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The Design of Stated Choice Experiments: The State of Practice and Future Challenges

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ABSTRACT: Since the work of Louviere and Woodworth (1983) and Louviere and Hensher (1983), stated choice (SC) methods have become the dominant data paradigm in the study of behavioural responses of individuals and households as well as other organizations, in fields as diverse as marketing, transport and environmental and health economics, to name but a few. In SC experiments, it is usual for sampled respondents to be asked to choose from amongst a number of labelled or unlabelled alternatives defined on a number of attribute dimensions, each in turn described by pre-specified levels drawn from some underlying experimental design. The choice task is then repeated a number of times, up to the total number of choice sets being offered over the experiment. Several experimental design strategies are available to the practitioner, however, within the transport literature, it appears that the most common form of experimental design used are orthogonal fractional factorial designs. In this paper we review the properties of such designs, and demonstrate that these properties are unlikely to be retained through to the estimation process. We also discuss an alternative design construction strategy, used to construct statistically optimal designs.

KEY WORDS: ***Stated Choice, Orthogonal Fractional Factorial Designs Optimal Designs***.

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1. Introduction

Given the ability to 'imitate' real world market decisions (Carson et al., 1994), stated choice (SC) methods have become a popular means of eliciting the behavioural responses of individuals, households and organizations over various choice contexts. In constructing SC experiments, the analyst must attempt to meet a number of (often conflicting) statistical criteria and balance these with (often conflicting) issues related to respondents' ability to complete the choice tasks that they are presented with in a meaningful manner. Recently, researchers have suggested that from a statistical perspective, experimental designs underlying SC tasks should impart the maximum amount of information about the parameters of the attributes relevant to each specific choice task (Zsolt and Wedel, 2001), whilst at the same time, minimising resultant loss of orthogonality in the designs used. Generation of statistically efficient designs has been addressed by several authors (Bunch et al., 1994; Huber and Zwerina, 1996, Kanninen, 2002; Kuhfeld et al., 1994, Lazari and Anderson, 1994; Sandor and Wedel, 2002; Zsolt and Wedel, 2001), each of whom offers differing construction strategies to generate such designs. Often, the premise behind the construction of statistically efficient designs is given as the need to reduce the number of choice sets shown to any one individual respondent, so as to reduce the cognitive effort and possible fatigue effects that each respondent may experience over the entire experiment. This represents a clear trade-off for choice modellers, given that larger designs potentially offer more information that may be used to estimate the parameters underlying the preferences within the sampled population.

Assuming that an aim of SC studies is the reduction in the cognitive effort placed on individual respondents through a reduction in the number of choices required to be made by each, or alternatively, the minimization of the sample size required to produce asymptotically efficient and reliable parameter estimates, the generation of more statistically efficient designs make sense. However, whilst the construction of statistically efficient designs is relatively straightforward for studies employing linear models (Atkinson and Donev, 1992; Lenk et al., 1996; Pliz, 1991), the use of non-linear models in SC studies, in particular the multinomial logit (MNL), the nested logit (NL), or the mixed logit (ML) model (to name but a few), complicates the construction process, perhaps to such an extent that the analyst must calculate the opportunity costs in using less statistically efficient designs to elicit information on choice behaviour.[1] The complication in constructing efficient SC experiments for use in non-linear models is the requirement, a priori, of knowledge of the population parameters for each of the design attributes due to the fact that the information obtained from the design is dependent on these parameter values. Given that it is improbable that the analyst will posses such knowledge, the efficiency of a design is unlikely to be 100% once data has been collected and the 'true' parameters estimated. This probable lack of advance knowledge of the true parameter estimates has resulted in the common assumption that the parameters associated with the design attributes are simultaneously equal to zero, forcing the design to be optimal only under the null hypothesis of sampled respondents having non-significant marginal utilities for all design attributes.

[1] Recent research by Hensher (2004) has raised important questions about the meaning of 'choice complexity'. Hensher argues that relevancy within a context of a particular information processing strategy is what matters and that analysts who criticise designs with more items are failing to understand the real behavioural issues.

The assumption of insignificant parameter estimates for all design attributes is, in all likelihood, unjustified for the majority of SC studies and is likely to result in the use of inefficient experimental designs. It is therefore desirable to optimise the efficiency of the experimental design for non zero values of the parameter estimates. Huber and Zwerina (1996) attempted to do so using a method of utility balancing whereby the attribute levels produced within the design are such that the differences in the utilities across alternatives for each treatment combination approaches zero, thus ensuring that the choice probabilities will be roughly equal across alternatives present within a choice set. The approach offered by Huber and Zwerina (1996) is limited in that it requires the use of pretested studies which also rely upon the construction of a SC design requiring knowledge of the true population parameter values. Sandor and Wedel (2001) attempted to overcome this deficiency by accommodating uncertainty in the true parameter values through a prior distribution over a range of plausible values. Whilst offering important advances, constraints imposed within the strategies offered by both Huber and Zwerina (1996) and Sandor and Wedel (2001) mean that such construction strategies cannot guarantee design optimality. Kanninen (2002) derives optimal designs for SC studies using algebraic manipulation and numerical optimization that maximises the determinant of the Fisher information matrix, O. Nevertheless, whilst the approach suggested by Kanninen (2002) will produce optimally statistically efficient designs, the method requires that one attribute be used to balance the response probabilities at their optimal levels. The process thus requires that the design be updated over the length of the experiment as the optimal attribute level of the attribute used to balance the response probabilities becomes known. As such, the approach is operationally limited to surveys conducted using either computer aided program interviews (CAPI) or the internet.

Aside from the complexity in the generation of optimal designs, it must be recognised that the design strategies used are limited to generic or unlabelled designs only. For many transport studies, this may present a problem. For example, mode choice studies generally require labelled choice experiments. However, for many route choice studies, the use of optimally efficient labelled experiments may prove beneficial. Nevertheless, it appears that transportation researchers have failed to develop optimal designs, rather relying on the generation and use of orthogonal fractional factorial designs.

Orthogonal fractional factorial designs are generated so that the attributes of the design are statistically independent (i.e., uncorrelated). Orthogonality between the design attributes represents the foremost criteria in the generation process; the statistical efficiency of the design is rarely considered. Thus, whilst optimal designs optimise the amount of information obtained from a design, the construction process for orthogonal fractional factorial designs minimize to zero the correlations evidenced within a design. Optimal designs will be statistically efficient but will likely have correlations, orthogonal fractional factorial designs will have no correlations but may not be the most statistically efficient design available. Hence, the type of design generated reflects the belief of analysts as to what is the most important property of the constructed design. The purpose of this paper is to offer a review of the state of practice in the construction of SC experiments. In this paper, we review and compare the two competing construction paradigms, and demonstrate that both methods have merits as well as problems.

This paper is organized as follows. In the next section we describe the statistical considerations in the design of orthogonal SC experiments. First we introduce the concept of orthogonal fractional factorial designs and the construction considerations necessary for such designs. In the following section we detail why orthogonality is unlikely to be carried through to data collected and used for model estimation. We next show this through use of an empirical example. The following section of the paper outlines the theoretical development of optimal SC designs, which is followed by an explanation of why such design strategies are limited to the generation of generic or unlabelled choice experiments. The final sections of the paper outline and test, using Monte Carlo methods, a proposed method to generate labelled optimal designs.

2. Orthogonal Fractional Factorial Experimental Designs

A review of the literature using SC methods published in top tier transportation journals[2] (see Bliemer and Rose, 2004) reveals that the majority of these studies generated orthogonal fractional factorial designs as opposed to using designs generated using optimal design techniques. The two approaches differ in that fractional factorial designs are used to produce design matrices that are orthogonal in the columns whereas optimal design techniques generate designs which are not necessarily orthogonal but which capture the maximum amount of information by minimizing the asymptotic joint confidence sphere surrounding the parameter estimates (Kanninen, 2002). Independent of the type of design, experimental designs underlying SC studies require that respondents be shown one or more choice sets (i.e., treatment combinations in the design) consisting of alternatives, each defined by a number of attributes which take discrete values called attribute levels. In order for the experiment to proceed, the attribute levels are assigned labels which provide cognitive meaning to the respondent. These attribute level labels may take any value within a range for quantitative attributes, or describe any characteristic for a qualitative attribute. The underlying experimental design varies the attribute levels shown within and across each choice set.

The total number of possible choice sets (or treatment combinations) for a given design is dependent on the number of alternatives, attributes and attribute levels required for the study, and the effects that are to be estimated as well as whether the design is labelled or unlabelled. Let M represent the number of alternatives, A the number of attributes and L the number of attribute levels. The full enumeration of possible choice sets is equal to L^{MA} for labelled choice experiments and L^{A} for unlabelled experiments (see Louviere et al., 2004). As the total number of possible treatment combinations increase exponentially given increases in the design dimensions, it is usual to utilise only a subset or fraction of the total possible treatment combinations available. Such designs are known as fractional factorial designs. Assuming the estimation of main effects only (ignoring interactions between attributes), the minimum numbers of choice sets required for model estimation are given in Table 1. Linear effects assume the estimation of a single parameter associated with a quantitative attribute. Non-linear effects represent dummy or effects coded qualitative or quantitative design attributes which require the estimation of multiple parameters.

[2] Tier one journals are considered to include (in alphabetical order): Journal of Transport Economics and Policy, Transportation, Transportation Research A: Policy & Practice, Transportation Research B: Methodological, Transportation Research Record (Journal of the Transportation Research Board) and Transportation Science (see Transportation & Logistics Journal Rankings, 2004).

Table 1: Minimum choice sets requirements for main effects only fractional factorial designs[3]

Experiment Effects	Unlabelled	Labelled
Linear	A +1	MA +1
Non-linear	(L-1)×A +1	(L-1)×MA +1

For large numbers of alternatives, attributes and attribute levels, the total number of treatment combinations generated for a main effects only fractional factorial design may still be considered to large for any single respondent to complete. It is therefore usual to generate additional orthogonal design columns, known as blocking columns, which are used to assign respondents to subsets of the fractional factorial design[4] (see Hensher et al., 2004, Ch5).

Independent of the minimum number of choice sets required of a design, the generation of orthogonal designs require a number of statistical and mathematical considerations. Mathematically, a matrix is orthogonal when the sum of the products between any two design columns equal zero. This property holds only when orthogonal codes are used (see Hensher et al., 2004) and the attribute levels balanced (i.e., within each attribute column, every attribute level appears an equal number of times). To demonstrate, consider a simple experiment involving three design attributes, each described by two levels. The full factorial design is a 2^3 design with a total of eight treatment combinations. Assuming main effects only, this may be reduced to a half factorial, reducing the number of treatment combinations to four (i.e., a $2^{(3-1)}$ design requiring a minimum of (2-1)×3+1 treatment combinations). It is possible to generate several designs that meet the above criteria. Table 1 shows one such design.

Table 1: $2^{(3-1)}$ orthogonal experimental design

Treatment combination	*A*	*B*	C	AB	AC	BC
1	-1	-1	1	1	-1	-1
2	1	-1	-1	-1	-1	1
3	-1	1	-1	-1	1	-1
4	1	1	1	1	1	1
	Column sums			0	0	0

The last three columns of Table 1 show the products for each row combination of the design columns. The resulting columns represent interaction columns (e.g., the AB

[3] The formulas shown are used to calculate the minimum degrees of freedom necessary for estimating the desired number of parameters. The numbers derived may however, not represent the true minimum number of treatment combinations necessary to achieve an orthogonal design due to the necessity to maintain attribute level balance within each attribute. For example, let M = 2, A = 3 and L = 2. The minimum number of treatment combinations assuming the estimation of non-linear effects in the marginal utilities in a labelled choice experiment is equal to (2-1)×2×3+1 or seven. However, such a design will not be balanced as each attribute has two levels which must appear an equal number of times over seven choice sets. This represents an additional constraint, such that the smallest possible design will have a number of treatment combinations equal to or greater than that calculated using the relevant formula shown in Table 1, but also be a number that produces an integer when divided by all L.
[4] If designed correctly, the addition of a blocking column will not impact upon the minimum number of treatment combinations of the design.

column represents the interaction between the A and B main effects columns). The sums of each interaction column sum to zero. The correlation matrix for the design is shown in Table 2 which shows zero pairwise correlations between each of the design attributes.

Table 2: Correlation matrix for the $2^{(3-1)}$ orthogonal experimental design

	A	*B*	*C*
A	1		
B	0	1	
C	0	0	1

To further demonstrate the property, consider the design shown in Table 3. The design is balanced in that each attribute level is shown an equal number of times, however, the sum of the AC interaction column is now equal to minus four. Table 4 shows the correlation matrix for this design which clearly demonstrates the existence of correlations (in this case, a perfect positive correlation between attribute A and C).

Table 3: $2^{(3-1)}$ non-orthogonal experimental design

Treatment combination	*A*	B	C	AB	AC	BC
1	-1	-1	1	-1	1	-1
2	1	-1	-1	1	1	1
3	1	1	-1	-1	1	-1
4	-1	1	1	1	1	1
	Column sums			0	-4	0

Table 4: Correlation matrix for the $2^{(3-1)}$ non-orthogonal experimental design

	A	*B*	*C*
A	1		
B	0	1	
C	1	0	1

If interactions are required, additional treatment combinations may be added and the attributes allocated to design columns such that the specified interaction is not correlated with any other main effect or possibly other interaction term (see Hensher et al., 2004).

2.1 Does orthogonality always translate through to Data sets?

One of the arguments for the use of orthogonal fractional factorial designs is the ability of such designs to produce unconfounded estimates of the population parameters due to the enforced statistical independence between the attributes contained within the design. However, parameters are estimated from data sets underlined by SC experiments, not from the design itself. As we demonstrate, only under exceptional circumstances will orthogonality be preserved within the data used to estimate discrete choice models, even if the experimental design is orthogonal. Indeed, with regards to choice data sets, one would expect orthogonality to be the exception rather than the rule. Further, even under circumstances where orthogonality is retained in a data set, as we show, orthogonality will likely be lost in the estimation process.

Rarely will a data set consist of only a single replication of a choice design. Rather, it will more often be the case that a data set will consist of multiple replications of a single SC design. Respondents participating in a choice study will each view either an entire replication of the design or subsets of the design. In the case of the use of subsets, the choice sets viewed by any given individual will be determined either by the use of a blocking variable or via random assignment. Independent of whether respondents complete an entire replication of a design or a subset of a design replication, the data set used for model estimation requires the stacking of data associated with all respondents. As such, the data set will be in the form of a matrix, with number of columns equal to the number of attributes and other variables collected, and, assuming the data is set up as single line data (see Greene, 2002), the number of rows equal to the number of choice sets completed over all respondents.

Using the fractional factorial design shown in Table 1 and assuming that respondents are assigned to a subset of the design such that a respondent will be asked to complete only two of the four possible treatment combinations, Table 5 represents one possible scenario whereby four respondents complete two whole replications of the design. In the scenario represented in Table 5, each treatment combination is observed exactly by two respondents; hence the data set will consist of two complete designs. For each attribute pair, the sum of the products will be exactly equal to zero and hence, orthogonality will be preserved within the data set.

Table 5: Two complete replications of a $2^{(3-1)}$ orthogonal experimental design over four respondents

Treatment combination	Respondent	A	B	C	AB	AC	BC
1	1	-1	-1	1	1	-1	-1
2	1	1	-1	-1	-1	-1	1
3	2	-1	1	-1	-1	1	-1
4	2	1	1	1	1	1	1
1	3	-1	-1	1	1	-1	-1
2	3	1	-1	-1	-1	-1	1
3	4	-1	1	-1	-1	1	-1
4	4	1	1	1	1	1	1
		Column sums			0	0	0

Consider, however, the issue of non-response. Assuming, for example, that respondent four failed to complete the choice questionnaire, the data set would look as shown in Table 6. The failure to respond by a sampled individual represents a loss of rows in one of the design replications within the data set. In the example shown, treatment combinations one and two are present twice in the data set but treatment combinations three and four only once. Examination of the sum of the AC interaction column reveals a non-zero amount (i.e., minus two). The failure of one respondent to complete the SC task has resulted in the loss of orthogonality within the data set. This is confirmed by the correlation matrix shown as Table 7.

Table 6: Non response using a $2^{(3-1)}$ orthogonal experimental design over four respondents

Treatment combination	Respondent	A	B	C	AB	AC	BC
1	1	-1	-1	1	1	-1	-1
2	1	1	-1	-1	-1	-1	1
3	2	-1	1	-1	-1	1	-1
4	2	1	1	1	1	1	1
1	3	-1	-1	1	1	-1	-1
2	3	1	-1	-1	-1	-1	1
		Column sums			0	-2	0

Table 7: Correlation matrix for the $2^{(3-1)}$ orthogonal design data set with missing treatment combinations

	A	B	C
A	1		
B	0	1	
C	-0.33333	0	1

As demonstrated above, unless equal representation of treatment combinations is captured within the data, orthogonality will not be preserved. Hence, non-response, the random assignment of treatment combinations of choice sets to respondents (which does not ensure equal replications of each choice set within the data; particularly in small samples), and the removal of observations by the analyst (a not uncommon practice; see for example Brownstone et al., 2000), will all likely result in the loss of orthogonality within a data set. Further, it is common practice to collect socio-demographic and contextual variables and include these in the utility functions of models of discrete choice. Even assuming equal representation of each treatment combination of a design, the current standard of sampling is such that analysts fail to ensure orthogonality between the design attributes and other variables within the data set. For example, consider the addition of a gender variable to the data shown in Table 5. Table 8 shows two possible samples, both of which consist of exactly two males and two females. Assuming females are coded one and males minus one, both female respondents are given treatment combinations one and two and both male respondents treatment combinations three and four. Examination of the correlation matrix under this sample show a perfect negative correlation between gender and design attribute B. In this case, either gender or design attribute B could be included within the estimated model, not both. Within sample two, one female is shown treatment combinations one and two, and the other treatment combinations three and four. A similar pattern of treatment combination assignment is observed across both males. Examination of the correlation matrix (see Table 9) under this second sample shows no correlations within the sample.

Table 8: Two different samples with equal numbers of gender representativeness

Treatment combination	Respondent	A	B	C	Gender1	Gender2
1	1	-1	-1	1	1	1
2	1	1	-1	-1	1	1
3	2	-1	1	-1	-1	-1
4	2	1	1	1	-1	-1
1	3	-1	-1	1	1	-1
2	3	1	-1	-1	1	-1
3	4	-1	1	-1	-1	1
4	4	1	1	1	-1	1

Table 9: Correlations for two different samples with equal numbers of gender representativeness

	Sample 1			
	A	*B*	*C*	*Gender1*
A	1			
B	0	1		
C	0	0	1	
Gender1	0	-1	0	1

	Sample 2			
	A	*B*	*C*	*Gender2*
A	1			
B	0	1		
C	0	0	1	
Gender2	0	0	0	1

Whilst sample one represents an extreme case, it does highlight the need to perhaps reconsider the sampling strategies used for SC studies, assuming retention of orthogonality remains a key criteria of such studies. Assuming this to be the case, then perhaps what is required is a form of quota sampling in which the quotas consist of various socio-demographic and contextual profiles which are in turn used to assign equal replications of the experimental design within and across each profile generated. Whilst producing unrepresentative samples, exogenous weighting during model estimation may be used to reconstruct representativeness and allow the study findings to be generalized from the sample to the population of interest. A problem of logistics remains somewhat more difficult to solve, however, as the analyst must still locate respondents that match each quota profile and assign them to the appropriate design block. For surveys conducted using paper and pencil or CAPI, this may prove a difficult task[5]. The increasing use of the internet as a data collection medium, however, may allow in the future for the smart assignment of choice sets to respondents as opposed to the random assignment of either choice sets or blocks of choice sets. In such cases, the survey will be able to detect missing rows of the design and assign these accordingly; that is, in a manner that reduces any loss of orthogonality not only between the design attributes, but also within and between the other variables collected and the design attributes.

Yet, even assuming that with the addition of covariates, a data set remains orthogonal, orthogonality is unlikely to be preserved through to model estimation. To demonstrate why, consider a situation in which an individual evaluates a finite number of alternatives. Let subscript n, j and s refer to individual n = 1, 2, …, N, alternative j = 1,

[5] The use of multiple laptops in CAPI questionnaires makes it difficult to maintain quotas over the entire data set. This may be offset if frequent downloading of the data occurs or interviewers are given strict criteria on which to sample, however this becomes logistically more difficult with greater numbers of interviewers (and hence laptops) or greater geographically dispersed sampling.

2, …, J and choice set s = 1,2, ..., S. Random utility theory (RUT) posits that the utility possessed by an individual, n, for alternative j present in choice set s may be expressed as:

$$U_{njs} = b_j x'_{njs} + e_{njs} \tag{1}$$

where
U_{njs} is the utility held by individual n for alternative j present in choice set s, $ß_j$ is a k-vector parameter weighting associated with the attributes listed in x'_{njs}, a k-vector, and e_{njs} is a stochastic error term which is independently and identically (IID) extreme value type I distributed.

Assuming all decision makers are utility maximizers, individual n will choose alternative i in choice situation s, when

$$U_{nis} \geq U_{njs} \qquad , \forall j \neq i \tag{2}$$

Under the assumption that e_{njs} is IID extreme value type I distributed. the probability that alternative i will be chosen can be expressed in closed form as:

$$P_{nis} = \frac{e^{b_i x'_{nis}}}{\sum_{j=1}^{J} e^{b_J x'_{njs}}} \tag{3}$$

In the case of a labelled choice experiment estimated with alternative specific parameter estimates, dividing equation (3) by its numerator and noting that $\frac{e^a}{e^b} = e^{(a-b)}$, produces the following result:

$$P_{nis} = \frac{1}{1 + \sum_{j \neq i} e^{-\left(b_i x'_{nis} - b_j x'_{njs}\right)}} \tag{4}$$

Equation (4) is the conditional logit choice or multinomial logit (MNL) model (see Louviere et al., 2000, Ch3). When demonstrated in this form, it becomes clear that in estimating models of discrete choice with alternative specific parameter estimates, what is important is the differences in the utility functions (and hence parameter estimates as well as attributes) of the alternatives within the data, and not the actual values observed for each of the attributes (Hensher and Barnard, 1990). Given that it is the differences in the utilities which are of importance, it is the correlations between these differences that count, not the correlations between the variables included in the utility functions of the model. As such, in the case of a labelled choice experiment in which alternative specific parameter estimates are to be obtained, ensuring orthogonality in the design attributes (and perhaps even covariates) will not preserve orthogonality where it counts.

For generic or unlabelled choice experiments, the parameters for the design attributes are generally estimated such that $b_I = bj = b$. Given the equality in the parameters estimated for each attribute across alternatives, equation (4) collapses to:

$$P_{nis} = \frac{1}{1 + \sum_{j \neq i} e^{-b\left(x'_{nis} - x'_{njs}\right)}} \quad (5)$$

The absence of the parameter estimates in equation (5) does not ensure unlabelled SC designs will retain orthogonality. Indeed, they will not. To demonstrate why, consider Table 10 in which the attribute pair difference is calculated for the orthogonal fractional factorial design shown in Table 1. The correlation matrix for the differences is shown in Table 11.

Table 10: Calculating the difference in attribute levels

Treatment combination	*A*	*B*	C	D=A-B	E=A-C	F=B-C
1	-1	-1	1	0	-2	-2
2	1	-1	-1	2	2	0
3	-1	1	-1	-2	0	2
4	1	1	1	0	0	0
				Column sums		

Table 11: Correlation matrix of the differences

	D	*E*	*F*
D	1		
E	0.5	1	
F	-0.5	0.5	1

Acknowledging that orthogonality is unlikely to be transferred from experimental designs to data sets and even more unlikely to be preserved in model estimation, the question arises as to what impact loss of orthogonality will likely have upon the asymptotic efficiency of the parameter estimates of fitted models. Unfortunately, research is yet to be conducted that adequately answers this question. We therefore offer some insights as to the likely effects, noting however that further research is required in this area. Using the same procedure as described in Bliemer and Rose (2004), a Monte Carlo experiment may be used to test the impact of varying the number of replications of blocks observed in a fixed sample has upon the asymptotic efficiency of the parameter estimates using the MNL model. We report some preliminary work conducted by Rose and Bliemer (2004), conducted on a single experimental design. Given the limited number of experimental designs tested, we caution that the results reported may not be generalized to wider SC experiments.

Consider the fractional factorial design shown in Table 12. The design has two alternatives each with three attributes levels described on four levels. The smallest orthogonal fractional factorial design assuming a labelled experiment estimating non-linear marginal utilities requires 19 (i.e., (4-1)×2×3 +1) degrees of freedom. The smallest balanced design possible generates twenty treatment combinations out of a possible 4096 (i.e., $4^{(2\times6)}$) treatment combinations. An additional two level orthogonal blocking column was also generated, thus producing two blocks of size 10. The generation of an additional blocking column allows for the independent assignment of choice sets over attributes and attribute levels to respondents.

Using the true or known parameter estimates shown in Table 13, a Monte Carlo experiment was conducted on the above design varying the proportions of respondents given blocks one and two using a fixed the sample size of 100 (therefore fixing the number of observations to 1000). Nine runs were conducted, varying the proportions of respondents observing each block. In the first run, 10 respondents (100 observations) were simulated to observe block one and 90 (900 observations) block two. In the second run, 20 respondents (200 observations) were simulated to observe block one and 80 (800 observations) block two. Each subsequent run increased the number of respondents simulated to have completed block one of the design by 10 and decrease the number of respondents simulated to have completed block two also by 10. The Monte Carlo simulation used 1000 replications per run (see Bliemer and Rose, 2004, for more details on the procedure employed).

Table 12: $4^{(2\times3)}$ fractional factorial orthogonal design

Treatment combination		Alternative 1			Alternative 2		
	Block	Attribute 1	Attribute 2	Attribute 3	Attribute 1	Attribute 2	Attribute 3
1	1	-1	1	-1	-1	3	-1
2	1	-3	-1	-3	-3	1	3
3	1	1	3	1	-3	3	-3
4	1	-1	-3	-3	3	3	-3
5	1	1	-3	3	3	-1	3
6	1	1	1	3	1	1	-1
7	1	3	3	-1	1	-1	1
8	1	3	-1	1	-1	-3	-3
9	1	-1	-3	3	-3	-3	1
10	1	-3	3	-3	3	-3	3
11	2	-3	1	-1	-3	-3	-3
12	2	-3	-3	1	1	3	1
13	2	-1	3	1	1	-1	-1
14	2	3	1	1	-1	1	3
15	2	3	-1	-1	-3	3	3
16	2	-1	3	3	3	1	1
17	2	-3	-1	3	-1	-1	-1
18	2	1	1	-3	-1	-1	1
19	2	1	-1	-1	3	1	-3
20	2	3	-3	-3	1	-3	-1

Table 13: True parameters used in the Monte Carlo simulation

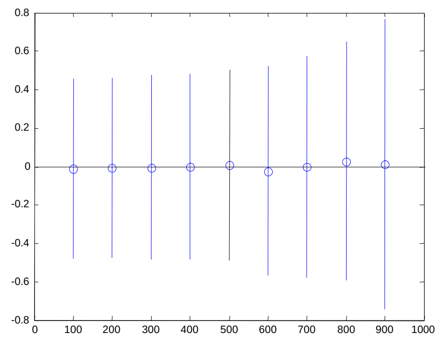
j \ ***k***	**Constant**	**A**	**B**	**C**
1	0	-0.7965	-0.1363	-0.1061
2	0	-0.7237	-0.1382	-0.1076

Figures 1(a) through 1(g) show the results for the Monte Carlo experiment. The results shown are for each of the six attribute parameter estimates and the constant for alternative one (the MNL model is homogenous of degree zero meaning that the constant for alternative two was normalized to zero and hence not estimated). The X-axis of each figure represents the number of observations observed for block one (i.e., each Monte Carlo run). As such, the middle of each figure represents the case where each block is equally replicated across respondents (i.e., each block is assigned to exactly 50 respondents each), and hence, orthogonality is retained within the data set. The Y-axis represents the parameter estimates obtained from the Monte Carlo run with the horizontal line representing the true parameter value used for the experiment. The vertical lines represent the standard errors for each run over the 1000 replications (equation (5)) and the circles the mean parameter estimate over replications (equation (6)).

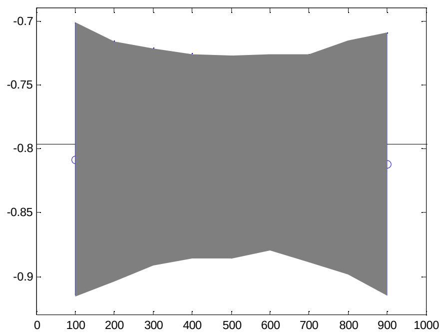
$$se(\hat{b}_{jk}) = \sqrt{\frac{\sum_{r=1}^{R}\left(\hat{b}_{jk}^{(r)} - m(\hat{b}_{jk})\right)^2}{R-2}} \xrightarrow{\text{lim}} 0 \qquad (6)$$

$$m(bjk) = \frac{1}{R}\sum_{r=1}^{R}\hat{b}_{jk}^{(r)} \xrightarrow{\text{lim}} \bar{b}_{jk} \qquad (7)$$

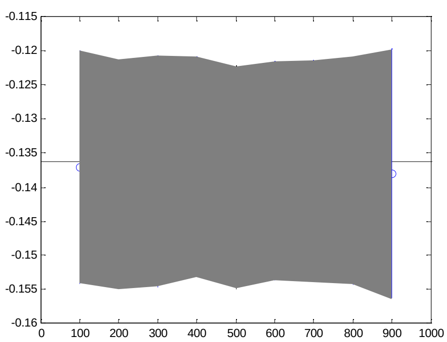
Where r denotes replication r = 1,2, …, 1000, and $\bar{b}$ is the 'true' or known parameters shown in Table 13.



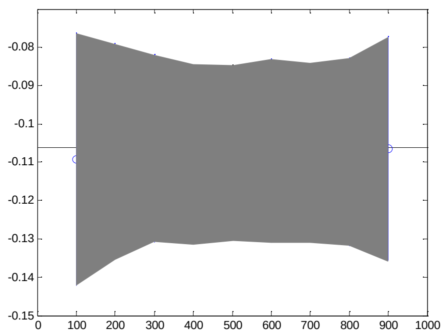
(a) Constant



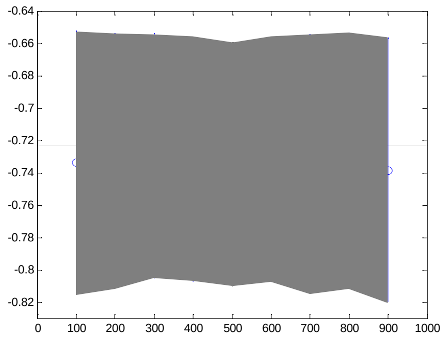
(b) Alt 1 Attribute A



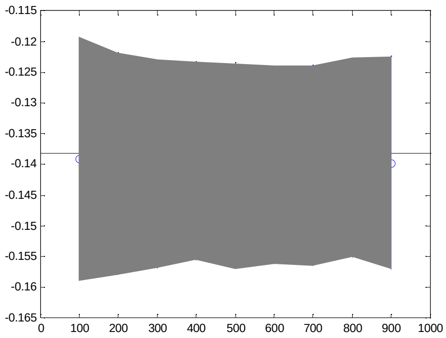
(c) Alt 1 Attribute B



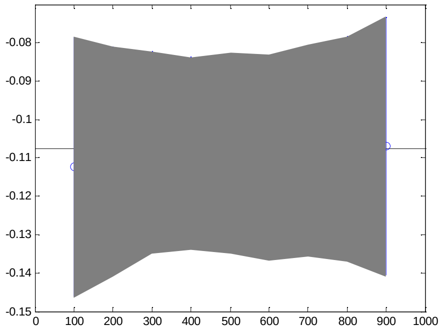
(d) Alt 1 Attribute C



(e) Alt 2 Attribute A



(f) Alt 2 Attribute B



(g) Alt 2 Attribute C

Figure 1: Mean and standard error of parameter estimates over varying proportions of block replications

Figures 1(b) through 1(g) show that in many instances, asymptotic efficiency (defined by both the distance of the mean from the true parameter and the greatest length of the standard error from the true parameter; see Bliemer and Rose, 2004) is improved or unchanged as one moves away from the ideal point of equal replications across blocks. Indeed, for the specific design used, it would appear that for all attributes, having either 40 or 60 respondents out of the 100 sample population complete block one, produces asymptotically efficient parameter estimates which are no worse than for the ideal case. Nevertheless, we note for attributes A and C of alternative one and attribute C of alternative two, that as one moves further from equal block replication in the data, much worse asymptotic efficient parameter estimates are observed. In order to explain why this might be so, we show in Table 14 the average pairwise correlation across replications for each attribute pair. Summing down the absolute values in each of the columns (ignoring the correlation within an attribute) reveals that for attributes with higher total correlations with all other attributes, the further one moves from the ideal point of orthogonality, the greater the impact upon the asymptotic efficiency of the parameter estimates. Whilst it is difficult to infer to wider SC studies from this finding, on the preliminary evidence available, it does appear that loss of orthogonality does impact upon the parameter estimates of discrete choice models.

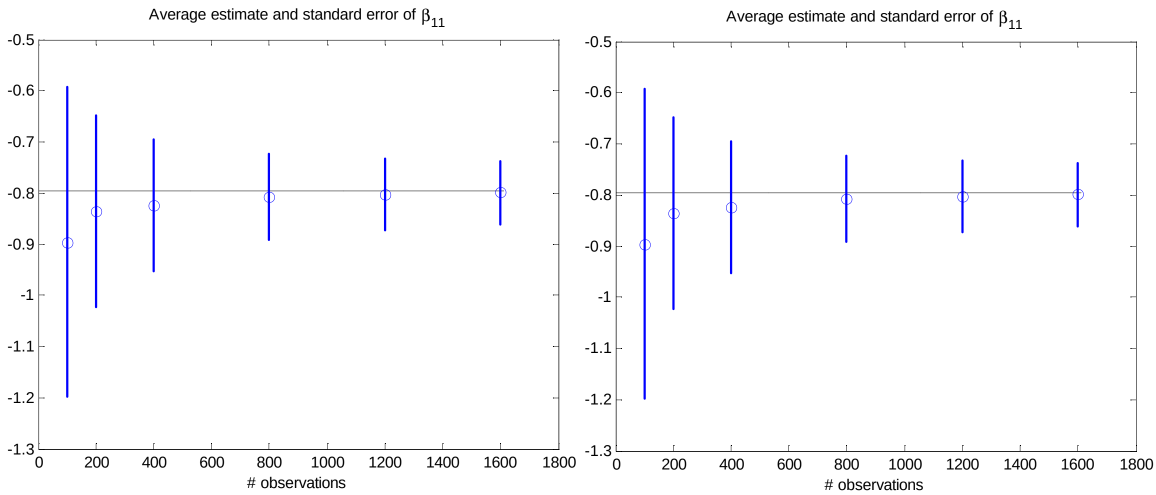
Table 14: Average pairwise correlations over 1000 replications

		Alternative 1			Alternative 2		
Alternative		**Attribute A**	**Attribute B**	**Attribute C**	**Attribute A**	**Attribute B**	**Attribute C**
1	**Attribute A**	1	0.096	0.416	0.032	-0.096	-0.352
1	**Attribute B**	0.096	1	-0.199	-0.028	0.085	-0.028
1	**Attribute C**	0.416	-0.199	1	-0.142	-0.199	-0.085
2	**Attribute A**	0.032	-0.028	-0.142	1	-0.085	0.142
2	**Attribute B**	0.096	0.085	-0.199	-0.085	1	-0.369
2	**Attribute C**	-0.352	-0.028	-0.085	0.142	-0.369	1
	Absolute Sum:	**0.992**	0.4364	**1.0401**	0.4292	0.8336	**0.9761**

Using the same design shown in Table12, we further test the hypothesis that loss of orthogonality in data sets will impact upon the asymptotic efficiency of the parameter estimates of discrete choice models by performing a second Monte Carlo experiment. In the second Monte Carlo Experiment, we assign blocks to respondents in equal proportions over various sample sizes and compare these to the random assignment of choice sets at the same sample sizes. The results are shown in Figure 2 for parameter A of alternative one. The remaining results produce similar results and are hence omitted. Comparing the asymptotic efficiency of parameter estimates given equal replication of blocks versus random assignment of choice sets over increasing sample sizes reveals similar biases in the parameter estimates over 1000 replications. The correlations incurred by the random assignment of choice sets are very small, such that for this particular design, there exist no difference between using equal block replications and the random assignment of choice sets to the sampled population, even in small samples.

3. From Design to Data: An Empirical Example

A stated choice experiment was conducted in 1994 as part of a study into Greenhouse Gas Emissions (GGE) commissioned by the Australian Federal Government. An important objective of the GGE study was the development of a data base to describe the characteristics of the population of households and passenger vehicles in a base year. Households comprise individuals who participate in travel activities; they have available in varying degrees automobiles of many types, as well as public transport. Although a major data component of the GGE study is a household travel survey, administered to over 1400 households in the six capital cities in mainland Australia (excluding Darwin), placing the sampled households in the context of the population requires additional data.



(a) Equal block replication **(b) random assignment of choice sets**

Figure 2: Comparing the asymptotic efficiency of parameter estimates given equal replication of blocks versus random assignment of choice sets over increasing sample sizes

Two SC surveys were developed as part of the study. The first SC experiment addressed commuter choice of mode and departure time when faced with congestion pricing, higher fuel prices, 'new' forms of public transport such a bus priority system and light rail. A second SC survey was also conducted addressing the choice of vehicle and fuel types when faced with higher fuel prices for conventional fuels but lower fuel prices for other fuels, the limitations of range and boot space of electric and alternative fuelled vehicles (e.g., LPG and CNG), greater variability in registration fees, and a new regime of vehicle purchase prices. In this paper, we discuss only the first, mode choice experiment. The vehicle choice experiment is discussed in detail together with estimation of choice models in Hensher and Greene (2001).

Respondents were asked to consider a context in which the offered set of attributes and levels represented the only available means of undertaking a commuter trip from their current residential location to their current workplace location. Respondents were informed that the purpose was to establish each respondent's coping strategies under the described circumstances.

Four alternatives appeared in each mode choice scenario, two private vehicle and two public transport alternatives. The public transport alternatives were rotated in the experiment such that for each replication, a respondent would only have to consider two public transport modes together with the two private automobile alternatives. Thus, each choice set consisted of i) *car no toll*, ii) *car toll road*, iii) *bus* or *busway*, and iv) *train* or *light rail* alternatives. A set of common attributes for the car alternatives used were travel time, fuel cost, parking cost, and travel time variability. The car toll road alternative was also defined on two additional attributes, toll road departure time and toll charge. The public transport alternatives were each defined by five design attributes. These included in-vehicle time, frequency of service, closest stop to home, closest stop to destination, and fare. Three levels were selected for each attribute. To ensure a meaningful interpretation of the travel times, the researchers identified the current trip length for the commute and segmented the showcard sets into three trip lengths. The trip lengths were a) up to 30 minutes, b) 30 - 45 minutes, and c) over 45 minutes. These trip lengths are the same for each urban area, even though the number of commuters in each category was expected to differ. The attribute levels used in the experiment are shown in Table 15.

Table 15: Attribute and attribute levels

(all cost items are in Australian $'s, all time items are in minutes)

SHORT (< 30 mins.)	Car no toll	Car toll rd	PUBLIC TRANSPORT	Bus	Train	Busway	Light Rail
Travel time to work	15,20,25	10,12,15	Total in-vehicle time (one-way)	10,15,20	10,15,20	10,15,20	10,15,20
Pay toll if you leave at this time (otherwise free)	None	6-10, 6:30-8:30, 6:30-9	Frequency of service	Every 5,15,25	Every 5,15,25	Every 5,15,25	Every 5,15,25
Toll (one-way)	None	1,1.5,2	Time from home to closest stop	Walk 5,15,25	Walk 5,15,25	Walk 5,15,25	Walk 5,15,25
Fuel cost (per day)	3,4,5	1,2,3	Time to destination from closest stop	Walk 5,15,25	Walk 5,15,25	Walk 5,15,25	Walk 5,15,25
Parking cost (per day)	Free,$10,$20	Free,$10,$20	Return fare	1,3,5	1,3,5	1,3,5	1,3,5
Time variability	0, ±4,±6	0,±1,±2					
MEDIUM (30-45 mins.)	**Car no toll**	**Car toll rd**	**PUBLIC TRANSPORT**	**Bus**	**Train**	**Busway**	**Light Rail**
Travel time to work	30,37,45	20,25,30	Total time in the vehicle (one-way)	20,25,30	20,25,30	20,25,30	20,25,30
Pay toll if you leave at this time (otherwise free)	None	6-10, 6:30-8:30, 6:30-9	Frequency of service	Walk 5,15,25	Walk 5,15,25	Walk 5,15,25	Walk 5,15,25
Toll (one-way)	None	2,3,4	Time from home to closest stop	Walk 5,15,25	Walk 5,15,25	Walk 5,15,25	Walk 5,15,25
Fuel cost (per day)	6,8,10	2,4,6	Time to destination from closest stop	Walk 5,15,25 Bus 4,6,8	Walk 5,15,25 Bus 4,6,8	Walk 5,15,25 Bus 4,6,8	Walk 5,15,25 Bus 4,6,8
Parking cost (per day)	Free,$10,$20	Free,$10,$20	Return fare	2,4,6	2,4,6	2,4,6	2,4,6
Time variability	0, ±7, ±11	0, ±2, ±4					
LONG (>45 mins.)	**Car no toll**	**Car toll rd**	**PUBLIC TRANSPORT**	**Bus**	**Train**	**Busway**	**Light Rail**
Travel time to work	45,55,70	30,37,45	Total time in the vehicle (one-way)	30,35,40	30,35,40	30,35,40	30,35,40
Pay toll if you leave at this time (otherwise free)	None	6-10, 6:30-8:30, 6:30-9	Frequency of service	Walk 5,15,25	Walk 5,15,25	Walk 5,15,25	Walk 5,15,25
Toll (one-way)	None	3,4.5,6	Time from home to closest stop	Walk 5,15,25	Walk 5,15,25	Walk 5,15,25	Walk 5,15,25
Fuel cost (per day)	9,12,15	3,6,9	Time to destination from closest stop	Walk 5,15,25 Bus 4,6,8	Walk 5,15,25 Bus 4,6,8	Walk 5,15,25 Bus 4,6,8	Walk 5,15,25 Bus 4,6,8
Parking cost (per day)	Free,$10,$20	Free,$10,$20	Return fare	3,5,7	3,5,7	3,5,7	3,5,7
Time variability	0, ±11, ±17	0, ±7, ±11					

The experimental design for the travel choice task was a $27 \times 3^{20} \times 2^2$ orthogonal fractional factorial, which produced 81 scenarios or choice sets. The 27-level factor was used to block the design into 27 versions each with three choice sets containing the four alternatives. Versions were balanced such that each respondent saw every level of each attribute exactly once. The 3^{20} portion of the master design is an orthogonal main effects design, which permits independent estimation of all effects of interest. The two 2-level attributes were used to describe bus/busway and train/light rail modes, such that bus/train options appear in 36 scenarios and busway/light rail in 45. Given the method used to determine which public transport methods were present within a choice set, the bus and LR and train and busway alternatives never appeared within the same choice set. The design allows for six alternative specific main effect models for car no toll, car toll road, bus, busway, train, and light rail. Linear by linear interactions are estimable for both car models, and generically for the bus/busway and train/light rail models. While cross effects have been assumed negligible, the four alternative design is perfectly balanced across all attributes. Further details of the design and survey are given in Hensher et al. (2004). An example showcard is shown in Figure 3.

Figure 3: An example mode choice show card

SA101	1. **CAR, TOLL ROAD**	2. **CAR, NON-TOLL ROAD**
Travel time to work	**10 min.**	**15 min.**
Time variability	**None**	**None**
Toll (one way)	**$1.00**	**Free**
Pay toll if you leave at this time (otherwise free)	**6-10 am**	—
Fuel cost (per day)	**$1.00**	**$3.00**
Parking cost (per day)	**Free**	**Free**
	3. **BUS**	4. **TRAIN**
Total time in the vehicle (one way)	**10 min.**	**10 min.**
Time from home to your closest stop	Walk **5 min.** Car/Bus **4 min.**	Walk **5 min.** Car/Bus **4 min.**
Time to your destination from the closest stop	Walk **5 min.** Bus **4 min.**	Walk **5 min.** Bus **4 min.**
Frequency of service	**Every 5 min.**	**Every 5 min.**
Return fare	**$1.00**	**$1.00**

The data for the mode choice study used here is made available in Hensher et al. (2004). The correlation matrix for the data is given in Appendix A. For space reasons, we limit this to show the pairwise correlations that exist for the attributes used in the experimental design only. Examining the matrix reveals the presence of large correlations, despite the underlying experimental design being orthogonal. Eight pairwise correlations are observed to be equal to or greater than 0.8 in absolute magnitude with the highest correlation observed between the fuel costs for the no toll alternative and the travel time for the train alternative (r = 0.88). A total of 32 pairwise correlations are observed to be greater than or equal to 0.5 in absolute magnitude. Although not shown, correlations between the design attributes and other variables collected range between -0.17 and 0.17 suggesting a much worse degradation in orthogonality between the design attributes than between the design attributes and the other variables.

Reported in Table 16 are three models fitted to the data; a MNL model, a nested logit (NL) model and a mixed logit (ML) model. The public transport egress time attributes were removed from the analysis due to poor significance levels. For the ML model, the fuel cost and public transport fares were estimated as random parameters using a constrained triangular distribution (see Hensher et al., 2004). All three models are statistically significant and in each case, all parameters are of the expected sign, despite the presence of significant correlations present within the data set.

Table 16: Model results from empirical data set

	Model 1: MNL		Model 2: NL		Model 3: ML		
	Coeff.	(T-ratio)	Coeff.	(T-ratio)		Coeff.	(T-ratio)
Car (toll) constant	-0.010	(-0.048)	0.504	(3.559)	*Random parameter Mean*		
Car (toll) cost	-0.120	(-3.684)	-0.102	(-3.201)	Car (toll) cost	-0.135	(-3.571)
Car (toll) TT	-0.055	(-7.167)	-0.034	(-4.444)	Car (no toll) cost	-0.132	(-3.875)
Car (toll) toll cost	-0.062	(-1.709)	-0.050	(-1.370)	Bus fare	-0.194	(-4.835)
Car (toll) parking cost	-0.100	(-17.436)	-0.099	(-17.368)	Train fare	-0.293	(-6.751)
Car (no toll) constant	0.312	(1.445)	0.801	(5.522)	CST5	-0.206	(-5.455)
Car (no toll) cost	-0.116	(-4.348)	-0.085	(-3.241)	CST6	-0.280	(-7.068)
Car (no toll) TT	-0.042	(-6.925)	-0.032	(-5.210)	*Random parameter Spread*		
Car (no toll) parking cost	-0.083	(-15.069)	-0.081	(-14.953)	Car (toll) cost	0.135	(3.571)
Bus Constant	0.064	(0.223)	0.427	(1.066)	Car (no toll) cost	0.132	(3.875)
Bus fare	-0.185	(-5.273)	-0.313	(-6.755)	Bus fare	0.194	(4.835)
Bus TT	-0.042	(-5.028)	-0.073	(-6.749)	Train fare	0.293	(6.751)
Bus frequency	-0.034	(-4.667)	-0.045	(-4.584)	Busway cost	0.206	(5.455)
Bus access time	-0.029	(-3.244)	-0.046	(-3.948)	Light rail cost	0.280	(7.068)
Train constant	-0.441	(-1.723)	-0.593	(-1.845)	*Non Random parameters*		
Train fare	-0.271	(-7.667)	-0.380	(-8.362)	Car (no toll) constant	-0.025	(-0.113)
Train TT	-0.015	(-1.795)	-0.029	(-2.667)	Car (no toll) TT	-0.055	(-6.970)
Train access time	-0.043	(-5.440)	-0.064	(-6.334)	Car (no toll) toll cost	-0.064	(-1.718)
Busway constant	-0.287	(-1.117)	-0.328	(-1.015)	Car (no toll) parking cost	-0.102	(-17.456)
Busway cost	-0.191	(-5.971)	-0.307	(-7.265)	Car (no toll) constant	0.336	(1.467)
Busway TT	-0.023	(-3.070)	-0.044	(-4.382)	Car (no toll) TT	-0.043	(-7.027)
Busway frequency	-0.013	(-2.058)	-0.015	(-1.806)	Car (no toll) parking cost	-0.085	(-14.996)
Busway access time	-0.040	(-5.720)	-0.061	(-6.918)	Bus Constant	0.042	(0.141)
Light rail cost	-0.260	(-8.053)	-0.415	(-9.684)	Bus TT	-0.043	(-4.917)
Light rail TT	-0.038	(-5.121)	-0.052	(-5.387)	Bus frequency	-0.035	(-4.700)
Light rail Frequency	-0.024	(-3.734)	-0.039	(-4.661)	Bus access time	-0.029	(-3.224)
Light rail access time	-0.022	(-2.977)	-0.038	(-3.941)	Train constant	-0.460	(-1.703)
IV parameters					Train TT	-0.015	(-1.674)
Car			1.000	Fixed	Train access time	-0.045	(-5.410)

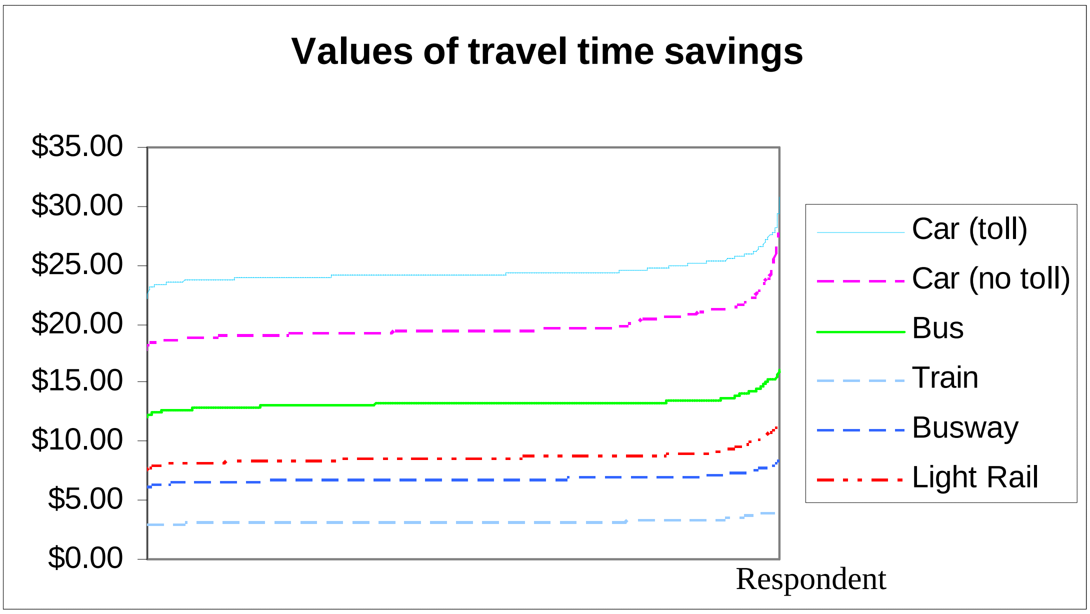
Public Transport		0.173	2.926	Busway constant	-0.309	(-1.147)
				Busway TT	-0.023	(-2.895)
				Busway frequency	-0.013	(-2.064)
				Busway access time	-0.041	(-5.714)
				Light rail TT	-0.040	(-5.048)
				Light rail Frequency	-0.025	(-3.733)
				Light rail access time	-0.022	(-2.881)
LL(0)	-6374.8746	-6374.875			-6475.419	
LL(β)	-4416.367	-4363.202			-4417.172	
χ^2	3854.84403	4023.345			4116.494	

Of interest are the values of travel time savings (VTTS), measured in Australian dollars per hour. These are shown in Table 17 for each of the three models estimated. For the ML model, shown are the average VTTS obtained from the conditional parameter estimates. The actual estimated VTTS are plotted in Figure 4. The x-axis of this figure represents the individual sampled respondents. Whilst the VTTS for the train alternative are low for all models, the remaining VTTS reflect the values observed for commuting trips reported within the literature. This finding raises interesting questions as to the validity of VTTS (and indeed other behavioural outputs) reported within the literature. Unfortunately, not a single study reviewed reported testing for correlations within the data. Assuming that these studies used SC data that had correlations, they cannot be used as a reliable benchmark until such time as it is shown that such correlations have little or no impact upon the outputs of discrete choice models.

Table 17: Value of travel time savings

	MNL	NL	ML (average)
Car (toll)	$27.66	$19.72	$24.40
Car (no toll)	$21.85	$22.28	$19.67
Bus	$13.65	$14.00	$13.27
Train	$3.37	$4.60	$3.10
Busway	$7.30	$8.61	$6.72
Light Rail	$8.86	$7.52	$8.57

Figure 4: ML mode specific VTTS



Although not reported here, we have discovered similar correlation patterns in three other data sets used in published work. The fact that the parameter estimates and behavioural outputs appear to conform to both theory and previous empirical findings (assuming that other SC studies provide a valid benchmark), both in the data set reported here and the others tested, despite the presence of correlations in the data, suggests that either (i) high levels of correlation within choice data sets have little impact upon the choice models we estimate or (ii) that any bias caused by the presence of correlations in choice data sets is having an equal influence on the outputs generated, both in terms of magnitudes and direction, in choice models estimated across data sets. Whilst we cannot rule out the later, it is far more probable that the former hypothesis is true.

3.1 Statistical efficiency and optimality in the generation of SC designs

Previous sections examined orthogonal fractional factorial designs and how they relate to both data sets and model estimation. In the remainder of the paper, we define optimal designs and show how they may be generated. Huber and Zwerina (1996), Sandor and Wedel (2001) and Kanninen (2002) each used Monte Carlo experiments to demonstrate that optimal designs produce asymptotically more efficient parameter estimates at smaller sample sizes than other, less statistically efficient designs. For reasons of space, we therefore omit such experiments here and refer the reader to these studies. We conclude the paper with a discussion of future research directions.

In determining what constitutes the most statistically efficient design, the literature has tended towards designs which maximize the determinant of variance-covariance matrix, otherwise known as the Fisher information matrix, of the model to be estimated. Such designs are known as D-optimal designs. In this paper we use the inversely related measure to calculate the level of D-efficiency; that is, we minimize the determinant of the inverse of the variance-covariance matrix. The determinant of the inverse of the variance-covariance matrix is known as D-error and will yield the same results maximizing the determinant of variance-covariance matrix.

The log likelihood function of the MNL model is shown as equation (7)

$$L = \sum_{n=1}^{N}\sum_{s=1}^{S}\sum_{j=1}^{J} y_{njs}\ln(P_{njs}) + c \tag{8}$$

where y_{njs} is a column matrix where 1 indicates that an alternative j was chosen by respondent n in choice situation s and 0 otherwise, and P_{njs} represents the choice probability obtained from equation (3).[and c is a constant] Maximising equation (7) yields the maximum likelihood estimator, $\hat{b}$, of the specified choice model given a particular set of choice data. McFadden (1974) showed that the distribution of $\hat{b}$ is asymptotically normal with a mean, b, and covariance matrix

$$\Omega = \left(X'PX\right) = \left[\sum_{m=1}^{M}\sum_{j=1}^{J} x'_{njs}\, P_{njs} x_{njs}\right] \tag{9}$$

and inverse,

$$\Omega^{-1} = \left(X'PX\right)^{-1} = \left[\sum_{m=1}^{M}\sum_{j=1}^{J} x'_{njs} P_{njs} x_{njs}\right]^{-1}. \quad (10)$$

where P is a $JS \times JS$ diagonal matrix with elements equal to the choice probabilities of the alternatives, j over choice sets, s.

For Ω, several established summary measures of error have been shown to be useful comparing between designs. The most often used summary measure is known as D-error which is inversely related to D-efficiency.

$$\text{D-error} = \left(\det \Omega^{-1}\right)^{\frac{1}{K}} \quad (11)$$

where K is the total number of generic parameters to be estimated from the design (see Appendix B).

Minimization of equation (10) will produce the design with the smallest possible errors around the estimated parameters.

To demonstrate the calculation of the D-error measure, consider a main effects only, fractional factorial orthogonal design involving two unlabelled alternatives each with three attributes described on four levels. Assuming the estimation of linear marginal utilities for each attribute, the smallest design available requires four degrees of freedom (i.e., 3 + 1). Unfortunately, there exists no combination of orthogonal columns in four rows. The smallest balanced design possible will have eight treatment combinations out of a possible 4096 (i.e., $4^{(2\times3)}$). An example design is shown in Table 18.

Table 18: Orthogonal main effects only fractional factorial design

Treatment combination	Alt 1			Alt 2		
	A	**B**	**C**	**A**	**B**	**C**
1	3	3	-3	1	1	-1
2	-3	3	3	-1	1	1
3	3	1	3	-1	-3	-1
4	-1	-3	-1	-3	-1	-3
5	-1	-1	1	3	3	-3
6	1	-1	-1	-3	3	3
7	1	-3	1	3	-1	3
8	-3	1	-3	1	-3	1

Prior knowledge of the parameters associated with each attribute may be used to determine the statistical efficiency of the design. The most common assumption is that the parameters for all attributes are simultaneously equal to zero. Using equation (1) and ignoring e_{njs}, the assumption of parameter insignificance constrains the utilities for all alternatives to be equal to zero. Substitution of utility estimates of zero into equation (2) will result in probabilities for each alternative being equal (i.e., 0.5 and 0.5 for two alternatives, 0.33, 0.33 and 0.33 for three alternatives etc). This in turn constrains the diagonal elements in matrix P to be equal.

To demonstrate the calculation of the D-error, we are required to reformat the presentation of the design shown in Table 19 so that each row of the table represents a separate alternative. We have done this in Table 19. In Table 19, we have also substituted the attribute level labels we would use in both the survey and in the data (-3 = 5, -1 = 10, 1 = 15 and 3 = 20). This last step is necessary to allow for a direct comparison with the D-error of designs (see Appendix B). The D-error for the design is calculated as 0.001587. The calculation for this is shown in Appendix B.

Table 19: Calculating the D-error under the assumption of $ß_{njs}$ = 0

Treatment combination	**Alt**	**A**	**B**	**C**	Prob(i)
1	1	20	20	5	0.5
1	2	15	15	10	0.5
2	1	5	20	20	0.5
2	2	10	15	15	0.5
3	1	20	15	20	0.5
3	2	10	5	10	0.5
4	1	10	5	10	0.5
4	2	5	10	5	0.5
5	1	10	10	15	0.5
5	2	20	20	5	0.5
6	1	15	10	10	0.5
6	2	5	20	20	0.5
7	1	15	5	15	0.5
7	2	20	10	20	0.5
8	1	5	15	5	0.5
8	2	15	5	15	0.5
			D-error		0.001587

Let us now assume that the analyst has some prior knowledge of the parameter estimates for each of the attributes under study. Such information may be used to estimate the expected choice probabilities for each alternative across all choice sets. To demonstrate, assume that the analyst believes that the parameters for attributes A, B and C are -0.5, 1.0 and 0.5 respectively. Using equations (1) and (2), the choice probabilities for each alternative present within a choice set may be calculated. We show this in Table 20. In Table 20, it can be seen that the utilities for the two alternatives in treatment combination one are balanced, hence the probabilities are both equal to 0.5. In the second treatment combination however, the utility for the first alternative is significantly greater than that of the second, thus suggesting that the first alternative is a dominant alternative. Assuming the parameter estimates used are correct, six of the eight treatment combinations contained within the design have dominant alternatives. The presence of a dominant alternative within a choice will provide no information as to the trade-offs being made by rational respondents. For the example shown, the only useful information captured in the choice task is captured within two of the eight choice sets observed over all respondents.

Table 20: Calculating the choice probabilities with ß$_{njs}$? 0

Treatment combination	Alt	A	B	C	β_A	β_B	β_C	V	exp(V)	Prob(i)
1	1	20	20	5	-0.5	1.0	0.5	12.5	268337.2865	0.5
1	2	15	15	10	-0.5	1.0	0.5	12.5	268337.2865	0.5
2	1	5	20	20	-0.5	1.0	0.5	27.5	8.77199×10^{11}	0.999954602
2	2	10	15	15	-0.5	1.0	0.5	17.5	39824784.4	4.53979×10^{-05}
3	1	20	15	20	-0.5	1.0	0.5	15	3269017.372	0.999954602
3	2	10	5	10	-0.5	1.0	0.5	5	148.4131591	4.53979E-05
4	1	10	5	10	-0.5	1.0	0.5	5	148.4131591	0.006692851
4	2	5	10	5	-0.5	1.0	0.5	10	22026.46579	0.993307149
5	1	10	10	15	-0.5	1.0	0.5	12.5	268337.2865	0.5
5	2	20	20	5	-0.5	1.0	0.5	12.5	268337.2865	0.5
6	1	15	10	10	-0.5	1.0	0.5	7.5	1808.042414	2.06115×10^{-09}
6	2	5	20	20	-0.5	1.0	0.5	27.5	8.77199×10^{11}	0.999999998
7	1	15	5	15	-0.5	1.0	0.5	5	148.4131591	0.006692851
7	2	20	10	20	-0.5	1.0	0.5	10	22026.46579	0.993307149
8	1	5	15	5	-0.5	1.0	0.5	15	3269017.372	0.999954602
8	2	15	5	15	-0.5	1.0	0.5	5	148.4131591	4.53979×10^{-05}
									D-error	0.001347

Table 21 shows the D-optimal design, generated so as to minimize the inverse of the determinant of the Fisher information matrix, as shown in equation (10). In generating the design, we have placed an additional constraint in that the attribute levels shown produce utility balance for the parameter priors, forcing the probabilities to be equal across all alternatives present within a choice set (see Huber and Zwerina, 1996). This strategy ensures that no alternative will dominate a choice set. Further, examination of the D-error calculated for this design reveals a lower value than that calculated for the orthogonal fractional factorial design. As such, the design will produce smaller errors around the estimated parameters, assuming that the priors used are correct. This however, has come at the cost of the introduction of substantial correlations (see Table 22).

Table 21: D-optimal design

Treatment combination	Alt	A	B	C	β_A	β_B	β_C	V	exp(V)	Prob(i)
1	1	5	5	20	-0.5	1.0	0.5	12.5	268337.2865	0.5
1	2	20	20	5	-0.5	1.0	0.5	12.5	268337.2865	0.5
2	1	15	5	20	-0.5	1.0	0.5	7.5	1808.042414	0.5
2	2	20	15	5	-0.5	1.0	0.5	7.5	1808.042414	0.5
3	1	5	15	20	-0.5	1.0	0.5	22.5	5910522063	0.5
3	2	10	20	15	-0.5	1.0	0.5	22.5	5910522063	0.5
4	1	15	5	20	-0.5	1.0	0.5	7.5	1808.042414	0.5
4	2	20	10	15	-0.5	1.0	0.5	7.5	1808.042414	0.5
5	1	5	10	20	-0.5	1.0	0.5	17.5	39824784.4	0.5
5	2	10	20	5	-0.5	1.0	0.5	17.5	39824784.4	0.5
6	1	20	5	20	-0.5	1.0	0.5	5	148.4131591	0.5
6	2	15	10	5	-0.5	1.0	0.5	5	148.4131591	0.5
7	1	15	10	20	-0.5	1.0	0.5	12.5	268337.2865	0.5
7	2	20	20	5	-0.5	1.0	0.5	12.5	268337.2865	0.5
8	1	5	20	5	-0.5	1.0	0.5	20	485165195.4	0.5
8	2	10	15	20	-0.5	1.0	0.5	20	485165195.4	0.5
									D-error	0.001143

Table 22: Correlation matrix for the D-optimal design

		Alt 1			***Alt 2***		
		A	*B*	*C*	*A*	*B*	*C*
Alt 1	A	1.0					
	B	-0.59801	1.0				
	C	0.364698	-0.76255	1.0			
Alt 2	A	0.563798	-0.75212	0.458682	1.0		
	B	-0.67883	0.322045	0.113961	-0.20328	1.0	
	C	-0.35632	0.6998	-0.68887	-0.44815	-0.22628	1.0

4. Conclusion and Discussion

We show that data sets created using orthogonal fractional factorial designs are unlikely to retain statistical independence between the attributes once data is collected. Further, we argue that only under the most exceptional of circumstances will the design attributes be uncorrelated with any covariates that are collected. Yet even in such cases where orthogonality has been maintained, correlations are likely to be induced through the model estimation process. The probable introduction of correlations in the data through the unequal replication of treatment combinations, through the collection of covariates, and through the model estimation process, raises interesting questions as to the impact such correlations have in terms of the parameters estimated from models of discrete choice. Indeed, the question is, does it really matter?

Through the use of an empirical example, we show that despite the presence of significant correlations, discrete choice models estimated from SC data produce empirically sensible parameter estimates (indeed, it is the Monte Carlo work that will guide one as to any biases since empirical evidence is typically problematic given one has little idea about biases in other studies being compared). Nevertheless, through the use of Monte Carlo simulation, we demonstrate that the presence of correlations may produce asymptotically inefficient parameter estimates, although we note that such inefficiencies are unlikely to result from the random assignment of choice sets to respondents in relatively large samples. These results suggest the need for further research to be conducted on the impact of the presence of correlations on models of discrete choice, particularly on the asymptotic efficiency of the parameter estimates derived from such models. Specific areas of research required, include research into sampling strategies that may reduce correlations within data, and the possible design of experiments which are orthogonal, not in the attributes, but rather in the differences between the attribute levels.

We further demonstrate, through the use of an example, an alternative design construction method, used to construct designs which minimise errors in the parameter estimates. Although not shown here, several researchers have demonstrated that such designs offer substantial benefits in the estimation of asymptotically efficient parameter estimates at reduced sample sizes. In constructing our example, we have employed the use of utility balancing, a method introduced by Huber and Zwerina (1996). Although not a necessary condition for the generation of D-optimal designs, Huber and Zwerina (1996) show that utility balanced designs provide substantial improvement in the efficiency of choice designs. We acknowledge that further improvements in design efficiency may be possible if such a constraint is not enforced. It is therefore left to the analyst to decide the relevancy of utility balance in the specific context of the study being undertaken. One potential problem with balancing the utilities of the alternatives present within each choice set arises if the parameter priors used correctly identify those of the population. Whilst this seems counterintuitive given that the objective of the design strategy is to both minimize the errors around the parameter estimates, which will occur only when the priors are correct, whilst at the same time promoting trading between similar alternatives, if respondents are truly indifferent between alternatives, then the choice process can only be random. Such an outcome is unlikely, however, if the marginal utilities are distributed over some range amongst the population. Such a proposition, testable via models such as the ML model, raises interesting questions as to how best the parameter distribution may be incorporated in the design of optimal SC designs.

The question of how to construct optimal designs with a fixed alternative has yet to be addressed by the literature, despite calls from some authors (e.g., Huber and Zwerina, 1996). Given the prevalence of such designs, research is required to develop theory as to how to generate such designs, so that they are statistically optimal given that fixed alternatives generally have no associated design parameters. Added to this is the necessity to further the theory of optimal designs to incorporate labelled SC experiments.

A further research issue involves the investigation of what constitutes the best source for determining the priors used in generating optimal designs. Should the analyst conduct a pilot study, and if so, what represents a sufficient sample size to obtain the priors? Alternatively, should the analyst rely upon managers and other practitioners beliefs and how best should such beliefs be captured?

SC methods are now an accepted methodology of capturing individual's preferences for goods and services. Such an acceptance has largely arisen due to the methods ability to emulate real behaviour and produce empirically sensible estimates. In this paper, we have concentrated on the statistical properties of SC designs and have ignored the role of the most important player in SC studies; the respondent. Whilst there has been a steady stream of research addressing the impact upon cognitive burden of varying aspects of SC designs, there needs to be a direct link made between respondent's ability to partake in SC methods in a meaningful manner given various dimensions of the experiment. Although, in this paper, we have concentrated on the statistical properties of SC experimental designs and data, it is the link between these properties and other important issues, such as the information processing strategies used by respondents in completing SC studies, that future research should concentrate. It is only through the combining of knowledge of respondent's behaviour and statistical design theory can SC methods reach their full potential.

Appendix A: Correlation matrix for Mode choice example

Table A: Mode choice example correlations

	Car toll travel time	Car no toll travel time	Bus travel time	Train travel time	Busway travel time	Light Rail travel time	Car toll time variability	Car no toll time variability	*Toll*	Toll departure time	Car toll fuel	Car no toll fuel	Car toll parking cost	Car no toll parking cost
Car toll travel time	1.00													
Car no toll travel time	***0.86***	1.00												
Bus travel time	0.71	***0.81***	1.00											
Train travel time	***0.83***	0.77	0.60	1.00										
Busway travel time	***0.82***	***0.85***	0.61		1.00									
Light Rail travel time	0.71	0.62		0.76	0.69	1.00								
Car toll time variability	0.35	0.34	0.28	0.32	0.33	0.30	1.00							
Car no toll time variability	-0.33	-0.32	-0.35	-0.32	-0.29	-0.30	-0.13	1.00						
Toll	0.59	0.58	0.49	0.61	0.52	0.47	0.18	-0.24	1.00					
Toll departure time	-0.01	-0.02	-0.02	0.02	0.00	0.00	0.03	0.01	-0.06	1.00				
Car toll fuel	0.70	0.67	0.45	0.63	0.69	0.63	0.27	-0.25	0.47	0.04	1.00			
Car no toll fuel	***0.87***	***0.83***	0.73	***0.88***	0.74	0.65	0.34	-0.33	0.59	0.00	0.67	1.00		
Car toll parking cost	0.00	0.00	-0.01	-0.07	0.04	0.02	-0.01	-0.02	0.01	-0.01	-0.01	0.00	1.00	
Car no toll parking cost	0.01	0.00	0.03	0.00	-0.01	-0.01	0.01	-0.04	0.03	-0.01	-0.03	0.00	-0.01	1.00
Bus frequency	0.01	0.03	0.06	-0.01	-0.04		0.08	-0.15	0.07	-0.10	-0.07	0.02	0.04	0.03
Train frequency	-0.02	-0.02	0.01	0.00		0.01	-0.02	0.04	0.02	-0.01	0.00	-0.02	-0.26	0.03
Busway frequency	0.01	0.02			0.02	-0.03	-0.03	0.03	0.00	0.05	0.03	-0.01	0.08	0.02
Light rail frequency	-0.01	-0.01		-0.02	0.03	0.01	0.07	0.12	-0.05	0.12	0.00	0.01	0.02	0.02
Bus fare	0.42	0.38	0.32	0.31	0.32		0.54	-0.18	0.27	-0.05	0.26	0.37	-0.03	0.27
Train fare	0.49	0.41	0.32	0.40		0.30	0.15	-0.19	0.31	0.02	0.33	0.42	-0.05	0.45
Busway fare	0.33	0.37	0.29		0.37	0.29	0.15	-0.10	0.24	-0.01	0.37	0.38	0.00	-0.38
Light rail fare	0.38	0.41		0.41	0.36	0.32	-0.09	-0.13	0.29	-0.01	0.34	0.41	0.02	-0.18
Bus access Time	0.02	0.03	0.02	-0.05	0.11		-0.01	0.02	0.04	0.01	0.01	-0.01	0.02	-0.01
Train access Time	-0.02	0.01	0.01	-0.06		-0.10	0.05	0.04	-0.06	0.19	-0.04	-0.01	-0.02	0.00
Busway access Time	-0.01	0.04	-0.06		0.02	-0.04	-0.07	-0.03	0.04	-0.16	0.04	0.00	-0.01	0.08
Light Rail access Time	-0.02	0.00		0.01	-0.02	-0.04	-0.03	-0.01	-0.05	0.02	-0.02	-0.03	0.04	0.01
Bus egress time	-0.05	0.01	0.00	-0.03	0.08		-0.05	0.02	-0.03	0.07	-0.01	-0.01	0.02	0.34
Train egress time	-0.03	0.00	-0.09	0.00		0.05	-0.01	0.02	0.07	0.03	0.05	-0.01	0.20	-0.05
Busway egress time	0.03	0.02	0.07		-0.01	-0.02	0.00	-0.06	-0.01	-0.08	-0.02	0.03	-0.10	0.08
Light rial egress time	0.04	0.01		-0.03	0.03	-0.02	0.06	-0.02	0.00	0.03	0.01	-0.01	0.04	-0.29

Table A: Mode choice example correlations (cont'd)

	Bus frequency	Train frequency	Busway frequency	Light rail frequency	Bus fare	Train fare	Busway fare	Light rail fare	Bus access Time	Train access Time	Busway access Time	Light Rail access Time	Bus egress time	Train egress time	Busway egress time	Light rial egress time
Bus frequency	1.00															
Train frequency	-0.02	1.00														
Busway frequency			1.00													
Light rail frequency		-0.01	0.00	1.00												
Bus fare	0.07	0.01			1.00											
Train fare	-0.02	-0.01		-0.02	-0.01	1.00										
Busway fare	-0.03		0.02	0.01	-0.06		1.00									
Light rail fare		-0.01	0.02	-0.02		0.61	0.16	1.00								
Bus access Time	-0.04	0.29			0.02	0.00	0.09		1.00							
Train access Time	0.21	-0.06		0.16	0.07	-0.02		-0.10	0.23	1.00						
Busway access Time	-0.38		-0.03	0.03	-0.01		-0.06	-0.04	0.13		1.00					
Light Rail access Time		-0.32	0.06	0.02		0.03	0.02	-0.03		0.17	0.18	1.00				
Bus egress time	-0.01	0.03			-0.10	0.29	0.00		0.21	0.14	0.15		1.00			
Train egress time	0.01	0.08		-0.02	-0.07	-0.01		0.03	0.34	0.05		0.25	0.18	1.00		
Busway egress time	0.18		-0.03	-0.03	0.07		0.01	-0.02	-0.25		0.11	0.13	0.16		1.00	
Light rial egress time		-0.08	0.09	0.03		-0.18	-0.02	0.08		0.05	0.11	0.18		0.12	0.15	1.00

Appendix B

The D-error is calculated as follows.

Let *X* =

20	20	5
15	15	10
5	20	20
10	15	15
20	15	20
10	5	10
10	5	10
5	10	5
10	10	15
20	20	5
15	10	10
5	20	20
15	5	15
20	10	20
5	15	5
15	5	15

In defining X, we have used the attribute level labels as opposed to the orthogonal codes used to generate fractional factorial designs. Although not applicable for this example, when parameter priors are used to calculate the choice probabilities for the alternatives present within choice set *s*, the probabilities should be generated using the actual data to be used in model estimation, not the orthogonal codes (assuming the orthogonal codes are not those to be used. To demonstrate, consider the utilities for alternatives one and two for choice set two. Using orthogonal coding and the parameter priors, the utilities for the two alternatives will be calculated as

$U_1 = -3\times-0.5 + 3\times1 + 3\times0.5 = 6$
$U_2 = -1\times-0.5 + 1\times1 + 1\times0.5 = 2$

and the probabilities as

$P(1) = 0.982014$ and $P(2) = 0.017986$

Using the actual data sown to respondents, the utilities become

$U_1 = 20\times-0.5 + 20\times1 + 5\times0.5 = 27.5$
$U_2 = 15\times-0.5 + 15\times1 + 10\times0.5 = 17.5$

and the choice probabilities

$P(1) = 0.999955$ and $P(2) = 4.54\times10^{-5}$.

Clearly the two are different. Given that the choices are made on the attribute level labels shown to respondents, the utilities and choice probabilities should be calculating on the actual data used.

Let $P =$

0.5	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
0	0.5	0	0	0	0	0	0	0	0	0	0	0	0	0	0
0	0	0.5	0	0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0.5	0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0.5	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0.5	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0.5	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0.5	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0.5	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0.5	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0.5	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0.5	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0	0.5	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0	0	0.5	0	0
0	0	0	0	0	0	0	0	0	0	0	0	0	0	0.5	0
0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0.5

Then $X'P =$

10	7.5	2.5	5	10	5	5	2.5	5	10	7.5	2.5	7.5	10	2.5	7.5
10	7.5	10	7.5	7.5	2.5	2.5	5	5	10	5	10	2.5	5	7.5	2.5
2.5	5	10	7.5	10	5	5	2.5	7.5	2.5	5	10	7.5	10	2.5	7.5

and $\left(X'PX\right)=$

1500	1250	1250
1250	1500	1250
1250	1250	1500

The inverse of the fisher information matrix, Ω^{-1}, is calculated as, $\left(X'PX\right)^{-1}$, giving

0.00275	-0.00125	-0.00125
-0.00125	0.00275	-0.00125
-0.00125	-0.00125	0.00275

The determinant of Ω^{-1} is next calculated and taken to the power of 1/*K*, where *K* is the number of parameters to be estimated.

$$\text{D-error} = \left(\det \Omega^{-1}\right)^{\frac{1}{K}} = \left|\left(X'PX\right)^{-1}\right|^{\frac{1}{3}} = 0.001587$$

It is only possible to calculate the D-error statistic for generic (i.e., unlabelled) choice designs. The reason for this is as follows. Assume *X* to be a *k*×*s* matrix, where *k* is the number of parameters to be estimated and *s* is the number of rows or treatment combinations within the design, *X'* an *s*×*k* matrix and *P* a *k*×*k* diagonal matrix where the diagonals are the choice probabilities given the assumed parameter priors. *X'PX* is therefore a *k*×*k* matrix. For a labelled choice design, each alternative, *j*, will have *k* attributes. Thus, Ω is required to be of dimensions *jk*×*jk*. In setting up the *X* matrix, each row of *X* represents an alternative within the design. Thus, *X* remains a *k*×*s* matrix, *P* a *k*×*k* diagonal matrix and the fisher information matrix, *X'PX* or Ω, a *k*×*k* matrix. In order for Ω to be of dimension *jk*×*jk*, *X* must be of dimension *jk*×*s* and P of dimension

$jk \times jk$, however as discussed, the X matrix must be set up as a $k \times s$ matrix. Setting up X as a $jk \times jk$ matrix posses problems in that each k is associated with a specific j in the design and hence will be zero for all alternatives, i?j. This poses problems in the multiplication of $X'PX$.

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