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Designs for Stated Preference Experiments

Jennifer Lynn Golek

University of Tennessee - Knoxville

To the Graduate Council:

I am submitting herewith a dissertation written by Jennifer Lynn Golek entitled "Designs for Stated Preference Experiments." I have examined the final electronic copy of this dissertation for form and content and recommend that it be accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy, with a major in Business Administration.

Robert W. Mee, Major Professor

We have read this dissertation and recommend its acceptance:

Halima Bensmail, Mary Leitnaker, Michael McKee, William Seaver

Accepted for the Council:
Carolyn R. Hodges

Vice Provost and Dean of the Graduate School

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Designs for Stated Preference Experiments

A Dissertation

Presented for the

Doctor of Philosophy

Degree

The University of Tennessee, Knoxville

Jennifer Lynn Golek
December 2005

DEDICATION

To my family, without your love and support this would not have been possible.

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I would like to thank my advisor, Dr. Robert Mee. I cannot express how much I appreciate the time and effort you have expended in making me a better researcher, teacher and student. The lessons you taught me will stay with me always.

Thank you to all the members of my committee. I have enjoyed with many hours I spent with Dr. Leitnaker, both in the classroom in pursuit of academic knowledge and during our many conversations outside the classroom. Thank you to Dr. Bensmail for encouraging me to broaden my horizons for my Masters project. Thank you to Dr. McKee for providing insight into how statistics can be used to enhance experimentation in other fields, and for providing the background that became the genesis of this work. Thank you to Dr. Seaver for teaching me how to be a better teacher and to broaden my horizons to consider all the possibilities.

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ABSTRACT

We explore the use of different strategies for the construction of optimal choice experiments and their impact on the overall efficiency of the resulting design. We then evaluate how these choice designs meet the desired characteristics of optimal choice designs (orthogonality, level balance, utility balance and minimum level overlap). We further explore the feasibility of using entropy as a secondary measure of design optimality. We find that current algorithms afford little flexibility for using this secondary measure. We further study the impact of misspecification of the assumed parameter values used in creation of optimal choice designs. We find that the impact of misspecification varies widely based on the discrepancy between the true and assumed parameter values. Further we find that entropy becomes a more feasible secondary measure of design optimality if one considers the potential of misspecification of the values. Current design and analysis strategies for stated preference experiments assume that compensatory decisions are made. We consider how different decision strategies may be represented through manipulating the assumed parameter values used in creating the choice designs. In this context, the consequences of misspecification of the decision strategy are also evaluated. Given the large prevalence of no-choice choices in stated preference experiments, we study how different measures of choice complexity impact the selection of the no-choice alternative. We conclude by suggesting a comprehensive strategy that should be followed in the creation of choice designs.

Keywords: Stated Preference Experiment, Discrete Choice Experiment, Entropy, Efficiency, Non-compensatory Decision Strategy, No-Choice Alternative

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List of Abbreviations

K – Number of Attributes

J_n – Number of Alternatives in Choice Set n

N – Number of Choice Sets

$J_{\bullet}$ – Total Number of Alternatives

I – Information Matrix

1. Introduction

1.1 Choice as a Way of Life

Choice is a way of life. In each and every activity there are choices – what to eat, where to eat, where to live, what to buy – the options are innumerable and as a result companies and not-for-profit organizations now find that they need to be more attentive to the needs and wants of their customers.

The term customer has assumed a much wider definition in recent years. The *American Heritage Dictionary of the English Language* indicates that a customer is "1. One that buys goods or services or, in a more informal setting, 2. An individual with whom one must deal". The customer to a marketing department is the individual who purchases the organization's goods or services; to a city government a customer is each of the constituents of the area; and to an environmentalist the customer is any individual impacted by an impending environmental change. Every organization must know who their customers are, what they need and want; otherwise the customers can choose to take their business elsewhere.

The rapid pace of change faced by organizations, whether for-profit or not-for-profit, evolves as new organizations enter the market and others leave. These organizations require tools to help monitor the needs and wants of their customers. Stated preference techniques afford organizations the opportunity to devise studies to comprehensively understand these needs and wants.

Stated preference methods are pervasive in many fields, though often identified by different names, including conjoint analysis, contingent valuation, and discrete choice analysis. In Marketing, the studies of consumers and the choices they make is a mature and well understood field. Starting with the seminal work of Tukey and Luce in 1964, the field of conjoint analysis was developed as an experimental technique to study consumer's choices. The field has evolved over time and is now a subset of a collection of techniques known as stated preference analysis. In Economics, the field of contingent valuation is a well researched, documented and applied field to understand consumer's preferences for non-market goods. The techniques in this field are also a subset of those known as stated preference analysis. Economic experimentation is classified into two broad categories, nomotheoretical experimentation (which is motivated by well-articulated formal theories) and nomoempirical experimentation (studying the effects of variables not well understood in formal theories). Stated preference techniques applied in economics most often relate to nomotheoretical experimentation (Madden 1995). Many other fields, including medicine and transportation, have adopted and researched the techniques available through stated preference analysis.

Although the modeling and cognitive techniques of stated preference analysis are well researched, both practically and academically, the experimental design work is not as complete as work on models. This affords an opportunity for statistical research on the design of stated preference studies.

1.2 The Evolution of Conjoint Analysis

Discrete choice analysis and conjoint experiments are the most widely applied techniques for measuring and analyzing the preferences of consumers by marketing professionals. The application of these techniques is embraced by academia and industry making this a rich field with great potential for further development, due especially to the constraints and complexities associated with experimental design in the industrial framework. The seminal work on conjoint analysis was published in 1964 by Luce, a mathematical psychologist, and Tukey, a statistician. In 1971, Green and Rao introduced the concepts of conjoint analysis to the field of marketing, and since that time there have been many well documented applications of the techniques. For a comprehensive review of the use of conjoint experiments see Green et al's (2001) article *Thirty Years of Conjoint Analysis: Reflections and Prospects*. The application of conjoint techniques, especially in the world of marketing, has been conducted worldwide.

Conjoint analysis asks respondents to sort (no ties allowed), rate or rank (ties allowed) a set of profiles, constructed using a selected group of attributes, on a given scale, for example the likelihood of purchase. The profiles given to the participants are designed through the use of experimental design techniques. Once compiled, analysis of this data is typically completed through the use of ordinary least squares (OLS) which provides estimates of the partworth values of each attribute. These partworths reflect the weights that respondents place on the levels of the attributes used to construct the experimental profile. The results can then be used to predict the market share for certain products or to respond to market segments.

Discrete choice experiments were introduced to the marketing literature in 1983 by Louviere and Woodworth and have since become a popular choice for studying the choice behavior of consumers. In discrete choice experiments participants are provided with a choice set that contains several different alternatives (profiles) and asked to make a choice among them. Often this choice is phrased as "Given the need and these options, which product would you select?" These alternatives are similar to those used in conjoint analysis as they are constructed through the use of experimental design techniques, but are presented in choice sets of smaller size. Each participant is presented with several choice sets and asked to make a set of sequential decisions regarding their preferences.

Stated preference experiments have several advantages over traditional conjoint experiments. First, the data collection for a discrete choice experiment involves simulated purchase decisions (Haaijer, Kamakura and Wedel 2001). The participant is provided with several different options and asked to select the one they would be the most likely to purchase, unlike the rating, ranking or sorting of a conjoint experiment. Second, stated preference methods provide a direct estimate of the market share for each product in the study, unlike conjoint experiments where these market shares must be estimated after estimating the parameters. Third, stated preference experiments provide the option for utilizing alternative or brand specific attributes and levels. Lastly stated preference experiments provide the ability for the consumer to state that they find none of the purchase options provided acceptable by making a no purchase decision.

Although stated preference experiments provide many advantages over traditional conjoint studies, there are several disadvantages that can be identified. First, the choice response in a stated preference experiment provides less information than the rating, ranking or sorting of the traditional conjoint experiment. Second, large sample sizes from each participant are often required in order to collect enough information for the results to have the necessary precision. Lastly, we are often unable to model the results of a stated preference experiment at the individual level as is done in conjoint experimentation.

Sample size restrictions often require that the modeling be completed at the aggregate level, whether by the entire population or by previously defined meaningful segments.

1.3 Evolution of Stated Preference Experiments

How consumers make a choice from among competing products is a major concern for many involved in marketing research. Understanding how the consumer makes decisions concerning multi-attribute products is analogous to understanding each consumers "black box" of decision making. Although difficult to accomplish, the ability to understand these internal processes provides a wealth of powerful information from which market share elasticities with respect to price, features and other variables can be calculated.

The use of choice theory in economics is a mature field, although it has primarily focused on the use of field data, also known as scanner data or revealed data, for the construction of the model. Field data, while entirely reflective of a consumer's preference, cannot answer every question about the consumer's choices. Field data often contains many limitations that can restrict its potential uses in economic analyses. Confounding or high collinearity between the observed attributes and a lack of variability in many attributes makes it impossible to gain a true picture of the elasticities from field data. Further, in some applications limited or insufficient sample sizes results in imprecise parameter estimates (Madden 1995).

The use of experimental choice data provides a solution to many of the problems associated with field data. First, the circumstances of choice are precisely specified, eliminating the need to analyze the field data to identify effects that may be dependent upon one another. Second, experimentation allows the estimation of the effects of interest with maximum precision. Thirdly and most importantly, in the case of new product introductions and any other new attribute or attribute level, there may not exist field data from which to understand consumer choice preferences. As a result, experimental data is a powerful tool for understanding the preferences of consumers. This data can be used alone or in conjunction with field data to create a more robust model.

Questions still remain about the validity of the data resulting from stated preference studies. Some believe that the results of stated preference studies may diverge from true preferences or revealed preferences due to experimental error or overly complex experimental designs. Others question the reliability of these designs. Reliability is generally considered to be comprised of two components, validity and stability. A design without validity indicates that there is a discrepancy between the stated preference data and actual behavior. The stability of the study concerns the magnitude of the random error present in the study (Madden 1995).

Both types of data, revealed and stated preference, can be found to have limitations and advantages. When designing a study, it is important to pay special attention to the needs of the study and to make careful considerations as to which sort of data will best suit those needs. In some situations, such as new product introductions and extensive product updates, there may be no alternative other than to use stated preference data as the revealed preference data may not exist. In other situations a combination of the field and stated preference data, either in modeling or validation, may be the best approach for the study.

1.4 Outline of Dissertation

Section Two of this dissertation will provide an introduction to stated preference models. Terminology and key topics will be introduced.

Section Three will discuss the cognitive reasoning of consumers in the process of making choices. The different decision strategies employed by consumers and their influence on stated preference experiments are explored.

Section Four will discuss the design of stated preference experiments. An overview of existing design methodologies will be presented along with their current applications. The efficiency of a choice design will be discussed as well as existing techniques for searching for more efficient designs in the design space. Limitations of the existing design techniques will also be presented and discussed. In addition measures for evaluating the complexity of a choice design will also be discussed.

Section Five will discuss and evaluate optimal design strategies for stated preference experiments. Several different methods of constructing efficient choice designs have been suggested in the literature and are reviewed and critiqued here.

Section Six addresses optimal choice designs with respect to the desired characteristics of choice designs. A review of the assumptions made in creating optimal designs and possible violations due to the structure of choice designs is also made.

Section Seven explores creating optimal designs that are optimal on two characteristics, efficiency and entropy. Simulations are used to explore the range of entropy that can be achieved on both optimal and randomly created designs.

Section Eight explores the consequences of misspecification of the assumed parameter values used in the creation of optimal choice designs to the entropy and efficiency of the choice designs created. This is explored through simulating optimal choice designs under a variety of assumed parameter vectors.

Section Nine discusses how non-compensatory decision strategies can be represented though varying the assumed parameter values used in the creation of optimal choice designs. Recommendations for making these assumptions are presented.

Section Ten discusses the issues in modeling and design when the no-choice alternative is presented as an option in the choice set. The effect of the complexity measures introduced in section four on the probability of no-choice responses will also be evaluated. In addition the effect of losing specific observations in the experiment to the overall efficiency of the design will be studied.

Section Eleven will discuss the proposed steps to be used in creating choice experiments. The individual steps and the reasoning behind them will be discussed. In addition a sample experiment will be created using these steps.

Section Twelve presents a summary of the main accomplishments of this dissertation.

Section Thirteen presents suggestions for future research opportunities in the design of stated preference experiments.

2. Stated Preference Models

This chapter will discuss the history of probabilistic choice models. We will provide a background of the necessary terminology and models intended to be comprehensive enough that a novice in the field can achieve a basic understanding of the concepts being employed.

Stated Preference methods employ the techniques of both linear models and probabilistic choice models. The response format for the stated preference experiment determines the model that will be used. If the response for the stated preference experiment is a scheme of rating then the theory of linear models will be used. If the response for the stated preference experiment is ranking where there are five or more levels, then again the linear models theory can be used (with fewer than five levels we must revert to non-linear methods for analysis). When the response for the stated preference experiment is a choice, then a probabilistic choice model will be employed. We will assume the reader has a background in linear models and will introduce probabilistic choice models here.

Stated preference methods have been used in the fields of economics, marketing, transportation, and even medicine. These fields see the benefits of understanding the needs and wants of their customers, and stated preference methods provide a technique for easily assessing the part-worths of consumer's values.

There are many different models of choice behavior, all of which share three central components:

- Objects of choice (e.g., computers)

- Sets of attributes (e.g., monitor size, hard drive size, memory, etc.)
- A model for understanding the individual choices and behavior patterns for the population

There are a plethora of different responses to choose from, including but not limited to:

1. Expressing Degrees of preference by rating options on a scale
2. Completely ranking from most to least preferred
3. Choosing either "Yes, I like this option" or "No, I do not like this option"
4. Choosing one option from a set of competing ones

The response is selected for the project based upon the desired results from the study.

2.1 Terminology and Notation

The terminology and notation of stated preference techniques is central to understanding the methods and models of individual choice behavior. Choice models are designed to understand the utility consumers have for a service or good. This utility is identified by having consumers evaluate sets of alternatives, called profiles or choice sets, for the relative preference for each alternative. Each alternative is comprised of several attributes, the components that comprise the product or service being evaluated. A series of consumers evaluate sets of profiles and this information is used to determine their utility for the product or service. An individual's utility is decomposed into two components, the systematic and random components as follows:

$$U_{iq} = V_{iq} + \in_{iq}$$

where U_{iq} is the true, unknown utility of the i^{th} alternative for the q^{th} individual, V_{iq} is the systematic component or representative utility of the i^{th} alternative for the q^{th} individual and $\in_{iq}$ is the unobserved individual idiosyncrasies or tastes of the q^{th} individual. The systematic component of the utility, V_{iq}, is decomposed into the sum of the attributes times their weight. It is assumed that the V_{iq} are homogeneous across the entire population or the segment under consideration. Parameter estimation for an individual's or segments utility is completed using maximum likelihood techniques. An individual will select alternative i over alternative j if $U_{iq} > U_{jq}$. The $\in_{iq}$ are assumed to be independent and identically distributed with a distribution that depends on the choice model selected for the particular study.

Basic choice experiments are estimated using logit or probit models. The models are validated by checking the assumptions discussed above, and are evaluated using overall goodness of fit tests and likelihood ratio tests. We shall derive the basics of the Multinomial Logit Model in the next section. The multinomial logit model is the workhorse of probabilistic choice models, although it is often too simplistic and restrictive in its assumptions.

2.2 Derivation and Assumptions of the Multinomial Logit Model

Probabilistic choice models originated in Psychometrics with Thurstone's (1927) work. His random utility model became the basis for the economic theory underlying discrete choice and stated preference models. These models begin with the assumption that each consumer chooses the alternative with the greatest utility.

As discussed above, it is assumed that each consumers true utility can be decomposed into a systematic component V_{iq} and random error $\in_{iq}$:

$$U_{iq} = V_{iq} + \in_{iq}$$

An assumption is also made that an individual selects the alternative with the greatest utility, known as utility maximization. The probability of selecting alternative i from choice set with J alternatives is given as:

$$P_i = P(U_i \geq U_j, j = 1,...J) \text{ or}$$

$$P_i = P(\varepsilon_j \leq V_i - V_j + \varepsilon_i, j = 1,...J)$$

Without loss of generality, if we assume that the an individual always chooses the first alternative then the choice probability above can be specified as:

$$P_1 = P(\varepsilon_2 \leq V_1 - V_2 + \varepsilon_1, \varepsilon_3 \leq V_1 - V_3 + \varepsilon_1, ..., \varepsilon_J \leq V_1 - V_J + \varepsilon_1).$$

Therefore the probability of selecting alternative 1 will be:

$$P_1 = \int_{-\infty}^{\infty} \int_{-\infty}^{V_1 - V_2 + \varepsilon_1} \cdots \int_{-\infty}^{V_1 - V_J + \varepsilon_1} f(\varepsilon_1, \varepsilon_2, ...\varepsilon_J) d\varepsilon_J d\varepsilon_{J-1} ... d\varepsilon_1$$

Any probabilistic model we select will have the same derivation of the choice probabilities. The models differ only in their specification of the error distribution. A traditional statistical assumption that the errors follow a multivariate normal distribution would result in the multinomial probit (MNP) model, a well-known choice model with no closed form. Selection of the error distribution as a Gumbel or extreme-value distribution will result in a closed form representation of the probabilities. This is the well known multinomial logit (MNL) model (McFadden, 1974). The Gumbel distribution resembles the normal distribution except that it is slightly positively skewed. Its probability density function is:

$$f(x) = \frac{1}{\beta} e^{-\frac{x-\mu}{\beta}} e^{-e^{-\frac{x-\mu}{\beta}}}$$

Since it is assumed that the errors are independently distributed, the joint probability density function can be written as the product of the J univariate density functions. Thus:

$$
\begin{aligned}
P_1 &= \int_{-\infty}^{\infty} \int_{-\infty}^{V_1-V_2+\varepsilon\ *} \cdots \int_{-\infty}^{V_1-V_J+\varepsilon\ *} f(\varepsilon_1,\varepsilon_2,...\varepsilon_J)d\varepsilon_J d\varepsilon_{J-1}...d\varepsilon_1 \\
&= \int_{-\infty}^{\infty} f(\varepsilon_1) \int_{-\infty}^{V_1-V_2+\varepsilon\ *} f(\varepsilon_2)... \int_{-\infty}^{V_1-V_J+\varepsilon\ *} f(\varepsilon_J) d\varepsilon_J d\varepsilon_{J-1}...d\varepsilon_1 \\
&= \int_{-\infty}^{\infty} f(\varepsilon_1) \prod_{j=2}^{J} F(V_1 - V_2 + \varepsilon_1) d\varepsilon_1
\end{aligned}
$$

If we use the representation of the standard Gumbel distribution, we see the following:

$$
\begin{aligned}
P_1 &= \int_{-\infty}^{\infty} e^{-\varepsilon_1} e^{-e^{-\varepsilon_1}} \prod_{j=2}^{J} e^{-e^{(-V_1+V_2-\varepsilon_1)}} d\varepsilon_1 \\
&= \int_{-\infty}^{\infty} e^{-\varepsilon_1} e^{(-e^{-\varepsilon_1 \sum_{j=1}^{J} e^{V_j - V_1}})} d\varepsilon_1
\end{aligned}
$$

Letting $a = \sum_{j=1}^{J} e^{(V_j - V_1)}$ and $z = e^{-\varepsilon_1}$ we see that this equation simplifies to:

$$
P_1 = \int_0^{\infty} -ze^{-az}\frac{1}{z}dz = \int_0^{\infty} e^{-az}dz.
$$

We can evaluate the integral as follows:

$$
P_1 = \int_0^{\infty} e^{-az}dz = -\frac{1}{a}e^{-az}\,\Big|_0^{\infty} = 0 - (-\frac{1}{a}) = \frac{1}{a} = \frac{1}{\sum_{j=1}^{J} e^{(V_j - V_1)}} = \frac{e^{V_1}}{\sum_{j=1}^{J} e^{V_j}}.
$$

This is formulation of the standard MNL model.

2.3 Properties of the Logit Model

The logit model formulation depends on the Independence from Irrelevant Alternatives Axiom (IIA). Suspected violation of this property can require the selection of a different choice model for the analysis.

The IIA axiom assumes that the ratio of the probabilities of choosing one alternative over another (given that both alternatives have a non-zero probability of choice) is unaffected by the presence of any additional alternatives in the choice set. Although this is a fundamental assumption in the early work of stated preference modeling, it is an often unrealistic and highly improbable assumption in real applications. The original derivation of the logit model by Luce (1959) depends on this assumption.

For example, consider the choice presented in Figure 1. A consumer is asked which form of transportation he would take to work given the option. We see that the available options are walking, taking a bus, and driving his or her own car. From this image we can determine the individual's preferences for transportation to work. The Independence from Irrelevant Alternatives Axiom says that if a alternative is entered into this scenario that the ratios of preference between the existing three choice objects will remain constant. Assume that the determined choice probabilities are 50% for the option of driving ones own car, 25% for walking and 25% for taking the bus. Now consider this second choice option seen in Figure 2. IIA tells us that our choice participant should still prefer the car to the white bus in the same fashion as before. In this scenario the white bus and the red bus are identical except for the color of the bus. This clearly

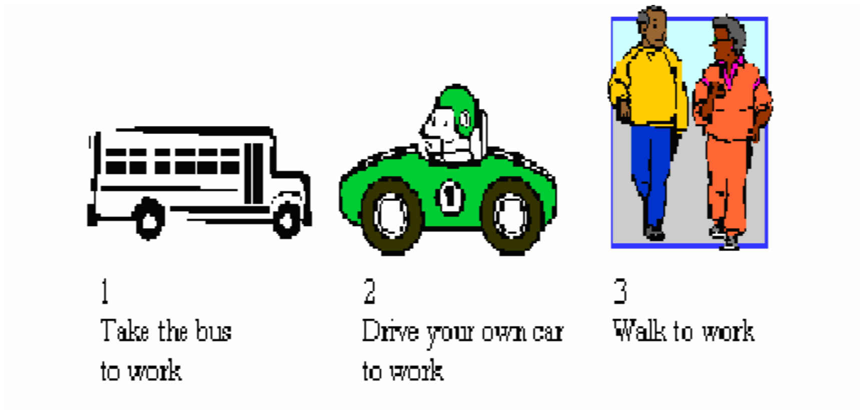


Figure 1 - A Sample Choice Set

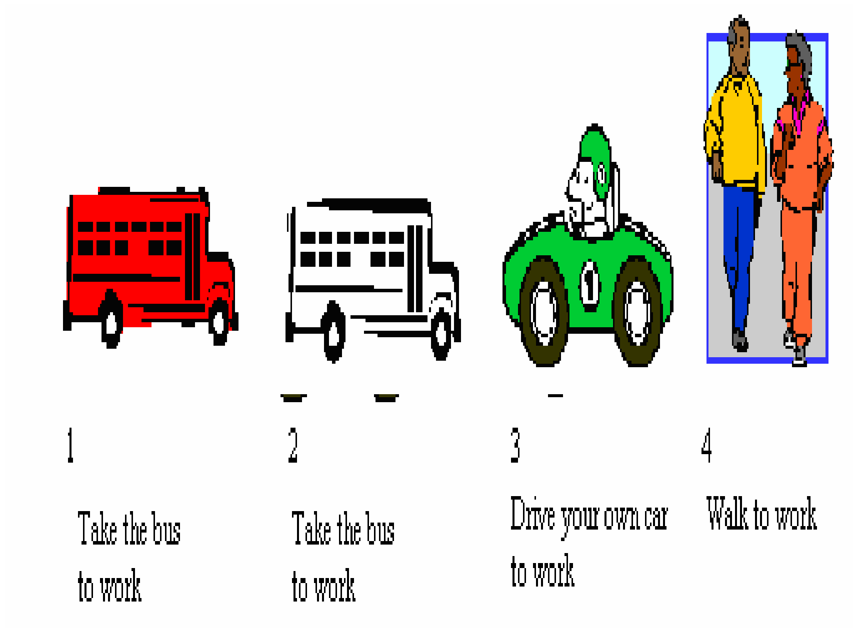


Figure 2 - A Sample Choice Set with a Fourth Alternative

exhibits a violation of the IIA axiom, we would expect that the ratio P(car) / P(white bus) to increase since the red bus option will clearly impact P(white bus) more than it impacts P(car).

Hausman and McFadden (1984) propose a test that compares the logit model with the more general nested logit model to determine if IIA is violated. McFadden (1987) proposes a regression-based specification test for the logit model to evaluate the IIA assumption.

2.4 Choice Models Not Subject to the IIA Assumption

One of the most widely discussed and often problematic properties of the simple logit model is the IIA property. Several suggestions have been made by researchers of models that are robust to this property and allow a richer pattern of alternative substitution.

2.4.1 The Tversky Model (EBA)

Tversky (1972) proposed a probabilistic choice model in which the decision rule is stochastic while the utility is deterministic. He shows that the EBA model is not subject to the IIA property and is consistent with the theory of random utility maximization. These advantages are offset by several weaknesses. First, the attributes are assumed to be binary which may be unrealistic in describing the alternatives. Second, the number of parameters that must be estimated increases exponentially with the number of choice sets in the experiment. This can result in the estimation of the model becoming computationally infeasible, requiring the use of heuristic choice techniques.

2.4.2 Generalizations of the Logit Model

The multinomial logit model discussed earlier has enjoyed the most widespread application. Several extensions of the multinomial logit model have evolved including the nested logit model, the generalized extreme value (GEV) model and the universal or mother logit model.

The nested logit model was proposed by McFadden (1978) and has the advantage of avoding the IIA problem by viewing choice as a hierarchical decision process. The decision process is segmented a priori into a tree-like structure. Estimation of the nested logit model requires sequential applications of the logit model for each branch of the decision making process.

The GEV model was also proposed by McFadden (1978) as a more general discrete choice model. The logit model and the nested logit model are both special cases of the GEV model. The GEV model has been shown to be consistent with the theory of random utility maximization.

The universal or mother logit also avoids the IIA problem. The IIA is avoided through estimation of cross effects. McFadden (1975) proposed this model and noted that it is useful for testing different model specifications but is generally inconsistent with random utility maximization.

2.4.3 The Multinomial Probit Model

The multinomial probit model is the most flexible model for specification of the error structure in its utility functions (Daganazo 1979, Currim 1982). Unlike the logit model, the probit model allows the error components to have different variances and also to be correlated. Another advantage of the probit model is that the covariance matrix can be specified to avoid the IIA problem (Hausman and Wise 1978) . One disadvantage of the probit model is that estimation requires the calculation of an integral in one fewer than the number of choice sets dimensions. Recent computational and simulation advances have improved the feasibility of this integration (McFadden 1989, Pakes and Pollard 1989).

2.5 Summary and Discussion

The choice of which model to employ depends on several different factors. First one must consider the type of response desired from the experiment. If the response is continuous in nature then linear model theory can be employed. If, however, the response is of a choice format then one must use a probabilistic choice model.

Which choice model to use remains controversial. Determination of the appropriate choice model prior to specifying the design of the experiment is highly desirable. If one selects too simple or complex a choice model then the usefulness of the experiment can be significantly impacted.

3. How Consumers Make Choices

3.1 Introduction

Researchers in the field of judgment and decision making (JDM) have studied how both individuals and organizations make decisions. Within the JDM field some researchers have formulated descriptive and mathematical models of different decision making strategies while others have studied the use of compensatory and non-compensatory decision strategies by decision makers as task complexity and context change (Shugan 1980).

3.2 Decision Strategies

The JDM literature has established that individuals utilize many different decision strategies depending on the context of the decision being made. The factors influencing the decision strategies employed include, but are not limited to the product category, format in which the information is presented, time of day, time pressure and alternative similarity (utility balance).

The framework for the use of heuristics by decision makers was first formalized by Shugan (1980) where he demonstrates the theoretical basis for strategy selection as a compromise between making the right decision and reducing the effort needed to make such a decision. Shugan (1980) discusses four strategies employed by decision makers that are intended to save decision making costs by simplifying the choice process. The four strategies are *conjunctive, disjunctive (maximin), minimax and lexicographic*. Using the *conjunctive* process, any product not meeting the minimum cutoff level on any attribute is eliminated from consideration. In the *disjunctive (maximin)* strategy products are compared on their most valued attributes and the product with the highest rating on the best characteristic is chosen. Using the *minimax* strategy the products are evaluated on their weakest attribute and the product with the best levels for the weakest attribute is chosen. The *lexicographic* strategy ranks the attributes in order of importance and then the product that ranks the best on the most important characteristics is chosen. This is an extension of the disjunctive strategy.

Another model of identifying strategy was developed by Russo and Dosher (1983). Their strategy concentrates on observing the strategies employed by decision makers as opposed to modeling their effect on choice behavior. Their studies identify two classes of processing: *holistic* and *dimensional*. In the *holistic strategy,* the alternatives are processed first. In the *dimensional strategy,* the attributes are processed first. In addition, their study identifies two simplification strategies employed by decision makers in the choice process. The first strategy, *dimensional reduction* (DR), occurs when one ignores attributes deemed of small importance in the choice process. The second strategy, *majority conforming decisions* (MCD), occurs when one ignores the magnitudes of differences and gives directional, but equal importance to all attributes. Their results do not see these simplification strategies tied to choice difficulty, which implies that they may be part of the routine decision making process.

It has been shown that the complexity of experimental choice tasks, including the number of attributes, the number of alternatives in the choice set and the similarity of the alternatives in the choice set do not influence the parameter estimates in discrete choice experiments, but instead influences the between subject variance components. This work includes a study addressing the following issues:

- Does task complexity affect decision strategy selection in experimental choice tasks?
- Does cumulative cognitive burden created by multiple choice scenarios done in sequence affect the selection of decision strategy by respondents?

Swait and Adamowicz (2001) attempt to identify the number of latent decision making states experienced by participants over the course of an experiment. They define complexity of the choice process through an information theoretic process. Given a set of alternatives that are described by a probability distribution π(x), the entropy (or uncertainty) of the choice process is defined as:

$$H(\pi_x) = -\sum_j \pi(x_j) \log \pi(x_j) \geq 0$$

Entropy will be minimized when there is one dominant alternative in the choice set and will achieve a maximum if each of the alternatives in the choice set is equally likely. Further, as the number of equally likely alternatives in a choice set increases so does the entropy. This allows the entropy measure to take into account the size of the choice set in the calculation of the difficulty of the choice scenario. In addition to calculating an individual measure of entropy for each alternative, the cumulative entropy of the choice process can be measured. This cumulative entropy measures the total amount of uncertainty faced by an individual through a sequence of choices (we can calculate the sum of the entropy for multiple choice sets due to the fact that entropy is additive) (Taneja 1996).

Measures of choice task uncertainty and cognitive burden can be used to identify the type of decision strategy being employed and the number of different decision strategies seen throughout a sequence of choice tasks. This allows the researcher to evaluate whether the different strategies have any influence on the quality of information obtained in the experiment and the stability of the estimates across different decision strategies is consistent with the selected choice model. Swait and Adamowicz (2001) show that the preference parameters depend on the degree of complexity faced during the choice task. They find that an increase in the complexity of the choice task produces a decrease in the variances of the estimates up to a point, then further increasing entropy results in an increase in the variance of parameter estimates.

4. Designs for Stated Preference Models

Designs for stated preference models originated with practitioners and often with an assumption that designs that are efficient for linear models will also be efficient for choice models. Recently statisticians have begun to work on efficient designs for choice models. We will review designs currently recommended for choice models, starting with traditional design techniques and moving on to efficient choice designs.

4.1 Introduction

Designs for stated preference studies present a research challenge. The traditional and proven design strategies of using balanced full factorial and fractional factorial designs and orthogonal arrays are not necessarily the most efficient designs for non-linear models. Further, each different non-linear model requires a new calculation for efficiency, and these efficiency measures are no longer independent of the true parameter values as is the case with linear design. One must make assumptions about the parameter values to optimize the designs, a difficult, but not impossible prospect in many situations. In most fields, assumptions can be made concerning at least the direction of these effects. Efficient designs have been shown to be fairly robust to misspecification of the parameter values (Huber and Zwerina 1996). However, assuming zero values for the parameters, a common assumption, may be inefficient for the selection of designs.

4.2 Review of Previous Design Strategies

Creating experimental designs for stated preference studies is similar to creating traditional statistical designs, with the added complexity of non-numerical responses, either binary or ordinal. The terminology used in the design of stated preference studies differs from that of traditional statistical design and needs to be understood prior to discussing design techniques. A *factor* is referred to as an *attribute* and *factor levels* are known as *attribute levels*. The *treatment combinations* are known as *alternatives* and *alternatives* are grouped together as *profiles or choice sets*.

Some traditional techniques for statistical design are used in the design of stated preference studies. Commonly used techniques include blocking and randomization and even optimal design.

Green (1974) introduces the use of orthogonal arrays and incomplete block designs for the design of stated preference experiments. He proposes five questions to answer prior to deciding the type of design and model to be used for the particular situation. They are:

1. What type of model does the researcher wish to apply?
    a. Main effects only
    b. Main effects plus selected interaction effects
2. What is the nature of the levels comprising each factor?
    a. Each factor has the same number of levels
    b. Number of levels varies across factors

3. How many factors does the researcher wish to consider in each set of profile presentations?
    a. All factors
    b. A subset of the factors
4. How many choice sets does the researcher wish to present in a single trial?
    a. All choice sets
    b. A subset of the choice sets
5. What type of stated preference model does the researcher wish to employ?
    a. Single-stage estimation model
    b. Multi-stage estimation model

Questions 1 and 2 are concerned with the experimental design of the study. The answers to these questions can lead one to consider fractional factorials, orthogonal arrays, latin square designs and other statistical techniques for the experimental design. Questions 3 and 4 may necessitate the use of balanced incomplete block designs or partially balanced incomplete block designs to reduce the number of profiles presented at one time while retaining balance across the presentations. Question 5 deals with the estimation procedure to be used to determine the utilities of the attributes under study.

Bunch, Louviere and Anderson (1996) present a comparison of existing design strategies for generic-attribute multinomial logit models that classifies designs into two categories, object-based and attribute-based designs. Object-based design strategies create a set of choice-objects using an existing design procedure, i.e. factorial designs, and then assign them to choice sets. Attribute-based design strategies design the entire choice experiment through manipulation of the attribute levels for all choice alternatives.

Before deciding to use an object or alternative based design strategy, there are several decisions, both statistical and non-statistical, that must be made. First, the number of attributes and attribute levels must be identified; second, the correct utility specification must be made; third, the number of alternatives within each choice set must be determined; lastly, the number of choice sets to present to each consumer must be identified. These decisions relate to the market realism and cognitive complexity of the experiment as discussed above. Given these specifications one now considers the efficiency of the design, often searching for an optimal design.

We will now discuss different strategies for designing stated preference experiments. All of the object and alternative based design strategies to be discussed below begin with three initial steps.

1. Determine the number of attributes, K, and index them from k = 1, 2, …, K.
2. Select the number of levels for each attribute
3. Generate a set of M attribute profiles from a fractional factorial design

Once these three steps have been accomplished, one can proceed with selecting the appropriate design strategy.

4.2.1 Object-Based Design Strategies

Continuing with the initial classifications of Bunch, Louviere and Anderson (1996), the following object based design strategies are identified. In step three above, a set of attribute profiles is constructed from a fractional factorial design. Using those attribute profiles, choice sets are constructed in one of the following manners:

1. Random Assignment

Random Assignment of profiles to choice sets

2. All Possible Pairs

    Take the profiles above and construct all possible pairs of these profiles for choice sets.

3. 2^M Block Assignment

    Create the smallest orthogonal main effects fraction of a 2^M factorial and treat each of the M factors as the presence or absence of that attribute profile in a choice set. This design technique generally results in choice sets of varying size.

4. Balanced Incomplete Block Designs (BIBD)

    Assign the M attribute profiles to choice sets using a BIBD (Cochran and Cox 1957, Raghavarao 1971). An advantage of the BIBD assignment is that choice sets are of fixed size and each of the profiles appears together a fixed number of times.

4.2.2 Attribute-Based Design Strategies

In attribute based choice designs one wished to fix the size of the choice set so that there are J choice alternatives per choice set. To accomplish this we can use one of the following procedures:

1. Foldover

    Pair each of the M attribute profiles created in initial step three with its exact foldover. A disadvantage of this strategy is that one can only estimate linear and additive forms.

2. Shifted Designs

Create one or more additional attribute profiles from each of the original attribute profiles by shifting the original designs through modulo arithmetic. Note that when the factors are all two level, this strategy is the same as foldover. Three variants of this strategy are suggested:

a. Shifted Pairs

Add one (mod L), where L is the number of attribute levels, to each attribute in the original J attribute profiles to create paired comparisons.

b. Shifted Triples

Begin with a shifted pairs design, as in *a* above, and construct a third attribute profile by adding one (mod L) to the second attribute profile.

c. Shifted Quadruples

Begin with the shifted triples design, as in *b* above, and construct a fourth attribute profile by adding one (mod L) to the third attribute profile.

3. L^{IK} orthogonal main effects

This design strategy differs from those discussed above as it does not employ the initial three steps for creating the J attribute profiles. Create an orthogonal main effects plan from these M*K columns to create the design plan (J_n is the size of the choice set, M is the number of alternatives and K is the number of attributes). Note that this technique often results in many more choice sets than those discussed above. This design can be used to estimate attribute cross effects. (Louviere 1986, Kuhfeld and Tobias 2005)

4.2.3 Other Design Strategies

The design strategies discussed above are the most common and pervasive design techniques employed currently. They can be used independently, as discussed above, or combined to create hybrid designs. More recently techniques for optimal design for choice experiments have been developed.

Huber and Zwerina (1996) introduce the use of an assumed model in the selection of efficient choice designs. They advocate estimating values for the parameters from 1) pre-tested questionnaires collected before the main experiment is conducted or 2) subject matter expert knowledge. This allows the final experimental design generated to be the most efficient given the knowledge we already have on the attributes. They suggest searching for the most efficient design through the use of relabeling and swapping the attributes and their levels within the design space. This approach assumes that the values of the parameter estimates obtained from the pilot are accurate and that the experimenters have enough initial information to devise a design that is sufficient for the pilot stage of the experiment. They also show the efficient designs are robust to modest misspecification of the parameter values.

Sandor and Wedel (2001) extend the work of Huber and Zwerina (1996) to create Bayesian designs through the use of prior distributions for the parameter values obtained through managers prior beliefs. Similar to Huber and Zwerina (1996) their work is restricted to the main effects MNL model only with qualitative predictors. To obtain the prior beliefs managers are asked to provide a direct visualization of their subjective probability distribution (APD) for each of the attributes in the design. The authors then construct 95% credibility intervals for these estimates and fit a normal prior distribution. The design is then constructed to maximize the efficiency through use of the concepts of relabeling and swapping (Huber, et al. 1996) and cycling (Sándor and Wedel 2001). SAS has a macro, %choiceff, for the construction of optimal choice designs under the MNL model (Kuhfeld 2004).

Moderated choice experiments were introduced by Chzran (2001) as a solution to the problem of pollution by carry-over effects. In certain applications the use of multiple price structure levels for one retailer may pollute the results from round to round of the experiment. Chzran proposes moderated choice experiments as a solution to this problem. In a moderated choice experiment each participant sees only one level for one or more of the factors under consideration. He proposes a two stage design process for moderated choice experiments. First a fraction of a L^k factorial is designed to determine the levels of the fixed factors. Once the fixed factor levels are identified another fraction of a L^k factorial is designed to set the levels of the remaining factors dependent upon the choice of the factor level for the fixed factors. This results in choice sets where each individual sees only one level of the fixed factors to reduce choice to choice pollution by carryover effects.

4.3 Efficiency in Choice Designs

Variance efficiency is a central component to the evaluation of an experimental design. More efficient choice designs result in more precise parameter estimates and the need for less data to achieve adequate precision.

When we refer to *prior estimates* in this work, we are not referring to a prior distribution as used in Bayesian statistics. As used by Huber and Zwerina (1996) and in this dissertation, prior estimates will refer to the assumed values of the parameters in the model used to create an optimal choice design. They are the equivalent of a degenerate Bayesian prior distribution with a single point having probability one.

4.3.1 Measures of Linear Design Efficiency

Efficiencies are measurements of design goodness. Measures of design efficiencies are based on the information matrix. For linear responses the covariance matrix for the least squares estimators is proportional to the inverse of the information matrix, I = X'X where X is the model matrix. The more efficient the design the "smaller" the covariance matrix will be, i.e. the estimates of the parameters will be more precise. There are several different measurements of design efficiency including A-efficiency and D-efficiency. In some applications the error of the design, the inverse of efficiency, is considered. A-efficiency is a measure of the average variance for the estimators of the model parameters. The formulation of A-error is as follows:

$$A = trace(I^{-1})/(K+1) = trace((X'X)^{-1})/(K+1)$$

where K+1 is the total number of parameters and I is the information matrix. A-efficiency is not the most utilized measure of efficiency for two reasons:

- Relative A-efficiency is not invariant to different recodings of the design matrix.
- A-efficiency is also computationally expensive to update.

D-efficiency overcomes these conflicts and is a related measure based upon the geometric mean of the eigenvalues of the covariance matrix:

$$D = \det(I^{-1})^{\frac{1}{K+1}} = \det((X'X)^{-1})^{\frac{1}{K+1}}$$

D-efficiency is the most commonly used measure of criterion for evaluating designs. There are efficient computational algorithms for updating the determinant of X'X as the design changes and ratios of D-efficiencies are invariant under different codings of the design matrix. The D-efficiency criterion takes into account both the variances and covariance of the parameter estimators in the selection of the "best" design.

4.3.2 Measure of Choice Design Efficiency

Using the work of McFadden (1974), we derive the measure of choice design efficiency for the logit model. Using the logit model the probability of choosing an alternative i from a choice set C_n is given by:

$$P_{in}(X_n, \beta) = \frac{e^{x_{in}\beta}}{\sum_{j=1}^{J_n} e^{x_{jn}\beta}},$$

where x_{jn} is the row vector of K attributes describing alternative i and β is a column vector of weights associated with those K attributes: Let X_n be a J_n x K matrix consisting of the row vectors x_{jn} ε C_n, and let $J_\bullet = \sum_{n=1}^{N} J_n$ be the total number of alternatives in the choice experiment. Then the design matrix for the choice experiment, X, is of size $J_\bullet$ x

K. Let Y be a matrix of choices with elements y_{in}, where y_{in} equals one if alternative i is chosen and zero otherwise. Then the log-likelihood of a sample Y is:

$$L(Y \mid X, \beta) = \sum_{n=1}^{N} \sum_{j=1}^{J_n} y_{jn} \ln(P_{jn}(X_n, \beta)) + \text{constant.}$$

By maximizing the likelihood equation above we can obtain the maximum likelihood estimator, $\hat{\beta}$, of the choice model. McFadden (1996) shows that $\hat{\beta}$ is asymptotically normal with mean β and covariance matrix:

$$\Sigma_p = (Z'PZ)^{-1} = [\sum_{n=1}^{N} \sum_{j-1}^{J} z'_{jn} P_{jn} z_{jn}]^{-1} ,$$

where P is an J$_\bullet$ x J$_\bullet$ diagonal matrix with elements P_{jn}, and Z is a J$_\bullet$ x K matrix with rows:

$$z_{jn} = x_{jn} - \sum_{i=1}^{J_n} x_{in} P_{in} \ .$$

When one assumes that the β's are zero the variance covariance matrix has a much simpler form:

$$\Sigma_0 = (Z'PZ)^{-1} = [\sum_{n=1}^{N} \frac{1}{J_n} \sum_{j-1}^{J} z'_{jn} z_{jn}]^{-1}$$

where:

$$z_{jn} = x_{jn} - \bar{x}_n, \text{ with } \bar{x}_n = \frac{1}{J_n} \sum_{i=1}^{J_n} x_{in} \ .$$

For a Fisher Information matrix, I, several established summary measures of efficiency are useful in comparing designs. D-efficiency is the most popular summary measure. D-efficiency is calculated as

$$D - efficiency = \det(I^{-1})^{\frac{1}{K}} = \det(Z'PZ)^{\frac{1}{K}}.$$

The larger the D-efficiency, the more efficient the choice design. When no prior information is assumed to be known about the parameter estimates β, the D-efficiency will be calculated using Σ_0 and will be referred to as D_0-efficiency. When we assume that β is non-zero the D-efficiency will be calculated using Σ_p and will be referred to as D_p-efficiency.

4.3.3 Principles of Efficient Choice Design

Huber and Zwerina (1996) discuss four principles that impact the efficiency of choice designs: level balance, orthogonality, minimal level overlap and utility balance. Level balance and orthogonality are important concepts retained from linear design theory, whereas minimal level overlap and utility balanced are measures of the within choice set structure of the design. Satisfying all four of these criteria simultaneously is the most desirable condition, but near impossible to achieve for most practical applications.

Level balance, also referred to as balance (Payne 1988, Ball 1997) is the requirement that all attribute levels occurs with equal frequency. For example, with a four level attribute, each level should occur in exactly one-fourth of the alternatives.

Orthogonality of main effects occurs when the joint occurrence of any two levels of different attributes appear in profiles with relative frequencies equal to the product of their marginal relative frequencies (Addelman 1962). In many practical applications level balance and orthogonality conflict and an improvement in one results in a degradation of the other.

Minimal level overlap is important in the structure of individual choice sets. Contrasts between attribute levels are only meaningful as differences within the choice set. Minimal level overlap means that the probability that an attribute level repeats itself in a choice set is as small as possible. The most extreme violation of minimal level overlap would occur when one factor has the same level across all alternatives within the choice set. Choices from this set provide no information about the participant's preferences for that particular factor.

Utility balance is achieved when the utilities of alternatives within choice sets are as close as possible. Achievement of utility balance requires some knowledge of the values of the parameters before construction of the design. Utility balance, along with the other three efficiency requirements is normally achieved and balanced through the maximization of D_p efficiency.

4.4 Summary of Characteristics of Existing Design Strategies

We consider the example of creating a discrete choice design for the consideration of three qualitative attributes ideally to be placed into nine choice sets of size 3. For each of the design strategies discussed above we will construct such a design and evaluate its efficiency using the measure of D_p efficiency defined previously. The summary results are presented in the Table 1, and a detailed evaluation of the designs constructed can be found in Appendix One.

We see that of our existing design strategies that the D-efficiency varies greatly according to strategy. The BIBD provides lowest D_p-efficiency under the equal-spaced prior assumption of [-1 0 -1 0 -1 0]. The best design under a zero prior is the design

Table 1 - Comparison of Existing Design Techniques

Design Strategy	Orthogonal	Balanced	Minimum Overlap	Utility Balance	Number of Choice Sets	D_0-Efficiency	D_p-Efficiency	Adjusted for # Choice Sets D_0-Efficiency	Adjusted for # Choice Sets D_p-Efficiency
Random Assignment 1	No	No	No	No	9	3.612717	2.331546	0.401413	0.259061
Random Assignment 2	No	No	No	No	9	3.721623	2.252252	0.413514	0.25025
Shifting	Yes	Yes	Yes	No	9	5.194805	2.651816	0.577201	0.294646
All Triples	No	No	No	No	84	36.36364	21.97802	0.4329	0.261643
Foldover	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A
BIBD	No	No	No	No	12	2.946376	5.194805	0.245531	0.4329
Orthogonal Main Effects	Yes	Yes	Yes	No	9	5.194805	2.651816	0.577201	0.294646

generated by shifting or the orthogonal main effects plan (these techniques result in identical designs). None of the designs remains desirable on all of the other characteristics of orthogonality, balance, minimum overlap and utility balance.

4.5 Criteria for Evaluating Choice Design Complexity

Measuring the complexity of a choice experiment allows one to evaluate differences between potential designs beyond their relative efficiencies. There have been many suggestions and implementations in the literature for measuring the complexity of an experiment (Swait, et al. 2001, DeSharzo and Fermo 2002). In this section we will introduce the different measures of complexity and discuss their uses and expected effects.

4.5.1 Entropy

Swait and Adamowicz (2001) discuss the use of entropy for measuring the complexity of choice designs. In their work they find that as the entropy of an individual choice and the cumulative entropy of the choice task increase the stability of the parameter estimates for the choice task decreases. The effect of this instability can be controlled through a modeling technique as described by Swait and Adamowicz (2001) or we can proactively control the effect by managing the entropy within a choice task (often measured as percent of maximum entropy for the particular choice task).

Entropy is a good measure of complexity as it increases as the number of choice sets and the number of alternatives increases. Figure 3 shows the relationship between the number of alternatives in the choice set (for a fixed number of attributes and choice sets)

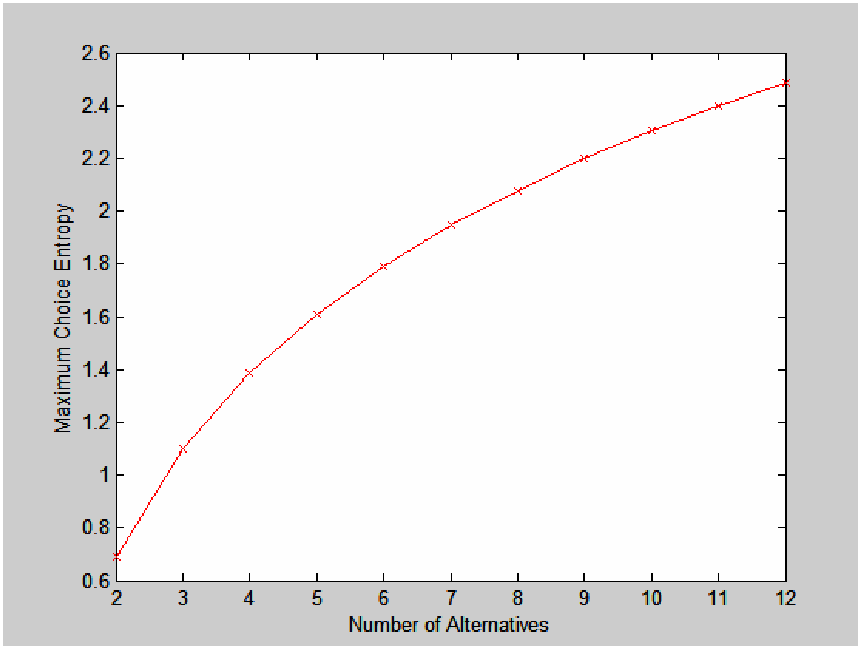


Figure 3 - Entropy Versus the Number of Alternatives

and the maximum entropy for an individual choice. We see that the maximum choice entropy increases logarithmically as the number of alternatives in the choice set increases. Figure 4 shows the relationship between the number of choice sets in the design and the maximum and minimum choice entropy for the task. (We assume there are 4 alternatives per choice set in this particular example and a fixed number of attributes.) The red line indicates the constant minimum entropy while the blue line indicates the linear relationship between the number of choice sets and the cumulative entropy of the experiment.

Entropy is not capable of measuring all complexity that is inherent in a choice experiment. Consider the two choice sets in Table 2. Choice set one has two alternatives and three attributes whereas choice set two has two alternatives and six attributes. Both of these choice sets have entropy of 0.6931, even though in choice set two one has to evaluate twice the number of attributes to make a choice. This is one clear deficiency of using entropy as the sole measure of complexity in a choice experiment.

4.5.2 Number of Tradeoffs

The number of tradeoffs in a choice set is another suggested measure of complexity for choice sets. The number of tradeoffs in the choice set solves one of the defficiencies of entropy as it increases according to the number attributes in the choice set. The potential number of tradeoffs also increases as the number of alternatives in the choice set increases, and it is limited by the number of levels of each attribute. If there are more alternatives than levels in an attribute then the potential number of tradeoffs will be smaller than if there are more levels than alternatives in an attribute.

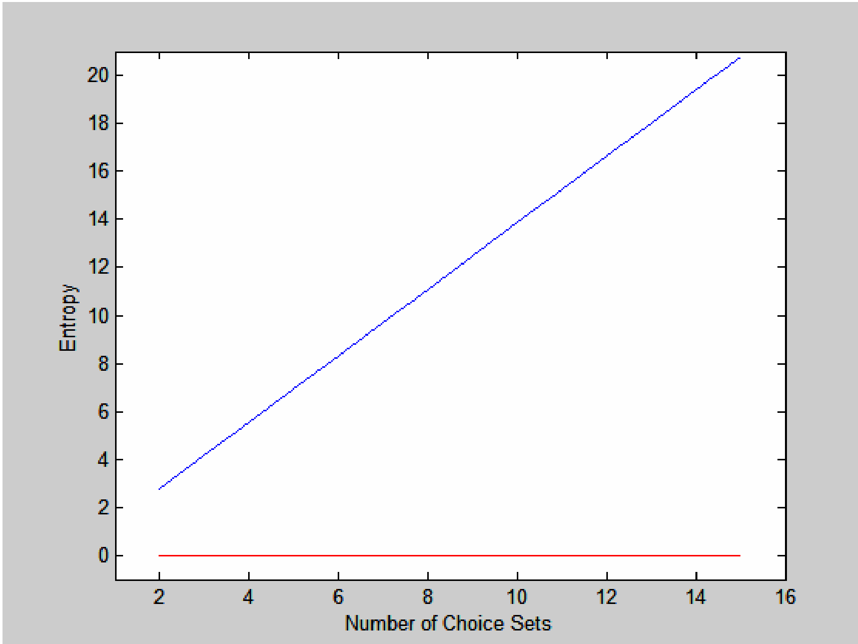


Figure 4 - Entropy Versus the Number of Choice Sets

Table 2 - Two Sample Choice Sets A

	Choice Set One			Choice Set Two					
	Att One	Att Two	Att Three	Att One	Att Two	Att Three	Att Four	Att Five	Att Six
Alternative One	1	2	3	1	2	3	1	2	3
Alternative Two	3	2	1	3	2	1	3	2	1

Although counting the number of tradeoffs addresses a deficiency of entropy (not being able to distinguish between the numbers of attributes in a choice set) it is not by itself a perfect measure of complexity in a choice set. Consider the two choice sets in Table 3. Let us assume the equal-spaced prior for β, i.e. an equally spaced linear relationship between the levels of each attribute. Obviously in choice set one, alternative two is preferable to alternative one. In choice set two, assuming that all attributes are equally preferable, alternative two is preferable to alternative one. The choice in choice set one is much easier than the choice in choice set two because it is a dominant alternative, i.e. preferable in every attribute. The number of tradeoffs in choice set one and two is three. Therefore, the number of tradeoffs is by itself an incomplete measure of choice set complexity.

Additionally the number of tradeoffs considers tradeoffs between all levels to be equally complex. Consider a six level attribute where level six is the best and level one is the worst, a tradeoff between level one and level two is considered to be the same as a tradeoff between level one and level six by counting only the number of tradeoffs. Obviously, the choice between level one and level six is much easier than the choice between level one and level two (This is something that entropy is capable of measuring).

4.5.3 Magnitude of Tradeoffs

Similar to the number of tradeoffs, the magnitude of tradeoffs is able to capture differences due to the number of attributes between different designs, something entropy is unable to accomplish. However, unlike the number of tradeoffs the magnitude of

Table 3 - Two Sample Choice Sets B

	Choice Set One			Choice Set Two		
	Att One	Att Two	Att Three	Att One	Att Two	Att Three
Alternative Two	1	1	1	1	2	1
Alternative Two	2	2	2	2	1	2

tradeoffs is able to distinguish between tradeoffs that are close together and tradeoffs that are far apart.

The magnitude of tradeoffs is still a deficient measure of complexity in that it measures the magnitude of the tradeoff, but cannot distinguish if one alternative in the choice set is superior to the others, it merely measures the magnitude of the tradeoffs without regard to where the tradeoffs come from.

4.5.4 Number of Attributes

DeSharzo and Fermo (2002) discuss several methods for measuring complexity in choice sets of stated preference experiments. In their study they show that as the number of attributes in a choice set increases the variance of the error component of utility also increases. This may be attributed to the greater cognitive burden, or changing decision strategies over the course of the experiment. This shows that increasing the realism of the experiment by increasing the number of attributes can actually be detrimental to the precision of the results.

4.5.5 Mean Standard Deviation of Attribute Levels within Each Alternative

In addition to the number of attributes, DeSharzo and Fermo (2002) also discuss the mean standard deviation of attribute levels within each alternative as a measure of choice set complexity. The standard deviation among the normalized attribute levels of alternative j in choice set n is defined as:

$$SD_{j_n} = \sqrt{\left(\left[\sum_{i=1}^{K}(x_{ij} - \bar{x}_j)^2\right] / K\right)}$$

where x_{ij} is the normalized i^{th} attribute of alternative j, and K is the total number of attributes in alternative j. When all of the attributes in an alternative are equally preferable, whether highly preferable or highly undesirable, the measure, SD_{jn}, will be small. To create a measure for the choice set as a whole, we calculate Average SD_n, defined as:

$$AverageSD_n = \left(\sum_{j=1}^{J_n} SD_{jn} \right) / J_n$$

where J_n is the total number of alternative in choice set n. They show that a higher value of Average SD_n increases the variance of the utility. Therefore, alternatives in choice sets that vary significantly in the normalized levels of attributes lead to more complex decisions.

4.5.6 Dispersion of the SD of Attribute Levels within Each Alternative

DeSharzo and Fermo (2002) also discuss the dispersion of the standard deviation of attribute levels within each alternative as a measure of choice set complexity. The dispersion of SD_{jn} is the standard deviation across alternatives of the SD_{jn} measure for each alternative in the choice set. Dispersion SD_n is defined as below:

$$DispersionSD_n = \sqrt{\left(\left[\sum_{j=1}^{J_n} (SD_{jn} - AverageSD_n)^2 \right] / J_n \right)}$$

where J_n is the total number of alternatives in choice set n. They show that an increase in Dispersion SD_n will lead to an increase in the variance of the utility.

4.5.7 Mean Standard Deviation of Attribute Levels within Each Attribute

DeSharzo and Fermo (2002) give us tools to evaluate attribute levels within an alternative. We also consider variability within an attribute as a measure of choice complexity. If the levels of an attribute across alternatives are similar (but not identical) then there is greater cognitive burden within that attribute for the decision maker. If, however, there is high variability within an attribute across alternatives then the participant may be able to spend less time considering tradeoffs. We define the standard deviation of attribute k in choice set n as:

$$SD_k = \sqrt{\left(\left[\sum_{j=1}^{J_n} (x_{mj} - \bar{x}_m)^2\right] / J_n\right)}$$

where J_n is the number of alternatives in the choice set. This measure is attribute specific, so to create a measure for the choice set as a whole, we calculate Average SD, defined as:

$$AverageSD_K = \left(\sum_{k=1}^{K} SD_k\right) / K$$

where K is the number of attributes in the choice set. We hypothesize that Average SD_M will present itself as a quadratic effect. When Average SD_k is zero the choice will be very easy and when Average SD_K achieves its maximum the choice will also be very easy, however between these two extremes complexity will increase and then decrease.

4.5.8 Dispersion of the SD of Attribute Levels within Each Alternative

The dispersion of the amount of variability between the attribute levels for the choice set may also impact the complexity of the choice task. We define the dispersion of attribute levels as:

$$DispersionSD_K = \sqrt{\left(\left[\sum_{k=1}^{K}(SD_k - AverageSD_K)^2\right] / K\right)}$$

We believe that as dispersion SD_K decreases, decisions will become more complex.

4.6 Special Considerations for Stated Preference Designs

The simplest stated preference study is what is known as a discrete choice study. In discrete choice studies, individuals are presented with a profile containing several alternatives and asked to select the one they would purchase given the opportunity. These simple tasks provide information concerning consumer's utility for the products under study. For this binary response situation and other more complex stated preference response techniques, there are a unique set of design considerations that need to be examined.

The first consideration that must be made is identifiability, i.e. the form of the utility function that will be estimated from a given experiment. All stated preference designs require a functional form including at least the main effects for each attribute under study. Many studies also seek to identify more complex models, i.e. second order or higher interactions, and designs must be capable of estimating those effects. The effects of interest may need to be evaluated as additive or multiplicative effects (Louviere, Hensher and Swait 2000a).

Precision is another key consideration in the design of stated preference models. The precision of the estimates relates to the width of the confidence intervals for the parameters of interest. The narrower the confidence intervals are, the more precise the estimates for the parameters of interest. We always seek the maximum precision we can achieve within the financial and time constraints of the experiment (Louviere, et al. 2000a). We have shown that there are several measures of design complexity that are known to impact the precision of these estimates (the magnitude of Var(ε)) (Swait and Adamowicz 1997, DeSharzo, et al. 2002).

In addition to considering the statistical attributes during the design phase it is also necessary to consider the degree of cognitive complexity that the experiment presents to the participants. The degree of complexity is determined in part by the number of alternatives in each profile and the number of profiles that are presented to each participant. There is considerable disagreement on the optimal level of complexity and the effect that the complexity of the experiment has on the validity of the results obtained. Many advocate presenting only a small number of choice sets to each participant, e.g. up to eight (Carson, et al. 1994), while others present evidence that participants can evaluate up to sixty choice sets without degradation of the results (Louviere, Hensher and Swait 2000b). Another issue that impacts the complexity of the task is the number of alternatives per choice set. Typical examples show three to six alternatives per profile. Again, the literature presents a conflicting view of the optimal number of alternatives per profile. (Carson, et al. 1994, Louviere, et al. 2000a)

Another important issue in the design of any stated preference study is the issue of market realism. Market realism is the degree to which the experiment and associated tasks match the actual decision environment that a consumer faces in the course of their normal activities. Situations that do not match market conditions may lead participants to believe the experiment is not serious, or to have unrealistic expectations about what they may see from a given market or company in the future. One must also consider the carry over effects present from choice set to choice set in the decision process. Carryover can often result in confusion of the consumer and further detract from market realism. (Haaijer, et al. 2001)

4.6.1 The Base Alternative

In choice experiments, a base alternative is often included to scale the utilities from choice set to choice set (Dhar 1997). A base alternative can be a profile that is held constant over each choice set, it can be the option to choose "your current brand", and including this alternative has the advantage of making the choice decision more realistic to the decision maker. However, it also provides an opportunity to avoid making difficult tradeoffs by selecting the well defined and easier own alternative.

Specific evaluation of the impact of the base alternative in the context of a stated preference experiment is not seen in the literature at this time. Macros exist within standard programs such as SAS® for the construction of designs with a constant alternative present in all choice sets.

One of the potential problems associated with the constant alternative is the fact that it is generally presented as the last alternative in each choice set. This violates the statistical practice of randomization, and assumption in the design of efficient choice experiments.

4.6.2 The No-Choice Alternative

Dhar reviews the times and reasons participants choose the no-choice alternative. He finds that the option may be selected when none of the presented alternatives appear attractive, or when participants believe that better alternatives may exist if they continue to search. He also finds that, in general, the no-choice option is selected more when the alternatives in the choice set have similar utility (i.e. high entropy) and less frequently when there is a dominant or dominated alternative in the choice set.

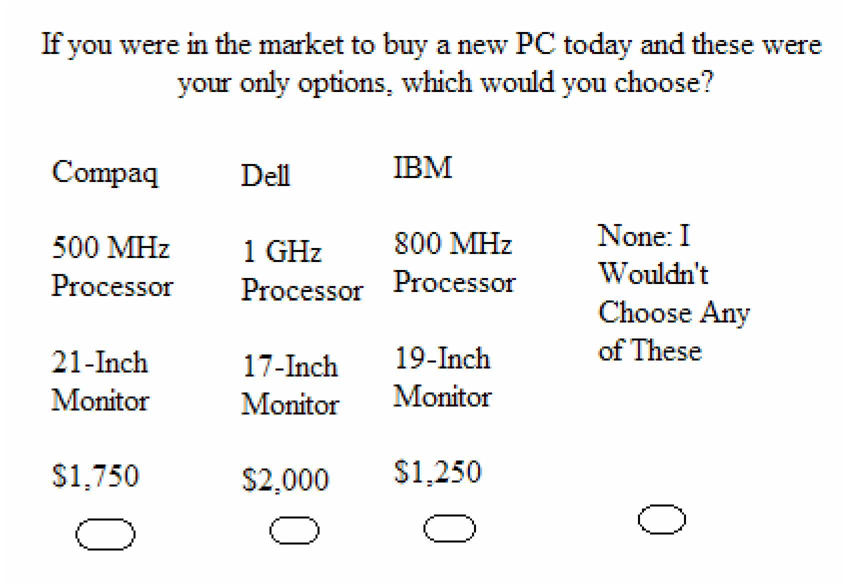
4.6.2.1 Modeling the No-Choice Alternative

Haaijer, Kamakura and Wedel (2001) study the effect of the no-choice alternative from the perspective of modeling. They discuss three models that may be used in the analysis of data containing the no-choice alternative. First they consider the standard multinomial logit model (MNL) in two ways. They use a series of zeros to describe the attribute values of the no-choice options and the standard multinomial logit model formulation. This may lead to biased results as it becomes a fixed part of the utility. Additioanlly, they consider the use of effects type coding, again with a multinomial logit model formulation. This resolves the issue of bias as all part-worth's are now specified relative to the no-choice option. The second model considered is the nested logit (NL). This model specifies two nests, one with the no-choice alternative and the other with the real profiles. This formulation assumes that the consumer first chooses whether or not to purchase a product and then which product to choose. The last model considered if the no-choice multinomial logit model (NCMNL). The difference between the standard MNL and the NCMNL is the addition of an extra constant (c_{nc}), but both models retain the context of the standard MNL. The NL model has one extra parameter, λ, called the dissimilarity coefficient. When $\lambda = 1$, the MNL and NL are equal.

In two separate applications they found that the no-choice multinomial logit model provided the best fit to the data. This evaluation was made using the log-likelihood value, AIC and BIC. Additionally they identify two separate situations in which consumers choose the no-choice option. One motivation for "no-choice" arises when a consumer has little interest in the product category under research. An alternative motivation prompting consumers to choose the no-choice option is when none of the alternatives within the choice set are attractive or when they are all equally attractive and the decision is too difficult.

4.6.2.2 Presentation of the No-Choice Alternative

Similar to the presentation of the base alternative, in general the no-choice alternative is presented at the end of a choice set. Figure 5 shows the typical presentation of this alternative in a choice set. This constant presentation of the no-choice alternative is counter to the statistical property of randomization. However in the no-choice situation the constant presentation of this specific alternative may be logical given that it is much different from the other alternatives presented and easily identified as the only constant alternative in the choice set. Since the no-choice alternative is defined as "None of the other alternatives listed," it is illogical to place it anywhere but last.



If you were in the market to buy a new PC today and these were your only options, which would you choose?

Compaq	Dell	IBM	
500 MHz Processor	1 GHz Processor	800 MHz Processor	None: I Wouldn't Choose Any of These
21-Inch Monitor	17-Inch Monitor	19-Inch Monitor	
$1,750	$2,000	$1,250	

Figure 5 - The No-Choice Alternative Presentation

4.7 Summary and Discussions

Existing choice design methodologies provide a definitive starting point for the development of new choice design structures. Once new designs are identified they must be evaluated for efficiency.

We see that there are several established measures of choice set complexity already in the stated preference literature. These measures have been shown to have impact on the amount of variability in the random component of utility. In addition, we propose two additional measures that may be capable of accounting for complexity in the choice process.

The ability to understand and model complexity in the choice process as Swait and Adamowicz (2001) and DeSharzo and Fermo (2002) do is important to improve the precision of our estimates. Improving the precision of the estimates improves the predictions from a choice experiment. However, as DeSharzo and Fermo (2002) suggest we may also use our knowledge of complexity in the choice process to take care in designing our choice experiment. If we can control the levels of these measures in our choice design then we may eliminate the necessity of modeling the variability caused by these effects in the analysis stage and so work with a much simpler model.

5. Optimal Design Strategies for Choice Models

Optimal design has become one of the preferred methods for constructing designs for choice experiments. In addition to the guarantee that the design will be near optimal in terms of variance characteristics, the prevalence of automated algorithms for their construction makes the experimental design process easy to complete. Recent work has focused on the use of optimal designs for choice models such as the multinomial logit, multinomial probit and mixed logit (Carson, et al. 1994, Kuhfeld and Tobias 1994, Lazari and Anderson 1994, Huber, et al. 1996, Kessels, Goos and Vanderbroek 2005, Kuhfeld, et al. 2005). Given the large potential costs associated with running choice experiments, using more efficient designs allows for less data collection to achieve adequate precision.

The use of efficient experimental designs for choice experimentation requires consideration of several different issues. These issues include:

- The optimality criterion to employ (D-optimal, A-Optimal, G-Optimal, etc.)
- The search algorithm to employ (A Federov type algorithm, a coordinate-exchange algorithm, etc.)
- The formulation of the variance-covariance matrix of the design

The next sections will review the concerns associated with each of the preceding points.

5.1 Selecting an Optimality Criterion

Section 4.3.1 presents a summary of the optimality criteria that are employed for selecting choice designs. Further study of the differences between these criteria and recommendations for their use can be found in Kessels, Goos et al. (2005). We will use

D-efficiency in our evaluation of choice design algorithms as it is the most readily available in the software for both researchers and practitioners.

5.2 The Selection of the Search Algorithm

The selection of a search algorithm is often based on the need for speed and the ability of an algorithm to locate the most efficient designs. For further information on the selection of a search algorithm, there are many specific resources to be studied (Kuhfeld, et al. 1994, 2005).

For the practitioner of choice experiments the selection of an algorithm for finding efficient choice designs is often limited by those available in the software. For our purposes we will use the algorithms available from SAS® for efficient design creation and choice modeling, including Proc Optex and the %choiceff macro.

5.3 Formulation of the Variance-Covariance (Information) Matrix

The selection of an efficient choice design is impacted by the formulation of the information matrix employed in the selected design algorithm. In the literature there are currently three different formulations considered:

- A linear model information matrix structured in the horizontal, traditional statistical setup (Kuhfeld, et al. 2005) (Technique 1)
- A linear model information matrix structured in the typical vertical choice format (Huber, et al. 1996) (Technique 2)
- An information matrix for the appropriate logit / probit model (vertical in structure) (Huber, et al. 1996) (Technique 3)

The question becomes which of these techniques will result in the best designs. Claims have been made in the literature that the three design construction techniques above are equivalent or result in designs that are sufficiently good. Let us consider the case of creating a design for an experiment intending to use the multinomial logit model for analysis. First, let us consider the differences in the construction of the data frame for the three techniques discussed above. From a traditional statistical perspective one would expect the data frame to be set up as seen in Table 4.This is the data frame that is used for the selection of an efficient design using technique one above. Techniques two and three will create a design using the data frame shown in Table 5. This data frame is the format that will be employed for the analysis of the choice experiment using the multinomial logit model.

The question now becomes are the designs created using these three techniques with the already admitted differences in the data frames equally efficient. We will evaluate these differences using the SAS® macro %mktex for technique 1, Proc Optex for technique 2 and the SAS® macro %choiceff for technique 3.

Formulating the variance-covariance matrix for a logit type model is a more complex process as it requires having prior knowledge concerning the parameters of the resulting model. There are three approaches currently being used to solve this problem:

- Using zero for the parameter values
- Using non-zero parameter values (Huber, et al. 1996)
- Using Bayesian design techniques to incorporate uncertainty about the parameter values, i.e. a probability distribution for each parameter rather than a single value

Table 4 - Technique One Data Frame

Choice Set	Attribute 1 Alternative 1	Attribute 2 Alternative 1	Attribute 1 Alternative 2	Attribute 2 Alternative 2	Attribute 1 Alternative 3	Attribute 2 Alternative 3	Choice (Response)
1							
…							
M							

Table 5 - Technique Two and Three Data Frame

Choice Set	Attribute 1	Attribute 2	Choice
1			0
1			0
1			1
…	…	…	…
M			0
M			1
M			0

Using zero values for the parameters assumes that respondents prefer each alternative in the choice set equally (each alternative contributes equally to the entropy of the choice set). This is likely an unrealistic assumption for most choice studies. Generally at least one knows the order in which the levels of an attribute will be preferred.
This can be taken into account using a very simple assumption on the prior. Several authors have suggested that this prior specification on β will result in equivalent designs to the linear model formulation of technique 2. We will evaluate these claims below.

Huber and Zwerina (1996) suggest using an equal-range prior with equally spaced levels for each attribute. Often it is not difficult to obtain more detailed information about the attribute levels by studying field data or manager's prior beliefs. Although these prior assumptions on the parameters are not exact, Huber and Zwerina (1996) show that some misspecification of the prior does not have a significant impact on the relative efficiency of a set of designs.

Using a Bayesian design algorithm allows one to further quantify the uncertainty faced in the parameters and is a more sophisticated method of creating choice designs. This works well when there is substantial uncertainty faced concerning the parameter estimates (Sandor 2001).

5.3.1 Comparing the Three Design Techniques

We compare the design techniques discussed above with the following scenarios:

- 3^3 in 9 choice sets of size 3 – main effects model
- 3^3 in 27 choice sets of size 3 – main effects model
- $3^2 \cdot 4 \cdot 5$ in 80 choice sets of size 4 – main effects model

- $3^2 \cdot 4^2$ in 80 choice sets of size 4 – main effects model
- 3^3 in 180 choice sets of size 3 – full factorial model
- $3^2 \cdot 4 \cdot 5$ in 80 choice sets of size 4 – main effects and 2 factor interactions
- $3^2 \cdot 4^2$ in 1600 choice sets of size 4 – main effects and 2 factor interactions

For each scenario we work with two different cases: β is zero and an equal-spaced prior assumption for β.

For all cases the designs created using technique one are inferior to those created using techniques two and three. In some cases technique one is not even capable of estimating the required design. When the assumption is that β is zero, technique three is slightly inferior to technique two, these results are seen in Table 6. When an equal-spaced prior assumption is used for β, the designs created using technique three are approximately ten to two hundred percent more efficient than those created using technique two. These results are seen summarized Table 7.

5.3.2 Explaining the Differences Between the Techniques

The simulations above show that technique 1 is clearly inferior to techniques 2 and 3. This difference can be seen merely by examining the structure and size of the variance-covariance matrix for technique one versus that for techniques two and three.

Example: 3^3 in choice sets of size three, 9 choice sets

Consider the designs shown in Tables 8 and 9. These designs have nine choice sets of size three each with three three-level attributes. Table 8 shows the design formulation for technique 1, Table 9 shows the design formulation for techniques 2 and 3. Efficiency measures for technique one are calculated using the information matrix in Table 10, for

Table 6 - Comparison of the Three Techniques with a Zero Prior Assumption

		Technique One	Technique Two	Technique Three
3^3 in choice sets of size three, 27 choice sets – Main effects	**Mean D-Efficiency**	3.4641	5.1666	5.1496
	Standard Deviation of Efficiency	0	0.0091	0.0157
$3^2 \cdot 4^2$ in choice sets of size four, 80 choice sets – Main Effects	**Mean D-Efficiency**	10.224	13.302	13.2611
	Standard Deviation of Efficiency	0.0130	0.0009	0.0062
$3^2 \cdot 4 \cdot 5$ in choice sets of size four, 80 choice sets – Main Effects	**Mean D-Efficiency**	24.5599	31.3224	31.3195
	Standard Deviation of Efficiency	0.0000	0.0689	0.0009
3^3 in choice sets of size three, 180 choice sets – All Interactions	**Mean D-Efficiency**	47.7987	55.7334	55.7025
	Standard Deviation of Efficiency	0.2342	0.0006	0.0030
$3^2 \cdot 4^2$ in choice sets of size four, 160 choice sets – Main Effects and 2 factor interactions	**Mean D-Efficiency**	29.5919	33.8934	33.8165
	Standard Deviation of Efficiency	0.2097	0.0016	0.0120
$3^2 \cdot 4 \cdot 5$ in choice sets of size four, 80 choice sets – Main Effects and 2 factor interactions	**Mean D-Efficiency**	11.6860	13.7977	13.7068
	Standard Deviation of Efficiency	0.1533	0.0037	0.0101

Table 7 - Comparison of the Three Techniques with an Equal-Spaced Prior Assumption

		Technique One	Technique Two	Technique Three
3^3 in choice sets of size three, 27 choice sets – Main Effects	**Mean D-Efficiency**	2.7242	3.8499	4.1309
	Standard Deviation of Efficiency	0.3561	0.5023	0.5332
$3^2 \cdot 4^2$ in choice sets of size four, 80 choice sets – Main Effects	**Mean D-Efficiency**	13.7784	15.8624	27.4915
	Standard Deviation of Efficiency	0.4773	0.5776	0.0911
$3^2 \cdot 4 \cdot 5$ in choice sets of size four, 80 choice sets – Main Effects	**Mean D-Efficiency**	11.6040	12.9402	24.1514
	Standard Deviation of Efficiency	0.6615	0.0590	0.6019
3^3 in choice sets of size three, 180 choice sets – All Interactions	**Mean D-Efficiency**	28.1984	32.7970	45.5153
	Standard Deviation of Efficiency	0.5132	0.5096	0.0044
$3^2 \cdot 4^2$ in choice sets of size four, 160 choice sets – Main Effects and 2 factor interactions	**Mean D-Efficiency**	13.9009	15.4494	29.4055
	Standard Deviation of Efficiency	0.4469	0.4007	0.0283
$3^2 \cdot 4 \cdot 5$ in choice sets of size four, 80 choice sets – Main Effects and 2 factor interactions	**Mean D-Efficiency**	4.5939	5.5937	11.8425
	Standard Deviation of Efficiency	0.1691	0.2877	0.0346

Table 8 - Design Formulation for Technique One

Choice Set	Alternative One			Alternative Two			Alternative Three		
	Attribute 1	Attribute 2	Attribute 3	Attribute 1	Attribute 2	Attribute 3	Attribute 1	Attribute 2	Attribute 3
1	3	3	1	1	1	1	1	1	3
2	3	3	2	2	2	1	3	3	3
3	2	1	3	1	3	1	1	2	3
4	1	3	2	2	1	2	2	2	2
5	2	2	3	2	1	1	3	1	3
6	1	2	1	3	1	1	2	3	2
7	3	1	2	2	3	1	3	2	1
8	1	2	2	2	3	3	3	2	2
9	3	2	3	1	3	3	1	1	2

Table 9 - Design Formulation for Techniques Two and Three

Choice Set	Attribute 1	Attribute 2	Attribute 3
1	3	3	1
1	1	1	1
1	1	1	3
2	3	3	2
2	2	2	1
2	3	3	3
3	2	1	3
3	1	3	1
3	1	2	3
4	1	3	2
4	2	1	2
4	2	2	2
5	2	2	3
5	2	1	1
5	3	1	3
6	1	2	1
6	3	1	1
6	2	3	2
7	3	1	2
7	2	3	1
7	3	2	1
8	1	2	2
8	2	3	3
8	3	2	2
9	3	2	3
9	1	3	3
9	1	1	2

Table 10 - Information Matrix for Technique One

	Att 1a	Att 1b	Att 2a	Att 2b	Att 3a	Att 3b	Att 1a	Att 1b	Att 2a	Att 2b	Att 3a	Att 3b	Att 1a	Att 1b	Att 2a	Att 2b	Att 3a	Att 3b
Att 1a	5	3	0	2	0	-1	-2	-2	2	0	-2	1	1	4	-1	0	1	4
Att 1b	3	5	2	3	-3	-4	0	-1	0	-1	-1	0	1	1	1	1	-1	0
Att 2a	0	2	5	3	-2	-2	0	-1	-4	-3	0	-1	0	-1	0	2	2	0
Att 2b	2	3	3	5	-1	-3	-2	-2	0	-1	0	-1	-1	0	0	-1	1	1
Att 3a	0	-3	-2	-1	4	2	-1	-2	2	1	0	0	1	2	-1	-2	1	2
Att 3b	-1	-4	-2	-3	2	5	-1	2	0	1	0	1	-2	0	-2	0	2	2
Att 1a	-2	0	0	-2	-1	-1	3	1	-1	-1	1	0	2	-1	2	2	-2	-3
Att 1b	-2	-1	-1	-2	-2	2	1	5	0	0	2	1	-3	-3	1	2	-1	-2
Att 2a	2	0	-4	0	2	0	-1	0	6	2	1	1	0	2	1	-2	-2	1
Att 2b	0	-1	-3	-1	1	1	-1	0	2	3	-1	0	-1	0	-1	-3	-1	0
Att 3a	-2	-1	0	0	0	0	1	2	1	-1	6	0	-1	-2	0	0	-3	-3
Att 3b	1	0	-1	-1	0	1	0	1	1	0	0	1	0	1	0	1	0	1
Att 1a	1	1	0	-1	1	-2	2	-3	0	-1	-1	0	5	3	1	1	-1	0
Att 1b	4	1	-1	0	2	0	-1	-3	2	0	-2	1	3	5	-1	0	1	4
Att 2a	-1	1	0	0	-1	-2	2	1	1	-1	0	0	1	-1	4	2	-1	-2
Att 2b	0	1	2	-1	-2	0	2	2	-2	-3	0	1	1	0	2	5	1	0
Att 3a	1	-1	2	1	1	2	-2	-1	-2	-1	-3	0	-1	1	-1	1	5	4
Att 3b	4	0	0	1	2	2	-3	-2	1	0	-3	1	0	4	-2	0	4	6

* $I = X'X$

technique two we work with the information matrix presented in Table 11, and for technique three we work with the information matrix presented in Table 12.

It becomes apparent that technique 1 can never be equivalent to techniques 2 and 3. The information matrix of technique 1 is of a completely different size and structure than for techniques 2 and 3. The information matrices for techniques 2 and 3 are equivalent in size (both are 6 by 6) although they differ in magnitude of elements.

Technique 1 is not equivalent to techniques 2 or 3, so the question remains when are techniques 2 and 3 equivalent? Let us consider the design in Table 13 with no overlap and perfect level balance within choice sets. We continue working with the assumption that no prior information on the parameter estimates can be obtained, i.e. β is zero. The information matrices for this design for techniques 2 and 3 are presented in Table 14. We notice that the information matrices for techniques 2 and 3 are equivalent and always will be in the case of designs with no overlap and prefect level balance.

When β is assumed to be zero the information matrix for a choice design, Z'PZ, becomes Z'cIZ = cZ'Z, where c is the inverse of the number of alternatives in the choice set. Therefore, to compare the differences between the linear and choice design calculations of efficiency we can compare X'X (the linear formulation) and Z'Z (the choice formulation). When β is zero, we recall that Z is a $J_{\bullet}$ x K matrix with rows:

$$z_{jn} = x_{jn} - \bar{x}_n \text{, where } \bar{x}_n = \frac{1}{J_n}\sum_{i=1}^{J_n} x_{in} \text{ .}$$

When the design is perfectly level balanced and has no overlap, the centering term is constant. Therefore, Z'Z and X'X are equivalent. When the design is not completely level balanced or when there is any overlap in the design, the centering term is no longer

Table 11 - Information Matrix for Technique Two

	Attribute 1a	**Attribute 1b**	**Attribute 2a**	**Attribute 2b**	**Attribute 3a**	**Attribute 3b**
Attribute 1a	18	9	0	0	0	0
Attribute 1b	9	18	0	0	0	0
Attribute 2a	0	0	18	9	0	0
Attribute 2b	0	0	9	18	0	0
Attribute 3a	0	0	0	0	18	9
Attribute 3b	0	0	0	0	9	18

* $I = Z'PZ$, with β assumed to be zero

Table 12 - Information Matrix for Technique Three

	Attribute 1a	**Attribute 1b**	**Attribute 2a**	**Attribute 2b**	**Attribute 3a**	**Attribute 3b**
Attribute 1a	12.67	7.33	-1	0	2	1
Attribute 1a	7.33	14.67	-1	-1	1	-1
Attribute 1a	-1	-1	14.67	8.33	0	2
Attribute 1a	0	-1	8.33	16.67	1	0
Attribute 1a	2	1	0	1	12.67	6.33
Attribute 1a	1	-1	2	0	6.33	10.67

* I = Z'PZ, with β assumed to be (-1 0 -1 0 -1 0)

Table 13 - Design with No Overlap and Perfect Level Balance

Choice Set	Attribute 1	Attribute 2	Attribute 3
1	3	1	2
1	2	2	2
1	1	3	1
2	3	1	2
2	2	3	1
2	1	2	3
3	3	2	1
3	2	1	3
3	1	3	2
4	3	1	1
4	1	3	3
4	2	2	2
5	2	1	3
5	3	3	1
5	1	2	2
6	2	3	1
6	3	2	2
6	1	1	3
7	1	3	2
7	3	1	1
7	2	2	3
8	2	3	2
8	3	2	1
8	1	1	3
9	1	2	3
9	3	1	2
9	2	3	1

Table 14 - Information Matrices for Techniques Two and Three

Technique 2					
18	9	-6	-3	-8	-4
9	18	-6	-3	-4	-2
-6	-6	18	9	-7	-5
-3	-3	9	18	-5	-1
-8	-4	-7	-5	18	9
-4	-2	-5	-1	9	18

Technique 3					
18	9	-6	-3	-8	-4
9	18	-6	-3	-4	-2
-6	-6	18	9	-7	-5
-3	-3	9	18	-5	-1
-8	-4	-7	-5	18	9
-4	-2	-5	-1	9	18

constant and differences between Z’Z and X’X result.

5.4 Summary and Conclusions

In selecting the information matrix formulation it seems logical to employ the structure that corresponds with the model you intend to use to analyze the data. Utilizing the linear model information matrix remains convenient because the formulation of efficient designs for this case are well studied and understood, and there are currently many algorithms available for construction of such designs.

Using the appropriate formulation of the information matrix in the selection of an efficient design allows one to consider all of the different issues related to the model before making a design selection. For example, although the efficiency measure for a linear and choice design are similar when there is no prior information assumed on the parameter estimates, that is not the case when β is assumed to be non-zero. Several authors have advocated the use of non-zero prior for the creation of efficient designs (Huber, et al. 1996, Swait, et al. 2001). Using only the linear model formulation for the information matrix ignores all of the additional information concerning the model under study. The issue becomes even more complex with the model is more involved that the multinomial logit. For example, considering the nested logit model, the model takes into consideration a two tiered decision making structure by the decision maker. The linear model is not capable of taking this into consideration. Further the linear model formulation does not take into consideration the structure of the choice set.

Even in the context of more traditional linear designs, one of the foremost assumptions in the creation of optimal designs is that the model has been specified correctly. In the linear model case, the model specification indicates that the correct terms are contained in the model. For choice designs the correct model specification means that we select a model, i.e. multinomial logit, probit, nested logit, etc., that is appropriate for the data being collected. In summary, the use of the information matrix for the appropriate model will result in designs that are best suited for the intended analysis.

6. Characteristics of Optimal Choice Designs

Earlier we discussed four primary characteristics that are used to evaluate choice designs: level balance, utility balance, orthogonality and minimal level overlap. Level balance occurs when each level of an attribute appears with equal frequency in the overall design. Utility balance is achieved when each alternative within a choice set is equally preferable. We will measure utility balance through the use of entropy. Orthogonality is a criterion from linear design theory that guarantees that parameter estimates are independent. In choice design theory, however, it has been shown traditional orthogonality measures do not guarantee independent estimates or increased efficiency. Minimal level overlap indicates that within a choice set there is as little overlap as possible within an attribute.

6.1 Evaluation of Efficient Choice Designs

Although technique one in Chapter Five was shown to be inferior to techniques two and three it does allow some flexibility in the design creation process that techniques two and three cannot provide. First technique one allows one to create choice sets where all alternatives do not have the same number of attributes or even the same attributes. This can be particularly useful in brand specific studies. Another notable feature of technique one is that it results in all or almost all of the attributes, both within and between choice sets, being orthogonal.

Although technique one allows greater flexibility in terms of orthogonality we have established that the designs it creates are inferior to those created using the correct model specification for standard models that include main effects and two-factor interactions. Therefore we will now evaluate the optimal designs created using the SAS® macro %choiceff on the four characteristics of choice designs. We will evaluate designs for the 3^3 in 9 choice sets of size 3, $3^2 \cdot 4^2$ in 10 choice sets of size four and $3^2 \cdot 4 \cdot 5$ in 10 choice sets of size four. The results for these smaller design sizes extend to larger, more complex designs. Each design size will be considered with a zero prior for β and also with an equal-spaced prior assumption for β. The equal-spaced prior assumption for β assumes that there is an equally spaced, linear relationship between the parameter estimates for the levels of each attribute. The designs created for this analysis are presented in Appendix Two. Table 15 shows the summary results for each of these cases.

We see that the designs created with the zero prior assumption preserve the criterion of minimum level overlap, whereas the designs created with the equal-spaced prior do not. Violations of minimum level overlap become more severe the more complicated the design is. We note that when evaluated under the zero prior, all designs have maximum entropy (utility balance). When the designs are evaluated under the equal-spaced prior, the designs created using the zero prior assumption are less utility balanced (as measured using entropy) than those created under the equal-spaced prior assumption. The only design that preserves the criterion of orthogonality is the 3^3 design created using the zero prior assumption.

Table 15 - Summary of Design Characteristics for Efficient Designs

	3^3	3^3	$3^2 \cdot 4^2$	$3^2 \cdot 4^2$	$3^2 \cdot 4 \cdot 5$	$3^2 \cdot 4 \cdot 5$
	β = 0	**Equal-spaced prior**	**β = 0**	**Equal-spaced prior**	**β = 0**	**Equal-spaced prior**
Efficiency	1.68	1.38	4.39	2.93	3.87	2.66
Level Overlap	Minimum	3.7%	Minimum	35%	Minimum	25%
Entropy (% Max) β = 0	9.89 100%	9.89 100%	13.86 100%	13.86 100%	13.86 100%	13.86 100%
Entropy (% Max) Equal-spaced prior	6.0168 60.84%	6.48 65.49%	7.6104 54.91%	12.06 86.96%	5.8353 42.10%	12.21 88.04%
Level Balance	Yes	Not in Attribute 3	Not in Attribute 2	Not in any Attribute	Not in Attribute 4	Not in any Attribute
Orthogonal	Yes	No	No	No	No	No

6.2 Assumptions of Optimal Designs

When we create an optimal design, there are several underlying assumptions that need to be met in order to ensure the design is truly optimal. Even if the intended analysis is simple descriptive statistics, departures from these assumptions can affect our interpretation of the experimental results.

Cox (1958) discusses a primary assumption underlying the design of experiments. When a particular treatment is applied to an experimental unit the observation obtained is assumed to be:

$$\left(\begin{array}{c}\text{A quantity}\\ \text{depending only}\\ \text{on only the}\\ \text{particular unit}\end{array}\right) + \left(\begin{array}{c}\text{A quantity}\\ \text{depending on the}\\ \text{treatment used}\end{array}\right)$$

This assumption results in three core points:

1. The effects of treatments and units are additive
2. The treatment effects are constant
3. The observation on one unit is unaffected by the treatment applied to other units

The treatment effects are additive on the utility scale in the multinomial logit model. The second component of this assumption that the treatment effects are constant is an issue that is already familiar in choice designs. If we are proposing the use of a simple model for analysis of our choice experiment, such as the multinomial logit, then we are already assuming that out population is homogeneous and that the effect of different choice sets will be constant across experimental units or in our case across participants. If we are electing to use a more complex model for the analysis of our experiment, such as the mixed logit, then we retain greater flexibility in modeling the heterogeneity between participants in the experiment and the violation of this point is not severe. The third component of this assumption is the one most likely to be a problem for choice experiments. The assumption that the observations are independent is one that the choice literature has identified to be untrue in many cases. Consider for example the learning effect at the beginning of a choice experiment. When participant embark on the choice task they often spend the first few choices learning about the situation being studied. This learning may lead to inconsistencies in their choices due to unfamiliarity with the choice task or because the participant is searching to identify the sample space under which the experiment takes place. Another potential problem with this assumption is that participants are known to utilize more than one decision making process throughout the course of a choice experiment. As discussed earlier, Shugan (1980) identifies four strategies (*conjunctive, disjunctive (maximin), minimax and lexicographic*) that decision markers employ. Swait (1996) shows that decision makers enter more than one decision state in the course of an experiment. During a choice experiment a participant may switch from a decision making strategy that uses all information in the choice set (lexicographic) to one that simplifies the process, ranking on only the important attributes (disjunctive). This change in decision making strategy may lead to different effects by the same treatment depending on the difficulty of the choices presented previously.

6.3 Summary and Conclusions

In reviewing the six designs in appendix two we notice the following characteristics of optimal choice designs:

- ***Orthogonality:*** Only the simplest of the optimal choice designs are orthogonal
- ***Level Balance:*** Designs created with a zero prior preserve level balance more than those with a non-zero prior
- ***Utility Balance (Entropy):*** The designs created with a zero prior all have maximum entropy under the assumed prior, i.e. all choices within the choice set are presumed to be equally preferable. When the equal-spaced prior is assumed the entropy is between 65 and 90% of the maximum possible. These designs are challenging, but not of maximum difficulty for the participants. We must note that a person's true β differs from the assumed β therefore actual entropy differs from supposed entropy.
- ***Level Overlap:*** The designs where there is an assumption of a zero prior for β all have minimum level overlap beyond that which is necessitated by more alternatives than attribute levels. The designs with the equal-spaced prior for β assumption have between 4 and 35% level overlap. Designs with the equal-spaced prior assumption always have some level overlap present, even for attributes with more levels than alternatives within a choice set.

Optimal choice designs do a good job of preserving the utility balance and minimum level overlap. Orthogonality and level balance are not preserved in optimal choice designs.

7. Optimal Design for Entropy and Efficiency

Swait and Adamowicz (2001) discuss the use of entropy for measuring choice complexity. They find that the stability of the parameter estimates change as the complexity of individual decisions and the cumulative cognitive burden increases. In addition to using the D-efficiency to decide which of a competing set of designs is most efficient, we can use entropy as an additional criterion in the design of the experiment. Using these two criteria simultaneously will allow the researcher to ensure that the design will result in the most efficient parameter estimates possible and that the potential efficiency of these estimates will not be influenced by the potential immeasurable effects of switching decision strategies due to the cognitive burden or the experiment.

Huber and Zwerina (1996) introduce the concept of utility balance, indicating that utility balance is one of the four criteria that should be evaluated in the creation of good choice. They posit that the higher the utility balance the better the design. High utility balance occurs when the alternatives in the choice set are well balanced in terms of preferences, leading to challenging tradeoffs between the alternatives in the choice set. Utility balance can be measured through entropy. In addition to measures of an individual choices utility balance, cumulative entropy can be used to measure the overall utility balance of the design.

Before further discussing entropy we must understand the implication of the prior on β and its effect on the entropy of a choice design. There are three primary methodologies for specifying the prior of a choice design as discussed earlier. These methodologies and their implications on the calculation of entropy are shown below:

1. Zero Prior Assumption

   When a zero prior is specified for β the assumption becomes that all alternatives in the choice set are equally preferable to the participants. This means that whenever we assume a zero prior for β we are assuming that every choice task is of maximum entropy.

2. Simple, Linear Prior Assumption

   When we assume an equal-spaced prior as advocated by Huber and Zwerina we can calculate the entropy for each choice in the choice set and then evaluate designs on their relative entropy. The equal-spaced prior assumption is that we are able to rank order the attribute levels in terms of their relative utility and that the levels are equally spaced in preference.

3. Bayesian Prior Assumption

   Similar to the use of a simple, linear prior assumption we can calculate the entropy for each choice set based on the distribution assumption for β for each choice in the choice set. We can then evaluate designs on their entropy distribution.

Huber and Zwerina (1996) conclude that for most applications we are able to identify, at the very least, simple directional priors for each parameter and that the increase in efficiency due to the use of these priors makes it a recommended practice. We will assume for the remainder of this discussion that, at very minimum, an equal-spaced prior is specified.

Although utility balance / entropy have been shown to be beneficial in the statistical design of experiments, what are the consequences of presenting a utility balanced design to a choice experiment participant? According to Dhar the tendency to defer choices is greater when differences in attractiveness between alternatives are small versus when they are large. This applies when there is a no-choice alternative present in the choice or when participants are prone to skip choices if they are too difficult. Therefore, increasing utility balance (entropy) as much as possible, which may lead to increased efficiency of the design, may be detrimental to other aspects of the choice experiment.

Maximizing entropy can result in the following issues for the experimenter:

- Fatigue effects for the participants
- Inconsistencies in choices due to difficulty of the task
- Disengaging from the process due to lack of incentives
- Disengaging from the choice task due to lack of realism of the alternatives presented

These issues can be far more detrimental to the results of the experiment than a slight decrease in efficiency or utility balance due to selecting an "easier" design.

The determination of an appropriate level of cumulative entropy for a task lies with the designer of the experiment. In some situations the use of a design with maximum cumulative entropy may be entirely appropriate. For example, when the participants are highly engaged in the task and feel that there is significant reward to accurately responding to the choice tasks at hand, the use of a design with maximum efficiency and entropy may be highly appropriate. In other situations there may be consequences in the quality of information collected due to the use of designs with high entropy. For example, when participants are not interested in the task at hand or when they have a limited understanding of the task, the use of the designs with high entropy can result in an increase in the amount of information lost through the selection of the no-choice alternative, or in the event that there is not a no-choice alternative in the design, the participant may skip the difficult questions. In addition to loss of information due to non-response, there may be significant degradation of the data that are collected. D-efficiency as a measure of design goodness assumes that there is no correlation between the quality of information collected and the efficiency of the design. In fact the overall results of the experiment can be more reliable when collected with a design that is slightly less efficient and has lower cumulative entropy for the task.

We seek to answer two primary questions concerning the design of experiments using both entropy and efficiency as criteria for optimality:

1. Can we create many D-efficient designs with a wide range of entropy values to afford experimenters choices in entropy?
2. Can we modify D-efficient designs to have lower entropy without greatly decreasing the efficiency of the design?

7.1 Optimal Design with Specific Entropy

We approach the problem of creating optimal design with specific entropy through two avenues:

1. Evaluation of the entropy of optimal designs created using the SAS® macro %choiceff
2. Random simulation of designs to understand the possible range of entropy and efficiency values

We will evaluate scenarios 1 and 2 above for the following designs:

- 3^3 in 9 choice sets of size 3
- $3^2 \cdot 4^2$ in 10 choice sets of size 4
- $3^2 \cdot 4 \cdot 5$ in 10 choice sets of size 4
- $3^2 \cdot 4 \cdot 5^2$ in 15 choice sets of size 3

Random simulation of designs will assist in understanding the space of possible combinations for entropy and efficiency for a particular scenario. Once the design space is understood it is possible to understand the flexibility in choices of entropy for efficient designs that is possible. We consider the cases below to understand the flexibility in selecting efficient designs.

Example: 3^3 in 9 choice sets of size 3

Examining the designs created using the %choiceff macro in SAS®, we see that there is very little correlation between the cumulative entropy and the efficiency of the design. There is a range of efficiency from 3.35 to 3.6, with the least efficient design being only 93.1% as efficient as the most efficient design. The cumulative entropy of these efficient designs are between 8 and 8.8. These entropy values are between 80.89 and 88.98% of the possible total cumulative entropy.

In considering the random creation of designs for a 3^3 in 9 choice sets of size 3, there are 27 possible alternatives which results in 2925 possible choice sets of size three. This results in 4.27 x 10^{25} possible choice sets. We will begin by randomly simulating 100,000 different choice designs and calculating the efficiency and entropy for each design. It should be noted that designs which are incapable of estimating the appropriate main effects model are removed from consideration in our simulation; there are 30 designs that were inestimable and removed from consideration. Figure 6 shows the results of the designs created by random simulation and with the %choiceff macro. We notice that there is a positive correlation between the cumulative entropy of the design and the D-efficiency of the design. The maximum possible cumulative entropy for this scenario is 9.89. We find that of the most efficient designs there is a range of cumulative entropy from 8 to 9, or from about 80 to 90% of possible cumulative entropy. This gives us some flexibility in selecting optimal designs with a varied range of entropy for different applications.

Example: $3^2 \cdot 4^2$ in 10 choice sets of size 4

Figure 7 shows the results for the designs created by random simulation and with the %choiceff macro. Our previous section discussed the results of a simulation in SAS® for optimal choice designs. The range of entropy for the efficient designs created using SAS® is from 11.4 to 12.6, or 82.25 to 90.91% of possible cumulative entropy. These designs are more complex than those created by the random selection. The efficiency of the designs create by SAS® have a range of efficiency from 2.75 to 3.15, whereas those created by random simulation have efficiency of 0.2 to 2. The designs created randomly are approximately 6.35 to 63.45% less efficient than those created using SAS®.

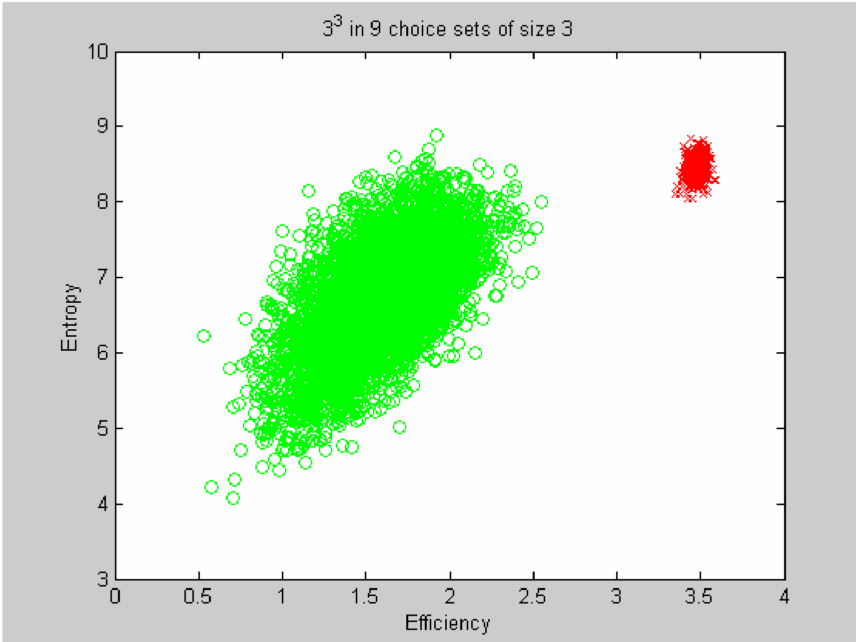


Figure 6 - Random versus Efficient Choice Designs Entropy and Efficiencies: 3^3

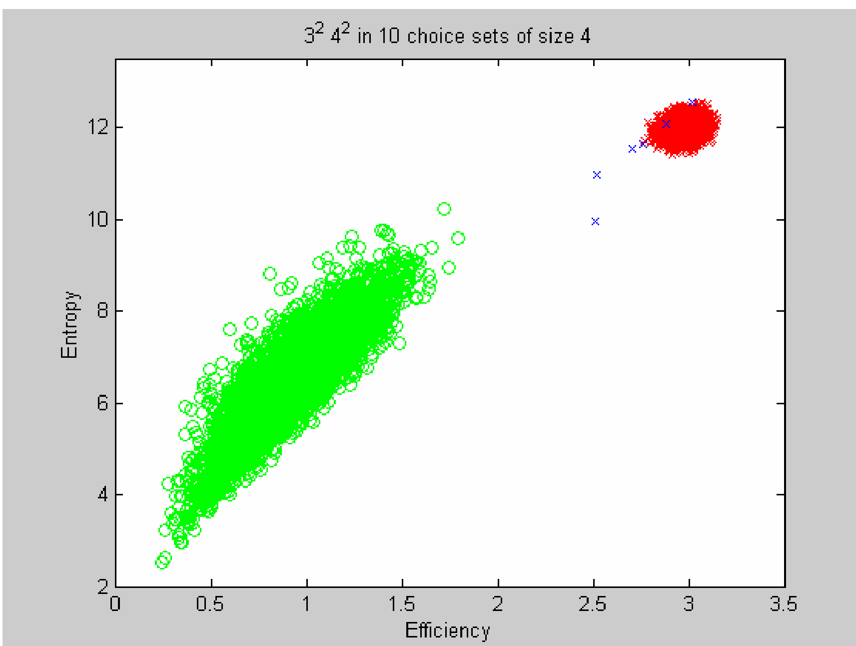


Figure 7 - Random versus Efficient Choice Designs Entropy and Efficiencies: $3^2 \cdot 4^2$

Example: $3^2 \cdot 4 \cdot 5$ in 10 choice sets of size 4

Figure 8 shows the results for designs created with random simulation and the %choiceff macro. The range of entropy for the efficient designs created using SAS® is from 11.2 to 12.8, or 80.81 to 92.35% of possible cumulative entropy. These designs are more complex that those created using random selection. The efficiency of the designs created by SAS® have a range of efficiency from 2.45 to 2.8, whereas those created by have efficiency of 0.2 to 1.8. The designs created randomly are only from 7 to 64.29% efficient as the most efficient design created using SAS®.

Example: $3^2 \cdot 4 \cdot 5^2$ in 15 choice sets of size 3

Figure 9 shows the results for the designs created by random simulation and with the %choiceff macro. The range of entropy for the efficient designs created using SAS® is from 12.6 to 14, or 76.46 to 84.96% of possible cumulative entropy. These designs are more complex than those created by the random selection. The efficiency of the designs create by SAS® have a range of efficiency from 2.95 to 3.4, whereas those created by random simulation have efficiency of 0.1 to 1.8. The designs created randomly are only from 2.94 to 52.94% efficient as the most efficient design created using SAS®.

Examining these four cases we see that using random simulation is not an appropriate method to create choice designs that are both efficient and give a good range of entropy. Figures 6 – 9 reveal the success of SAS's %choiceff macro in obtaining highly efficient designs. Using the SAS® macro %choiceff to create efficient designs allows us to have a range of entropy of about 10% for most cases. This provides us with some flexibility in creating designs with different values of entropy.

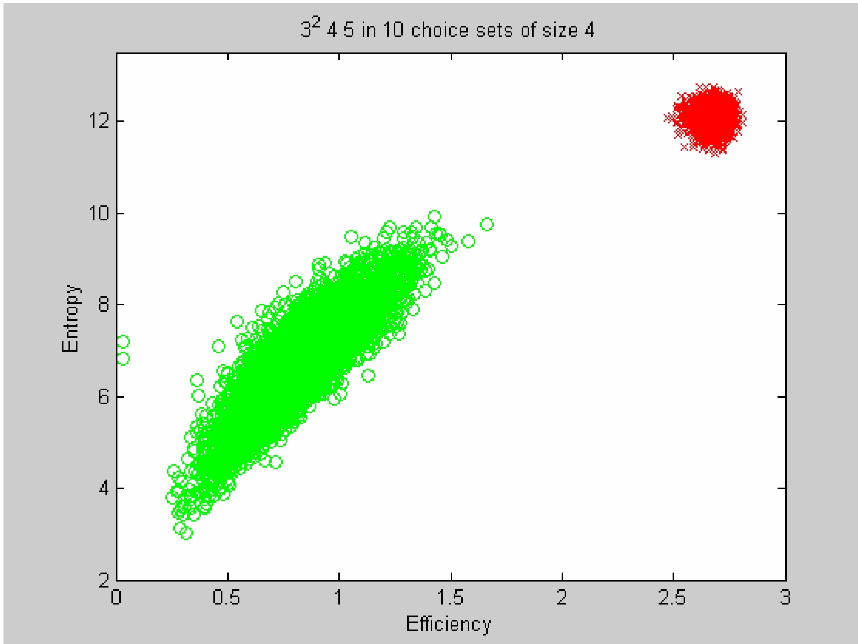


Figure 8 - Random versus Efficient Choice Designs Entropy and Efficiencies: $3^2 \cdot 4 \cdot 5$

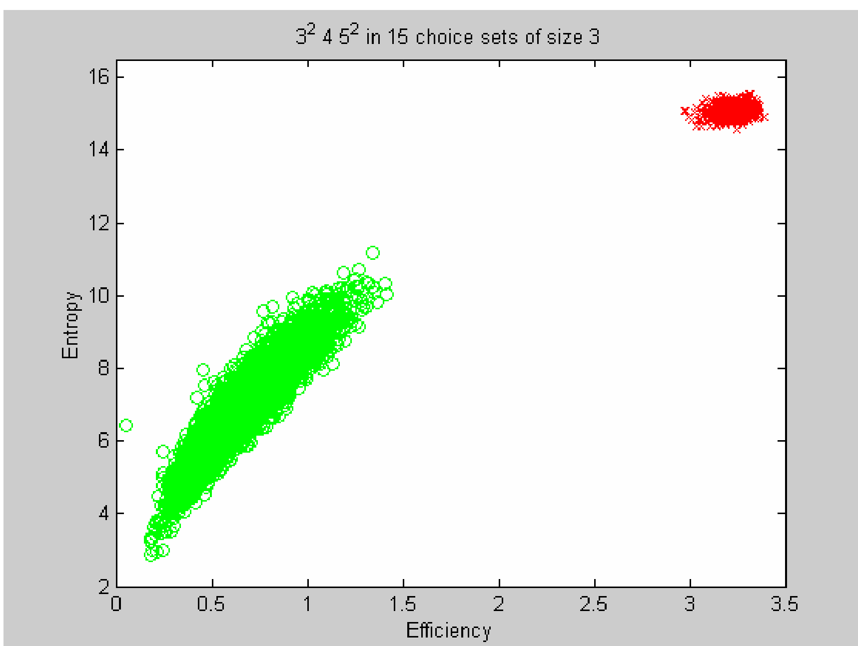


Figure 9 - Random versus Efficient Choice Designs Entropy and Efficiencies: $3^2 \cdot 4 \cdot 5^2$

7.2 Manipulating Efficient Designs

Consider the efficient choice design created by the %choiceff macro in SAS® in Table 16. This design has a D-efficiency of 3.0164 and cumulative entropy of 12.5528. This design has 93% of the maximum possible cumulative entropy. We see the entropy of each choice set in Table 17. Choice set 1 makes the largest contribution to the overall entropy of the choice task. We therefore decide to manipulate choice set one to reduce the cumulative entropy of the task and reevaluate the D-efficiency of the choice task. We continue this process for six iterations of updating the choice sets with the highest entropy. The results are shown in the Table 18. Over these six iterations we obtain a 20.82% reduction in the cumulative entropy of the task while accepting a 16.73% reduction in the efficiency of the design. Figure 7 shows these iterations in relation to the efficiency and entropy of optimal and randomly generated choice designs. We see that our process results in designs that are less efficient than the optimal designs but also have less entropy that the optimal designs.

7.3 Summary and Conclusions

Creating efficient designs for choice experiments is best accomplished using a specialized algorithm for the construction of choice designs. Random creation of choice designs results in designs that show a wide range of values for both entropy and efficiency, however finding those that are the most efficient or with the highest entropy requires extensive, time consuming simulation. Given the space of possible designs, completing random searches in cases with more than three attributes or three levels per attribute is not a realistic approach to the problem. The random simulations do reveal that

Table 16 - An Efficient Choice Design

	Attribute 1	Attribute 2	Attribute 3	Attribute 4
1	3	2	1	4
1	2	3	2	3
1	1	2	4	2
1	3	3	3	1
2	2	2	3	1
2	2	3	1	2
2	1	1	4	1
2	3	1	2	2
3	3	3	1	1
3	1	1	3	1
3	2	2	2	1
3	1	2	2	2
4	2	1	2	4
4	1	2	4	2
4	3	3	2	1
4	1	3	1	3
5	3	2	1	4
5	2	3	3	2
5	1	3	2	4
5	2	1	4	3
6	3	1	3	2
6	1	3	1	4
6	2	2	4	1
6	3	2	2	3
7	2	2	4	1
7	3	1	2	3
7	3	2	1	3
7	2	3	3	2
8	2	1	1	4
8	1	2	1	3
8	3	3	2	1
8	1	1	3	1
9	3	1	4	2
9	2	3	1	3
9	2	1	2	4
9	1	2	3	3
10	3	1	3	2
10	1	3	2	4
10	3	1	1	3
10	2	2	4	1

Table 17 - Choice Set Entropy

Set	Entropy
1	**1.3235**
2	1.2683
3	1.2683
4	1.2754
5	1.2683
6	1.2683
7	1.2683
8	1.1727
9	1.2754
10	1.1644

Table 18 - Iterative Adaptation of Entropy and Efficiency

	Entropy	Efficiency
1	12.5528	3.0164
2	12.0671	2.8864
3	10.9732	2.5174
4	11.649	2.7604
5	11.5381	2.705
6	9.9397	2.5118

there is a strong, positive correlation between the cumulative entropy of a choice design and the efficiency of that design. Therefore, it will be unrealistic to assume that we can create very easy tasks (designs with very low cumulative entropy) that are very efficient.

The SAS® simulations reveal that within the efficient designs we still retain some flexibility surrounding the cumulative entropy of the design. We see the flexibility to choose entropy values within a range of approximately 10% for all of the cases studied. This, coupled with the fact the maximum cumulative entropy percentage does not much exceed 90%, tells us that we are not maximally tasking the participants.

If one is looking to further reduce the cumulative entropy of the choice task then manually updating individual choice sets with the highest entropy will allow one to reduce the entropy of the choice task and evaluate its effect on the efficiency of the design. If a particular update results in a severe decrease in the efficiency of the design, then the update can be retracted and another change attempted. These updates can result in a fairly significant decrease in the cumulative entropy of the choice task without severe decreases in the efficiency of the design.

Given the possible ramifications of over taxing the participant through the choice task inducing effects of fatigue, inconsistent choices or disengaging from the choice task, one should take into consideration the individual question entropy and cumulative entropy of the choice task when designing the choice task. Given that this consideration to the cumulative entropy of the task can be made without the design or use of a new algorithm, it should be considered whenever possible.

The exploration of the efficiency and entropy of designs created under different decision strategies gives additional insight into picking a choice design for a particular study. Earlier research has established that consumers do enter multiple decision states over the course of the experiment. Therefore when selecting an efficient choice design, if we suspect that a particular decision strategy will be the secondary strategy of choice to the compensatory method, then we should pick a design that is a resistant as possible to misspecification of the decision strategy.

8. Prior Assumptions in Creating Optimal Choice Designs

In our earlier discussions of the creation of optimal choice designs we explored the use of both a zero prior assumption and an equal-spaced prior assumption. These are the two most frequent recommendations currently made in the literature for creating optimal choice designs. Earlier work has shown that some misspecification of the magnitude of the equal-spaced prior does not have severe consequences for the efficiency of the choice design (Huber, et al. 1996). We will explore the consequences of different misspecifications of the prior estimates of the parameters to the efficiency, entropy and level balance of the resulting choice design.

8.1 Possible Prior Assumptions

We will introduce six different prior assumptions that have been formulated based on the type of parameter estimates seen in experiments analyzed in the literature and standard assumptions for the design creation. We will explore these possible prior assumptions and the consequences of misspecifying the priors for two different designs, a 3^3 in thirty choice sets of size three and a $3^2 \cdot 4 \cdot 5$ in forty choice sets of size four.

8.1.1 Zero Prior

The first prior assumption is a zero prior. A zero prior assumes that we have no prior knowledge of the effect of the different attribute levels nor the relative importance of the attributes under study. We earlier discussed that this is likely an unrealistic assumption in most research dealing with existing product categories. The zero prior assumption results in designs that are very similar to those created using linear design techniques. An illustration of this prior for the two designs on consideration can be seen in Figures 10 and 11. This will be referred to as prior assumption one.

We examine one such design created for the 3^3 in thirty choice sets of size three and a second design for the $3^2 \cdot 4 \cdot 5$ in forty choice sets of size four to study the effect of the zero prior assumption on the level balance of the optimal choice design. Tables 19 and 20 present a summary of the level balance seen in the designs created by each of the prior assumptions presented in this chapter. We see that when the zero prior is assumed the designs created are as close to level balanced as possible given the number of attribute levels and number of alternatives in a choice set. The 3^3 design exhibits perfect level balance and the $3^2 \cdot 4 \cdot 5$ exhibits near perfect level balance.

8.1.2 Equal-Spaced Prior

The second prior assumption is one that is well established in the optimal choice design literature. The equal-spaced prior assumption is that we are able to rank order the attribute levels in terms of their relative utility and that the levels are equally spaced in preference. It further assumes that the relative importance of the attributes is equal. The equal-spaced prior is illustrated in Figures 12 and 13 for the two design cases under consideration and will be referred to as prior assumption number two.

Again we refer to Tables 19 and 20 to study the level balance of the designs resulting form the equal-spaced prior assumption. We see that the equal-spaced prior assumption for the 3^3 results in a design that still has near perfect level balance with a slight decrease in the frequency of the least preferred level and a relative increase in the

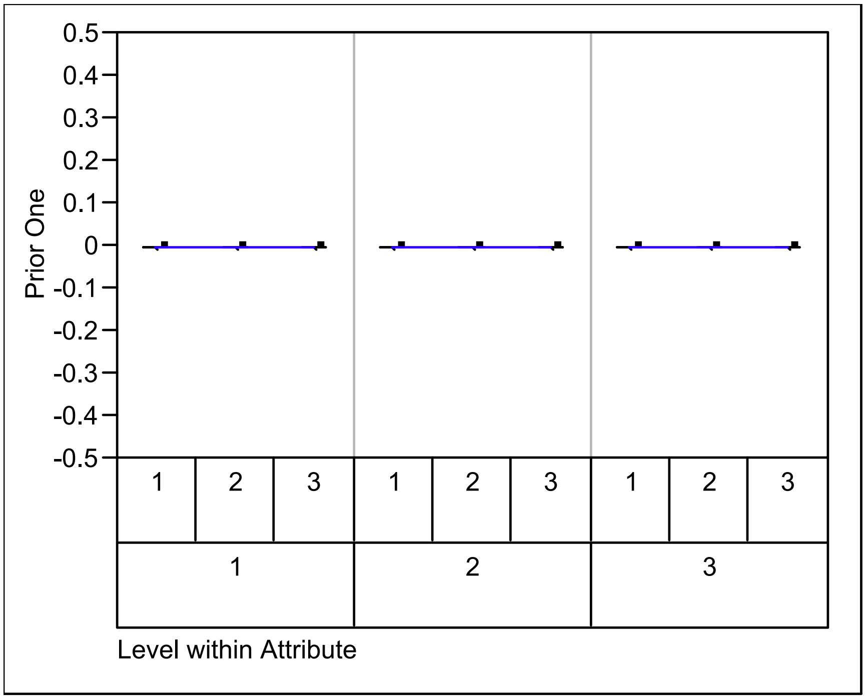


Figure 10 – Prior Assumption One for a 3^3 in 30 Choice Sets of 3

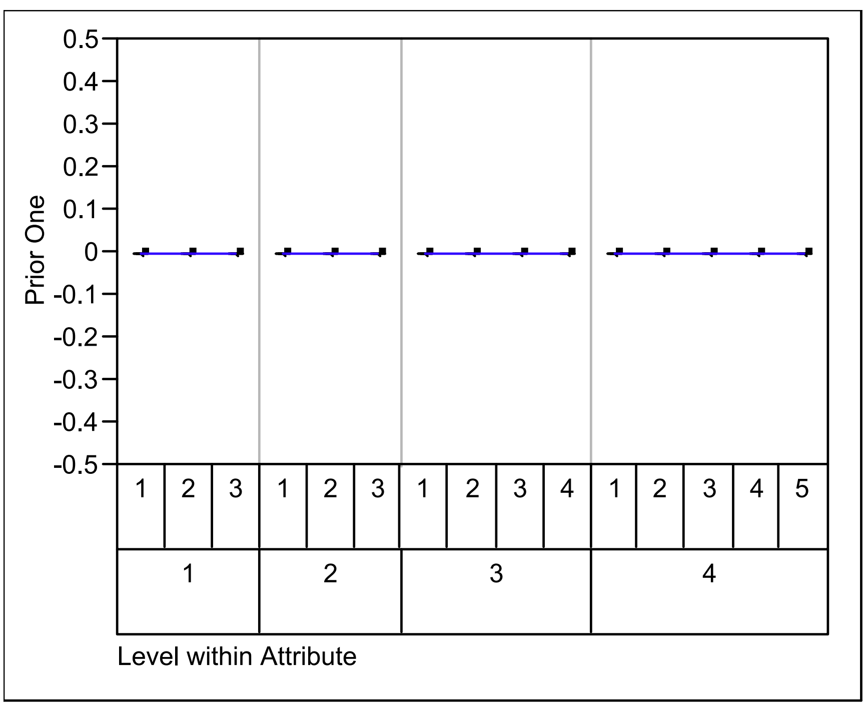


Figure 11 - Prior Assumption One for a $3^2 \cdot 4 \cdot 5$ in 40 Choice Sets of 4

Table 19 - Level Balance for a 3^3 in 30 Choice Sets of 3

		Attribute 1	**Attribute 2**	**Attribute 3**
Prior One	Level 1	30	30	30
	Level 2	30	30	30
	Level 3	30	30	30
Prior Two	Level 1	30	31	31
	Level 2	32	30	31
	Level 3	28	29	28
Prior Three	Level 1	27	29	32
	Level 2	38	35	28
	Level 3	25	26	30
Prior Four	Level 1	30	25	23
	Level 2	29	35	40
	Level 3	31	30	27
Prior Five	Level 1	27	23	25
	Level 2	32	29	32
	Level 3	31	38	33
Prior Six	Level 1	23	31	30
	Level 2	39	31	31
	Level 3	28	28	29

Table 20 - Level Balance for a $3^2 \cdot 4 \cdot 5$ in 40 Choice Sets of 4

		Attribute 1	Attribute 2	Attribute 3	Attribute 4
Prior One	**Level 1**	53	55	40	32
	Level 2	52	52	40	32
	Level 3	55	53	40	32
	Level 4			40	32
	Level 5				32
Prior Two	**Level 1**	43	46	34	24
	Level 2	55	52	39	33
	Level 3	62	62	43	36
	Level 4			44	37
	Level 5				30
Prior Three	**Level 1**	45	42	25	17
	Level 2	51	55	34	25
	Level 3	64	63	42	30
	Level 4			59	41
	Level 5				47
Prior Four	**Level 1**	30	36	35	31
	Level 2	50	51	37	28
	Level 3	80	73	43	31
	Level 4			45	34
	Level 5				36
Prior Five	**Level 1**	33	34	23	17
	Level 2	61	58	45	34
	Level 3	66	68	48	35
	Level 4			44	38
	Level 5				36
Prior Six	**Level 1**	46	44	23	17
	Level 2	52	52	46	36
	Level 3	62	64	47	36
	Level 4			44	36
	Level 5				35

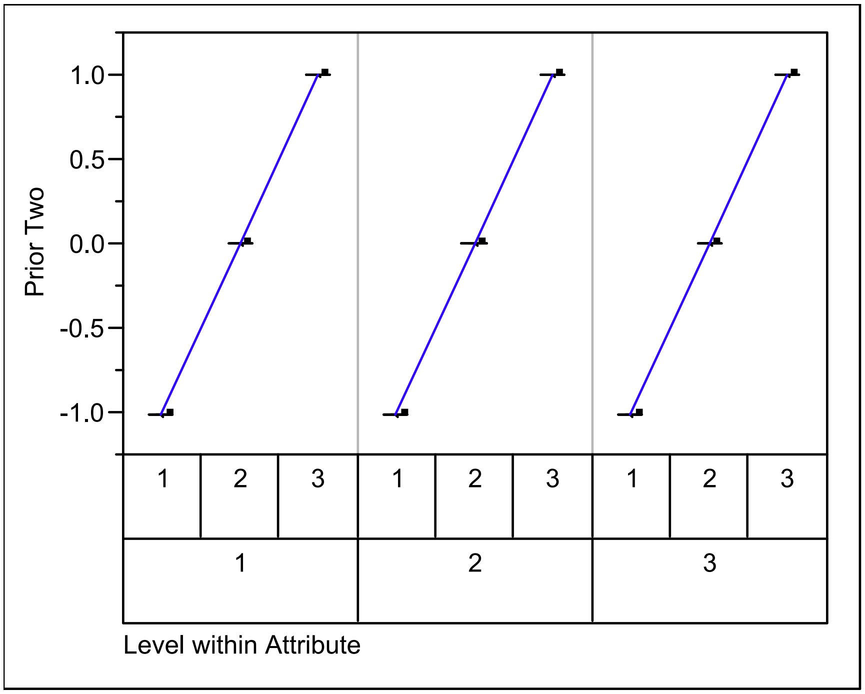


Figure 12 - Prior Assumption Two for a 3^3 in 30 Choice Sets of 3

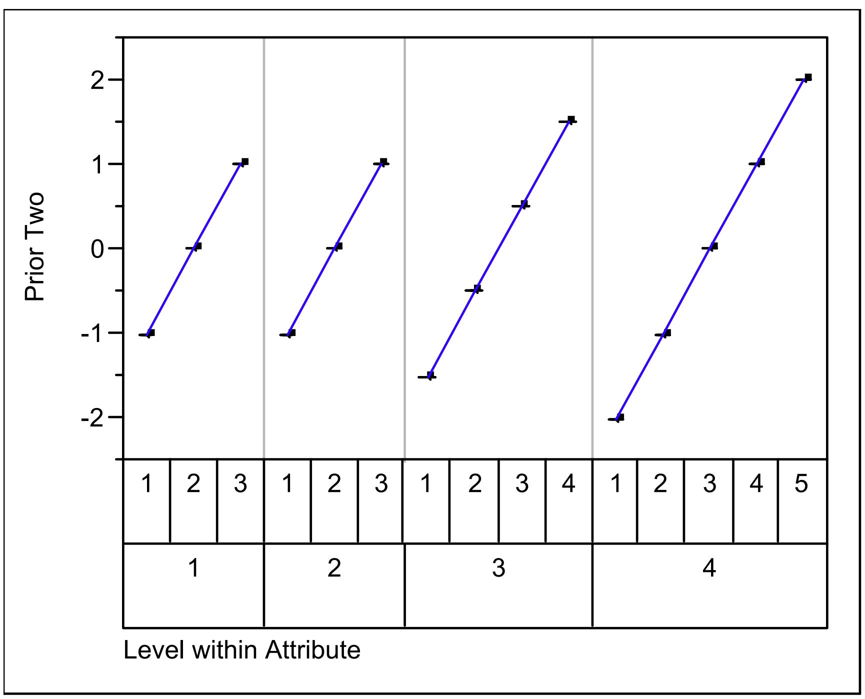


Figure 13 - Prior Assumption Two for a $3^2 \cdot 4 \cdot 5$ in 40 Choice Sets of 4

most preferred level. For the $3^2 \cdot 4 \cdot 5$ design we see that the levels occur proportionally to their respective utilities, the least preferable level occur the least frequently whereas the most preferred levels occur the most frequently (with the exception of level five on attribute four).

8.1.3 Some Attributes are More Important than Others

Another prior assumption that can be made is that perhaps in addition to knowing the rank ordering of the levels, we also know the relative importance of the attributes. For example if we are studying preferences for laptop computers among scientific researchers we may know that the amount of memory and cost will be the most important attributes (with memory being slightly more important) and that video card and processing speed will be of lesser importance. In addition to assuming that we know the relative importance of the attributes, we also assume that we know the relative importance of the attribute levels for each attribute. Figures 14 and 15 illustrate the prior assumption we will make for the two designs under consideration. These will be referred to as prior assumption three.

We will additionally consider an additional prior assumption where some attributes are more important than others. These are illustrated in Figures 16 and 17. They are equivalent to those seen in Figures 14 and 15 but the opposite factors are considered to be the most important. This will be referred to as prior assumption four. Note that although Figures 14 and 16 appear indistinguishable, for the purpose of evaluating the consequences of misspecification of the prior values the differences will be notable.

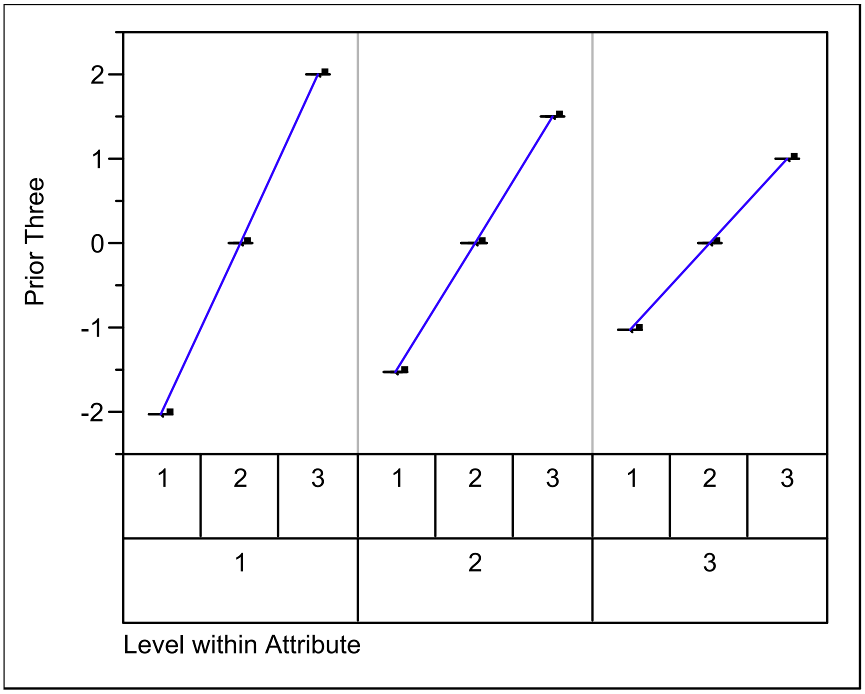


Figure 14 - Prior Assumption Three for a 3^3 in 30 Choice Sets of 3

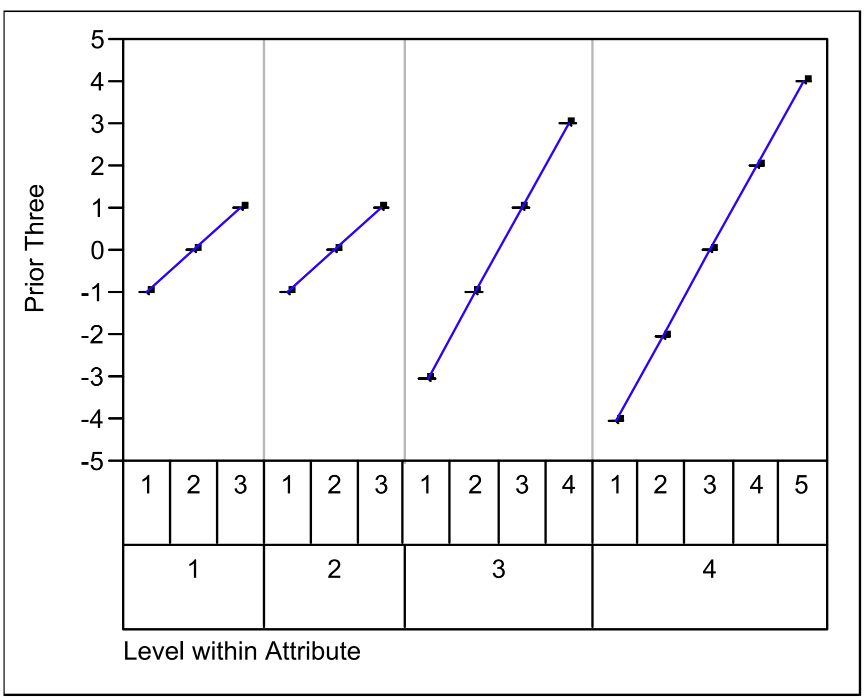


Figure 15 - Prior Assumption Three for a $3^2 \cdot 4 \cdot 5$ in 40 Choice Sets of 4

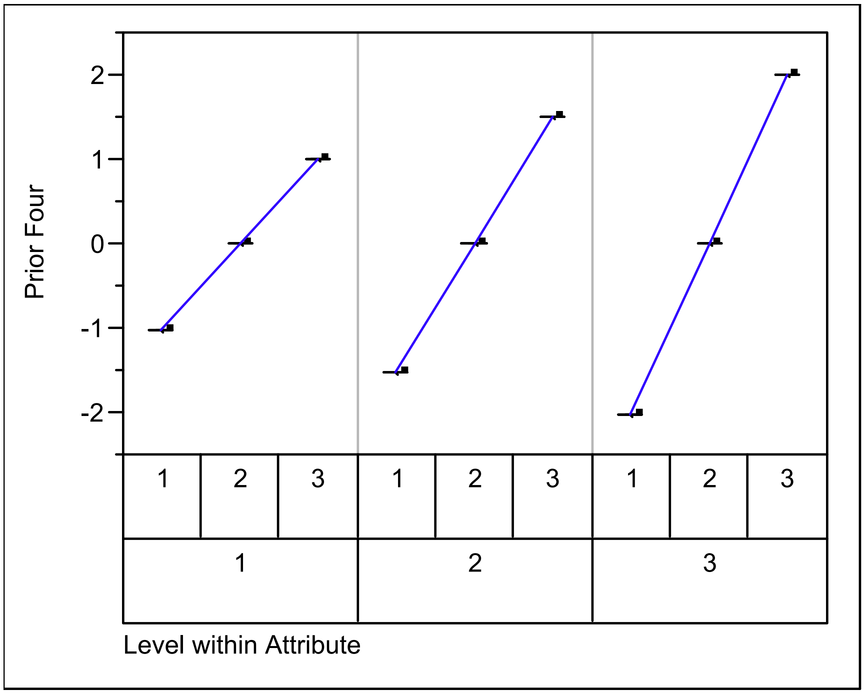


Figure 16 - Prior Assumption Four for a 3^3 in 30 Choice Sets of 3

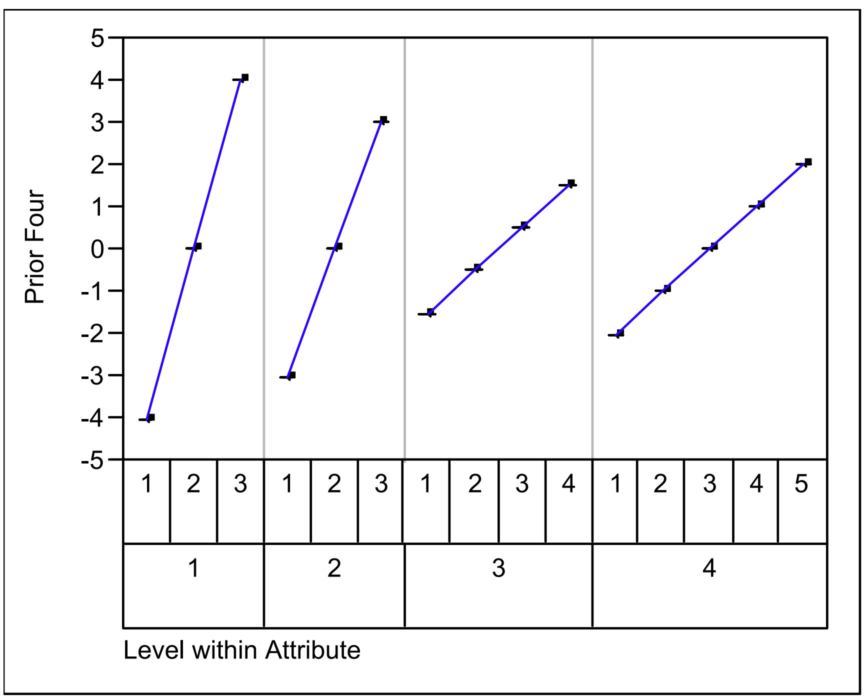


Figure 17 - Prior Assumption Four for a $3^2 \cdot 4 \cdot 5$ in 40 Choice Sets of 4

Again we refer to Tables 19 and 20 to study the level balance of the designs resulting form the prior assumptions three and four. For prior assumption three and the 3^3 design we see close to level balance in the attributes with the smallest magnitude prior (attribute three) and the furthest from level balance in the greatest magnitude prior (attribute one). For the $3^2 \cdot 4 \cdot 5$ the distribution of levels is farthest from level balanced when the magnitude of the prior assumption is greater (attributes three and four). For prior assumption four we see behavior similar to prior assumption three except that the most and least preferred attributes are reversed. For the $3^2 \cdot 4 \cdot 5$ we see that the behavior is the same, the level balance is most severely violated on attributes one and two, the attributes with the greatest magnitude priors assumed.

8.1.4 Attribute Levels are Not Equally Spaced

Another possible prior assumption is that in addition to knowing the relative importance of the attribute levels, we know that they will not be equally spaced. This may be known for all of the attributes or only some of the attributes. Figures 18 and 19 illustrate these prior assumptions assuming that all attributes are believed to exhibit this behavior for the two designs under consideration. This will be referred to as prior assumption five. Figures 20 and 21 illustrate the prior that will be used when only a subset of the attributes is believed to exhibit this behavior. This will be referred to as prior assumption six.

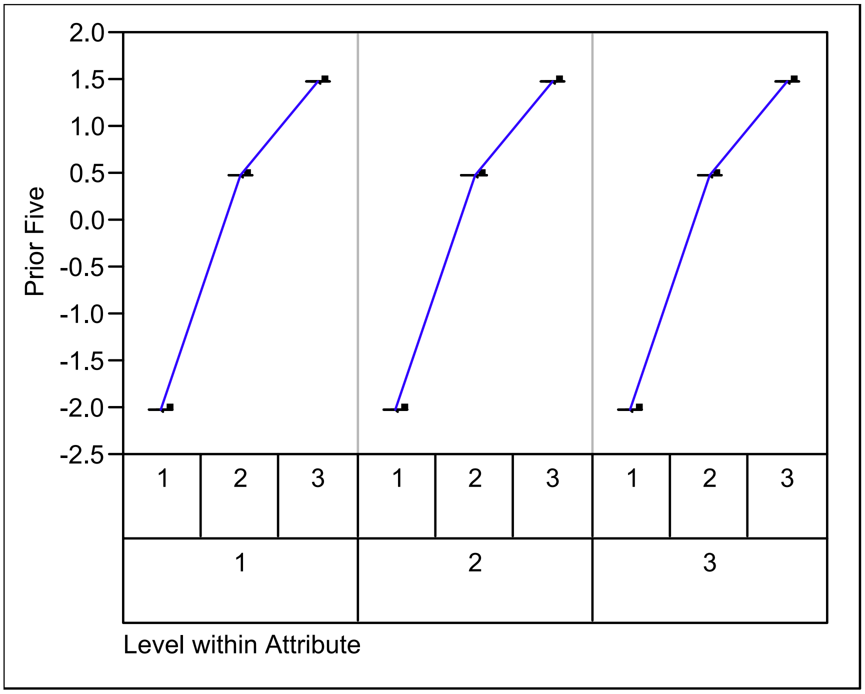


Figure 18 - Prior Assumption Five for a 3^3 in 30 Choice Sets of 3

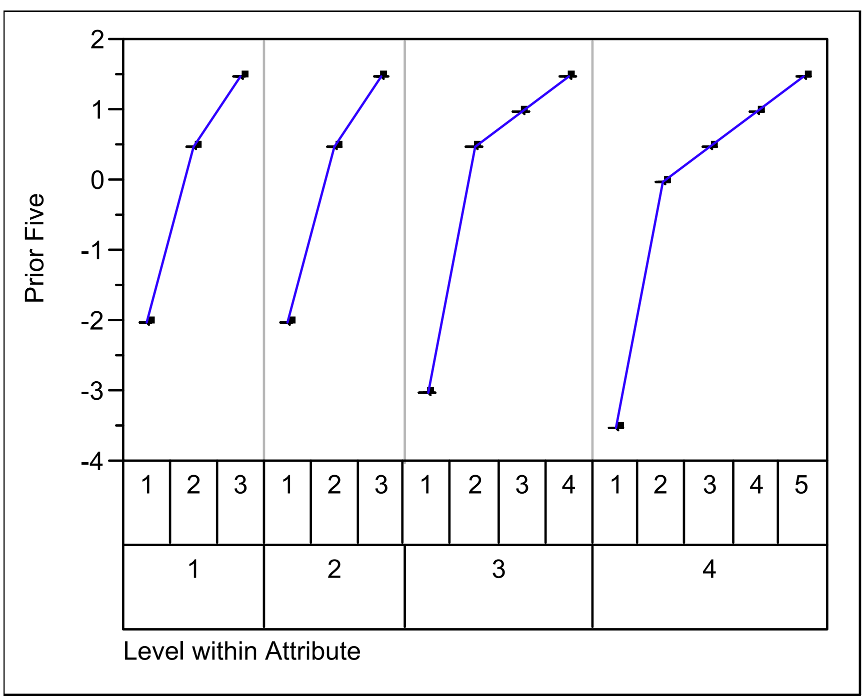


Figure 19 - Prior Assumption Five for a $3^2 \cdot 4 \cdot 5$ in 40 Choice Sets of 4

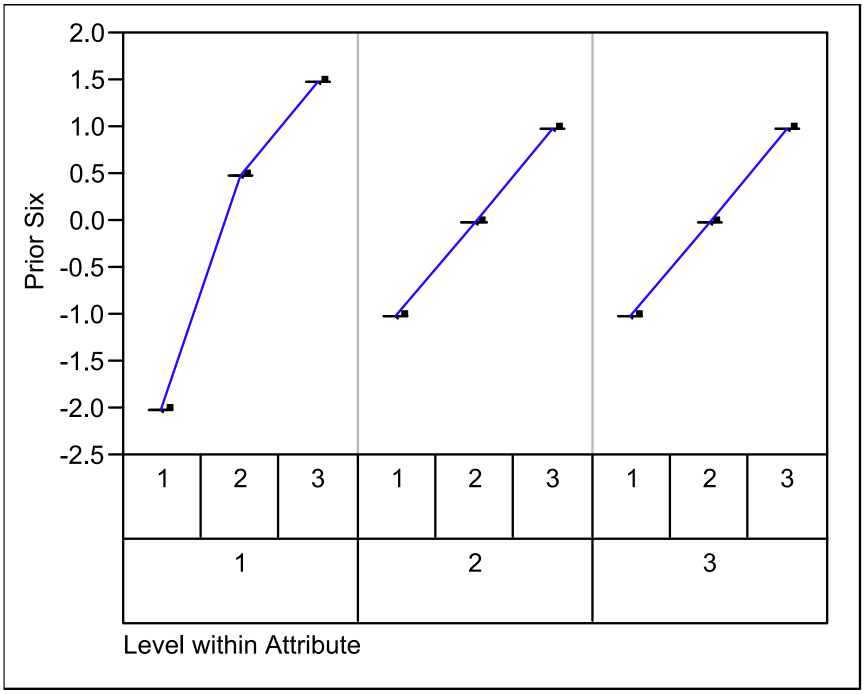


Figure 20 - Prior Assumption Six for a 3^3 in 30 Choice Sets of 3

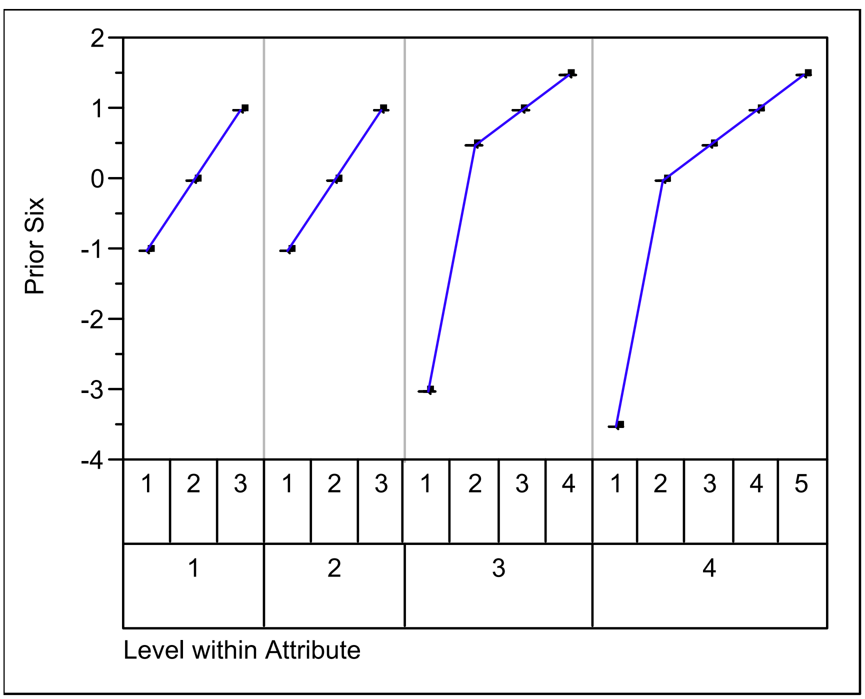


Figure 21 - Prior Assumption Six for a $3^2 \cdot 4 \cdot 5$ in 40 Choice Sets of 4

Again we refer to Tables 19 and 20 to study the level balance of the designs resulting form the equal-spaced prior assumption. We see that prior assumption five for the 3^3 results in a design where the least preferred attribute levels are the least frequent and the other two attribute levels are fairly balanced (there is a slight deviation in attribute two). For the $3^2 \cdot 4 \cdot 5$ the behavior is similar to that of the 3^3 design, the least preferred level of each attribute has the smallest frequency and the remaining levels are closer to level balanced in relation to their relative utility. Prior assumption six for the 3^3 design results in near perfect level balance in all attributes except for the attribute with the unequally spaced attribute level. For the $3^2 \cdot 4 \cdot 5$ we see behavior similar to that of the equal-spaced prior for the variables with an equal magnitude assumption, but the attributes where one level is consider to be unacceptable find behavior as in prior five.

8.1.5 Summary

We introduced six different prior assumptions that are motivated both by standard practices in the creation of optimal choice designs and those that mimic the behavior of parameter estimates in actual choice experiments. We will use these prior assumptions to study the effects of misspecification of the prior on the efficiency and entropy of choice designs.

Studying the effect of the six different prior assumptions on the 3^3 design we see that the prior does impact the level balance of the resulting design. The further that a prior is from being equally spaced about zero, the further from level balanced the resulting design will be. When some attributes are more important the level balance of the design is also influenced.

Studying the effect of the six different prior assumptions on the $3^2 \cdot 4 \cdot 5$ design we see that again the prior does impact the level balance of the resulting design. The impacts of the prior assumptions behave similarly to the 3^3 design but deviations from level balance are generally more severe.

8.2 Effects of Misspecification of Parameter Values in Creating Optimal Choice Designs

We wish to study the effect of misspecification of parameter values on both the efficiency and entropy of choice designs. Tables 21 and 22 show us the results of evaluating 100 simulated designs created under each set of parameter value assumptions on each of the other five priors discussed in Section 8.1. We evaluate the mean efficiency under each prior assumption, represented as the true efficiency and also the relative efficiency (calculated as a percent of the efficiency of the design created under the true prior assumption). We also evaluate the standard deviation of the design efficiency under each of the prior assumptions. In additional we evaluate each design for its entropy under the true prior assumption and the remaining five prior assumptions. We also report the average standard deviation of entropy between choice sets for each design. This measure is calculated as the standard deviation of the entropy of each choice set in a design averaged over the 100 simulated designs. This measure allows us to determine how consistent the entropy of each choice set within a choice task is under misspecification of the parameter values.

We will begin by discussing the effect of misspecification of a prior for the 3^3 design:

Table 21 – Misspecification of Priors for a 3^3 in 30 Choice Sets of 3

		Prior One	Prior Two	Prior Three	Prior Four	Prior Five	Prior Six
Prior One Design	**Mean Efficiency**	17.1817 (100%)	8.5173 (71.75%)	5.1886 (51.62%)	5.2033 (51.79%)	4.9871 (51.88%)	6.7917 (63.58%)
	Std Efficiency	0.0388	0.3596	0.3794	0.3819	0.5406	0.3826
	Mean Entropy	32.9584	19.1818	13.3877	13.4354	12.8841	16.2526
	Avg Std Entropy	0.0000	0.2653	0.3014	0.3040	0.3225	0.2693
Prior Two Design	**Mean Efficiency**	13.8060 (80.35%)	11.8702 (100%)	8.5875 (85.44%)	8.5636 (85.24%)	8.2690 (89.76%)	9.4682 (88.63%)
	Std Efficiency	0.1839	0.0425	0.1185	0.1534	0.1334	0.0962
	Mean Entropy	32.9584	28.2190	22.2685	22.2080	21.5395	23.9626
	Avg Std Entropy	0.0000	0.1077	0.2368	0.2372	0.2865	0.1761
Prior Three Design	**Mean Efficiency**	11.6977 (68.08%)	10.4115 (87.71%)	10.0511 (100%)	5.9363 (59.09%)	7.4672 (77.68%)	9.4713 (88.66%)
	Std Efficiency	0.2196	0.1253	0.0486	0.1646	0.2032	0.1011
	Mean Entropy	32.9584	29.0177	27.6070	20.0125	23.2090	27.1227
	Avg Std Entropy	0.0000	0.1122	0.0899	0.2466	0.2644	0.1535
Prior Four Design	**Mean Efficiency**	11.7022 (68.11%)	10.4022 (87.63%)	5.9376 (59.07%)	10.0465 (100%)	7.4306 (77.30%)	7.5786 (70.94%)
	Std Efficiency	0.1902	0.1232	0.1830	0.0468	0.2039	0.1971
	Mean Entropy	32.9584	29.0369	20.0690	27.5942	23.1241	23.7508
	Avg Std Entropy	0.0000	0.1116	0.2449	0.0901	0.2643	0.1464
Prior Five Design	**Mean Efficiency**	11.7629 (68.46%)	10.4468 (88.01%)	7.4791 (74.41%)	7.4765 (74.42%)	9.6129 (100%)	8.9485 (83.77%)
	Std Efficiency	0.1853	0.1559	0.2049	0.2218	0.0670	0.1711
	Mean Entropy	32.9584	29.8151	23.5140	23.5468	27.7693	26.7804
	Avg Std Entropy	0.0000	0.0985	0.2366	0.2371	0.1432	0.1390
Prior Six Design	**Mean Efficiency**	12.5719 (73.17%)	10.3954 (87.58%)	8.9057 (88.60%)	6.1395 (61.11%)	7.6900 (80.00%)	10.6826 (100%)
	Std Efficiency	0.2067	0.1603	0.0873	0.2777	0.2507	0.0372
	Mean Entropy	32.9584	27.5320	24.4358	18.5873	21.8761	27.9859
	Avg Std Entropy	0.0000	0.1282	0.1932	0.2883	0.2408	0.0806

Table 22 - Misspecification of Priors for a $3^2 \cdot 4 \cdot 5$ in 40 Choice Sets of 4

		Prior One	Prior Two	Prior Three	Prior Four	Prior Five	Prior Six
Prior One Design	**Mean Efficiency**	15.6526 (100%)	6.2722 (52.34%)	3.1372 (31.01%)	2.5572 (26.28%)	4.3697 (40.46%)	5.3272 (54.93%)
	Std Efficiency	0.0024	0.3909	0.3819	0.3631	0.4967	0.4030
	Mean Entropy	55.4518	25.9590	15.4779	12.7727	19.7497	24.3175
	Avg Std Entropy	0.0000	0.3388	0.3405	0.3195	0.3252	0.2916
Prior Two Design	**Mean Efficiency**	13.0652 (83.47%)	11.9835 (100%)	7.7184 (76.30%)	3.9127 (41.21%)	7.2257 (66.91%)	8.4132 (86.76%)
	Std Efficiency	0.0988	0.0477	0.2674	0.3016	0.2781	0.1344
	Mean Entropy	55.4518	49.0403	33.8487	21.0868	32.4775	38.3744
	Avg Std Entropy	0.0000	0.0817	0.3081	0.3546	0.3008	0.2081
Prior Three Design	**Mean Efficiency**	11.1001 (70.92%)	9.7466 (81.33%)	10.1161 (100%)	2.7589 (28.35%)	5.7161 (52.93%)	7.1545 (73.78%)
	Std Efficiency	0.1548	0.1509	0.1423	0.3256	0.2894	0.2049
	Mean Entropy	55.4518	48.3339	48.1643	18.8368	31.9170	39.4124
	Avg Std Entropy	0.0000	0.1474	0.1098	0.3460	0.3048	0.2506
Prior Four Design	**Mean Efficiency**	10.6844 (68.26%)	6.5671 (54.80%)	2.7459 (27.14%)	9.7300 (100%)	5.8745 (54.50%)	5.4441 (56.14%)
	Std Efficiency	0.1423	0.2739	0.3463	0.1134	0.2003	0.1448
	Mean Entropy	55.4518	36.9900	18.9642	49.1797	35.8755	35.8187
	Avg Std Entropy	0.0000	0.2271	0.3222	0.0834	0.1964	0.1977
Prior Five Design	**Mean Efficiency**	11.7772 (75.24%)	9.0560 (75.57%)	5.0081 (49.51%)	3.3127 (34.05%)	10.7992 (100%)	9.0739 (93.57%)
	Std Efficiency	0.1216	0.1704	0.3738	0.2985	0.0963	0.1264
	Mean Entropy	55.4518	43.7072	27.5537	19.7890	48.6935	44.3938
	Avg Std Entropy	0.0000	0.2012	0.3525	0.3539	0.0911	0.1519
Prior Six Design	**Mean Efficiency**	12.3571 (78.95%)	7.8180 (65.24%)	4.3192 (42.70%)	2.8801 (29.60%)	8.0365 (74.72%)	9.6976 (100%)
	Std Efficiency	0.0707	0.1784	0.2015	0.3014	0.1866	0.0837
	Mean Entropy	55.4518	37.5566	24.1552	17.9572	37.1793	43.1140
	Avg Std Entropy	0.0000	0.2426	0.3445	0.3019	0.2650	0.1933

- Using the zero prior assumption results in extremely poor performance for any of the other parameter specifications (on average only 60% as efficient as the correct prior)
- The equal-spaced prior (prior assumption two) has the smallest impact on the relative efficiency of all the other decision strategies (they are approximately 85% as efficient as the true prior designs)
- Prior assumption five is the second most resistant to misspecification (Misspecification results in designs approximately 75% as efficient as the true design)

The remaining design strategies are less consistent in their impacts on the efficiency of designs created under other prior assumptions.

If we examine the impact on efficiency for misspecification of the prior in $3^2 \cdot 4 \cdot 5$ designs, we find the following results:

- The zero prior assumption results in very severe consequences if the prior is something other than non-zero. (These designs are only 30% as efficient as the designs created under the correct prior) Therefore the consequences of assuming no prior information are more severe than assuming a slightly incorrect prior.
- The equal-spaced prior assumption (prior assumption two) is the best overall assumption is one is not confident enough to make stronger assumptions regarding the priors

The remaining prior assumptions result in designs that are very inconsistent. They should not be considered is there is a chance that the true structure of the parameter estimates could be greatly different in shape or magnitude.

Misspecification of the prior does not have a large impact on the overall utility balance of the resulting choice designs (measured as entropy) for either the 3^3 design. The resulting designs are within a twenty percent of the entropy of the true design. For the $3^2 \cdot 4 \cdot 5$ design the impact of misspecification of the prior has a much larger impact on the entropy of the resulting design. The resulting design can have up to 70% less entropy that the design created under the correct prior assumption. This indicates that the decisions being made are much simpler. This is seen especially when the true prior is prior four and any other prior is specified for the creation of the choice design.

8.3 Summary and Discussion

From our study of the effects of misspecification of prior for the creation of choice designs we make the following recommendations:

- When the design is easily balanced (in terms of the number of attribute levels and alternatives per choice set):
    - Specifying a zero prior is not a recommended practice unless one is absolutely unable to make any conjectures concerning at least the relative attractiveness of attribute levels
    - The effect of misspecification of an informative prior on the efficiency of the true design are not very severe
    - The effect of misspecification of the prior on the utility balance (entropy) of the true design are not very severe
- When the structure of the design is not balanced (in terms of the number of attribute levels relative to the number of alternatives per choice set):

- The effects of misspecification of a prior on the efficiency of the design when one is not at least certain of the rank ordering of the levels or the shape of the prior can be very severe.
- Misspecification of a zero prior results in the worst performance overall (26 – 55% of potential efficiency)
- The impact of misspecification on the entropy of the design is equally severe as those to the efficiency of the design
- Assuming prior four when it is not true has the second most severe impact on the efficiency of the design
- None of the other priors are able to create efficient designs if prior four coincides with the true parameter levels (This may be due to the much larger magnitude of the assumed parameter values in this prior)

From the result of our analysis we find that making a simple assumption (prior assumption two) concerning the prior results in designs that are most resilient to misspecification of the prior. One should not make assumptions concerning a change in shape of the prior (prior assumptions five and six) unless one if very confident that this is the true behavior. The designs created under these assumptions do not perform well under any of the other prior assumptions.

9. Decision Strategies and Optimal Choice Designs

We earlier introduced four non-compensatory decision strategies – the conjunctive strategy, disjunctive strategy, minimax strategy and lexiographic strategy (Shugan 1980). The majority of methods used for the design and analysis of choice experiments assume that consumers make compensatory decisions – they use the available information from all attributes and alternatives to formulate their decision. This is aligned with the axiom of utility maximization that is fundamental to choice theory. Behavioral research has shown that participants do not always behave in a compensatory manner (Shugan 1980). Given that consumers may enter different decision making strategies in the course of an experiment (Swait, et al. 2001), can we create designs that are efficient for each strategy? Further, what are the consequences of misspecifying the decision strategy in creating a choice design? The minimax strategy is not easily represented through the manipulation of the prior assumption in the creation of efficient designs and is not studied as deeply in the literature as the other decision strategies. Therefore we will not consider it in our analysis.

9.1 Decision Strategies and Prior Assumptions

We will discuss which prior assumptions may be appropriate for the creation of designs in which decision makers are assumed to use a particular non-compensatory decision strategy. Although we use tools for analyzing the results of choice experiments that assume consumers use a compensatory decision strategy, the use of priors that reflect the real decision strategies may minimize the loss of efficiency resulting from switching decision strategies.

9.1.1 Compensatory Decision Making

The compensatory decision making strategy assumes that all information available in the choice set is taken into consideration during the choice process. It assumes that all attributes and levels are considered in the decision making process. Any of the priors discussed in Chapter 8 could represent an accurate prior for a compensatory decision making strategy. There are no restrictions for assuming a prior in the construction of a choice design where individuals are assumed to use the compensatory decision making strategy for the choice task.

Based on our findings in Chapter 8 we should use any available information in formulating our priors for creating the choice design. If we are not confident in our knowledge of anything beyond the ranking of the levels for an attribute, then the equal-spaced prior assumption is the best choice. The consequences of misspecification of the prior assumption for the compensatory decision strategy are worst when the shape of the prior is misspecified.

9.1.2 Conjunctive Decision Making Strategy

The conjunctive decision making strategy assumes that a consumer eliminates from consideration all alternatives that contain attribute levels that do not meet a predetermined minimum level of acceptability. Let us consider the parameter estimates that would result if this decision strategy were being employed with one of the attribute levels being unacceptable. The parameter estimate of the unacceptable level would result in utility much lower than that of the remaining levels. This behavior is similar to that studied in prior assumptions five and six in Chapter 8. These are the only priors whose shapes align with the parameter estimates resulting from this decision strategy.

Based on our results in Chapter 8 we know that the consequences of misspecification of a prior with a distinct non-linear shape (like those appropriate for the conjunctive decision making strategy) are more severe than specifying a simpler prior to create the choice design. Therefore we should only specify a prior reflective of the conjunctive decision making strategy if we are sure that this strategy will be employed for the entire choice task (or that one level is extremely unattractive for those using a compensatory strategy). Further, if we specify a prior reflective of the conjunctive decision making strategy in creating our choice design we are assuming that all people participating in the choice task will use this prior. Again, this is not likely a true assumption.

9.1.3 Disjunctive Decision Making Strategy

The disjunctive decision making strategy assumes that the consumer decides which attributes are most important and then picks the alternative that has the most attractive level for those attributes. If we consider the parameter estimates that might result from a disjunctive decision making process, we would have one parameter that is of greater magnitude than the others in the study. This could be represented by priors three and four from Chapter 8, however these would not be the only way to specify such a prior.

Based on the results from Chapter 8, we would not recommend specifying a disjunctive decision strategy when constructing a choice design, the consequences of misspecification of this prior are more severe than other priors. In addition, similarly to the conjunctive decision strategy, if we assume that a disjunctive strategy will be used by participants in a choice design we are assuming that all participants will use this strategy at all times, a very unrealistic assumption.

9.1.4 Lexiographic Decision Making Strategy

The lexicographic decision strategy assumes that the consumer ranks the attributes in order of preference and then selects the alternative that ranks the highest on the most important attributes. The prior assumption for this decision making strategy would have large magnitude for the most important attribute, slightly smaller magnitude for the second most important attribute and continuingly decreasing in magnitude priors for the remaining attributes.

Similar to the problems with specifying a disjunctive decision strategy, there are several problems with assuming a lexicographic decision making strategy in the creation of a choice design. First, prior assumptions four and five from Chapter 8 would be one appropriate specification of the lexicographic decision strategy and misspecification of these priors were shown to be the most costly. Secondly, if we assume the lexicographic strategy in creating choice designs we are assuming that all participants will use this strategy at all times. Both of these assumptions could be costly in creating a choice design.

9.2 Summary and Discussion

We have reviewed several non-compensatory decision strategies that are recognized by the behavioral literature. For these strategies we have reviewed ways that the behavior of a participant making choices in this manner would be represented in the parameter estimates from a choice task. For these strategies we reviewed how specifying the prior assumptions associated with these strategies in the creation of a choice design can impact the efficiency of the choice design if the decision strategy is misspecified. We see that the conjunctive strategy is the only one where the benefits may outweigh the risk of misspecification if one is confident that the decision strategy will be employed.

Although we can specify priors that are reflective of a particular decision strategy, one needs to remember that the use of these priors to create the choice design assumes that everyone will use this strategy at all times in the choice task, likely an unrealistic assumption. Therefore we are likely better devoting our time to specifying appropriate priors (see Chapter 8) and creating choice tasks where participants will not use simplifying strategies in answering choice tasks.

10. Designs for the No-Choice Alternative

As discussed earlier Haaijer, Kamakura and Wedel (Haaijer 2001) discuss the most appropriate models to use when dealing with data that contain a no-choice alternative. In particular, the use of the multinomial logit, the no-choice multinomial logit and the nested-logit models are discussed. The results of the models are only accurate to the point that the data collected are appropriate for that type of model. Earlier work in design efficiency has shown that data that are efficient for estimating one model are not necessarily the most efficient for another model.

Before one can consider modeling the results of a choice analysis with the no-choice option, it becomes necessary to consider if the prevalence of no-choice responses are significant. Examining twenty sample data sets made available to us by Sawtooth Software Inc., nineteen offered the no-choice option as a valid response. Amongst the nineteen relevant data sets the minimum percentage of no-choice responses was 2.65% and the maximum number of no-choice responses was 50.71%. One average approximately 22.28% of the responses was no-choice. Losing approximately one quarter of the data intended to be collected in an experiment could have extreme implications for the results of the analysis, especially if there is a systematic reason behind occurrence of no-choice responses in the study.

10.1 Why the No-Choice Alternative is Selected

If we can understand why the no-choice option is selected by respondents then we can try to protect against it in our choice experiments. Possible systematic causes of the no-choice option may include:

1. A learning curve

   The no-choice option may occur more frequently in early choice tasks due to unfamiliarity with the task at hand or searching for a better choice

2. A Fatigue Effect

   As the task progresses, if it is too long, respondents may become fatigued with the task and select no-choice for this reason

3. A difficulty correlation

   It may be that the selection of the no-choice alternative has to do with the difficulty of the choice task. The difficulty of the choice task can be measured through a measure of entropy.

If the presence of the no-choice selection is believed to be systematic, special care should be taken in the analysis of the data from the choice task. If information can be gained on the prevalence of the no choice response prior to the choice task (perhaps through a pilot study or based on other studies) then the final experiment can be designed with the no-choice alternative being taken into consideration.

Another consideration to be taken into account regarding the no-choice response is the purpose of the choice experiment. Consider the following example from Sawtooth Data Set I: each participant is presented with twenty choice sets to evaluate. Figure 22

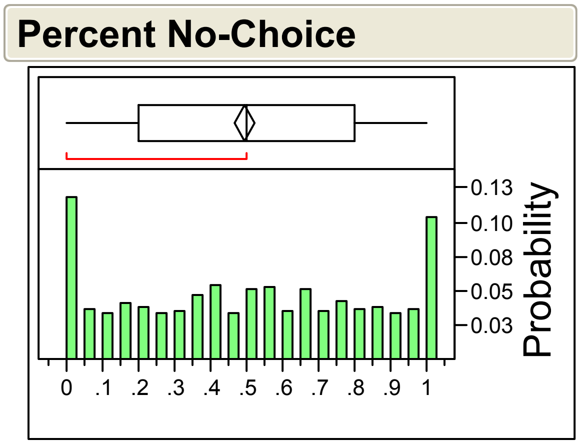


Figure 22 - Percent of No-Choice Responses

illustrates the percent of each participant's responses that were no-choice for this experiment. We see that nearly an equal number of participants never responded to any choice set (approximately 11%) as those who responded to every choice set (approximately 13%). In considering how and why to use the no-choice responses from the experiment, one must consider what we are hoping to learn from the experiment. There are several different options:

- What are the preferences for this product among all consumers in the marketplace? (Including those who will never purchase the product, or are not interested in replacing their current product)
- What are the preferences for this product amongst the engaged consumers in the marketplace? (Just those consumers who will purchase the product or who will replace there current product in the near future)

If we are interested in studying preferences among all consumers in the marketplace, then behavior such as that illustrated in this example is acceptable and should be modeled in the experiment. If we are only interested in studying engaged consumers for our product then behavior such as that seen in this example creates concern. First, we have targeted the wrong audience for our experiment and potentially incurred much more cost than necessary. Secondly, we now have to consider how to use the large amount of no-choice data in modeling the results. For example, the people who never responded are obviously disengaged from the choice task. But what about the participants who only responded to ninety, eighty or seventy percent of the tasks? Are they completely disengaged from the process or did they just not find the offerings in the choice sets they did not respond to engaging? These types of questions become very challenging as one approaches the analysis of the experiment. Thus, in the planning stage of the experiment it is very important to consider the desired use and application of the no-choice alternative. Further, the no-choice multinomial logit model only accounts for no-choice responses due to unattractiveness of the other alternatives, not for other reasons such as difficulty of the choice.

Once we decide the purpose of our experiment, and the inclusion of the no-choice alternative is confirmed, it becomes necessary to understand when and why consumers select the no-choice alternative. Understanding these reasons serves three purposes:

- In the design phase we can attempt to reduce the propensity of consumers to select no-choice, thereby maximizing the information collected from the experiment
- In the analysis phase we can control the effect of the no-choice alternative by modeling the reasons for the no-choice as covariates
- To make the design robust to the selection of the no-choice alternative

By combining these two ideas we can ensure that we achieve maximum precision in the collection and analysis of information for our experiment.

The behavioral literature on the no-choice alternative is fairly extensive and there are many documented reasons that the no-choice alternative becomes preferred in given situations. Dhar indicates that consumers are more likely to select the no-choice alternative when there is high conflict in the choice set, i.e. if all or most of the alternative within the choice set are equally balanced in preference (utility balanced) then the decision becomes more complex and the no-choice alternative becomes preferred. Dhar and Nowlis (1996) also indicate that time pressure in the choice task leads to deferral of choice in many contexts, however this conflicts with the logic of the no-choice logit model. Dhar and Sherman (1996) find that the willingness to choose is greater for unique good sets than for unique bad sets (even when matched for overall attractiveness).

Dhar and Nowlis (2004) show that there are two steps to the consumer decision making process, whether or not to buy and what to buy; however the order of these two decisions is not fixed. Luce (1996) finds that the readiness to choose is impacted by the ease of tradeoffs amongst alternatives in the choice set. Dhar (1997) also finds that difficulty in selecting only one alternative leads to choice deferral and further the tendency to defer is greater when differences in attractiveness are small versus when they are large. In addition, the preference for no-choice increases with the introduction of a new alternative that is relatively equal in overall attractiveness and the preference for no-choice decreases with the introduction of a new alterative that is inferior to the existing choices.

The literature in general shows that when the complexity of the choice increases so does the propensity towards the no-choice alternative in the choice set. Therefore, the complexity measures discussed earlier may also impact the propensity towards selecting the no-choice alternative in the experiment. We will discuss those measures and their impact on the selection of the no-choice alternative in the next section.

10.2 Decreasing the Propensity of No-Choice Alternatives

Every time the no-choice alternative is selected in a choice experiment there is a loss of information and a decrease in efficiency for the experiment as a whole. Reducing the prevalence of the no-choice selection will increase the information collected from the experiment. In addition, given the large number of reasons for the no-choice alternative being selected if we can eliminate all but those resulting from truly not preferring the other available alternatives then we will be able to better model and understand the data. In a traditional choice experiment there are no-choice answers resulting from fatigue, start-up unfamiliarity, complexity of the choice and non-attractiveness of the other alternatives. When we model the data we would benefit from knowing why the no-choice alternatives were chosen, and generally the only assumption concerning the no-choice alternative in modeling is that the other alternatives in the choice set are unattractive.

We earlier identified several measures of complexity for choice sets. We now examine whether these complexity measures and the order of the choices have any impact on the percent of responses that were the no-choice alternative. We evaluate these complexity measures and their impact on the no-choice alternative for eleven sample data sets using a random effects logistic regression model (ID is the random effect and the complexity measure of interest is the fixed effect). A summary of these data sets is presented in Table 23.

10.2.1 Entropy

The entropy of a choice experiment has been discussed as a measure of complexity shown to impact the amount of variability in individual's responses to choice tasks. Since entropy is considered to be a measure of choice task complexity one would assume that as the entropy of a choice set increases the propensity to select the no-choice alternative will also increase. We examine our eleven sample data sets and fit a model to

Table 23 - Summary of Sample Data Sets

Data Set	Attributes	Number of Alternatives besides no-choice	Number of Choice Sets per Participant	Number of Participants
A	$2 \cdot 3 \cdot 4^2$	2	12	136
B	$2 \cdot 3 \cdot 4^2$	2	12	110
C	$3^2 \cdot 4 \cdot 5$	4	10	539
D	$3^2 \cdot 4^2$	4	10	92
E	$3 \cdot 4 \cdot 5$	3	10	400
F	$2^3 \cdot 4 \cdot 5 \cdot 6$	3	15	250
G	$5 \cdot 8 \cdot 9$	5	12	1202
H	$5 \cdot 6 \cdot 9$	5	12	1181
I	$2^2 \cdot 3^3 \cdot 6$	3	12	50
J	$4^3 \cdot 5^2 \cdot 6$	3	20	586
K	$3 \cdot 4 \cdot 8$	3	10	270

determine if there is a relationship between the response, choice or no-choice, and the entropy of the choice set. We find conflicting results in our analysis. In the seven experiments where the model was estimable, five found that an increase of one in entropy decreased the probability of no-choice responses by a factor of .17 to .8 (data sets B, C, F, H and K) and two found that an increase of one in entropy increased the probability of no-choice responses by a factor of between 1.4 and 2.5 (Data sets E and G). The results of this analysis can be seen in Appendix Three. Based on earlier work using entropy as a measure of complexity (Swait, et al. 2001) we would have expected an increase in entropy to result in an increase in the selection of the no-choice alternative.

The results of this study are inconclusive; we cannot say for sure that increasing the entropy of a choice set will increase the propensity of the no-choice alternative. One potential problem with our analysis of entropy is that we have used the actual parameter estimates of a main effects only model to estimate the entropy of a choice set. This may not be a valid assumption for many of these data sets; however, we have no further information with which to evaluate the model assumptions. Entropy still remains a useful measure of complexity for a choice experiment as it has been shown to increase the variability in the responses, but was not a useful measure for predicting the probability of a no-choice response in these choice experiments with only 10 – 12 choice sets.

10.2.2 Number of Tradeoffs

The number of tradeoffs has been proposed as a measure of choice task complexity. To date it has not been shown to have any conclusive impact on the results of a choice experiment. As discussed earlier the number of tradeoffs is not without flaws as a measure of choice task complexity. Of the eleven choice experiments available to analyze with a no-choice alterative only two of the models were estimable. In both of those models, the addition of a tradeoff in the experiment decreased the probability of the no-choice alternative (Data sets D and F). (The results of this analysis can be seen in Appendix Three).

The distribution of the number of tradeoffs in a choice experiment is a fairly fixed number, given that the number of attribute levels generally exceeds the number of alternatives in the choice experiment. Therefore, the number of tradeoffs in a choice experiment is only viable as a measure of task complexity when there are attributes with a number of levels less than or equal to the number of alternatives in the choice set.

10.2.3 Magnitude of Tradeoffs

The magnitude of tradeoffs has also been proposed as a measure of choice task complexity. Examining our eleven choice experiments, we find that only four are capable of estimating the necessary logistic regression model. In three of these experiments an increase in the magnitude of tradeoffs for the choice experiment results in an increase in the probability of no-choice alternative selection (Data Sets G, J and K) and in the last experiment an increase in the magnitude of tradeoffs resulted in a decrease in the probability of the no-choice alternative (Data set D). (The results of this analysis can be seen in Appendix Three). We expect that an increase in the magnitude of tradeoffs to result in an increase in the probability of no-choice due over the limited range of values for the magnitude of tradeoffs seen in these studies. For the one experiment with the counterintuitive result, a $3^2 \cdot 4^2$ in 10 choice sets of size four, there is little potential variation in the number of tradeoffs since there are at most a number of levels equal to the number of alternatives in the choice experiment. Since choice designs tend to have little overlap in order to maximize the information collected from each choice set, we do not expect these results to carry over to more complex scenarios.

The three experiments with intuitive results (an increase in the magnitude of tradeoffs resulting in an increase in the probability of no-choice responses), a $5 \cdot 8 \cdot 9$ in 12 choice sets of size five, a $4^3 \cdot 5^2 \cdot 6$ in 20 choice sets of size three and a $3 \cdot 4 \cdot 8$ in ten choice sets of size three, there is more variability in the magnitude of tradeoffs as there are more levels than alternatives in the majority of the attributes. We have shown that increasing the magnitude of the tradeoffs in the choice experiment increases the probability of a participant selecting the no-choice alternative and the resulting loss of information from the choice made. These results are intuitive given the limited range of the magnitude of tradeoffs seen in these studies.

10.2.4 Number of Attributes

The effect of the number of attributes on the percent of no-choice responses within an experiment requires much more information than we have available for analysis. Figure 23 shows a scatter plot of the number of attributes in our eleven choice experiments we are analyzing and the percent of no-choice responses in the experiment. This plot shows that there is no significant relationship between the number of attributes in the choice experiment and the percent of no-choice responses by participants. Although this relationship is not significant it is not a reason to conclusively decide that the number of attributes is not a significant predictor of percent no-choice responses. We

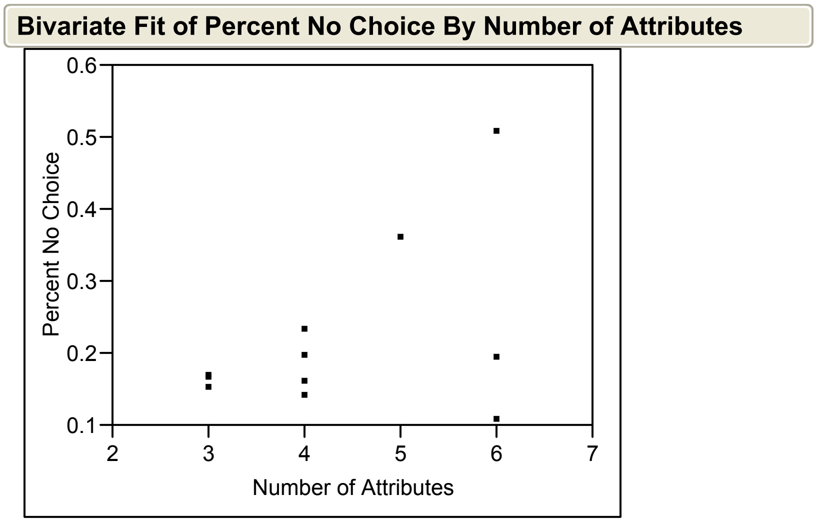


Figure 23 - Percent of No-Choice Responses by the Number of Attributes

cannot make a true determination here since there are many other factors that are confounded between these experiments. For example, we do not control for the number of alternatives in the experiment, the entropy of the choice tasks, or any of the other complexity measures that impact the percent of no-choice responses in the experiment. The number of attributes remains a viable measure of choice task complexity without a sure link to the selection of the no-choice alternative, but something that should be considered in the creation of a choice experiment.

10.2.5 Mean Standard Deviation of Attribute Levels within Each Alternative

The effect of the mean standard deviation of attribute levels within each alternative is evaluated as a predictor of the probability of a participant selecting no-choice alternatives within a choice set. As defined earlier the mean standard deviation of attribute levels within each alternative is defined as:

$$AverageSD_n = \left(\sum_{j=1}^{J_n} SD_{jn} \right) / J \,,$$

where SD_{jn} is defined as:

$$SD_{j\,n} = \sqrt{\left(\left[\sum_{i=1}^{K} (x_{ij} - \overline{x}_j)^2 \right] / K \right)} \,.$$

Of the eleven choice experiments evaluated for this analysis we find that nine result in estimable logistic regression models with choice / no-choice as the response variable and a fixed effect mean standard deviation of the attribute levels within an alternative and a random effect ID for respondents. Of these nine experiments six find that as the mean standard deviation of attribute levels with an alternative increases, the probability of a no-choice response decreases (Data set A, B, C, G, H, and K). The remaining three experiments find that as the mean standard deviation of attribute levels within an alternative increases, the probability of a no-choice response increases (Data sets E, F and K). (The results from this analysis can be found in appendix three.)

These results are inconclusive in terms of the direction of the effect of the mean standard deviation of attribute levels within an alternative on the probability of a no-choice response. Although DeSharzo and Fermo (2002) show that the mean standard deviation of attribute levels within an alternative is a viable measure of choice task complexity in that it increases the variance of the random component of utility, we are unable to show that it contributes to an individuals propensity to select the no-choice response within an experiment.

10.2.6 Dispersion of the SD of Attribute Levels within Each Alternative

DeSharzo and Fermo (2002) conclude that as the dispersion of the standard deviation of attribute levels within each alternative increases the variance of the utility also increases. The dispersion of the standard deviation of attribute levels within each alternative is defined as:

$$DispersionSD_n = \sqrt{\left(\left[\sum_{j=1}^{J_n}(SD_{jn} - AverageSD_n)^2\right] / J_n\right)},$$

where SD_j is defined as above. We hypothesize that as the dispersion of the standard deviation of attribute levels within an alternative increases, the propensity to select the no-choice alternative will also increase. Of the eleven data sets available for study we find that six result in estimable models. Of these six, five agree with our hypothesis that as the dispersion of the standard deviation of attribute levels within an attribute increases the probability of the no-choice alternative also increases (Data sets A, E, F, G and K). The last comes to the conclusion that as the dispersion increases the probability of the no-choice alternative decreases (Data set D). (The results of these analyses can be seen in Appendix Three). As mentioned when examining the magnitude of tradeoffs as a predictor of no-choice behavior, data set D is very simplistic in its structure. With only four attributes, each with only three or four levels, in four alternatives per choice set there is very little room for variability amongst the levels in an alternative. Therefore, the fact that its behavior is different from that of data sets A, E, F, G and K is not surprising. In all of these other experiments the context of the number of attributes, alternatives and attribute levels was far more complex. In conclusion, we find that in addition to being a good predictor of the variability in the utility of a choice, the dispersion of the attribute levels within an alternative it is also a good predictor of the probability if a no-choice response from a given choice set.

10.2.7 Mean Standard Deviation of Attribute Levels within Each Attribute

We now seek to evaluate whether the mean standard deviation of attribute levels within each attribute has an impact on the probability of no-choice responses in the choice experiment. Of the eleven available experiments, five result in an estimable model. Of those five, four find that as the mean standard deviation of the attribute levels within an attribute increase so does the probability of a no-choice response (Data sets G, H, J and K), while data set D again results in a counterintuitive result, that as the mean standard deviation of attribute levels within an attribute increases the probability of the no-choice response decreases. We again believe that the structure of data set D, as discussed earlier for the magnitude of tradeoffs and the dispersion of the standard deviation of attribute levels within an alternative, leads to this counterintuitive result. The results for these analyses may be seen in Appendix Three. We thus conclude that for data sets with a more complex structure, i.e. number of levels exceeding number of attributes, etc. that the mean standard deviation of attribute levels within an attribute is a good measure of the propensity to select the no-choice alternative in a choice experiment.

10.2.8 Dispersion of the SD of Attribute Levels within Each Attribute

We now seek to evaluate whether the dispersion of the standard deviation of attribute levels within each attribute has an impact on the probability of no-choice responses in the choice experiment. Of the eleven data sets available for exploration, we find that four result in estimable models with choice / no-choice as the response, dispersion of the standard deviation of the attribute levels within an attribute as a fixed effect and ID as a random effect. Of those three find that as the dispersion of the standard deviation of attribute levels within an attribute increases, the probability of a no-choice response also increases (Data sets D, G, and K), whereas the final data set A finds that as the dispersion of the standard deviation of attribute levels within an attribute increases, the probability of a no-choice response decreases. Data set A provides the counterintuitive result in this analysis, and this may be attributed to the more simplistic structure of the choice experiment. Data set A was a $2 \cdot 3 \cdot 4^2$ in choice sets of size 2, much simpler than the structure of all the other experiments with the exception of data set

D. The magnitude of the effect of the dispersion of the standard deviation of attribute levels within an attribute for data set A was also smaller than the corresponding increasing effects for the other three data sets. We therefore conclude that when the structure of the choice experiment is sufficiently complex, increasing the dispersion of the standard deviation of the attribute levels within an attribute results in an increased probability of the selection of the no-choice alternative (a .1 increase in the dispersion of the standard deviation of attribute levels within an alternative results in between a 5 and 20% increased possibility of selecting the no-choice alternative).

10.2.9 Choice Order within the Choice Task

For each choice experiment we divide the choices made by a participant into three classes, the early choices, the middle choices and the end choices. Earlier discussions have indicated that the order of choices is believed to have an impact on whether or not an individual makes a choice. Early choices are often impacted by learning or searching effects. Participants are trying to learn and understand the choice task and may elect not to choose as a result. At the end of the choice task an individual's willingness to choose is often impacted by a fatigue effect due to boredom with the choice task or the effects of the cumulative cognitive burden of the choice task as a whole. For each individuals choice task we create three indicator variables, the first that the choice set was in the learning phase (early), the second that the choice was in the middle of the experiment and the last that the choice was in the fatigue / burden stage (late). We use these variables to create a model where the response is choice / no-choice, there is a fixed effect choice order and a random effect ID. We see that this model is significant in eight of the eleven data sets (A, C, D, E, F, G, H and J). In each of these cases, a choice being in the learning stage of the experiment results in a decreased probability of the no-choice alternative being selected in the first time period as oppose to the second and third. As no-choice selections are most likely to be made in the learning stages of choice experiments, we should take care to insure that choice sets which are very important to the overall experiment, i.e. they contribute greatly to the efficiency of the experiment, should not be placed late in the choice task for an individual as they are more likely to selected as no-choice. Figures 24 and 25 give an example of the behavior exhibited in data sets C and G of the percent no-choice on a question by question basis.

10.2.10 Summary and Conclusions

Of the seven measures that have been introduced as measures of choice task complexity, we see that five illustrate conclusive (although not necessarily practically significant) results concerning the propensity of individuals to select no-choice alternative (the dispersion of the standard deviation of attribute levels within an alternative, the mean standard deviation of attribute levels within an attribute, the dispersion of the standard deviation of attribute levels within an attribute, the number of tradeoffs and the magnitude of those tradeoffs). The other two proven measures of task complexity, the mean standard deviation of attribute levels within an alternative and the entropy of the choice task, lead to inconclusive results concerning the propensity to select the no-choice alternative. In designing choice experiments when we have the flexibility to manage these measures as we assign choice sets to participants we should take care not

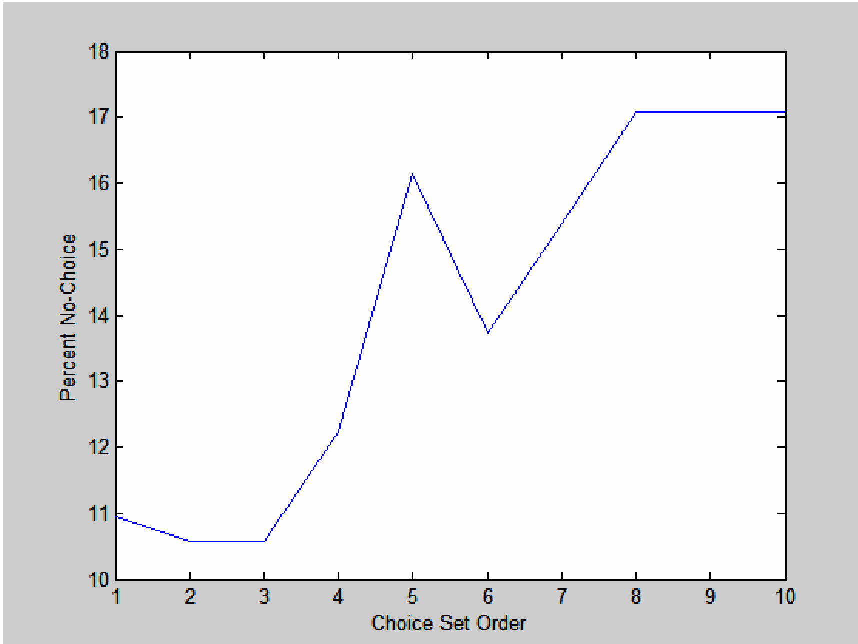


Figure 24 - Percent No-Choice by Question for Data Set C

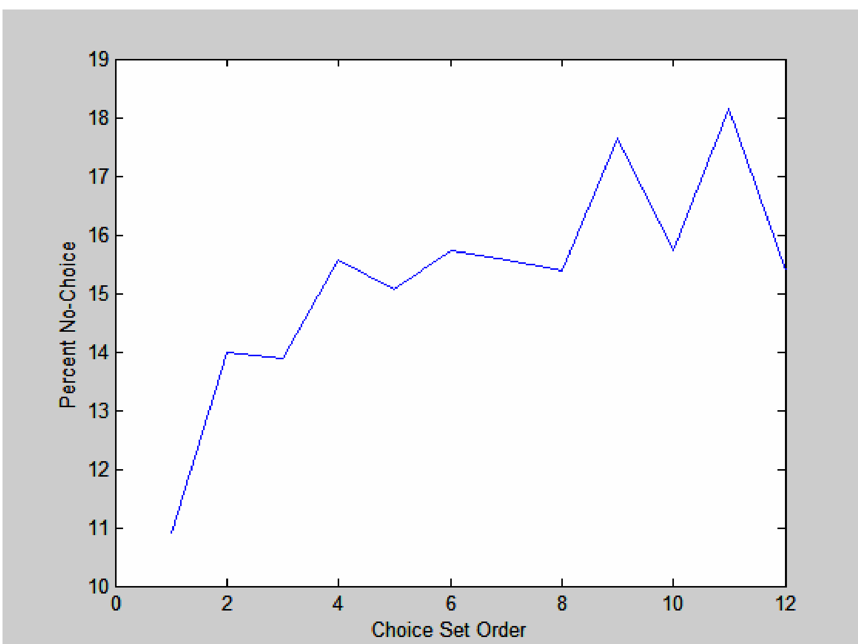


Figure 25 - Percent No-Choice by Question for Data Set G

to over burden participants by giving them choice sets that rank high on all the complexity measures that encourage no-choice responses. In addition, we have shown the placement of a choice set in an individual's overall choice task has an effect on their propensity to select the no-choice alternative. Therefore, we should attempt to refrain from placing choice sets with high complexity according to the other complexity measures late in an individual's choice task to attempt to reduce the likelihood of no-choice responses.

Additionally we should note that out evaluation of these measures of choice design complexity are based on observational studies. In order to come to more conclusive recommendations concerning the effects of these measures on the propensity towards selecting the no-choice alternative it is necessary to study their effect via experimentation.

10.3 Reducing the Severity of No-Choice Responses to Design Efficiency

We have shown that in choice experiments the selection of the no-choice alternative is very common. As previously discussed, we have seen that generally between 20 and 40 percent of the responses in a choice experiment will be for the no-choice alternative. Even in other experimental situations it may be known that the process under study is prone to missing observations. In choice designs we can distinguish between missing responses and the selection of the no-choice alternative, but this data is not often used in the analysis of the results. It is therefore important to be able to evaluate the resistance of a design to missing data points. We consider the impact of losing ten, twenty, thirty and forty percent of the intended data to either the no-choice alternative or non-response. We continue to work with four primary examples for evaluating choice designs: 3^3 in 9 choice sets of size 3, $3^2 \cdot 4^2$ in 10 choice sets of size 4 and $3^2 \cdot 4 \cdot 5$ in 10 choice sets of size four.

We note that the sample sizes evaluated in the following examples are smaller than we would normally see in applications (the size of the entire experiment would be much larger). The results here are more applicable when we are interested in being able to estimate models for each participant in the experiment.

Example: 3^3 in 9 choice sets of size 3

Consider an optimal 3^3 design in 9 choice sets of size 3 created using the %choiceff macro in SAS® with an equal-spaced prior for β. We examine the consequences of deleting ten, twenty, thirty and forty percent of the choice sets, attributing their deletion to the selection of the no-choice alternative in the experiment. For each scenario we calculate the number of choices expected to be no-choice, for example with 22% missing data, two of the nine choice sets will be assumed missing, and then compute the D-efficiency for all possible combinations of the original nine choice sets into designs with only seven choice sets.

The original design has a D-efficiency of 3.51 and is shown in Figure 26 by the red line. We observe that the larger the percentage of data assumed to be missing, the worse the D-efficiency of the design becomes. We also notice that there can be considerable variability in the D-efficiencies of the designs constructed of all combinations of the remaining choice sets. In some cases, the resulting designs may not even be capable of estimating the required parameters of the model. Figure 26 shows these results.

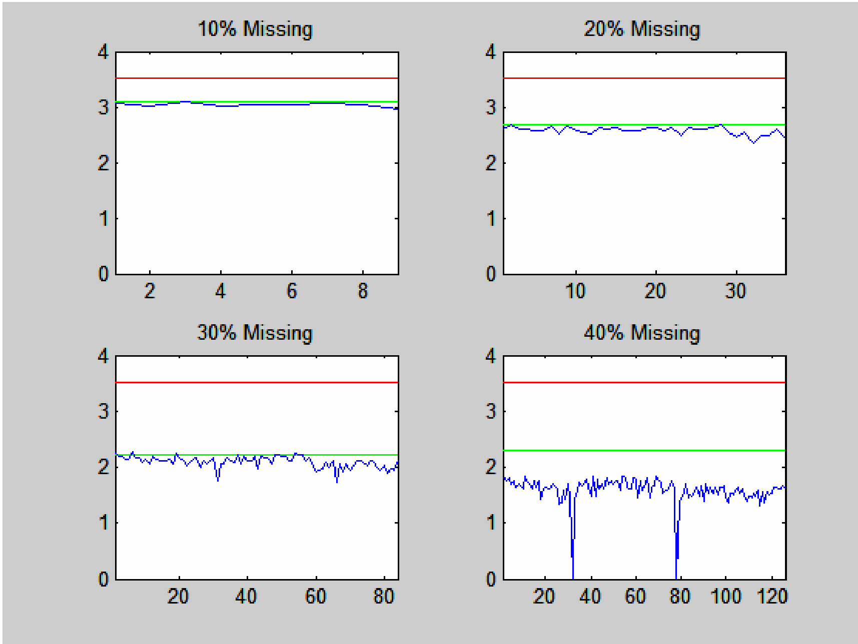


Figure 26 - 3^3 in 9 Choice Sets of Size 3

We also consider the D-efficiency of the optimal designs created by SAS® for the cases with 11, 22, 33 and 44% missing data. The results are presented in the Table 24. We notice that in some cases that the designs resulting from deleting the appropriate number of choice sets exhaustively do nearly as well as those created by SAS®, the 11 and 22% missing cases, whereas in other cases the optimal designs created using SAS® are far superior, e.g. the 44% missing case.

Example: $3^2 \cdot 4^2$ in 10 choice sets of size 4

Consider an optimal $3^2 \cdot 4^2$ design in 10 choice sets of size 4 created using the %choiceff macro in SAS® with an equal-spaced prior for β. We examine the consequences of deleting ten, twenty, thirty and forty percent of the choice sets, attributing their deletion to the selection of the no-choice alternative in the experiment. For each scenario we calculate the number of choices expected to be no-choice, for example with 20% missing data, two of the ten choice sets will be assumed missing, and then compute the D-efficiency for all possible combinations of the original ten choice sets into designs with only eight choice sets.

The original design has a D-efficiency of 2.56 and is shown in Figure 27 by the red line. We observe that the larger the percentage of data assumed to be missing, the worse the D-efficiency of the design becomes. We also notice that there can be considerable variability in the D-efficiencies of the designs constructed of all combinations of the remaining choice sets. In some cases, the resulting designs may not even be capable of estimating the required parameters of the model. In the case of 30% missing choice sets the resulting designs have a range of D-efficiencies with the worst

Table 24 - 3^3 in 9 Choice Sets of Size 3

	11% Missing	22% Missing	33% Missing	44% Missing
Number of Missing Choice Sets	1	2	3	4
SAS Optimal Design D-efficiency	3.09	2.68	2.23	2.29

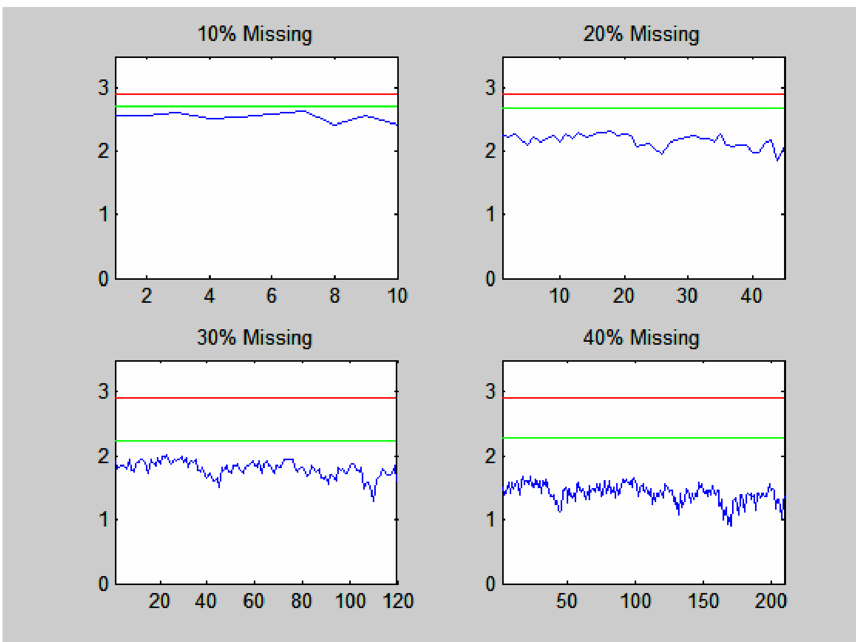


Figure 27 - $3^2 \bullet 4^2$ in 10 Choice Sets of Size 4

being only 64% as efficient as the best. This tells us that the consequences of loosing particular triplets of choice sets together can have catastrophic effects on the D-efficiency of the design, and that we should try to avoid losing these choice sets if at all possible. Figure 27 shows these results.

We also consider the D-efficiency of the optimal designs created by SAS® for the cases with 10, 20, 30 and 40% missing data. The results are presented in the Table 25. We see that the designs resulting from losing 10 – 40% of the choice sets are less efficient that the designs created using SAS®.

Example: $3^2 \cdot 4 \cdot 5$ in 10 Choice Sets of size 4

Consider an optimal $3^2 \cdot 4 \cdot 5$ design in 10 choice sets of size 4 created using the %choiceff macro in SAS® with an equal-spaced prior for β. We examine the consequences of deleting ten, twenty, thirty and forty percent of the choice sets, attributing their deletion to the selection of the no-choice alternative in the experiment. For each scenario we calculate the number of choices expected to be no-choice, for example with 20% missing data, two of the ten choice sets will be assumed missing, and then compute the D-efficiency for all possible combinations of the original ten choice sets into designs with only eight choice sets.

The original design has a D-efficiency of 2.7273 and is shown in Figure 28 by the red line. We observe that the larger the percentage of data assumed to be missing, the worse the D-efficiency of the design becomes. We also notice that there can be considerable variability in the D-efficiencies of the designs constructed of all combinations of the remaining choice sets. In some cases, the resulting designs may not even be capable of estimating the required parameters of the model. Figure 28 shows the

Table 25 - $3^2 \cdot 4^2$ in 10 Choice Sets of Size 4

	10% Missing	20% Missing	30% Missing	40% Missing
Number of Missing Choice Sets	1	2	3	4
SAS Optimal Design D-efficiency	2.70	2.46	1.95	1.70

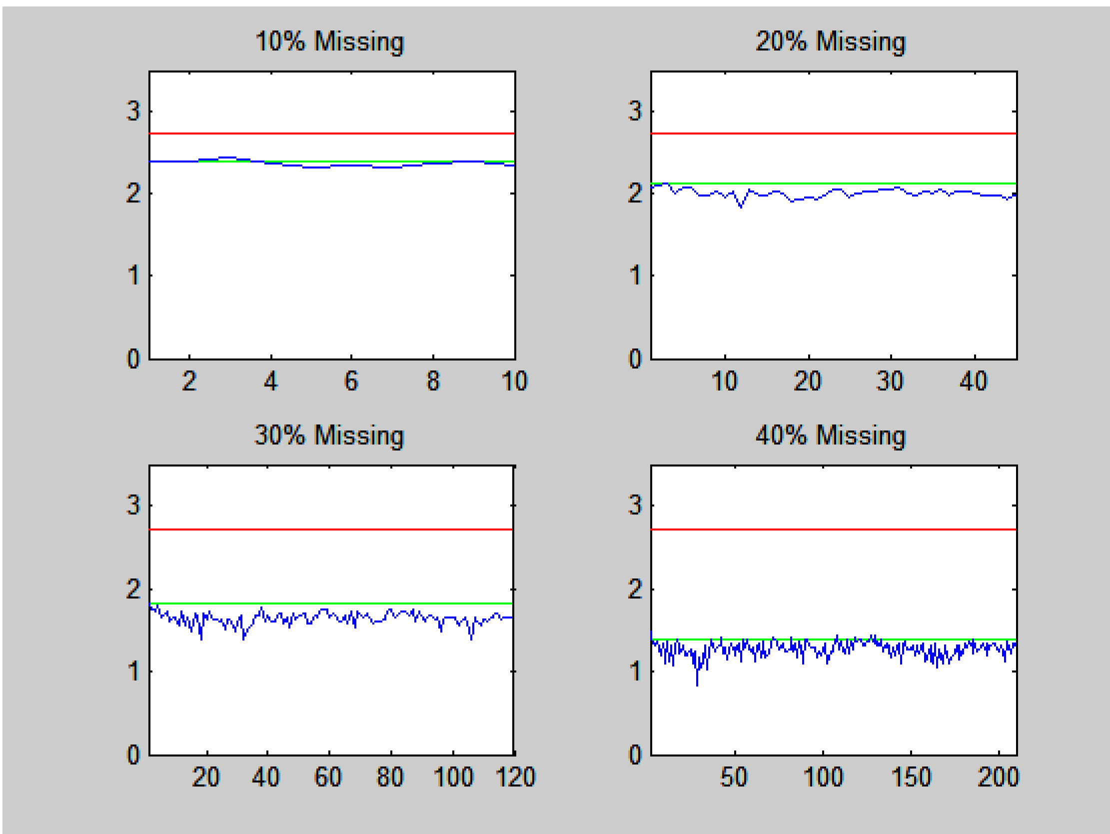


Figure 28 - $3^2 \cdot 4 \cdot 5$ in 10 Choice Sets of Size 4

results for 10, 20, 30 and 40% of the data missing. We also consider the D-efficiency of the optimal designs created by SAS® for the remaining choice sets above, the results are presented in the Table 26.

We notice that in some cases that the designs resulting from deleting the appropriate number of choice sets exhaustively do nearly as well as those created by SAS® (the 10, 20 and 30% missing cases).

10.4 Summary and Conclusions

We have shown that the effect of participants selecting the no-choice alternative in choice experiments can have significant impact on the D-efficiency of the remaining choices used for analysis. Taking this knowledge into consideration during the selection of a design may lead one to pick a design that is as resistant as possible to the missing data. We also know that participants are predisposed to select the no-choice alternative under certain conditions, for example based upon the difficulty of the task. Our analysis of the eleven sample data sets did not always support this, which may be accounted to our lack of knowledge concerning the choice data sets under study. For example in our analysis we assumed a main effects only model with all qualitative factors, which is likely an invalid assumption for many of the choice experiments. Earlier literature has shown that the incidence of the no-choice alternative seems to be somewhat correlated with the order of the choice task, and we support this conclusion with our work on the sample data sets.

We have also shown that there are several measures of choice task complexity that have an impact on the probability of a no-choice response from a particular choice

Table 26 - $3^2 \cdot 4 \cdot 5$ in 10 Choice Sets of Size 4

	10% Missing	**20% Missing**	**30% Missing**	**40% Missing**
Number of Missing Choice Sets	1	2	3	4
SAS Optimal Design D-efficiency	2.40	2.13	1.83	1.40

set. We can work with the timing and difficulty measures of these criteria to effectively place choice sets within a participant choice task to minimize the propensity of their selecting the no-choice alternative for a reason other unattractiveness.

The idea of certain choice tasks being more important to the experiment than others is useful beyond the scope of choice experimentation. Consider, for example, a manufacturing process that tends to be unstable immediately after any changes in setup. Consider an experiment being run in a split plot format where the changes that lead to the instability are found upon changing the whole plot factor. We would therefore wish to analyze the repercussions of loosing specific runs of our experiment within each whole plot group and ensure that the most costly runs to lose are not placed immediately after the changeover of the whole plot unit level.

11. Designing Choice Experiments

This chapter will discuss the steps that should be considered in the creation of a design for a choice experiment. We will start by discussing the recommended steps for a practitioner to take in the creation of a choice experiment, and conclude with an example.

11.1 Steps to Creating a Choice Experiment

The process of designing a choice experiment is not a simple one. It requires knowledge of the topic under study in addition to the statistical measures of design goodness that will assist in creating the most efficient and stable model at the conclusion of the choice task. Failure to consider any of the recommended steps in the creation of a choice experiment can result in a design that is not capable of answering the questions of interest.

Step One: Overview of Topic to be Investigated

This stage of the choice experiment is often the easiest for the practitioner charged with creating the choice experiment. Generally there are several subject matter experts who are capable of identifying the attributes of interest and their levels for the choice experiment. In addition to identifying the attributes of interest and their levels, we need to begin to develop the prior parameter estimates that will be used in the analysis. This need not be more than a simple ranking of the anticipated effects of attribute levels from most to least attractive, and if anything can be anticipated about the differences in magnitude between the attribute levels that knowledge should also be collected.

In addition to identifying the attributes, their levels and expected effects, it will also be necessary to rank these attributes in order of importance for the intended study. It may not be possible to study each and every attribute of interest so ranking the attributes will allow one to consider thoroughly the importance of each attribute to the intended results.

Step Two: Consider the Target Population

Considering the target population of the choice experiment is a very important step in creating a choice design. The following questions should be answered in conjunction with identifying the target population:

1. What do we want to learn from the experiment?

   Are we studying a group of consumers already engaged and knowledgeable about this product category or are we studying the entire potential population for this product, some of whom may be knowledgeable about the product and attributes and others who may have little to no knowledge concerning the product category. We may wish to estimate market shares amongst current users or amongst current and potential users. Our consideration of the target population may be linked to the stage of market development for the product. Are we studying our product to induce the majority to buy or are we attempting to create a new product where only the innovators and early adopters are in the market?

2. How engaged will the target population be?

   Once we understand our intended learning from the choice experiment, we can better understand the level of engagement that will be expected from our participants. For example if we are studying airline travel and we want to learn about the preferences of business travelers (from question one), then we know we very likely have a highly engaged and opinionated target population. This tells that we do not need to be as sensitive to the complexity of the choice task as the participants are likely going to be very interested in making their feelings known through the choice task. The opposite situation should also be considered. Assume we are interested in studying the market penetration of hybrid cars in the complete market of car buyers. There will be some people in this population who have very strong opinions about hybrid cars, such as car enthusiasts, people highly concerned with the environment or people seeking to maximize their gas mileage. On the other hand there will be people who have no opinions on hybrid cars, such as people who are not in the market to purchase a car, people who just view a car as a way to get from point A to point B or those without cars. In this case increasing the complexity of the choice task too much may result in some participants disengaging from the choice task.

3. Can the target population be assumed to be homogeneous?

   Once we understand the intended results from the experiment and how engaged the target population is, we need to decide whether we can assume that the target population is homogeneous. Decisions on the homogeneity of the population may influence the set-up of the choice task in later steps. With one homogeneous group we will just create one master choice task to be delivered to all participants. If we decide there are several heterogeneous groups within the target population creating the choice task becomes more complex process. First a way to identify the different groups must be decided upon; second we must decide whether or not each group will receive the same choice task. If all groups will receive the same choice task then a decision about a model capable of handling several heterogeneous groups must be made.

Considering and understanding the issues pertaining to the target population will provide clarity in making decision concerning the remaining steps of designing the choice experiment.

Step Three: Select the Number of Attributes and the Number of Attribute Levels

Although it would be desirable to use all attributes and all attribute levels for our choice experiment, in many cases it may be unrealistic to do so within the financial and time constraints. Using the ranking of attributes from step one and the input of subject matter experts, we can decide which attributes will be included in the experiment. Carson et al. (1994) indicate that they have been involved with choice experiments with between two and thirty attributes, with an average of about seven attributes per choice set being seen. They also note that generally as the number of attributes and levels in the choice experiment increase, other things such as the number of alternatives and the number of choice sets tend to decrease. In deciding on the number of necessary attributes, one should also start to consider the number of alternatives and choice sets that will be allocated per participant (see next step).

Step Four: Select the Number of Alternatives and the Number of Choice Sets

As mentioned above, considering the number of attributes in a choice experiment cannot be considered without simultaneously considering the number of choice sets and alternatives that will be in the experiment. Carson et al (1994) note that they have been involved with experiments that have between one and 32 choice sets and two to 28 alternatives per choice set. They indicate that the average number of choice sets found in experiments is four, with the average number of alternatives in each choice set also being four.

Selecting the number of alternatives and the number of choice sets should be considered in balance with the understood target population for the experiment. Again, although we need to balance the number of attributes, alternatives and choice sets, populations that are more engaged in the choice topic can handle more complex choice tasks. Selecting the number of choice sets and the number of alternatives may also be related to the anticipated delivery and reward system for the choice experiment. For example, to receive a choice task in the mail with twenty choice sets may seem more daunting than clicking through twenty choice sets on the internet, and if the reward for completing the choice task is nothing, participants are going to be less willing to participate unless they have strong opinions on the topic being studied.

In addition to considering the number of "experimental" alternatives in the choice task, now is also the time to decide if a constant alternative will be provided as part of the choice task. Remember that this constant alternative may be one fixed alternative, a statement of your current product or most commonly the option of none or no-choice. The presentation of this alternative also needs to be considered. For example if the no-choice alternative is selected to be in the experiment will it be presented as:

1. Select the no-choice alternative if you find none of the other options desirable, or
2. Select the no-choice alternative if you are unable to decide between the other alternatives.

The no-choice alternative may also be presented as a combination of these two, or something else entirely. The presentation of the no-choice alternative also becomes a consideration in the model that will be used to analyze the data.

Step Five: Select the Number of Participants

Selecting the number of participants in a choice experiment is often a financial decision. Obviously having more participants is desirable. The number of participants may also depend on the delivery system for the experiment, whether mail, internet or in person. One last thing to consider in deciding the number of participants is that, on average, choice experiments see between 20 and 40% of the responses being no-choice, something that can become problematic if you are running a small experiment with just enough data collection for estimating your model.

Step Six: Create Several Candidate Master Designs for Evaluation

Once we have answered all the questions in steps one through five, we need to begin creating designs to consider for our choice task. The first decision will be the model that will be used to analyze the data, and then we need to decide on the assumed parameter values for our attributes to be used in creating potential choice designs. Once we have decided the model and the priors we can use a design creation algorithm to create the candidate designs. These designs should all have the required number of attributes, attribute levels and alternatives and the number of choice sets should be the number of participants times the number of choice sets per participant.

Step Seven: Evaluate and Examine the Complexity Measures for Each of the Candidate Master Designs

Now that the candidate master designs have been created we should evaluate each choice set in the candidate designs on some of the following criteria:

- Mean Standard Deviation of Attribute Levels within an Alternative
- Dispersion of the Standard Deviation of Attribute Levels within an Alternative
- Mean Standard Deviation of Attribute Levels within an Attribute
- Dispersion of the Standard Deviation of Attribute Levels within an Attribute
- Entropy
- Number of Tradeoffs
- Magnitude of Tradeoffs
- Efficiency with the Choice Set Deleted

Once these complexity measures have been calculated for each choice set, we need to examine the distribution of these values for each candidate design.

Step Eight: Select the Final Master Design

In examining the distributions of the complexity measures for each of the candidate master designs we should identify the candidate design that has the best distribution of levels for each of the measures as seen below:

- Mean Standard Deviation of Attribute Levels within an Alternative – Smaller is more desirable
- Dispersion of the Standard Deviation of Attribute Levels within an Alternative - Smaller is more desirable
- Mean Standard Deviation of Attribute Levels within an Attribute - Smaller is more desirable
- Dispersion of the Standard Deviation of Attribute Levels within an Attribute - Smaller is more desirable
- Entropy - Smaller is more desirable
- Number of Tradeoffs – Larger is more desirable
- Magnitude of Tradeoffs - Smaller is more desirable
- Efficiency with the Choice Set Deleted - Larger is more desirable

Select the design with the best variation in each of the complexity measures.

Step Nine: Allocate Choice Sets to Participants According to Complexity Measures

Once we have selected the master design we need to allocate the choice sets to the participants of the experiment.

- For each complexity measure divide choice sets into groups according to the desired levels of the criterion

- Allocate choice sets to participant by balancing the levels of each complexity measure within each participants choices (each participant will receive a different collection of choice sets). If individual level estimation is desired, ensure that the choice sets allocated to each individual result in an estimable model.
- Order the choice sets for each participant by not placing the choice sets that have a large impact on efficiency in the beginning of a participant's task and the choice sets with the least desirable complexity measures also not in the beginning of the task.

Once these tasks have been completed we have a choice design that can be used to study our topic of consideration. This design should attempt to decrease the variability of estimates of utility and the propensity of the no-choice alternative being selected in the experiment.

11.2 Creating a Sample Choice Design

We will now implement the steps discussed above for the following choice task, based on data set J. This is a $4^3 \cdot 5^2 \cdot 6$ experiments in twenty choice sets of size three with five hundred and eighty-six participants. We will create a master deign for 29 participants and assume that they will be repeated for the remaining participants.

Step One – Step Five:

Since we are basing this study on data set J we will use the parameters for that experiment. We will assume that the population is well engaged in the topic of interest and that they are homogeneous. Further, we will assume that a constant alternative of no-choice is used in the experiment and that the instructions surrounding this alternative are to select it when the other alternatives are unattractive. We will also assume that we are using the Multinomial Logit model with main effects and two factor interactions to analyze this data and those equal-spaced priors have been assumed on the main effect parameter estimates.

Step Six:

We will use the SAS® macro %choiceff to generate four candidate master designs for this analysis. We use the equal-spaced prior assumption when generating the data sets.

Step Seven:

We evaluate each of the choice sets in each of the four master designs on seven different complexity measures, mean standard deviation of attribute levels within an alternative, dispersion of the standard deviation of attribute levels within an alternative, mean standard deviation of attribute levels within an attribute, dispersion of the standard deviation of attribute levels within an attribute, the magnitude of tradeoffs, the number of tradeoffs and the entropy. Figures 29 though 35 show the histograms of the distributions of the levels of these complexity measures. We notice that there are no significant differences between these efficient designs. Tables 27 though 33 identify the numerical summaries of these measures for each data set.

Step Eight:

We see that data set four has the most desired characteristics on the majority of the complexity measures. For this reason we select data set four as our final master design.

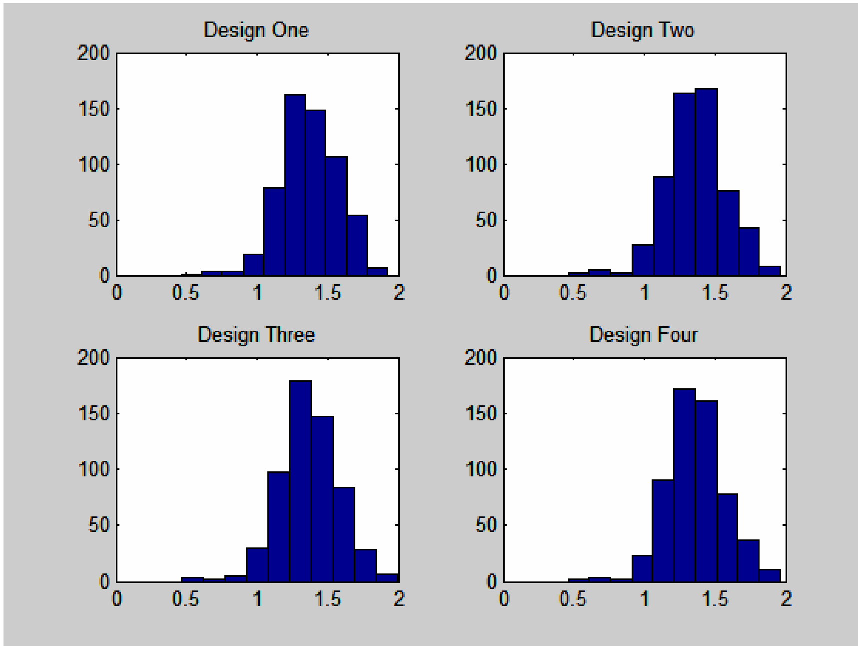


Figure 29 - Distribution of the Mean Standard Deviation of Attribute Levels within an Alternative

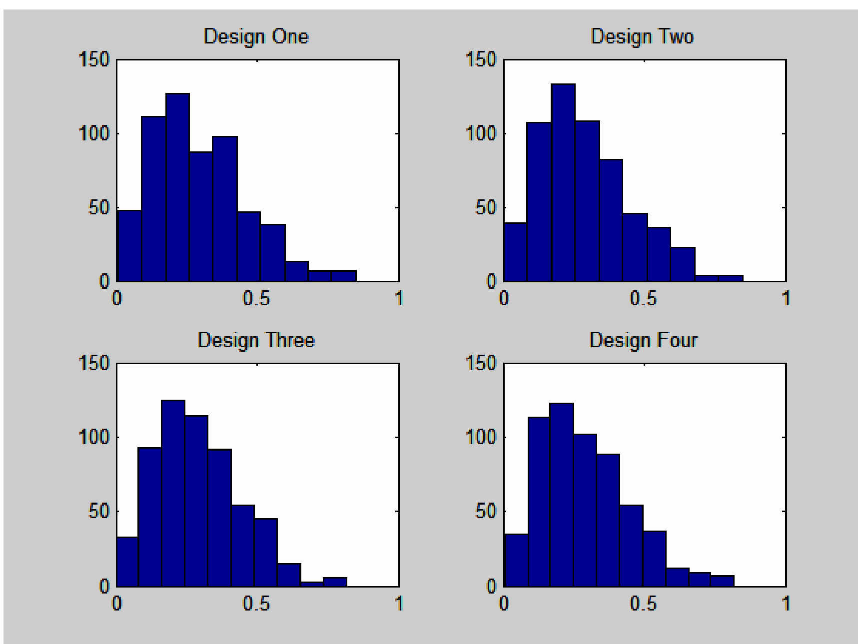


Figure 30 -Distribution of the Dispersion of the Standard Deviation of Attribute Levels within an Alternative

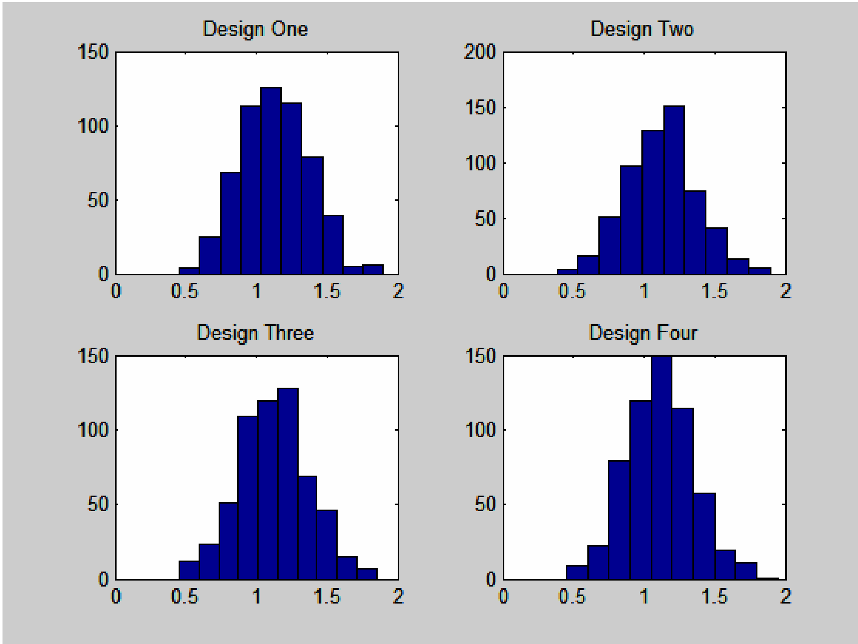


Figure 31 -Distribution of the Mean Standard Deviation of Attribute Levels within an Attribute

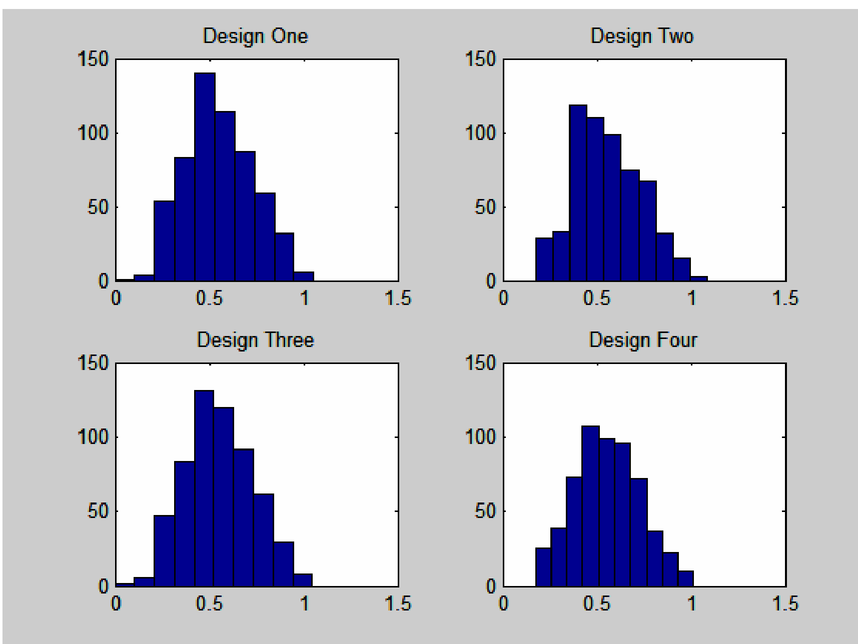


Figure 32 - Distribution of the Dispersion of the Standard Deviation of Attribute Levels within an Attribute

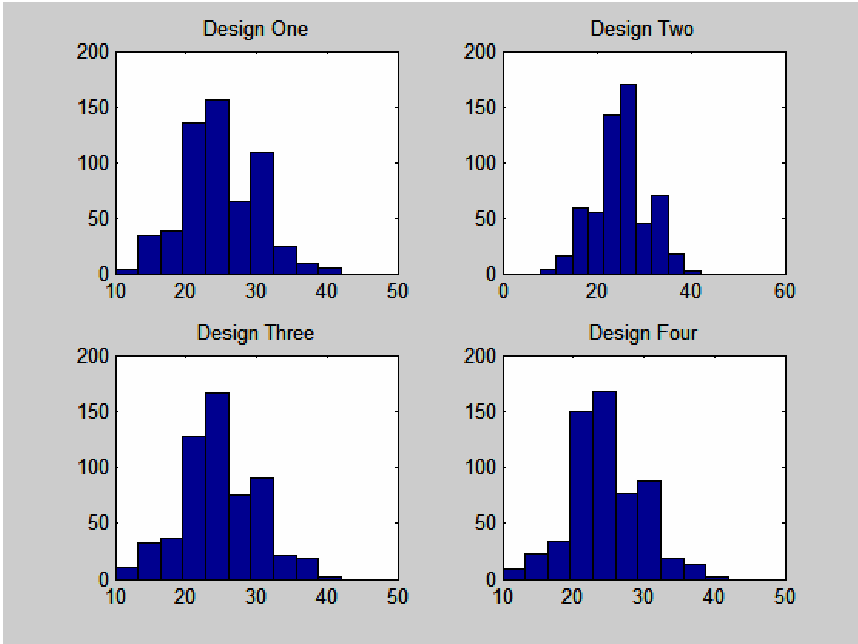


Figure 33 - Distribution of the Magnitude of Tradeoffs

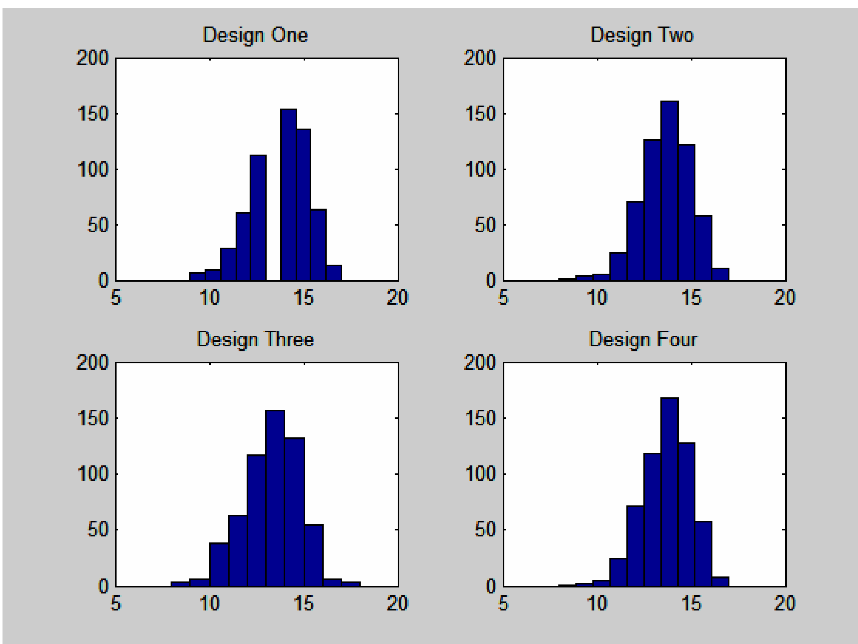


Figure 34 - Distribution of the Number of Tradeoffs

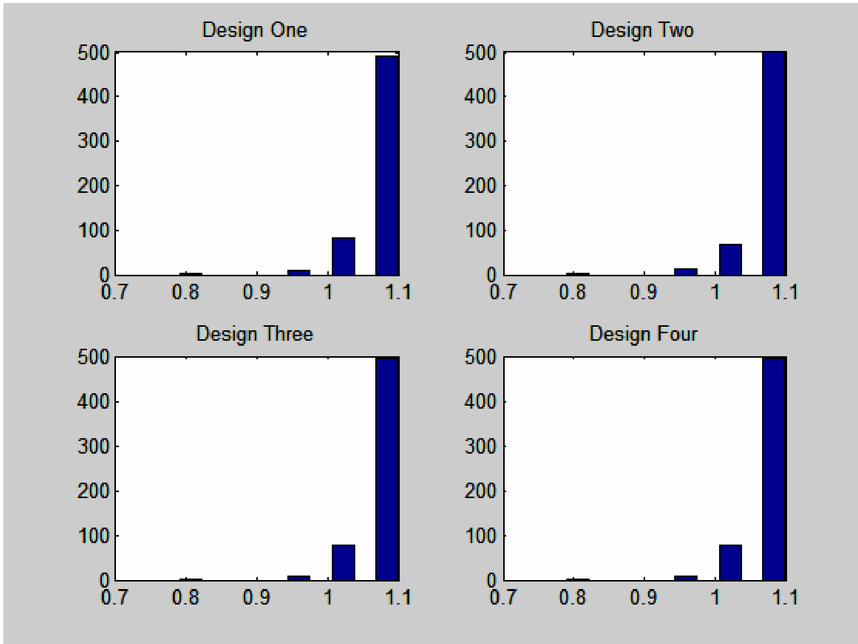


Figure 35 - Distribution of Entropy

Table 27 - Distribution of the Mean Standard Deviation of Attribute Levels within an Alternative

	D4	D3	D2	D1
Max	1.9578	1.9966	1.9645	1.92227
Q3	1.4863	1.5072	1.4908	1.5054
Med	1.3521	1.3533	1.3667	1.353
Q1	1.2344	1.2372	1.2417	1.2341
Min	0.4615	0.4615	0.4615	0.4615
Mean	1.3645	1.3677	1.3639	1.3647
Std	0.2059	0.2128	0.2105	0.201

Table 28 - Distribution of the Dispersion of the Standard Deviation of Attribute Levels within an Alternative

	D4	D3	D2	D1
Max	0.81847	0.81504	0.84766	0.84766
Q3	0.38144	0.38779	0.39425	0.39487
Med	0.266	0.26619	0.26223	0.26331
Q1	0.16891	0.17226	0.16907	0.17364
Min	0.00701	0	0	0.0079
Mean	0.2885	0.2882	0.2896	0.2959
Std	0.15797	0.1538	0.1584	0.161

Table 29 - Distribution of the Mean Standard Deviation of Attribute Levels within an Attribute

	D4	D3	D2	D1
Max	1.9465	1.849	1.8909	1.8971
Q3	1.279	1.279	1.2829	1.28293
Med	1.099	1.1224	1.1312	1.1289
Q1	0.96	0.9645	0.9645	0.9573
Min	0.4553	0.4553	0.3849	0.4553
Mean	1.117	1.124	1.1275	1.126
Std	0.2354	0.25	0.2459	0.2427

Table 30 - Distribution of the Dispersion of the Standard Deviation of Attribute Levels within an Attribute

	D4	D3	D2	D1
Max	1.0154	1.0487	1.0888	1.0559
Q3	0.6708	0.6758	0.6877	0.6708
Med	0.5347	0.5496	0.541	0.5369
Q1	0.4267	0.4267	0.4147	0.4258
Min	0.1725	0	0.1725	0
Mean	0.552	0.5542	0.5551	0.55199
Std	0.1747	0.1799	0.181	0.178125

Table 31 - Distribution of the Magnitude of Tradeoffs

	D4	D3	D2	D1
Max	42	42	42	42
Q3	28	28	28	30
Med	24	26	26	26
Q1	22	22	22	22
Min	10	10	8	10
Mean	24.8967	25.044	25.1114	25.1067
Std	5.24	5.598	5.5065	5.454

Table 32 - Distribution of the Number of Tradeoffs

	D4	D3	D2	D1
Max	17	18	17	17
Q3	15	15	15	15
Med	14	14	14	14
Q1	13	13	13	13
Min	8	8	8	9
Mean	13.82	13.777	13.8086	13.8586
Std	1.435	1.522	1.4609	1.5497

Table 33 - Distribution of Entropy

	D4	D3	D2	D1
Max	1.0986	1.0986	1.0986	1.0986
Q3	1.0744	1.0744	1.0744	1.0744
Med	1.0744	1.0744	1.0744	1.0744
Q1	1.0684	1.0684	1.0684	1.0684
Min	0.7906	0.7906	0.7906	0.7906
Mean	1.068	1.068	1.068	1.068
Std	0.0275	0.0297	0.03057	0.02739

Step Nine:

We now wish to allocate the 580 choice sets in our master design to 29 groups of 20 choice sets for our participants. To do this we identify the position of each choice set within each of the seven measures of complexity (see Table 34). We then identify the median position for each choice set from the positions for each of complexity measures. We then allocate the choice sets in 29 sets of 20 choice sets based on the median overall position (see Table 35). The final remaining task is to order the choice sets that an individual participant will see. The order shown in Table 35 places the choice sets from easiest to hardest. This is not a bad choice of ordering since we earlier established that the choices that occur in the early stages of the experiment have a greater probability of resulting in a no-choice selection. We may wish to reorder the choice sets in the middle of the experiment either in a randomized fashion (to preserve the statistical characteristics of the design) or we may elect to keep the order as is.

11.3 Summary and Conclusions

We have reviewed the proposed steps involved in the creation of efficient choice designs for stated preference experiments. We have shown that there is sufficient variability in the seven measures of choice set complexity to use them as a method to allocate choice sets to participants, ensuring that no one participant is over tasked in the course of the experiment.

Table 34 - Relative Position of Each Choice Set for Each Complexity Measure

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
1	383	254	166	457	131	33	245
2	574	560	578	497	572	222	85
3	149	269	111	172	67	223	246
4	23	255	179	397	132	34	247
5	554	514	532	547	506	35	248
6	95	106	128	40	68	104	249
7	21	465	210	91	133	105	9
8	146	70	86	97	69	389	250
9	430	385	459	291	460	516	86
10	43	349	51	29	33	224	251
11	109	343	247	357	216	106	87
12	421	387	435	556	383	36	88
13	354	457	487	405	461	225	89
14	313	30	264	402	217	226	252
15	489	34	119	34	70	227	253
16	324	179	88	24	71	390	90
17	156	53	20	251	20	37	36
18	482	333	558	460	566	573	254
19	520	83	574	46	573	391	473
20	527	450	224	352	218	392	255
21	407	470	507	94	462	107	256
22	213	29	248	219	219	393	91
23	160	57	42	368	34	38	2
24	29	319	60	44	35	108	92
25	295	187	89	316	72	228	10
26	171	176	56	122	36	109	257
27	450	395	413	245	384	394	474
28	46	578	54	567	37	9	258
29	321	332	75	359	73	110	259
30	490	375	517	335	463	111	260
31	292	116	183	265	134	229	93
32	96	107	265	562	220	39	261
33	406	414	227	491	221	230	475
34	271	280	246	301	222	395	94
35	184	40	64	343	38	10	95

Table 34 - Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
36	359	236	219	141	223	517	262
37	320	379	339	236	303	396	96
38	176	494	342	3	304	397	476
39	136	407	438	126	385	231	263
40	25	341	180	398	135	40	477
41	474	231	484	572	464	41	264
42	214	243	314	136	305	518	265
43	283	91	76	361	74	112	37
44	199	482	223	503	224	398	266
45	49	357	8	76	3	4	267
46	314	31	353	332	306	399	268
47	230	213	406	221	386	400	269
48	35	126	67	257	75	232	270
49	284	92	28	26	21	113	38
50	195	132	29	27	22	114	478
51	384	114	355	540	307	233	479
52	150	485	363	555	308	42	39
53	99	566	429	376	387	234	97
54	348	199	100	105	76	235	98
55	484	353	239	150	225	401	480
56	317	190	346	131	309	402	99
57	285	159	162	6	136	403	40
58	549	111	469	20	465	519	271
59	341	131	249	215	226	404	481
60	219	471	334	538	310	115	100
61	280	534	362	372	311	236	272
62	252	401	462	284	388	116	101
63	110	439	14	114	10	11	4
64	20	398	101	407	77	43	102
65	517	503	575	350	574	405	103
66	565	25	250	220	227	406	104
67	469	313	535	253	507	237	273
68	310	552	454	233	466	520	274
69	476	386	376	521	312	44	275
70	220	472	430	377	389	238	105
71	113	366	327	320	313	407	276

Table 34 – Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
72	388	117	12	228	11	12	106
73	32	473	46	293	39	45	107
74	397	400	234	384	228	408	277
75	336	27	57	124	40	117	482
76	68	44	129	183	78	118	278
77	391	277	431	536	390	239	108
78	337	351	379	516	391	240	109
79	395	250	90	23	79	409	110
80	174	403	202	156	137	241	483
81	465	361	358	479	314	242	279
82	451	8	184	81	138	243	11
83	394	208	181	83	139	244	280
84	580	93	235	484	229	119	281
85	417	512	328	446	315	120	484
86	526	356	526	524	508	410	111
87	152	175	7	60	4	13	41
88	27	320	9	77	5	5	282
89	236	36	87	98	80	411	12
90	559	335	301	565	316	121	283
91	471	390	392	404	392	245	485
92	88	412	286	208	230	246	112
93	185	41	422	509	467	412	284
94	234	139	38	15	41	413	42
95	269	6	33	197	23	46	113
96	24	256	22	9	24	247	114
97	60	576	98	566	81	47	115
98	208	506	298	459	231	122	13
99	333	491	271	310	232	48	43
100	124	136	112	322	82	49	285
101	374	32	47	103	42	248	286
102	428	100	544	318	509	249	116
103	129	11	240	147	233	414	117
104	89	337	71	10	83	521	14
105	162	292	367	117	317	250	287
106	2	5	2	17	1	6	15
107	577	97	102	408	84	50	486

Table 34 – Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
108	38	21	263	280	234	251	288
109	447	540	547	523	547	522	487
110	538	556	562	393	548	252	289
111	420	404	48	100	43	253	290
112	435	212	472	431	468	415	488
113	257	498	216	161	140	123	291
114	322	297	343	132	318	416	292
115	418	517	563	260	549	254	16
116	202	312	185	84	141	255	489
117	302	98	70	11	85	523	293
118	166	50	4	62	6	14	44
119	125	302	228	541	235	51	294
120	216	480	333	519	319	256	295
121	360	76	272	434	236	52	17
122	54	80	97	225	86	257	490
123	361	77	153	69	142	417	118
124	518	252	371	418	320	258	296
125	177	163	37	164	44	124	491
126	294	393	357	382	321	53	297
127	423	454	432	378	393	259	492
128	457	476	447	553	394	260	119
129	50	526	214	394	237	261	18
130	516	340	573	380	575	418	493
131	261	336	323	452	322	125	45
132	215	183	23	138	25	54	46
133	550	492	546	370	510	262	298
134	334	52	296	473	323	524	120
135	114	367	321	342	324	419	299
136	33	294	130	39	87	126	47
137	97	108	303	195	325	525	300
138	303	541	471	474	469	526	121
139	197	427	439	424	395	263	122
140	122	563	389	412	326	127	123
141	190	219	420	144	396	420	301
142	11	88	49	101	45	264	302
143	491	446	529	551	511	421	303

Table 34 – Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
144	51	305	113	323	88	55	48
145	157	54	52	30	46	265	49
146	61	22	123	420	89	15	124
147	501	441	524	290	512	422	50
148	311	359	313	73	327	527	125
149	5	172	10	79	12	56	19
150	8	555	103	106	90	266	304
151	477	448	527	488	513	423	126
152	499	376	364	373	328	267	494
153	14	558	114	324	91	57	305
154	223	266	206	282	143	128	306
155	371	230	372	419	329	268	127
156	70	430	324	187	330	424	128
157	246	194	311	392	331	269	495
158	515	479	542	287	514	270	496
159	353	217	501	307	470	271	51
160	69	45	182	266	144	58	307
161	370	10	308	273	238	129	497
162	356	170	500	442	471	59	129
163	139	238	172	198	145	272	130
164	253	575	167	571	146	7	308
165	275	570	475	423	472	425	52
166	203	339	479	87	473	528	498
167	48	38	158	70	147	426	499
168	351	149	557	5	550	427	309
169	167	51	3	306	7	2	5
170	144	321	302	344	332	273	53
171	305	383	491	41	474	428	310
172	459	345	488	495	475	274	311
173	263	90	50	102	47	275	312
174	100	567	384	354	397	529	131
175	41	525	205	453	148	130	500
176	238	358	416	246	398	429	313
177	59	384	146	336	149	131	314
178	431	573	136	576	150	60	501
179	274	103	329	520	333	132	315

Table 34 – Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
180	470	118	541	464	515	276	132
181	568	154	377	522	334	133	54
182	488	488	497	530	516	530	316
183	140	239	273	308	239	277	133
184	218	75	93	512	92	61	317
185	396	251	131	184	93	134	134
186	298	378	217	313	151	135	135
187	119	559	335	249	335	430	318
188	240	487	168	199	152	278	136
189	169	481	375	113	399	574	319
190	519	218	536	575	517	62	320
191	424	268	359	243	336	279	502
192	415	78	365	374	337	280	321
193	561	423	571	305	567	281	137
194	553	484	251	55	240	431	322
195	312	360	322	454	338	282	323
196	128	289	252	53	241	432	324
197	58	86	274	155	242	283	325
198	286	160	149	338	153	136	55
199	18	143	109	107	94	284	138
200	478	205	565	437	551	285	326
201	579	16	580	312	580	286	139
202	323	542	412	89	400	433	140
203	6	173	11	80	13	63	20
204	445	281	165	504	154	64	503
205	463	262	360	480	339	287	141
206	573	373	127	544	155	65	327
207	192	200	154	389	156	137	504
208	402	225	152	448	157	138	328
209	366	274	409	339	401	434	329
210	504	368	569	403	568	288	142
211	563	311	340	470	340	139	21
212	498	145	143	422	158	140	330
213	178	166	401	314	402	435	331
214	496	283	551	525	518	141	332
215	536	310	543	494	519	289	143

Table 34 – Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
216	221	406	275	309	243	290	333
217	400	134	331	321	341	436	334
218	487	96	514	465	520	531	335
219	379	185	548	78	552	532	505
220	4	153	15	2	14	142	506
221	45	65	68	258	95	291	336
222	94	322	380	230	403	533	144
223	436	388	493	441	476	292	337
224	71	431	31	28	26	143	338
225	196	133	13	229	15	16	145
226	241	46	266	485	244	66	339
227	364	94	115	35	96	293	340
228	244	121	433	238	404	294	341
229	464	298	396	558	405	144	146
230	338	228	39	162	48	145	147
231	222	405	458	458	477	534	148
232	86	369	159	71	159	437	22
233	212	501	229	277	245	438	342
234	556	495	194	436	160	67	507
235	116	191	186	267	161	295	149
236	130	12	150	190	162	439	150
237	291	364	354	563	342	68	508
238	389	408	173	201	163	296	151
239	444	557	516	188	478	146	343
240	296	317	351	181	343	440	344
241	243	257	476	59	479	535	345
242	508	198	69	259	97	297	152
243	155	421	287	533	246	69	153
244	331	186	174	202	164	298	509
245	440	508	481	445	480	441	510
246	276	147	72	12	98	536	346
247	264	89	278	356	247	70	154
248	288	260	198	515	165	17	347
249	228	247	195	514	166	71	348
250	332	326	533	206	521	442	349
251	118	459	253	214	248	443	350

Table 34 - Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
252	179	164	104	108	99	299	23
253	485	447	236	483	249	147	511
254	258	547	473	64	406	148	512
255	141	110	53	31	49	300	513
256	67	308	187	268	167	301	351
257	76	240	254	358	250	149	155
258	82	152	255	54	251	444	352
259	455	9	463	568	407	150	514
260	345	435	448	177	408	302	515
261	242	442	91	440	100	72	56
262	376	102	330	447	344	151	156
263	544	286	572	304	576	445	516
264	30	276	40	16	50	446	57
265	315	33	317	500	345	152	517
266	40	151	19	18	16	73	157
267	210	71	94	223	101	303	353
268	432	314	443	112	409	304	518
269	209	249	126	416	168	305	158
270	416	513	66	451	102	74	519
271	281	535	424	545	410	75	58
272	441	374	417	388	411	447	159
273	537	79	464	443	412	153	354
274	468	67	520	326	522	537	520
275	168	246	196	158	169	306	355
276	290	130	147	191	170	448	59
277	55	82	197	159	171	307	160
278	433	39	288	209	252	308	161
279	481	245	556	383	553	449	356
280	413	455	506	406	523	575	357
281	170	486	492	185	481	450	60
282	132	2	211	92	172	154	358
283	75	543	289	50	253	309	359
284	567	346	105	409	103	76	360
285	492	519	560	178	554	451	521
286	120	127	207	283	173	155	24
287	254	263	387	498	346	18	361

Table 34 – Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
288	231	253	21	252	27	77	362
289	145	415	393	489	413	310	363
290	123	451	408	340	414	452	25
291	47	273	138	65	174	538	364
292	108	344	132	182	104	156	522
293	382	544	534	47	524	453	523
294	551	214	281	210	254	311	524
295	529	309	465	444	415	157	162
296	566	372	356	561	347	78	365
297	493	282	325	189	348	454	525
298	64	315	116	33	105	312	366
299	373	515	496	502	482	313	367
300	297	157	304	345	349	314	163
301	452	461	141	428	175	158	368
302	339	192	256	56	255	455	6
303	13	381	35	262	51	159	61
304	273	355	310	74	350	539	369
305	570	469	576	569	577	79	62
306	328	64	368	297	351	315	164
307	330	119	386	355	416	540	370
308	204	579	297	548	256	160	63
309	304	229	451	32	483	576	371
310	72	354	319	413	352	456	372
311	564	206	434	379	417	316	373
312	147	72	106	109	106	317	374
313	104	142	155	255	176	161	64
314	268	155	83	285	107	162	165
315	92	167	402	22	418	541	526
316	235	140	156	256	177	163	166
317	267	156	482	242	484	457	527
318	405	458	307	552	353	164	375
319	126	128	32	505	52	19	376
320	282	188	510	240	525	542	528
321	442	499	539	222	555	577	377
322	319	226	291	276	257	165	26
323	138	394	398	167	419	543	65

Table 34 - Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
324	309	490	145	580	178	20	529
325	87	290	142	300	179	458	378
326	575	210	545	319	526	318	379
327	546	500	410	528	420	166	530
328	467	463	550	367	556	544	380
329	543	377	552	542	557	459	381
330	191	135	282	49	258	319	167
331	539	279	320	578	354	167	531
332	414	363	453	438	485	545	382
333	522	438	391	529	421	460	383
334	479	220	478	449	486	461	532
335	572	182	577	482	578	462	533
336	367	73	36	263	53	168	168
337	44	49	163	137	180	463	169
338	578	112	361	478	355	320	170
339	316	397	92	317	108	169	66
340	117	391	270	154	259	321	171
341	427	18	124	299	109	170	384
342	57	577	99	531	110	171	67
343	358	571	397	559	422	172	172
344	410	270	326	543	356	173	385
345	194	58	245	429	260	174	173
346	510	444	426	348	423	464	386
347	226	370	84	286	111	175	174
348	111	440	16	115	17	21	7
349	306	574	452	475	424	22	387
350	233	410	436	127	425	322	388
351	80	392	122	237	181	465	389
352	355	350	241	148	261	466	390
353	232	271	134	327	182	323	391
354	381	60	175	203	183	324	392
355	443	399	347	133	357	467	393
356	342	202	537	254	527	325	394
357	512	221	107	410	112	80	175
358	34	47	336	67	358	468	176
359	17	104	148	192	184	469	395

Table 34 - Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
360	307	295	449	38	426	326	534
361	434	69	502	513	487	327	27
362	448	456	366	227	359	328	177
363	266	528	405	466	427	176	535
364	201	382	43	369	54	81	536
365	270	84	468	166	488	546	396
366	560	502	540	121	528	329	397
367	1	474	1	99	2	3	1
368	329	63	456	366	489	547	178
369	15	342	257	51	262	470	179
370	555	227	480	275	490	471	398
371	335	523	494	477	491	330	537
372	375	464	440	128	428	331	399
373	483	334	466	169	429	177	400
374	200	460	276	435	263	82	68
375	325	181	212	93	185	178	180
376	557	365	521	537	492	179	401
377	390	284	373	118	360	332	538
378	287	307	137	244	186	333	402
379	419	14	226	526	264	180	181
380	148	478	258	57	265	472	403
381	63	531	176	204	187	334	28
382	198	505	404	432	430	473	182
383	79	74	171	346	188	83	404
384	16	211	133	334	113	23	69
385	437	61	523	235	529	548	405
386	525	113	441	425	431	335	406
387	248	328	199	14	189	336	183
388	318	562	437	129	432	337	184
389	377	287	120	171	114	338	407
390	36	222	188	269	190	339	185
391	207	527	81	455	115	181	539
392	454	101	139	481	191	182	408
393	473	568	279	462	266	340	186
394	62	532	82	95	116	474	187
395	363	15	455	234	493	549	540

Table 34 - Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
396	188	193	259	216	267	475	409
397	456	509	483	535	494	476	541
398	154	258	457	365	495	550	410
399	523	316	518	329	530	477	411
400	10	66	160	72	192	478	188
401	411	409	267	386	268	341	542
402	249	516	423	469	433	479	412
403	552	467	567	468	569	342	543
404	547	204	117	173	117	84	189
405	265	285	41	439	55	85	70
406	571	158	570	381	570	183	413
407	247	195	260	217	269	480	414
408	524	362	290	211	270	343	415
409	513	122	189	270	193	344	416
410	115	329	34	396	56	24	417
411	387	565	505	463	531	345	190
412	135	553	230	274	271	481	544
413	346	522	277	311	272	86	545
414	505	483	512	130	532	551	546
415	460	161	549	239	558	552	547
416	399	537	203	160	194	346	191
417	227	24	140	66	195	553	418
418	163	19	421	145	434	482	192
419	497	275	74	573	57	1	193
420	151	496	177	450	196	87	194
421	106	197	381	231	435	554	419
422	250	299	390	111	361	184	195
423	514	497	419	146	436	483	196
424	217	85	411	341	437	484	420
425	507	536	348	507	362	185	71
426	256	432	450	175	438	347	421
427	462	507	460	292	496	555	548
428	78	177	61	42	58	186	197
429	211	259	394	272	439	556	422
430	158	55	26	288	28	25	8
431	466	422	369	119	363	348	423

Table 34 - Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
432	453	87	349	427	364	485	198
433	66	169	204	45	273	578	199
434	121	124	425	205	440	486	424
435	509	539	218	163	197	187	200
436	172	504	299	207	274	188	201
437	541	380	559	456	533	88	425
438	42	524	237	518	198	89	549
439	133	3	44	467	59	8	202
440	3	137	5	63	8	26	29
441	205	572	338	471	365	349	72
442	279	445	108	411	118	90	73
443	289	261	467	25	441	189	550
444	52	306	27	289	29	27	3
445	26	99	85	96	119	487	426
446	369	293	370	298	366	350	551
447	107	323	403	21	442	557	427
448	548	174	378	174	367	351	428
449	425	48	407	546	443	190	203
450	542	301	341	472	368	352	204
451	528	318	538	510	559	579	552
452	239	418	477	58	497	558	205
453	480	62	568	180	571	353	429
454	344	477	489	496	498	354	430
455	486	554	485	296	499	488	206
456	426	189	280	579	275	91	431
457	403	68	77	362	120	191	432
458	308	1	395	539	444	192	207
459	259	548	470	19	500	559	553
460	386	244	509	476	534	489	208
461	521	549	498	493	501	355	433
462	438	325	233	577	276	92	434
463	350	493	374	417	369	356	554
464	90	338	238	149	277	490	209
465	7	396	17	116	18	28	30
466	245	120	24	139	30	93	210
467	372	561	486	501	502	193	211

Table 34 - Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
468	98	7	95	371	121	94	74
469	532	138	519	37	535	560	212
470	531	533	283	532	278	95	435
471	180	165	445	294	445	357	75
472	206	178	208	281	199	194	555
473	187	129	344	506	370	96	436
474	31	207	284	212	279	358	213
475	558	17	193	86	200	359	556
476	19	296	121	36	122	360	437
477	449	462	503	430	536	561	438
478	182	215	261	52	280	491	214
479	412	241	220	142	281	562	439
480	562	235	499	560	537	361	557
481	503	425	461	527	446	195	215
482	352	150	215	395	282	362	440
483	299	115	446	176	447	363	216
484	161	426	315	303	371	492	441
485	83	564	300	351	283	196	31
486	164	20	262	218	284	493	442
487	28	580	55	375	60	197	217
488	101	125	164	7	201	494	218
489	404	59	110	110	123	364	219
490	461	162	561	179	560	495	443
491	225	538	269	387	285	365	558
492	277	146	118	325	124	97	76
493	102	475	6	61	9	29	32
494	183	429	294	401	286	366	220
495	127	511	62	194	61	30	444
496	73	416	25	140	31	98	445
497	12	550	73	433	125	198	446
498	22	196	65	193	62	199	33
499	429	530	495	554	538	367	221
500	153	232	352	331	372	368	222
501	81	233	30	170	32	31	559
502	569	26	332	487	287	99	447
503	165	109	125	120	126	200	77

Table 34 - Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
504	392	278	316	508	373	369	223
505	85	420	135	328	202	370	224
506	112	141	399	168	448	563	448
507	56	81	200	157	203	371	78
508	535	402	525	333	539	496	449
509	74	436	285	213	288	372	225
510	131	13	144	421	204	201	226
511	189	452	292	88	289	373	560
512	502	267	414	247	449	497	561
513	142	510	528	261	540	498	227
514	224	265	530	165	541	499	562
515	327	411	385	353	450	564	563
516	91	371	96	224	127	374	450
517	77	419	78	363	128	202	228
518	533	330	566	517	561	203	451
519	134	4	79	364	129	204	229
520	229	272	190	85	205	375	564
521	362	248	490	186	503	500	565
522	181	216	221	511	290	205	230
523	458	569	418	557	451	100	34
524	495	417	513	415	504	206	566
525	506	352	309	490	291	207	79
526	349	348	383	534	374	208	231
527	530	466	388	499	375	209	232
528	255	264	244	151	292	501	233
529	137	42	58	123	63	210	567
530	143	424	63	43	64	211	234
531	385	518	508	492	505	212	568
532	53	520	382	232	452	565	569
533	251	300	169	200	206	376	570
534	365	95	428	550	453	101	452
535	65	428	213	399	293	377	235
536	39	234	170	347	207	102	453
537	262	123	45	461	65	103	80
538	260	144	293	278	294	378	454
539	408	303	564	75	562	379	571

Table 34 - Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
540	398	546	515	104	542	566	455
541	159	56	18	1	19	213	81
542	494	437	531	302	543	502	456
543	272	304	191	271	208	380	457
544	540	242	415	248	454	503	236
545	380	389	400	315	455	504	458
546	500	449	554	486	563	505	572
547	357	433	345	4	376	506	573
548	173	347	231	400	295	214	82
549	105	545	318	414	377	507	35
550	193	201	225	48	296	567	237
551	378	288	427	349	456	508	459
552	278	148	59	125	66	215	83
553	93	168	209	90	209	216	460
554	409	551	474	574	457	32	574
555	475	184	555	226	564	509	575
556	186	237	201	13	210	381	238
557	293	453	511	241	544	568	239
558	340	529	157	390	211	217	240
559	37	331	192	82	212	382	461
560	393	489	553	264	565	569	462
561	576	209	295	279	297	218	241
562	103	223	80	360	130	219	463
563	439	324	306	564	378	383	464
564	422	171	222	143	298	570	465
565	446	521	232	570	299	220	466
566	343	203	312	391	379	384	467
567	175	35	151	337	213	221	468
568	9	28	242	152	300	510	469
569	326	180	337	68	380	511	576
570	472	224	161	8	214	512	577
571	84	443	268	385	301	385	578
572	534	327	522	549	545	513	579
573	237	37	178	330	215	386	242
574	347	43	504	135	546	580	580
575	300	291	444	295	458	387	243

Table 34 - Continued

Choice Set	MSD Alt	DSD Alt	MSD Att	DSd Att	Magnitude Tradeoffs	# Tradeoffs	Entropy
576	401	23	243	153	302	514	470
577	545	468	579	250	579	571	244
578	511	413	442	426	459	388	471
579	368	434	305	196	381	572	84
580	301	105	350	134	382	515	472

Table 35 - Allocation of Choice Sets to Participants

	G1	G2	G3	G4	G5	G6	G7	G8	G9	10	G11	G12	G13	G14	G15	G16	G17	G18	G19	G20	G21	G22	G23	G24	G25	G26	G27	G28	G29
CS1	367	106	169	63	220	149	203	348	96	440	88	430	465	444	493	17	49	266	87	23	94	118	73	95	45	264	10	145	28
CS2	132	541	439	24	303	35	498	224	428	501	104	405	89	136	142	144	146	16	43	184	221	468	8	122	97	117	496	445	261
CS3	64	107	173	101	537	6	7	54	72	150	252	199	312	79	255	342	384	50	153	227	410	298	394	75	76	442	15	466	26
CS4	476	489	529	100	492	503	552	48	319	495	225	185	519	82	123	530	160	103	167	230	246	177	236	198	282	286	313	292	507
CS5	212	57	232	277	400	314	125	337	488	316	336	339	276	341	383	3	163	404	291	347	357	358	4	40	375	31	359	235	25
CS6	381	390	330	392	457	207	418	419	345	420	417	275	487	242	497	188	387	433	435	475	238	364	510	80	116	244	517	354	416
CS7	175	472	391	436	536	83	269	553	267	556	559	474	567	478	11	113	186	22	562	32	522	154	505	516	570	208	550	379	66
CS8	249	520	92	98	288	548	84	119	36	121	129	351	438	573	464	183	257	558	197	568	226	1	243	247	59	422	108	258	533
CS9	14	196	278	111	164	251	302	528	308	322	369	29	340	396	248	407	486	39	374	42	380	44	256	47	409	34	99	353	543
CS 10	161	412	233	561	456	204	294	479	485	62	509	105	378	389	283	443	216	325	511	535	538	228	471	494	564	483	137	575	141
CS 11	170	576	300	159	52	458	56	324	525	60	61	157	413	201	67	148	46	179	306	90	265	102	114	120	37	71	124	126	131
CS 12	194	222	134	195	135	156	462	140	360	206	155	262	213	500	502	181	217	187	191	192	388	569	205	211	393	38	356	240	408
CS 13	473	241	284	287	297	250	441	526	304	352	580	78	168	20	450	482	55	237	310	331	51	338	162	344	81	176	362	377	425
CS 14	432	296	491	209	368	446	448	301	579	431	504	307	309	484	152	463	30	189	318	53	69	547	70	549	311	326	566	421	171
CS 15	563	74	174	571	401	12	350	527	77	91	289	355	323	429	27	139	229	365	343	372	506	202	373	545	315	210	447	13	33
CS 16	254	21	449	260	290	539	268	271	398	272	424	515	93	273	406	295	544	270	85	115	452	426	127	193	349	434	165	386	346
CS 17	363	551	534	453	9	382	178	402	361	112	470	223	234	423	200	385	332	333	147	578	239	481	565	128	253	279	512	281	523
CS 18	532	557	68	259	280	395	437	317	231	172	490	477	411	41	180	58	328	138	370	19	166	554	460	151	399	454	334	245	18
CS 19	299	455	467	218	560	521	143	371	133	376	130	215	397	542	499	524	158	214	427	508	182	461	513	321	514	327	459	366	65
CS 20	574	219	531	546	86	555	320	414	5	540	263	190	518	285	469	274	109	293	335	572	480	110	451	329	403	577	415	2	305

12. Accomplishments of this Dissertation

We now review the major accomplishments of this dissertation:

- We provide numerous links between choice design practice and preferred statistical practices (see Section 4.2)

  Although there are many accomplishments in the field of choice analysis there is not currently a comprehensive source that reviews the common practices in the creation of designs for choice experiments and the preferred statistical practices of designing experiments. We also review where the assumptions of statistical design are met or violated in the process of creating choice designs. We find that traditional tabled designs (fractional factorials, BIBD, etc.) are too restrictive for generating good choice designs.

- We evaluate the effectiveness of techniques suggested for the creation of optimal choice designs. (see Chapter 5)

  There are several different suggestions concerning how one should create optimal choice designs in the literature. We review these suggestions and several different existing computational programs available for the construction of choice designs. In addition we study through simulation the comparative effectiveness of these methods for creating efficient choice designs. We find that effective algorithms exist for the creation of optimal choice designs for the multinomial logit model. We also find that the most efficient designs are created using a simple informative prior.

- We identify and explore new and existing criteria for evaluating the complexity of a choice design (see Section 4.5)

  The use of entropy has been suggested and reviewed numerous times in the literature as a measure of choice design complexity. In addition the mean standard deviation of attribute levels within an alternative and the dispersion of the standard deviation of attribute levels within an alternative have also been reviewed as a measure of complexity in the choice task. We review these measures and suggest some additional measures of choice task complexity.

- We explore what flexibility exists for using entropy as a secondary measure of design optimality. (see Sections 7.1 and 7.2)

  We explore whether entropy can be used as a secondary criterion of design optimality to control for effects that result from a participant experiencing too complex a choice task. We find that in some situations there is flexibility afforded for using entropy as a secondary criterion but in the majority of situations the range of entropy achieved through the use of optimal design algorithms presents too narrow a range of values to be practically useful. To explore this idea further would require modifications to the current algorithms, or entirely new algorithms.

- We explore the consequences of misspecifying the prior in the creation of optimal choice designs. (see Chapter 8)

  We explore six different priors and their impact on the efficiency and entropy of the resulting choice design. We find that using a zero prior assumption is never a good assumption as it results in designs that are extremely inefficient when we have even very simple information available about the preferences for specific attribute levels. We also find that misspecification of the shape of the prior creates inefficient designs if the shape is not as assumed. We conclude that the use of the equal-spaced prior is recommended unless we have concrete information to recommend other shaped priors.

- We explore how designs should be constructed under the assumption that individuals often use non-compensatory decision making strategies. (see Chapter 9)

  We discuss four different types of decision making (compensatory, conjunctive, disjunctive, and lexicographic) that have been introduced as ways consumers can make decisions. Although currently choice design construction generally assumes that users make compensatory decisions we explore the use of different prior assumptions that follow different decision strategies and study the effect of misspecification of the decision strategy in the creation of the design. Our findings suggest that changing decision strategies during the course of an experiment can have a negative impact on the true efficiency of the design.

- We explore how different criteria for design complexity relate to the selection of the no-choice alternative in the choice experiments. (see Section 10.2)

  We explore how established and newly suggested methods for measuring choice complexity impact the propensity towards selecting the no-choice alternative. Although we do not find completely consistent results concerning the effects of these measures, many of the inconsistencies may be due to the limited range of the effects within a particular sample data set or a misspecified model due to our lack of knowledge regarding the data set. We do find that the selection of the no-choice alternative consistently occurs more frequently at the end of sequences of choices than the beginning.

- We explore how the effect of losing a particular percent of choices within a certain choice task impacts the overall efficiency of the design. (see Section 10.3)

  We find that losing specific combinations of choice sets within a particular choice experiment can have varying effects on the efficiency of the remaining choice sets. This suggests that we might violate the traditional statistical assumption of randomization when ordering the choice sets within a choice task for a participant. We should attempt to place the choice sets with the greatest effect on the overall efficiency of the experiment in a location that will be least likely to be selected as no-choice due to other systematic effects (such as cumulative burden or fatigue or learning effects). We also find that there exist designs where the impact of losing choice sets is less severe.

13. Suggestions for Future Research

Designing against the selection of the no-choice alternative and effectively communicating the reasons that the no-choice alternative should be selected are two lines of defense against systematic violations of the design and analysis assumptions. However, we know that it is impossible to completely control against the selection of the no-choice alternative for systematic reasons other than unattractiveness. Currently the only models we have available for analyzing the results of experiments with the no-choice alternative only model the selection of the no-choice alternative due to the unattractiveness of the other alternatives in the choice set. Developing models that can also model selection of the no-choice alternative due to choice difficulty will further enhance the results of stated preference models with a no-choice alternative.

Since choice experiments involve working with people, there are many facets in the information collection process that can result in data sets that are not necessarily clean. Issues including changing decision strategies, fatigue, learning effects and outside distractions are some issues that can impact the clarity of individual's decisions. The use of fuzzy methods for the analysis of data collected from choice experiments may be a way to alleviate some of these problems. If several solutions for a problem are available, fuzzy methods can provide a way to identify the dominant solutions. Using fuzzy methods may allow us to clarify the problems inherent in working with people to collect data.

Given the prevalence of the no-choice alternative in our sample of choice designs, the designers of choice experiments need to explore additional means of collecting information from a choice experiment. For example, can we ask a follow-up question to the selection of a no-choice alternative that will identify why the participant selected the no-choice alternative? This additional information may somewhat negate the effects of the propensity of no-choice alternative selections in choice experiments.

Choice design and analysis techniques rely on the assumption that individuals make compensatory decisions. Given that it has been documented that individuals often use simplifying decisions strategies, the techniques used for the design and analysis of choice experiments need to be evaluated as to their effectiveness under these different decision strategies. If they no longer hold true then new techniques that account for these issues need to be developed.

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Appendices

A1 Appendix One

A1.1 Design One: Random Assignment of 9 Profiles to 9 Choice Sets of Size 3

Select an orthogonal array of a 3^3 design in the desired number of runs (for this example 9 runs to create 9 choice sets). Make 3 copies of this design and randomly assign one profile from each copy into a choice set at a time. Ensure that no two copies of the same profile exist in the same choice set.

The 9 run orthogonal array:

X=[1 1 1
1 2 2
1 3 3
2 1 2
2 2 3
2 3 1
3 1 3
3 2 1
3 3 2]

The selected order of these 9 profiles into 9 choice sets of size three is:

Set 1 - 8 7 1
Set 2 - 3 9 7
Set 3 - 2 3 6
Set 4 - 6 4 8
Set 5 - 5 8 4
Set 6 - 1 2 3
Set 7 - 4 1 2
Set 8 - 9 6 5
Set 9 - 7 5 9

The final design indicating the profiles and their respective choice sets is:

X1	X2	X3	Choice Set
3	2	1	1
3	1	3	1
1	1	1	1
1	3	3	2
3	3	2	2
3	1	3	2
1	2	2	3
1	3	3	3
2	3	1	3
2	3	1	4
2	1	2	4
3	2	1	4
2	2	3	5
3	2	1	5
2	1	2	5
1	1	1	6
1	2	2	6
1	3	3	6
2	1	2	7
1	1	1	7
1	2	2	7
3	3	2	8
2	3	1	8
2	2	3	8
3	1	3	9
2	2	3	9
3	3	2	9

This design has a D_0-efficiency of 3.613 and a D_p-efficiency of 2.331, assuming a prior on β of [-1 0 -1 0 -1 0].

A1.2 Design Two: Random Assignment of 3 Different Sets of 9 Profiles to 9 Choice Sets of Size 3

Select three different copies of a 9 run orthogonal array of a 3^3. Randomly assign one profile from each copy at a time into 9 choice sets of size three. This is similar to design one above except that one does not have to worry that about repeat copies of profiles within a choice set as the three copies of the fractional factorial are different.

The three copies of the fractional factorial:

```
X1=[
    1    1    1
    1    2    2
    1    3    3
    2    1    2
    2    2    3
    2    3    1
    3    1    3
    3    2    1
    3    3    2];
```

```
X2=[
    1    1    2
    1    2    3
    1    3    1
    2    1    3
    2    2    1
    2    3    2
    3    1    1
    3    2    2
    3    3    3];
```

```
X3=[
    1    1    3
    1    2    1
    1    3    2
    2    1    1
    2    2    2
    2    3    3
    3    1    2
    3    2    3
    3    3    1];
```

The selected order of these 9 profiles into 9 choice sets of size three is:

Set 1 - 3 7 5
Set 2 - 2 6 1
Set 3 - 8 2 7
Set 4 - 1 5 3
Set 5 - 6 8 9
Set 6 - 4 3 8
Set 7 - 5 1 2
Set 8 - 3 4 4
Set 9 - 9 9 6

The final design indicating the profiles and their respective choice sets is:

X1	X2	X3	Choice Set
1	3	3	1
3	1	1	1
2	2	2	1
1	2	2	2
2	3	2	2
1	1	3	2
3	2	1	3
1	2	3	3
3	1	2	3
1	1	1	4
2	2	1	4
1	3	2	4
2	3	1	5
3	2	2	5
3	3	1	5
2	1	2	6
1	3	1	6
3	2	3	6
2	2	3	7
1	1	2	7
1	2	1	7
1	3	3	8
2	1	3	8
2	1	1	8
3	3	2	9
3	3	3	9
2	3	3	9

This design has a D_0-efficiency of 3.722 and a D_p-efficiency of 2.252, assuming a prior on β of [-1 0 -1 0 -1 0].

A1.3 Design Three: Use Shifting to Create 9 Choice Sets of Size Three from a 9 Run Fractional Factorial of a 3^3 Design

Select one 9 run fractional factorial of a 3^3 design. From each of the original 9 profiles create 2 additional profiles to put into a choice set by adding one modulo 3 to each of the alternatives. This creates nine choice sets of size three.

The original fractional factorial:

X1=[
1 1 1
1 2 2
1 3 3
2 1 2
2 2 3
2 3 1
3 1 3
3 2 1
3 3 2]

The first and second choice sets are constructed by shifting the levels of the first and second profiles as shown below:

$$(1, 1, 1) \rightarrow (2, 2, 2) \rightarrow (3, 3, 3)$$
$$(1, 2, 2) \rightarrow (2, 1, 1) \rightarrow (3, 2, 2)$$

The final design indicating the profiles and their respective choice sets is:

X1	X2	X3	Choice Set
1	1	1	1
2	2	2	1
3	3	3	1
1	2	2	2
2	3	3	2
3	1	1	2
1	3	3	3
2	1	1	3
3	2	2	3
2	1	2	4
3	2	3	4
1	3	1	4
2	2	3	5
3	3	1	5
1	1	2	5
2	3	1	6
3	1	2	6
1	2	3	6
3	1	3	7
1	2	1	7
2	3	2	7
3	2	1	8
1	3	2	8
2	1	3	8
3	3	2	9
1	1	3	9
2	2	1	9

This design has a D_0-efficiency of 5.195, and a D_p-efficiency of 2.652 assuming on β of [-1 0 -1 0 -1 0].

A1.4 Design Four: All Pairs / All Triples

This method will create choice sets by combining all possible pairs or all possible triples. This will result 36 choice sets of size two or 84 choice sets of size three. The results for all triples has a D_0-efficiency of 36.363, and a D_p-efficiency of 21.978 assuming on β of [-1 0 -1 0 -1 0].

A1.5 Design Five: Foldover

Foldover will not be useful for the example as it is only defined for attributes with two levels. Further, with foldover we can only generate choice sets of size two.

A1.6 Design Six: BIBD

Balanced incomplete block designs can be used for the construction of choice sets. Similar to design one above we can place our 9 original profiles into 9 choice sets of size three by using a BIBD with t = 9 , k = 3, b = 12, r = 4.

The 9 run fractional factorial:

```
X=[1 1 1
   1 2 2
   1 3 3
   2 1 2
   2 2 3
   2 3 1
   3 1 3
   3 2 1
   3 3 2]
```

The selected order of these 9 profiles into 12 choice sets of size three is:

Set 1 – 123
Set 2 – 456
Set 3 – 789
Set 4 – 147
Set 5 – 258
Set 6 – 369
Set 7 – 159
Set 8 – 729
Set 9 – 483
Set 10 – 186
Set 11 – 429
Set 12 - 753

The final design indicating the profiles and their respective choice sets is:

X1	X2	X3	Choice Set
1	1	1	1
1	2	2	1
1	3	3	1
2	1	2	2
2	2	3	2
2	3	1	2
3	1	3	3
3	2	1	3
3	3	2	3
1	1	1	4
2	1	2	4
3	1	3	4
1	2	2	5
2	2	3	5
3	2	1	5
1	3	3	6
2	3	1	6
3	3	2	6
1	1	1	7
2	2	3	7
3	3	2	7
3	1	3	8
1	2	2	8
2	3	1	8
2	1	2	9
3	2	1	9
1	3	3	9
1	1	1	10
3	2	1	10
2	3	1	10
2	1	2	11
1	2	2	11
3	3	2	11
3	1	3	12
2	2	3	12
1	3	3	12

This design has a D_0-efficiency of 2.946 and a D_p-efficiency of 5.195 assuming a prior β of [-1 0 -1 0 -1 0].

A1.7 Design Seven: Orthogonal Main Effects Design

We can construct our 9 choice sets of size three by creating an orthogonal blocking scheme that is capable of estimating at least all of the main effects for our design. In fact for this example we will retain the power to estimate some of the higher order interactions.

See design three above for the final design inclusive of the profiles and their choice sets. This design has a D_0-efficiency of 5.195 and a D_p-efficiency of 2.652 assuming a prior β of [-1 0 -1 0 -1 0].

A2 Appendix Two

The designs in this section are the optimal designs evaluated for their performance on the 4 criteria of choice designs: orthogonality, level balance, minimum overlap and utility balance (entropy). For each of the three design scenarios designs are presented with a zero prior assumption and also with an equal-spaced prior assumption.

A2.1 – 3^3 in 9 Choice Sets of Size 3 – Zero Prior

Choice Set	Attribute 1	Attribute 2	Attribute 3
1	1	1	1
1	3	2	2
1	2	3	3
2	1	2	1
2	2	1	2
2	3	3	3
3	1	3	2
3	2	2	1
3	3	1	3
4	3	1	2
4	2	2	1
4	1	3	3
5	3	2	2
5	2	3	1
5	1	1	3
6	1	3	1
6	3	2	3
6	2	1	2
7	2	2	3
7	3	3	1
7	1	1	2
8	2	3	2
8	1	2	3
8	3	1	1
9	2	1	1
9	1	3	3
9	3	2	2

A2.2 – 3^3 in 9 Choice Sets of Size 3 – Equal-Speaced Prior

Choice Set	**Attribute 1**	**Attribute 2**	**Attribute 3**
1	3	1	3
1	2	2	2
1	1	3	1
2	1	2	3
2	3	1	2
2	2	3	1
3	3	1	3
3	2	2	1
3	1	3	2
4	3	2	1
4	2	1	2
4	1	3	3
5	3	3	1
5	2	1	2
5	1	2	3
6	2	1	1
6	3	3	3
6	1	2	2
7	1	3	2
7	2	2	3
7	3	1	1
8	1	1	2
8	2	3	3
8	3	2	2
9	3	3	2
9	1	2	1
9	2	1	3

A2.3 – $3^2 \cdot 4^2$ in 10 Choice Sets of Size 4 – Zero Prior

Choice Set	Attribute 1	Attribute 2	Attribute 3	Attribute 4
1	2	3	4	3
1	3	1	1	2
1	1	3	2	4
1	2	2	3	1
2	3	1	2	3
2	3	2	3	4
2	2	3	4	2
2	1	1	1	1
3	1	3	3	3
3	2	2	4	1
3	3	1	2	2
3	2	1	1	4
4	2	1	4	4
4	1	2	3	3
4	1	2	2	2
4	3	3	1	1
5	2	3	1	2
5	3	2	4	4
5	2	1	2	3
5	1	1	3	1
6	1	3	2	4
6	3	1	4	3
6	3	1	3	2
6	2	2	1	1
7	1	2	4	2
7	2	1	2	3
7	1	3	1	4
7	3	3	3	1
8	1	1	4	1
8	3	2	1	4
8	2	3	3	2
8	3	2	2	3
9	1	2	2	2
9	1	3	1	3
9	3	3	4	1
9	2	1	3	4
10	2	1	4	4
10	3	1	3	2
10	1	2	1	3
10	3	3	2	1

A2.4 - $3^2 \cdot 4^2$ in 10 Choice Sets of Size 4 – Equal-Spaced Prior

Choice Set	Attribute 1	Attribute 2	Attribute 3	Attribute 4
1	1	3	4	4
1	3	2	4	3
1	2	1	3	4
1	2	1	3	4
2	1	3	1	4
2	3	1	3	2
2	2	2	4	1
2	2	3	2	3
3	1	2	4	2
3	3	3	2	1
3	2	1	1	4
3	2	1	2	3
4	1	2	4	2
4	2	1	2	4
4	3	2	1	3
4	3	3	3	1
5	2	3	4	2
5	3	1	4	3
5	3	2	3	4
5	2	3	4	2
6	2	1	4	4
6	1	3	3	3
6	3	2	1	4
6	3	3	3	1
7	2	2	4	1
7	3	1	4	2
7	3	2	2	4
7	1	3	3	3
8	2	2	3	1
8	1	1	4	1
8	2	3	2	2
8	3	1	1	3
9	1	1	4	1
9	3	1	2	2
9	2	2	2	1
9	2	3	1	2
10	3	3	4	1
10	2	1	3	4
10	1	2	3	3
10	1	3	2	4

A2.5 – $3^2 \cdot 4 \cdot 5$ in 10 Choice Sets of Size 4 – Zero Prior

Choice Set	Attribute 1	Attribute 2	Attribute 3	Attribute 4
1	2	2	3	5
1	1	3	2	4
1	1	2	1	1
1	3	1	4	2
2	3	1	3	1
2	2	2	2	5
2	1	3	4	4
2	2	1	1	3
3	1	3	3	3
3	2	2	2	1
3	3	2	4	4
3	1	1	1	5
4	1	2	2	1
4	3	1	1	2
4	3	3	4	5
4	2	1	3	4
5	3	1	2	2
5	1	3	3	3
5	2	1	1	4
5	2	2	4	5
6	2	3	4	1
6	2	3	3	2
6	1	1	2	5
6	3	2	1	4
7	3	2	4	3
7	3	3	1	5
7	2	1	2	4
7	1	2	3	2
8	2	3	2	2
8	3	2	3	4
8	1	1	4	1
8	1	3	1	3
9	2	3	1	1
9	3	2	2	3
9	1	1	3	5
9	1	2	4	2
10	2	1	4	3
10	3	3	2	5
10	3	3	3	1
10	1	2	1	2

A2.6 - $3^2 \cdot 4 \cdot 5$ in 10 Choice Sets of Size 4 – Equal-Spaced Prior

Choice Set	Attribute 1	Attribute 2	Attribute 3	Attribute 4
1	3	2	2	3
1	1	3	1	4
1	3	1	3	1
1	2	3	1	2
2	1	2	4	2
2	2	1	4	3
2	3	3	3	1
2	1	1	2	5
3	1	1	4	5
3	3	2	2	3
3	2	3	3	2
3	3	2	1	4
4	1	3	3	3
4	3	1	4	2
4	2	2	1	5
4	1	2	4	1
5	3	1	3	1
5	3	2	1	3
5	2	2	4	1
5	1	3	2	4
6	1	3	3	3
6	2	2	2	5
6	3	1	4	2
6	2	1	3	4
7	2	2	3	5
7	3	3	2	5
7	2	1	4	3
7	1	3	4	4
8	3	2	2	4
8	3	3	1	5
8	2	3	4	1
8	1	1	3	5
9	2	1	4	3
9	2	3	4	2
9	1	1	4	5
9	3	2	3	4
10	2	1	2	4
10	3	2	1	3
10	2	3	2	1
10	1	2	3	2

A3 Appendix Three

Each of the following tables illustrates the results of a random effect logistic regression model with a response of choice / no-choice, a fixed effect of the complexity measure indicated and a random effect indicating the ID of the person making the choice. DNC indicates that the model did not converge.

A3.1 – Effect of Entropy on the Percent of No-Choice Alternatives

	A	**B**	**C**	**D**	**E**	**F**	**G**	**H**	**I**	**J**	**K**
Estimate	Not Sig	-0.232	-0.915	Not Sig	0.898	-1.5793	0.3472	-0.6496	Not Sig	DNC	-1.7565
P-value		0.0711	0.0013		0.0092	0.0027	0.0803	0.079			<0.0001

A3.2 – Effect of the Number of Tradeoffs on the Percent of No-Choice Alternatives

	A	**B**	**C**	**D**	**E**	**F**	**G**	**H**	**I**	**J**	**K**
Estimate	DNC	DNC	DNC	-0.0971	Not Sig	-0.3122	DNC	DNC	DNC	Not Sig	Not Sig
P-value				0.0146		0.0386					

A3.3 – Effect of the Magnitude of Tradeoffs on the Percent of No-Choice Alternatives

	A	B	C	D	E	F	G	H	U	J	K
Estimate	Not Sig	Not Sig	DNC	-0.07422	Not Sig	Not Sig	DNC	DNC	Not Sig	0.01524	0.1206
P-value				0.0133						0.0176	<0.0001

A3.4 – Effect of the Mean Standard Deviation of Attribute Levels within an Alternative on the Percent of No-Choice Alternatives

	A	B	C	D	E	F	G	H	I	J	K
Estimate	-1.0843	-1.7461	DNC	Not Sig	0.7825	0.9734	-0.4291	-0.3224	Not Sig	-0.8129	0.2941
P-value	0.0087	<0.0001			<0.0001	0.0015	<0.0001	<0.0001		<0.0001	0.0162

A3.5 – Effect of the Dispersion of Attribute Levels within an Alternative on the Percent of No-Choice Alternatives

	A	B	C	D	E	F	G	H	I	J	K
Estimate	1.1756	Not Sig	DNC	-1.905	0.6047	0.9762	Not Sig	Not Sig	Not Sig	Not Sig	0.6022
P-value	0.0021			0.0251	<0.0001	0.002					<0.0001

A3.6 – Effect of the Mean Standard Deviation of Attribute Levels within an Attribute on the Percent of No-Choice Alternatives

	A	**B**	**C**	**D**	**E**	**F**	**G**	**H**	**I**	**J**	**K**
Estimate	Not Sig	Not Sig	DNC	-2.3854	Not Sig	Not Sig	0.5382	0.3003	Not Sig	0.344	1.3845
P-value				0.0279			<0.0001	0.0201		0.0199	<0.0001

A3.7 – Effect of the Dispersion of Attribute Levels within an Attribute on the Percent of No-Choice Alternatives

	A	**B**	**C**	**D**	**E**	**F**	**G**	**H**	**I**	**J**	**K**
Estimate	-0.7374	Not Sig	DNC	1.7678	Not Sig	Not Sig	0.5626	Not Sig	Not Sig	Not Sig	1.0748
P-value	0.0706			0.0797			<0.0001				<0.0001

A3.8 – Effect of the Choice Quantile on the Percent of No-Choice Alternatives

	A	B	C	D	E	F	G	H	I	J	K
Estimate Q1	0.4585	Not Sig	0.5422	0.523	0.5781	0.3225	0.2548	0.3695	Not Sig	0.3213	Not Sig
P-value Q1	0.0072		<0.0001	0.0679	<0.0001	0.0344	<0.0001	<0.001		<0.0001	
Estimate Q2	0.367	Not Sig	0.2034	-0.2735	-0.0598	0.1091	0.09959	0.03423	Not Sig	0.1687	Not Sig
P-value Q2	0.0297		0.0241	0.2667	0.5011	0.4605	0.0791	0.5304		0.0019	

Vita

Jennifer Golek graduated from the State University of New York at Potsdam in 2000 with a Bachelors degree in Mathematics and Music and a Masters degree in Mathematics. She received a Masters degree in Statistics from the University of Tennessee, Knoxville. She earned a Doctorate in Business Administration with a concentration in Statistics from the University of Tennessee, Knoxville.

In 2004 Jennifer received the John R. Moore Graduate Teaching award from the college of Business Administration at the University of Tennessee, Knoxville. Additionally she received the Department of Statistics Graduate Student Excellence award in 2003. She received the 2003 Provost award for Extraordinary Professional Promise from the University of Tennessee, Knoxville. In 2000 she was awarded the State University of New York's Chancellors Award for Student Excellence.