ISSUES INVOLVED IN THE CHOICE OF EXPERIMENTAL DESIGN STRATEGIES

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Technical Report No. SMU/DS/TR/253
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October 1991

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Abstract

Quality improvement programs in recent years have increasingly stressed the need to conduct experiments to identify features of products or processes that can lead to enhanced quality. Competitive pressures mandate that these experiments be simultaneously information rich and resource efficient. Many multifactor experiments are conducted by varying only one factor at a time. Proponents of statistical design strategies, which vary several factors simultaneously, have demonstrated superiority according to technical criteria such as design efficiency. In this article, the dynamic character of one-factor-at-a-time experiments is exploited to demonstrate its clear geometric disadvantages relative to statistically designed experiments.

Key Words: Experimental design, Factorial experiments, One-factor-at-a-time experiments.

1. Introduction

One-Factor-At-a-Time (OFAT) experimental design strategies for changing factor settings are cited often in the statistical literature as strategies that should be avoided. However, these same design strategies are widely used and advocated in industrial applications. In contrast, statistical design strategies (e.g., complete or fractional factorials conducted in completely randomized designs) are strongly recommended by the statistical community and are often avoided in industrial applications.

Statistical design strategies involve an experimental protocol which specifies simultaneous changes in two or more factors from one test run to the next. The resulting designs geometrically attempt to cover the experimental region of interest within the constraints imposed by the number of test runs allowed. The OFAT strategy includes a dynamic or sequential aspect which results in designs that have substantially inferior coverage of the experimental region than the statistical designs. This dynamic aspect results more from a mixing of experimental goals than from an inherent advantage of the OFAT strategy over the traditional statistical approach. Our objective in this article is to contrast the properties of the OFAT strategies with those ensuing from classical statistical design approaches. We attain this objective primarily by comparing the geometric properties of each type of design.

Many statistical textbooks on experimental design provide a description of an OFAT strategy (e.g., Box, Hunter, and Hunter 1978, Chapter 15; Mason, Gunst, and Hess 1989, Chapter 6). Taylor, et al. (1973) provide a detailed explanation of a sequential OFAT strategy for the minimization of a cost function, a strategy based on Friedman and Savage (1947, Chapter 13), within the context of a discussion of three multivariate optimization techniques. This OFAT design strategy can be briefly

described as follows (see Section 2 for further details).

Suppose that $y = f(x_1, x_2, \ldots, x_k)$ is a response which is a function of factors x_1, x_2, ... , x_k and it is desired to find a combination of values of the factors which maximizes y. Assume for simplicity that the factors are discrete-valued; i.e., each x_j can take on only m distinct values. First, the values of x_2, x_3, ... , x_k are fixed and the response is obtained for each of the m values of x_1. The value (or level) of x_1 that produces the maximum value of the response for the m test runs is then identified. The value of x_1 is then fixed at the level, say x_{10}, which maximizes y. Now x_2 is varied across its levels and a value, say x_{20}, is identified which maximizes y. This procedure is repeated sequentially for all of the factors until the list is completed. Note that this process requires a maximum of mk observations -- an important feature that is discussed later. There is still some ambiguity in this description of the OFAT procedure due to the ordering of the factors (i.e., which factor is named x_1, etc.), the choice of initial values for each of the factors, how many of the levels of each factor are actually tested, and whether any repeat tests are conducted to assure a stable solution. These ambiguities do not materially affect the comparisons made in the sections which follow.

A representative illustration of the OFAT design methodology is Marlow and Mason (1985). In this experiment on leakage of pipe connections used in the oil and gas wells, a number of controllable factors, including misalignment, tong speed, rocking, pipe rotation, and manufacturer, were varied one-at-a-time in an OFAT scheme. The resulting data base was expanded to include several uncontrolled variables (covariates): shouldering torque, final torque, thread contact, multiple torque peaks, crooked pipe, and tong slippage. Once the data were collected, the imbalance in the OFAT design and the presence of uncontrolled covariates mandated that sophisticated regression modeling, including an assessment and accommodation of collinearities, be used to

model the response. The need for sophisticated statistical modeling due to the imbalance in the design is a common feature of OFAT experimentation. Had a balanced statistical design been used, some of these problems could have been alleviated.

Pilon (1989) reports that product design often is optimized using finite element analysis on data collected by changing factors in an unstructured OFAT experiment. An alternative approach using inner and outer orthogonal arrays (fractional factorials) popularized by Taguchi is applied to explore the finite element model space in a structured manner that is more efficient than the OFAT strategy. The inner- and outer-array type of design, although somewhat controversial because of the potential for the confounding of effects and because of the size of the experiment, permits the examination of both "control" and "noise" factors.

Leigh and Taylor (1990) refer to OFAT as the so-called classical experimental design and "the one that has been favored almost exclusively among scientists and engineers." They describe it as a logical and orderly approach to experimentation that is superior to the "shotgunning" intuitive approach sometimes favored by novice researchers. They note as drawbacks to the technique "lack of information about errors resulting from material variation, bias errors, and errors resulting from the sequence of testing". Perhaps the most important drawback is the lack of information regarding the interaction between factors. These authors recognize the advantages of statistical designs and suggest that their use will increase with the advent of the personal microcomputer and the development of statistical software packages which incorporate statistical designs.

Bajaria and Copp (1987) note that for improving a stable manufacturing process there are two competing strategies -- one relying on the OFAT "trial-and-error approach" and the other relying on methods grouped in the phrase "statistical problem

solving." The authors draw an effective analogy between trying to open a lock by testing one key (variable) at a time, OFAT, versus trying to open a combination lock by simultaneously changing several factors, statistical problem solving. The "one-key-at-a-time" approach is no longer preferable since optimal solutions to most current scientific and engineering problems ordinarily rely on optimum combinations of several factors, not on a single one. It is further noted that most American manufacturers today have gained knowledge of their products and processes through OFAT strategies and may have reached the upper limit of the rate at which trial-and-error creativity can progress. Appropriate use of statistical methods is recommended as a means of achieving further progress.

Thomas (1974) notes that in small models where all factors are independent and have no interaction effects on output, OFAT may be acceptable. However, in sensitivity testing of large-scale computer models where such assumptions are not justified, the use of statistical design of experiment techniques (e.g., factorials) leads to a better understanding of model sensitivity to specific factors and, equally important, to their combined influences.

Roussel et al. (1984) note that in the photoresist process, a critical technology in semiconductor wafer fabrication, start-up is very critical. A common method used to characterize the needed processes is to first find a point where the process works, then to vary a single parameter (factor) at a time while keeping the others constant, an OFAT strategy. Since several parameters can be important in a positive photoresist process, it can be a lengthy process to explore all of them one-at-a-time, and interactions between two or more of the factors may not be discovered. The authors recommend a "statistical strategy of experimentation" utilizing factorials, Plackett-Burman designs, and Box-Behnken designs to overcome the limitations of OFAT

techniques.

These references to the use of OFAT strategies in the engineering literature point out both the widespread use of OFAT designs in engineering and scientific applications and the anecdotal recognition of their shortcomings. In the remainder of this paper, the weaknesses of OFAT experimentation are more comprehensively discussed in a structured discussion of design geometry and the requirements of good experiments. In Section 2, a paradigm for the dynamics of OFAT experimentation is developed. Deficiencies in this paradigm are geometrically demonstrated. In Section 3, guidelines for the statistical planning of experiments are shown to be seriously lacking in OFAT experiments. Recommendations for the selective use and the avoidance of OFAT experiments are made in Section 4.

2. Characterization of OFAT Designs

OFAT experimental designs, like all experimental designs, consist of a sequence of design points laid out in a geometric pattern. If an experiment involves changing k factors, each test run can be represented by a set of values $(x_1, x_2, \ldots, x_k)$, where x_j represents the level chosen for the jth factor. Because of this representation, the selected design points form a geometric pattern in k-dimensional space. The shape and dimensions of this pattern depend on the strategy and dynamics used to select the points.

As an illustration, consider a three-factor experiment. If each factor is to be tested at three equally spaced levels, the experimental region, perhaps after suitable coding, can be represented as the surface and interior of a cube. A complete factorial experiment with one or more test runs conducted at the center of the design also could be geometrically represented as a cube similar to Figure 1. The points represented in

Figure 1 are all combinations of (x_1, x_2, x_3), with each x_j taking the values 1, 2, and 3. Observe that test runs are conducted on each edge, each face, at each corner, and at the center of the cube. The cube in Figure 1 could either define the limits of the experimental region or it may represent a subregion interior to the experimental region.

Using the same three levels as shown in Figure 1, all fractional factorial and OFAT experimental designs for three factors, each at three levels, are geometric figures consisting of subsets of the points in Figure 1. Figure 2 shows a one-third fraction of the complete factorial in Figure 1. This fraction is commonly recommended (e.g., Cochran and Cox 1957, p. 271) when it is not possible to conduct a complete factorial and it is known that no interactions exist among the design factors. Qualitatively, it is a reasonable design in such circumstances since each level of each factor occurs an equal number of times in the experiment. Moreover, there is some geometric balancing in that design points occur on three corners, three edges, and three faces. Of perhaps greatest importance is the fact that one can evaluate the potential for this design to achieve the experimental goals because the design points are fixed and known in advance of the experimentation. Other designs may be deemed more appropriate once such an evaluation occurs.

OFAT designs can also be represented as a geometric figure defined by subsets of the points in Figure 1. Unlike traditional complete and fractional factorial experiments, OFAT designs are dynamic in the sense that the final selection of points in the design is dictated by outcomes observed during the course of the experimentation based on a predefined strategy for choosing "optimum" points. A small initial set of points (x_1, x_2, ... , x_k) is selected; i.e., the m points defined by listing all the m levels of one factor while holding the levels of the other factors fixed. Once the outcomes from the initial m test runs are known, a decision strategy is utilized to determine subsequent points at

which to test. All such strategies are based on the initial and subsequent outcomes and are therefore dynamic.

In a three-factor, three-level experiment, one might initially choose to test all three levels of Factor A with Factors B and C set at their middle levels. Alternatively, one might initially choose to test all three levels of A with B and C set at their lowest or highest levels. One could also choose to test all three levels of A with the levels of B and C set to standard levels, regardless of whether the standard level for each of the factors is its lowest, middle, or highest value. Obviously, there are many choices for initial sets of points; however, once the data are collected on the initial set of points, the strategy and the dynamics of the procedure dictate the next tests, as described in Section 1.

Figures 3 and 4 illustrate two of the many possible geometric patterns that could result from such a dynamic procedure. Suppose that the initial level for each factor is chosen to be its middle level. Suppose further that as each factor is evaluated its middle level turns out to be the optimum level. The experimental region tested is shown in Figure 3. The centers of each of the six faces of the cube are all included, but there is no information about extreme combinations of the factors on the edges or on the corners of the cube. Alternatively, suppose again that that the initial level for each factor is chosen to be its middle level but that as each factor is tested its highest level is selected as the optimum level. The experimental region tested is shown in Figure 4. Five of the seven locations of the test runs are on one face of the cube, and almost no information is provided on all the other edges, faces, and corners of the cube. Note that in general, for a design having k factors, *if at any stage of the testing the optimal level of a factor occurs on a face of the k-dimensional cube, all further testing will be restricted to that face of the design space.*

While one of the two OFAT designs shown in Figures 3 and 4 may be considered equally appropriate as the design in Figure 2 in a particular experimental setting, it is unlikely that both would be considered appropriate. Moreover, due to the dynamic nature of OFAT experimentation, it is unknown prior to the experimentation which of these two designs, or which one of many possible others, would actually be realized. *It is therefore impossible to evaluate, prior to conducting the experiment, whether the final design points are likely to enable the experimental goals to be achieved.*

To illustrate the potential detrimental effects of OFAT experimentation, consider the data in Table 1 (Hart 1963). These data were obtained from a complete factorial experiment involving four factors, three having two levels and one having four levels. The outcome of interest is the final tension (psi) of an automobile engine head bolt. The purpose of the experiment is to determine which combination of the design factors produces the greatest (mean) final tension. The four design factors of interest are stud type, initial tension, gasket type, and the position of the head bolt on the engine.

One test run of this experiment necessarily involves all four of the positions on the engine. Because of this, the position factor does not influence the selection of a design involving the other three factors. Consequently, attention can be concentrated on the first three factors, a three-factor experiment in which each factor is to be tested at two levels. Using the averages across the four positions as the responses for this experiment, the eight averages in Table 1 constitute the results of a complete factorial experiment in these three factors. It is clear from the averages in Table 1 that the combination of stud B, initial tension B, and gasket B resulted in the highest average final tension.

Suppose that the experiment had been run using an OFAT design strategy. Suppose further that stud A and tension level A were chosen to be the initial levels as each level of gasket type was tested (run #1 = gasket A, run#2 = gasket B). Note that

gasket A (average = 2967 psi) would be selected over gasket B (average = 2875 psi) and would be used in the remainder of the test runs. Next, with stud A and gasket A fixed, initial tension would be tested at its B level (run #3). Initial tension B (average = 3638 psi) would be selected over initial tension A (average = 2967 psi, from run #1). Finally, with initial tension set at its B level and gasket at its A level, stud B would be tested (run #4). Stud A (average 3638 psi, from run #3) would be selected over stud B (average = 3157 psi). The "optimum" combination of factor levels would then be chosen to be stud A, initial tension B, and gasket A, with the mean final tension estimated to be 3638 psi.

As mentioned above, the true optimum levels are stud B, initial tension B, and gasket B, with mean final tension estimated to be 3790 psi. The cause of the selection of the suboptimal levels of the factors is the choice of initial levels. There are starting values that will lead to the correct optimum combination of levels; e.g., beginning with stud B and initial tension B. There are, however, other starting levels in addition to the one just illustrated that will lead to suboptimal levels; e.g., stud A and initial tension B.

Apart from this danger of mislocating an optimum combination of factor levels, there are other features of the data which are clearly evident from the complete factorial experiment but which would likely be unobservable from an OFAT experiment. An analysis of the complete factorial data using analysis of variance procedures uncovers a statistically significant interaction between stud type and gasket type. Stud type A produces a higher average final tension with gasket type A, but stud type B has a higher average final tension with gasket type B. In addition, there are two or three very high final tension measurements for position #1 on the engine.

Interactions are not usually evaluated in OFAT experiments because of the lack of simultaneous changes of the factor levels and the mistaken belief that the changes in

the levels of one factor, while holding all the other factors fixed, provides all the information needed to completely assess its influence on the response. Thus, the interaction between stud type and gasket type would not ordinarily be investigated, even if the data were sufficient to evaluate the statistical significance of the interaction. The lack of balance in the OFAT design could also lead to substantial bias in the interaction effects that might be observed. Similarly, the high final tension measurements for position #1 would likely not be discovered because some of the key combinations of the first three factors would not be run in the OFAT experiment.

3. Statistical Designs vs. OFAT

In the context of designing experiments to ascertain conditions for which a response surface attains a maximum, Friedman and Savage (1947) note three deficiencies of factorial experiments. First, they allocate test runs to regions of the factor space which may be of no interest because the regions are far from the maximum. Second, because of the balance that is inherent in factorial experiments, the design points can only effectively cover a small region. Large regions are explored superficially. Third, factorial experiments make no use of the fact that some of the factors might be continuous and others, though discrete, might be ordered. This last deficiency, the authors note, can be remedied.

Further, the authors note that a complete factorial experiment without replication would require m^k test runs for k factors each having m levels. An OFAT experiment in which all k levels of each individual factor were included would only require at most $m \cdot k$ test runs. Thus, the ratio of the number of test runs required by the complete factorial experiment to that of the OFAT design is at least $m^k/mk = m^{k-1}/k$, an increasing function of both m and k. For an experiment involving k = 5

factors each having m = 3 levels, the factorial experiment would require $3^5 = 243$ test runs while the OFAT design would utilize at most $3 \cdot 5 = 15$ test runs. Thus the OFAT could be replicated 16 times and still would require fewer test runs than the complete factorial experiment.

There are a number of rebuttals to these arguments for the preference of OFAT designs to complete factorial experiments. Some obvious ones are the need to explore, even if superficially, the entire experimental region if one does not know *a priori* the location of the maximum. If one does have such information, the experimental region explored can be made smaller for the statistical designs. Likewise, a complete factorial experiment is ordinarily not recommended when the number of test runs is excessive. Fractional factorials and composite designs are some of many alternatives to complete factorials when the number of runs is deemed excessive.

These arguments, pro and con, touch on only a few of many issues that arise in the context of designing experiments. The formulation of a comprehensive set of requirements for well designed experiments is needed to ensure that optimization of the choice of a design to fit one criterion does not lead to a poor choice of a design from some other important criterion.

Cox (1958, Chapter 1) lists the following requirements for a good experiment: absence of systematic error, sufficient precision, conclusions should have a wide range of validity, designs and analyses should be as simple as possible, and the uncertainty in the conclusions should be assessable. The wide variety of statistical designs available (e.g., Cochran and Cox 1957; Mason, Gunst, and Hess 1989; Ostle and Malone 1988) permit these requirements to be satisfied to the greatest extent possible within the constraints of time, budget, equipment, and personnel imposed by the nature of the experiment. It should be clear from the previous discussions that OFAT designs place a premium on

the simplicity requirement and the number of test runs, often to the serious detriment of the others.

Freedom from systematic error, the first of Cox's requirements, implies that fitted models and estimated factor effects are not biased. Bias can occur in experiments for any of a number of reasons, including differences in experimental units to which factor combinations are applied, the choice of a design, the model fit to the data, or because of any combination of these reasons. The wide range of blocking designs (e.g., latin and graeco-latin squares, randomized complete block, balanced incomplete block), when coupled with the physical act of randomization, enable an investigator to control for heterogeneous experimental units or test conditions. Complete factorials (with randomization), as opposed to fractional factorials (including OFAT designs), enable all individual and joint factor effects to be estimated and provides better assurance that an adequate model can be fit to the data. Adequate modeling does depend on the knowledge and experience of the investigator, but it is also critically dependent on the design used. An OFAT design will not usually permit the inclusion of all two-factor interactions in a model and is usually quite imbalanced (some factor levels are included in the design many times, others only a few times, still others only once). Modeling deficiencies or severe imbalance in a design can lead to serious bias in estimates of model parameters and factor effects.

Adequate precision, the second of Cox's necessary qualities for a good experiment, generally implies that model parameter estimates and factor effects be estimated with suitably small standard errors. Depending on the intrinsic variability in observations taken under similar conditions, adequate precision can be achieved through the use of blocking designs or through sufficient replication of some or all of the factor combinations included in the design. The larger the intrinsic variability in repeat

observations, the larger the experiment size that is needed to ensure adequate precision. If large experiments are conducted by repeating factor combinations, the increased experiment size is often accompanied by increased variability due to heterogeneous experimental units or test conditions. Blocking is then needed to control this extraneous source of variability. Blocking and replication are in direct conflict with the prevailing priorities of OFAT experiments: simplicity and small experiment size. Thus OFAT designs often ignore the requirement for adequate precision in an experiment.

The factorial layout in Figure 1 comprehensively covers the experimental region of interest, the third of Cox's requirements, within the constraint of selecting three levels for each factor. In contrast, the OFAT experiment geometrically portrayed in Figure 4 is severely restricted to a small area within the region of interest. The tradeoff for fewer test runs for the OFAT design is a serious compromise of the investigator's ability to draw conclusions over a wide range of interest.

The ability to properly asses factor effects in the presence of uncertainty is the last of Cox's requirements. In many types of industrial experiments there is more than one type or source of uncertainty that must be quantified. Split-plot designs, initially used predominantly in agricultural experimentation because of differences in variability associated with different plot sizes, are becoming increasingly popular statistical designs in industrial experiments because of constraints on randomization of test runs (e.g., Addleman 1964; Mason, Gunst, and Hess 1989, Section 10.4). Split-plot designs permit the estimation of two or more sources of uncontrolled variability and the consequent correct estimation of uncertainty of factor effects which have different sources of variability (see also, Milliken and Johnson 1984, Chapter 24). Whether from different plot sizes, different types of experimental units, or because of restricted randomization, different sources of experimental uncertainty must be accounted for in the analysis of

such data. OFAT experiments do not acknowledge the possible presence of two or more sources of uncontrolled variability and, therefore, do not permit correct assessment of factor effects relative to the various sources of variability. Thus, OFAT experimental strategies must sacrifice a correct analysis of data from experiments in which uncertainty arises from two or more sources in order to maintain priority on simplicity.

Of Cox's five requirements for a good experiment, the fourth one, simplicity, is arguably not as critical as the other four, at least at first glance; moreover, it diminishes in importance as more advanced experimental design computer software becomes readily available. What makes simplicity so strong a consideration in industrial experimentation is the lack of statistical sophistication of many investigators who design or conduct experiments. Relatively few engineers and scientists performing industrial experiments are trained in the statistical design of experiments. On the other hand, very few of these same investigators wish to use procedures with which they are not familiar or that have properties that are not understood. Yet this paradox is at the heart of the concern over the use of OFAT designs.

While OFAT designs are conceptually simple and easy to understand, their properties cannot be detailed prior to an experiment precisely because of the dynamic character of OFAT experimentation. Whether an OFAT experimental design will have desirable statistical properties is unknown at the beginning of the experiment. Cox's requirements for the absence of systematic error, a wide range of validity, and the ability to assess factor effects in the presence of uncertainty cannot be assured. In contrast, the properties of the designs and the resulting analyses for any of the statistical designs mentioned above have been well studied and documented.

There are numerous other discourses on the requirements for "good" experimental designs in addition to the above discussion of Cox's. The discussions by

Box and Draper (1959) on response surface designs and Hunter (1985) on the design of industrial experiments are especially germane. The latter article, in a discussion of orthogonal arrays, addresses the need for considering the properties of both the design and the intended model prior to the selection of an appropriate experimental plan.

4. Concluding Remarks

The primary rationale for the use of OFAT experimental designs is the folklore that the effects of factors in multifactor experiments cannot be satisfactorily interpreted when two or more factors are simultaneously changed. The voluminous literature on the analysis of statistically designed experiments contradicts this belief. It is true, moreover, that the geometric pattern of OFAT designs may render the analysis of data from these designs incapable of separating the true effects of the factors.

Statistical models arising from designed experiments can conveniently be categorized into the following four groups: (1) models about which nothing is known or assumed, (2) models assumed to be additive in the individual factor effects, (3) models assumed to be linear in the parameters but not necessarily additive in the individual factor effects, and (4) models assumed to have some other specified functional form. The goals of a designed experiment can be stipulated in many ways, including the following three: (a) comprehensively model the response as a function of the factors, (b) comprehensively assess individual and joint factor effects for the factor levels included in the experiment, and (c) determine the level(s) of the factors that produce optimum responses. The OFAT design strategy would appear to have merit for goal (c) if model (2) could be considered appropriate. All other combinations of models and goals listed above are better served by a careful selection of a statistical design.

As stressed in this paper, the crucial deficiency of OFAT experimental designs is

their dynamic nature. Because of this dynamic character of OFAT designs, no optimal geometrical or statistical properties can be cited for them. It is clear from the examples in this paper that there are geometrical difficulties with these designs, difficulties that affect the statistical properties needed for adequate modeling and correct inferences on the factor effects. On the other hand, statistical designs have known geometrical and statistical properties. Adequate modeling, subject to the information known or suspected prior to the experiment, can be guaranteed. For example, highly efficient designs can be chosen that will guarantee that quadratic models in the factors can be fit to the data without unplanned confounding of the various factor effects. OFAT designs in general cannot provide such a guarantee.

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Table 1. Final Tension Measurements.

	Stud Type A				Stud Type B			
	Initial Tension A		Initial Tension B		Initial Tension A		Initial Tension B	
	Gasket Type		Gasket Type		Gasket Type		Gasket Type	
Position	A	B	A	B	A	B	A	B
1	3460	3250	4110	3950	3300	3330	3340	4410
2	2950	2750	3220	3425	2010	3010	3160	3560
3	2600	2400	3575	2870	2150	2300	2930	3530
4	2860	3100	3650	3610	2940	2490	3200	3660
Average	2967	2875	3638	3463	2600	2782	3157	3790
OFAT Run No.	1	2	3				4	

Fig. 1 Complete Factorial in Three Factors, Each Having Three Levels.

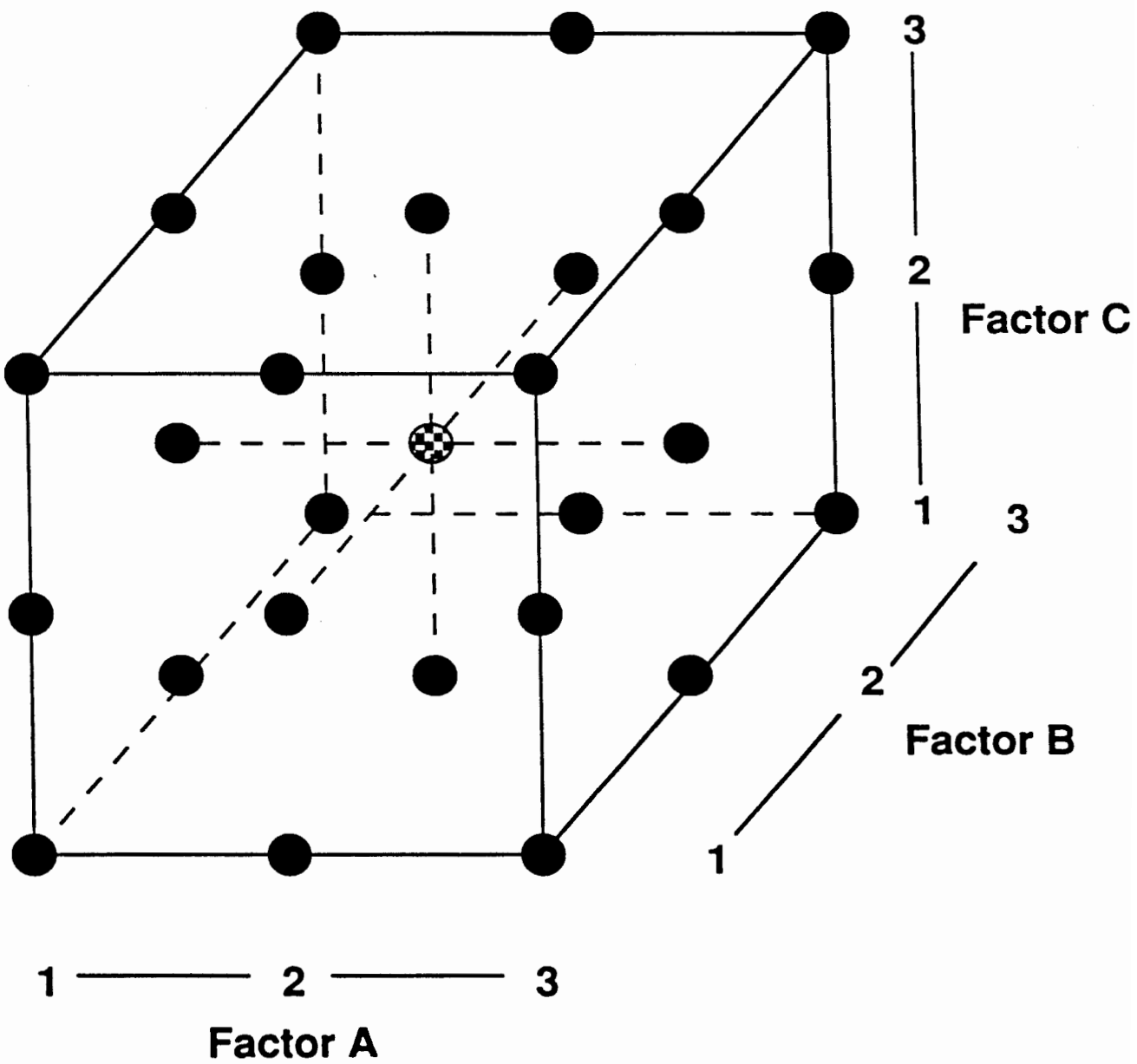

Fig. 2 One-Third Fractional Factorial.

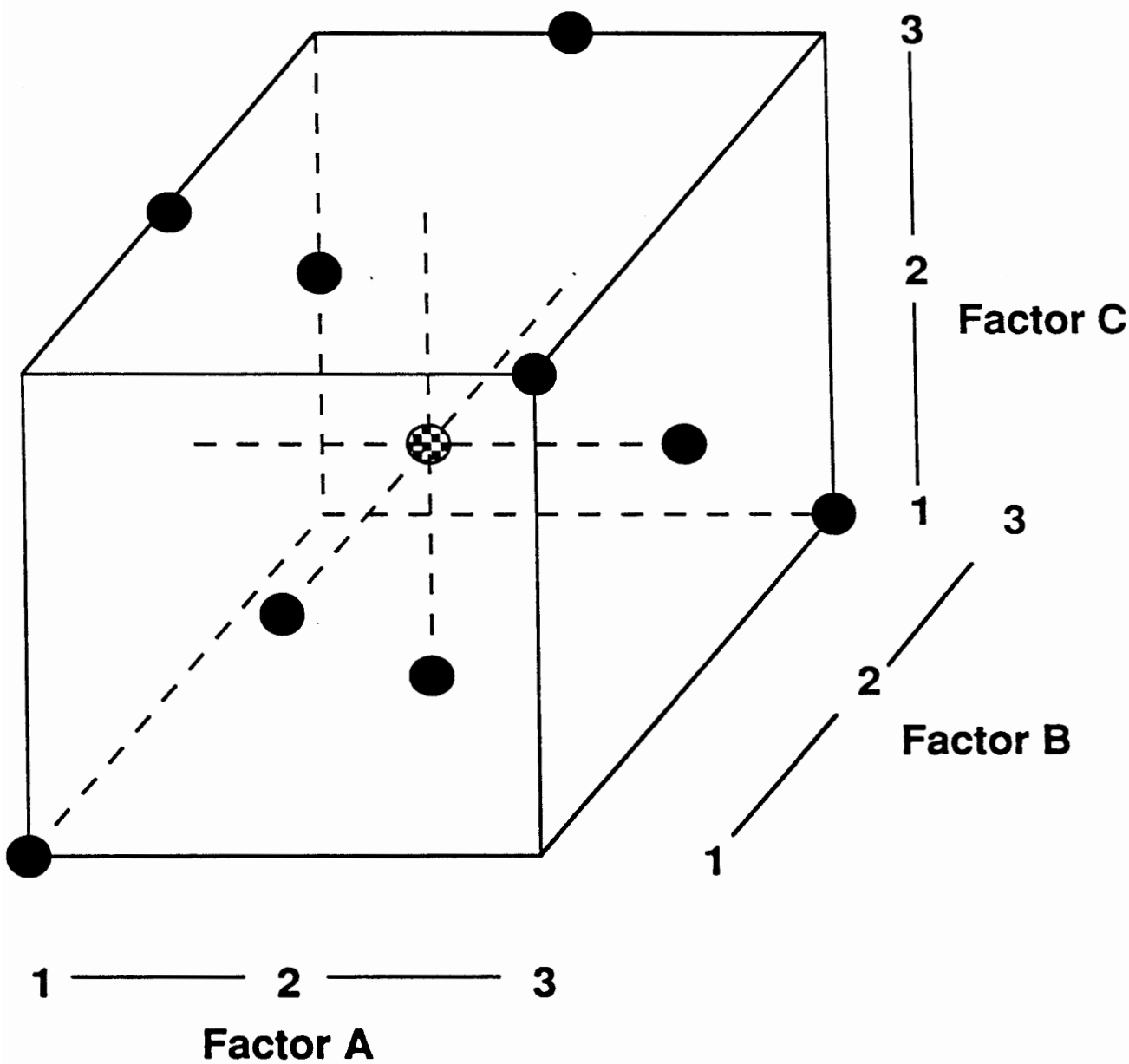

Fig. 3 One-Factor-at-a-Time Experiment,
Middle Levels Selected as Optimum.

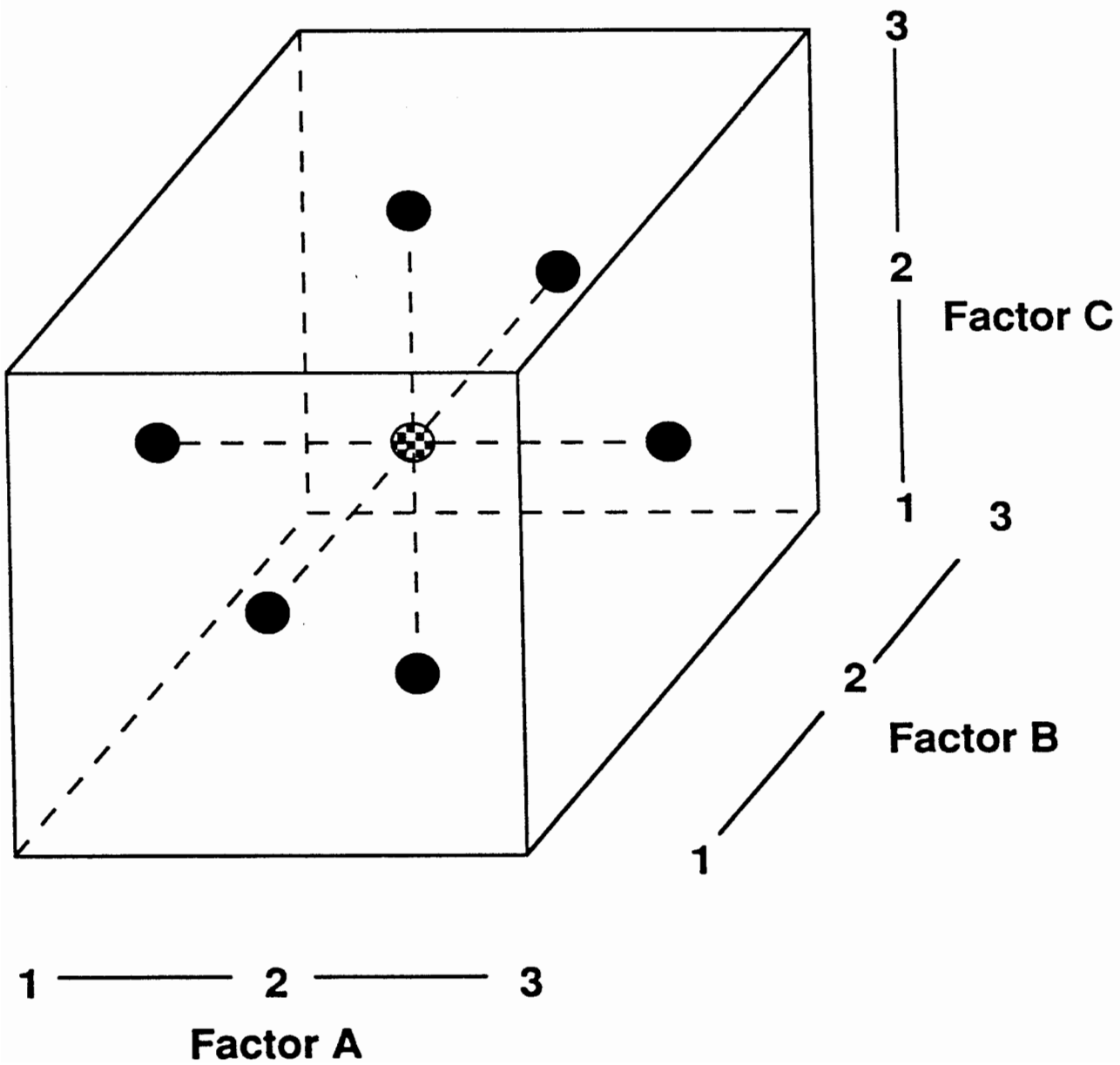

Fig. 4 One-Factor-at-a-Time Experiment,
Highest Levels Selected as Optimum.