Selection Between Variance-Optimal and Bias-Optimal Designs when Some Two-Factor Interactions are Important

by

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Abstract

Fractional factorial designs are useful for collecting data in many fields of studies because they allow us to study the effects of many factors on the response. As the primary interest of most experiments is for screening important factors, interactions are generally assumed to be negligible. When some two-factor interactions are important, variance-optimal designs and bias-optimal designs are available. In this study, we compare these two types of designs by using a mean squared error criterion that takes effect sparsity into consideration. We obtain a closed-form expression of this mean squared error criterion for the two types of designs. Under different levels of sparsity, results are obtained for designs of 10, 12, 14, 20, 26, 28 runs, which will help practitioners to choose between the two types of designs.

Keywords: Effect sparsity; foldover design; mean squared error criterion; orthogonal array

Dedication

To my dearest and the loveliest lady, my grandma, Peiyin Lin.

Acknowledgements

Praise to my God, the Almighty, who is my strength and my shield! Thank You for Your unconditional love that I could be able to do everything in You. I would also like to express my deepest appreciation to my supervisor, Dr. Boxin Tang, for his constant support over the past few years during my undergraduate and master's study at SFU. I am thankful for his guidance and effort in helping me through my graduate and my project. A special gratitude to Guanzhou Chen, who provided me with his kind help when I encountered with difficulties on my project. Further, an enormous 'Thank You' to the wonderful instructors I have worked with closely, Dr. Lloyd Elliott and Ian Bercovitz, for their encouragements and patience in guiding me at each step of work. I especially want to thank Dr. Rachel Altman for spending time not only helping me with my schoolwork, but also providing me with helpful suggestions in my life. Also, I sincerely thank Sadika Jungic for encouraging me to ask for every opportunity that help achieve my goal, and thanks to Charlene Bradbury for her generous help. Thank you to all the faculty members and staffs in our department that made my time at SFU so comfortable as home. From outside the department, a thank you to Dr. Wan-Cheng Tan for her guidance in maturing my writing skill and improving my professions at work. Last but not least, the biggest thank you for the love and support from my family, my church family and friends, and my best friend and partner, Shanzhao.

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Chapter 1

Introduction

Factorial designs are useful for examining which factors have primary effects on a response variable in a factor-screening experiment (Cheng, 2016). In general, a two-level factorial design consists of two or more factors, where each factor has two levels represented by ± 1. Each combination of levels of the factors is called a treatment or a run, and the change caused by a treatment or a run to the response is called an effect. A full factorial design consists of all possible combinations of the factors, and it requires a run size to be a power of two when there are two levels per factor. We use m to denote the number of factors in a two-level factorial design. Given that m factors are to be studied, at least 2^m runs are needed in order to estimate all $2^m - 1$ effects. However, higher-order interaction effects are generally not expected to be important in a screening experiment. Fractional factorial designs are more popular because they allow us to study the effects of multiple factors using only a fraction of runs from a full factorial design. One consequence of using a fractional factorial design is that some effects may not be distinguishable from other effects, and this is known as aliasing or confounding. There are two common ways to construct fractional factorial designs, which give rise to regular and nonregular designs.

A regular design is generally referred to as a 2^{m-p} design, where 2^{-p} indicates the proportion of a full factorial design to be used. The main idea in constructing a fractional factorial design is to use a set of generators. For example, in a 2^{5-2} design, only one fourth (2^{-2}) of 32 runs of a full 2^5 factorial design will be used. The process of creating a 2^{5-2} fractional factorial design begins with first assigning three letters A, B, C to three of the five factors and then writing out a full factorial design of eight runs of these three factors. For the two remaining factors, we use generators D=AB and E=AC, meaning that the column for factor D is obtained by multiplying the elements in the columns of A and B, and the column for factor E is obtained by multiplying columns A and C. The effects in a 2^{m-p} fractional factorial design are either orthogonal or fully aliased.

Nonregular designs such as Plackett-Burman designs (Plackett & Burman, 1946) are orthogonal arrays, which can be obtained by selecting columns from a Hadamard matrix. A Hadamard matrix is a square matrix of entries ±1 with orthogonal columns. For example, to create an orthogonal array for five factors and eight runs, we select any five columns from an 8×8 Hadamard matrix with an exception of a column of all $+1$'s. The main difference between a regular design and a nonregular design is that, for a nonregular design, two effects can be partially aliased. The run sizes of nonregular designs are more flexible than regular designs, and they only need to be multiples of four.

We consider the problem of estimating main effects, but allow possible existence of two-factor interactions (2fis). In this case, orthogonal arrays and nearly orthogonal arrays are variance-optimal but not bias-optimal. On the other hand, non-orthogonal foldover designs as considered by Margolin (1969) and further studied by Miller and Sitter (2005) are bias-optimal but not variance-optimal. The thrust of this project is to evaluate and compare these two types of designs using a mean squared error criterion that takes effect sparsity into consideration.

We now give an overview. In Chapter 2, we introduce linear models, discuss orthogonal arrays and nonorthogonal foldover designs, and obtain a closed-form expression of the MSE criterion for comparing the variance-optimal designs and bias-optimal designs. In Chapter 3, we provide our comparison results and discuss our findings. We also include a brief discussion of an alternative design of 14 runs that has a smaller value of the MSE than both of the variance-optimal and bias-optimal designs. Lastly, in Chapter 4, we summarize our study and research findings, and discuss potential future work.

Chapter 2

Methodology

Section 2.1 introduces two linear models that are used in our study. In Section 2.2, we provide construction details and properties of variance-optimal designs, orthogonal arrays and related designs. We discuss bias-optimal designs, those given by foldover designs in Section 2.3. Lastly, an MSE criterion with consideration of sparsity for comparing the two types of designs is presented in Section 2.4.

2.1 Two Linear Models

Consider a fractional factorial design involving $m \geqslant 2$ two-level factors, where the two levels are denoted by -1 or 1. Suppose all interactions are absent. Then a main-effect linear model of n experimental runs can be written as

$$E(Y) = X_1\beta^{(1)}, \tag{2.1}$$

where $Y = (Y_1, \ldots, Y_n)^T$ is a vector containing n responses, $X_1 = [1_n \; D]$ with 1_n being a vector of n +1's and D being a design matrix of n runs for m factors, and $\beta^{(1)} = (\beta_0, \beta_1, \ldots, \beta_m)^T$ is a vector of grand mean and all the main effects. We assume that all observations $Y_1, \ldots, Y_n$ are uncorrelated and have a common variance σ^2. Then $\beta^{(1)}$ can be estimated by using the least square estimator of $\beta^{(1)}$, which is to minimize $\left\| Y - X_1\beta^{(1)} \right\|^2$. The standard linear model theory gives $\hat{\beta}^{(1)} = (X_1^T X_1)^{-1} X_1^T Y$. The expectation of $\beta^{(1)}$ is

$$
\begin{aligned}
E(\hat{\beta}^{(1)}) &= (X_1^T X_1)^{-1} X_1^T E(Y) \\
&= (X_1^T X_1)^{-1} X_1^T X_1 \beta^{(1)} \\
&= \beta^{(1)},
\end{aligned}
$$

showing that $\hat{\beta}^{(1)}$ is unbiased for $\beta^{(1)}$. The variance-covariance matrix of $\hat{\beta}^{(1)}$ is

$$
\begin{aligned}
Var(\hat{\beta}^{(1)}) &= (X_1^T X_1)^{-1} X_1^T Var(Y) [(X_1^T X_1)^{-1} X_1^T]^T \\
&= (X_1^T X_1)^{-1} X_1^T Var(Y) X_1 (X_1^T X_1)^{-1} \\
&= \sigma^2 (X_1^T X_1)^{-1} X_1^T X_1 (X_1^T X_1)^{-1} \\
&= \sigma^2 (X_1^T X_1)^{-1}.
\end{aligned}
$$

The idea of optimal design is to minimize the variance-covariance matrix, which is the same as minimizing $(X_1^T X_1)^{-1}$. Although we cannot minimize the whole matrix of $(X_1^T X_1)^{-1}$, we can minimize some functionals of this matrix. There are many available optimality criteria that assess how good a design is based on different aspects of $(X_1^T X_1)^{-1}$. In our study, the A-optimality criterion, as defined in Definition 1, is used to measure the matrix $(X_1^T X_1)^{-1}$ for its simplicity.

Definition 1. *The A-optimality criterion is the one that minimizes the sum of the variances of the estimated main effects.*

On the other hand, if there are some important 2fis, the true model under D becomes

$$
E(Y) = X_1 \beta^{(1)} + X_2 \beta^{(2)}, \tag{2.2}
$$

where $\beta^{(2)}$ is a vector of all the important 2fis and X_2 is a matrix obtained by collecting corresponding interaction columns of the design matrix D. Although important 2fis may affect the responses, they are not of interest to us. The least square estimator of $\beta^{(1)}$,

$\hat{\beta}^{(1)} = (X_1^T X_1)^{-1} X_1^T Y$ now has an expectation

$$
\begin{aligned}
E(\hat{\beta}^{(1)}) &= (X_1^T X_1)^{-1} X_1^T E(Y) \\
&= (X_1^T X_1)^{-1} X_1^T (X_1 \beta^{(1)} + X_2 \beta^{(2)}) \\
&= (X_1^T X_1)^{-1} X_1^T X_1 \beta^{(1)} + (X_1^T X_1)^{-1} X_1^T X_2 \beta^{(2)} \\
&= \beta^{(1)} + (X_1^T X_1)^{-1} X_1^T X_2 \beta^{(2)},
\end{aligned}
$$

showing that $\hat{\beta}^{(1)}$ is biased under the true model in (2.2), with the bias given by

$$
\begin{aligned}
Bias(\hat{\beta}^{(1)}, \beta^{(1)}) &= E(\hat{\beta}^{(1)}) - \beta^{(1)} \\
&= (X_1^T X_1)^{-1} X_1^T X_2 \beta^{(2)}.
\end{aligned}
$$

However, the variance-covariance matrix of $\hat{\beta}^{(1)}$ is still $\mathrm{Var}(\hat{\beta}^{(1)}) = \sigma^2 (X_1^T X_1)^{-1}$, remaining unchanged and unaffected by the presence of important 2fis. For two competing designs D_1 and D_2 of the same sizes, preference should be given to the one with a smaller MSE for the estimation of all main effects, as given by

$$
MSE = Var^*(\hat{\beta}^{(1)}) + \left\| Bias^*(\hat{\beta}^{(1)}, \beta^{(1)}) \right\|^2, \tag{2.3}
$$

where $Var^*(\hat{\beta}^{(1)}) = \sigma^2 tr(M)$ with M being the matrix obtained by deleting the first row and the first column of $(X_1^T X_1)^{-1}$, and $Bias^*(\hat{\beta}^{(1)}, \beta^{(1)}) = B\beta^{(2)}$ with B being the matrix obtained by deleting the first row of $(X_1^T X_1)^{-1} X_1^T X_2$.

2.2 Orthogonal Arrays and Related Designs

Introduced by Rao (1947), an orthogonal array(OA) of n runs for m factors, with strength t, denoted by $OA(n, 2^m, t)$, is an $n \times m$ matrix of ± 1 such that all level combinations appear exactly the same number of times in each $n \times t$ submatrix. The following is an example of an $OA(8, 2^4, 3)$:

A	B	C	D
1	1	1	1
1	1	−1	−1
1	−1	1	−1
1	−1	−1	1
−1	1	1	−1
−1	1	−1	1
−1	−1	1	1
−1	−1	−1	−1

In this example, the eight triplets, $(1, 1, 1)$, $(1, 1, -1)$, $(1, -1, 1)$, $(1, -1, -1)$, $(-1, 1, 1)$, $(-1, 1, -1)$, $(-1, -1, 1)$, $(-1, -1, -1)$ appear exactly once in any three columns. Furthermore, each column in this array is orthogonal to another column and is also orthogonal to the products of any two columns. This implies that we can obtain unbiased estimates for the main effects even when some 2fis are important. In general, OAs of strength three allow main effects to be estimated with minimum variance and without bias. A disadvantage of OAs of strength three is that the run size needs to be a multiple of eight. For run sizes of 12, 20 and 28 in our study, no OAs of strength three can exist. In this situation, we consider using OAs of strength two, which still optimize the variance, but the estimate of $\hat{\beta}^{(1)}$ is no longer unbiased if not all 2fis are negligible. OAs of strength two are optimal for estimating the main effects, but they require run sizes to be multiples of four. If a design of run size n when n is even but not a multiple of four is desired, we can obtain an optimal design by adding two specific runs to an OA. The resulting nearly orthogonal array (NOA) is still optimal. This result, avaiable in Dey and Mukerjee (1999), is given below.

Lemma 1. *The design obtained by adding two runs of form* $\ell_1 = (1, \ldots, 1)$ *and* $\ell_2 = (1, \ldots, 1, -1, \ldots, -1)$ *to an orthogonal array is universally optimal and hence A-optimal.*

The first run ℓ_1 is a vector containing m $+1$'s. The second run ℓ_2 is a vector of m_1 $+1$'s and m_2 -1's, where m_1 is the largest integer that is smaller than or equal to $m/2$, and

m_2 is the smallest integer that is greater than or equal to $m/2$. For example, in order to construct a 10-run NOA with 5 factors, we first obtain an $OA(8, 2^5, 2)$ matrix by selecting five columns from an 8×8 Hadamard matrix, as displayed below:

A	B	C	D	E
1	1	1	1	1
−1	1	−1	1	−1
1	−1	−1	1	1
−1	−1	1	1	−1
1	1	1	−1	−1
−1	1	−1	−1	1
1	−1	−1	−1	−1
−1	−1	1	−1	1

We then add the two runs, $\ell_1 = (1, 1, 1, 1, 1)$ and $\ell_2 = (1, 1, -1, -1, -1)$ to the OA matrix and obtain an NOA with 10 runs and five two-level factors

A	B	C	D	E
1	1	1	1	1
−1	1	−1	1	−1
1	−1	−1	1	1
−1	−1	1	1	−1
1	1	1	−1	−1
−1	1	−1	−1	1
1	−1	−1	−1	−1
−1	−1	1	−1	1
1	1	1	1	1
1	1	−1	−1	−1.

In our study, we consider OAs of strength two with 12, 20, 28 runs and NOAs with 10, 14 and 26 runs. Since all these designs optimize the variance, we refer them as variance-optimal designs.

2.3 Nonorthogonal Foldover Designs

Variance-optimal designs are flexible in their run sizes and minimize the variance, but their bias may become too large to dominate the MSE. An alternative class of designs are those introduced by Margolin (1969). A Margolin design is obtained by folding over a nonorthogonal resolution III design. Such a nonorthogonal foldover design allows unbiased estimation of main effects under the true model (2.2). According to Miller and Sitter (2005), these foldover designs can sometimes outperform OAs when only a few 2fis are nonnegligible. For convenience, in the following, we refer to such designs as the best foldover designs (BFDs). Miller and Sitter (2005) contains a list of BFDs for small run sizes. A 10-run BFD is shown as follows.

A	B	C	D	E
1	1	1	−1	1
1	1	−1	1	1
1	−1	1	1	1
−1	1	1	1	1
−1	−1	−1	−1	1
−1	−1	−1	1	−1
−1	−1	1	−1	−1
−1	1	−1	−1	−1
1	−1	−1	−1	−1
1	1	1	1	−1

BFDs of 12, 14, 20, 26, 28 runs are provided in the Appendix.

2.4 An MSE Criterion with Sparsity Consideration

Given two designs of the same sizes, one being a variance-optimal design and another being a bias-optimal design, how should practitioners choose one over another? Since our interest lies in the main effects, we use the MSE of the main effect estimator for design comparison. A general expression for this criterion has previously been given in (2.3). BFDs have zero bias, so only the variance contributes the MSE. On the other hand, variance-optimal designs have a bias depending $\beta^{(2)}$, the important 2fis. We deal with unknown $\beta^{(2)}$ using a Bayesian-inspired formulation with effect sparsity consideration. Suppose that there are f important 2fis. We assume that all subsets of f 2fis are equally likely to be significant, and that the significant 2fis follow an $MN(\mathbf{0}, \tau^2\mathbf{I})$. Then the expected MSE, denoted by Δ, can be shown to be

$$\Delta = \sigma^2 tr(M) + \pi\tau^2 K_2, \tag{2.4}$$

where $\pi = \frac{f}{\binom{m}{2}}$ represents the fraction of sparsity, and $K_2 = tr(BB^T)$. Matrices M and B are defined earlier in Section 2.1. This result was previously obtained by Mukejee and Tang (2012) under baseline parameterization. Designs in our study are under the orthogonal parameterization.

Chapter 3

Results

Section 3.1 presents the results of our study. We compare the MSEs of variance-optimal designs and bias-optimal designs in six experiments with n runs and $m = n/2$ factors for $n = 10$, 12, 14, 20, 26, and 28. For each situation, the variance, bias and MSE are first evaluated assuming all 2fis are nonnegligible for many values of $C = \tau/\sigma$, representing various situations for the sizes of 2fis related to σ. This helps us to find a threshold C^* such that variance-optimal designs are better when $C < C^*$ and bias-optimal designs are better when $C > C^*$. We then conduct a comprehensive study by taking effect sparsity into consideration by looking at $\pi = 1, 1/2, 1/4, 1/8, 1/16, 1/32$. In Section 3.2, we construct an alternative design with 14 runs for seven factors that outperforms the other two types of designs for a range of C values.

3.1 Evaluation and Comparison for Variance-optimal Designs and Bias-optimal Designs

For convenience, we take $\sigma = 1$ in our empirical studies. Then $C = \tau/\sigma = \tau$. We compare six pairs of variance-optimal designs and bias-optimal designs of n runs for $m = n/2$ factors with $n = 10$, 12, 14, 20, 26, and 28, by computing the variance, bias and MSE of each design. This is done for seven different values of $\tau = 0.025, 0.05, 0.1, 0.25, 0.5, 1, 2$, assuming all 2fis are important. The results are displayed in Table 3.1.

Table 3.1: Comparison of variance, bias and MSE for OA/NOA and BFD when all 2fis are important

			OA/NOA			**BFD**		
n	**m**	$C = \frac{\tau}{\sigma}$	**Variance**	**Bias**	**MSE**	**Variance**	**Bias**	**MSE**
10	**5**	0.025	0.536	0.004	0.539	0.556	0	0.556
		0.05	0.536	0.014	0.55	0.556	0	0.556
		0.1	0.536	0.057	0.592	0.556	0	0.556
		0.25	0.536	0.353	0.889	0.556	0	0.556
		0.5.	0.536	1.413	1.949	0.556	0	0.556
		1	0.536	5.653	6.189	0.556	0	0.556
		2	0.536	22.612	23.148	0.556	0	0.556
12	**6**	0.025	0.5	0.004	0.504	0.6	0	0.6
		0.05	0.5	0.017	0.517	0.6	0	0.6
		0.1	0.5	0.067	0.567	0.6	0	0.6
		0.25	0.5	0.417	0.917	0.6	0	0.6
		0.5	0.5	1.667	2.167	0.6	0	0.6
		1	0.5	6.667	7.167	0.6	0	0.6
		2	0.5	26.667	27.167	0.6	0	0.6
14	**7**	0.025	0.525	0.006	0.531	0.77	0	0.77
		0.05	0.525	0.025	0.55	0.77	0	0.77
		0.1	0.525	0.1	0.625	0.77	0	0.77
		0.25	0.525	0.625	1.15	0.77	0	0.77
		0.5	0.525	2.5	3.025	0.77	0	0.77
		1	0.525	10	10.525	0.77	0	0.77
		2	0.525	40	40.525	0.77	0	0.77

20	**10**	0.025	0.5	0.013	0.513	0.556	0	0.556
		0.05	0.5	0.053	0.553	0.556	0	0.556
		0.1	0.5	0.211	0.711	0.556	0	0.556
		0.25	0.5	1.32	1.82	0.556	0	0.556
		0.5	0.5	5.28	5.78	0.556	0	0.556
		1	0.5	21.12	21.62	0.556	0	0.556
		2	0.5	84.48	84.98	0.556	0	0.556
26	**13**	0.025	0.513	0.023	0.536	0.52	0	0.52
		0.05	0.513	0.091	0.604	0.52	0	0.52
		0.1	0.513	0.362	0.875	0.52	0	0.52
		0.25	0.513	2.265	2.778	0.52	0	0.52
		0.5	0.513	9.058	9.571	0.52	0	0.52
		1	0.513	36.232	36.745	0.52	0	0.52
		2	0.513	144.928	145.441	0.52	0	0.52
28	**14**	0.025	0.5	0.028	0.528	0.538	0	0.538
		0.05	0.5	0.111	0.611	0.538	0	0.538
		0.1	0.5	0.443	0.943	0.538	0	0.538
		0.25	0.5	2.77	3.27	0.538	0	0.538
		0.5	0.5	11.082	11.582	0.538	0	0.538
		1	0.5	44.327	44.827	0.538	0	0.538
		2	0.5	177.306	177.806	0.538	0	0.538

From Table 3.1, we observe that the MSEs for variance-optimal designs are small at lower values of C. This makes sense when we look back at equation (2.4), where the variance of estimated main effects remains unchanged for both variance-optimal and bias-optimal designs as τ changes, but the bias varies with τ for variance-optimal designs. Thus, when τ

is small, the bias (and correspondingly the MSE) for variance-optimal designs is also small. We see that before C reaches a boundary point, the MSE of a variance-optimal design is smaller than the MSE of a bias-optimal design, and this suggests that designs such as OAs and NOAs are better than BFDs at lower values of C. For example, between the two designs of 14 runs, when $C < 0.25$, the NOA gives lower Δ values than the BFD, and this indicates that practitioners should consider using the NOA as it performs better than the BFD.

But, when sparsity is considered, for instance, suppose 50% of 2fis are nonnegligible, that is when $\pi = 1/2$, the bias of the NOA with seven factors needs to be multiplied by π based on Equation (2.4), which gives a smaller value of Δ than the one with a full set of important 2fis. Table 3.2 provides the C^* values for different designs and different levels of sparsity when $C = \frac{\tau}{\sigma} < C^*$, variance-optimal designs are better than bias-optimal designs. Taking $n = 14$ as an example, C^* values are 0.157, 0.222, and 0.314 for $\pi = 1, 1/2$ and $1/4$, respectively. Figure 3.1 illustrates the comparisons of Δ values for the two types of designs for $n = 14$ when $\pi = 1$, 1/2, 1/4, 1/8, 1/16, 1/32. Figure 3.2 displays the value of C^* as a function of π for designs of 14 runs. The results as shown in Table 3.2 provide a guidance for practitioners to decide which type of design to use.

Table 3.2: C^* values for $\pi = 1$, 1/2, 1/4, 1/8, 1/16, 1/32 with $n = 10$, 12, 14, 20, 26, 28

n	$\pi = \mathbf{1}$	$\pi = \mathbf{1/2}$	$\pi = \mathbf{1/4}$	$\pi = \mathbf{1/8}$	$\pi = \mathbf{1/16}$	$\pi = \mathbf{1/32}$
10	0.059	0.084	0.119	0.168	0.238	0.336
12	0.123	0.174	0.245	0.347	0.490	0.693
14	0.157	0.222	0.314	0.443	0.627	0.886
20	0.052	0.073	0.103	0.146	0.206	0.291
26	0.014	0.020	0.028	0.039	0.056	0.078
28	0.030	0.042	0.059	0.084	0.118	0.167

Figure 3.1: Comparisons of Δ values of variance-optimal design (red line) and bias-optimal design (blue line) for $n = 14$ when π = 1, 1/2, 1/4, 1/8, 1/16, 1/32

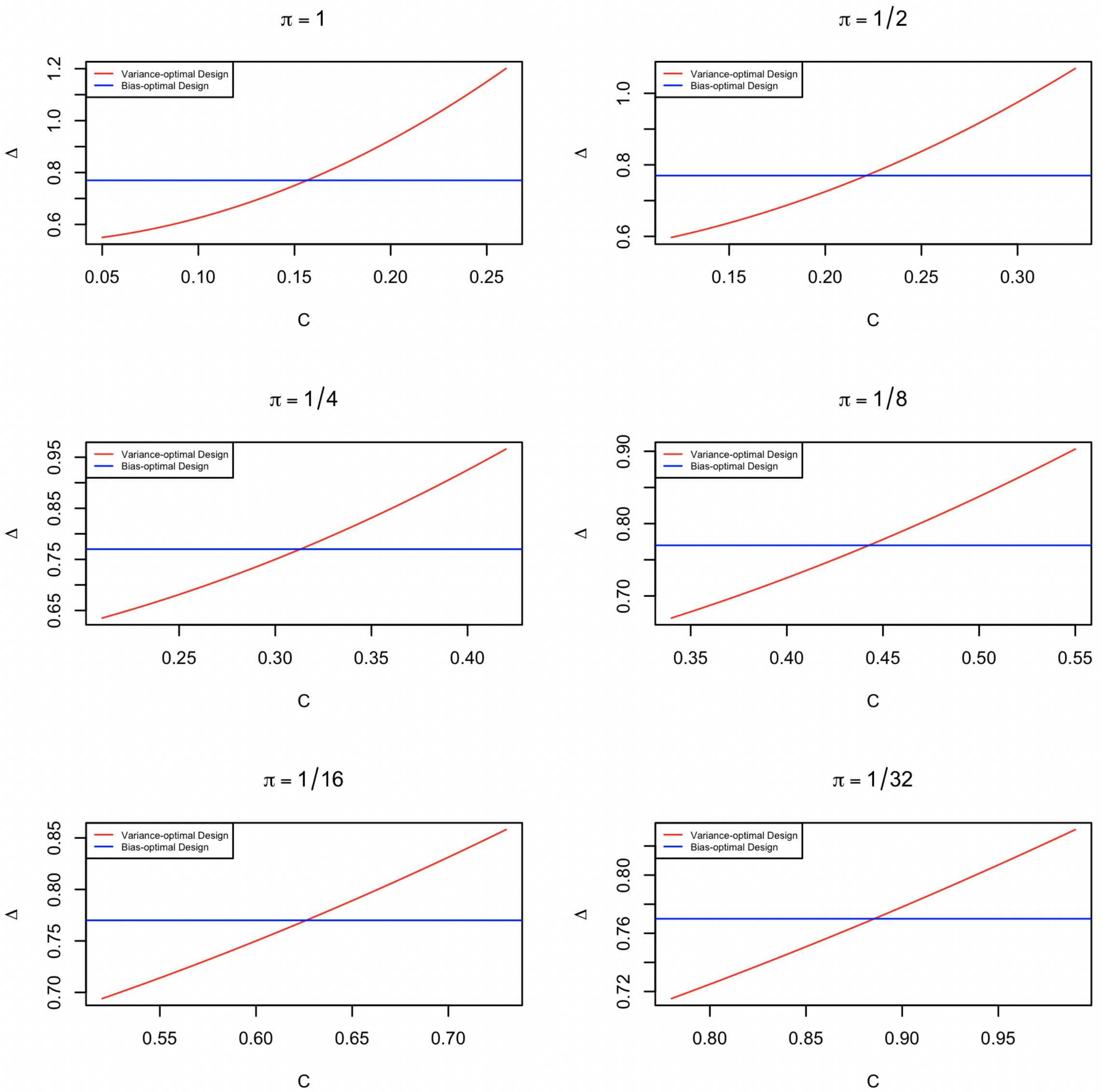
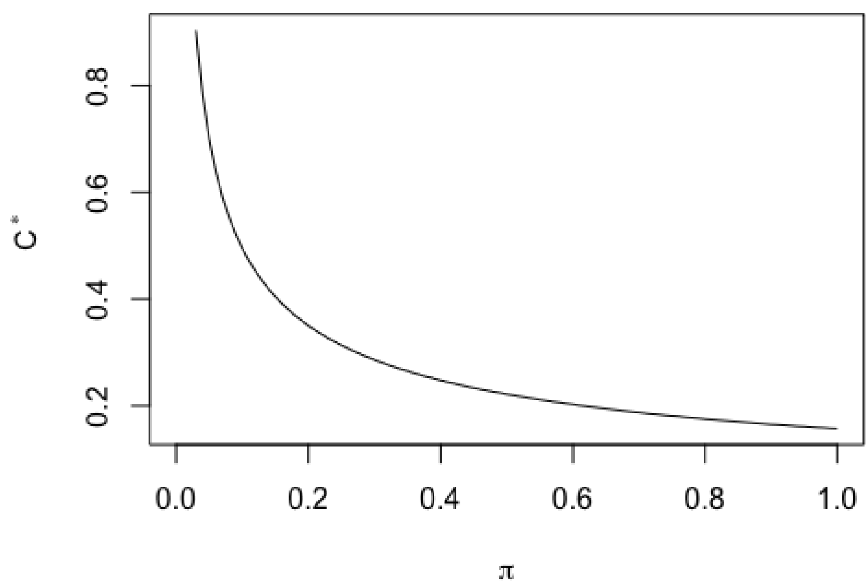


Figure 3.2: Change of C^* values at different π for designs of 14 runs



3.2 An Alternative Design

This section studies an alternative design, which represents a compromise between variance-optimal and bias-optimal designs. In our exploration, we focus on designs with 14 runs. We demonstrate the construction details and present the comparison results of this design with the variance-optimal and the bias-optimal designs of 14 runs in terms of the MSE. To obtain an alternative design, we partially fold over some runs from an OA. Specifically, a 14-run alternative design matrix is constructed by combining an $OA(8, 2^7, 2)$ matrix with six additional runs obtained by mirroring the first six runs of the original OA, which is created by selecting all columns from an 8×8 Hadamard matrix except for the one column with all +1's. The resulting 14-run design is displayed as follows.

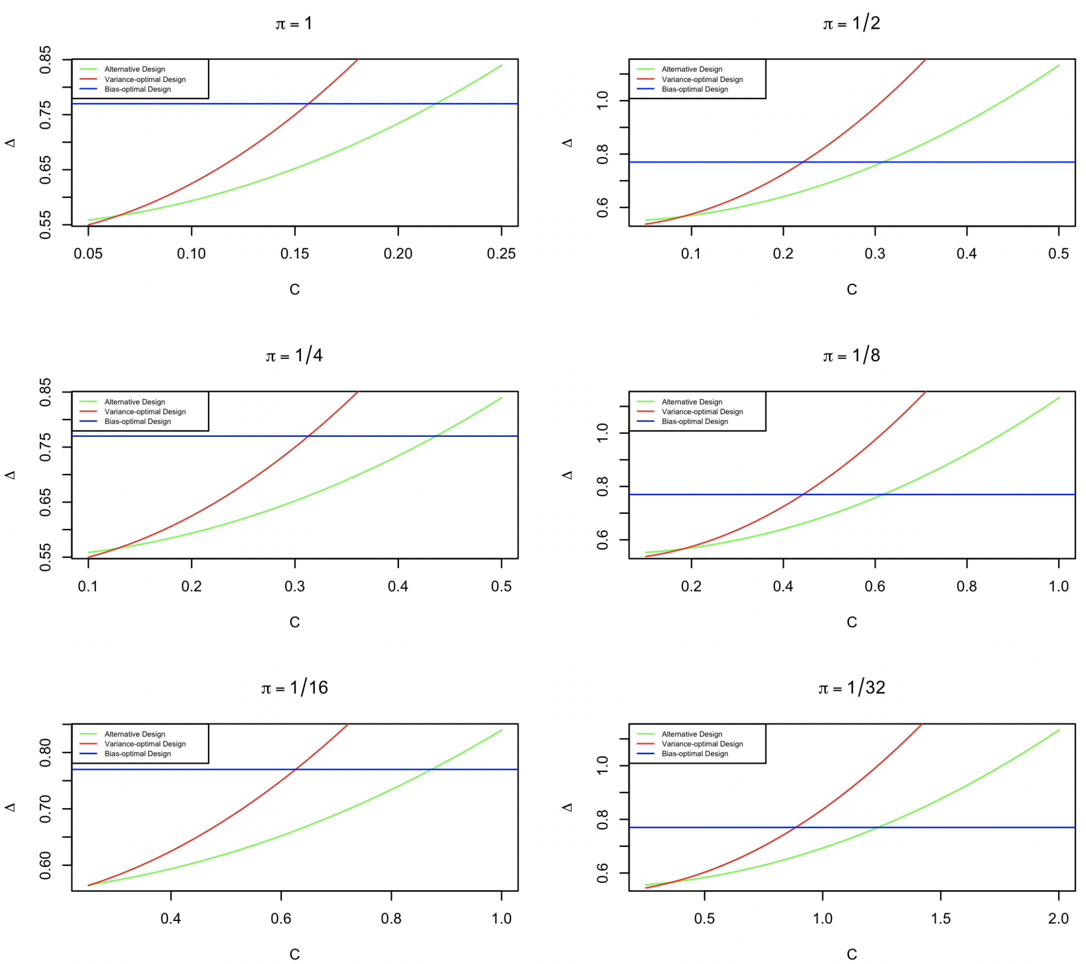
A	B	C	D	E	F	G
1	1	1	1	1	1	1
−1	1	−1	1	−1	1	−1
1	−1	−1	1	1	−1	−1
−1	−1	1	1	−1	−1	1
1	1	1	−1	−1	−1	−1
−1	1	−1	−1	1	−1	1
1	−1	−1	−1	−1	1	1
−1	−1	1	−1	1	1	−1
−1	−1	−1	−1	−1	−1	−1
1	−1	1	−1	1	−1	1
−1	1	1	−1	−1	1	1
1	1	−1	−1	1	1	−1
−1	−1	−1	1	1	1	1
1	−1	1	1	−1	1	−1

We compare three designs of 14 runs and seven factors using their Δ values for $\pi = 1, 1/2, 1/4, 1/8, 1/16, 1/32$. In Table 3.3, we present the range of C values, $C_1 < C < C_2$, within which the alternative design outperforms the other two designs. All values are rounded up to three decimal places. For example, when all 2fis are significant, that is when $\pi = 1$, as long as $0.065 < C = \frac{\tau}{\sigma} < 0.219$, the alternative design is the best among the three designs. Figure 3.3 displays a more detailed comparison of the Δ values of the three types of designs: alternative design (green line), variance-optimal design (red line), bias-optimal design (blue line) for different C values for different given π values. As clearly shown in Figure 3.3, the variance-optimal design is the best if $C < C_1$; the alternative design is the best if $C_1 < C < C_2$; and the bias-optimal design is the best if $C > C_2$.

Table 3.3: Range of C Values for Alternative Design to be the Optimal Design with 14 Runs

	$\pi = \mathbf{1}$	$\pi = \mathbf{1/2}$	$\pi = \mathbf{1/4}$	$\pi = \mathbf{1/8}$	$\pi = \mathbf{1/16}$	$\pi = \mathbf{1/32}$
$\mathbf{C_1}$	0.065	0.091	0.129	0.182	0.257	0.363
$\mathbf{C_2}$	0.219	0.309	0.437	0.618	0.873	1.235

Figure 3.3: Comparisons of Δ values of alternative design (green line), variance-optimal design (red line) and bias-optimal design (blue line) for n =14 when π = 1, 1/2, 1/4, 1/8, 1/16, 1/32



Chapter 4

Summary and Future Work

This project examines and compares two types of designs, namely variance-optimal designs and bias-optimal designs, in terms of MSE. Variance-optimal designs, given by OAs of strength two and NOAs, are optimal if interaction terms are negligible. However, when some 2fis are important, the bias emerges and may become dominant in the MSE. This situation may favor bias-optimal designs. To compare the two types of designs, we obtain a closed-form expression of an MSE criterion that takes sparsity into consideration to compare the two types of designs. Comparison results for run sizes of $n = 10, 12, 14, 20, 26, 28$ runs are obtained, which should provide practitioners with some helpful guidance for choosing between the two types of designs under various sparsity scenarios. In the future, we will consider extension of this study to designs of larger run sizes.

This project also presents an interesting result that within a certain range of C values, a 14-run compromise design outperforms the other two types in terms of the MSE. It would be very useful to obtain more compromise designs. A future work of high potential is to conduct a systematic investigation of such compromise designs.

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Appendix A

Variance-optimal Designs

For n=10,

A	B	C	D	E
1	1	1	1	1
−1	1	−1	1	−1
1	−1	−1	1	1
−1	−1	1	1	−1
1	1	1	−1	−1
−1	1	−1	−1	1
1	−1	−1	−1	−1
−1	−1	1	−1	1
1	1	1	1	1
1	1	−1	−1	−1

For n=12,

A	B	C	D	E	F
1	−1	−1	−1	1	−1
1	1	−1	1	1	1
1	1	1	−1	−1	−1
1	−1	1	1	1	−1
1	−1	1	1	−1	1
−1	−1	−1	−1	−1	−1
−1	1	−1	1	1	−1
−1	−1	−1	1	−1	1
−1	−1	1	−1	1	1
1	1	−1	−1	−1	1
−1	1	1	1	−1	−1
−1	1	1	−1	1	1

For n=14,

A	B	C	D	E	F	G
-1	-1	1	-1	1	1	-1
-1	-1	-1	1	-1	1	1
1	-1	-1	-1	1	-1	1
-1	-1	-1	-1	-1	-1	-1
1	1	-1	-1	-1	1	-1
1	1	1	-1	-1	-1	1
1	1	-1	1	1	1	-1
-1	1	-1	1	1	-1	1
1	-1	1	1	-1	1	1
1	-1	1	1	1	-1	-1
-1	1	1	-1	1	1	1
-1	1	1	1	-1	-1	-1
1	1	1	1	1	1	1
1	1	1	-1	-1	-1	-1

For n=20,

A	B	C	D	E	F	G	H	I	J
1	1	-1	-1	1	1	1	1	-1	1
1	1	-1	1	-1	1	-1	-1	-1	-1
-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
1	-1	1	1	-1	-1	1	1	1	1
1	1	1	-1	1	-1	1	-1	-1	-1
-1	1	-1	1	-1	-1	-1	-1	1	1
1	1	-1	1	1	-1	-1	1	1	1
1	-1	-1	1	1	1	1	-1	1	-1
-1	-1	-1	1	1	-1	1	1	-1	-1
-1	1	1	1	1	-1	1	-1	1	-1
-1	1	1	-1	1	1	-1	-1	1	1
-1	-1	1	1	-1	1	1	-1	-1	1
1	1	1	1	-1	1	-1	1	-1	-1
1	-1	1	-1	-1	-1	-1	1	1	-1
-1	1	-1	-1	-1	-1	1	1	-1	1
1	-1	-1	-1	-1	1	1	-1	1	1
-1	-1	1	1	1	1	-1	1	-1	1
-1	-1	-1	-1	1	1	-1	1	1	-1
1	-1	1	-1	1	-1	-1	-1	-1	1
-1	1	1	-1	-1	1	1	1	1	-1

For n=26,

A	B	C	D	E	F	G	H	I	J	K	L	M
-1	1	1	-1	-1	1	-1	1	-1	-1	-1	-1	1
1	1	-1	-1	1	-1	1	-1	-1	-1	-1	1	1
1	-1	1	1	-1	-1	1	1	-1	-1	1	-1	1
1	1	-1	-1	1	1	-1	-1	1	-1	1	-1	-1
1	-1	1	-1	1	1	-1	-1	1	1	-1	-1	1
-1	-1	-1	-1	1	1	1	1	1	-1	1	-1	1
-1	1	1	-1	-1	1	1	-1	-1	1	-1	1	-1
1	-1	-1	1	-1	1	-1	-1	-1	-1	1	1	1
1	1	1	-1	1	-1	1	1	-1	-1	1	1	-1
-1	-1	-1	1	1	1	1	1	-1	1	-1	1	1
-1	1	-1	1	1	-1	-1	1	1	-1	-1	1	-1
1	-1	-1	-1	-1	1	1	1	1	1	-1	1	-1
-1	1	-1	-1	-1	-1	1	1	1	1	1	-1	1
1	1	-1	1	-1	1	1	-1	-1	1	1	-1	-1
-1	1	1	1	1	1	-1	1	-1	1	1	-1	-1
-1	1	-1	1	-1	-1	-1	-1	1	1	1	1	1
1	-1	-1	1	1	-1	-1	1	-1	1	-1	-1	-1
1	1	1	1	-1	1	-1	1	1	-1	-1	1	1
-1	-1	1	1	-1	-1	1	-1	1	-1	-1	-1	-1
-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
1	1	1	1	1	-1	1	-1	1	1	-1	-1	1
-1	-1	1	-1	1	-1	-1	-1	-1	1	1	1	1
1	-1	1	-1	-1	-1	-1	1	1	1	1	1	-1
-1	-1	1	1	1	1	1	-1	1	-1	1	1	-1
1	1	1	1	1	1	1	1	1	1	1	1	1
1	1	1	1	1	1	-1	-1	-1	-1	-1	-1	-1

For n=28,

A	B	C	D	E	F	G	H	I	J	K	L	M	N
1	1	−1	−1	1	1	1	1	−1	−1	−1	−1	1	1
1	−1	−1	−1	−1	1	1	−1	−1	1	1	−1	−1	1
1	−1	1	1	1	1	−1	−1	−1	−1	1	−1	−1	−1
1	−1	−1	−1	1	−1	−1	1	−1	1	−1	1	−1	1
−1	−1	1	1	−1	−1	1	−1	−1	−1	1	1	1	1
1	−1	−1	1	−1	−1	−1	−1	1	1	1	−1	1	1
1	1	1	−1	−1	−1	1	1	−1	1	−1	−1	1	−1
−1	1	−1	−1	1	−1	1	−1	−1	−1	1	1	−1	1
1	−1	1	1	1	−1	1	−1	1	−1	−1	−1	1	−1
−1	1	1	1	1	1	−1	−1	−1	1	−1	−1	−1	1
1	1	1	−1	−1	−1	−1	1	1	−1	1	−1	−1	1
1	−1	1	−1	1	1	−1	1	1	−1	1	1	1	1
1	1	−1	1	1	−1	−1	1	1	1	1	1	−1	−1
−1	1	−1	−1	−1	1	−1	−1	1	1	1	−1	1	−1
−1	1	−1	1	−1	−1	−1	1	−1	−1	1	1	1	−1
−1	−1	−1	−1	1	1	1	1	1	−1	1	−1	1	−1
−1	−1	1	−1	1	−1	−1	−1	1	1	−1	1	1	1
−1	1	1	1	−1	1	−1	1	1	−1	−1	−1	−1	1
1	1	−1	1	−1	1	1	−1	1	1	−1	1	1	1
−1	−1	1	−1	−1	1	−1	1	−1	1	−1	1	1	−1
1	1	−1	1	1	1	−1	−1	−1	−1	−1	1	1	−1
−1	1	1	−1	1	1	1	−1	1	1	1	1	−1	−1
−1	−1	−1	−1	−1	−1	−1	−1	−1	−1	−1	−1	−1	−1
1	−1	1	1	−1	1	1	1	−1	1	1	1	−1	−1
−1	−1	−1	1	−1	1	1	1	1	−1	−1	1	−1	1
−1	−1	−1	1	1	−1	1	1	1	1	−1	−1	−1	−1
−1	1	1	1	1	−1	1	1	−1	1	1	−1	1	1
1	1	1	−1	−1	−1	1	−1	1	−1	−1	1	−1	−1

Appendix B

Bias-optimal Designs

For n=10,

A	B	C	D	E
1	1	1	−1	1
1	1	−1	1	1
1	−1	1	1	1
−1	1	1	1	1
−1	−1	−1	−1	1
−1	−1	−1	1	−1
−1	−1	1	−1	−1
−1	1	−1	−1	−1
1	−1	−1	−1	−1
1	1	1	1	−1

For n=12,

A	B	C	D	E	F
1	1	1	1	1	1
1	−1	−1	−1	−1	1
−1	1	−1	−1	−1	1
−1	−1	1	1	−1	1
−1	−1	1	−1	1	1
−1	−1	−1	1	1	1
−1	−1	−1	−1	−1	−1
−1	1	1	1	1	−1
1	−1	1	1	1	−1
1	1	−1	−1	1	−1
1	1	−1	1	−1	−1
1	1	1	−1	−1	−1

For n=14,

A	B	C	D	E	F	G
1	−1	−1	−1	−1	−1	−1
−1	1	−1	−1	−1	−1	−1
−1	−1	1	−1	−1	−1	−1
−1	−1	−1	1	−1	−1	−1
−1	−1	−1	−1	1	−1	−1
−1	−1	−1	−1	−1	1	−1
−1	−1	−1	−1	−1	−1	1
−1	1	1	1	1	1	1
1	−1	1	1	1	1	1
1	1	−1	1	1	1	1
1	1	1	−1	1	1	1
1	1	1	1	−1	1	1
1	1	1	1	1	−1	1
1	1	1	1	1	1	−1

For n=20,

A	B	C	D	E	F	G	H	I	J
1	−1	−1	−1	−1	1	−1	−1	−1	−1
−1	1	−1	−1	−1	−1	1	−1	−1	−1
−1	−1	1	−1	−1	−1	−1	1	−1	−1
−1	−1	−1	1	−1	−1	−1	−1	1	−1
−1	−1	−1	−1	1	−1	−1	−1	−1	1
−1	1	1	1	1	1	−1	−1	−1	−1
1	−1	1	1	1	−1	1	−1	−1	−1
1	1	−1	1	1	−1	−1	1	−1	−1
1	1	1	−1	1	−1	−1	−1	1	−1
1	1	1	1	−1	−1	−1	−1	−1	1
−1	1	1	1	1	−1	1	1	1	1
1	−1	1	1	1	1	−1	1	1	1
1	1	−1	1	1	1	1	−1	1	1
1	1	1	−1	1	1	1	1	−1	1
1	1	1	1	−1	1	1	1	1	−1
1	−1	−1	−1	−1	−1	1	1	1	1
−1	1	−1	−1	−1	1	−1	1	1	1
−1	−1	1	−1	−1	1	1	−1	1	1
−1	−1	−1	1	−1	1	1	1	−1	1
−1	−1	−1	−1	1	1	1	1	1	−1

For n=26,

A	B	C	D	E	F	G	H	I	J	K	L	M
1	1	−1	1	−1	−1	−1	−1	−1	1	−1	−1	−1
−1	1	1	−1	1	−1	−1	−1	−1	−1	1	−1	−1
−1	−1	1	1	−1	1	−1	−1	−1	−1	−1	1	−1
−1	−1	−1	1	1	−1	1	−1	−1	−1	−1	−1	1
1	−1	−1	−1	1	1	−1	1	−1	−1	−1	−1	−1
−1	1	−1	−1	−1	1	1	−1	1	−1	−1	−1	−1
−1	−1	1	−1	−1	−1	1	1	−1	1	−1	−1	−1
−1	−1	−1	1	−1	−1	−1	1	1	−1	1	−1	−1
−1	−1	−1	−1	1	−1	−1	−1	1	1	−1	1	−1
−1	−1	−1	−1	−1	1	−1	−1	−1	1	1	−1	1
1	−1	−1	−1	−1	−1	1	−1	−1	−1	1	1	−1
−1	1	−1	−1	−1	−1	−1	1	−1	−1	−1	1	1
1	−1	1	−1	−1	−1	−1	−1	1	−1	−1	−1	1
−1	−1	1	−1	1	1	1	1	1	−1	1	1	1
1	−1	−1	1	−1	1	1	1	1	1	−1	1	1
1	1	−1	−1	1	−1	1	1	1	1	1	−1	1
1	1	1	−1	−1	1	−1	1	1	1	1	1	−1
−1	1	1	1	−1	−1	1	−1	1	1	1	1	1
1	−1	1	1	1	−1	−1	1	−1	1	1	1	1
1	1	−1	1	1	1	−1	−1	1	−1	1	1	1
1	1	1	−1	1	1	1	−1	−1	1	−1	1	1
1	1	1	1	−1	1	1	1	−1	−1	1	−1	1
1	1	1	1	1	−1	1	1	1	−1	−1	1	−1
−1	1	1	1	1	1	−1	1	1	1	−1	−1	1
1	−1	1	1	1	1	1	−1	1	1	1	−1	−1
−1	1	−1	1	1	1	1	1	−1	1	1	1	−1

For n=28,

A	B	C	D	E	F	G	H	I	J	K	L	M	N
1	−1	−1	−1	−1	−1	−1	1	1	−1	1	−1	−1	−1
−1	1	−1	−1	−1	−1	−1	1	−1	1	−1	−1	−1	1
−1	−1	1	−1	−1	−1	−1	−1	1	−1	−1	−1	1	1
−1	−1	−1	1	−1	−1	−1	1	−1	−1	−1	1	1	−1
−1	−1	−1	−1	1	−1	−1	−1	−1	−1	1	1	−1	1
−1	−1	−1	−1	−1	1	−1	−1	−1	1	1	−1	1	−1
−1	−1	−1	−1	−1	−1	1	−1	1	1	−1	1	−1	−1
−1	−1	1	−1	1	1	1	1	−1	−1	−1	−1	−1	−1
−1	1	−1	1	1	1	−1	−1	1	−1	−1	−1	−1	−1
1	−1	1	1	1	−1	−1	−1	−1	1	−1	−1	−1	−1
−1	1	1	1	−1	−1	1	−1	−1	−1	1	−1	−1	−1
1	1	1	−1	−1	1	−1	−1	−1	−1	−1	1	−1	−1
1	1	−1	−1	1	−1	1	−1	−1	−1	−1	−1	1	−1
1	−1	−1	1	−1	1	1	−1	−1	−1	−1	−1	−1	1
−1	1	1	1	1	1	1	−1	−1	1	−1	1	1	1
1	−1	1	1	1	1	1	−1	1	−1	1	1	1	−1
1	1	−1	1	1	1	1	1	−1	1	1	1	−1	−1
1	1	1	−1	1	1	1	−1	1	1	1	−1	−1	1
1	1	1	1	−1	1	1	1	1	1	−1	−1	1	−1
1	1	1	1	1	−1	1	1	1	−1	−1	1	−1	1
1	1	1	1	1	1	−1	1	−1	−1	1	−1	1	1
1	1	−1	1	−1	−1	−1	−1	1	1	1	1	1	1
1	−1	1	−1	−1	−1	1	1	−1	1	1	1	1	1
−1	1	−1	−1	−1	1	1	1	1	−1	1	1	1	1
1	−1	−1	−1	1	1	−1	1	1	1	−1	1	1	1
−1	−1	−1	1	1	−1	1	1	1	1	1	−1	1	1
−1	−1	1	1	−1	1	−1	1	1	1	1	1	−1	1
−1	1	1	−1	1	−1	−1	1	1	1	1	1	1	−1