Fractional Box-Behnken Designs

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Abstract

In contrast to the usual sequential nature of response surface methods (RSM), recent literature has proposed both screening and response surface exploration using only one three-level design; this approach is known as "one-step RSM". We discuss and illustrate two shortcomings of the current one-step RSM designs and analysis. Subsequently, we propose a class of three-level designs and an analysis that will address these shortcomings and aid the user in being appropriately advised as to factor importance. We illustrate the designs and analysis with simulated and real data.

Keywords: Box-Behnken designs, effect heredity, effect sparsity, incomplete block designs, one-step RSM, subset designs

Introduction and Motivation

Cheng and Wu (2001) (henceforth CW) introduce a novel method for exploring a response surface using only one design. This is in contrast to the common practice in response surface optimization, which is to use a sequential experimentation process that consists of beginning with a screening experiment in order to identify important factors, moving to a new region using steepest ascent if main effects indicate such an opportunity for improvement, and finally fitting a response surface model based on a second-order design in the new region. Rather, CW proposes to use 3-level orthogonal arrays (OAs) in order to first screen a large number of factors and then project from the larger factor space onto a smaller space to perform response surface exploration reusing the initial design. Assuming that all factors are quantitative and are denoted by $x_1, x_2, \ldots, x_t$, the second-order model is given by

$$y = \beta_0 + \sum_{i=1}^{t} \beta_i x_i + \sum_{i=1}^{t} \beta_{ii} x_i^2 + \sum_{i<j}^{t} \beta_{ij} x_i x_j + \varepsilon, \tag{1}$$

where β_i are the linear main effects, β_{ii} are the quadratic main effects, β_{ij} are the linear-by-linear interactions, and ε is the error term. The one-step approach to RSM is also described in Lawson (2003).

While the sequential nature of RSM is usually viewed as advantageous, as it gives the experimenter an opportunity to learn from each experiment, CW mentions the case in which experimental preparation is time-consuming or its duration long as a disadvantage to the sequential framework. Examples of such instances are given in CW and Lawson (2003) and include running experiments on a production line requiring a change of work schedule, training of operators, and/or trial runs; experimentation under fixed deadlines such as pilot plants or the development and production of prototypes; or medium optimization in biological experiments in which a single run could take up to a month.

In the first stage of CW's method, factor screening is performed to identify which of a potentially large number of factors are active. This step usually involves a main effects only analysis, which can have the unfortunate consequence of missing important interactions and can lead to a misspecification of the response surface. Bingham (2001) comments that because the first step considers main effects only, an underlying assumption is that every factor affecting the response has a significant main effect and thus, one is assuming that all factors with significant interaction effects also have significant main effects (this is known as *strong effect heredity*). Bingham (2001) further comments that "while it is convenient to perform the first stage of the analysis ignoring interactions, it seems unrealistic to expect that the strong heredity assumption will apply in most applications". If the bias to main effects due to omitted interactions is ignored and results in missing an active effect, the loss is substantial.

The key step in CW's method is projection; providing the link between the screening stage and response surface exploration. Clearly, in order to sufficiently explore the response surface in the projected factors, the projected design needs to be second-order. Such projected designs are called *eligible*. Otherwise, the projection is said to be *ineligible*. The possibility that a projection for a desired set of factors may be ineligible is another drawback of CW's method. Consider the following example.

Example 1

CW illustrates their method with a 27-run experiment to study the PVC insulation for electric wire with nine continuous factors denoted by A-H and J. This design is a regular 3^{9-6} with $C = AB$, $D = AB^2$, $F = AE$, $G = AE^2$, $H = BE^2$, and $J = AB^2E$. The design matrix and response (denoted as Y_1) are given in Table 1.

In the screening step of CW's analysis of Table 1's Y_1 data, factors A, B, C, D, and G are identified as significant based on fitting a main effects (linear and quadratic) only model to the data. The next step of the analysis would be to fit a second-order model using the

Table 1: Example from CW

Run	A	B	C	D	E	F	G	H	J	Y_1	Y_2
1	-1	-1	-1	-1	-1	-1	-1	-1	-1	5	-13
2	-1	-1	-1	-1	0	0	0	0	0	2	-16.9
3	-1	-1	-1	-1	1	1	1	1	1	8	-23.2
4	-1	0	0	0	-1	-1	-1	1	1	-15	0.054
5	-1	0	0	0	0	0	0	-1	-1	-6	-0.84
6	-1	0	0	0	1	1	1	0	0	-10	0.216
7	-1	1	1	1	-1	-1	-1	0	0	-28	12.42
8	-1	1	1	1	0	0	0	1	1	-19	16.5
9	-1	1	1	1	1	1	1	-1	-1	-23	23.2
10	0	-1	0	1	-1	0	1	-1	0	-13	-8.11
11	0	-1	0	1	0	1	-1	0	1	-17	-9.58
12	0	-1	0	1	1	-1	0	1	-1	-7	-8.86
13	0	0	1	-1	-1	0	1	1	-1	-23	13.58
14	0	0	1	-1	0	1	-1	-1	0	-31	1.305
15	0	0	1	-1	1	-1	0	0	1	-23	6.835
16	0	1	-1	0	-1	0	1	0	1	-34	-2.49
17	0	1	-1	0	0	1	-1	1	-1	-37	7.361
18	0	1	-1	0	1	-1	0	-1	0	-29	2.414
19	1	-1	1	0	-1	1	0	-1	1	-27	-2
20	1	-1	1	0	0	-1	1	0	-1	-27	3.69
21	1	-1	1	0	1	0	-1	1	0	-30	-7.41
22	1	0	-1	1	-1	1	0	1	0	-35	-7.52
23	1	0	-1	1	0	-1	1	-1	1	-35	-12.5
24	1	0	-1	1	1	0	-1	0	-1	-38	-0.84
25	1	1	0	-1	-1	1	0	0	-1	-39	9.75
26	1	1	0	-1	0	-1	1	1	0	-40	11.09
27	1	1	0	-1	1	0	-1	-1	1	-41	9.663

projected design of the five active factors. However, this is not possible for $\{A, B, C, D, G\}$. Indeed, there is no eligible projected design of five factors in the 3^{9-6} design. Recall that $C = AB$ and $D = AB^2$. Thus, C and D cannot be considered for projection once A and B are chosen. CW proceeds to find an eligible projection in the three most significant factors (A, B, and G). By not considering C and D in subsequent analyses, it is possible that important interactions involving these factors will be missed, and that some estimates for effects in the model will be biased, as the following example illustrates. ■

Example 2

Using the design of Example 1, suppose we simulate a new response variable from the following simple model:

$$y = 10x_B + 8x_C + 6x_{CG} + \varepsilon. \tag{2}$$

where $\varepsilon \sim N(0,1)$. Note that the true model does not satisfy strong effect heredity. The simulated response is given as Y_2 in Table 1. If a model with all nine linear and quadratic main effects is fit, one obtains the ANOVA table shown in Table 2. Factors B, C, and J stand out as active with nothing else identified as significant. We, therefore, project the 3^{9-6} design onto B, C, and J (which is eligible) and fit a second-order model. The results of this fit are shown in Table 3. No new effects are declared active. Thus, CW's analysis strategy identified an insignificant main effect as active and failed to identify the important interaction. ■

Examples 1 and 2 illustrate two shortcomings of CW's method. In particular, the initial screening step is unable to entertain the possibility of two-factor interactions forcing one to potentially miss important effects. Missing interactions often bias the main effect estimates of CW's recommended designs. Furthermore, the projection onto the factors of interest does not always yield a second-order design (i.e. the projection is ineligible). These issues would certainly be problematic to the experimenter seeking to implement the proposed method.

In order to find suitable designs, CW propose the *projection efficiency* criterion given as follows:

1. The number of eligible projected designs should be large and lower-dimensional projections are more important than higher-dimensional projections.

2. Among the eligible projected designs, the estimation efficiency should be high. One

Table 2: Example 2 - Pure Quadratic Model

Term	Estimate	Std Error	t Ratio	P-value
Intercept	-1.388	4.077	-0.340	0.742
A	0.304	1.146	0.270	0.797
B	9.738	1.146	8.500	0.000
C	7.489	1.146	6.540	0.000
D	0.310	1.146	0.270	0.793
E	-0.037	1.146	-0.030	0.975
F	-0.145	1.146	-0.130	0.902
G	0.306	1.146	0.270	0.796
H	0.081	1.146	0.070	0.945
J	-2.820	1.146	-2.460	0.039
A*A	-0.141	1.984	-0.070	0.945
B*B	0.220	1.984	0.110	0.915
C*C	-0.296	1.984	-0.150	0.885
D*D	0.102	1.984	0.050	0.960
E*E	0.247	1.984	0.120	0.904
F*F	-0.257	1.984	-0.130	0.900
G*G	0.372	1.984	0.190	0.856
H*H	-0.249	1.984	-0.130	0.903
J*J	2.351	1.984	1.180	0.270

Table 3: Example 2 - Second-Order Model

Term	Estimate	Std Error	t Ratio	P-value
Intercept	-1.340	1.670	-0.800	0.433
B	9.733	0.773	12.590	0.000
C	7.484	0.773	9.680	0.000
J	-2.816	0.773	-3.640	0.002
B*B	0.224	1.339	0.170	0.869
B*C	-0.206	0.947	-0.220	0.830
C*C	-0.292	1.339	-0.220	0.830
B*J	-0.008	0.947	-0.010	0.994
C*J	1.043	0.947	1.100	0.286
J*J	2.349	1.339	1.750	0.097

such measure of efficiency is D-efficiency given by

$$D_{eff} = (|\mathbf{M}(\mathbf{d})| \, / \, |\mathbf{M}(\mathbf{d}^*)|)^{1/p} \tag{3}$$

where $\mathbf{M}$ is the moment matrix defined as $\mathbf{X}'\mathbf{X}/n$, $|\mathbf{M}(\mathbf{d}^*)| = max_d\,|\mathbf{M}(\mathbf{d})|$, and $\mathbf{X}$ denotes the $n \times p$ $[p = (t+1)(t+2)/2]$ second-order model matrix for a t-factor design. That is, $\mathbf{d}^*$ is the D-optimum continuous design for the second-order model in (1).

Xu et al. (2004) (henceforth XCW) further propose an optimality criterion, known as *projection aberration*, to assess the performance of projections of three combinatorially non-isomorphic OA(18,3^7)s and three combinatorially non-isomorphic OA(27,3^{13})s. The 18 and 27-run OAs can screen up to 7 and 13 factors, respectively. Their criterion is based on the generalized word-length pattern of Xu and Wu (2001), $(A_1, A_2, ..., A_t)$, where A_i serves to measure the overall aliasing between all i-factor effects and the general mean and is defined to be

$$A_i = n^{-2} \sum_{j=1}^{t_i} \left[\sum_{h=1}^{n} x_{hj}^{(i)} \right]^2 \tag{4}$$

where t_i is the number of all i-factor effect contrasts and $x_{hj}^{(i)}$ is the h^{th} component of the j^{th}-factor effect contrast.

When a design with t factors is projected onto any three factors, it produces $\binom{t}{3}$ three-factor projected designs. Each of these designs has an A_3 value, known as the *projected* A_3 value. XCW comment that for an OA with smaller A_3, its main effects suffer less contamination when a main effects model is fitted, and thus factor screening is more effective as long as strong effect heredity is present. The frequency of the projected A_3 values is called the *projection frequency*.

The projection aberration criterion sequentially minimizes the projection frequency starting from the largest projected A_3 value. XCW utilize this criterion to screen out poor OAs and then to consider designs obtained by level permutations from remaining OAs using the projection efficiency criterion of CW. They recommend an 18 and 27-run OA, which we will examine in more detail later. Unfortunately, these designs still suffer from the shortcomings illustrated in Examples 1 and 2 above.

Justification for the projection efficiency criterion is based on the *factor sparsity* principle (i.e., the number of relatively important factors is small) and thus deem an eligible projection for a lower dimension to be more important than that for a higher dimension. While the factor sparsity assumption provides a reasonable rationalization for the projection efficiency criterion, it is likely that *effect sparsity* (i.e., the number of active *effects* is relatively small) is more appropriate when many factors are under consideration. That is, the assumption of effect sparsity may still hold even when factor sparsity does not. Thus, it makes sense to search for designs that can screen and project onto more than just a few factors.

While we do not dispute the ability of XCW's criteria to find reasonable designs for projection, unless one can correctly identify the important factors to explore further, the projection properties of a design have little utility. XCW briefly mentions this drawback and suggests the "more elaborate" Bayesian procedure for factor screening in the first stage. As noted in Chipman et al. (1997), this approach is computationally intensive and requires specification of prior information based on the experimenter's beliefs regarding effect heredity.

In this paper, we propose new three-level response surface designs by taking subsets of Box-Behnken designs. Their impact for one-step RSM is to provide the experimenter with the opportunity to explore the presence of interactions and therefore be more appropriately advised to the choice of active factors in fewer runs than required for a standard response surface design (e.g., Central Composite, Box-Behnken Designs). In the next section, we introduce the designs, discuss their construction method, and compare them with existing designs based on established design criterion. We then propose a simple graphical analysis strategy and provide several illustrative examples. The article concludes with a discussion and suggestions for future research.

Fractional Box-Behnken Designs

We assume a spherical design region, which will allow for response surface exploration with wider ranges for each factor. While narrow spacing is appropriate when the initial design is followed by steepest ascent, we require a wider range for each factor with the intent of detecting large effects.

Mee (2007) discusses how to utilize optimal design algorithms to construct three-level response surface designs for spherical regions. In particular, D-optimal designs are constructed for various run sizes using a candidate set of runs from a specific orbit of points the same distance from the design center. This is in contrast to using the full 3^t factorial as a candidate set. For t factors, the k^{th} orbit $(k = 0, 1, ..., t)$ contains $\binom{t}{k}2^k$ points with k factors at ± 1 and the remaining factors at 0.

Gilmour (2006) introduced a class of three-level response surface designs known as *subset designs* constructed by combining subsets of the 3^t factorial. Letting S_k represent the k^{th} orbit, such designs have the form $c_{k_1}S_{k_1} + c_{k_2}S_{k_2} + ...$ where c_{k_i} is the number of replicates of the points in S_{k_i}. For example, Gilmour (2006) proposes a 28-run response surface design in 3 factors of the form $2S_3 + 2S_2$. Thus, this design consists of two replicates of the 8 factorial points in the 3^{rd} orbit and two replicates of the 6 points in the 2^{nd} orbit.

If smaller run sizes are needed, *fractional subset designs* and *incomplete subset designs* are introduced. Fractional subset designs are constructed by replacing all of the two-level factorials in at least one S_k by a fractional factorial. The notation $\frac{1}{2}S_4^{IV}$ represents a resolution IV one-half fraction of S_4 and hence, for four factors $\frac{1}{2}S_4^{IV} + S_2 + 4S_0$ is a 36-run design. Incomplete subset designs do not use all of the $\binom{t}{k}$ factorial sets of k factors, but sets of k factors based on incomplete block designs. Incomplete subsets of the k^{th} orbit are denoted as (S_k). Thus, in seven factors, $\frac{1}{4}S_7^{IV} + (\frac{1}{2}S_3^{III}) + 4S_0$ consists of (32+28+4=)64 runs where (S_3) is based on an incomplete block design with 7 blocks of three units.

Many of the designs illustrated by Gilmour (2006) consist of points taken from S_t.

Given a spherical region of interest, however, the factorial points of the t^{th} orbit should be avoided. Following the recommendations of Mee (2007), the designs we propose consist of points taken from a single orbit. Namely, we consider incomplete subset designs based on Box-Behnken designs (BBDs) (Box and Behnken (1960)). Focusing on bioprocessing applications, which typically have high error variance, the subset designs proposed by Gilmour (2006) necessarily possess large run sizes. While our designs are similar in structure, we have in mind more traditional RSM applications with smaller error variance (e.g., mechanical and chemical engineering).

BBDs are popular three-level designs for estimating second-order models in spherical regions. For 3-5 factors, BB designs involve all points from S_2 whereas for more factors, these designs contain just a subset of points from S_k, where $k > 2$. BBDs possess a simple construction method by combining two-level factorial designs with incomplete block designs (IBDs). The following standard IBD notation will be useful:

- t: number of factors,
- k: block size,
- b: number of blocks,
- r: number of replicates of the t factors,
- $\lambda = r(k-1)/(t-1)$.

If possible, BBDs are based on balanced IBDs (BIBDs) where all pairs of treatment levels occur together within a block for λ blocks. When a BIBD does not exist or where run sizes become prohibitive with a BIBD, regular graph IBDs (RGs) are used in which all pairs of treatment levels occur together within a block either $\lambda_1 = \lceil \lambda \rceil$ or $\lambda_2 = \lfloor \lambda \rfloor$ blocks. Table 4 illustrates a 6-factor BB design, which is as a subset of points from S_3 and is based on a RG design with k=3, b=6, r=3, $\lambda_1 = 2$, and λ_2=1. In the table, the symbol $(\pm 1, \pm 1, ..., \pm 1)$ represents a full factorial in t factors. Thus, with the inclusion of a center-point run, the 6-factor BB design consists of 49 distinct points.

Table 4: Box-Behnken Design for 6 factors

A	B	C	D	E	F
± 1	± 1	0	± 1	0	0
0	± 1	± 1	0	± 1	0
0	0	± 1	± 1	0	± 1
± 1	0	0	± 1	± 1	0
0	± 1	0	0	± 1	± 1
± 1	0	± 1	0	0	± 1
0	0	0	0	0	0

Although BBDs are optimal or near-optimal, they are often too large to be of practical use. For five or more factors, BBDs contain many more runs than required to fit a second-order model (e.g., a BBD in 5 factors requires 40+ runs to estimate 21 parameters). Thus, it is unusual to see a BBD with 5 or more factors used in practice.

Fractional Box-Behnken designs (FBBDs) are constructed as their name suggests by taking subsets of the two-level factorial designs that compose a BBD. For $n \geq p$, FBBDs are able to estimate the full second-order model if assembled using appropriately chosen fractional subsets. On the other other hand, for $n < p$, it is possible to construct FBBDs with a simpler aliasing structure than the recommended OAs of XCW and which attain eligibility of every projection for $t-1$ (or fewer) factors.

In particular, for $t = 4, 6, 7, 9 (k = 3)$, a three-quarter fraction of a BBD is suggested to estimate the second-order model. In general, a three-quarter fraction of a full 2^k design, denoted $3(2^{k-2})$, is constructed simply by omitting a quarter fraction from the 2^k (see John (1962)). For instance, with $k = 3$, one can obtain a $3(2^{3-2})$ by combining the three quarter replicates with defining relations:

- $I = P = -Q = -PQ$,

- $I = -P = -Q = PQ$,
- $I = P = Q = PQ$,

where P, Q, and PQ are effects. Then, a $\frac{3}{4}$-BBD is constructed by combining an IBD with a $3(2^{k-2})$.

FBBDs are also assembled using one-half fractions of the two-level factorial subsets. For five factors, a BIBD with $k = 2$, $b = 10$, $r = 4$, and $\lambda = 1$ exists and estimation of the second-order model (in all five factors) with high D-efficiency is achieved by pairing 2_{I}^{2-1} subsets. That is, each resolution I subset is paired with another "opposite" subset (e.g. I=A with I=-A). For all other t, we make use of resolution III fractions.

With $t = 6,7,9 (k = 3)$, we construct a $\frac{1}{2}$-FBBD with $n < p$ by combining an IBD with a resolution III, 2^{3-1} fractional factorial. In doing so, each main effect is partially aliased with r two-factor interactions. Although the second-order model in t factors is not estimable, every projection onto $t-1$ factors is eligible. Later, we outline a simple analysis strategy for these designs to aid the user in being appropriately advised as to factor importance and hence, make an informed decision regarding projection.

For $t = 9-13 (k = 4)$, FBBDs are constructed by combining an IBD with a 2_{III}^{4-1}. Although this may appear counter-intuitive since a 2_{IV}^{4-1} exists, words of length four lead to aliasing among two-factor interactions that cannot be separated and, thus, should be avoided. In contrast, use of resolution III subsets *and* requiring that each factor appear a maximum of three times among the b generators, every main effect and two-factor interaction is estimable since $r = 4$. Making use of RG designs with k=4, $b = t$, $r = 4$, $\lambda_1 = 2$ and $\lambda_2 = 1$, the highest average D-efficiency of projection for t=9-12 is obtained when each generator involves factor pairs associated with $\lambda_1 = 2$. For $t = 13$, a BIBD exists with $\lambda = 1$. Table 5 presents a summary of our proposed designs where $n_0 \geq 1$ is the number of center point runs. Appendix Table A1 provides recommended FBBDs based on D-efficiency.

Although we wish to keep run sizes as small as possible, it is clearly advantageous to

Table 5: Summary of FBBDs

Design	t	Design Structure	Number of Parameters (Second-Order Model)	Number of Runs
$\frac{3}{4}$BB4	4	$\frac{1}{2}S_2^I + \frac{1}{4}S_2^I + n_0S_0$ (BIB: $6 \times 3(2_I^{2-2})$)	15	18+n_0
$\frac{1}{2}$BB5	5	$\frac{1}{2}S_2^I + n_0S_0$ (BIB: $10 \times 2_I^{2-1}$)	21	20+n_0
$\frac{1}{2}$BB6	6	$(\frac{1}{2}S_3^{III}) + n_0S_0$ (RG: $6 \times 2_{III}^{3-1}$)	28	24+n_0
$\frac{3}{4}$BB6	6	$(\frac{1}{2}S_3^I) + (\frac{1}{4}S_3^I) + n_0S_0$ (RG: $6 \times 3(2_I^{3-2})$)	28	36+n_0
$\frac{1}{2}$BB7	7	$(\frac{1}{2}S_3^{III}) + n_0S_0$ (BIB: $7 \times 2_{III}^{3-1}$)	36	28+n_0
$\frac{3}{4}$BB7	7	$(\frac{1}{2}S_3^I) + (\frac{1}{4}S_3^I) + n_0S_0$ (BIB: $7 \times 3(2_I^{3-2})$)	36	42+n_0
$\frac{1}{2}$BB9.1	9	$(\frac{1}{2}S_3^{III}) + n_0S_0$ (BIB: $12 \times 2_{III}^{3-1}$)	55	48+n_0
$\frac{3}{4}$BB9.1	9	$(\frac{1}{2}S_3^I) + (\frac{1}{4}S_3^I) + n_0S_0$ (BIB: $12 \times 3(2_I^{3-2})$)	55	72+n_0
$\frac{1}{2}$BB9.2	9	$(\frac{1}{2}S_4^{III}) + n_0S_0$ (RG: $9 \times 2_{III}^{4-1}$)	55	72+n_0
$\frac{1}{2}$BB10	10	$(\frac{1}{2}S_4^{III}) + n_0S_0$ (RG: $10 \times 2_{III}^{4-1}$)	66	80+n_0
$\frac{1}{2}$BB11	11	$(\frac{1}{2}S_4^{III}) + n_0S_0$ (RG: $11 \times 2_{III}^{4-1}$)	78	88+n_0
$\frac{1}{2}$BB12	12	$(\frac{1}{2}S_4^{III}) + n_0S_0$ (RG: $12 \times 2_{III}^{4-1}$)	91	96+n_0
$\frac{1}{2}$BB13	13	$(\frac{1}{2}S_4^{III}) + n_0S_0$ (BIB: $13 \times 2_{III}^{4-1}$)	105	104+n_0

construct designs based on BIBDs or RGs with $\lambda_2 \geq 1$ in order to avoid factor pairs not appearing together. Note that we do not provide a design for t=8 as no BIBD or RG design exists of a reasonable run size. See John and Mitchell (1977) and Clatworthy (1973) for extensive tables of IBDs.

Design Comparisons

For spherical regions, D-efficiencies are calculated as

$$D-eff = \left[\frac{|\mathbf{X}'\mathbf{X}/n|}{D_\infty}\right]^{1/p}, \tag{5}$$

where

$$D_\infty = 2^t(t+1)^{-p}(t+2)^{-t(t+2)}(t+3)^{p-1}. \tag{6}$$

For design comparison purposes, we require the t-factor design matrix $\mathbf{D}$ to be constrained (scaled) to the unit hypersphere (i.e. all diagonal elements of $\mathbf{DD}' \leq 1$).

We now compare the recommended designs of XCW with FBBDs in terms of eligible projections and average D-efficiency of projection (denoted as $\overline{D}$). D-efficiencies are calculated for the FBBDs given in Table A1 with $n_0 = 1$. Comparisons are also made with three-level D-optimal designs with the same run size as the FBBDs based on the recommended candidate orbits of Mee (2007). Results are displayed in Tables 6, 7, and 8.

To search for optimal designs based on a specified orbit, the modified Federov algorithm was implemented using SAS's OPTEX procedure with 1000 or more random starts. For t=12, 13, no D-optimal design was identified with higher D-efficiency than the Table A1 FBBDs using the entire fourth orbit as the candidate set of points (even after 3 days of searching and 8000 random starts). Upon reducing the candidate sets to incomplete subsets of S_4, a subsequent computer search uncovered the FBBDs in Table A1. As it remains uncertain if more extensive searching may eventually reveal an optimal design with better

D-efficiency, a "?" follows the reported values in Table 6.

Table 6: Eligible Projections and Average *D*-Efficiencies of FBBDs and *D*-optimal Designs

t	Run Size	Projection	Number of Projections	Number of Eligible Projections	$\overline{D}_{FBBD}$	$\overline{D}_{opt}$
4	17	2	6	6	0.532	0.533
		3	4	4	0.688	0.688
		4	1	1	0.879	0.890
5	21	3	10	10	0.527	0.560
		4	5	5	0.634	0.643
		5	1	1	0.749	0.828
6	25	3	20	20	0.668	-
		4	15	15	0.521	-
		5	6	6	0.616	-
		6	1	0	-	-
6	37	3	20	20	0.664	0.611
		4	15	15	0.541	0.572
		5	6	6	0.689	0.742
		6	1	1	0.858	0.937
7	29	3	35	35	0.632	-
		4	35	35	0.558	-
		5	21	21	0.563	-
		6	7	7	0.639	-
		7	1	0	-	-
7	43	3	35	35	0.619	0.590
		4	35	35	0.555	0.529
		5	21	21	0.586	0.614
		6	7	7	0.720	0.767
		7	1	1	0.867	0.938
9	49	3	84	84	0.507	-
		4	126	126	0.503	-
		5	126	126	0.436	-
		6	84	84	0.519	-
		7	36	36	0.599	-
		8	9	9	0.655	-
		9	1	0	-	-
9	72	3	84	84	0.496	0.477
(k=3)		4	126	126	0.495	0.437
		5	126	126	0.438	0.454
		6	84	84	0.535	0.550
		7	36	36	0.643	0.667
		8	9	9	0.761	0.795
		9	1	1	0.884	0.930
9	72	3	84	84	0.584	0.477
(k=4)		4	126	126	0.528	0.437
		5	126	126	0.559	0.454
		6	84	84	0.523	0.550
		7	36	36	0.585	0.667
		8	9	9	0.685	0.795
		9	1	1	0.789	0.930
10	81	3	120	120	0.559	0.431
		4	210	210	0.489	0.409
		5	252	252	0.526	0.396

Table 6 – Continued

t	Run Size	Projection	Number of Projections	Number of Eligible Projections	$\overline{D}_{FBBD}$	$\overline{D}_{opt}$
		6	210	210	0.538	0.468
		7	120	120	0.557	0.564
		8	45	45	0.627	0.670
		9	10	10	0.731	0.786
		10	1	1	0.837	0.906
11	89	3	165	165	0.531	0.394
		4	330	330	0.472	0.385
		5	462	462	0.488	0.356
		6	462	462	0.525	0.402
		7	330	330	0.515	0.480
		8	165	165	0.561	0.566
		9	55	55	0.655	0.662
		10	11	11	0.754	0.764
		11	1	1	0.855	0.870
12	97	3	220	220	0.502	0.502(?)
		4	495	495	0.461	0.461(?)
		5	792	792	0.450	0.450(?)
		6	924	924	0.500	0.500(?)
		7	792	792	0.501	0.501(?)
		8	495	495	0.509	0.509(?)
		9	220	220	0.584	0.584(?)
		10	66	66	0.670	0.670(?)
		11	12	12	0.759	0.759(?)
		12	1	1	0.847	0.847(?)
13	105	3	286	286	0.475	0.475(?)
		4	715	715	0.451	0.451(?)
		5	1287	1287	0.416	0.416(?)
		6	1716	1716	0.472	0.472(?)
		7	1716	1716	0.489	0.489(?)
		8	1287	1287	0.482	0.482(?)
		9	715	715	0.535	0.535(?)
		10	286	286	0.606	0.606(?)
		11	78	78	0.685	0.685(?)
		12	13	13	0.763	0.763(?)
		13	1	1	0.835	0.835(?)

When compared with the 18-run OA from XCW for $t \leq 7$, the FBBDs clearly outperform in terms of $\overline{D}$ with the exception of the lowest order projections for $t = 4, 5$. Although the FBBDs for $t = 5 - 7$ can project onto any 5 factors, it is an unfair comparison to say that they *outperform* the 18-run OA in terms of number of eligible projections given that this design does not have enough runs to estimate a second-order model in 5 factors (21 runs are needed).

Table 7: Eligible Projections and Average D-Efficiencies of XCW's Recommended 18-run OA

t	Projection	Number of Projections	Number of Eligible Projections	$\overline{D}$
4	2	6	6	0.725
	3	4	4	0.589
	4	1	1	0.465
5	3	10	10	0.589
	4	5	5	0.465
	5	1	0	-
6	3	20	20	0.589
	4	15	15	0.465
	5	6	0	-
7	3	35	34	0.579
	4	35	31	0.445
	5	21	0	-

The recommended 27-run OA from XCW can screen up to 13-factors and all projections are eligible for up to 5 factors. However, this design's inability to project beyond 5-factors and its complex aliasing scheme make it difficult to successfully use in practical situations. For $t \leq 7$, where run sizes are most comparable, the FBBDs have better average D-efficiency for three factor projections for $t = 4, 6, 7$, four-factor projections for t=4-7, and five-factor projections for t=5-7. When $t \geq 9$, FBBDs have higher D-efficiencies for five-factor projections. Although we cannot compare the designs beyond 5-factor projections, it is worthy to note that $\overline{D}$ is monotonically decreasing as the projection size increases for the 27-run OA. In contrast, the FBBDs show no such monotonicity for the lower-order projections, but do appear to have a monotonically increasing $\overline{D}$ for projections beyond 5-factors.

As expected, the D-optimal designs have similar or superior efficiency for higher order projections. For instance, when t=10, the D-optimal 81-run design has higher average efficiency for 7-10 factor projections. On the other hand, the FBBDs have higher average efficiency for lower order projections.

Table 8: Eligible Projections and Average D-Efficiencies of XCW's Recommended 27-run OA

t	Projection	Number of Projections	Number of Eligible Projections	$\overline{D}$
4	2	6	6	0.725
	3	4	4	0.616
	4	1	1	0.555
5	3	10	10	0.607
	4	5	5	0.527
	5	1	1	0.445
6	3	20	20	0.601
	4	15	15	0.512
	5	6	6	0.408
7	3	35	35	0.598
	4	35	35	0.505
	5	21	21	0.407
8	3	56	56	0.597
	4	70	70	0.502
	5	56	56	0.402
9	3	84	84	0.596
	4	126	126	0.501
	5	126	126	0.401
10	3	120	120	0.595
	4	210	210	0.498
	5	252	252	0.396
11	3	165	165	0.594
	4	330	330	0.496
	5	462	462	0.393
12	3	220	220	0.593
	4	495	495	0.495
	5	792	792	0.391
13	3	286	286	0.593
	4	715	715	0.494
	5	1287	1287	0.390

Analysis Strategy

In this section, we introduce a simple graphical analysis strategy for $\frac{1}{2}BB6$, $\frac{1}{2}BB7$, and $\frac{1}{2}BB9.1$ (i.e. those FBBDs that cannot estimate the full second-order model). The following assumptions are needed:

- 1. *Effect Hierarchy:* Main effects are more likely to be important than interaction effects and effects of the same order are equally important.
- 2. *Effect Sparsity:* The number of important effects is relatively small.
- 3. *Weak Effect Heredity:* An interaction is more likely to be important if one or more of its parent factors is also important.

By constructing these designs in the manner discussed above, we obtain r independent estimates of each factor's linear main effect. Let γ_{ij}, $i = 1,2,...,r$; $j = 1,2,...,t$ be the i^{th} estimate of the j^{th} factor's linear main effect and Γ be the $r \times t$ matrix with ij^{th} element γ_{ij}. A pseudo-estimate of each linear main effect is obtained by computing the median of each column of Γ, denoted as m_j. Let δ_{ij} denote the deviation of γ_{ij} from m_j. If effect sparsity holds, many of the δ_{ij}'s are expected to be close to zero while large deviations from m_j likely represent the presence of two-factor interactions.

We therefore recommend a simple graphical method that plots the m_j's on the x-axis and the δ_{ij}'s on the y-axis labeled with its corresponding interaction. Factors with m_j's that stand away from zero would tend to indicate the importance of a linear main effect, whereas a δ_{ij} that deviates from zero indicates the importance of a two-factor interaction. Note that unbiased estimates of the quadratic main effects can be obtained using ordinary least squares and can be evaluated using a normal plot of effects.

Example- Simulated Data

In this example, consider a 9-factor, 49-run FBBD with aliasing shown in Table 9. Suppose we simulate from the following model:

$$Y = 2x_A - 1.5x_E + 2x_G + 2.5x_{E^2} - 3x_{H^2} + 4x_{AC} - 5x_{CG} + 3.5x_{EH} - 4x_{GH} + \varepsilon \quad (7)$$

where $\varepsilon \sim N(0,1)$.

Table 9: Aliasing of 49-run, 9-factor FBBD

Linear Main Effect	Aliasing
A	+ B*C + D*G + F*H + E*J
B	+ A*C + F*G + E*H + D*J
C	+ A*B + E*G + D*H + F*J
D	+ E*F + A*G + C*H + B*J
E	+ D*F + C*G + B*H + A*J
F	+ D*E + B*G + A*H + C*J
G	+ A*D + C*E + B*F + H*J
H	+ C*D + B*E + A*F + G*J
J	+ B*D + A*E + C*F + G*H

Based on Figure 1, factors A, B, E, and G appear to stand out as important along with C*G, G*H, A*C, E*H, F*G, and D*J. A normal plot of the quadratic main effects (not shown) shows evidence for E^2 and H^2.

By relying on the likely linear and quadratic main effects identified above and the weak effect heredity assumption, one may be able to disregard certain interactions aliased with B, E, and J. For instance, only D*J does not satisfy weak effect heredity. Thus, we proceed to project the FBBD onto factors A, C, E, F, G, and H and fit a second-order model in these six factors, obtaining an R^2 of 97.4% with the important effects clearly identified (we omit the fitted model). It is worthwhile to note that upon simulating data from the same model and conducting a main effects only analysis using the 27-run OA of XCW, no linear or quadratic main effects were detected at $\alpha = 0.1$. In fact, the smallest p-value is 0.143 for

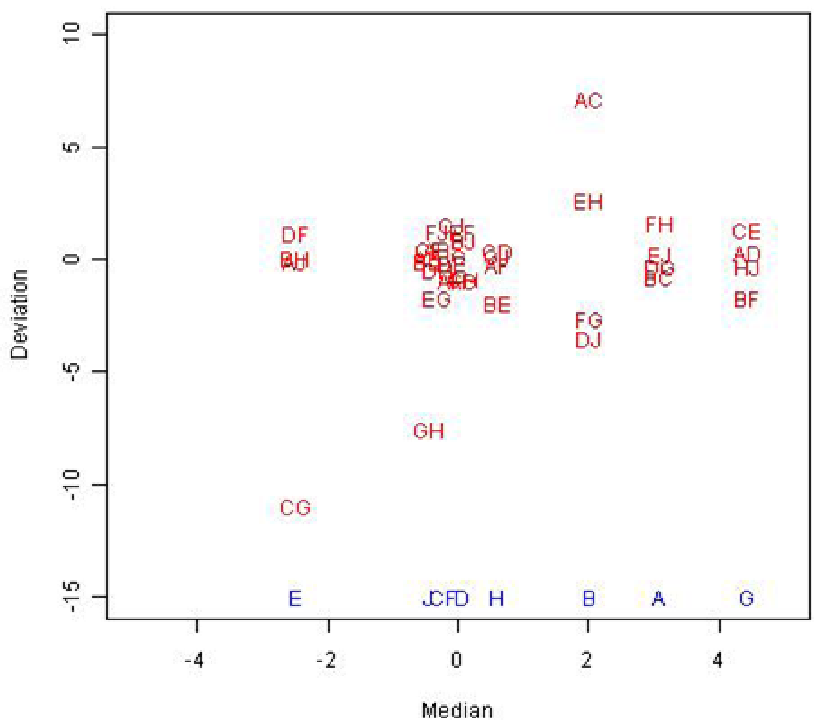

Figure 1: Simulated Example - δ_{ij} vs. m_j

the quadratic main effect of factor A. Thus, it is clear that their recommended design would be unsuccessful for this model.

Example - Real Data

This example involves data from Alvarez et al. (1988), where the objective was to optimize a very-large-scale-integrated (VLSI) process, device, and circuit design using a six factor, 50-run Box-Behnken design. According to Rubin (1987), "such circuits are becoming increasingly common due to their ease of manufacture, low cost, and simplified design methodologies."

Referring simply to their factors as A-F, the authors identify the main effects (both linear and quadratic) of factors D and E and the D*E interaction as active. Table 2 of their paper displays only the first block of the 50-run Box-Behnken design used for their experiment (the remaining half is omitted from the article). We suspect they do so for brevity as no other explanation is provided. Regardless, we intend to use only the available block (which happens to be a $\frac{1}{2}BB6$) to illustrate the proposed analysis strategy.

The 25-run FBBD and response are given in Table 10 and the aliasing is provided in Table 11. Figure 2 indicates the following effects: D, E, D*E, B*E, A*E, A*D. A normal plot of quadratic effects points toward D^2 and E^2 as likely. Hence, all of the identified interactions satisfy weak effect heredity.

The 6-factor FBBD is projected onto factors A, B, D, and E and a second-order model is fit. The results are given in Table 12. This fitted model has an R^2=0.9977 and identifies D, E, D^2, E^2, A*E, and D*E at a α=0.01 level of significance. A reduced model with only these five terms produces an R^2 of 99.2%. Therefore, we are able to identify the same effects as Alvarez et al. (1988) in half as many runs.

Table 10: Design and Response for VLSI Data

A	B	C	D	E	F	Y
1	1	0	-1	0	0	107.80
1	-1	0	1	0	0	44.46
-1	1	0	1	0	0	45.01
-1	-1	0	-1	0	0	109.32
0	1	1	0	-1	0	84.08
0	1	-1	0	1	0	65.36
0	-1	1	0	1	0	66.25
0	-1	-1	0	-1	0	83.58
0	0	1	1	0	-1	45.30
0	0	1	-1	0	1	112.40
0	0	-1	1	0	1	45.51
0	0	-1	-1	0	-1	107.15
1	0	0	1	-1	0	58.84
1	0	0	-1	1	0	117.76
-1	0	0	1	1	0	45.31
-1	0	0	-1	-1	0	153.08
0	1	0	0	1	-1	65.05
0	1	0	0	-1	1	89.93
0	-1	0	0	1	1	65.25
0	-1	0	0	-1	-1	79.98
1	0	1	0	0	-1	64.12
1	0	-1	0	0	1	64.03
-1	0	1	0	0	1	65.19
-1	0	-1	0	0	-1	63.40
0	0	0	0	0	0	63.07

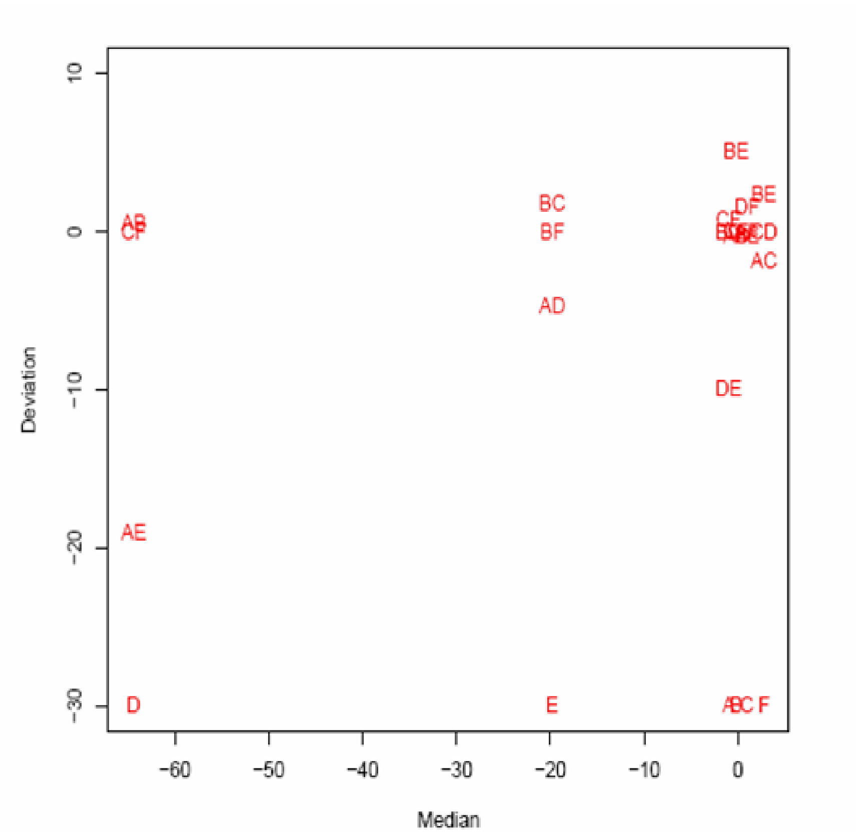

Figure 2: VLSI Data - δ_{ij} vs. m_j

Table 11: Aliasing of VLSI Data FBBD

Linear Main Effect	Aliasing
A	-B*D -D*E -C*F
C	-B*E -A*F -D*F
B	-A*D -C*E -E*F
D	-A*B -C*F -A*E
E	-B*C -A*D -B*F
F	-C*D -B*E -A*C

Table 12: Second-Order Analysis Results for VLSI Data

Term	Estimate	Std Error	t Ratio	P-value
Intercept	62.781	0.975	64.42	.000
A	-0.110	1.025	-0.11	0.917
B	1.394	0.662	2.11	0.062
D	-32.185	1.025	-31.39	.000
E	-9.681	0.662	-14.63	.000
A*A	1.452	0.908	1.6	0.141
A*B	-0.273	1.450	-0.19	0.855
B*B	-2.467	0.908	-2.72	0.022
A*D	2.084	0.888	2.35	0.041
B*D	0.408	1.450	0.28	0.784
D*D	14.857	0.908	16.35	.0001
A*E	9.488	1.450	6.54	.0001
B*E	-1.443	0.725	-1.99	0.075
D*E	5.338	1.450	3.68	0.004
E*E	14.633	0.908	16.11	.0001

Discussion

In this article, we have proposed the use of fractions of Box-Behnken designs for the purpose of conducting response surface exploration. In particular, Fractional Box-Behnken Designs (FBBDs) are three-level designs based on BIBDs or RG designs with $\lambda_i \geq 1$. In particular, we construct these designs with subsets of the two-level designs that make up a BBD. For FBBDs that are unable to estimate the full second-order model, our construction method allows for r independent estimates of each factor's linear main effect. Since each of the rt estimates is aliased with one two-factor interaction, each linear main effect is partially aliased with r two-factor interactions. Although the restriction to RG and BIBDs requires more runs (especially for $t \geq 10$) than the CW/XCW designs, their simple construction and aliasing structure allows for an improved analysis that cannot be applied to the CW/XCW designs.

One suggestion for constructing FBBDs with smaller run sizes (which thus forfeit the ability to estimate the second-order model in all t factors) would be to utilize RG designs with some $\lambda_i = 0$. For instance, rather than requiring the 10-factor FBBD to have 81 runs, it would be possible to construct one with 41 distinct runs using a RG design with $r = 3$, $k = 3$, and $b = 10$. The analysis of these smaller designs could presumably be performed similarly to that proposed in the previous section. Note that if two factors of interest do not appear together in any subset of 4-runs, their respective two-factor interaction will not be estimable. Thus, augmentation with additional runs to allow such estimation will be required. On the other hand, if the practitioner has some prior knowledge regarding which interactions may be most important, then those factors could be assigned to columns in such a way to guarantee that they appear together in at least one subset of runs. This may prove to be a worthy topic of future research.

When compared to existing designs in terms of number of eligible projections and average D-efficiency of eligible projections, we see that the FBBDs compete well with its counterparts. The increased eligibility of projections is one advantage of FBBDs, especially when factor sparsity is not expected to be valid. Furthermore, FBBDs are more intuitive than optimal designs as they possess a simple structure and do not require the use of software to construct. Comparisons in terms of D-efficiency show favorable results for both the OAs and the FBBDs depending on the number of factors and projection size. These results provide us with an indication that the CW/XCW designs do possess reasonable model discrimination ability provided that the the number of important factors is known.

Simulation results (not shown) indicate, however, that the FBBDs are superior to the CW/XCW designs in terms of their ability to identify important factors when performing a main-effects only analysis. For instance, for $t = 7$ factors (three of which are important) and a error standard deviation of $\sigma = 2$, the 27-run OA can only identify the three important factors 21.4% of the time whereas the FBBD can do so 88.1% of the time.

A useful graphical tool is also proposed for analyzing FBBDs that cannot estimate the second-order model (see Figures 1 and 2); the plot displays the median of the r estimates for each linear main effect on the x-axis and each estimate's deviation from this median (δ_{ij}) on the y-axis. Deviations far from zero tend to indicate the presence of two-factor interactions. As an alternative to the plot, a more objective approach might entail conducting Lenth-type tests based on the δ_{ij}'s and the development of critical values for testing groups of interactions aliased with main effects. This method is currently under investigation.

Ambiguity in any analysis could lead to the unfortunate consequence of improperly identifying important main effects or other interactions. However, there is some expectation that the practitioner will consider the use of confirmation runs or follow-up experimentation to verify results. Since the aliasing of the FBBDs is simple, it is easy to choose additional runs to resolve any previous confusion regarding identification of active factors. Augmentation of FBBDs is straightforward since one need only complete runs in a two-level fractional factorial. Furthermore, the ease of augmentation makes the prospect of using FBBDs in a multi-stage approach to response surface exploration plausible.

By proposing designs that can be easily constructed, possess a simple aliasing structure, and have a straightforward analysis method, we hope that practitioners will find the FBBDs a useful alternative to the existing designs recommended for screening and response surface exploration.

Appendix

Table A1: Some Recommended FBBDs

Design	Design Matrix & Construction
$\frac{3}{4}$BB4	$\begin{array}{r} 3(2_I^{2-2}) \\ \text{exclude } I{=}-A{=}B{=}-AB\rightarrow \\ \text{exclude } I{=}-C{=}D{=}-CD\rightarrow \\ \text{exclude } I{=}-A{=}D{=}-AD\rightarrow \\ \text{exclude } I{=}-B{=}C{=}-BC\rightarrow \\ \text{exclude } I{=}A{=}-C{=}-AC\rightarrow \\ \text{exclude } I{=}B{=}-D{=}-BD\rightarrow \\ \end{array} \begin{array}{c} \begin{array}{cccc} A & B & C & D \end{array} \\ \left[\begin{array}{cccc} \pm1 & \pm1 & 0 & 0 \\ 0 & 0 & \pm1 & \pm1 \\ \pm1 & 0 & 0 & \pm1 \\ 0 & \pm1 & \pm1 & 0 \\ \pm1 & 0 & \pm1 & 0 \\ 0 & \pm1 & 0 & \pm1 \\ 0 & 0 & 0 & 0 \end{array}\right] \end{array}$
$\frac{1}{2}$BB5	$\begin{array}{r} 2_I^{2-1} \\ I{=}A\rightarrow \\ I{=}-A\rightarrow \\ I{=}B\rightarrow \\ I{=}-B\rightarrow \\ I{=}C\rightarrow \\ I{=}-C\rightarrow \\ I{=}D\rightarrow \\ I{=}-D\rightarrow \\ I{=}E\rightarrow \\ I{=}-E\rightarrow \\ \end{array} \begin{array}{c} \begin{array}{ccccc} A & B & C & D & E \end{array} \\ \left[\begin{array}{ccccc} \pm1 & \pm1 & 0 & 0 & 0 \\ \pm1 & 0 & \pm1 & 0 & 0 \\ 0 & \pm1 & \pm1 & 0 & 0 \\ 0 & \pm1 & 0 & 0 & \pm1 \\ 0 & 0 & \pm1 & 0 & \pm1 \\ 0 & 0 & \pm1 & \pm1 & 0 \\ \pm1 & 0 & 0 & \pm1 & 0 \\ 0 & \pm1 & 0 & \pm1 & 0 \\ \pm1 & 0 & 0 & 0 & \pm1 \\ 0 & 0 & 0 & \pm1 & \pm1 \\ 0 & 0 & 0 & 0 & 0 \end{array}\right] \end{array}$
$\frac{1}{2}$BB6	$\begin{array}{r} 2_{III}^{3-1} \\ D{=}AB\rightarrow \\ E{=}AD\rightarrow \\ E{=}BC\rightarrow \\ F{=}BE\rightarrow \\ F{=}CD\rightarrow \\ F{=}AC\rightarrow \\ \end{array} \begin{array}{c} \begin{array}{cccccc} A & B & C & D & E & F \end{array} \\ \left[\begin{array}{cccccc} \pm1 & \pm1 & 0 & \pm1 & 0 & 0 \\ \pm1 & 0 & 0 & \pm1 & \pm1 & 0 \\ 0 & \pm1 & \pm1 & 0 & \pm1 & 0 \\ 0 & \pm1 & 0 & 0 & \pm1 & \pm1 \\ 0 & 0 & \pm1 & \pm1 & 0 & \pm1 \\ \pm1 & 0 & \pm1 & 0 & 0 & \pm1 \\ 0 & 0 & 0 & 0 & 0 & 0 \end{array}\right] \end{array}$
$\frac{3}{4}$BB6	$\begin{array}{r} 3(2_I^{3-2}) \\ \text{exclude } I{=}-A{=}D{=}-AD\rightarrow \\ \text{exclude } I{=}A{=}-D{=}-AD\rightarrow \\ \text{exclude } I{=}-B{=}E{=}-BE\rightarrow \\ \text{exclude } I{=}B{=}-E{=}-BE\rightarrow \\ \text{exclude } I{=}-C{=}F{=}-CF\rightarrow \\ \text{exclude } I{=}C{=}-F{=}-CF\rightarrow \\ \end{array} \begin{array}{c} \begin{array}{cccccc} A & B & C & D & E & F \end{array} \\ \left[\begin{array}{cccccc} \pm1 & \pm1 & 0 & \pm1 & 0 & 0 \\ \pm1 & 0 & 0 & \pm1 & \pm1 & 0 \\ 0 & \pm1 & \pm1 & 0 & \pm1 & 0 \\ 0 & \pm1 & 0 & 0 & \pm1 & \pm1 \\ 0 & 0 & \pm1 & \pm1 & 0 & \pm1 \\ \pm1 & 0 & \pm1 & 0 & 0 & \pm1 \\ 0 & 0 & 0 & 0 & 0 & 0 \end{array}\right] \end{array}$
$\frac{1}{2}$BB7	$\begin{array}{r} 2_{III}^{3-1} \\ D{=}AB\rightarrow \\ E{=}BC\rightarrow \\ F{=}CD\rightarrow \\ G{=}DE\rightarrow \\ F{=}AE\rightarrow \\ G{=}BF\rightarrow \\ G{=}AC\rightarrow \\ \end{array} \begin{array}{c} \begin{array}{ccccccc} A & B & C & D & E & F & G \end{array} \\ \left[\begin{array}{ccccccc} \pm1 & \pm1 & 0 & \pm1 & 0 & 0 & 0 \\ 0 & \pm1 & \pm1 & 0 & \pm1 & 0 & 0 \\ 0 & 0 & \pm1 & \pm1 & 0 & \pm1 & 0 \\ 0 & 0 & 0 & \pm1 & \pm1 & 0 & \pm1 \\ \pm1 & 0 & 0 & 0 & \pm1 & \pm1 & 0 \\ 0 & \pm1 & 0 & 0 & 0 & \pm1 & \pm1 \\ \pm1 & 0 & \pm1 & 0 & 0 & 0 & \pm1 \\ 0 & 0 & 0 & 0 & 0 & 0 & 0 \end{array}\right] \end{array}$

Table A1 – Continued

Design	Design Matrix & Construction	A	B	C	D	E	F	G	H	J
$\frac{3}{4}$BB7	$3(2_I^{3-2})$	A	B	C	D	E	F	G		
	exclude $I{=}{-}A{=}B{=}{-}AB\rightarrow$	±1	±1	0	±1	0	0	0		
	exclude $I{=}B{=}{-}E{=}{-}BE\rightarrow$	0	±1	±1	0	±1	0	0		
	exclude $I{=}C{=}{-}D{=}{-}CD\rightarrow$	0	0	±1	±1	0	±1	0		
	exclude $I{=}{-}E{=}G{=}{-}EG\rightarrow$	0	0	0	±1	±1	0	±1		
	exclude $I{=}{-}A{=}E{=}{-}AE\rightarrow$	±1	0	0	0	±1	±1	0		
	exclude $I{=}B{=}{-}G{=}{-}BG\rightarrow$	0	±1	0	0	0	±1	±1		
	exclude $I{=}A{=}{-}G{=}{-}AG\rightarrow$	±1	0	±1	0	0	0	±1		
		0	0	0	0	0	0	0		
$\frac{1}{2}$BB9.1	2_{III}^{3-1}	A	B	C	D	E	F	G	H	J
	$C{=}AB\rightarrow$	±1	±1	±1	0	0	0	0	0	0
	$G{=}AD\rightarrow$	±1	0	0	±1	0	0	±1	0	0
	$J{=}AE\rightarrow$	±1	0	0	0	±1	0	0	0	±1
	$H{=}AF\rightarrow$	±1	0	0	0	0	±1	0	±1	0
	$J{=}BD\rightarrow$	0	±1	0	±1	0	0	0	0	±1
	$H{=}BE\rightarrow$	0	±1	0	0	±1	0	0	±1	0
	$G{=}BF\rightarrow$	0	±1	0	0	0	±1	±1	0	0
	$H{=}CD\rightarrow$	0	0	±1	±1	0	0	0	±1	0
	$G{=}CE\rightarrow$	0	0	±1	0	±1	0	±1	0	0
	$J{=}CF\rightarrow$	0	0	±1	0	0	±1	0	0	±1
	$F{=}DE\rightarrow$	0	0	0	±1	±1	±1	0	0	0
	$J{=}FG\rightarrow$	0	0	0	0	0	0	±1	±1	±1
		0	0	0	0	0	0	0	0	0
$\frac{3}{4}$BB9.1	$3(2_I^{3-2})$	A	B	C	D	E	F	G	H	J
	exclude $I{=}{-}A{=}B{=}{-}AB\rightarrow$	±1	±1	±1	0	0	0	0	0	0
	exclude $I{=}{-}A{=}D{=}{-}AD\rightarrow$	±1	0	0	±1	0	0	±1	0	0
	exclude $I{=}{-}A{=}J{=}{-}AJ\rightarrow$	±1	0	0	0	±1	0	0	0	±1
	exclude $I{=}A{=}{-}H{=}{-}AH\rightarrow$	±1	0	0	0	0	±1	0	±1	0
	exclude $I{=}B{=}{-}J{=}{-}BJ\rightarrow$	0	±1	0	±1	0	0	0	0	±1
	exclude $I{=}B{=}{-}H{=}{-}BH\rightarrow$	0	±1	0	0	±1	0	0	±1	0
	exclude $I{=}{-}F{=}G{=}{-}FG\rightarrow$	0	±1	0	0	0	±1	±1	0	0
	exclude $I{=}{-}D{=}H{=}{-}DH\rightarrow$	0	0	±1	±1	0	0	0	±1	0
	exclude $I{=}{-}E{=}G{=}{-}EG\rightarrow$	0	0	±1	0	±1	0	±1	0	0
	exclude $I{=}F{=}{-}J{=}{-}FJ\rightarrow$	0	0	±1	0	0	±1	0	0	±1
	exclude $I{=}E{=}{-}F{=}{-}EF\rightarrow$	0	0	0	±1	±1	±1	0	0	0
	exclude $I{=}H{=}{-}J{=}{-}HJ\rightarrow$	0	0	0	0	0	0	±1	±1	±1
		0	0	0	0	0	0	0	0	0
$\frac{1}{2}$BB9.2	2_{III}^{4-1}	A	B	C	D	E	F	G	H	J
	$D{=}AB\rightarrow$	±1	±1	0	±1	±1	0	0	0	0
	$E{=}BC\rightarrow$	0	±1	±1	0	±1	±1	0	0	0
	$F{=}CD\rightarrow$	0	0	±1	±1	0	±1	±1	0	0
	$G{=}DE\rightarrow$	0	0	0	±1	±1	0	±1	±1	0
	$H{=}EF\rightarrow$	0	0	0	0	±1	±1	0	±1	±1
	$J{=}FG\rightarrow$	±1	0	0	0	0	±1	±1	0	±1
	$H{=}AG\rightarrow$	±1	±1	0	0	0	0	±1	±1	0
	$J{=}BH\rightarrow$	0	±1	±1	0	0	0	0	±1	±1
	$J{=}AC\rightarrow$	±1	0	±1	±1	0	0	0	0	±1
		0	0	0	0	0	0	0	0	0

Table A1 – Continued

Design	2^{4-1}_{III}	A	B	C	D	E	F	G	H	J	K
$\frac{1}{2}$BB10	$G=BF\rightarrow$	0	±1	0	0	0	±1	±1	0	0	±1
	$K=AB\rightarrow$	±1	±1	0	0	±1	0	0	0	0	±1
	$G=BC\rightarrow$	0	±1	±1	0	0	0	±1	±1	0	0
	$J=DF\rightarrow$	0	±1	0	±1	0	±1	0	0	±1	0
	$K=AH\rightarrow$	±1	0	0	0	0	0	0	±1	±1	±1
	$K=CE\rightarrow$	0	0	±1	±1	±1	0	0	0	0	±1
	$H=DG\rightarrow$	±1	0	0	±1	0	0	±1	±1	0	0
	$J=CE\rightarrow$	0	0	±1	0	±1	0	±1	0	±1	0
	$J=AF\rightarrow$	±1	0	±1	0	0	±1	0	0	±1	0
	$H=DE\rightarrow$	0	0	0	±1	±1	±1	0	±1	0	0
		0	0	0	0	0	0	0	0	0	0

Design	2^{4-1}_{III}	A	B	C	D	E	F	G	H	J	K	L
$\frac{1}{2}$BB11	$C=AB\rightarrow$	±1	±1	±1	0	0	±1	0	0	0	0	0
	$D=BC\rightarrow$	0	±1	±1	±1	0	0	±1	0	0	0	0
	$E=CD\rightarrow$	0	0	±1	±1	±1	0	0	±1	0	0	0
	$F=DE\rightarrow$	0	0	0	±1	±1	±1	0	0	±1	0	0
	$G=EF\rightarrow$	0	0	0	0	±1	±1	±1	0	0	±1	0
	$H=FG\rightarrow$	0	0	0	0	0	±1	±1	±1	0	0	±1
	$J=GH\rightarrow$	±1	0	0	0	0	0	±1	±1	±1	0	0
	$K=HJ\rightarrow$	0	±1	0	0	0	0	0	±1	±1	±1	0
	$L=JK\rightarrow$	0	0	±1	0	0	0	0	0	±1	±1	±1
	$L=AK\rightarrow$	±1	0	0	±1	0	0	0	0	0	±1	±1
	$L=AB\rightarrow$	±1	±1	0	0	±1	0	0	0	0	0	±1
		0	0	0	0	0	0	0	0	0	0	0

Design	2^{4-1}_{III}	A	B	C	D	E	F	G	H	J	K	L	M
$\frac{1}{2}$BB12	$H=AB\rightarrow$	±1	±1	0	±1	0	0	0	±1	0	0	0	0
	$J=BC\rightarrow$	0	±1	±1	0	±1	0	0	0	±1	0	0	0
	$K=CD\rightarrow$	0	0	±1	±1	0	±1	0	0	0	±1	0	0
	$L=DE\rightarrow$	0	0	0	±1	±1	0	±1	0	0	0	±1	0
	$M=EF\rightarrow$	0	0	0	0	±1	±1	0	±1	0	0	0	±1
	$G=AF\rightarrow$	±1	0	0	0	0	±1	±1	0	±1	0	0	0
	$H=BG\rightarrow$	0	±1	0	0	0	0	±1	±1	0	±1	0	0
	$J=CH\rightarrow$	0	0	±1	0	0	0	0	±1	±1	0	±1	0
	$K=DJ\rightarrow$	0	0	0	±1	0	0	0	0	±1	±1	0	±1
	$L=EK\rightarrow$	±1	0	0	0	±1	0	0	0	0	±1	±1	0
	$M=FL\rightarrow$	0	±1	0	0	0	±1	0	0	0	0	±1	±1
	$M=AG\rightarrow$	±1	0	±1	0	0	0	±1	0	0	0	0	±1
		0	0	0	0	0	0	0	0	0	0	0	0

Design	2^{4-1}_{III}	A	B	C	D	E	F	G	H	J	K	L	M	N
$\frac{1}{2}$BB13	$D=AB\rightarrow$	±±1	±1	0	±1	0	0	0	0	0	±1	0	0	0
	$J=AC\rightarrow$	±1	0	±1	0	0	0	0	0	±1	0	0	0	±1
	$F=AE\rightarrow$	±1	0	0	0	±1	±1	0	±1	0	0	0	0	0
	$M=GL\rightarrow$	±1	0	0	0	0	0	±1	0	0	0	±1	±1	0
	$E=BC\rightarrow$	0	±1	±1	0	±1	0	0	0	0	0	±1	0	0
	$G=BF\rightarrow$	0	±1	0	0	0	±1	±1	0	±1	0	0	0	0
	$N=HM\rightarrow$	0	±1	0	0	0	0	0	±1	0	0	0	±1	±1
	$F=CD\rightarrow$	0	0	±1	±1	0	±1	0	0	0	0	0	±1	0
	$K=GH\rightarrow$	0	0	±1	0	0	0	±1	±1	0	±1	0	0	0
	$N=DE\rightarrow$	0	0	0	±1	±1	0	±1	0	0	0	0	0	±1
	$L=HJ\rightarrow$	0	0	0	±1	0	0	0	±1	±1	0	±1	0	0
	$M=JK\rightarrow$	0	0	0	0	±1	0	0	0	±1	±1	0	±1	0
	$N=KL\rightarrow$	0	0	0	0	0	±1	0	0	0	±1	±1	0	±1
		0	0	0	0	0	0	0	0	0	0	0	0	0

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