choose and all the interactions involving Choose. (The codes for the remaining effects, namely the main effects of Breath and Prep and the Breath × Prep interaction, are constants in this design.) Similarly, in the experiment investigating Breath, the main effect and all the interactions involving Breath are aliased, and in the experiment investigating Prep, the main effect and all the interactions involving Prep are aliased.

The aliasing in single-factor experiments using the comparative treatment strategy is identical to the aliasing in the individual-experiments approach. As shown in the second section of Table 2, for the hypothetical example, a comparative treatment experiment would involve Experimental Conditions 1, 2, 3, and 5, which are the same conditions as in the individual-experiments approach. The effects of each factor are assessed by means of the same comparisons. For example, the effect of Choose would be assessed by comparing Experimental Conditions 1 and 5. The primary difference is that only one control condition would be required in the single-factor experiment, whereas in the individual-experiments approach three control conditions are required.

The constructive treatment strategy comprises a different subset of experimental conditions from the full factorial than the individual-experiments and comparative treatment

² In the social and behavioral sciences literature, the term *fractional factorial* has sometimes been applied to reduced designs that do not maintain the balance property, such as the individual-experiments and single-factor designs. In this article we maintain the convention established in the statistics literature (e.g., Wu & Hamada, 2000) of reserving the term *fractional factorial* for the subset of reduced designs that maintain the balance property.

Table 2
Effect Coding for Reduced Designs That Comprise Subsets of the Design in Table 1

Experimental condition	Factor 1 Choose	Factor 2 Breath	Factor 3 Prep	Main effects			Interactions			
				1	2	3	1×2	1×3	2×3	$1 \times 2 \times 3$
Subset comprising individual experiments										
Effect of Choose										
1	Off	Off	Off	-1	-1	-1	1	1	1	-1
5	On	Off	Off	1	-1	-1	-1	-1	1	1
Effect of Breath										
1	Off	Off	Off	-1	-1	-1	1	1	1	-1
3	Off	On	Off	-1	1	-1	-1	1	-1	1
Effect of Prep										
1	Off	Off	Off	-1	-1	-1	1	1	1	-1
2	Off	Off	On	-1	-1	1	1	-1	-1	1
Subset comprising single factor design: Comparative treatment design										
1	Off	Off	Off	-1	-1	-1	1	1	1	-1
2	Off	Off	On	-1	-1	1	1	-1	-1	1
3	Off	On	Off	-1	1	-1	-1	1	-1	1
5	On	Off	Off	1	-1	-1	-1	-1	1	1
Subset comprising single factor design: Constructive treatment design										
1	Off	Off	Off	-1	-1	-1	1	1	1	-1
5	On	Off	Off	1	-1	-1	-1	-1	1	1
7	On	On	Off	1	1	-1	1	-1	-1	-1
8	On	On	On	1	1	1	1	1	1	1
Subset comprising fractional factorial design										
2	Off	Off	On	-1	-1	1	1	-1	-1	1
3	Off	On	Off	-1	1	-1	-1	1	-1	1
5	On	Off	Off	1	-1	-1	-1	-1	1	1
8	On	On	On	1	1	1	1	1	1	1

approaches. Nevertheless, the aliasing is similar. As the third section of Table 2 shows, the effect of adding Choose would be assessed by comparing Experimental Conditions 1 and 5, so the aliasing would be the same as that in the individual experiment investigating Choose as discussed above. The cumulative effect of adding Breath would be assessed by comparing Experimental Conditions 5 and 7. The effect codes in these two experimental conditions for the main effect of Breath are perfectly (positively or negatively) correlated with those for all the interactions involving Breath, although here the effect codes for the interactions are reversed, in comparison with the individual-experiments and comparative treatment approaches. The same reasoning applies to the effect of Prep, which is assessed by comparing Experimental Conditions 7 and 8.

As the fourth section of Table 2 illustrates, the aliasing in fractional factorial designs is different from the aliasing seen in the individual-experiments and single-factor approaches. In this fractional factorial design, the effect of Choose is estimated by comparing the mean of Experimental Conditions 2 and 3 with the mean of Experimental

Conditions 5 and 8; the effect of Breath is estimated by comparing the mean of Experimental Conditions 3 and 8 to the mean of Experimental Conditions 2 and 5; and the effect of Prep is estimated by comparing the mean of Experimental Conditions 2 and 8 to the mean of Experimental Conditions 3 and 5. The effect codes show that the main effect of Choose and the Breath × Prep interaction are aliased. The remaining effects are either orthogonal to the aliased effect or constant. Similarly, the main effect of Breath and the Choose × Prep interaction are aliased, and the main effect of Prep and the Choose × Breath interaction are aliased.

Note that each source of variation in this fractional factorial design has two aliases (e.g., Choose and the Breath × Prep interaction form a single source of variation). This is characteristic of fractional factorial designs that, like this one, are one-half fractions. The denominator of the fraction always reveals how many aliases each source of variation has. Thus, in a fractional factorial design that is a one-quarter fraction, each source of variation has four aliases; in a fractional factorial design that is a one-eighth fraction, each source of variation has eight aliases; and so on.

Aliasing and Scientific Questions

An investigator who is interested in using a reduced design to estimate the effects of k factors faces several considerations. These include whether the research questions of primary scientific interest concern simple effects or main effects, whether the design's aliasing means that assumptions must be made in order to address the research questions, and how to use aliasing strategically. Each of these considerations is reviewed in this section.

Simple Effects and Main Effects

In this article we have been discussing a situation in which a finite set of k independent variables is under consideration and the individual effects of each of the k variables are of interest. However, the question "Does a particular factor have an effect?" is incomplete; different research questions may involve different types of effects. Let us examine three research questions concerning the effect of Breath in the hypothetical example to see how they correspond to effects in a factorial design.

Question 1: Does the factor Breath have an effect on the outcome variable when the factors Choose and Prep are set to Off?

Question 2: Will an intervention consisting of only the factors Choose and Prep set to On be improved if the factor Breath is changed from Off to On?

Question 3: Does the factor Breath have an effect on the outcome variable on average across levels of the other factors?

In the language of experimental design, Questions 1 and 2 concern simple effects, and Question 3 concerns a main effect. The distinction between simple effects and main effects is subtle but important. A simple effect of a factor is an effect at a particular combination of levels of the remaining factors. There are as many simple effects for each factor as there are combinations of levels of the remaining factors. For example, the simple effect relevant to Question 1 is the conditional effect of changing Breath from Off to On, if we assume that both Prep and Choose are set to Off. The simple effect relevant to Question 2 is the conditional effect of changing Breath from Off to On, if we assume that both other factors are set to On. Thus, although Questions 1 and 2 both are concerned with simple effects of Breath, they are concerned with different simple effects.

A significant main effect for a factor is an effect on average across all combinations of levels of the other factors in the experiment. For example, Question 3 is concerned with the main effect of Breath, that is, the effect of Breath averaged across all combinations of levels of Prep and

Choose. Given a particular set of k factors, there is only one main effect corresponding to each factor.

Simple effects and main effects are not interchangeable, unless we assume that all interactions are negligible. Thus, neither necessarily tells anything about the other. A positive main effect does not imply that all the simple effects are nonzero or even nonnegative. It is even possible (owing to a large interaction) for one simple effect to be positive, another simple effect for the same factor to be negative, and the main (averaged) effect to be zero. In the public speaking example, the answer to Question 2 does not imply anything about whether an intervention consisting of Breath alone would be effective, or whether there would be an incremental effect of Breath if it were added to an intervention initially consisting of Choose alone.

Research Questions, Aliasing, and Assumptions

Suppose an investigator is interested in addressing Question 1. The answer to this research question depends only upon the particular simple effect of Breath when both of the other factors are set to Off. The research question does not ask whether any observed differences are attributable to the main effect of Breath, the Breath \times Prep interaction, the Breath \times Choose interaction, the Breath \times Prep \times Choose interaction, or some combination of the aliased effects. The answer to Question 2, which also concerns a simple effect, depends only upon whether changing Breath from Off to On has an effect on the outcome variable when Prep and Choose are set to On; it does not depend on establishing whether any other effects in the model are present or absent. As Kirk (1995) pointed out, simple effects "represent a partition of a treatment sum of squares plus an interaction sum of squares" (p. 380). Thus, although there is aliasing in the individual-experiments and comparative treatment strategies, these designs are appropriate for addressing Question 1 because the aliased effects correspond exactly to the effect of interest in Question 1. Similarly, although there is aliasing in the constructive treatment strategy, this design is appropriate for addressing Question 2. In other words, although in our view it is important to be aware of aliasing whenever considering a reduced experimental design, the aliasing ultimately is of little consequence if the aliased effect as a package is of primary scientific interest.

The individual-experiments and comparative treatment strategies would not be appropriate for addressing Question 2. The constructive treatment strategy could address Question 1 but only if Breath was the first factor set high, with the others low, in the first noncontrol group. The conclusions drawn from these experiments would be limited to simple effects and cannot be extended to main effects or interactions.

The situation is different if a reduced design is to be used to estimate main effects. Suppose an investigator is inter-

ested in addressing Question 3, that is, in the main effect of Breath. As discussed above, in the individual-experiments, comparative treatment, and constructive treatment approaches, the main effect of Breath is aliased with all the interactions involving Breath. It is appropriate to use these designs to draw conclusions about the main effect of Breath only if it is reasonable to assume that all the interactions involving Breath up to the k -way interaction are negligible. Then any effect of Breath observed by means of an individual-experiments or a single-factor design is attributable to the main effect.

The difference in the aliasing structure of fractional factorial designs compared with individual-experiments and single-factor designs becomes particularly salient when the primary scientific questions that motivate an experiment require estimating main effects as opposed to simple effects and when larger numbers of factors are involved. However, the small three-factor fractional factorial experiment in Table 2 can be used to demonstrate the logic behind the choice of a particular fractional factorial design. In the design in Table 2, the main effect of Breath is aliased with one two-way interaction: Prep \times Choose. If it is reasonable to assume that this two-way interaction is negligible, then it is appropriate to use this fractional factorial design to estimate the main effect of Breath. In general, investigators considering using a fractional factorial design seek a design in which main effects and scientifically important interactions are aliased only with effects that can be assumed to be negligible.

Many fractional factorial designs in which there are four or more factors require far fewer and much weaker assumptions for estimation of main effects than those required by the small hypothetical example used here. For these larger problems, it is possible to identify a fractional factorial design that uses fewer experimental conditions than the complete design but in which main effects and also two-way interaction are aliased only with interactions involving three or more factors. Many of these designs also enable identification of some three-way interactions that are to be aliased only with interactions involving four or more factors. In general, the appeal of fractional factorial designs increases as the number of factors becomes larger. By contrast, individual-experiments and single-factor designs always alias main effects and all interactions from the two-way up to the k -way, no matter how many factors are involved.

Strategic Aliasing and Designating Negligible Effects

A useful starting point for choosing a reduced design is sorting all the effects in the complete factorial into three categories: (a) effects that are of primary scientific interest and therefore are to be estimated, (b) effects that are expected to be zero or negligible, and (c) effects that are not

of primary scientific interest but may be nonnegligible. Strategic aliasing involves ensuring that effects of primary scientific interest are aliased only with negligible effects. There may be nonnegligible effects that are not of scientific interest. Resources are not to be devoted to estimating such effects, but care must be taken not to alias them with effects of primary scientific interest.

Considering which, if any, effects to place in the negligible category is likely to be an unfamiliar, and perhaps in some instances uncomfortable, process for some social and behavioral scientists. However, the choice is critically important. On the one hand, when more effects are designated negligible, the available options will in general include designs involving smaller numbers of experimental conditions; on the other hand, incorrectly designating effects as negligible can threaten the validity of scientific conclusions. The best bases for making assumptions about negligible effects are theory and prior empirical research. Yet there are few areas in the social and behavioral sciences in which theory makes specific predictions about higher order interactions, and it appears that to date there has been relatively little empirical investigation of such interactions. Given this lack of guidance, on what basis can an investigator decide on assumptions?

A very cautious approach would be to assume that each interaction up to the k -way interaction is likely to be sizable, unless there is empirical evidence or a compelling theoretical basis for assuming that it is negligible. This is equivalent to leaving the negligible category empty and designating each effect either of primary scientific interest or nonnegligible. There are two strategies consistent with this perspective. One is to conduct a complete factorial experiment, being careful to ensure adequate statistical power to detect any interactions of scientific interest. The other strategy consistent with assuming that all interactions are likely to be sizable is to frame research questions only about simple effects that can reasonably be estimated with the individual-experiments or single-factor approaches. For example, as discussed above, the aliasing associated with the comparative treatment design may not be an issue if research questions are framed in terms of simple effects.

If these cautious strategies seem too restrictive, another possibility is to adopt some heuristic guiding principles (see Wu & Hamada, 2000) that are used in engineering research for informing the choice of assumptions and aliasing structure and to help target resources in areas in which they are likely to result in the most scientific progress. The guiding principles are intended for use when theory and prior research are unavailable; if guidance from these sources is available, it should always be applied first. One guiding principle is called hierarchical ordering. This principle states that when resources are limited, the first priority should be estimation of lower order effects. Thus, main effects are the first investigative priority, followed by two-

way interactions. As Green and Rao (1971) noted, “in many instances the simpler (additive) model represents a very good approximation of reality” (p. 359), particularly if measurement quality is good and floor and ceiling effects can be avoided. Another guiding principle is called effect sparsity (Box & Meyer, 1986), or sometimes the Pareto principle in experimental design (Wu & Hamada, 2000). This principle states that the number of sizable and important effects in a factorial experiment is small in comparison with the overall number of effects. Taken together, these principles suggest that unless theory and prior research specifically suggest otherwise, there are likely to be relatively few sizable interactions except for a few two-way interactions and even fewer three-way interactions, and that aliasing the more complex and less interpretable higher order interactions may well be a good choice.

Resolution of Fractional Factorial Designs

Some general information about aliasing of main effects and two-way interactions is conveyed in a fractional factorial design’s resolution (Wu & Hamada, 2000). Resolution is designated by a Roman numeral, usually either III, IV, V, or VI. The aliasing of main effects and two-way interactions in these designs is shown in Table 3. As Table 3 shows, as design resolution increases, main effects and two-way interactions become increasingly free of aliasing with lower order interactions. Importantly, no design that is Resolution III or higher aliases main effects with other main effects.

Table 3 shows only which effects are not aliased with main effects and two-way interactions. Which and how many effects are aliased with main effects and two-way interactions depends on the exact design. For example, consider a 2^{6-2} fractional factorial design. As mentioned previously, this is a one-quarter fraction design, so each source of variance has four aliases; thus, each main effect is aliased with three other effects. Suppose this design is Resolution IV. Then none of the three effects aliased with the main effect will be another main effect or a two-way interaction. Instead, they will be three higher order interactions.

According to the hierarchical ordering and effect sparsity principles, in the absence of theory or evidence to the

contrary, it is reasonable to make the working assumption that higher order interactions are less likely to be sizable than lower order interactions. Thus, all else being equal, higher resolution designs, which alias scientifically important main effects and two-way interactions with higher order interactions, are preferred to lower resolution designs, which alias these effects with lower order interactions or with main effects. This concept has been called the maximum resolution criterion by Box and Hunter (1961a, 1961b).

In general, higher resolution designs tend to require more experimental conditions, although for a given number of experimental conditions there may be design alternatives with different resolutions.

Relative Resource Requirements of the Four Design Alternatives

Number of Experimental Conditions and Subjects Required

The four design options considered here can vary widely with respect to the number of experimental conditions that must be implemented and the number of subjects required to achieve a given statistical power. These two resource requirements must be considered separately. In single-factor experiments, the number of subjects required to perform the experiment is directly proportional to the number of experimental conditions to be implemented. However, when comparing different designs in a multifactor framework, this is not the case. For instance, a complete factorial may require many more experimental conditions than the corresponding individual-experiments or single-factor approach, yet require fewer total subjects.

Table 4 shows how to compute a comparison of the number of experimental conditions required by each of the four design alternatives. As Table 4 indicates, the individual-experiments, single-factor, and fractional factorial approaches are more economical than the complete factorial approach in terms of number of experimental conditions that must be implemented. In general, the single-factor approach requires the fewest experimental conditions.

Table 3
Resolution of Fractional Factorial Designs and Aliasing of Effects

Design resolution	Main effects not aliased with	Two-way interactions not aliased with
Resolution III	main effects	
Resolution IV	main effects and two-way interactions	main effects
Resolution V	main effects, two-way interactions, and three-way interactions	main effects and two-way interactions
Resolution VI	main effects, two-way interactions, three-way interactions, and four-way interactions	main effects, two-way interactions, and three-way interactions

Table 4
Aliasing and Economy of Four Design Approaches With k Two-Level Independent Variables

Design	Number experimental conditions	Number subjects
Complete factorial	2^k	N^a
Individual experiments	$2k$	kN
Single factor	$k + 1$	$(k + 1)\frac{N}{2}$
Fractional factorial	2^{k-1} or fewer	N

^a N = total sample size required to maintain desired level of power in complete factorial design.

Table 4 also provides a comparison of the minimum number of subjects required to maintain the same level of statistical power. Suppose a total of k factors are to be investigated, with the smallest effect size among them equal to d , and that a total minimum sample size of N is required to maintain a desired level of statistical power at a particular Type I error rate. The effect size d might be the expected normalized difference between two means, or it might be the smallest normalized difference considered clinically or practically significant. (Note that in practice there must be at least one subject per experimental condition, so at a minimum N must at least equal the number of experimental conditions. This may require additional subjects beyond the number needed to achieve a given level of power when implementing complete factorial designs with large k .) Table 4 shows that the complete factorial and fractional factorial designs are most economical in terms of sample size requirements. In any balanced factorial design, each main effect is estimated with all subjects, averaging across the other main effects. In the hypothetical three-factor example, the main effects of Choose, Breath, and Prep are each based on all N subjects, with the subjects sorted differently into treatment and control groups for each main effect estimate. For example, Table 2 shows that in both the complete and fractional factorial designs, a subject assigned to Experimental Condition 3 is in the Off group for the purpose of estimating the main effects of Choose and Prep but in the On group for the purpose of estimating the main effect of Breath.

Essentially, factorial designs recycle subjects by placing every subject in one of the levels of every factor. As long as the sample sizes in each group are balanced, orthogonality is maintained, so that estimation and testing for each effect can be treated as independent of the other effects. (The idea of balance here assumes that each level of each factor is assigned the same amount of subjects, which may not hold true in practice; however, the benefits associated with balance hold approximately even if there are slight imbalances in the number of subjects per experimental condition.) Because they recycle subjects while keeping factors mutually

orthogonal to one another, balanced factorial designs make very efficient use of experimental subjects. In fact, this means that an increase in the number of factors in a factorial experiment does not necessarily require an increase in the total sample size to maintain approximately the same statistical power for testing main effects. This efficiency applies only to main effects, though. For example, given a fixed sample size N , the more experimental conditions there are, the fewer subjects there will be in each experimental condition and the less power there will be for, say, pairwise comparisons of particular experimental conditions.

By contrast, the individual-experiments approach sometimes requires many more subjects than the complete factorial experiment to obtain a given level of statistical power, because it cannot reuse subjects to test different orthogonal effect estimates simultaneously as balanced factorial experiments can. As Table 4 shows, if a factorial experiment with k factors requires an overall sample size of N to achieve a desired level of statistical power for detecting a main effect of size d at a particular Type I error rate, the comparable individual-experiments approach requires kN subjects to detect a simple effect of the same size at the same Type I error rate. This is because the first experiment requires N subjects, the second experiment requires another N subjects, and so on, for a total of kN . In other words, in the individual-experiments approach, subjects are used in a single experiment to estimate a single effect and then discarded. The extra subjects provide neither increased Type I error protection nor appreciably increased power, relative to the test of a simple effect in the single-factor approach or the test of a main effect in the factorial approach. Unless there is a special need to obtain results from one experiment before beginning another, the extra subjects are largely wasted resources.

As Table 4 shows, if a factorial experiment with k factors requires an overall sample size of N to achieve a desired level of statistical power to detect a main effect of size d at a particular Type I error rate, the comparable single-factor approach requires a sample size of $(k + 1)(N/2)$ to detect a simple effect of the same size at the same Type I error rate. This is because in the single-factor approach, to maintain power each mean comparison must be based on two experimental conditions including a total of N subjects. Thus $N/2$ subjects per experimental condition would be required. However, this single-factor experiment would be adequately powered for k simple effects, whereas the comparable factorial experiment with N subjects, although adequately powered for k main effects, would be underpowered for k simple effects. This is because estimating a simple effect in a factorial experiment essentially requires selecting a subset of experimental conditions and discarding the remaining conditions along with the subjects that have been assigned to them. This would bring the sample size considerably below N for each simple effect.

Subject, Condition, and Overall Costs

To compare the resource requirements of the four design alternatives, it is helpful to draw a distinction between per-subject costs and per-condition overhead costs. Examples of subject costs are recruitment and compensation of human subjects and housing, feeding, and care of laboratory animals. Condition overhead costs refer to costs required to plan, implement, and manage each experimental condition in a design, beyond the cost of the subjects assigned to that condition. Examples of condition overhead costs are training and salaries of personnel to run an experiment, preparation of differing versions of materials needed for different experimental conditions, and cost of setting up and taking down laboratory equipment. Thus, the overhead cost associated with an experimental condition may be either more or less than the cost of a subject. Because the absolute and relative costs in these two domains vary considerably according to the situation, the absolute and relative costs associated with the four designs considered here can vary considerably as well.

A possible scenario is one in which both per-condition overhead costs and per-subject costs are low. For example, consider a social psychology experiment in which experimental conditions consist of different written materials, the experimenters are graduate students on stipends, and a large departmental subject pool is at their disposal. This represents the happy circumstance in which a design can be chosen on purely scientific grounds with little regard to financial costs. Another possible scenario is one in which per-condition overhead costs are low but per-subject costs are high, as might occur if an experiment is to be conducted via the Internet. In this study perhaps adding an experimental condition is a fairly straightforward computer programming task, but substantial cash incentives are required to ensure subject participation. Another example might be an

experiment in which individual experimental conditions are not difficult to set up, but the subjects are laboratory animals whose purchase, feeding, and care are very costly. Per-condition costs might roughly equal per-subject costs in a similar scenario in which each experimental condition involves time-intensive and complicated reconfiguring of laboratory equipment by a highly paid technician. Per-condition overhead costs might greatly exceed per-subject costs when subjects are drawn from a subject pool and are not monetarily compensated, but each new experimental condition requires additional training of personnel, preparation of elaborate new materials, or difficult reconfiguration of laboratory equipment.

Comparing Relative Estimated Overall Costs Across Designs

In this section we demonstrate a comparison of relative financial costs across the four design alternatives, based on the expressions in Table 4. In the demonstration we consider four situations: effect sizes of $d = .2$ and $d = .5$ (corresponding to Cohen's, 1988, benchmark values for small and medium, respectively) and $k = 6$ and $k = 10$ two-level independent variables. The starting point for the cost comparison is the number of experimental conditions required by each design, and the sample sizes required to achieve statistical power of at least .8 for testing the effect of each factor in the way that seemed appropriate for the design. Specifically, for the full and fractional factorial designs, we calculated the total sample size N needed to have a power of .8 for each main effect. For the individual-experiments and single-factor designs, we calculated the N needed for a power of .8 for each simple effect of interest. These are shown in Table 5. As the table indicates, the fractional factorial designs used for $k = 6$ and $k = 10$ are both Resolution IV.

Table 5
Number of Experimental Conditions and Sample Size Used to Maintain Per-Factor Power $\geq .8$ for Designs in Figures 1 and 2

Design	Number of experimental conditions	$d = .2$		$d = .5$	
		n per experimental condition	Overall N	n per experimental condition	Overall N
$k = 6$					
Complete factorial	64	13	832 ^a	2	128
Individual experiments	12	394	4,728	64	768
Single factor	7	394	2,758	64	448
Fractional factorial Resolution IV (2^{6-2})	16	50	800 ^a	8	128
$k = 10$					
Complete factorial	1,024	1	1,024 ^a	1	1,024 ^a
Individual experiments	20	394	7,880	64	1,280
Single factor	11	394	4,334	64	704
Fractional factorial Resolution IV (2^{10-5})	32	25	800 ^a	4	128

^a To achieve integer n per experimental condition, N is larger than the minimum needed to achieve sufficient power.

A practical issue arose that influenced the selection of the overall sample sizes N that are listed in Table 5. Let N_{\min} designate the minimum overall N required to achieve a desired level of statistical power. In some cases (as footnoted) the overall N that was actually used exceeds N_{\min} because experimental conditions cannot have fractional numbers of subjects. Let n designate the number of subjects in each experimental condition, assuming equal n s are to be assigned to each experimental condition. In theory the minimum n per experimental condition for a particular design would be N_{\min} divided by the number of experimental conditions. However, in some of the cases in Table 5, this would have resulted in a noninteger n . In these cases the per-condition n was rounded up to the nearest integer. For example, consider the complete factorial design with $k = 10$ factors and $d = .2$. In theory a per-factor power of $\geq .8$ would be maintained with $N_{\min} = 788$. However, the complete factorial design required 1,024 experimental conditions, so the minimum N that could be used was 1,024. All cost comparisons reported here are based on the overall N listed in Table 5.

For purposes of illustration, per-subject cost is defined here as the average incremental cost of adding a single research subject to a design without increasing the number of experimental conditions, and condition overhead cost is defined as the average incremental cost of adding a single experimental condition without increasing the number of subjects. (For simplicity we assume per-subject costs do not differ dramatically across conditions.) Then a rough estimate of total costs can be computed as follows, providing a basis for comparing the four design alternatives:

$$\begin{aligned} \text{total cost} = & (\text{total sample size} \times \text{per-subject cost}) \\ & + (\text{number of experimental conditions} \\ & \times \text{per-condition overhead cost}). \end{aligned}$$

Figure 1 illustrates total costs for experiments corresponding to the situations and designs in Table 5, for experiments in which per-subject costs equal or exceed per-condition overhead costs. To compute total costs on the y-axis, we arbitrarily fixed per-condition costs at \$1. Thus, the x-axis

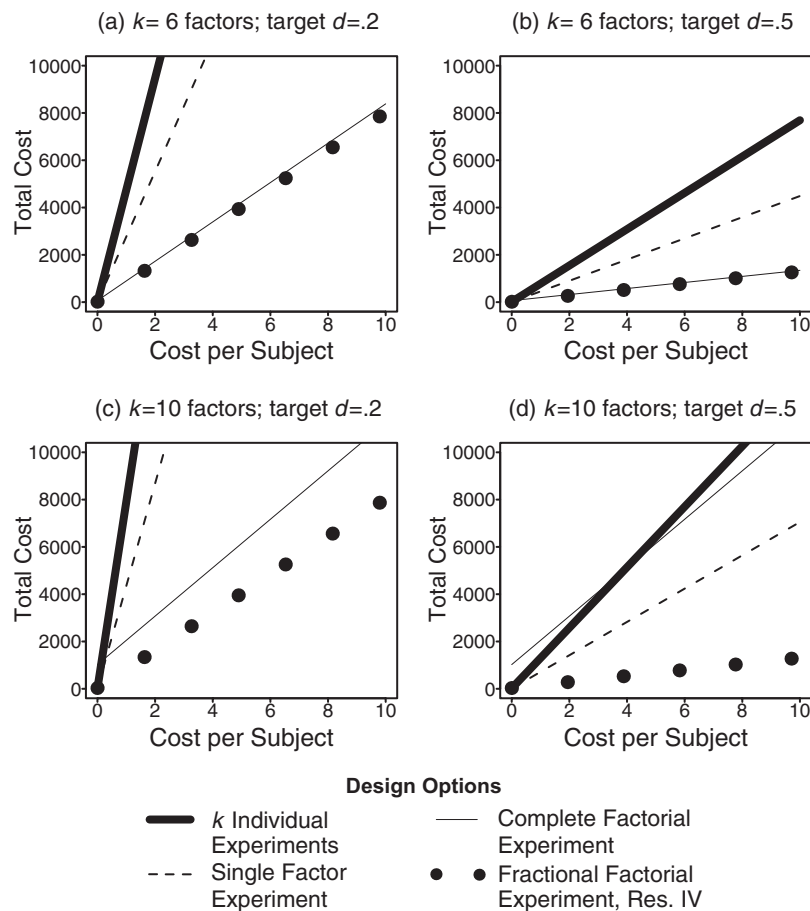


Figure 1. Costs of different experimental design options when per-subject costs exceed per-condition overhead costs. Total costs are computed with per-condition costs fixed at \$1. Res = Resolution.

can be interpreted as the ratio of per-subject costs to per-condition costs. For example, 4 on the x -axis means that per-subject costs are 4 times per-condition costs.

In the situations considered in Figure 1, fractional factorial designs were always either least expensive or tied with complete factorial designs for least expensive. As the ratio of per-subject costs to per-condition costs increased, the economy of complete and fractional factorial designs became increasingly evident. Figure 1 shows that when per-subject costs outweighed per-condition costs, the single-factor approach and, in particular, the individual-experiments approach were often much more expensive than even complete factorial designs, and fractional factorials were often the least expensive.

Figure 2 examines the same situations as in Figure 1, but now total costs are shown on the y -axis for experiments in which per-condition overhead costs equal or exceed per-subject costs. To compute total costs, we arbitrarily fixed per-subject costs at \$1. Thus, the x -axis represents the ratio of per-condition costs to per-subject costs; in this figure 40

on the x -axis means that per-condition costs are 40 times per-subject costs. The picture here is more complex than that in Figure 1. For the most part, in the four situations considered here, the complete factorial was the most expensive design, frequently by a wide margin. The complete factorial requires many more experimental conditions than any of the other design alternatives, so it is not surprising that it was expensive when condition costs were relatively high. It is perhaps more surprising that the individual-experiments approach, although it requires far fewer experimental conditions than the complete factorial, was usually the next most expensive. The individual-experiments approach even exceeded the cost of the complete factorial under some circumstances when the effect sizes were small. This is because the reduction in experimental conditions afforded by the individual-experiments approach was outweighed by much greater subject requirements (see Table 4). Figure 2 shows that the least expensive approaches were usually the single-factor and fractional factorial designs. Which of these two was less expensive depended on effect

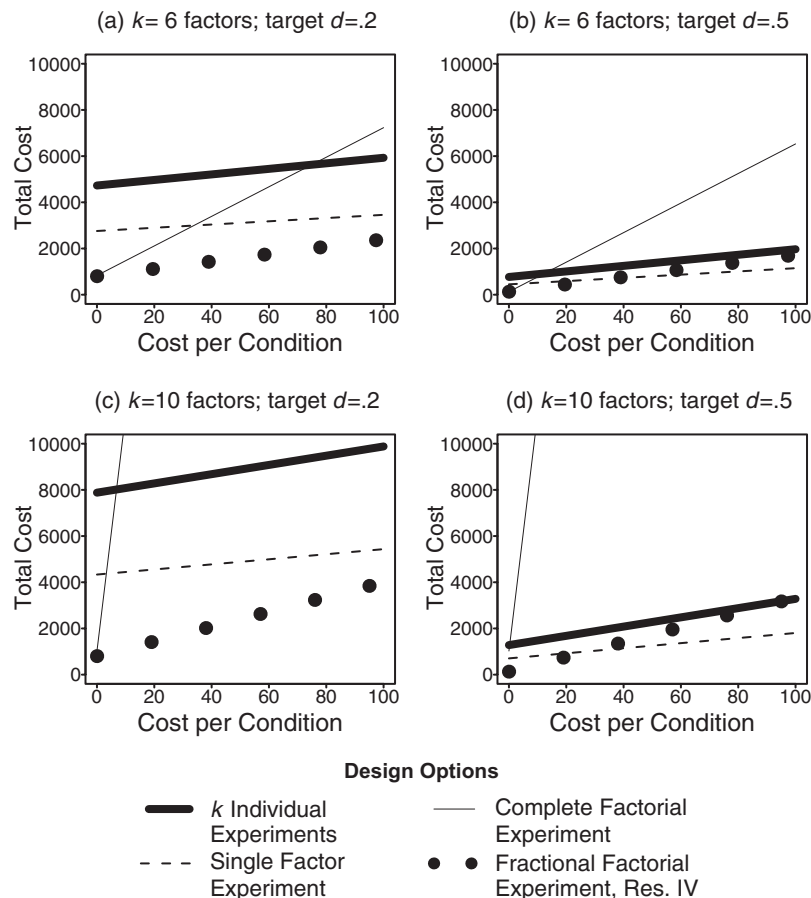


Figure 2. Costs of different experimental design options when per-condition overhead costs exceed per-subject costs. Total costs are computed with per-subject costs fixed at \$1. Res = Resolution.

size and the ratio of per-condition costs to per-subject costs. When the effect sizes were large and the ratio of per-condition costs to per-subject costs was less than about 20, fractional factorial designs tended to be more economical; the single-factor approach was most economical once per-condition costs exceeded about 20 times per-subject costs. However, when effect sizes were small, fractional factorial designs were cheaper until the ratio of per-condition costs to per-subject costs substantially exceeded 100.

A Brief Tutorial on Selecting a Fractional Factorial Design

In this section we provide a brief tutorial intended to familiarize investigators with the basics of choosing a fractional factorial design. The more advanced introduction to fractional factorial designs provided by Kirk (1995) and Kuehl (1999) and the detailed treatment in Wu and Hamada (2000) are excellent resources for further reading.

When the individual-experiments and single-factor approaches are used, typically the choice of experimental conditions is made on intuitive grounds, with aliasing per se seldom an explicit basis for choosing a design. By contrast, when fractional factorial designs are used, aliasing is given primary consideration. Usually, the investigator selects a design to achieve a particular aliasing structure while considering cost. Although the choice of experimental conditions for fractional factorials may be less intuitively obvious, this should not be interpreted as meaning that the selection of a fractional factorial design has no conceptual basis. On the contrary, fractional factorial designs are carefully chosen with key research questions in mind.

There are many possible fractional factorial designs for any set of k factors. The designs vary in how many experimental conditions they require and the nature of the aliasing. Fortunately, the hard work of determining the number of experimental conditions and aliasing structure of fractional factorial designs has largely been done. The designs can be found in books (e.g., Box et al., 1978; Wu & Hamada, 2000) and on the Internet (e.g., National Institute of Standards and Technology/SEMATECH, 2006), but the easiest way to choose a fractional factorial design is with computer software. Here we demonstrate the use of PROC FACTEX (SAS Institute, 2004a). Using this approach, the investigator specifies the factors in the experiment, and may specify which effects are in the estimate, negligible, and nonnegligible categories; the desired design resolution; the maximum number of experimental conditions (sometimes called runs); and other aspects relevant to choice of a design. The software returns a design that meets the specified criteria, or indicates that such a design does not exist. Minitab (see Mathews, 2005; Ryan, Joiner, & Cryer, 2004) and S-PLUS (Insightful Corporation, 2007) can also be used for designing fractional factorial experiments.

To facilitate the presentation, let us increase the size of the hypothetical example. In addition to the factors (a) Choose, (b) Breath, and (c) Prep, the new six-factor example will include factors corresponding to whether or not (d) an audience is present besides the investigator (Audience), (e) the subject is promised a monetary reward if the speech is judged good enough (Stakes), and (f) the subject is allowed to speak from notes (Notes). A complete factorial experiment would require $2^6 = 64$ experimental conditions. Three ways of choosing a fractional factorial design with PROC FACTEX are illustrated below.

Specifying a Desired Resolution

One way to use software to choose a fractional factorial design is to specify a desired resolution and instruct the software to find the smallest number of experimental conditions needed to achieve it. For example, suppose the investigator in the hypothetical example finds it acceptable to alias main effects with interactions as low as three-way and to alias two-way interactions with other two-way interactions and higher order interactions. A design of Resolution IV will meet these criteria and may be requested as follows:

```
PROC FACTEX;
  FACTORS breath audience choose prep notes stakes;
  SIZE DESIGN = MINIMUM;
  MODEL RESOLUTION = 4;
  EXAMINE ALIASING(6) DESIGN;
  OUTPUT OUT = dataset1;
  RUN;
```

PROC FACTEX will find a design with these characteristics if it can, print information on the aliasing and design matrix, and save the design matrix in the data set dataset1. The ALIASING(6) command requests a list of all aliasing up to six-way interactions, and DESIGN asks for the effect codes for each experimental condition in the design to be printed.

Table 6 shows the effect codes from the PROC FACTEX output for this design. The design found by PROC FACTEX requires only 16 experimental conditions; that is, the design is a 2^{6-2} , or a one-quarter fractional factorial, because it requires only $2^{-2} = 1/4 = 16/64$ of the experimental conditions in the full experiment. In a one-quarter fraction each source of variance has four aliases. This means that each main effect is aliased with three other effects. Because this is a Resolution IV design, all these other effects are three-way interactions or any higher order interactions; they will not be main effects or two-way interactions. Similarly, each two-way interaction is aliased with three other effects. Because this is a Resolution IV design, these other effects may be any interactions.

Different fractional factorial designs, even those with the same resolution, have different aliasing structures, some of which may appeal more to an investigator than others. PROC FACTEX simply returns the first one it can find that

Table 6
An Effect-Coded Design Matrix for a 2^{6-2}
Fractional Factorial Experiment

Condition	Breath	Audience	Choose	Prep	Notes	Stakes
1	-1	-1	-1	-1	-1	-1
2	-1	-1	-1	1	1	1
3	-1	-1	1	-1	1	1
4	-1	-1	1	1	-1	-1
5	-1	1	-1	-1	1	-1
6	-1	1	-1	1	-1	1
7	-1	1	1	-1	-1	1
8	-1	1	1	1	1	-1
9	1	-1	-1	-1	-1	1
10	1	-1	-1	1	1	-1
11	1	-1	1	-1	1	-1
12	1	-1	1	1	-1	1
13	1	1	-1	-1	1	1
14	1	1	-1	1	-1	-1
15	1	1	1	-1	-1	-1
16	1	1	1	1	1	1

fits the desired specifications. There is no feature in PROC FACTEX, to the best of our knowledge, that automatically returns multiple possible designs with the same resolution, but it is possible to see different designs by arbitrarily changing the order in which the factors are listed in the FACTORS statement. Another possibility is to use the MINABS option to request a design that meets the minimum aberration criterion, which is a mathematical definition of least aliased (see Wu & Hamada, 2000).

Specifying Which Effects Are in Which Categories

The above methods of identifying a suitable fractional factorial design did not require specification of which effects are of primary scientific interest, which are negligible, and which are nonnegligible, although the investigator would have to had to determine this to decide that a Resolution IV design was desired. Another way to identify a fractional factorial design is to specify directly which effects fall in each of these categories and instruct the software to find the smallest design that does not alias effects of primary interest either with one another or with effects in the nonnegligible category. This method enables a little more fine-tuning.

Suppose in addition to the main effects, the investigator wants to be able to estimate all two-way interactions involving Breath. The remaining two-way interactions and all three-way interactions are not of scientific interest but may be sizable, so they are designated nonnegligible. In addition, one four-way interaction, Breath \times Prep \times Notes \times Stakes, might be sizable because those factors are suspected in advance to be the most powerful factors, and so their combination might lead to a floor or ceiling effect, which

could act as an interaction. This four-way interaction is placed in the nonnegligible category. All remaining effects are designated negligible. Given these specifications, a design with the smallest possible number of experimental conditions is desired. The following code will produce such a design:

```
PROC FACTEX;
FACTORS breath audience choose prep notes stakes;
SIZE DESIGN = MINIMUM;
MODEL ESTIMATE = (breath audience choose prep notes
  stakes breath*audience breath*choose breath*prep breath*
  notes breath*stakes)
NONNEGLEGIBLE = (breath | audience | choose | prep | notes |
  stakes @ 3 breath*prep*notes*stakes);
*ABOVE SPECIFIES ALL 3-WAY INTERACTIONS AND
  BELOW. IF EFFECTS INCLUDED IN;
*BOTH ESTIMATE AND NONNEGLEGIBLE, ESTIMATE
  CATEGORY TAKES PRECEDENCE;
EXAMINE ALIASING(6) DESIGN;
OUTPUT OUT = dataset2;
RUN;
```

The ESTIMATE statement designates the effects that are of primary scientific interest and must be aliased only with effects expected to be negligible. The NONNEGLEGIBLE statement designates effects that are not of scientific interest but may be sizable; these effects must not be aliased with effects mentioned in the ESTIMATE statement. It is necessary to specify only effects to be estimated and those designated nonnegligible; any remaining effects are assumed negligible.

The PROC FACTEX output (not shown) indicates that the result is a 2^{6-1} design, which has 32 experimental conditions, and that this design is Resolution VI. Because this design is a one-half fraction of the complete factorial, each source of variation has two aliases, or, in other words, each main effect and interaction is aliased with one other effect. The output provides a complete account of the aliasing, indicating that each main effect is aliased with a five-way interaction and each two-way interaction is aliased with a four-way interaction. This aliasing is characteristic of Resolution VI designs, as was shown in Table 3. Because the four-way interaction Breath \times Prep \times Notes \times Stakes has been placed in the nonnegligible category, the design aliases it with another interaction in this category, Audience \times Choose, rather than with one of the two-way interactions in the estimate category.

Specifying the Maximum Number of Experimental Conditions

Another way to use software to choose a design is to specify the number of experimental conditions in the design and let the software return the aliasing structure. This approach may make sense when resource constraints impose a strict upper limit on the number of experimental conditions that can be implemented, and the investigator wishes to

decide whether key research questions can be addressed within this limit. Suppose in our hypothetical example the investigator can implement no more than eight experimental conditions; that is, we need a 2^{6-3} design. The investigator can use the following code:

```
PROC FACTEX;
FACTORS breath audience choose prep notes stakes;
SIZE DESIGN = 8;
*THIS SPECIFIES A DESIGN WITH 8 CONDITIONS;
MODEL RESOLUTION = MAXIMUM;
*THIS SPECIFIES A DESIGN WITH HIGHEST RESOLUTION
  GIVEN THE OTHER SPECIFICATIONS;
EXAMINE ALIASING(6) DESIGN;
OUTPUT OUT = dataset3;
RUN;
```

In this case, the PROC FACTEX output suggests a design with Resolution III. Because this Resolution III design is a one-eighth fraction, each source of variance has eight aliases. Each main effect is aliased with seven other effects. These effects may be any interaction; they will not be main effects.

A Comparison of Results for Several Experiments

This section contains direct comparisons among the various experimental designs discussed in this article, based on artificial data generated with the same model for all the designs. This can be imagined as a situation in which after each experiment time is turned back and the same factors are again investigated with the same experimental subjects but with a different experimental design.

Method

Let us return to the hypothetical example with six factors (Breath, Audience, Choose, Prep, Notes, Stakes), each with two levels per factor, coded -1 for Off and $+1$ for On. Suppose there are a total of 320 subjects, with 5 subjects randomly assigned to each of the 64 experimental conditions of a 2^6 full factorial design, and the outcome variable is a reverse-scaled questionnaire about public speaking anxiety, that is, a higher score indicates less anxiety. We generated data so that the score of participant j in the i th experimental condition was modeled as $\mu_i + \epsilon_{ij}$, where the μ_i are given by

$$\begin{aligned} \mu = & 5.00 + .25*\text{Breath} + .50*\text{Choose} + .30*\text{Prep} \\ & + .30*\text{Notes} - .10*\text{Stakes} + .25*\text{Breath}*\text{Prep} \\ & + .20*\text{Prep}*\text{Notes} + .25*\text{Prep}*\text{Stakes} \\ & - .15*\text{Breath}*\text{Notes} - .15*\text{Breath}*\text{Choose} \\ & - .05*\text{Choose}*\text{Stakes}*\text{Notes} \\ & + .05*\text{Stakes}*\text{Audience}*\text{Choose} \\ & + .05*\text{Choose}*\text{Prep}*\text{Stakes}*\text{Notes} \end{aligned} \quad (1)$$

and the errors are $N(0, 2^2)$. Because the outcome variable in Equation 1 is reverse scored, helpful (anxiety-reducing) main effects can be called positive and harmful ones negative. The standard deviation of 2 was used so that the regression coefficients above could also be interpreted as Cohen's d s despite the $-1/+1$ metric for effect coding. Thus, the main effects coefficients in Equation 1 represent half the long-run average raw difference between participants receiving the Off and On levels of the factor and also represent the normalized difference between the -1 and $+1$ groups.

The example was deliberately set up so as not to be completely consistent with the investigator's ideas as expressed in the previous section. In the model above, anxiety is reduced on average by doing the breathing relaxation exercise, by being able to choose one's own topic, by having extra preparation time, and by having notes available. There is a small anxiety-increasing effect of higher stakes. The Audience factor had zero main effect on anxiety. The first two positive two-way interactions indicate that longer preparation time intensified the effects of the breathing exercise or notes or, equivalently, that shorter preparation time largely neutralized their effects (as the subjects had little time to put them into practice). The third interaction indicates that higher stakes were energizing for those who were prepared but anxiety-provoking for the less prepared. The first pair of negative two-way interactions indicate that the Breath intervention was somewhat redundant with the more conventional aids of having notes and having one's choice of topic or, equivalently, that breathing relaxation was more important when those aids were not available. There follow several other small higher order nuisance interactions with no clear interpretability, as might occur in practice.

We generated data using the above model for the following seven experimental designs: complete factorial, individual experiments, two single factor (comparative treatment and constructive treatment), and the Resolution III, IV, and VI fractional factorial designs arrived at in the previous section. The total number of subjects used was held constant at 320 for all the designs. For the individual-experiments approach, we simulated six experiments, each with either 53 or 54 subjects. For the single-factor designs, we assigned either 45 or 46 subjects to each of seven experimental conditions. The comparative treatment design included a no-treatment control (i.e., all factors set to Off) and six experimental conditions, each with one factor set to On and the others set to Off. The constructive treatment design included a no-treatment control and six experimental conditions, each of which added a factor set to On in order from left to right. For example, in the first treatment condition only Breath was set to On, in the second treatment condition Breath and Audience were set to On and the remaining factors were set to Off, and so on, until in the seventh experimental condition all six factors were set to On. To

simulate data for the Resolution III, IV, and VI fractional factorial designs, we assigned 40, 20, and 10 subjects, respectively, to each experimental condition. In simulating data for each of the seven design alternatives, we recalculated the μ_i s accordingly but left the vector of ϵ s the same.

Results

Analysis of variance models were fit to each data set in the usual way with SAS PROC GLM (SAS Institute, 2004b). For example, the code used to fit an analysis of variance model to the data set corresponding to the Resolution III fractional factorial design was as follows:

```
PROC GLM DATA = res3;
MODEL y = breath audience choose prep notes stakes;
RUN;
```

This model contained no interactions because they cannot be estimated in a Resolution III design. An abbreviated version of the PROC GLM output corresponding to this code appears in Figure 3. In the comparative treatment strategy, each of the treatment conditions was compared with the no-treatment control. In the constructive treatment strategy, each treatment condition was compared with the condition with one fewer factor set to On. For example, the condition in which Breath and Audience were set to On was compared with the condition in which only Breath was set to On.

Table 7 contains the regression coefficients corresponding to the effects of each factor for each of the seven

designs. For reference, the true values of the regression coefficients used in data generation are shown at the bottom of the table.

In the complete factorial experiment, Breath, Choose, Prep, and Notes were significant. The true main effect of Stakes was small; with $N = 320$ this design had little power to detect it. Audience was marginally significant at $\alpha = .15$, although the data were generated with this effect set at exactly zero. In the individual-experiments approach, only Choose was significant, and Breath was marginally significant. The results for the comparative treatment experiment were similar to those of the individual-experiments approach, as would be expected given that the two have identical aliasing. An additional effect was marginally significant in the comparative treatment approach, reflecting the additional statistical power associated with this design compared with the individual-experiments approach. In the constructive treatment experiment, none of the factors were significant at $\alpha = .05$. There were two marginally significant effects, Breath and Notes.

In the Resolution III design, every effect except Prep was significant. One of these, the significant effect of Audience, was a spurious result (probably caused by aliasing with the Prepare \times Stakes interaction). By contrast, results of the Resolution IV and VI designs were very similar to those of the complete factorial, except that in the Resolution VI design Stakes was significant. In the individual-experiments and single-factor approaches, the estimates of the coefficients varied considerably from the true values. In the fractional factorial designs, the estimates of the coefficients tended to be closer to the true values, particularly in the Resolution IV and Resolution VI designs.

Table 8 shows estimates of interactions from the designs that enable such estimates, namely the complete factorial design and the Resolution IV and Resolution VI factorial designs. The Breath \times Prep interaction was significant in all three designs. The Breath \times Choose interaction was significant in the complete factorial and the Resolution VI fractional factorial but was estimated as zero in the Resolution IV design. In general, the coefficients for these interactions were very similar across the three designs. An exception was the coefficient for the Breath \times Choose interaction and, to a lesser degree, the coefficient for the Breath \times Notes interaction.

Discussion

Differences observed among the designs in estimates of coefficients are due to differences in aliasing plus a minor random disturbance owing to reallocating the error terms when each new experiment was simulated, as described above. In general, more aliasing was associated with greater deviations from the true coefficient values. No effects were aliased in the complete factorial design, which had coefficient estimates closest to the true values. In the Resolution

The GLM Procedure				
Dependent Variable: y				
Source	DF	Type III SS	Mean Square	
notes	1	131.7856809	131.7856809	
stakes	1	39.5901055	39.5901055	
Source		F Value	Pr > F	
breath		14.39	0.0002	
audience		14.01	0.0002	
choose		35.14	<.0001	
prep		1.73	0.1892	
notes		30.78	<.0001	
stakes		9.25	0.0026	
Parameter	Estimate	Standard Error	t Value	Pr > t
Intercept	5.072232020	0.11566548	43.85	<.0001
breath	0.438787150	0.11566548	3.79	0.0002
audience	0.432918822	0.11566548	3.74	0.0002
choose	0.685689571	0.11566548	5.93	<.0001
prep	-0.152182959	0.11566548	-1.32	0.1892
notes	0.641740020	0.11566548	5.55	<.0001
stakes	-0.351737231	0.11566548	-3.04	0.0026

Figure 3. Partial output from SAS PROC GLM for simulated Resolution III data set.

Table 7

Coefficient Estimates for Main Effects Under Different Designs in the Simulated Example, With Total Sample Size $N = 320$

Design	Breath	Audience	Choose	Prep	Notes	Stakes
Complete factorial	0.24 ^a	0.18 ^b	0.69 ^c	0.40 ^c	0.51 ^c	-0.07
Individual experiments	0.51 ^d	0.16	1.14 ^c	-0.39	0.10	-0.39
Single factor						
Comparative treatment	0.32 ^b	0.26	0.79 ^c	-0.32 ^b	0.31	-0.19
Constructive treatment	0.32 ^b	0.24	0.18	0.09	0.38 ^d	0.25
Fractional factorial						
Resolution III	0.44 ^c	0.43 ^c	0.69 ^c	-0.15	0.64 ^c	-0.35 ^c
Resolution IV	0.24 ^a	0.18 ^b	0.69 ^c	0.40 ^c	0.42 ^c	-0.05
Resolution VI	0.24 ^a	0.18 ^b	0.69 ^c	0.40 ^c	0.51 ^c	-0.24 ^a
Population main effect (see Equation 1)	0.25	0.00	0.50	0.30	0.30	-0.10

^a Coefficient significant at $\alpha = .05$. ^b Coefficient significant at $\alpha = .15$. ^c Coefficient significant at $\alpha = .001$. ^d Coefficient significant at $\alpha = .10$. ^e Coefficient significant at $\alpha = .01$.

IV design, each effect was aliased with three other effects, all of them interactions of three or more factors; and in the Resolution VI design, each effect was aliased with one other effect, an interaction of four or more factors. These designs had coefficient estimates that were also very close to the true values. The Resolution III fractional factorial design, which aliased each effect with seven other effects, had coefficient estimates somewhat farther from the true values. The coefficient estimates associated with the individual-experiments and single-factor approaches were farthest from the true values of the main effect coefficients. In the individual-experiments and single-factor approaches, each effect was aliased with 15 other effects (the main effect of a factor was aliased with all the interactions involving that factor, from the two-way up to the six-way). The comparative treatment and constructive treatment approaches aliased the same number of effects but differed in the coding of the aliased effects (as can be seen in Table 2), which is why their coefficient estimates differed.

Although the seven experiments had the same overall sample size N , they differed in statistical power. The complete and fractional factorial experiments, which had identical statistical power, were the most powerful. Next most powerful were the comparative treatment and constructive treatment designs. The individual-experiments approach was the least powerful. These differences in statistical power, along with the differences in

coefficient estimates, were reflected in the effects found significant at various alpha levels across the designs. Among the designs examined here, the individual-experiments approach and the two single-factor designs showed the greatest disparities with the complete factorial.

Given the differences among them in aliasing, it is perhaps no surprise that these designs yielded different effect estimates and hypothesis tests. The research questions that motivate individual-experiments and single-factor designs, which often involve pairwise contrasts between individual experimental conditions, may not require estimation of main effects per se, so the relatively large differences between the coefficient estimates obtained with these designs and the true main effect coefficients may not be important. Instead, what may be more noteworthy is how few effects these designs detected as significant compared with those of the factorial experiments.

General Discussion

Some Overall Recommendations

Despite the situation-specific nature of most design decisions, it is possible to offer some general recommendations. When per-subject costs are high in relation to per-condition overhead costs, complete and fractional factorials are usu-

Table 8

Coefficient Estimates for Selected Interactions Under Different Designs in the Simulated Example, With Total Sample Size $N = 320$

Design	Breath × Audience	Breath × Choose	Breath × Prep	Breath × Notes	Breath × Stakes
Complete factorial	-0.03	-0.25 ^a	0.29 ^a	-0.07	-0.03
Resolution IV fractional factorial	-0.03	0.00	0.29 ^a	-0.16	-0.02
Resolution VI fractional factorial	0.02	-0.25 ^a	0.29 ^a	-0.07	0.04
Population interaction effect (see Equation 1)	0.00	-0.15	0.25	-0.15	0.00

^a Coefficient significant at $\alpha = .05$.

ally the most economical designs. When per-condition costs are high in relation to per-subject costs, usually either a fractional factorial or single-factor design will be most economical. Which is most economical will depend on such considerations as the number of factors, the sample size required to achieve the desired statistical power, and the particular fractional factorial design being considered.

In the limited set of situations examined in this article, the individual-experiments approach emerged as the least economical. Although the individual-experiments approach requires far fewer experimental conditions than a complete factorial and usually requires fewer than a fractional factorial, it requires more experimental conditions than a single-factor experiment. In addition, it makes the least efficient use of subjects of any of the designs considered in this article. Of course, an individual-experiments approach is necessary whenever the results of one experiment must be obtained first to inform the design of a subsequent experiment. Except for this application, in general the individual-experiments approach is likely to be the least appealing of the designs considered here. Investigators who are planning a series of individual experiments may wish to consider whether any of them can be combined to form a complete or fractional factorial experiment, or whether a single-factor design can be used.

Although factorial experiments with more than two or three factors are currently relatively rare in psychology, we recommend that investigators give such designs serious consideration. All else being equal, the statistical power of a balanced factorial experiment to detect a main effect of a given size is not reduced by the presence of other factors, except to a small degree caused by the reduction of error degrees of freedom in the model. In other words, if main effects are of primary scientific interest and interactions are not of great concern, then factors can be added without needing to increase N appreciably.

An interest in interactions is not the only reason to consider using factorial designs; investigators may simply wish to take advantage of the economy these designs afford, even when interactions are expected to be negligible or are not of scientific interest. In particular, investigators who undergo high subject costs but relatively modest condition costs may find that a factorial experiment will be much more economical than other design alternatives. Investigators faced with an upper limit on the availability of subjects may even find that a factorial experiment enables them to investigate research questions that would otherwise have to be set aside for some time. As Oehlert (2000, p. 171) explained, "There are thus two times when you should use factorial treatment structure—when your factors interact, and when your factors do not interact."

One of the objectives of this article was to demonstrate that fractional factorial designs merit consideration for use in psychological research alongside other reduced designs and complete factorial designs. Previous authors have noted

that fractional factorial designs may be useful in a variety of areas within the social and behavioral sciences (Landsheer & van den Wittenboer, 2000), such as behavioral medicine (e.g., Allore, Peduzzi, Han, & Tinetti, 2006; Allore, Tinetti, Gill, & Peduzzi, 2005), marketing research (e.g., Holland & Cravens, 1973), epidemiology (Taylor et al., 1994), education (McLean, 1966), human factors (Simon & Roscoe, 1984), and legal psychology (Stolle, Robbenolt, Patry, & Penrod, 2002). Shaw (2004) and Shaw, Festing, Peers, and Furlong (2002) noted that factorial and fractional factorial designs can help reduce the number of animals that must be used in laboratory research. Cutler, Penrod, and Martens (1987) used a large fractional factorial design to conduct an experiment studying the effect of context variables on the ability of participants to identify the perpetrator correctly in a video of a simulated robbery. Their experiment included 10 factors, with 128 experimental conditions but only 290 participants.

An Important Special Case: Development and Evaluation of Behavioral Interventions

As discussed by Allore et al. (2006); Collins, Murphy, Nair, and Strecher (2005); Collins, Murphy, and Strecher (2007); and West et al. (1993), behavioral intervention scientists could build more potent interventions if there was more empirical evidence about which intervention detracts from overall efficacy. However, as these authors note, generally behavioral interventions are designed a priori and then evaluated by means of the typical randomized controlled trial (RCT) consisting of a treatment group and a control group (e.g., Experimental Conditions 8 and 1, respectively, in Table 2). This all-or-nothing approach, also called the treatment package strategy (West et al., 1993), involves the fewest possible experimental conditions, so in one sense it is a very economical design. The trade-off is that all main effects and interactions are aliased with all others. Thus, although the treatment package strategy can be used to evaluate whether an intervention is efficacious as a whole, it does not provide direct evidence about any individual intervention component. A factorial design with as many factors as there are distinct intervention components of interest would provide estimates of individual component effects and interactions between components.

Individual intervention components are likely to have smaller effect sizes than the intervention as a whole (West & Aiken, 1997), in which case sample size requirements will be increased, compared with a two-experimental-condition RCT. One possibility is to increase power by using a Type I error rate larger than the traditional $\alpha = .05$, that is, to tolerate a somewhat larger probability of mistakenly choosing an inactive component for inclusion in the intervention to reduce the probability of mistakenly rejecting an active intervention component. Collins et al. (2005, 2007) recommended this and similar tactics as part of a phased

experimental strategy aimed at selecting components and levels to compose an intervention. In this phased experimental strategy, after the new intervention is formed its efficacy is confirmed in an RCT at the conventional $\alpha = .05$. As Hays (1994, p. 284) suggested, "In some situations, perhaps, we should be far more attentive to Type II errors and less attentive to setting α at one of the conventional levels."

One reason for eschewing a factorial design in favor of the standard two-experimental-condition RCT may be a shortage of resources needed to implement all the experimental conditions in a complete factorial design. If this is the primary obstacle, it is possible that it can be overcome by identifying a fractional factorial design requiring a manageable number of experimental conditions. Fractional factorial designs are particularly apropos for experiments in which the primary objective is to determine which factors out of an array of factors have important effects (where *important* can be defined as "statistically significant," "effect size greater than d ," or any other reasonable empirical criterion). In engineering these are called screening experiments. For example, suppose an investigator is developing an intervention and wishes to conduct an experiment to ascertain which of a set of possible intervention features are likely to contribute to an overall intervention effect. In most cases an approximate estimate of the effect of an individual factor is sufficient for a screening experiment, as long as the estimate is not so far off as to lead to incorrect inclusion of an intervention feature that has no effect (or, worse, has a negative effect) or incorrect exclusion of a feature that makes a positive contribution. Thus, in this context the increased scientific information that can be gained with a fractional factorial design may be an acceptable trade-off against the somewhat reduced estimation precision that can accompany aliasing. (For a Monte Carlo simulation examining the use of a fractional factorial screening experiment in intervention science, see Collins, Chakraborty, Murphy, & Strecher, 2009.)

It must be acknowledged that even very economical fractional factorial designs typically require more experimental conditions than intervention scientists routinely consider implementing. In some areas in intervention science, there may be severe restrictions on the number of experimental conditions that can be realistically handled in any one experiment. For example, it may not be reasonable to demand of intervention personnel that they deliver different versions of the intervention to different subsets of participants, as would be required in any experiment other than the treatment package RCT. Or the intervention may be so complex and demanding, and the context in which it must be delivered so chaotic, that implementing even two experimental conditions well is a remarkable achievement and trying to implement more would surely result in sharply diminished implementation fidelity (West & Aiken, 1997). Despite the undeniable reality of such difficulties, we wish to suggest that they do not necessarily rule out the use of

complete and, in particular, fractional factorial designs across the board in all areas of intervention science. There may be some areas in which a careful analysis of available resources and logistical strategies will suggest that a factorial approach is feasible. One example is Strecher et al. (2008), who described a 16-experimental-condition fractional factorial experiment to investigate five intervention components in a smoking cessation intervention. Another example can be found in Nair et al. (2008), who described a 16-experimental-condition fractional factorial experiment to investigate five features of decision aids for women choosing among breast cancer treatments. Commenting on the Strecher et al. article, Norman (2008) wrote, "The fractional factorial design can provide considerable cost savings for more rapid prototype testing of intervention components and will likely be used more in future health behavior change research" (p. 450). Collins et al. (2005) and Nair et al. (2008) have provided some introductory information on the use of fractional factorial designs in intervention research. Collins et al. (2005, 2007) discussed the use of fractional factorial designs in the context of a phased experimental strategy for building more efficacious behavioral interventions.

One interesting difference between the RCT on the one hand and factorial and fractional factorial designs on the other is that, compared with the standard RCT, a factorial design assigns a much smaller proportion of subjects to an experimental condition that receives no treatment. In a standard two-arm RCT, about half the experimental subjects are assigned to some kind of control condition (e.g., a wait list or the current standard of care). By contrast, in a factorial experiment there is typically only one experimental condition in which all the factors are set to Off. Thus, if the design is a 2^3 factorial, say, seventh eighths of the subjects will be assigned to a condition in which at least one of the factors is set to On. If the intervention is sought after and assignment to a control condition is perceived as less desirable than assignment to a treatment condition, there may be better compliance because most subjects will receive some version of an intervention. In fact, it often may be possible to select a fractional factorial design in which there is no experimental condition in which all factors are set to Off.

Investigating Interactions Between Individual Characteristics and Experimental Factors in Factorial Experiments

Investigators are often interested in determining whether there are interactions between individual subject characteristics and any of the factors in a factorial or fractional factorial experiment. As an example, suppose an investigator is interested in determining whether gender interacts with the six independent variables in the hypothetical example used in this article. There are two ways this can be

accomplished: One is exploratory, and the other is a priori (e.g., Murray, 1998).

In the exploratory approach, after the experiment has been conducted, gender is coded and added to the analysis of variance as if it were another factor. Even if the design was originally perfectly balanced, such an addition nearly always results in a substantial disruption of balance. Thus, the effect estimates are unlikely to be orthogonal, and so care must be taken in estimating the sums of squares. If a reduced design was used, it is important to be aware of what effects, if any, are aliased with the interactions being examined. In most fractional factorial experiments, the two-way interactions between gender and any of the independent variables are unlikely to be aliased with main effects, but some higher order interactions involving gender might be aliased with main effects.

In the a priori approach, gender is built into the design as an additional factor before the experiment is conducted, by ensuring that it is crossed with every other factor. Orthogonality will be maintained and power for detecting gender effects will be optimized if half the participants are men and half are women, with randomization done separately within each gender, as if gender were a blocking variable. However, in blocking it is assumed that there are no interactions between the blocking variable and the independent variables; the purpose of blocking is to control error. By contrast, in the a priori approach the interactions between gender and the manipulated independent variables are of particular interest, and the experiment should be powered accordingly to detect these interactions. In comparison with the exploratory approach, with the a priori approach it is much more likely that balance can be maintained or nearly maintained.

Variables such as gender can easily be incorporated into fractional factorial designs with the a priori approach. These variables can simply be listed with the other independent variables when using software such as PROC FACTEX to identify a suitable fractional factorial design. A fractional factorial design can be chosen so that important two-way and even three-way interactions between, for example, gender and other independent variables are aliased only with higher order interactions.

How Negligible Is Negligible?

To the extent that an effect placed in the negligible category is nonzero, the estimate of any effect of primary scientific interest that is aliased with it will be different from an estimate based on a complete factorial experiment. Thus, a natural question is, How small should the expected size of an interaction be for the interaction to be placed appropriately in the negligible category?

The answer depends on the field of scientific endeavor, the value of the scientific information that can be gained

with a reduced design, and the kind of decisions that are to be made based on the results of the experiment. There are risks associated with assuming an effect is negligible. If the effect is in reality nonnegligible and positive, it can make a positive effect aliased with it look spuriously large or make a negative effect aliased with it look spuriously zero or even positive. If an effect placed in the negligible category is nonnegligible and negative, it can make a positive effect aliased with it look spuriously zero or even negative or make a negative effect aliased with it look spuriously large.

Placing an effect in the negligible category is not the same as assuming it is exactly zero. Rather, the assumption is that the effect is small enough not to be very likely to lead to incorrect decisions. If highly precise estimates of effects are required, it may be that few or no effects are deemed small enough to be eligible for placement in the negligible category. If the potential gain of additional scientific information obtained at a cost of fewer resources offsets the risk associated with reduced estimation precision and the possibility of some spurious effects, then effects expected to be nonzero, but small, may more readily be designated negligible.

Some Limitations of This Article

The discussion of reduced designs in this article is limited in a number of ways. One limitation of the discussion is that it focuses on between-subjects designs. It is straightforward to extend every design here to incorporate repeated measures, which will improve statistical power. However, all else being equal, the factorial designs will still have more power than the individual-experiments and single-factor approaches. There have been a few examples of the application of within-subjects fractional designs in legal psychology (Cutler, Penrod, & Dexter, 1990; Cutler, Penrod, & Martens, 1987; Cutler, Penrod, & Stuve, 1988; O'Rourke, Penrod, Cutler, & Stuve, 1989; Smith, Penrod, Otto, & Park, 1996) and in other research on attitudes and choices (e.g., Sorenson & Taylor, 2005; van Schaik, Flynn, van Wersch, Douglass, & Cann, 2005; Zimet et al., 2005) in which a fractional factorial structure is used to construct the experimental conditions assigned to each subject. In fact, the Latin squares approach for balancing orders of experimental conditions in repeated measures studies is a form of within-subjects fractional factorial. Within-subjects fractional designs of this kind could be seen as a form of planned missingness design (see Graham, Taylor, Olchowski, & Cumsille, 2006).

Another limitation of this article is the focus on factors with only two levels. Designs involving exclusively two-level factors are very common, and factorial designs with two levels per factor tend to be more economical than those involving factors with three or more levels, as well as much more interpretable in practice, because of their simpler

interaction structure (Wu & Hamada, 2000). However, any of the designs discussed here can incorporate factors with more than two levels, and different factors may have different numbers of levels. Factors with three or more levels, and in particular an array of factors with mixed numbers of levels, add complexity to the aliasing in fractional factorial experiments. Although this requires careful attention, it can be handled in a straightforward manner with software like PROC FACTEX.

This article does not discuss what to do when unexpected difficulties arise. One such difficulty is unplanned missing data, such as with an experimental subject failing to provide outcome data. The usual concerns about informative missingness (e.g., dropout rates that are higher in some experimental conditions than in others) apply in complete and reduced factorial experiments just as they do in other research settings. In any complete or reduced design, unplanned missingness can be handled in the usual manner, via multiple imputation or maximum likelihood (see, e.g., Schafer & Graham, 2002). If experimental conditions are assigned unequal numbers of subjects, use of a regression analysis framework can deal with the resulting lack of orthogonality of effects with very little extra effort (e.g., PROC GLM in SAS). Another unexpected difficulty that can arise in reduced designs is evidence that assumptions about negligible interactions are incorrect. If this occurs, one possibility is to implement additional experimental conditions to address targeted questions, in an approach often called sequential experimentation (Meyer, Steinberg, & Box, 1996).

The Resource Management Perspective: Strategic Weighing of Resource Requirements and Expected Scientific Benefit

According to the resource management perspective, the choice of an experimental design requires consideration of both resource requirements and expected scientific benefit; the preferred research design is the one expected to provide the greatest scientific benefit in relation to resources required. Although aliasing may sometimes be raised as an objection to the use of fractional factorial designs, it must be remembered that aliasing in some form is inescapable in any reduced design, including individual-experiments and single-factor designs. We recommend considering all feasible designs and making a decision taking a resource management perspective that weighs resource demands against scientific costs and benefits.

Paramount among the considerations that drive the choice of an experimental design is addressing the scientific question motivating the research. At the same time, if this scientific question can be addressed only by a very resource-intensive design, but a closely related question can be addressed by a much less resource-intensive design, the in-

vestigator may wish to consider reframing the question to conserve resources. For example, when research subjects are expensive or scarce, it may be prudent to consider whether scientific questions can be framed in terms of main effects rather than simple effects so that a factorial or fractional factorial design can be used. Or when resource limitations preclude implementing more than a very few experimental conditions, it may be prudent to consider framing research questions in terms of simple effects rather than main effects. When a research question is reframed to take advantage of the economy offered by a particular design, it is important that the interpretation of effects be consistent with the reframing and that this consistency be maintained not only in the original research report but in subsequent citations of the report, as well as integrative reviews or meta-analyses that include the findings.

Resource requirements can often be estimated objectively, as discussed above. Tables like Table 5 may be helpful and can readily be prepared for any N and k . (An SAS macro to perform these computations can be found on the Web at <http://methodology.psu.edu>.) In contrast, assessment of expected scientific benefit is much more subjective because it represents the investigator's judgment of the value of the scientific knowledge proffered by an experimental design in relation to the plausibility of any assumptions that must be made. For this reason, weighing resource requirements against expected scientific benefit can be challenging. Because expected scientific benefit usually cannot be expressed in purely financial terms, or even readily quantified, a simple benefit-to-cost ratio is unlikely to be helpful in choosing among alternative designs. For many social and behavioral scientists, the decision may be simplified somewhat by the existence of absolute upper limits on the number of subjects that are available, number of experimental conditions that can be handled logistically, availability of qualified personnel to run experimental conditions, number of hours shared equipment can be used, and so on. Designs that would exceed these limitations are immediately ruled out, and the preferred design now becomes the one that is expected to provide the greatest scientific benefit without exceeding available resources. This requires careful planning to ensure that the design of the study clearly addresses the scientific questions of most interest.

For example, suppose an investigator who is interested in six two-level independent variables has the resources to implement an experiment with at most 16 experimental conditions. One possible strategy is a complete factorial design involving four factors and holding the remaining two factors constant at specified levels. Given that six factors are of scientific interest, this complete factorial design is actually a reduced design. This approach enables estimation of the main effects and all interactions involving the four factors included in the experiment, but these effects will be

aliased with interactions involving the two omitted factors. Therefore, for the investigator to draw conclusions, either these effects must be assumed negligible or interpretation must be restricted to the levels at which the two omitted factors were set. Another possible strategy is a Resolution IV fractional factorial design including all six factors, which enables investigation of all six main effects and many two-way interactions but no higher order interactions. Instead, this design requires assuming that all three-way and higher order interactions are negligible. Thus, both designs can be implemented within available resources, but they differ in the kind of scientific information they provide and the assumptions they require. Which option is better depends on the value of the information provided by each experiment in relation to the research questions. If the ability to estimate the higher order interactions afforded by the four-factor factorial design is more valuable than the ability to estimate the six main effects and additional two-way interactions afforded by the fractional factorial design, the four-factor factorial may have greater expected scientific benefit. On the other hand, if the investigator is interested primarily in main effects of all six factors and selected two-way interactions, the fractional factorial design may provide more valuable information.

Strategic use of reduced designs involves taking calculated risks. To assess the expected scientific benefit of each design, the investigator must also consider the risk associated with any necessary assumptions in relation to the value of the knowledge that can be gained by the design. In the example above, any risk associated with making the assumptions required by the fractional factorial design must be weighed against the value associated with the additional main effect and two-way interaction estimates. If other, less powerful reduced designs are considered, any increased risk of a Type II error must also be considered. If an experiment is an exploratory endeavor intended to determine which factors merit further study in a subsequent experiment, the ability to investigate many factors may be of paramount importance and may outweigh the risks associated with aliasing. A design that requires no or very safe assumptions may not have a greater net scientific benefit than a riskier design if the knowledge it proffers is meager or is not at the top of the scientific agenda motivating the experiment. Put another way, the potential value of the knowledge that can be gained in a design may offset any risk associated with the assumptions it requires.

References

- Allore, H., Peduzzi, P., Han, L., & Tinetti, M. (2006). *Using the SAS system for experimental designs for multicomponent interventions in medicine* (Paper No. 127–31) [White paper]. Retrieved from www2.sas.com/proceedings/sugi31/127-31.pdf
- Allore, H. G., Tinetti, M. E., Gill, T. M., & Peduzzi, P. N. (2005). Experimental designs for multicomponent interventions among persons with multifactorial geriatric syndromes. *Clinical Trials*, 2, 13–21.
- Bolger, N., & Amarel, D. (2007). Effects of social support visibility on adjustment to stress: Experimental evidence. *Journal of Personality and Social Psychology*, 92, 458–475.
- Box, G. E. P., & Hunter, J. S. (1961a). The 2^{k-p} fractional factorial designs: Part I. *Technometrics*, 3, 311–351.
- Box, G. E. P., & Hunter, J. S. (1961b). The 2^{k-p} fractional factorial designs: Part II. *Technometrics*, 3, 449–458.
- Box, G. E. P., Hunter, W. G., & Hunter, J. S. (1978). *Statistics for experimenters: An introduction to design, data analysis, and model building*. New York: Wiley.
- Box, G. E. P., & Meyer, R. (1986). An analysis for unreplicated fractional factorials. *Technometrics*, 28, 11–18.
- Cohen, J. (1988). *Statistical power analysis for the behavioral sciences*. Mahwah, NJ: Erlbaum.
- Collins, L. M., Chakraborty, B., Murphy, S. A., & Strecher, V. (2009). Comparison of a phased experimental approach and a single randomized clinical trial for developing multicomponent behavioral interventions. *Clinical Trials*, 6, 5–15.
- Collins, L. M., Murphy, S. A., Nair, V., & Strecher, V. (2005). A strategy for optimizing and evaluating behavioral interventions. *Annals of Behavioral Medicine*, 30, 65–73.
- Collins, L. M., Murphy, S. A., & Strecher, V. (2007). The Multiphase Optimization Strategy (MOST) and the Sequential Multiple Assignment Randomized Trial (SMART): New methods for more potent eHealth interventions. *American Journal of Preventive Medicine*, 32, S112–S118.
- Cutler, B. L., Penrod, S. D., & Dexter, H. R. (1990). Juror sensitivity to eyewitness identification evidence. *Law and Human Behavior*, 14, 185–191.
- Cutler, B. L., Penrod, S. D., & Martens, T. K. (1987). Improving the reliability of eyewitness identification: Putting context into context. *Journal of Applied Psychology*, 71, 629–637.
- Cutler, B. L., Penrod, S. D., & Stuve, T. E. (1988). Juror decision making in eyewitness identification cases. *Law and Human Behavior*, 12, 41–55.
- Graham, J. W., Taylor, B. J., Olchowski, A. E., & Cumsille, P. E. (2006). Planned missing data designs in psychological research. *Psychological Methods*, 11, 323–343.
- Green, P. E., & Rao, V. R. (1971). Conjoint measurement for quantifying judgmental data. *Journal of Marketing Research*, 8, 355–363.
- Hays, W. L. (1994). *Statistics* (5th ed.). Orlando, FL: Harcourt Brace.
- Holland, C. W., & Cravens, D. W. (1973). Fractional factorial experimental designs in marketing research. *Journal of Marketing Research*, 10, 270–276.
- Insightful Corporation. (2007). *S-PLUS 8 for Windows user's guide*. Seattle, WA: Author.
- Kirk, R. (1995). *Experimental design: Procedures for the behavioral sciences* (3rd ed.). Pacific Grove, CA: Brooks/Cole.
- Kuehl, R. O. (1999). *Design of experiments: Statistical principles*

- of research design and analysis (2nd ed.). Pacific Grove, CA: Duxbury/Thomson.
- Landsheer, J. A., & van den Wittenboer, G. (2000). Fractional designs: A simulation study of usefulness in the social sciences. *Behavior Research Methods*, 32, 528–536.
- Mathews, P. G. (2005). *Design of experiments with Minitab*. Milwaukee, WI: Quality Press.
- McLean, L. D. (1966). Phantom classrooms. *The School Review*, 74, 139–149.
- Meyer, R. D., Steinberg, D. M., & Box, G. E. P. (1996). Follow-up designs to resolve confounding in multifactor experiments. *Technometrics*, 38, 303–313.
- Murray, D. M. (1998). *Design and analysis of group-randomized trials*. New York: Oxford University Press.
- Nair, V., Strecher, V., Fagerlin, A., Ubel, P., Resnicow, K., Murphy, S., et al. (2008). Screening experiments and the use of fractional factorial designs in behavioral intervention research. *American Journal of Public Health*, 98(8), 1354–1359.
- National Institute of Standards and Technology/SEMATECH. (2006). *E-handbook of statistical methods*. Retrieved July 17, 2007, from <http://www.itl.nist.gov/div898/handbook>
- Norman, G. J. (2008). Answering the “What works?” question in health behavior change. *American Journal of Preventive Medicine*, 34, 449–450.
- Oehlert, G. W. (2000). *A first course in design and analysis of experiments*. New York: Freeman.
- O’Rourke, T. E., Penrod, S. D., Cutler, B. L., & Stuve, T. E. (1989). The external validity of eyewitness identification research: Generalizing across subject populations. *Law and Human Behavior*, 13, 385–398.
- Ryan, B. F., Joiner, B. L., & Cryer, J. D. (2004). *Minitab handbook* (5th ed.). Belmont, CA: Duxbury/Thomson.
- SAS Institute. (2004a). *SAS/QC 9.1 user’s guide*. Cary, NC: Author.
- SAS Institute. (2004b). *SAS/STAT 9.1 user’s guide*. Cary, NC: Author.
- Schafer, J. L., & Graham, J. W. (2002). Missing data: Our view of the state of the art. *Psychological Methods*, 7, 147–177.
- Shaw, R. (2004). Reduction in laboratory animal use by factorial design. *Alternatives to Laboratory Animals*, 32, 49–51.
- Shaw, R., Festing, M. F. W., Peers, I., & Furlong, L. (2002). Use of factorial designs to optimize animal experiments and reduce animal use. *Institute for Laboratory Animal Research Journal*, 43, 223–232.
- Simon, C. W., & Roscoe, S. N. (1984). Application of a multifactor approach to transfer of training research. *Human Factors*, 26, 591–612.
- Smith, B. C., Penrod, S. D., Otto, A. L., & Park, R. C. (1996). Jurors’ use of probabilistic evidence. *Law and Human Behavior*, 20, 49–82.
- Sorenson, S. B., & Taylor, C. A. (2005). Female aggression toward male intimate partners: An examination of social norms in a community-based sample. *Psychology of Women Quarterly*, 29, 78–96.
- Stolle, D. P., Robbennolt, J. K., Patry, M., & Penrod, S. D. (2002). Fractional factorial designs for legal psychology. *Behavioral Sciences and the Law*, 20, 5–17.
- Strecher, V. J., McClure, J. B., Alexander, G. L., Chakraborty, B., Nair, V. N., Konkel, J. M., et al. (2008). Web-based smoking-cessation programs: Results of a randomized trial. *American Journal of Preventive Medicine*, 34, 373–381.
- Taylor, P. R., Li, B., Dawsey, S. M., Li, J.-Y., Yang, C. S., Gao, W., et al. (1994). Prevention of esophageal cancer: The nutrition intervention trials in Linxian, China. *Cancer Research*, 54, 2029s–2031s.
- van Schaik, P., Flynn, D., van Wersch, A., Douglass, A., & Cann, P. (2005). Influence of illness script components and medical practice on medical decision making. *Journal of Experimental Psychology: Applied*, 11, 187–199.
- West, S. G., & Aiken, L. S. (1997). Toward understanding individual effects in multicomponent prevention programs: Design and analysis strategies. In K. J. Bryant, M. Windle, & S. G. West (Eds.), *The science of prevention: Methodological advances from alcohol and substance abuse research* (pp. 167–209). Washington, DC: American Psychological Association.
- West, S. G., Aiken, L. S., & Todd, M. (1993). Probing the effects of individual components in multiple component prevention programs. *American Journal of Community Psychology*, 21, 571–605.
- Wu, C., & Hamada, M. (2000). *Experiments: Planning, analysis, and parameter design optimization*. New York: Wiley.
- Zimet, G. D., Mays, R. M., Sturm, L. A., Ravert, A. A., Perkins, S. M., & Juliar, B. E. (2005). Parental attitudes about sexually transmitted infection vaccination for their adolescent children. *Archives of Pediatrics and Adolescent Medicine*, 159, 132–137.

Received October 3, 2007

Revision received February 6, 2009

Accepted February 13, 2009 ■