DEPARTMENT OF MATHEMATICS,
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Staggered designs for experiments with more than one hard-to-change factor

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Staggered designs for experiments with more than one hard-to-change factor

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Abstract

In many industrial experiments, some of the factors are not independently reset for each run. This is due to time and/or cost constraints and to the hard-to-change nature of these factors. Most of the literature restricts the attention to split-plot designs in which all the hard-to-change factors are independently reset at the same points in time. This constraint is to some extent relaxed in split-split-plot designs because these require the least hard-to-change factors to be reset more often than the most hard-to-change factors. A key feature of the split-split-plot designs, however, is that the least hard-to-change factors are reset whenever the most hard-to-change factors are reset. In this article, we relax this constraint and present a new type of design which allows the hard-to-change factor levels to be reset at entirely different points in time. We show that the new designs are cost-efficient and that they outperform split-plot and split-split-plot designs in terms of statistical efficiency. Because of the fact that the hard-to-change factors are independently reset alternatingly, an appropriate name for the new design is staggered design.

Keywords: A- and D-optimality criterion, cost, hard-to-change factors, split-plot design, split-split-plot design, staggered design.

1 Introduction

In many industrial experiments, complete randomization with independent settings of all the experimental factors is not feasible. Much research has already been done for the situation in which there is only one hard-to-change factor or where all the hard-to-change factors are reset at the same time. Designs recommended in the literature for these situations are the split-plot designs. Bisgaard (2000) recognized that split-plotting is common and much more frequently used than the literature on design of experiments in engineering would suggest. Ganju and Lucas (1999) stated that split-plot designs should be designed instead of being the accidental outcome of a random run order. Ju and Lucas (2002) compared the precision of the estimator of the regression coefficients for various run-order scenarios and showed that classical split-plot designs are superior to random run orders.

Vining, Kowalski and Montgomery (2005) discussed how to modify the standard central composite and Box-Behnken designs to accommodate a split-plot structure and discovered that some of the constructed designs achieved the equivalence of ordinary least squares and generalized least squares estimation. Goos and Vandebroek (2003) showed that certain two-level factorial and fractional factorial split-plot designs are D-optimal for the estimation of first-order response surface models for a specific number and size of whole plots. Like Jones and Goos (2007), they also proposed an algorithm to construct D-optimal split-plot designs with given numbers and sizes of whole plots. More recently, Anbari and Lucas (2008) listed practical split-plot patterns that give higher G-efficiency and higher cost efficiency than completely randomized designs.

Following the basic idea of split-plot designs and using three strata instead of two leads to split-split-plot designs. This type of design can be useful for situations where there are two groups of hard-to-change factors, one of which includes factors that are more difficult to reset than the other. The literature on the design of such studies is rather limited. Trinca and Gilmour (2001) discussed the design and analysis of multi-stratum experiments, special cases of which are split-plot and split-split-plot designs. They orthogonalize each stratum of the design as much as possible with respect to the higher strata. Schoen (1999) constructed an orthogonal two-level split-split-plot design in a combinatorial way by joining fractional factorial designs in order to create the desired nesting structure. Recently, Jones and Goos (2008) provided a coordinate-exchange algorithm to compute D-optimal split-split-plot designs. Typical for these designs is that when the most hard-to-change factors are reset, this is also done for the least hard-to-change factor.

Real-life examples, however, show that it might also be interesting to reset the various hard-to-change factor levels at different points in time in case of several hard-to-change factors some of which are easier to change than others. Such an example is described in Webb, Lucas and Borkowski (2004). The experiment in the example was conducted at a computer component manufacturing company and aimed at improving the performance of a wrapper machine. Three factors were involved: the spacing of a seal crimper, the speed at which the machine is run and the temperature of the crimper. The experimenters decided to perform a 15-run Box-Behnken design. From the beginning they considered spacing as being difficult to change and therefore the level of that factor was set only four times. However, when performing the experiment it became clear that the speed was also hard to set but less so than the spacing. As a consequence they decided to set the speed only eight times. This resulted in a design, displayed in Table 1, where speed was reset more often than spacing because it was less hard to change. Also, the levels of the factors speed and spacing were reset at different points in time.

Table 1: Wrapper machine example with two hard-to-change factors (spacing and speed) and one easy-to-change factor where spacing is set four times, speed is set eight times and temperature is independently set for each run

spacing	speed	temp
0	1	-1
0	1	1
0	0	0
1	0	1
1	0	-1
1	-1	0
1	1	0
-1	1	0
-1	-1	0
-1	0	-1
-1	0	1
0	0	0
0	-1	-1
0	-1	1
0	0	0

In a second real-life example researchers are performing a series of experiments involving two hard-to-change factors one of which is less difficult to reset than the other. The experiments concern an atmospheric plasma deposition of antibacterial coatings. This is for example useful for the lining of a refrigerator or for medical devices. The hard-to-change factors in these experiments were the gap between an electrode and the sample surface and the frequency of a transformer. To change the levels of these two factors a technician has to be called in which complicates the resetting of the factors. However, the gap is more difficult to change than the frequency, because resetting the gap involves adaptations to the pressure in order to guarantee favorable conditions for switching on the discharge whereas to change the frequency the technician only has to plug in a new transformer. The other factors - power, gas flow rate and precursor injection - are easy to change and therefore they are reset independently for each run. The experimenters' primary interest is in the main effects and the two-factor interactions of the five factors, so that a 2^5 full factorial design is considered. The question is how to run that design in a cost-efficient fashion so that the effects of interest can be estimated as precisely as possible.

These two examples show that there is a need in practice for designs that take into account the fact that some of the hard-to-change factors are easier to reset than others and allow the hard-to-change factors to be reset at different points in time. In this paper a new type of design, which is called the staggered design, is introduced which gives the possibility to do this in such a way that the experiment is not only statistically efficient but also cost efficient. The design also remediates some of the drawbacks of split-plot and split-split-plot designs. It should be noted, however, that the designs presented here are useful only when the number of settings of the hard-to-change factor level is not dictated by the physicalities of the experiments, such as oven sizes or batches of material, as in the examples above.

The remainder of this paper focuses on two-level factorial designs and the estimation of models containing main effects and two-factor interaction effects only. In the next section we describe in general the model utilized in Webb, Lucas and Borkowski (2004) for situations involving several hard-to-change factors some of which are less difficult to reset than others. Next, some algorithmic results in finding useful designs in the presence of several hard-to-change factors are described. Because these results were not fully satisfactory for large problems, we generalize the computational results obtained for smaller problems in a combinatorial fashion. This generalization is described in detail for 2^4 and 2^5 full factorial designs, and the resulting designs are compared to split-plot and split-split-plot designs. The extension to 2^6 and 2^7 full factorial designs is briefly discussed. Finally the sensitivity of our results to the relative magnitude of the variance components in the statistical model is investigated. For clarity of the exposition, we restrict our attention to experiments with two hard-to-change factors.

2 Model

In the experiments considered in this paper, the set of f factors is divided in three groups: the most hard-to-change factor, denoted by w in the model, the least hard-to-change factor, s, and the remaining $f-2$ easy-to-change factors, $t_1, ..., t_{f-2}$. The experimental runs will be partioned in two ways, one for each hard-to-change factor. Such a partitioning is illustrated in Table 1, where the settings of the factor spacing divide the runs in four subsets and the settings of the factor speed divide the runs in eight different subsets. To capture the correlation between runs for which the most hard-to-change factor w is not independently reset, we include random effects δ_i, $i = 1, \ldots, r$, in the model for each of the r independent settings of w. To capture the correlation between runs for which the least hard-to-change factor s is not independently reset, we also include random effects γ_i, $i = 1, \ldots, g$, in the model for each of the g independent settings of s. The kth response ($k = 1, \ldots, n$), obtained at the ith setting of w and the jth setting of s can then be written as

$$\begin{aligned} Y_{ijk} =& \beta_0 + \beta_w w_i + \beta_s s_j + \sum_{l=1}^{f-2} \beta_{tl} t_{lk} + \beta_{ws} w_i s_j + \sum_{l=1}^{f-2} \beta_{wt_l} w_i t_{lk} \\ &+ \sum_{l=1}^{f-2} \beta_{st_l} s_j t_{lk} + \sum_{l=1}^{f-3} \sum_{m=l+1}^{f-2} \beta_{t_l t_m} t_{lk} t_{mk} + \delta_i + \gamma_j + \epsilon_k, \end{aligned} \quad (1)$$

where δ_i represents the random effect of the ith setting of the most hard-to-change factor w, γ_j is the random effect of the jth setting of the least hard-to-change factor s, ϵ_k is the random error and n denotes the number of observations.

In matrix notation, Equation (1) can be written as

$$\mathbf{Y} = \mathbf{X}\boldsymbol{\beta} + \mathbf{Z}_\delta\boldsymbol{\delta} + \mathbf{Z}_\gamma\boldsymbol{\gamma} + \boldsymbol{\epsilon}, \tag{2}$$

where $\mathbf{Y}$ is the $n \times 1$ vector containing the n responses of the experiment, $\boldsymbol{\beta}$ is a $p \times 1$ vector that contains the $p = 1 + f + f(f-1)/2$ model parameters, $\mathbf{X}$ is the $n \times p$ design matrix (containing the settings of all the factors and their pairwise cross-products), $\mathbf{Z}_\delta$ is the $n \times r$ matrix with (i, j)th entry equal to 1 if the ith run is conducted at the jth setting of w and equal to 0 otherwise, $\mathbf{Z}_\gamma$ is the $n \times g$ matrix with (i, j)th entry equal to 1 if the ith run is conducted at the jth setting of s and equal to 0 otherwise, $\boldsymbol{\delta}$ and $\boldsymbol{\gamma}$ are the $r \times 1$ and $g \times 1$ vectors containing the random effects associated with the independent settings of the most hard-to-change and the least hard-to-change factor, respectively, and $\boldsymbol{\epsilon}$ is the $n \times 1$ vector of random errors.

Furthermore we assume that

$$\mathrm{E}(\boldsymbol{\delta}) = \mathbf{0}_r, \mathrm{cov}(\boldsymbol{\delta}) = \sigma_\delta^2\mathbf{I}_r,$$
$$\mathrm{E}(\boldsymbol{\gamma}) = \mathbf{0}_g, \mathrm{cov}(\boldsymbol{\gamma}) = \sigma_\gamma^2\mathbf{I}_g,$$
$$\mathrm{E}(\boldsymbol{\epsilon}) = \mathbf{0}_n, \mathrm{cov}(\boldsymbol{\epsilon}) = \sigma_\epsilon^2\mathbf{I}_n$$

and

$$\mathrm{cov}(\boldsymbol{\delta}, \boldsymbol{\gamma}) = \mathbf{0}_{r\times g},\ \ \mathrm{cov}(\boldsymbol{\delta}, \boldsymbol{\epsilon}) = \mathbf{0}_{r\times n},\ \ \mathrm{cov}(\boldsymbol{\gamma}, \boldsymbol{\epsilon}) = \mathbf{0}_{g\times n},$$

where $\mathbf{0}_c$ and $\mathbf{I}_c$ represent a c-dimensional zero vector and identity matrix, respectively, and $\mathbf{0}_{c\times d}$ is a zero matrix of dimension $c \times d$. When using this model the variance-covariance matrix of the responses, $\mathbf{Y}$, is

$$\begin{aligned} \mathbf{V} &= \sigma_\epsilon^2\mathbf{I}_n + \sigma_\delta^2\mathbf{Z}_\delta\mathbf{Z}_\delta' + \sigma_\gamma^2\mathbf{Z}_\gamma\mathbf{Z}_\gamma', \\ &= \sigma_\epsilon^2(\mathbf{I}_n + \eta_\delta\mathbf{Z}_\delta\mathbf{Z}_\delta' + \eta_\gamma\mathbf{Z}_\gamma\mathbf{Z}_\gamma'), \end{aligned} \tag{3}$$

where η_δ and η_γ are the variance ratios $\sigma_\delta^2/\sigma_\epsilon^2$ and $\sigma_\gamma^2/\sigma_\epsilon^2$ for the most hard-to-change factor and the least hard-to-change factor, respectively. The larger these ratios, the stronger the correlation between runs conducted at the same setting of the most hard-to-change and/or the least hard-to-change factor. In most applications it will be reasonable to assume that the variance ratio η_δ is larger than the variance ratio η_γ because the former ratio corresponds to a factor that is harder to change than the latter.

The statistical model in Equation (2) generalizes the split-plot model and the split-split-plot model. For the model to reduce to the split-plot model, it is required that $\mathbf{Z}_\gamma = \mathbf{Z}_\delta$, which means that the most hard-to-change factor and the least hard-to-change factor are reset at the same points in time. In that case, the variance components σ_δ^2 and σ_γ^2 cannot be estimated separately. This problem does not occur in split-split-plot designs, where $\mathbf{Z}_\delta = \mathbf{I}_r \otimes \mathbf{1}_{c_1}$ and $\mathbf{Z}_\gamma = \mathbf{I}_g \otimes \mathbf{1}_{c_2}$ (with $\otimes$ the Kronecker product), $\mathbf{1}_{c_i}$ is a c_i-dimensional vector of ones, c_1 and c_2 are the numbers of runs in a whole plot and subplot, respectively, and $n = rc_1 = gc_2$.

Under the assumption of normality, the maximum likelihood estimator of the unknown model parameter $\boldsymbol{\beta}$ is the generalized least squares estimator

$$\hat{\boldsymbol{\beta}} = (\mathbf{X}'\mathbf{V}^{-1}\mathbf{X})^{-1}\mathbf{X}'\mathbf{V}^{-1}\mathbf{y}. \quad (4)$$

The variance-covariance matrix of the estimator can be expressed as

$$\mathrm{cov}(\hat{\boldsymbol{\beta}}) = (\mathbf{X}'\mathbf{V}^{-1}\mathbf{X})^{-1}, \quad (5)$$

and the information matrix on the unknown parameter $\boldsymbol{\beta}$ is given by

$$\mathbf{M} = \mathbf{X}'\mathbf{V}^{-1}\mathbf{X}. \quad (6)$$

A criterion to select designs is the D-optimality criterion which seeks designs that maximize the determinant of this information matrix. In this article, we report the pth root of that determinant as is customary in the design of experiments literature. To compare two designs with information matrices $\mathbf{M}_1$ and $\mathbf{M}_2$ in terms of the D-optimality criterion, the D-efficiency,

$$\left(\frac{|\mathbf{M}_1|}{|\mathbf{M}_2|}\right)^{1/p},$$

is used. Another optimality criterion used to compare the various designs presented in this paper is the A-optimality criterion. This criterion seeks designs that minimize the sum of the variances of the parameter estimators, which is proportional to the trace of the variance-covariance matrix in Equation (5).

3 Algorithmic Approach

The goal of our research is to find nice and well-structured designs for practical situations where there are two hard-to-change factors one of which is less difficult to reset than the other. For that purpose, we initially used an adapted version of the variable-neighbourhood search algorithm of Garroi, Goos and Sörensen (2009), which was designed to search for optimal run orders of given designs in the presence of serial correlation. We used the neighbourhood structures and perturbation move of this algorithm to tackle our specific problem, that is to look for the D-optimal run order of a two-level factorial design in the presence of hard-to-change factors and for the optimal points in time to reset the levels of the hard-to-change factors. For an experiment with two hard-to-change

Table 2: 16-run staggered design for a main-effects model with two-factor interactions in two hard-to-change factors, w and s, and two easy-to-change factors t_1 and t_2

Run	w	s	t_1	t_2
1	-1	1	1	1
2	-1	1	-1	-1
3	-1	-1	1	-1
4	-1	-1	-1	1
5	1	-1	1	1
6	1	-1	-1	-1
7	1	1	-1	1
8	1	1	1	-1
9	-1	1	-1	1
10	-1	1	1	-1
11	-1	-1	1	1
12	-1	-1	-1	-1
13	1	-1	1	-1
14	1	-1	-1	1
15	1	1	-1	-1
16	1	1	1	1

factors and two easy-to-change factors, and using the points of a 2^4 full factorial design, the modified variable-neighbourhood search algorithm produced several nicely structured D-optimal run orders, where the two hard-to-change factors' levels were reset at different points in time. One of these designs is displayed in Table 2. The design involves four settings of the most hard-to-change factor w and five settings of the least hard-to-change factor s. It thus turns out that the D-optimal run order of the 2^4 factorial design is neither a split-plot design nor a split-split-plot design. Because of the pattern in the settings of the two hard-to-change factors, we call the new design a staggered design.

For the problem of finding optimal run orders of 2^5 and 2^6 factorial designs, the modified variable-neighbourhood search algorithm produced less desirable designs with irregularly sized subsets of runs dictated by the settings of the hard-to-change factors. Often this is due to the fact that the algorithm tends to get stuck in local optima very easily for the design problem considered in this paper. An example of such a design found by the algorithm is the 32-run design in Table 3. This design, which was obtained for $\eta_\delta = 1$ and $\eta_\gamma = 0.1$, involves seven settings of the most hard-to-change factor w and ten settings of the least hard-to-change factor s.

An alternative algorithm, namely a coordinate-exchange algorithm as in Jones and Goos (2008), did not produce better designs than the modified variable-neighbourhood search algorithm for the five- and six-factor problems.

Table 3: 32-run D-optimal design found by the variable-neighbourhood search algorithm for a main-effects model with two-factor interactions in two hard-to-change factors, w and s, and three easy-to-change factors t_1, t_2 and t_3 (assuming $\eta_\delta = 1$ and $\eta_\gamma = 0.1$)

Run	w	s	t_1	t_2	t_3	Run	w	s	t_1	t_2	t_3
1	1	1	-1	-1	1	17	-1	-1	-1	-1	-1
2	1	1	1	1	-1	18	-1	-1	1	-1	-1
3	1	-1	-1	-1	-1	19	-1	-1	-1	1	1
4	1	-1	1	1	1	20	1	-1	1	-1	1
5	-1	-1	1	1	-1	21	1	-1	-1	1	-1
6	-1	-1	1	-1	1	22	1	1	1	1	1
7	-1	1	-1	-1	-1	23	1	1	-1	-1	-1
8	-1	1	1	1	1	24	-1	-1	-1	-1	1
9	1	-1	1	1	-1	25	-1	-1	1	1	1
10	1	-1	-1	-1	1	26	-1	-1	-1	1	-1
11	1	1	1	-1	-1	27	-1	1	-1	1	1
12	1	1	-1	1	1	28	-1	1	1	-1	-1
13	-1	1	1	-1	1	29	1	1	-1	1	-1
14	-1	1	1	1	-1	30	1	1	1	-1	1
15	-1	1	-1	1	-1	31	1	-1	1	-1	-1
16	-1	1	-1	-1	1	32	1	-1	-1	1	1

As neither of the two algorithms produced satisfactory results for large problems, we turned our attention to a combinatorial construction method which is described in the following sections. The method generalizes the structure of the design in Table 2.

4 Comparison with Split-Plot and Split-Split-Plot Designs

In this section, we first compare the algorithmically constructed staggered design for two hard-to-change factors and two easy-to-change factors displayed in Table 2 with two split-plot designs and one split-split-plot design. We describe in detail what the alternative designs look like and compare them to the staggered design in terms of cost-efficiency and in terms of the D- and A-optimality criteria. Next, we compare the staggered design to split-plot and split-split-plot alternatives for 32 runs and three easy-to-change factors.

4.1 16 runs

The literature already offers several possibilities to run a 16-run two-level design with two hard-to-change factors, w and s, and two easy-to-change factors, t_1 and t_2, to estimate a main-effects-plus-interactions model. A first possibility is to reset both hard-to-change factors at the same time and use a split-plot design. Jones and Goos (2007) provide a coordinate-exchange algorithm to build D-optimal split-plot designs. Using their approach, we obtained the D-optimal split-plot design with four whole plots of size four displayed in Table 4. In terms of the D-optimality criterion, this design is equally good as the 2^4 design arranged in four whole plots using the contrast columns of w and wst_1t_2 as block defining relations.

Table 4: D-optimal 16-run split-plot design with four whole plots of size four for a main-effects model with two-factor interactions in two hard-to-change factors, w and s, and two easy-to-change factors t_1 and t_2

Run	WP	w	s	t_1	t_2	Run	WP	w	s	t_1	t_2
1	1	1	-1	-1	-1	9	3	1	1	1	-1
2	1	1	-1	-1	1	10	3	1	1	1	1
3	1	1	-1	1	1	11	3	1	1	-1	-1
4	1	1	-1	1	-1	12	3	1	1	-1	1
5	2	-1	-1	-1	1	13	4	-1	1	-1	1
6	2	-1	-1	-1	-1	14	4	-1	1	1	-1
7	2	-1	-1	1	-1	15	4	-1	1	1	1
8	2	-1	-1	1	1	16	4	-1	1	-1	-1

The two hard-to-change factors, w and s, are set only four times, which makes this split-plot design a cost-efficient design to perform. On the negative side, this design does not take advantage of the fact that the second hard-to-change factor, s, is easier to change than w. This means that in practice the factor s can be set more often without increasing the experimental cost substantially. If this were done, it would result in a more precise estimation of the main effect of s and its interaction effect with the most hard-to-change factor w (as we will see later, at the expense of a slightly less precise estimate of the interaction effect between t_1 and t_2). Another problem with the design in Table 4 is that it does not have enough degrees of freedom at the whole-plot level to allow the estimation of the whole-plot error variance (which equals $\sigma_\delta^2 + \sigma_\gamma^2$ because a split-plot design does not allow for a separate estimation of σ_δ^2 and σ_γ^2). A design with more whole plots, for example with eight whole plots, would allow $\sigma_\delta^2 + \sigma_\gamma^2$ to be estimated. The D-optimal split-plot design with eight whole plots of size two is displayed in Table 5. This design, which can be constructed with w, s, wst_1t_2 as block defining relations, remedies the problem of the degrees of freedom at the whole-plot level but it is an expensive option as it requires twice as many settings of both hard-to-change factors.

A design that allows the least hard-to-change factor to be reset more often than the most hard-to-change factor is a split-split-plot design. Applying the algorithmic approach of Jones and Goos (2008) for constructing D-optimal split-split-plot designs yields the design shown in Table 6. The design has four whole plots, eight subplots and two runs

Table 5: D-optimal 16-run split-plot design with eight whole plots of size two for a main-effects model with two-factor interactions in two hard-to-change factors, w and s, and two easy-to-change factors t_1 and t_2

Run	WP	w	s	t_1	t_2	Run	WP	w	s	t_1	t_2
1	1	1	1	1	1	9	5	1	-1	-1	1
2	1	1	1	-1	-1	10	5	1	-1	1	-1
3	2	-1	1	-1	-1	11	6	-1	1	-1	1
4	2	-1	1	1	1	12	6	-1	1	1	-1
5	3	-1	-1	1	-1	13	7	1	1	1	-1
6	3	-1	-1	-1	1	14	7	1	1	-1	1
7	4	-1	-1	1	1	15	8	1	-1	1	1
8	4	-1	-1	-1	-1	16	8	1	-1	-1	-1

within each of the subplots. In this design the most hard-to-change factor w is set four times whereas the least hard-to-change factor s is set eight times. As a result the number of settings of s is doubled in comparison to the split-plot design in Table 4. Clearly the split-split-plot design makes use of the fact that one of the hard-to-change factors is easier to reset than the other. A possible disadvantage, however, could be the fact that this is still a rather expensive design to perform in terms of time and/or cost because of the larger number of settings of s. It is, however, cheaper still than the split-plot design in Table 5 because the most hard-to-change factor is set only four times.

Table 6: D-optimal 16-run split-split-plot design with four whole plots each consisting of two subplots of size two for a main-effects model with two-factor interactions in two hard-to-change factors, w and s, and two easy-to-change factor t_1 and t_2

Run	WP	SP	w	s	t_1	t_2	Run	WP	SP	w	s	t_1	t_2
1	1	1	1	1	-1	1	9	3	5	1	-1	-1	1
2	1	1	1	1	1	-1	10	3	5	1	-1	1	-1
3	1	2	1	-1	1	1	11	3	6	1	1	1	1
4	1	2	1	-1	-1	-1	12	3	6	1	1	-1	-1
5	2	3	-1	-1	1	1	13	4	7	-1	1	-1	-1
6	2	3	-1	-1	-1	-1	14	4	7	-1	1	1	1
7	2	4	-1	1	1	-1	15	4	8	-1	-1	1	-1
8	2	4	-1	1	-1	1	16	4	8	-1	-1	-1	1

The staggered design shown in Table 2 is an alternative design option that counters the possible drawbacks of the split-plot and split-split-plot designs. This is realized by allowing the two hard-to-change factors to be reset at different points in time. The first hard-to-change factor, w, is held constant at its low level for the first four runs. This is followed by a reset after which the next four runs are performed at the factor's high level, and so on. This means that, just like in the split-plot design in Table 4 and the split-split-plot design in Table 6, the most hard-to-change factor is set four times. The least hard-to-change factor s, is already reset after two runs performed at its high level. After this, it is held constant for three series of four runs. The experiment then ends with two runs at the high level of s. As a result the least hard-to-change factor is set five times.

In comparison to the split-plot design with four whole plots presented in Table 4, the staggered design in Table 2 takes advantage of the fact that the levels of the second hard-to-change factor can be set more often without increasing the experimental cost substantially, leading to a more precise estimation of the main effect of s and the interaction effect between w and s. Compared to the split-plot design with eight whole plots in Table 5 and the split-split-plot design shown in Table 6 there are fewer settings of s, namely five instead of eight. This is a reduction of almost 38%, suggesting that statistical efficiency and cost efficiency can go hand in hand. This is confirmed by a comparison of the variances of the parameter estimates of the four competing designs, and of their D- and A-optimality criterion values. Detailed computational results for the designs in Tables 2, 4, 5 and 6 are given in Table 7. The results were obtained by assuming that $\eta_\delta = 1$, $\eta_\gamma = 0.5$ and $\sigma_\varepsilon^2 = 0.5$.

Table 7: Variances of estimates of fixed model parameters for the 16-run staggered design in Table 2, the split-plot design with four whole plots in Table 4, the split-plot design with eight whole plots in Table 5 and the split-split-plot design in Table 6 when $\eta_\delta = 1$, $\eta_\gamma = 0.5$ and $\sigma_\varepsilon^2 = 0.5$

effect	staggered Table 2	split-plot Table 4	split-plot Table 5	split-split-plot Table 6
w	0.163	0.219	0.125	0.188
s	0.086	0.219	0.125	0.063
t_1	0.031	0.031	0.031	0.031
t_2	0.031	0.031	0.031	0.031
ws	0.037	0.219	0.125	0.063
wt_1	0.031	0.031	0.031	0.031
wt_2	0.031	0.031	0.031	0.031
st_1	0.031	0.031	0.031	0.031
st_2	0.031	0.031	0.031	0.031
t_1t_2	0.052	0.031	0.125	0.063
D-criterion	19.898	15.771	17.040	19.124
A-criterion	0.525	0.875	0.688	0.563

Comparing the staggered design to the split-plot designs in terms of the variance of the parameter estimates, it is clear from Table 7 that the main effect of the factor s as well as the interaction effect of w and s are estimated more precisely from the staggered design. This is bad news especially for the split-plot design with eight whole plots in Table 5, which is the most expensive design option and, yet, does not lead to the most precise estimation of the main effect of s and its interaction effect with w. The staggered design also allows a more accurate estimation of the main effect of the most hard-to-change factor w than the split-plot design with four whole plots despite the fact that this factor is set the same number of times in the two design options. This is due to the fact that, for split-plot designs, the main effects of w and s and their interaction are affected by both the random effects δ_i and γ_j, whereas, for the staggered design, the main effect estimate of w is impacted only by δ_i, and the main effect of s and the interaction ws are affected only by γ_j.

Compared to the split-split-plot design, the staggered design leads to a more precise estimation of the main effect of w and the interaction effect of w and s. Only the main effect of s is estimated less accurately. Evidently, this is due to the fact that the number of independent settings of the least hard-to-change factor s is considerably higher in the split-split-plot design.

The overall conclusion is that the split-plot design in Table 5 offers the best estimation of the main effect of w since it involves the largest number of settings of this factor. The split-split-plot design in Table 6 generates the most accurate estimate of the main effect of s due to the high number of settings of the least hard-to-change factor. The staggered design is the best for estimating the interaction effect of w and s. The latter design, in comparison to the split-split-plot design, sacrifices some precision on the estimation of the main effect of s to gain on the estimation of the interaction effect of w and s.

It is also useful to compare the performance of the four designs in terms of some optimality criteria. The staggered design performs best in terms of the D-optimality criterion with a D-criterion value of 19.898 in comparison to the split-plot designs with four and eight whole plots which have D-criterion values of 15.771 and 17.040, respectively. Also the split-split-plot design is outperformed by the staggered design as it has a D-criterion value of 19.124 only. Thus, the staggered design is 26% better than the split-plot design with four whole plots in terms of the D-optimality criterion, 17% better than the split-plot design with eight whole plots and 4% better than the split-split-plot design. Evidently, a 4% efficiency gain is not large, but we must not forget that the gain in D-efficiency is obtained by using a cheaper design. Thus, the staggered design is better in terms of cost and in terms of D-efficiency. The trace of the variance-covariance matrix of the parameter estimates given in (5) was also calculated to evaluate the different designs in terms of the A-optimality criterion. As the staggered design yields a smaller trace, that is 0.525, than the split-plot design with four whole plots (0.875), the split-plot design with eight whole plots (0.688), and the split-split-plot design (0.563), it is also more desirable in terms of the A-optimality criterion.

To complete the comparison of the four designs, we also investigated the correlation ma-

Table 8: Correlation matrix of the parameter estimates for the staggered design in Table 2 calculated with $\eta_1 = 1$, $\eta_2 = 0.5$ and $\sigma_\varepsilon^2 = 0.5$

	β_0	β_w	β_s	β_{t_1}	β_{t_2}	β_{ws}	β_{wt_1}	β_{wt_2}	β_{st_1}	β_{st_2}	$\beta_{t_1t_2}$
β_0	1	0	-0.074	0	0	0	0	0	0	0	-0.095
β_w	0	1	0	0	0	0.076	0	0	0	0	0
β_s	-0.074	0	1	0	0	0	0	0	0	0	-0.150
β_{t_1}	0	0	0	1	0	0	0	0	0	0	0
β_{t_2}	0	0	0	0	1	0	0	0	0	0	0
β_{ws}	0	0.076	0	0	0	1	0	0	0	0	0
β_{wt_1}	0	0	0	0	0	0	1	0	0	0	0
β_{wt_2}	0	0	0	0	0	0	0	1	0	0	0
β_{st_1}	0	0	0	0	0	0	0	0	1	0	0
β_{st_2}	0	0	0	0	0	0	0	0	0	1	0
$\beta_{t_1t_2}$	-0.095	0	-0.150	0	0	0	0	0	0	0	1

trix of the parameter estimates. The correlation matrix for the staggered design is shown in Table 8. It turns out that there is some correlation between a limited number of pairs of parameter estimates. For example, there is a correlation of 0.076 between the most hard-to-change factor's main-effect estimate and the estimate of the interaction effect between the two hard-to-change factors w and s. Furthermore one can observe a negative correlation of -0.150 between the main-effect estimate of s and the estimate of the interaction effect between the easy-to-change factors, t_1 and t_2. These non-zero correlations could be considered to be a statistical disadvantage of the staggered design, since the split-plot and split-split-plot designs yield uncorrelated estimates. However, only four out of the 55 correlations are non-zero, and, furthermore, the largest absolute correlation is only 0.150.

4.2 32 runs

The excellent performance of the staggered design in Table 2 stimulated us to apply the principle of not resetting the levels of the hard-to-change factors at the same time to larger designs than the 2^4 factorial design. We start by looking at a staggered arrangement of the 2^5 factorial design. For this extension, we used a simple combinatorial construction method, which will be described in Section 5, because the two algorithms we tested did not yield attractive design options. As we shall see below, the combinatorially constructed designs outperform the split-plot and split-split-plot designs.

Again several good experimental design options can be constructed using some of the

Table 9: D-optimal 32-run split-plot design with eight whole plots of size four for a main-effects model with two-factor interactions in two hard-to-change factors, w and s, and three easy-to-change factors t_1, t_2 and t_3

Run	WP	w	s	t_1	t_2	t_3	Run	WP	w	s	t_1	t_2	t_3
1	1	-1	1	1	-1	-1	17	5	-1	1	-1	-1	1
2	1	-1	1	1	1	1	18	5	-1	1	1	-1	-1
3	1	-1	1	-1	-1	1	19	5	-1	1	1	1	1
4	1	-1	1	-1	1	-1	20	5	-1	1	-1	1	-1
5	2	1	1	1	-1	1	21	6	1	-1	1	-1	-1
6	2	1	1	-1	1	1	22	6	1	-1	-1	-1	1
7	2	1	1	1	1	-1	23	6	1	-1	1	1	1
8	2	1	1	-1	-1	-1	24	6	1	-1	-1	1	-1
9	3	-1	-1	-1	1	1	25	7	1	1	1	1	-1
10	3	-1	-1	1	-1	1	26	7	1	1	1	-1	1
11	3	-1	-1	-1	-1	-1	27	7	1	1	-1	1	1
12	3	-1	-1	1	1	-1	28	7	1	1	-1	-1	-1
13	4	1	-1	-1	-1	1	29	8	-1	-1	1	-1	1
14	4	1	-1	-1	1	-1	30	8	-1	-1	1	1	-1
15	4	1	-1	1	-1	-1	31	8	-1	-1	-1	-1	-1
16	4	1	-1	1	1	1	32	8	-1	-1	-1	1	1

approaches in the literature. The D-optimal 32-run split-plot design in five factors would then be one where both w and s are set eight times. This design, which is a 2^5 factorial design with blocking relations w, s and $t_1t_2t_3$, is shown in Table 9. Like in the 16-run case, a D-optimal split-plot design can be constructed with half the number of whole plots. This design is not discussed here because it is inferior to the design in Table 9 in terms of the D- and A-optimality criterion. The best 32-run split-split-plot design in terms of the D-optimality criterion involves four settings of w and eight settings of s. It is displayed in Table 10.

The design we propose is shown in Table 11. In the design, the most hard-to-change factor w is held constant for sequences of eight runs. For example, in the first eight runs the factor is held at its low level. Then, there is a reset and the next eight runs are performed at the factor's high level. Eventually this results in four settings of the most hard-to-change factor w, just like in the split-split-plot design. The least hard-to-change factor is at its high level during the first four runs and during the last four runs so that the design begins and ends with a group of runs of size four. All intermediate groups involve eight runs. Between these groups, the level of the least hard-to-change factor is alternated. This makes that s is set five times instead of eight like in the split-plot and split-split-plot designs. This is again a reduction in the number of settings of almost 38%. The split-plot design is the most expensive option for running the 2^5 full factorial design

Table 10: D-optimal 32-run split-split-plot design with four whole plots each consisting of two subplots of size four for a main-effects model with two-factor interactions in two hard-to-change factors, w and s, and three easy-to-change factors t_1, t_2 and t_3

Run	WP	SP	w	s	t_1	t_2	t_3	Run	WP	SP	w	s	t_1	t_2	t_3
1	1	1	-1	-1	-1	-1	-1	17	3	5	1	1	-1	-1	1
2	1	1	-1	-1	-1	1	1	18	3	5	1	1	1	1	1
3	1	1	-1	-1	1	-1	1	19	3	5	1	1	1	-1	-1
4	1	1	-1	-1	1	1	-1	20	3	5	1	1	-1	1	-1
5	1	2	-1	1	1	1	-1	21	3	6	1	-1	1	-1	-1
6	1	2	-1	1	-1	1	1	22	3	6	1	-1	-1	-1	1
7	1	2	-1	1	1	-1	1	23	3	6	1	-1	-1	1	-1
8	1	2	-1	1	-1	-1	-1	24	3	6	1	-1	1	1	1
9	2	3	1	-1	1	-1	1	25	4	7	-1	1	1	1	1
10	2	3	1	-1	1	1	-1	26	4	7	-1	1	-1	-1	1
11	2	3	1	-1	-1	1	1	27	4	7	-1	1	1	-1	-1
12	2	3	1	-1	-1	-1	-1	28	4	7	-1	1	-1	1	-1
13	2	4	1	1	1	-1	1	29	4	8	-1	-1	1	-1	-1
14	2	4	1	1	1	1	-1	30	4	8	-1	-1	1	1	1
15	2	4	1	1	-1	1	1	31	4	8	-1	-1	-1	1	-1
16	2	4	1	1	-1	-1	-1	32	4	8	-1	-1	-1	-1	1

due to the double number of settings of w in comparison to the staggered and the split-split-plot design. As a consequence, the split-plot design allows a more precise estimation of the main effect of w. This is shown in Table 12. The results in this table were again obtained using $\eta_\delta = 1$, $\eta_\gamma = 0.5$ and $\sigma^2_\varepsilon = 0.5$.

The other results are also similar to those for the 2^4 full factorial design. Compared to the split-plot design, the staggered design gives a more precise estimate of the main effect of s and the interaction effect between w and s. Compared to the split-split-plot design, the staggered design results in a better estimation of the main effect of w and the interaction effect between w and s. The 32-run staggered design also outperforms the two other designs in terms of D-optimality. It has a D-criterion value of 42.521 whereas the split-plot and the split-split-plot designs have D-criterion values of only 39.346 and 41.339, respectively. The staggered design is thus 8% better than the split-plot design and 3% better than the split-split-plot design in terms of the D-optimality criterion. Looking at the trace of the variance-covariance matrix of the parameter estimates learns that the staggered design also performs better in terms of the A-optimality criterion. It has an A-criterion value of 0.424 compared to 0.516 and 0.453 for the split-plot and split-split-plot design, respectively. As a result, the design that is cheapest to conduct is also the one that is most efficient statistically.

Table 11: 32-run staggered design for a main-effects model with two-factor interactions in two hard-to-change factors, w and s, and three easy-to-change factors t_1, t_2 and t_3. The design involves four settings of the most hard-to-change factor and five settings of the least hard-to-change factor.

Run	w	s	t_1	t_2	t_3	Run	w	s	t_1	t_2	t_3
1	-1	1	1	1	-1	17	-1	1	1	1	1
2	-1	1	1	-1	1	18	-1	1	1	-1	-1
3	-1	1	-1	1	1	19	-1	1	-1	1	-1
4	-1	1	-1	-1	-1	20	-1	1	-1	-1	1
5	-1	-1	1	1	1	21	-1	-1	1	1	-1
6	-1	-1	1	-1	-1	22	-1	-1	1	-1	1
7	-1	-1	-1	1	-1	23	-1	-1	-1	1	1
8	-1	-1	-1	-1	1	24	-1	-1	-1	-1	-1
9	1	-1	1	1	1	25	1	-1	1	1	-1
10	1	-1	1	-1	-1	26	1	-1	1	-1	1
11	1	-1	-1	1	-1	27	1	-1	-1	1	1
12	1	-1	-1	-1	1	28	1	-1	-1	-1	-1
13	1	1	1	1	-1	29	1	1	1	1	1
14	1	1	1	-1	1	30	1	1	1	-1	-1
15	1	1	-1	1	1	31	1	1	-1	1	-1
16	1	1	-1	-1	-1	32	1	1	-1	-1	1

Table 12: Variances of estimates of fixed model parameters for the 32-run staggered design in Table 11, the split-plot design in Table 9 and the split-split-plot design in Table 10 when $\eta_\delta = 1$, $\eta_\gamma = 0.5$ and $\sigma_\varepsilon^2 = 0.5$

effect	staggered Table 11	split-plot Table 9	split-split-plot Table 10
w	0.147	0.109	0.172
s	0.069	0.109	0.047
t_1	0.016	0.016	0.016
t_2	0.016	0.016	0.016
t_3	0.016	0.016	0.016
ws	0.022	0.109	0.047
other	0.016	0.016	0.016
D-criterion	42.521	39.346	41.339
A-criterion	0.424	0.516	0.453

Table 13: Correlation matrix of the parameter estimates for the staggered design in Table 11 calculated with $\eta_\delta = 1$ and $\eta_\gamma = 0.5$

	β_0	β_w	β_s	β_{t_1}	β_{t_2}	β_{t_3}	β_{ws}	β_{wt_1}	β_{wt_2}	β_{wt_3}	β_{st_1}	β_{st_2}	β_{st_3}	$\beta_{t_1t_2}$	$\beta_{t_1t_3}$	$\beta_{t_2t_3}$
β_0	1	0	0.078	0	0	0	0	0	0	0	0	0	0	0	0	0
β_w	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0
β_s	0.078	0	1	0	0	0	-0.180	0	0	0	0	0	0	0	0	0
β_{t_1}	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0
β_{t_2}	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0
β_{t_3}	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0
β_{ws}	0	0	-0.180	0	0	0	1	0	0	0	0	0	0	0	0	0
β_{wt_1}	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0
β_{wt_2}	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0
β_{wt_3}	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0
β_{st_1}	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0
β_{st_2}	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0
β_{st_3}	0	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0
$\beta_{t_1t_2}$	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0	0
$\beta_{t_1t_3}$	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	0
$\beta_{t_2t_3}$	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1

Furthermore the correlation matrix of the parameter estimates of the staggered design, shown in Table 13, demonstrates another nice feature of the staggered design. Only two out of the 120 possible correlations are different from zero. First, there is a negligible correlation of 0.078 between the estimate of the main effect of the least hard-to-change factor, s, and the estimate of the intercept. Second, there is a small negative correlation of -0.18 between the estimate of the main effect of s and the estimate of the interaction effect between the two hard-to-change factors.

A design that is 1% better than the staggered design in Table 11 in terms of the D-optimality criterion is the design in Table 3 constructed by the modified variable-neighbourhood search algorithm. However, the improvement in D-criterion value is minor compared to the large number of settings of the hard-to-change factors required by that design. The staggered design in Table 11 thus offers a much better trade-off between cost and statistical efficiency.

5 Structure of the staggered design

Since the modified variable-neighbourhood search algorithm as well as the coordinate-exchange algorithm did not produce desirable designs for problems involving more than four factors, we applied the structure of the staggered designs in Tables 2 and 11 to larger two-level factorial designs. To do so, it was necessary to analyze the structure of these designs.

5.1 16- and 32-run designs

First, consider the new 16-run design in Table 2. When looking at the settings of the most hard-to-change factor w, the runs are divided in four groups of size four with w at its low level in the first group. The runs are also divided in groups by the settings of the least hard-to-change factor s. This division begins and ends with a subset of runs half as large as the subsets defined by the settings of w, i.e. with subsets of size two, and with the factor s at its high level. This results in the structure shown in Table 14. In the table, the symbol $\mathbf{1}_d$ represents a d-dimensional vector of ones.

Table 14: Basic structure of the 16-run staggered design in Table 2

w	s	t_1, t_2
$-\mathbf{1}_4$	$\mathbf{1}_2$	Block 1
	$-\mathbf{1}_4$	Block 2
$\mathbf{1}_4$		Block 1
	$\mathbf{1}_4$	Block 2
$-\mathbf{1}_4$		Block 2
	$-\mathbf{1}_4$	Block 1
$\mathbf{1}_4$		Block 2
	$\mathbf{1}_2$	Block 1

The remaining problem is to determine the levels of the two easy-to-change factors for each run. It should be clear from Table 14 that the design for the easy-to-change factors consists of blocks of size two. Selecting the levels of t_1 and t_2 therefore comes down to arranging the runs of a quadruplicated 2^2 factorial design in eight blocks of size two. This can be done using block generator $B = t_1t_2$ for each of the four 2^2 factorial designs, which leads to the following two blocks:

$$\text{block 1} = \begin{pmatrix} -1 & -1 \\ 1 & 1 \end{pmatrix} \text{ and block 2} = \begin{pmatrix} -1 & 1 \\ 1 & -1 \end{pmatrix}.$$

Each of these blocks appears four times in the entire design and must be assigned to one of the eight available positions. Each block must occur with each possible combination of levels of w and s to ensure that the design contains all the points of a 2^4 full factorial design. Using the two blocks in alternating order leads to the best possible design in terms of the D- and A-optimality criteria. The order of the blocks does matter here because they are of size two. This makes that the two-factor interaction effect between t_1 and t_2 cannot be made orthogonal to the subsets of runs determined by the independent settings of w and s.

This construction method also applies to the new 32-run design shown in Table 11 so that this highly efficient design can also be easily constructed by hand. The runs are divided in four sets of size eight by the settings of the most hard-to-change factor w, and the factor w is at its low level in the first subset. When looking at the division of the runs defined by the settings of the least hard-to-change factor s, the design again starts and ends with a group of runs only half as large as the groups determined by w, and with the factor s at its high level. The remaining groups are of the same size as the groups determined by

w. This yields the structure in Table 15.

Table 15: Basic structure of the 32-run staggered design in Table 11

w	s	t_1, t_2, t_3
$-\mathbf{1}_8$	$\mathbf{1}_4$	Block 1 or 2
	$-\mathbf{1}_8$	Block 1 or 2
$\mathbf{1}_8$		Block 1 or 2
	$\mathbf{1}_8$	Block 1 or 2
$-\mathbf{1}_8$		Block 1 or 2
	$-\mathbf{1}_8$	Block 1 or 2
$\mathbf{1}_8$		Block 1 or 2
	$\mathbf{1}_4$	Block 1 or 2

It is clear from the table that the design for the three easy-to-change factors, t_1, t_2 and t_3, consists of blocks of size four instead of two. The possible values of t_1, t_2 and t_3 are the points of a quadruplicated 2^3 full factorial design. Selecting the levels of t_1, t_2 and t_3 therefore comes down to arranging the runs of a quadruplicated 2^3 factorial design in eight blocks of size four. This can be done using block generator $B = t_1t_2t_3$ for each of the four 2^3 factorial designs, and leads to the following two blocks:

$$\text{block 1} = \begin{pmatrix} 1 & 1 & 1 \\ 1 & -1 & -1 \\ -1 & 1 & -1 \\ -1 & -1 & 1 \end{pmatrix} \text{ and block 2} = \begin{pmatrix} 1 & 1 & -1 \\ 1 & -1 & 1 \\ -1 & 1 & 1 \\ -1 & -1 & -1 \end{pmatrix}.$$

These two blocks are then arranged so that each block occurs with each possible combination of levels of w and s to ensure that the design contains all the points of a 2^5 full factorial design. When these two blocks are used the columns corresponding to t_1, t_2 and t_3 are orthogonal to the subsets of runs determined by the settings of w and s, just like the colums corresponding to all two-factor interactions involving at least one of the three easy-to-change factors. Therefore, the assignment of the blocks to the eight available positions has no impact on the quality of the design if the interest is in the main-effects-plus-two-factor-interactions model.

5.2 Larger designs

The approach described here can be generalized to factorial designs with six and more two-level factors. These situations are not discussed in detail here since the overall conclusions are similar to those mentioned above. We merely provide some guidelines for these more complex situations where there are two hard-to-change factors.

It should be clear from the discussion above that the settings of the most hard-to-change factor w divide the experimental runs in groups with a size that is a power of two. The settings of the least hard-to-change factor divide the runs in groups of two different sizes. The first and last group are half as large as the groups determined by w, whereas the other groups have the same size. The exact sizes of the groups formed by the settings of both hard-to-change factors depend on the number of factors. Since the size of the groups formed by the most hard-to-change factor w determines the size of the blocks for the easy-to-change factors, it is recommendable to select a size for which there will be no confounding between the two-factor interaction effects of the easy-to-change factors and the subsets of runs dictated by the hard-to-change factors. This is the reason why, for the 32-run design, groups of runs of size eight and thus four independent settings of the most hard-to-change factor were chosen. This resulted in blocks of size four for the easy-to-change factors. For six factors we recommend to choose the same group size, while for seven factors is it advisable to increase the group sizes corresponding to the most hard-to-change factor to sixteen and thus to increase the block sizes to eight to avoid confounding.

Once the number of the independent settings of both hard-to-change factors has been determined, a block generator can be used to arrange the various easy-to-change factor level combinations in the blocks of the required size and spread these blocks over the combinations of the most and least hard-to-change factor levels. For the 64-run design, the assignment of the blocks does matter, while it does not for the 128-run design.

The 64- and 128-run staggered designs, constructed using this method and using an optimal assignment of the blocks, are given in Table 18 and 19 in the appendix. These two designs outperform the traditional split-plot and split-split-plot designs in terms of A- and D-optimality and require fewer resets of the less hard-to-change factor. For example, the new 64-run design has almost 44% fewer resets of s than a split-plot design with 16 whole plots and a split-split-plot design with eight whole plots and 16 subplots.

6 Sensitivity to η_δ and η_γ

As mentioned in Section 4, the relative performance of the competing designs in terms of D- and A-optimality depends on the two variance ratios η_δ and η_γ. Therefore it is necessary to investigate the effects of changing these two variance ratios over broad ranges. This will enable us to find out whether the staggered design outperforms the alternatives in all practical instances. Three situations will be considered: small, average and large η_δ. For each of these, three or four values for η_γ were used. Initially, the focus is on situations where $\eta_\delta > \eta_\gamma$, which is the most realistic scenario given that η_δ corresponds to the factor that is hardest to change. We focus on the sensitivity studies done for the 16- and 32-run designs in Tables 2, 5, 6, 9, 10 and 11. The results are shown in Table 16.

Table 16: D-and A-efficiencies of the staggered design relative to the split-plot and split-split-plot designs for the 16- and 32-run cases for various values of η_δ and η_γ

			16-run		32-run	
			split-plot	split-split-plot	split-plot	split-split-plot
η_δ=0.1	$\eta_\gamma = 0.025$	D-eff	1.022	1.001	1.010	1.001
		A-eff	1.042	1.000	1.032	1.008
	$\eta_\gamma = 0.05$	D-eff	1.023	1.004	1.010	1.002
		A-eff	1.061	1.021	1.035	1.008
	$\eta_\gamma = 0.075$	D-eff	1.025	1.006	1.011	1.004
		A-eff	1.047	1.009	1.039	1.016
η_δ=1	$\eta_\gamma = 0.1$	D-eff	1.204	1.006	1.110	1.004
		A-eff	1.276	1.004	1.182	1.013
	$\eta_\gamma = 0.25$	D-eff	1.182	1.019	1.091	1.014
		A-eff	1.302	1.042	1.193	1.033
	$\eta_\gamma = 0.75$	D-eff	1.165	1.061	1.079	1.041
		A-eff	1.339	1.116	1.237	1.100
η_δ=10	$\eta_\gamma = 0.1$	D-eff	1.951	1.006	1.408	1.004
		A-eff	1.796	1.006	1.421	1.003
	$\eta_\gamma = 0.5$	D-eff	1.763	1.031	1.315	1.008
		A-eff	1.781	1.022	1.420	1.017
	$\eta_\gamma = 1$	D-eff	1.640	1.054	1.262	1.037
		A-eff	1.760	1.041	1.422	1.035
	$\eta_\gamma = 5$	D-eff	1.377	1.137	1.153	1.076
		A-eff	1.686	1.167	1.442	1.146

When η_δ is as small as 0.1, the differences between the staggered design and the split-plot and split-split-plot designs are small. The staggered design is 2% better in terms of D-optimality than the D-optimal split-plot design from Table 5, while the new 32-run design is only marginally better than the split-plot design in terms of D-optimality. Thus the staggered design remains the best option when $\eta_\delta = 1$, since statistically it is better than or as good as the split-plot and split-split-plot designs, and, more importantly, it is more cost efficient.

When η_δ is equal to one, the staggered design performs 16 to 20% better than the split-plot design in case of 16 runs and 8 to 11% better in case of 32 runs in terms of the D-optimality criterion, depending on the value of η_γ. The D-efficiency of the staggered design relative to the split-plot design decreases with η_γ. The D-efficiency relative to the more expensive split-split-plot design is smaller (but larger than one) and increases with η_γ when $\eta_\delta = 1$. Finally when η_δ is as large as ten, the staggered design is a lot better than the split-plot design and also outperforms the split-split-plot design, especially for larger values of η_γ.

Table 16 also displays the A-efficiencies of the staggered designs relative to split-plot and split-split-plot designs. From these relative A-efficiencies, it will be clear that the staggered design is the best of the three design options for any value of η_δ and η_δ in terms of the A-optimality criterion too.

As mentioned earlier, in most practical applications η_δ will be larger than η_γ. However, even when this assumption is not correct, the staggered design outperforms the split-plot and split-split-plot designs in terms of the D-optimality criterion. For instance, when $\eta_\delta = 0.5$ and $\eta_\gamma = 1$ the 16-run staggered design is 9% better than the split-plot design and 10% better than the split-split-plot design.

As a result, the 16- and 32-run staggered designs are good design options for any possible value of the variance ratios. They are always statistically better than or as good as the split-plot and split-split-plot designs and very cost efficient to perform. Similar results were obtained for the 64- and 128-run designs.

7 Possible deviation from predescribed structure

The staggered designs presented in Section 4 all have a fixed structure which is described thoroughly in Section 5. Concerning this structure, several questions might arise:

1. Is it possible to swap the levels of the hard-to-change factors?
2. Can the small groups of runs determined by the independent setting of s be placed somewhere else instead of at the beginning and end of the experiment?
3. What is the consequence of not alternating the two blocks with easy-to-change factor levels for the 16-run design?

First, it is indeed possible to swap the levels of the hard-to-change factors. In the designs shown in the previous sections the most hard-to-change factor w is at its low level at the start of the experiment while the least hard-to-change factor s is at its high level. It is possible to begin with the two hard-to-change factors both at their low or high level without any loss of efficiency.

A more interesting question perhaps is the second one, i.e. whether the groups of runs determined by the settings of s can be placed somewhere else instead of at the beginning and end of the experiment. To answer this question some other possible scenarios were investigated.
Figure 1 shows the different arrangements of the four subsets determined by the independent settings of w and the five subsets dictated by the settings of s used in each of these scenarios for the 16-run design. The best alternative is Option 1, which has a D-criterion value of 17.506 and a relative efficiency of 88% in comparison to the staggered design presented in Table 2. The worst alternative is Option 2, which has a D-criterion value of 15.800 and a relative efficiency of 79%.

A similar study was done for the 32-run design. Figure 2 provides an overview of the structures we investigated. Here, the best alternative is again Option 1, which results in a D-criterion value of 37.178 and an efficiency of 87% relative to the staggered design in Table 11. The worst alternative, Option 2, has a D-criterion value of 34.059 and a relative efficiency of 69%.

Finally, in Section 4 it is mentioned that the order of the two blocks for the easy-to-change factors does matter in the case of the 16-run design presented in Table 2. An algorithmic

Figure 1: Other possible structures for the staggered 2^4 design

<table>
<tr><th colspan="2">Option 1</th><th colspan="2">Option 2</th><th colspan="2">Option 3</th><th colspan="2">Option 4</th></tr>
<tr><th>w</th><th>s</th><th>w</th><th>s</th><th>w</th><th>s</th><th>w</th><th>s</th></tr>
<tr><td rowspan="2">$-\mathbf{1}_4$</td><td rowspan="2">$-\mathbf{1}_4$</td><td rowspan="2">$-\mathbf{1}_4$</td><td>$\mathbf{1}_2$</td><td rowspan="2">$\mathbf{1}_4$</td><td>$\mathbf{1}_2$</td><td rowspan="2">$-\mathbf{1}_4$</td><td rowspan="2">$-\mathbf{1}_4$</td></tr>
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<tr><td>$\mathbf{1}_2$</td></tr>
</table>

Figure 2: Other possible structures for the staggered 2^5 design

<table>
<tr><th colspan="2">Option 1</th><th colspan="2">Option 2</th><th colspan="2">Option 3</th><th colspan="2">Option 4</th></tr>
<tr><th>w</th><th>s</th><th>w</th><th>s</th><th>w</th><th>s</th><th>w</th><th>s</th></tr>
<tr><td rowspan="2">$-\mathbf{1}_8$</td><td rowspan="2">$-\mathbf{1}_8$</td><td rowspan="2">$-\mathbf{1}_8$</td><td>$\mathbf{1}_4$</td><td rowspan="2">$\mathbf{1}_8$</td><td>$\mathbf{1}_4$</td><td rowspan="2">$-\mathbf{1}_8$</td><td rowspan="2">$-\mathbf{1}_8$</td></tr>
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<tr><td rowspan="2">$\mathbf{1}_8$</td><td rowspan="2">$-\mathbf{1}_8$</td><td rowspan="2">$-\mathbf{1}_8$</td><td rowspan="2">$-\mathbf{1}_8$</td><td rowspan="2">$\mathbf{1}_8$</td><td rowspan="2">$\mathbf{1}_8$</td><td rowspan="2">$-\mathbf{1}_8$</td></tr>
<tr><td>$\mathbf{1}_4$</td></tr>
</table>

search revealed that these two blocks should appear in alternating order. In Figure 3 some other possible orders are shown. All of these are of inferior quality in terms of the D-criterion. The worst alternative is Option 1 which has a D-criterion value of 17.696 and a D-efficiency of 89%. The best alternative is Option 3 which has a D-criterion value of 19.419 and a D-efficiency of 98%.

Even though it is not difficult to construct designs that can be run at the same cost as the staggered designs, it should be clear that only a few modifications can be made without harming its statistical efficiency.

Figure 3: Other possible orders of the two blocks for the easy-to-change factors in the staggered 16-run design

		Original design	Option 1	Option 2	Option 3
w	s	e-t-c	e-t-c	e-t-c	e-t-c
$-\mathbf{1}_4$	$\mathbf{1}_2$	block1	block1	block1	block1
	$-\mathbf{1}_4$	block2	block1	block1	block2
$\mathbf{1}_4$		block1	block1	block2	block2
	$\mathbf{1}_4$	block2	block1	block2	block1
$-\mathbf{1}_4$		block2	block2	block2	block2
	$-\mathbf{1}_4$	block1	block2	block2	block1
$\mathbf{1}_4$		block2	block2	block1	block1
	$\mathbf{1}_2$	block1	block2	block1	block2

8 Discussion

In this paper, we have studied experiments with two hard-to-change factors, one of which was considered harder to change than the other. In practice it is possible that there are several most hard-to-change factors and several least hard-to-change factors. The ideas for constructing a staggered design in the presence of hard-to-change factors discussed here can obviously be extended to such situations.

Since the number of runs of the two-level factorial designs considered here grows exponentially with the number of factors, it is certainly also interesting to expand the idea of resetting the different hard-to-change factors at different points in time to fractional factorial designs. Although it is not obvious whether regular or nonregular fractional factorial designs should be used, a detailed exploration of this issue will most likely build on the work by Bingham and Sitter (1999, 2001, 2003) and Bingham, Schoen and Sitter (2004).

In some cases we came across slightly more statistically efficient designs than the ones shown in this paper. This was especially the case when we sought for optimal run orders for the 32-run 2^5 factorial design. The staggered design proposed in Table 11 involves four independent settings of the most hard-to-change factor w and five settings of the least hard-to-change factor s. In some situations a design with eight settings of w and nine settings of s was statistically slightly better. However, we do not recommend the design for three reasons. First, as shown in Table 17, the statistical advantage of the alternative design is limited. The largest gain in D-efficiency is merely 2%. Second, the alternative design is less efficient than the one in Table 2 as soon η_γ is larger than 0.5 or when η_δ is large. Third, the alternative design is far more expensive to run than the one in Table 11

Table 17: D-criterion values and relative D-efficiencies of a 32-run staggered design with four independent settings of the most hard-to-change factor w and five independent settings of the least hard-to-change factor s (labelled I and displayed in Table 11) and a 32-run design with eight and nine independent settings of w and s (labelled II)

		I	II
$\eta_\delta = 0.1$	D-opt	58.399	59.271
$\eta_\gamma = 0.025$	rel.eff	1	1.015
$\eta_\delta = 0.1$	D-opt	57.480	58.438
$\eta_\gamma = 0.05$	rel.eff	1	1.017
$\eta_\delta = 0.1$	D-opt	56.665	57.701
$\eta_\gamma = 0.075$	rel.eff	1	1.018
$\eta_\delta = 1$	D-opt	46.561	47.036
$\eta_\gamma = 0.1$	rel.eff	1	1.010
$\eta_\delta = 1$	D-opt	44.610	44.818
$\eta_\gamma = 0.25$	rel.eff	1	1.005
$\eta_\delta = 1$	D-opt	41.073	40.495
$\eta_\gamma = 0.75$	rel.eff	1	0.986
$\eta_\delta = 10$	D-opt	35.537	35.467
$\eta_\gamma = 0.1$	rel.eff	1	0.998

as it requires the first hard-to-change factor to be set eight times instead of four and the second hard-to-change factor to be set nine times instead of five.

In any case, it is clear that setting the hard-to-change factors at different points in time leads to efficient designs. The staggered designs proposed here are very appealing when one or more hard-to-change factors are harder to change than others.

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Appendix

The 64- and 128-run staggered designs are displayed in Table 18 and 19.

Table 18: 64-run staggered design for a main-effects model with two-factor interactions in two hard-to-change factors, w and s, and four easy-to-change factors t_1-t_4. The design involves eight settings of the most hard-to-change factor and nine settings of the least hard-to-change factor.

Run	w	s	t_1	t_2	t_3	t_4	Run	w	s	t_1	t_2	t_3	t_4
1	-1	1	1	1	1	-1	33	-1	1	1	-1	1	1
2	-1	1	1	1	-1	1	34	-1	1	1	-1	-1	-1
3	-1	1	-1	-1	1	1	35	-1	1	-1	1	-1	1
4	-1	1	-1	-1	-1	-1	36	-1	1	-1	1	1	-1
5	-1	-1	1	-1	1	1	37	-1	-1	1	1	1	1
6	-1	-1	1	-1	-1	-1	38	-1	-1	1	1	-1	-1
7	-1	-1	-1	1	-1	1	39	-1	-1	-1	-1	1	-1
8	-1	-1	-1	1	1	-1	40	-1	-1	-1	-1	-1	1
9	1	-1	1	1	1	1	41	1	-1	1	-1	1	1
10	1	-1	1	1	-1	-1	42	1	-1	1	-1	-1	-1
11	1	-1	-1	-1	1	-1	43	1	-1	-1	1	-1	1
12	1	-1	-1	-1	-1	1	44	1	-1	-1	1	1	-1
13	1	1	1	-1	1	-1	45	1	1	1	1	1	-1
14	1	1	1	-1	-1	1	46	1	1	1	1	-1	1
15	1	1	-1	1	1	1	47	1	1	-1	-1	1	1
16	1	1	-1	1	-1	-1	48	1	1	-1	-1	-1	-1
17	-1	1	1	1	1	1	49	-1	1	1	-1	1	-1
18	-1	1	1	1	-1	-1	50	-1	1	1	-1	-1	1
19	-1	1	-1	-1	1	-1	51	-1	1	-1	1	1	1
20	-1	1	-1	-1	-1	1	52	-1	1	-1	1	-1	-1
21	-1	-1	1	-1	1	-1	53	-1	-1	1	1	1	-1
22	-1	-1	1	-1	-1	1	54	-1	-1	1	1	-1	1
23	-1	-1	-1	1	1	1	55	-1	-1	-1	-1	1	1
24	-1	-1	-1	1	-1	-1	56	-1	-1	-1	-1	-1	-1
25	1	-1	1	1	1	-1	57	1	-1	1	-1	1	-1
26	1	-1	1	1	-1	1	58	1	-1	1	-1	-1	1
27	1	-1	-1	-1	1	1	59	1	-1	-1	1	1	1
28	1	-1	-1	-1	-1	-1	60	1	-1	-1	1	-1	-1
29	1	1	1	-1	1	1	61	1	1	1	1	1	1
30	1	1	1	-1	-1	-1	62	1	1	1	1	-1	-1
31	1	1	-1	1	-1	1	63	1	1	-1	-1	1	-1
32	1	1	-1	1	1	-1	64	1	1	-1	-1	-1	1

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Table 19: 128-run staggered design for a main-effects model with two-factor interactions in two hard-to-change factors, w and s, and five easy-to-change factors t_1-t_5. The design involves eight settings of the most hard-to-change factor w and nine settings of the least hard-to-change factor.

Run	w	s	t_1	t_2	t_3	t_4	t_5	Run	w	s	t_1	t_2	t_3	t_4	t_5
1	-1	1	1	1	1	1	1	65	-1	1	1	1	-1	1	1
2	-1	1	1	1	1	-1	-1	66	-1	1	1	1	-1	-1	-1
3	-1	1	1	-1	-1	-1	-1	67	-1	1	1	-1	1	1	1
4	-1	1	-1	1	-1	1	-1	68	-1	1	1	-1	1	-1	-1
5	-1	1	-1	1	-1	-1	1	69	-1	1	-1	1	1	1	-1
6	-1	1	-1	-1	1	1	-1	70	-1	1	-1	1	1	-1	1
7	-1	1	-1	-1	1	-1	1	71	-1	1	-1	-1	-1	1	-1
8	-1	1	1	-1	-1	1	1	72	-1	1	-1	-1	-1	-1	1
9	-1	-1	1	1	1	1	-1	73	-1	-1	1	1	-1	1	-1
10	-1	-1	1	1	1	-1	1	74	-1	-1	1	1	-1	-1	1
11	-1	-1	1	-1	-1	1	-1	75	-1	-1	1	-1	1	1	-1
12	-1	-1	1	-1	-1	-1	1	76	-1	-1	1	-1	1	-1	1
13	-1	-1	-1	1	-1	1	1	77	-1	-1	-1	1	1	1	1
14	-1	-1	-1	1	-1	-1	-1	78	-1	-1	-1	1	1	-1	-1
15	-1	-1	-1	-1	1	1	1	79	-1	-1	-1	-1	-1	1	1
16	-1	-1	-1	-1	1	-1	-1	80	-1	-1	-1	-1	-1	-1	-1
17	1	- 1	1	1	-1	1	1	81	1	-1	1	1	1	1	1
18	1	-1	1	1	-1	-1	-1	82	1	-1	1	1	1	-1	-1
19	1	-1	1	-1	1	1	1	83	1	-1	1	-1	-1	-1	-1
20	1	-1	1	-1	1	-1	-1	84	1	-1	-1	1	-1	1	-1
21	1	-1	-1	1	1	1	-1	85	1	-1	-1	1	-1	-1	1
22	1	-1	-1	1	1	-1	1	86	1	-1	-1	-1	1	1	-1
23	1	-1	-1	-1	-1	1	-1	87	1	-1	-1	-1	1	-1	1
24	1	-1	-1	-1	-1	-1	1	88	1	-1	1	-1	-1	1	1
25	1	1	1	1	-1	1	-1	89	1	1	1	1	1	1	-1
26	1	1	1	1	-1	-1	1	90	1	1	1	1	1	-1	1
27	1	1	1	-1	1	1	-1	91	1	1	1	-1	-1	1	-1
28	1	1	1	-1	1	-1	1	92	1	1	1	-1	-1	-1	1
29	1	1	-1	1	1	1	1	93	1	1	-1	1	-1	1	1
30	1	1	-1	1	1	-1	-1	94	1	1	-1	1	-1	-1	-1
31	1	1	-1	-1	-1	1	1	95	1	1	-1	-1	1	1	1
32	1	1	-1	-1	-1	-1	-1	96	1	1	-1	-1	1	-1	-1
33	-1	1	1	1	1	1	-1	97	-1	1	1	1	-1	1	-1
34	-1	1	1	1	1	-1	1	98	-1	1	1	1	-1	-1	1
35	-1	1	1	-1	-1	1	-1	99	-1	1	1	-1	1	1	-1
36	-1	1	1	-1	-1	-1	1	100	-1	1	1	-1	1	-1	1
37	-1	1	-1	1	-1	1	1	101	-1	1	-1	1	1	1	1
38	-1	1	-1	1	-1	-1	-1	102	-1	1	-1	1	1	-1	-1
39	-1	1	-1	-1	1	1	1	103	-1	1	-1	-1	-1	1	1
40	-1	1	-1	-1	1	-1	-1	104	-1	1	-1	-1	-1	-1	-1
41	-1	-1	1	1	1	1	1	105	-1	-1	1	1	-1	1	1
42	-1	-1	1	1	1	-1	-1	106	-1	-1	1	1	-1	-1	-1
43	-1	-1	1	-1	-1	-1	-1	107	-1	-1	1	-1	1	1	1
44	-1	-1	-1	1	-1	1	-1	108	-1	-1	1	-1	1	-1	-1
45	-1	-1	-1	1	-1	-1	1	109	-1	-1	-1	1	1	1	-1
46	-1	-1	-1	-1	1	1	-1	110	-1	-1	-1	1	1	-1	1
47	-1	-1	-1	-1	1	-1	1	111	-1	-1	-1	-1	-1	1	-1
48	-1	-1	1	-1	-1	1	1	112	-1	-1	-1	-1	-1	-1	1
49	1	-1	1	1	-1	1	-1	113	1	-1	1	1	1	1	-1
50	1	-1	1	1	-1	-1	1	114	1	-1	1	1	1	-1	1
51	1	-1	1	-1	1	1	-1	115	1	-1	1	-1	-1	1	-1
52	1	-1	1	-1	1	-1	1	116	1	-1	1	-1	-1	-1	1
53	1	-1	-1	1	1	1	1	117	1	-1	-1	1	-1	1	1
54	1	-1	-1	1	1	-1	-1	118	1	-1	-1	1	-1	-1	-1
55	1	-1	-1	-1	-1	1	1	119	1	-1	-1	-1	1	1	1
56	1	-1	-1	-1	-1	-1	-1	120	1	-1	-1	-1	1	-1	-1
57	1	1	1	1	-1	1	1	121	1	1	1	1	1	1	1
58	1	1	1	1	-1	-1	-1	122	1	1	1	1	1	-1	-1
59	1	1	1	-1	1	1	1	123	1	1	1	-1	-1	-1	-1
60	1	1	1	-1	1	-1	-1	124	1	1	-1	1	-1	1	-1
61	1	1	-1	1	1	1	-1	125	1	1	-1	1	-1	-1	1
62	1	1	-1	1	1	-1	1	126	1	1	-1	-1	1	1	-1
63	1	1	-1	-1	-1	1	-1	127	1	1	-1	-1	1	-1	1
64	1	1	-1	-1	-1	-1	1	128	1	1	1	-1	-1	1	1

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