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# A New Analysis Strategy for Designs With Complex Aliasing

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**ABSTRACT**

Nonregular designs are popular in planning industrial experiments for their run-size economy. These designs often produce partially aliased effects, where the effects of different factors cannot be completely separated from each other. In this article, we propose applying an adaptive lasso regression as an analytical tool for designs with complex aliasing. Its utility compared to traditional methods is demonstrated by analyzing real-life experimental data and simulation studies.

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## 1. Introduction

Screening designs are widely used for identifying important factors in industrial experiments. In many practical situations, engineers prefer to identify only a few factors that can be adjusted to yield outcomes with desired characteristics. This approach is appropriate because the effect sparsity principle (Wu and Hamada 2009, p. 173) suggests that only a handful of factors relative to the total number of factors needs to be considered. Regular designs can be used for such screening experiments, but a disadvantage of such designs is that the run sizes grow exponentially with the number of factors. For example, a full factorial design with $k$ factors at 2 levels requires $2^k$ runs. Fractional factorial designs reduce the number of runs at the expense of making the estimates for the effects of certain factors indistinguishable from one another based on the observed data. This is especially the case for the conventional orthogonal components (OC) parameterization of factorial effects (Wu and Hamada 2009, p. 274), which we consider throughout for analyzing an experiment. Specifically, under their run size reductions, these designs force sets of main effects and/or interactions to be completely correlated, or aliased, under the OC system, making it impossible to disentangle their effects without additional runs.

Nonregular designs avoid these pitfalls by forcing factors and interaction effects to be partially, but not completely, aliased. Popular examples of such designs include orthogonal arrays (Hedayat, Sloane, and Stufken 1999) and Plackett–Burman designs (Plackett and Burman 1946). Hamada and Wu (1992) recognized that one can take advantage of this partial correlation and, with additional assumptions, effectively use nonregular designs to identify significant interactions that would otherwise be missed.[1]

However, the existing methods available in the literature for analyzing data from such designs are not satisfactory. Some of these methods do not enforce effect heredity, which can lead to the identification of an uninterpretable model. Others rely on prior knowledge the researcher may not possess of the nature of the true model, such as assumptions on the maximum number of effects that are in the model. Some methods require supervision on the part of the researcher, or qualitative evaluations in certain steps of the method. This supervision can lead to the resulting model being somewhat subjective.

In this article, we propose a technique based on adaptive lasso regression (Zou 2006) for variable selection in the presence of partially aliased factorial effects. Our algorithm identifies a set of significant factors while enforcing the effect heredity principle, without requiring the researcher to possess any special knowledge on the nature of the true model, or to supervise the algorithm and make subjective decisions. In this regard, our algorithm provides a user-friendly and accurate approach to identify significant effects. Its effectiveness and consistency are demonstrated by means of simulations for a wide variety of models and designs.

As a motivating example, consider the experiment conducted by Brigham Young University graduate students to improve the accuracy of an automated car-fueling system. The details of the experiment are described in Grimshaw et al. (2001). The students identified 10 factors (ring type, ring thickness, lighting type, lighting angle, gas-cap angle ($Z$ axis), gas-cap angle ($Y$ axis skew), car-distance, threshold step-value, sharpening, and smoothing, denoted by $A$ to $J$ in our article) and employed a 20-run Plackett-Burman design to investigate how each factor affected the variance of the system's perception of distance. As we will see later, different algorithms identify different models, some of which are clearly better than others. For example, Method 1 (discussed in Section 3.1) identifies a model which does not obey the effect heredity principle, whereas the proposed method identifies a model that does.

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Supplementary materials for this article are available online. Please go to *www.tandfonline.com/r/TAS*.
[1]The interested reader may refer to the review paper by Xu, Phoa, and Wong (2009) for the development of research on nonregular designs.

The rest of the article is organized as follows: in Section 2, we discuss complex aliasing and the unique challenges it poses to variable selection algorithms. In Section 3, we describe some existing methods used for variable selection in designs with complex aliasing. In Section 4, we introduce the adaptive lasso and our algorithm. We conduct extensive simulations in Section 5 to demonstrate the performance of the proposed algorithm. Concluding remarks are provided in Section 6. Some details are relegated to the Appendix. We provide additional details of the simulation results in the supplementary materials.

## 2. What Is a Complex Aliasing Pattern?

Any experiment with fewer runs than the corresponding full factorial design must partially compromise the ability to estimate all factorial effects. This is because in these experiments, some factorial effects are *aliased* with one another. As a result, a factor not under consideration can confound the effect of a factor of interest to the researcher. Suppose a response $y$ is generated from the model $y = X_1\beta_1 + X_2\beta_2 + \epsilon$, whereas the assumed or working model is $y = X_1\beta_1 + \epsilon$. Here $X_1$ and $X_2$ are part of the model matrix whereas $\beta_1$ and $\beta_2$ represent the corresponding factorial effects, and $\epsilon$ is the random error. Under the working model, the ordinary least squares (OLS) estimates of the regression parameters are given by $\hat{\beta}_1 = (X_1^TX_1)^{-1}X_1^Ty$. These estimates is not unbiased, and it is easy to show that the expectation of $\hat{\beta}_1$ is $E(\hat{\beta}_1) = \beta_1 + (X_1^TX_1)^{-1}X_1^TX_2\beta_2$. Hence, the bias of $\hat{\beta}_1$ is $L\beta_2$, where $L = (X_1^TX_1)^{-1}X_1^TX_2$. This $L$ is called the alias matrix since it contains the aliasing coefficients for the estimate of $\beta_1$. Two effects are said to be "partially aliased" if the absolute value of their aliasing coefficient is strictly between 0 and 1. In case of "complete" aliasing, it becomes 1. For nonregular designs like orthogonal arrays or Plackett–Burman designs, an enormous number of two-factor interactions become partially aliased with the main effects. This justifies the use of the terminology "complex aliasing."

For illustration, consider the cast fatigue experiment discussed in Hunter, Hodi, and Eager (1982). The experimenters used a Plackett–Burman design, shown in the Appendix, with 12 runs to study the effects of seven factors (initial structure, bead size, pressure, heat, cooling rate, and polish final treatment, denoted by $A$ to $G$) on the fatigue life of weld-repair castings. Each factor includes a high and a low level, which are denoted by $+1$ and $-1$, respectively. The lifetime was recorded and the log of the value was calculated as the response (Wu and Hamada 2009). Note that the Plackett–Burman design used for this experiment ensures that main effects are not fully aliased with each other and, instead, are partially aliased with interaction effects. Employing the effect hierarchy principle to rule out significant effects involving more than two factors, the expectation of the OLS estimate for effect $D$, for example, is as follows[2]:

$$E(\hat{D}) = D - \frac{1}{3}AB + \frac{1}{3}AC + \frac{1}{3}AE + \frac{1}{3}AF - \frac{1}{3}AG - \frac{1}{3}BC + \frac{1}{3}BE - \frac{1}{3}BF - \frac{1}{3}BG - \frac{1}{3}CE - \frac{1}{3}CF - \frac{1}{3}CG - \frac{1}{3}EF - \frac{1}{3}EG + \frac{1}{3}FG. \quad (1)$$

Consequently, the OLS estimate for each main effect will be biased by the interaction effects. Even with the assumption that only main effects and two-factor interactions are nonzero, and all other factorial effects are ignorable, each of the main effects is partially aliased with 15 two-factor interactions. This is called complex aliasing.

## 3. Traditional Analysis Methods for Designs With Complex Aliasing

Three empirical principles, effect sparsity, effect hierarchy, and effect heredity, have often been used to analyze data from a screening experiment. Simply put, the effect sparsity principle suggests that the total number of important effects in a factorial experiment is small. The effect hierarchy principle (Chipman 1996) states that main effects are more likely to be important than two-factor interactions, which in turn are more likely to be important than three-factor interactions, and so on. And according to the effect heredity principle, it is unlikely that a two-factor interaction is significant unless at least one of its parent main effects is also significant. Li, Sudarsanam, and Frey (2006) conducted a meta-analysis of two-level factorial experiments in the literature, and find that all three principles hold. In particular, they found strong evidence in support of the effect heredity principle.

The two most popular frequentist methods use these principles to eliminate the necessity of searching through all possible model combinations, greatly reducing computation time and coding complexity. They are credited to Hamada and Wu (1992) and are described next.

### 3.1. Method 1

The first method, Method 1, uses a stepwise selection technique in determining the best model:

1. Let $k$ be the total number of factors. For each factor $A$, consider a model with $A$ and all possible two-factor interactions ($AB$, $AC$, $AD$, etc.) involving $A$. Apply a stepwise regression procedure to select a model and record its significant effects. Repeat this process for the rest of the $k-1$ factors. Out of the $k$ selected models, choose the best one and proceed to Step 2.
2. Use the stepwise regression procedure to select significant effects from a model that consists of all the main effects and the two-factor interactions from the best model in the previous step.
3. Following the effect heredity principle, consider all the effects identified in the previous step, as well as the two-factor interactions that have at least one parent factor identified as significant in the previous step. At this stage, interaction effects suggested by the experimenter may also be included.

[2]Part of the interaction effects matrix $X_2$ and the complete alias matrix $L$ are given in the Appendix.

Use the stepwise regression procedure to determine which effects are significant. Using this model, go to Step 2.
4. Repeat Steps 2 and 3 until there are no further changes in effects identified as significant.

Method 1 greatly simplifies variable selection since the method assumes that higher-order interactions are insignificant and includes, at most, two-factor interactions. The disadvantage of this method is that it is subjective since it incorporates expert opinion, and hence, effect heredity is not consistently enforced in all steps of the method. In some applications, this may give rise to an uninterpretable model.

### 3.2. Method 2

Relying on the effect sparsity principle, Method 2 assumes that the final model has no more than $h$ effects. This method uses a sensible model selection criterion, for example, the Mallows' $C_p$, the Akaike information criterion (AIC), or the Bayesian information criterion (BIC), to select significant variables from an exhaustive set of possible models. The steps for Method 2 are as follows:

1. Let $h$ be the maximum number of effects that may be contained in the final model.
2. Search through all possible models with no more than $h$ effects that satisfy the effect heredity principle.
3. Determine which model is best according to some model selection criteria and declare it the final model using Method 2.

The drawbacks for Method 2 are clear. It assumes that one already knows the maximum number of truly significant effects. In practice, the true number of significant effects is seldom known and must be computed through statistical analysis. Moreover, as $h$ becomes larger, the total number of evaluations multiplies, dramatically increasing the computation time of the method.

## 4. Proposed Method

The traditional methods, while not necessarily ineffective, rely on the experimenter having relatively detailed prior knowledge of the nature of the true model. As an alternative variable selection method more suited to experiments whose true model is unknown, we propose using a modification of lasso regression, adaptive lasso, for designs with complex aliasing.

Lasso regression solves a minimization problem similar to OLS regression, with the addition of an $\ell_1$ penalty on the size of the $\hat{\beta}$ coefficients (Tibshirani 1996). Adaptive lasso chooses each $\hat{\beta}_j$ by minimizing

$$\sum_{i=1}^{n}\left(y_i-\beta_0-\sum_{j=1}^{p}\beta_j x_{ij}\right)^2+\lambda\sum_{j=1}^{p}w_j\left|\beta_j\right| = \text{Residual sum of squares} + \text{penalty}, \quad (2)$$

where $p$ is the total number of factorial effects being entertained, $n$ is the run size, $i$ and $j$ are indices used to represent an observation and a factorial effect, respectively, and $\lambda\sum_{j=1}^{p}w_j\left|\beta_j\right|$ is the penalty on the size of the vector of factorial effects. The smaller the weight on a specific coefficient, the less the size of that coefficient is penalized in the regression, and the less likely it is that the coefficient will fall to 0 as $\lambda$ increases. Zou (2006) proposes assigning each $\hat{w}_j$ as a function of the OLS estimate of $\beta_j$ and demonstrates that the adaptive lasso using this weighting system has the "oracle property": it consistently identifies variables with true nonzero coefficients.

We propose an algorithm that applies the adaptive lasso to a dataset several times and chooses the best resulting model that obeys effect heredity. Its $\ell_1$ penalty will force certain regression coefficients to be 0, implying that these factors are not significant. The factors that the adaptive lasso regression assigns nonzero values to are deemed "significant effects." Once we have applied the adaptive lasso $m$ times, we remove solutions that do not obey effect heredity, ensure the resulting solutions obey effect heredity, and rank them by some model selection criteria (in our case, BIC). The solution that ranks best according to the criteria is the adaptive lasso algorithm solution. The proposed algorithm is as follows:

1. Divide the observations into $r$ folds. For each of the $r$ folds,
   (a) Use the other $r-1$ folds as a training set. Fold $r$ will be the testing set.
   (b) In the training set, find the lasso solutions that correspond to a grid of potential penalty terms.
   (c) Record the difference between the predicted and actual values of the response in the testing set for each lasso solution.
2. From the errors found in Step 1, compute the mean squared error of the lasso prediction across all $r$ test sets for each value of the penalty term. The penalty value $\lambda^*$ that minimizes this MSE is the lasso penalty.
3. Compute the lasso solution $\hat{\beta}_1,\ldots,\hat{\beta}_p$ given $\lambda^*$. Assign the inverse of the absolute value of the coefficients, $1/|\hat{\beta}_1|,\ldots,1/|\hat{\beta}_p|$, to be the weights for the adaptive lasso penalty $\hat{w}_1,\ldots,\hat{w}_p$, respectively.
4. Remove the effects whose lasso coefficients are 0 from consideration. Apply Steps 1 and 2 again using the weights found in Step 3 to minimize equation (2). The estimated coefficients are the adaptive lasso solution.
5. Repeat Steps 1 through 4 above, $m$ times, recording the nonzero effects from each application of the adaptive lasso.
6. For each model $k < m$
   (a) If there is at least one main effect in model $k$, remove every interaction term that does not obey effect heredity within model $k$.
   (b) If there are no main effects in model $k$, examine the set of models that contain the interaction effects of model $k$ and combinations of main effects such that every interaction effect obeys effect heredity within the model. For example, if model $k$ consists of the interaction effect of $A$ and $B$, the models entertained contain as factors $(A, AB)$, $(B, AB)$, and $(A, B, AB)$. Apply some model selection criterion to this set of models and select the best model.

7. The significant effects of the solution $k^* \in 1, \ldots, m$ that ranks best according to the criteria are the variables the algorithm identifies as belonging to the true model.

To implement the adaptive lasso, we use the R package `parcor` developed by Kraemer and Schaefer (2015) to estimate partial correlation matrices for gene association networks (Kraemer, Schaefer, and Boulesteix 2009). This method allows the adaptive lasso to perform variable selection even when the number of effects of interest exceeds the number of observations, as is often the case when studying interaction effects in designs with complex aliasing.

We apply this algorithm with $m = 5, 10, 20$, and 100. In our experience, the results of our simulations in Section 5 do not vary much, and we chose to report the results for $m = 5$. Finally, we set $r = 10$ for all designs, although this can be lowered for designs with fewer than 10 observations. Note that the algorithm can accommodate $p$ which is larger than $n$. The `R` codes for applying this method can be obtained from *http://faculty.franklin.uga.edu/amandal*.

For illustration, consider the motivating cast fatigue example. Hunter, Hodi, and Eager (1982) originally found that effects $F$ and $D$ were the most significant among all effects. However, Wu and Hamada (2009) concluded that $F$ and $FG$ were significant. The difference between these two conclusions is a result of the complex aliasing of the design. In Equation (1), if we ignore all second order interactions except $FG$, we find that

$$E(\hat{D}) = D + \frac{1}{3}FG.$$

Therefore, the estimate for main effect $D$ is biased by the $FG$ interaction: $D$ is partially aliased with $FG$. Since the design makes the main effects partially aliased with interaction effects, this ambiguity cannot be resolved. Wu and Hamada (2009) argued that a model with $F$ and $FG$ is better, with a significantly higher $R^2$. After applying the adaptive lasso algorithm on the data, we found that $F$ and $FG$ were significant in our results as well.

Now we analyze the data for the motivating nozzle experiment mentioned in the introduction. We apply each method to this example. Method 1 identifies the model with $G$ and $BJ$ as significant factors. This model, although sparse, is hard to interpret. On the other hand, the proposed adaptive lasso method identifies $B$ and $BJ$ as significant effects. This model obeys weak heredity principle and hence is more interpretable, even though it has a slightly lower $R^2$ value (0.46 as opposed to 0.56). Method 2 identifies the same model with $h = 2$, whereas a bigger value of $h$ leads to more complicated models.

## 5. Simulations

To compare the effectiveness of the adaptive lasso algorithm to the traditional methods more generally, we used five different designs to simulate data for various models with different magnitudes of factorial effects and different model variances, and applied each method to each simulation setup. For each design, we simulated data from several models with normally distributed errors whose SDs $\sigma$ equal 0.1, 0.25, 0.5, 0.75, or 1.0. Continuous, quadratic, and linear factors are considered. Coefficients were randomly generated from two uniform distributions: $U(0.5, 1.5)$ for "big" effects and $U(0.1, 0.3)$ for "small" effects.[3] We assume that interaction and quadratic effects[4] were big only if their parent main effects were big as well.

For each combination of design, model, effect sizes, and SD of the error term, we simulated 100 datasets. In each simulation, once we had generated the data, we applied the adaptive lasso algorithm, Method 1, and Method 2 with $h = 2$, 3, and 4, as well as the Dantzig selector (Candes and Tao 2007; Phoa, Pan, and Xu 2009), the LARS method used by Yuan, Joseph, and Lin (2007), and the nonnegative garotte method proposed by Yuan, Joseph, and Zou (2009). A brief discussion of these methods, along with a complete list of data generating processes, is provided in the supplementary materials.

The five different designs considered for simulation are as follows:

1. *Cast fatigue experiment:* The first simulation corresponds to the motivating cast fatigue experiment discussed in Hunter, Hodi, and Eager (1982). The design used was a 12-run Plackett–Burman design with seven factors.
2. *Contaminant experiment:* The second simulation corresponds to the contaminant experiment discussed in Miller and Sitter (2001). This industrial experiment was conducted to investigate ways of reducing the toxic contaminant from the waste stream of a chemical process. This is a 24-run design with nine two-level factors given by columns $A$ to $I$ of Table 1 in their paper.
3. *Antiviral drug experiment:* Xu, Jaynes, and Ding (2014) discussed an antiviral drug experiment where a 34-run design was used, with five factors each at three levels. In this simulation, we considered the design given by columns $A$ to $E$ of Table 1 in their paper.
4. *Wood pulp experiment:* Chipman, Hamada, and Wu (1997) described a study involving some hard-to-control factors. We took the design for this wood pulp production process, given by the columns $A$ to $K$ of Table 4 in their paper. This is a 19-run design with 11 factors. Three of these factors, denoted by $E$, $I$, and $K$, were continuous, and we simulated data involving continuous factors as well.
5. *Ceramics experiment:* Finally, we considered an 18-run design used to study Silicon nitrate ceramic discussed in the unpublished Ph.D. dissertation of Yuan (1998). This mixed-level design has one factor ($A$) at two levels and six factors (denoted by $B$ to $G$) at three levels.

Figure 1 presents the percentage of simulations in which each algorithm correctly identified a model, separated by design and model size, and aggregated over smaller values of $\sigma$, 0.10 and 0.25. The results for the proposed adaptive lasso algorithm are in a shade of red behind the other methods. In Figure 2, the bars show the average number of effects identified by different methods. Here also the adaptive lasso algorithm corresponds to

[3] These specifications are similar to the motivating cast fatigue example, where the data can be thought of as being generated from the model $Y = A + AB + \epsilon$ with $A$ and $AB$ having coefficients with absolute value close to 0.5 and model SD close to 0.25.

[4] Following Wu and Hamada (2009, p. 287), we define the quadratic effect of a three-level factor as being proportional to $(y_2 - y_1) - (y_1 - y_0)$ where $y_0$, $y_1$, and $y_2$ represent the observations at levels 0, 1, and 2, respectively.

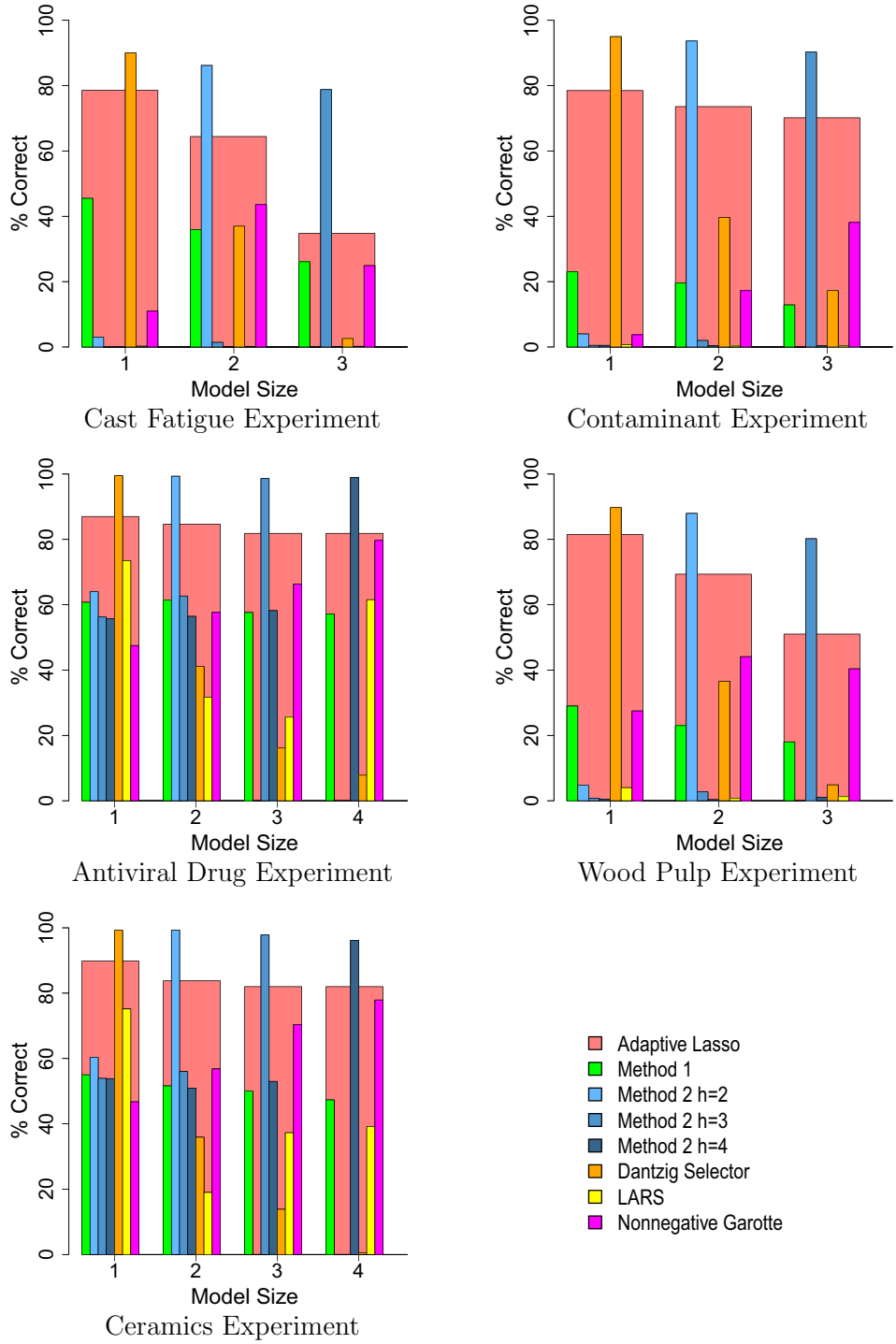


**Figure 1.** Correctly identified models for different experiments.

a shade of red behind the other methods. Note that the adaptive lasso algorithm is more conservative than the traditional methods, but less conservative than the Dantzig selector. This balance may suggest that the adaptive lasso algorithm is more likely to choose a model of the correct size.

The results from these simulations show that the adaptive lasso algorithm provides a versatile and effective variable selection procedure compared to the other methods. The adaptive lasso algorithm outperforms the others except in the case that the true model contains exactly one effect (in which case the Dantzig selector is more accurate) or when the maximum number of effects for Method 2, $h$, is set to the true number of significant effects. Phoa, Pan, and Xu (2009) used a conservative modification of the AIC to select the final model for the Dantzig selector; this criterion makes it more likely the Dantzig selector will choose a small model, which makes it more appropriate for sparse models than complex ones. While Method 2 performs well when $h$ is set appropriately, the effective implementation of Method 2 may be difficult in practice because $h$ is rarely known in advance. In contrast, the adaptive lasso

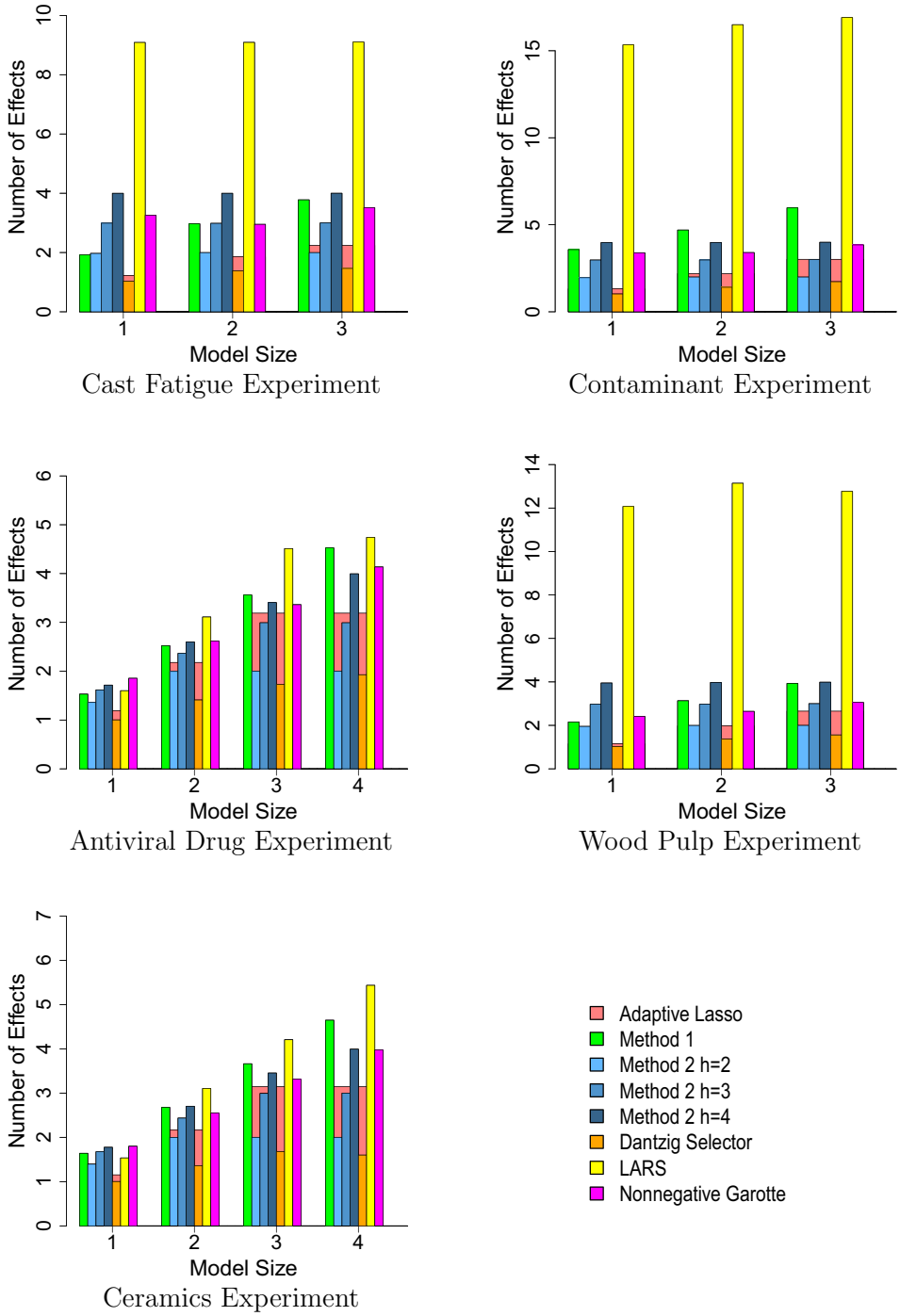


**Figure 2.** Number of significant effects identified for different experiments.

algorithm performs relatively well for models of all sizes, and does not require knowledge of the size of the model as Method 2 does.

## 6. Conclusions

In this article, we proposed using an adaptive lasso algorithm for analyzing data from designs with complex aliasing—this algorithm is easy to use and does not require much user intervention or prior knowledge of the nature of the true model. For a variety of models, and especially for cases with errors with low SDs, the algorithm provides an effective model identification method that outperforms traditional methods dealing with complex aliasing in several different kinds of design. As demonstrated by our simulations, the algorithm identifies the exact model more frequently than either of the traditional frequentist methods. Furthermore, it outperforms more recently developed algorithms in identifying larger, more complicated models. While other analysis methods have been proposed such as Chipman et al.'s (1997) Bayesian variable selection strategy, those methods are computationally intensive and have not been considered here. We propose the usage of the algorithm because of its effectiveness and ease of implementation. The R codes

are available from the author's website, and do not require any specialized knowledge or subjective judgment. Our results indicate that the adaptive lasso algorithm can be used to reliably identify models in the presence of complex aliasing. However, one should keep in mind that the final model obtained by any data analysis technique should always be assessed using residual diagnostics and other tools, before drawing inference on the significance of factorial effects.

## Supplementary Materials

Introductions to the Dantzig selector, the LARS algorithm, and the Non-negative Garotte, as well as additional details of the simulation results along with a complete list of data generating processes, are provided in the supplementary materials. Figures 1 and 2 are taken from these results.

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## Appendix

**Table A.1.** Cast fatigue design and data.

| *A* | *B* | *C* | *D* | *E* | *F* | *G* | Response |
|---|---|---|---|---|---|---|---|
| +1 | +1 | −1 | +1 | +1 | +1 | −1 | 6.058 |
| +1 | −1 | +1 | +1 | +1 | −1 | −1 | 4.733 |
| −1 | +1 | +1 | +1 | −1 | −1 | −1 | 4.625 |
| +1 | +1 | +1 | −1 | −1 | −1 | +1 | 5.899 |
| +1 | +1 | −1 | −1 | −1 | +1 | −1 | 7.000 |
| +1 | −1 | −1 | −1 | +1 | −1 | +1 | 5.752 |
| −1 | −1 | −1 | +1 | −1 | +1 | +1 | 5.682 |
| −1 | −1 | +1 | −1 | +1 | +1 | −1 | 6.607 |
| −1 | +1 | −1 | +1 | +1 | −1 | +1 | 5.818 |
| +1 | −1 | +1 | +1 | −1 | +1 | +1 | 5.917 |
| −1 | +1 | +1 | −1 | +1 | +1 | +1 | 5.863 |
| −1 | −1 | −1 | −1 | −1 | −1 | −1 | 4.809 |

**Table A.2.** $X_2$ matrix for cast fatigue design.

| *AB* | *AC* | *AD* | *AE* | *AF* | *AG* | *BC* | ... |
|---|---|---|---|---|---|---|---|
| +1 | −1 | +1 | +1 | +1 | −1 | −1 | ... |
| −1 | +1 | +1 | +1 | −1 | −1 | −1 | ... |
| −1 | −1 | −1 | +1 | +1 | +1 | +1 | ... |
| +1 | +1 | −1 | −1 | −1 | +1 | +1 | ... |
| +1 | −1 | −1 | −1 | +1 | −1 | −1 | ... |
| −1 | −1 | −1 | +1 | −1 | +1 | +1 | ... |
| +1 | +1 | −1 | +1 | −1 | −1 | +1 | ... |
| +1 | −1 | +1 | −1 | −1 | +1 | −1 | ... |
| −1 | +1 | −1 | −1 | +1 | −1 | −1 | ... |
| −1 | +1 | +1 | −1 | +1 | +1 | −1 | ... |
| −1 | −1 | +1 | −1 | −1 | −1 | +1 | ... |
| +1 | +1 | +1 | +1 | +1 | +1 | +1 | ... |

**Table A.3.** Alias matrix *L* for the cast fatigue experiment.

| | *AB* | *AC* | *AD* | *AE* | *AF* | *AG* | *BC* | *BD* | *BE* | *BF* | *BG* | *CD* | *CE* | *CF* | *CG* | *DE* | *DF* | *DG* | *EF* | *EG* | *FG* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| *A* | 0 | 0 | 0 | 0 | 0 | 0 | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | $\frac{1}{3}$ | $\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ |
| *B* | 0 | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | $-\frac{1}{3}$ | 0 | 0 | 0 | 0 | 0 | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | $\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | $\frac{1}{3}$ | $-\frac{1}{3}$ |
| *C* | $-\frac{1}{3}$ | 0 | $\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | 0 | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | 0 | 0 | 0 | 0 | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ |
| *D* | $-\frac{1}{3}$ | $\frac{1}{3}$ | 0 | $\frac{1}{3}$ | $\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | 0 | $\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | 0 | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | 0 | 0 | 0 | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ |
| *E* | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | 0 | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | 0 | $\frac{1}{3}$ | $\frac{1}{3}$ | $-\frac{1}{3}$ | 0 | $\frac{1}{3}$ | $-\frac{1}{3}$ | 0 | $-\frac{1}{3}$ | $-\frac{1}{3}$ | 0 | 0 | $-\frac{1}{3}$ |
| *F* | $\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | $-\frac{1}{3}$ | 0 | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | 0 | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | 0 | $\frac{1}{3}$ | $-\frac{1}{3}$ | 0 | $\frac{1}{3}$ | 0 | $-\frac{1}{3}$ | 0 |
| *G* | $-\frac{1}{3}$ | $\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $-\frac{1}{3}$ | 0 | $\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | $-\frac{1}{3}$ | 0 | $-\frac{1}{3}$ | $-\frac{1}{3}$ | $\frac{1}{3}$ | 0 | $-\frac{1}{3}$ | $\frac{1}{3}$ | 0 | $-\frac{1}{3}$ | 0 | 0 |

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