

## Chapter 10

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# Experimental design and the estimation of willingness to pay in choice experiments for health policy evaluation

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### 10.1 Introduction

This chapter focuses on stated preferences obtained from discrete choice experiments (DCEs also known as SPDCEs), as opposed to data that reflect real market choices (revealed preferences (RP) as discussed in Chapter 9). DCEs try to simulate the essential elements of real market options that consumers might face in the future. Unlike real market choice data, DCEs rely on constructed markets in which key factors that are hypothesized to drive choices are systematically varied. To the extent that the consumers in a DCE make choices in a manner consistent with the way in which they would actually choose in a real market, one can derive standard welfare estimates for policy changes. The remainder of this chapter is devoted to discussing and illustrating how this can be accomplished with DCEs. More details on DCEs can be found in Louviere, Hensher, and Swait (1).

In order to collect DCE data information from consumers, one must identify factors that drive the choices of interest. These factors are called ‘attributes’ of the choice options. Once these attributes are identified, one must assign them values, known in experimental design parlance as ‘levels’. Taken together, the attributes and levels define and determine possible choice options that can be offered to consumers in a DCE survey. That is, a factorial combination of attribute levels completely defines the possible choice options. So, for example, if there are three attributes, say A(4), B(3), and C(2), with the associated number of levels in parentheses, the factorial combinations, or all possible options are  $4 \times 3 \times 2 = 24$ . We refer to that factorial combination as a ‘full’ or ‘complete’ factorial design. Typically, the number of combinations in a full factorial design is too many to use in practical field applications of DCE surveys, so one has to sample from the full factorial to reduce the size of the problem. There are many ways to sample from full factorials, but a common approach in DCE surveys is to sample based on what is known as a ‘fractional factorial design’. We return to these ideas in more detail later in the chapter.

The type of experimental design used to construct a DCE survey is important for three reasons: 1. it determines the economic quantities of potential interest that

1 can be statistically identified from an estimation perspective; 2. it strongly influences  
 2 confidence intervals associated with these quantities (statistics) given a fixed sample  
 3 size or equivalently, influences the sample size needed to achieve a given confidence  
 4 interval or level of precision for a statistic, and so plays a major role in the cost of a  
 5 project; 3. the attributes/levels can influence the plausibility of questions; and hence,  
 6 influence the quality of data obtained. The first two issues are largely statistical in  
 7 nature and are the subject of this chapter.<sup>1</sup> The third issue deals more with issues of  
 8 survey design and are not considered further here, although it may place constraints  
 9 on what can be measured in a DCE survey.<sup>2</sup>

10 This chapter uses examples from health policy research, but these examples also  
 11 have application in analyzing consumer choices in areas like culture, environment,  
 12 transport, utilities, or more generally where government has a strong role in determin-  
 13 ing which options are available and/or how they are priced, like health. Overviews  
 14 of the use of DCE methods to value health issues can be found in various sources  
 15 (e.g., 8–11).<sup>3</sup>

16 We begin by considering a single binary choice (SBC) contingent valuation ques-  
 17 tion because many basic issues associated with experimental design can be easily seen  
 18 in the context of the SBC format (see, e.g., (13)). This question format is popular in  
 19 the stated preference literature and enjoys a number of desirable incentive properties  
 20 under certain conditions.<sup>4</sup> This question format also is the simplest example of a more  
 21 general discrete choice experiment format (see, e.g., (1)). The key properties of the  
 22 SBC format from our perspective are:

- 23 1. Only one choice question [set] is asked;
  - 24 2. The question asked offers only two alternatives; and,
  - 25 3. Only one attribute of the scenario, typically cost, is varied across respondents.
- 26 For example, consider the following simple proposition. A large university in the USA  
 27 is considering offering employees the option of purchasing a dental plan. The dental  
 28 plan covers 80% of normal costs associated with all standard, non-cosmetic dental  
 29 procedures. The university is interested in what fraction of employees will subscribe to  
 30 the dental plan at various prices. A DCE for this policy problem would describe the

<sup>1</sup> The two key statistical properties of an experimental design are identification and precision. Louviere, Hensher, and Swait (1) note two other properties that can influence the desirability of a design that are not statistical in nature. These are cognitive complexity and market realism. Both considerations can restrict the nature of the attributes and design used.

<sup>2</sup> There are several general books on survey design (e.g., 2–4) but there is a surprising lack of guidance on the issue of SP choice questions in a policy context. Some exceptions are (5;6). A small but growing literature (e.g., (7)) looks at the implications of task complexity in choice experiments.

<sup>3</sup> For an overview of different health valuation methods including SP methods with an emphasis on determining the value of a statistical life, see (12).

<sup>4</sup> The SBC question format was recommended by the Arrow *et al.* (14) panel that looked at SP methods for valuing natural resource damages for the U.S. Government. Carson and Groves (15) examined the incentive properties of different types of SP questions in detail.

1 dental plan in sufficient detail for employees to understand what they would be pur-  
 2 chasing, provides the monthly cost associated with the plan and asks the question  
 3 ‘Would you subscribe to this plan if offered to you?’. The employee can choose to  
 4 accept the plan or reject it (the two options). In this case, a single attribute is varied  
 5 over a range of levels, namely the cost attribute. This allows the analyst to trace out the  
 6 fraction of employees who would subscribe at each presented level of cost.

7 In more general DCE formats, one often sees: (a) more than one choice set asked,  
 8 (b) more than two alternatives offered, and (c) more than one attribute varied.  
 9 Frequently, however, only one or two of the three generalizations of the SBC format  
 10 are used, so it is useful to keep in mind the specific generalization of the SBC format  
 11 when thinking about issues involving DCEs. The nature and properties of different  
 12 experimental designs become more important as DCE formats grow more compli-  
 13 cated and we systematically illustrate where new issues arise and how the statistical  
 14 models that can be estimated are tied to the experimental design used to construct  
 15 a SP survey.<sup>5</sup>

16 From an applications standpoint, we focus on three cases:

- 17 1. A new good may be provided, and if it is provided, the person using it has to pay  
 18 for it (everyone pays if it is a pure public good provided via coercive taxation).  
 19 Interest lies in estimating total willingness to pay (WTP) for having the good sup-  
 20 plied rather than the current status quo good.
- 21 2. One wishes to estimate the WTP to have one or more new alternatives added to a  
 22 set of choices available to a consumer.
- 23 3. One wishes to estimate the WTP for a change in one or more of the attributes of an  
 24 alternative.

25 We begin our discussion by laying out the theoretical welfare measures for the three  
 26 above cases. We then introduce the basic concepts of experimental design in the con-  
 27 text of the SBC format. Next, we illustrate the issues involved in moving to different  
 28 types of DCE formats. Finally, we try to provide guidance to applied researchers who  
 29 want to conduct a DCE study using reasonably efficient designs where the statistics of  
 30 primary interest are statistically identified.

## 31 10.2 Economic welfare measures for health policy changes

32 SP surveys analyzed from a random utility perspective often aim to produce estimates  
 33 for policy purposes; hence, we briefly review the theory relating to welfare economic  
 34 measures of value. Literature on this topic is vast, so to more fully appreciate the  
 35 issues involved, interested readers may wish to pursue comprehensive treatments in  
 36 (16–18).

37 We begin by denoting the item being valued by  $q$  and initially treat this as a single  
 38 item that could be a commodity or a program involving some mix of commodities

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5 There are many other relevant cases for measuring changes in welfare. In particular, there are a set of analogous measures that focus on minimum willingness to accept (WTA) compensation for undesirable changes (see Chapter 6 & 7).

1 treated as a fixed group – the key feature is that  $q$  is a scalar. Later, we will let  $q$  consist  
 2 of a bundle of attributes and we will ask questions about how a change in one attribute  
 3 influences economic values. The latter does not change the underlying framework as  
 4 one can define two distinct  $q$ 's that differ only in a change in one attribute of interest.  
 5 We assume that a consumer has a utility function defined over quantities of various  
 6 market commodities, denoted by the vector  $x$ , from which a consumer can freely  
 7 choose and the item  $q$ . Thus, the direct utility function is given by  $u(x, q)$ . Often analysts  
 8 work with the corresponding indirect utility function,  $v(p, q, y)$ , where  $p$  is the vector  
 9 of the prices of the market commodities  $x$ , and  $y$  is the consumer's income.<sup>6</sup> We make  
 10 the conventional assumption that  $u(x, q)$  is increasing and quasi-concave in  $x$ , which  
 11 implies that  $v(p, q, y)$  satisfies the standard properties with respect to  $p$  and  $y$ <sup>7</sup>; but  
 12 we make no assumptions about  $q$ . If the agent regards  $q$  as a 'good',  $u(x, q)$  and  
 13  $v(p, q, y)$  both will be increasing in  $q$ ; if it is regarded as a 'bad',  $u(x, q)$  and  $v(p, q, y)$   
 14 both will be decreasing in  $q$ ; and if the agent is indifferent to  $q$ ,  $u(x, q)$  and  $v(p, q, y)$   
 15 both will be independent of  $q$ . We make no assumption about quasi-concavity with  
 16 respect to  $q$ .

17 The act of valuation implies a contrast between two situations: one with item  $q$ , and  
 18 one without  $q$ . This is an important concept because economic valuation always  
 19 involves a comparison/tradeoff between two or more situations where the 'or more'  
 20 part always can be rewritten as a set of binary comparisons. We interpret what is being  
 21 valued as a change in  $q$ .<sup>8</sup> Specifically, suppose that  $q$  changes from  $q^0$  to  $q^1$ ; the con-  
 22 sumer's utility changes from  $u^0 \equiv v(p, q^0, y)$  to  $u^1 \equiv v(p, q^1, y)$ . If she sees this change  
 23 as positive,  $u^1 > u^0$ ; if she sees it as negative,  $u^1 < u^0$ ; and if she is indifferent,  $u^1 = u^0$ .  
 24 The value of the change to her in monetary terms is represented by the Hicksian  
 25 income compensation measure,  $C$ , which is the amount of money that satisfies:

$$26 \quad v(p, q^1, y - C) = v(p, q^0, y).^9 \quad (10.1)$$

<sup>6</sup> The definition of income is always problematic in empirical work. Ideally, it refers to some notion of permanent household supernumerary income, which is disposal permanent income less total expenditure on subsistence minima and previously committed expenditures.

<sup>7</sup> That is, we assume  $v(p, q, y)$  is homogeneous of degree zero in  $p$  and  $y$ , increasing in  $y$ , non-increasing in  $p$ , and quasiconvex in  $p$ .

<sup>8</sup> The alternative is to represent it as a change in  $p$ . McConnell (19) adopts this approach for a valuation question of the form 'Would you accept a payment of \$A to give up your right to use this commodity for 1 year?' Let  $p^*$  be the choke price vector (*i.e.*, a cost vector such that, at these costs, the individual would choose not to consume the resource), and let  $p^0$  be the baseline price vector. McConnell represents the change as a shift from  $(p^0, q, y)$  to  $(p^*, q, y)$ .

<sup>9</sup> Note that we also can get the Hicksian equivalence measure, which in this case is WTA for giving up the right to  $q^1$ . If the sign of  $u^1 - u^0$  is positive then WTP and WTA will both be positive. The Hicksian income compensation function often is formally defined as the difference between two (Hicksian) expenditure functions, another alternative representation of the direct utility function that describes how much income is needed to achieve a specified level of utility given a price vector for marketed goods and the level of  $q$ .  $C = m(p, q^0, u^0) - m(p, q^1, u^0)$ . The first term on the right is simply equal to  $y$ .

To emphasize the dependence of the compensating measure on (i) the starting value of  $q$ , (ii) the terminal value of  $q$ , and (iii) the value of  $(p, y)$  at which the change in  $q$  occurs, we sometimes write it as:

$$C = C(q^0, q^1, p, y). \tag{10.2}$$

For a desirable change which the consumer does not have the property right to enjoy without paying

$$\text{WTP} = C(q^0, q^1, p, y). \tag{10.3}$$

One can parameterize either the WTP function directly (20) or begin with a parameterization of the underlying utility function (21). Typically, applied researchers fit simple linear or logarithmic functions, but one can fit more complex utility functions, and SP data often is better suited for this than RP data. We illustrate these concepts with a specific example, the Box–Cox indirect utility function:<sup>10</sup>

$$v_q = \alpha_q + \beta_q \left( \frac{y^\lambda - 1}{\lambda} \right), \quad q = 0, 1, \tag{10.4a}$$

where  $\alpha_1 \geq \alpha_0$  and  $\beta_1 \geq \beta_0$ . Equation (10.4a) can be regarded as a form of CES utility function in  $q$  and  $y$  with  $\lambda$  being the income elasticity of WTP. The corresponding formula for  $C$  is

$$C = \left( \frac{\beta_0 y^\lambda}{\beta_1} - \frac{\lambda \alpha}{\beta_1} + \frac{\beta_1 - \beta_0}{\beta_1} \right)^{\frac{1}{\lambda}}, \tag{10.4b}$$

where  $\alpha \equiv \alpha_1 - \alpha_0$ . McFadden and Leonard (22) employ a restricted version of this model with  $\beta_1 = \beta_0 \equiv \beta > 0$ , yielding

$$v_q = \alpha_q + \beta \left( \frac{y^\lambda - 1}{\lambda} \right), \quad q = 0, 1, \tag{10.5a}$$

$$C = y - \left( y^\lambda - \frac{\alpha}{b} \right)^{\frac{1}{\lambda}}, \tag{10.5b}$$

where  $b \equiv \beta/\lambda$ . This specification is somewhat flexible as it permits a variety of income elasticities of WTP; the income elasticity of WTP is negative when  $\lambda > 1$ , zero when  $\lambda = 1$ , and positive when  $\lambda \leq 0$ . It also nests many utility models in the existing literature. For example, if  $\lambda = 1$ ,  $C$  equals the familiar ratio of  $-\alpha/\beta$  often associated with a measure of mean WTP from a logit or probit model.<sup>11</sup>

Now, consider the case where there is more than one possible alternative to the status quo good. As long as a status quo good will remain available and a consumer

<sup>10</sup> For simplicity, we suppress  $p$  and write the indirect utility function as a function of  $q$  and  $y$ ; however,  $\alpha_q$  and/or  $\beta_q$  are in fact functions of  $p$  and  $z$ .

<sup>11</sup> Often there are measurement issues with respect to  $y$  that play a major role in econometric estimation, pushing empirical researchers to assume that  $\lambda = 1$ .

1 will only get utility from at most one good that is an alternative to the status quo good  
 2 then the economic value of the set of alternatives is simply the maximum WTP defined  
 3 over all binary paired comparisons involving the status quo good. If the status quo  
 4 good is chosen in a deterministic setting, WTP equals zero. Otherwise, the economic  
 5 value equals the maximum amount of money a consumer has to pay for the most  
 6 preferred of the alternatives relative to being indifferent between choosing the status  
 7 quo good and the most preferred alternative.<sup>12</sup>

8 One can make  $q$  a function of a bundle of attributes in the sense originally explicated  
 9 by Lancaster (23). Here  $\alpha_q$  is replaced by a function,  $g(z)$ , where  $z$  is a vector of attri-  
 10 butes. Typically,  $g(z)$  is represented as an additive linear function of the individual  
 11 attributes some of which are potentially indicator variables, i.e.,  $\gamma_1 z_1 + \gamma_2 z_2 + \gamma_3 z_3$ . In  
 12 this case, there is nothing particularly special from a welfare economic view from  
 13 moving from a change in a status quo good versus compared to changes in the attri-  
 14 butes of goods other than having to choose the functional form that specifies how the  
 15 attributes enter the utility function.<sup>13</sup> Other common ways to specify the attribute  
 16 function are: (a) an additive linear function of the logs of the individual attributes,  
 17 (b) an additive linear function of the individual attributes plus the first-order interac-  
 18 tions between the individual  $z$ , and (c) an additive linear representation like a Translog  
 19 utility function that is a second order approximation to an unknown function that  
 20 include squares of individual attributes and first order interaction terms between  
 21 attributes. When there are attributes in the model one often is interested in the  
 22 marginal effect on WTP of a change in  $z_i$ , which is simply  $\partial C/\partial z_i$ , or the relative effect  
 23 of a marginal change in one attribute relative to another attribute that can be scaled in  
 24 a comparable way by looking at  $(\partial C/\partial z_i)/(\partial C/\partial z_j)$ .

### 25 10.3 Going from choice to WTP estimates

26 SP questions measure a consumer's WTP (or WTA) for change in  $q$  or a discrete indi-  
 27 cator related to WTP. The utility theoretic model of consumer preference outlined  
 28 above provides a way to interpret responses to these questions. From a statistical  
 29 modelling viewpoint, the convention is to treat the survey responses as the realization  
 30 of a random variable. So, it is necessary to recast the deterministic model of WTP  
 31 outlined above into stochastic models that can generate probability distributions of  
 32 the survey responses. Mapping from a deterministic WTP model to a probabilistic  
 33 model of survey responses involves two steps: 1. adding a stochastic component into

12 The case where a consumer would use more than one of the alternatives is beyond the scope of this chapter and rarely is examined in SP choice models. Such cases are often dealt with by going to some type of allocation model or bundling alternatives so that bundles are mutually exclusive.

13 The only 'attribute' that plays a special role is a good's cost. A consumer does not get utility from the cost of a good per se, but instead via the effect of a good's cost on income. In most empirical work this distinction is ignored or an implicit assumption is made about the marginal utility of income so that the only thing that enters the utility function for two (or more) goods is the difference in the cost of the goods.

1 the deterministic utility model that leads to what is called a *WTP distribution* and 2.  
 2 specifying a connection between the WTP distribution and what we will call the *survey*  
 3 *response probability distribution* based on the assumption of a utility-maximizing  
 4 response to the survey question. We denote the WTP cumulative distribution func-  
 5 tion (cdf) as  $G_C(z_p)$ ; for a given individual it specifies the probability that the  
 6 individual's WTP for the item in question is less than the cost  $z_p$ , and we now use  
 7 the convention of denoting attributes of the good as  $z_i$  and the special attribute of  
 8 cost as  $z_p$ :

$$9 \quad G_c(z_p) \equiv Pr(C \leq z_p), \quad (10.6)$$

10 where the compensating variation,  $C$ , is now viewed as a random variable.<sup>14</sup> The  
 11 corresponding density function is denoted as  $g_C(z_p)$ .

12 We illustrate this via a simple example with a *closed-ended, single-bound* discrete  
 13 choice format. That is, a respondent is asked 'Would you favour a change from  $q^0$   
 14 to  $q^1$  if it would cost you  $z_p$ ?' Suppose the response is 'yes'. This means that the value  
 15 of  $C$  for this individual is some amount more than  $z_p$ . In terms of the underlying WTP  
 16 distribution, the probability of obtaining a 'yes' response is given by

$$17 \quad Pr(\text{Response is 'yes'}) = Pr(C \geq z_p) \equiv 1 - G_c(z_p). \quad (10.7)$$

18 Note that a response to this question does not reveal the exact value of  $C$ , but instead  
 19 provides information that  $C$  lies in an interval bounded from above or below by  $z_p$ .<sup>15</sup>

20 There are two basic sources of the stochastic component: (a) factors related to the  
 21 nature of the good or the consumer that influence choice, and are known to the con-  
 22 sumer but unknown to the analyst (e.g., 25;26) and (b) a true random component  
 23 potentially including recording and optimization errors.<sup>16</sup> These two sources effec-  
 24 tively are equivalent from the perspective of the simplest statistical estimators, but  
 25 they have quite different implications for WTP estimates. While source (a) leads to  
 26 the well-known model of random utility maximization (RUM) in which error compo-  
 27 nents play an integral role in the estimate of summary statistics involving WTP distri-  
 28 butions; in contrast, it is desirable to purge source (b) error components from WTP  
 29 estimates. These two views of error sources also have different implications for what  
 30 might be observed. Under (a) the probability of picking a dominated alternative  
 31 should be zero, while under (b) some respondents should pick dominated alternatives  
 32 with positive probability. For more complex statistical estimators (a) leads one to try

14 For now, we assume the change is regarded as an improvement so that  $C$  measures WTP.

15 Other relevant information may help to more tightly bound the interval in which the consumer's WTP lies. For example, if the alternative cannot be a 'bad', it may be reasonable to assume a distribution for WTP with no support in the negative range. Carson and Jeon (24) look at ways to use constraints on the upper end related to income.

16 Excellent discussions of the two perspectives are provided by Hanemann (21) and Cameron (20). McConnell (19) lays out the relationship between the two from the perspective of estimating welfare measures. It is important to note that analysts often claim to estimate a RUM model but use measurement error perspectives when calculating WTP measures.

1 to allow for heteroscedasticity in preference parameters, while (b) leads one to try to  
 2 model the error term to allow for heteroscedasticity in some fashion.<sup>17</sup>

3 The RUM approach proceeds by specifying a particular indirect utility function  
 4  $v(p, q, y; \varepsilon)$  and a particular distribution for  $\varepsilon$ . An example of a RUM version of the  
 5 restricted Box–Cox model is

$$6 \quad u_q = \alpha_q + \beta \left( \frac{y^\lambda - 1}{\lambda} \right) + \varepsilon_q \quad q = 0, 1, \quad (10.8a)$$

7 where  $\varepsilon_0$  and  $\varepsilon_1$  are random variables with a mean of zero. Consequently,

$$8 \quad C = y - \left( y^\lambda - \frac{\alpha}{b} - \frac{\eta}{b} \right)^{\frac{1}{\lambda}}, \quad (10.8b)$$

9 where  $\alpha \equiv \alpha_1 - \alpha_0$ ,  $b \equiv \beta/\lambda$ , and  $\eta \equiv \varepsilon_1 - \varepsilon_0$ .

10 In contrast, if we take the second view and operationalize it with an additive error  
 11 term, we would have

$$12 \quad C = C(q^0, q^1, p, y) + \varepsilon. \quad (10.9)$$

13 In the case of the Box–Cox model (7), for example,

$$14 \quad C = y - \left( y^\lambda - \frac{\alpha}{b} \right)^{\frac{1}{\lambda}} + \varepsilon. \quad (10.10)$$

15 Comparison of (10.8b) with (10.10) illustrates the difference between the two  
 16 approaches to formulating a WTP distribution. Inserting an additive random term in  
 17 utility function (10.8a) leads to a random term that enters into the formula for  $C$  in a  
 18 *non-additive* manner. Even when both approaches lead to similar estimates for mean  
 19 and median WTP, the implied pdf's may be quite different, particularly in the tails.

20 Often there can be substantial problems in empirically estimating WTP measures  
 21 from discrete choice data from either survey choices or market choices.<sup>18</sup> The prob-  
 22 lems largely stem from the fact that in all discrete choice models the parameters are  
 23 identified only up to a scale factor.<sup>19</sup> Because of scale, WTP estimates are a ratio of  
 24 parameter estimates; hence, they can be ill-behaved even if the individual parameter  
 25 estimates are normally distributed (as suggested by the theoretical foundation

17 Under (a) the main source of heterogeneity typically is assumed to be differences in prefer-  
 ences, while under (b) the main source of heterogeneity typically is assumed to be differences  
 in ability to answer questions.

18 Indeed, it usually is better to work with SP data than RP data because the cost variable  
 typically has a much more limited range and there are high correlations between various  
 attributes.

19 In the case of binary discrete choice, where only cost is varied, one can use non-parametric  
 techniques to avoid some of the problems associated with the scale issue. However, these  
 techniques give much coarser estimates and have not been generalized to the multinomial  
 choice case. See (18) for a discussion.



1 underlying maximum likelihood estimation) because the ratio of two normal vari-  
 2 ables is distributed as a Cauchy distribution (although this can be simulated). Further,  
 3 there is a very tight relationship between the functional form assumed for the cost ( $z_p$ )  
 4 variable and the assumed distribution of WTP.<sup>20</sup> For example, it is common to specify  
 5  $\ln(z_p)$  in a logit model, which implies that the WTP distribution is log-logistic.  
 6 Unfortunately, this distribution has an (implausible) infinite mean for a wide range  
 7 of estimated parameter values, although it typically cannot be distinguished from a  
 8 log-normal (or a Weibull) that has a finite mean in terms of statistical fit.

9 The problem is that similarly shaped WTP distributions over a wide range of mon-  
 10 etary values may have very different behaviour in the far tails,<sup>21</sup> and using more flex-  
 11 ible functional forms to allow heterogeneity in preferences can exacerbate problems.  
 12 For example, if one specifies a cost variable as a random effect and assumes the effect  
 13 to be normally distributed, it typically implies that some consumers have a negative  
 14 WTP even if this is implausible. This may concentrate a large fraction of the distribu-  
 15 tion near zero, causing traditional formulas for mean WTP to blow up.<sup>22</sup>

16 Often problems in estimating mean WTP are not reported because the analyst  
 17 assumes them away by estimating a logit or probit model with a linear specification for  
 18 the cost variable, forcing mean and median WTP to be equal. A similar problem  
 19 occurs if one estimates a model with the log of cost as a regressor but mistakenly  
 20 assumes that the correct formula for mean WTP is  $\text{EXP}[-\alpha/\beta]$ , where  $\alpha$  is the  
 21 constant term (assuming no other attributes) and  $\beta$  is the coefficient on  $\ln(\text{cost})$ .  
 22 This is the correct formula for median WTP but the correct formula for the mean  
 23 includes a function of the variance, such as the following for a normal distribution:  
 24  $\text{mean WTP} = \text{EXP}[-\alpha/\beta] \text{EXP}[1/2\beta^2]$ . Often a better solution to this problem is to  
 25 recognize that percentiles of the distribution, including the median, usually can be  
 26 reliably estimated far out in the tails. Traditional welfare economics focuses on mean  
 27 WTP but policymakers typically care about more than one summary statistic of the  
 28 WTP distribution.

## 29 10.4 Experimental design for a single binary discrete 30 choice question

31 The simplest case for experimental design of a choice experiment occurs when one  
 32 asks a single binary discrete choice CV question of each respondent and only one  
 33 attribute (typically cost ( $z_p$ )) is varied, as earlier noted. Collection of discrete choice

20 Typically, analysts use the cost of the alternative instead of the more theoretically suitable income minus cost, which is justified by particular assumptions about marginal utility of income. A large amount of measurement error in income also may offset the theoretically desirable properties.

21 Cost amounts in the far tails are rarely if ever observed in market data and it may be implausible to ask respondents about them in SP surveys.

22 The key issue is that a non-trivial fraction of consumers may be indifferent to the introduction of any of the alternatives to the status quo, leading to a spike at zero that formally can be modeled as a mixture distribution (27).

1 data requires the use of a set of design points that represent the cost to agents who are  
 2 then randomly assigned to those design points. Choice of these design points can  
 3 greatly influence how many observations are required for a given level of statistical  
 4 efficiency, which is often referred to as the precision of the estimate.<sup>23</sup>

5 We begin by considering a linear regression model measuring WTP as a function of  
 6 changes in a single design factor, say the number of treatments an insurance plan  
 7 would pay for,  $z_i$ , where the line goes through the origin if the value of the design  
 8 factor is zero. Now, we ask the question ‘if you have  $n$  observations and can run the  
 9 experiment at two values of the factor, what values would you chose and how many  
 10 observations should you allocate to each to minimize the confidence interval of the  
 11 WTP estimate at a particular level of the factor’ (i.e., the estimated slope parameter  
 12 times the factor level of interest)? In this case, the confidence interval for WTP is  
 13 simply a function of the confidence interval for the slope parameter, so one should  
 14 choose two values of the factor that are as far apart as feasible. In the case of DCEs, the  
 15 two values should be chosen to be as far apart as is plausible to respondents. One  
 16 should allocate half the sample to each of these two values; and it is straightforward to  
 17 show that this minimizes the confidence interval on the slope parameter.<sup>24</sup> This is a  
 18 desirable property of a simple DCE because in cases where the expected response to  
 19 cost is linear, one only needs two levels of cost to accurately estimate the slope. The  
 20 trick is to ensure that the two points are placed sufficiently far apart to cover much of  
 21 the response distribution, but not so far into the tails of the distribution that one  
 22 observes only a few choices.

23 For example, consider a plan where as before WTP for the plan varies with the  
 24 number of treatments paid for, but the plan also has other fixed benefits (e.g., infor-  
 25 mation, access to other services at discounts) that do not vary with the number of  
 26 treatments. Let us represent the WTP for a plan with no treatments by  $\alpha$ . Now, WTP  
 27 for a plan is represented by  $\alpha + \beta z_i$ , and the objective is to minimize the confidence  
 28 interval around this quantity for a particular  $z_i$ . One can do this by choosing two  
 29 values for  $z_i$  that are as far apart as possible because the confidence interval for  $\alpha$  also  
 30 is minimized by this choice.

31 Much of this basic intuition extends to binary discrete choice models with a single  
 32 factor, typically cost. DCEs for these models are analogs of dose–response experiments  
 33 in medical and related applications; For example, instead of ‘cost’, experimenters vary  
 34 the magnitude of a dose of – say – an insecticide, and analytical interest focuses on the  
 35 percent of the sample population alive falls as dose amount increases. In the case of a  
 36 DCE for – say – a dental insurance plan, the ‘dose’ is the levels of cost, and analytical  
 37 interest focuses on the fraction still in ‘favour’ as cost increases. Different choice models

<sup>23</sup> Like survey design, experimental design is not generally taught in economics departments. A classic text is (28). A more modern, comprehensive reference is (29) or (30).

<sup>24</sup> The slope parameter is proportionate to the reciprocal of the square root of  $\sum_i (z_i - E(z_i))^2$ . Given any finite constraint on how far apart the two values of  $z_i$  can be from each other, it is possible to show that this quantity is maximized by placing half of the  $z_i$  at each end of the constrained distance.

1 have different likelihood functions and most are non-linear in the model parameters,  
 2 which has four major implications:

- 3 1. The curvature of the likelihood function for most commonly used choice models  
 4 suggests that the design points should be closer together than in a traditional linear  
 5 model, but the general principle that they should not be very close remains. The  
 6 main caveat is not to place the design points too far in the tails because there is too  
 7 little density to accurately measure the choice probabilities in samples of reasona-  
 8 ble size.
- 9 2. The optimal design will depend on the number of parameters in the underlying  
 10 distribution.
- 11 3. The optimal design also will depend on the values of those parameters. Generally,  
 12 one does not need more design points than parameters, but to be able to test more  
 13 general distributions than one assumes, more design points are needed. Yet, the  
 14 general principle is that if one fits a parametric distribution characterized by a  
 15 small number of parameters, one should have relatively few design points so the  
 16 distribution can be estimated with reasonable precision at a small number of  
 17 places.
- 18 4. The choice of  $z_i$  that minimizes the confidence interval on  $\beta$  in non-linear models  
 19 generally is not the one that minimizes confidence intervals on functions of  $\alpha$  and  
 20  $\beta$ , and hence, the confidence interval for WTP.

21 As noted earlier, in the simplest case, estimates of mean (and median) WTP are a  
 22 ratio of two parameters ( $-\alpha/\beta$ ), where  $\alpha$  is the estimate of the constant from a logit or  
 23 probit model and  $\beta$  is the estimate of the cost parameter. Two basic criteria are used  
 24 in the stated preference literature for this case: 1. directly minimize the confidence  
 25 interval around the mean WTP estimate and 2. maximize the determinant of the  
 26 information matrix for the estimated parameters. Statistical designs that minimize  
 27 confidence intervals around mean WTP are known as C-optimal designs. Alberini and  
 28 Carson (31) and Alberini (32) show that C-optimality can be substantially more  
 29 efficient (on the order of 50%) than maximizing the determinant of the information  
 30 matrix (D-optimality) under conditions relevant to DCE studies.<sup>25</sup> Both the C- and  
 31 D-optimality criteria lead to choosing only two design points if the underlying distri-  
 32 bution can be fully characterized by two parameters and the design is not constrained  
 33 to have more design points.<sup>26</sup> C- and D-optimal designs differ in where the points  
 34 are placed, with D-optimal designs generally placing them further in the tails of the  
 35 distribution.

25 C-optimal designs are closely related to fiducial designs popular in biometrics.

26 A design can be constrained to have more design points, but forcing a design to have four design points results in two design points being replicates, or being arbitrarily close to the two original points if they are forced to be distinct. If the distribution is assumed symmetric, equal numbers of observations generally are assigned to design points on either side of the median; asymmetric distributions can result in an asymmetric assignment of observations being optimal.

1 D-optimal designs are popular even in binary discrete choice cases with one  $z_p$  as a  
 2 regressor, as it is natural to think in terms of maximum likelihood estimation; they also  
 3 are easier to construct than C-optimal designs. D-optimal designs become more com-  
 4 pelling in cases where goods are bundles of attributes and interest lies not in a single  
 5 WTP estimate but in WTP estimates for a sizeable number of marginal tradeoffs. In this  
 6 case, D-optimal designs effectively strike a balance in estimating all the marginal effects  
 7 with reasonable precision for a given sample size. Much of the rest of this chapter is  
 8 devoted to D-optimal designs when there are multiple attributes of interest.

9 Generally speaking, a D-optimal design is one that minimizes the determinant of the  
 10 Fisher Information Matrix associated with a particular class of designs. The best  
 11 D-optimal design is the one with the largest determinant. Street and Burgess (33) show  
 12 that such designs exhibit level balance (each level of each attribute occurs equally often),  
 13 and the differences in the attribute levels are orthogonal. It is difficult to generalize  
 14 beyond this description because designs for non-linear models like choice models  
 15 depend on the particular problem specification, namely the number of attributes, the  
 16 number of levels associated with each attribute, the indirect utility specification associ-  
 17 ated with the problem, and the form of the underlying choice process model.

18 Both C- and D-optimality rely on certain knowledge of the model parameters, but  
 19 this is never satisfied in practice because if the parameters were known there would be  
 20 no need to do an experiment. Yet, one usually has at least some knowledge of the likely  
 21 parameter values, so a good way to begin is to ask if theory can bound the parameter  
 22 space, with inequality constraints being quite useful. Additionally, does existing litera-  
 23 ture on related goods help to bound the likely estimate of mean/median WTP? An  
 24 obvious next step is to use data from pre-test and pilot studies to assist with this. Such  
 25 a process is better thought of as 'sequential design', and Kanninen (34) discusses issues  
 26 related to such a sequential design process. In general, the more uncertainty about the  
 27 nature of the underlying WTP distribution, the more design points one should use,  
 28 which can be shown using a formal Bayesian approach to design problems. Yet, one  
 29 needs to recognize that there is a clear tradeoff between the precision at which the  
 30 distribution is pinned down at individual design points and the number of design  
 31 points.<sup>27</sup>

## 32 10.5 Generalizing attributes of binary discrete 33 choice questions

34 Now, we consider what happens if an attribute is not continuous, but instead categor-  
 35 ical, with greater than two levels. For example, an attribute of a GP practice might

.....  
 27 Alberini and Carson (31) suggest it is hard to justify more than eight design points, and  
 show that four to six design points spanning the expected quartiles of the expected WTP give  
 estimates that are reasonably efficient and robust to fairly large deviations in expected and  
 observed WTP distributions, as long as the presumed distribution of WTP is of low dimen-  
 sionality. McFadden (35) shows that a very different design is required if one wants to be  
 able to consistently estimate mean WTP without making parametric assumptions about the  
 nature of the distribution; this design involves spacing a large number of design points over  
 the support of the WTP distribution.

1 be opening hours, such as 9–5, 9–7 and 9–9. If only this attribute is presented and the  
 2 respondent’s response options are to keep the status quo or choose the new opening  
 3 hours, the theory and analysis are the same as in Section 10.4. Now, consider the case  
 4 of adding a cost attribute with three discrete levels that cover the possible range of  
 5 costs to opening hours. Now, we have a case where we must jointly vary two attributes  
 6 each having three levels. The design involves both attributes; all combinations of them  
 7 represent a  $3 \times 3$  factorial design (= 9 combinations).

8 The implied DCE involves offering a respondent nine (or fewer) combinations of  
 9 opening hours and costs. For each of the nine combinations that we will call ‘scenar-  
 10 ios’, a respondent is asked whether they will stay with the status quo or switch to the  
 11 new health service represented by a particular level of opening hours and a level of  
 12 cost. Any design that uses less than all nine combinations will have some parameters  
 13 that are not statistically identified without some identifying assumption/restriction  
 14 for the underlying utility function.

15 If one believes that the true relationship between utility and cost is linear, the proper  
 16 way to design this experiment is to only use two levels for the cost attribute, as dis-  
 17 cussed in Section 10.4. Thus, this DCE would have only six combinations. On the  
 18 other hand, if one does not know the true relationship, and it is possible, perhaps  
 19 likely, that it is non-linear, then one needs to assign *at least* three levels to the cost  
 20 attribute. Typically, one would assign four levels to the cost attribute to be able to  
 21 visualize relationships between utility and cost and rule out a quadratic polynomial if  
 22 it is inappropriate. For example, if the true relationship is S-shaped, one needs at least  
 23 four levels to visualize and test this.

24 The previous theoretical insights also apply to this case. That is, one may wish to  
 25 value a change in opening hours from 9–5 to 9–9. This requires one to estimate  
 26 the value of the utility difference between the two levels of opening hours, and if the  
 27 relationship between utility and cost is linear, one would divide this utility difference by  
 28  $\beta$ , the estimate of the cost effect. If the status quo option varies across consumers, one  
 29 must calculate the difference between the status quo and the proposed change in open-  
 30 ing hours for each consumer and use the method of sample enumeration (36) to calcu-  
 31 late the implied WTP. As before, if cost is treated as a random effect, one needs to  
 32 calculate statistics for the WTP distribution, and one may need to simulate the WTP  
 33 distribution in the case of complex models that allow random effects and covariances  
 34 among effects and/or non-constant diagonal error variances and covariances.

35 Lancsar and Savage (42) discuss calculation of WTP for cases involving forced choice  
 36 of one or more of the alternatives compared with the status quo, relying heavily on  
 37 Hanemann’s (21) discussion of issues involved in applying welfare ideas to discrete  
 38 choice problems. For example, in the case of a simple binary choice model where the  
 39 choice is between a constant status quo and a series of one-at-a-time designed choice  
 40 options, one needs to examine the utility of each alternative and the probability  
 41 of each alternative being chosen using ‘expected utility’. The WTP expression for  
 42 this case is

43 
$$\frac{1}{\beta(\text{cost})} \left[ \ln \sum_{i=1}^n e^{V_i^0} - \ln \sum_{i=1}^n e^{V_i^1} \right], \quad (10.11)$$

1 where the cost effect is as previously defined, and  $\ln \sum_{i=1}^n e^{V_i}$  is the so-called ‘inclusive  
 2 value’, or expected maximum utility for the status quo (superscript 0) and the alter-  
 3 native of interest (superscript 1). So, (10.11) tells us that in cases where consumers  
 4 can choose two or more alternatives one must evaluate the difference in expected  
 5 utility between two options divided by the cost effect to calculate WTP. If the cost  
 6 effect is non-linear, a more complicated expression is required but the concept is  
 7 the same. If both cost and attribute are random effects, one must simulate the  
 8 distribution.

9 Including more attributes is a direct extension of the above discussion. In general,  
 10 for attributes  $X_1(l_1), X_2(l_2), \dots, X_k(l_k)$ , the total number of combinations is given by  
 11 the full factorial expansion  $X_1(l_1) \times X_2(l_2) \times \dots \times X_k(l_k)$ , where  $X_k$  is the  $k$ th attribute  
 12 and  $l_k$  is the number of levels of that attribute. Thus, a DCE that involves asking a  
 13 sample of respondents to compare a status quo option with some number of designed  
 14 options one-at-a-time can be designed by (a) constructing the full factorial, and if  
 15 sufficiently small, assigning all respondents to it, or if too large to do that, blocking  
 16 the factorial into subsets (typically, randomly assigning sets without replacement) and  
 17 assigning respondents randomly to each block (version); (b) using a fractional factor-  
 18 ial design to sample from the full factorial and assigning all respondents to the  
 19 scenarios given by the fraction, or blocking the fraction as described for the factorial,  
 20 and randomly assigning respondents to one of the blocks (versions).

21 Random utility again underpins the specification of statistical models used to  
 22 describe the choice process of respondents who participate in such DCEs. That is, as  
 23 before we think of an indirect utility function with systematic and random compo-  
 24 nents. Respondents seek to maximize their utility in their choices, but the analyst fails  
 25 to include all factors known to the respondent and/or the respondent makes choices  
 26 imperfectly, giving rise to the random utility case. Appropriate statistical models of the  
 27 choice process for this case include (a) fixed effects for the attributes with additional  
 28 terms that represent interactions of observable covariates with the intercept that  
 29 reflects the propensity to choose the status quo versus the other options and/or inter-  
 30 actions with the attributes or (b) random effects for the intercept and/or attributes to  
 31 capture unobserved, latent differences in preferences (or, possibly a hybrid of a and b).  
 32 Currently, random effects models are popular with academics and practitioners, but it  
 33 remains unclear how to use such models to forecast choices and/or evaluate policies  
 34 that will occur in the future and/or in other locations unless one assumes that random  
 35 components are stable over time and/or space.

### 36 10.6 Multinomial alternatives

37 This case has two different versions: 1. there are multiple alternatives but all alterna-  
 38 tives are generic and 2. at least one of the multiple alternatives differs in some signifi-  
 39 cant way that requires the analyst to view this as ‘non-generic’ (or, ‘alternative-specific’).  
 40 For example, suppose a person’s GP asks them to have a particular diagnostic test, and  
 41 informs them that the test service is provided by (a) several named hospitals, (b) several  
 42 named clinics, and (c) several named stand-alone testing services. Suppose further

1 that the testing options can be described by (i) waiting time to be tested, (ii) locational  
 2 convenience to the person, and (iii) cost.

3 If the objective is to understand and model the *type of service* that will be chosen by  
 4 people facing this decision, or the *particular named option* within each type of service  
 5 that will be chosen, then the problem is alternative-specific. If, on the other hand, the  
 6 objective is to understand people’s decisions/preferences for types or services and  
 7 features of these services, where particular manifestations of the service options avail-  
 8 able for each type are examples, then the problem is generic. That is, alternative-spe-  
 9 cific problems arise when one wants to model the choices of particular named options  
 10 that are members of a general class of options; generic options arise when one wants  
 11 to model the choices of non-named options that lie within the general class. The  
 12 former provides very specific information about the choices of particular options that  
 13 would be of interest to – say – the owners of each type of option (e.g., owners of testing  
 14 clinics); the latter provides very general information about the entire class of possible  
 15 options. Tables 10.1a and b below illustrate two possible choice tasks for these cases  
 16 using the testing example.

17 These cases are treated at length in (1), Louviere, Hensher, and Swait (hereafter  
 18 ‘LHS’) as ‘generic’ and ‘alternative-specific’ DCEs. For generic DCEs, designs discussed  
 19 and illustrated in LHS are obsolete because optimal design theory developed by  
 20 Deborah Street, Leonie Burgess and colleagues (e.g., (33)) provides the theory and  
 21 methods to construct optimal designs for this case. For ‘alternative-specific’ DCEs, the  
 22 design theory originally proposed by Louviere and Woodworth (37) remains the pri-  
 23 mary way to construct such experiments. It is important to note that in the latter case,  
 24 identification issues are well-understood and typically can be satisfied in virtually all

**Table 10.1a** An alternative-specific task

Features	Hospital	Clinic	Stand-alone
Wait time for testing	Same day	1 week	Next day
Locational convenience	15 min away	1 hour away	2 hour away
Cost	\$75	\$50	\$100
I most likely will choose:	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

**Table 10.1b** A generic task

Features	Option A	Option B
Type of test service	Hospital	Clinic
Wait time for testing	1 week	Next day
Locational convenience	2 hour away	15 min away
Cost	\$100	\$50
I most likely will choose:	<input type="checkbox"/>	<input type="checkbox"/>

1 applications but the efficiency of the designs relative to an optimal design is  
2 unknown.

3 Those who wish to construct optimally efficient designs for the generic case should  
4 consult Street and Burgess (33), which provides software to help analysts implement  
5 the design theory. In the case of alternative-specific designs, the theory detailed in LHS  
6 (1) or Louviere and Woodworth (37) provide the way to construct the designs. In the  
7 case of generic designs, attribute parameters specified in indirect utility functions are  
8 the same for all choice options, whether specified as fixed or random. In the case of  
9 alternative-specific designs, attribute parameters can be specified to be the same for  
10 some, but not all effects. That is, at least one attribute effect must differ for at least one  
11 alternative, regardless of whether the effects are specified as fixed or random.

12 Generic DCEs and associated models are consistent with the previous discussion of  
13 the theory that underlies calculation of WTP. If some model effects are alternative-  
14 specific, this implies that WTP will differ by (at least one) alternative. If cost effects are  
15 alternative-specific, this raises interesting issues over which cost effects to use in WTP  
16 calculations, as different cost effects imply different values of marginal values for  
17 income. Our position is that one generally should use cost effects associated with a  
18 given option to calculate the WTP associated with changes for that option, particularly  
19 when the cost for the base option is known. Comparisons between multiple programs  
20 are more complicated and, for this reason, researchers often try to use only one cost  
21 parameter unless there is clear evidence to the contrary.

22 A variation on the above theme is a DCE that presents respondents with multiple  
23 choice options, where one of these options is a constant option. To this point, the  
24 constant option has always implicitly been the status quo, but when multiple choice  
25 options are offered to respondents, a logical choice often is to choose none of  
26 the options, which is feasible since it involves zero cost. In the case of a constant status  
27 quo option, one can choose to incorporate the attribute levels of the status quo option  
28 in the estimation matrix or treat it as a fixed or random effect. In the case of the  
29 'choose none' option, there are no associated attributes, and hence, one must be care-  
30 ful about how to specify this option. For example, as before, one can choose to allow  
31 it to be a fixed or random effect and/or one can allow the variance of the random  
32 component associated with this option to differ from the other options (as in nested  
33 or tree logit models).

## 34 10.7 Common designs

35 Two good sources of information about designs used in SP studies are LHS (1) and  
36 Street and Burgess (33). As these sources are available, our focus here will be on briefly  
37 describing the options and their advantages and disadvantages.

### 38 10.7.1 Ad hoc designs

39 From time-to-time one sees ad hoc designs used in SP studies. By 'ad hoc' we mean a  
40 design that is constructed without reliance on formal statistical design theory. Basically,  
41 one should *NEVER* do this, primarily because the properties of such designs are rarely  
42 known in advance, and it is likely that they are (a) statistically inefficient relative to an



1 optimal design and/or (b) poorly conditioned, including the possibility of identifica-  
 2 tion issues. Because applied economists rarely receive training in experimental design,  
 3 and because econometrics historically has had to deal with ‘messy’ data, there is a  
 4 tendency for applied economists to think that ‘any design will do’. The sooner this  
 5 notion is dispelled, the better.

### 6 10.7.2 Full-factorial designs

7 These designs may not be practical because the number of combinations of attribute  
 8 levels can be very large. That said, let us distinguish two types of applications: 1. a  
 9 design administered to everyone in a sample and 2. a design blocked into ‘versions’  
 10 with respondents randomly assigned to a particular version without replacement.  
 11 Type 1 designs typically are used when one wants to be able to compare individuals  
 12 and/or if one wants to estimate a model for each individual. For example, one type of  
 13 comparison that often arises in practice is to group the individuals into segments  
 14 based on their choices. The latter application is well beyond the scope of this chapter.  
 15 Interested readers may wish to consult reference works on taxonomic methods such  
 16 as cluster analysis or latent class methods. Type 2 designs typically are used when the  
 17 design of interest has more than 16 or 32 attribute level combinations or choice sets  
 18 and one does not want to compare individuals’ choices directly.

19 Full factorials can be used as both type 1 and type 2 designs. In the case of type 1  
 20 designs, the class of factorials probably is restricted to those designs that have 32  
 21 choice sets or fewer, although it may be possible to use larger designs in certain cases  
 22 where the incentives are sufficiently high, such as paying physicians enough to moti-  
 23 vate them to ‘do’ perhaps 64 or more scenarios. In the case of multiple choice response  
 24 tasks, however, only very small factorials are possible for type 1 applications.  
 25 Researchers interested in such applications should consult Street and Burgess (33). In  
 26 the case of type 2 applications, it is likely that full factorials can be practical for many  
 27 cases because the factorial can be blocked into versions. For example, suppose that a  
 28 researcher wished to design an experiment for ten attributes, each with two levels  
 29 ( $2^{10}$ ). The full factorial has 1,024 attribute level combinations, and if the researcher is  
 30 confident that each respondent can and will ‘do’ 16 scenarios, the design can be  
 31 blocked into 64 versions, with each respondent randomly assigned to one version. So,  
 32 a full factorial of this size would be practical with samples of 400–800 people, which  
 33 are not uncommon in SP applications. In our experience, many researchers rule out  
 34 full factorials due to their size without realizing that they could have been used.

35 The major advantage of full factorials is that they allow one to estimate and test all  
 36 possible main and interaction effects. In type 1 applications, there typically is a lot of  
 37 statistical power to conduct these tests, and to the extent that one takes differences in  
 38 individuals into account (e.g., preference heterogeneity), one can estimate and test  
 39 these effects allowing for differences. The primary advantage of being able to estimate  
 40 and test interaction effects is that one does not have to assume strictly additive indirect  
 41 utility functions but instead can allow for more complex forms. The disadvantage, of  
 42 course, is that there are typically many more effects to estimate, so analyses are more  
 43 complicated. The advantage that is associated with type 1 applications does not neces-  
 44 sarily apply to type 2 applications because (a) the power of the tests will be less due

1 to smaller sample sizes associated with interaction effects, (b) it may not be possible  
 2 to take individual differences into account as easily or as thoroughly as one can with  
 3 type 1 applications because versions may be confounded with differences, and (c) it is  
 4 unrealistic to rely on assumptions that all respondents have exactly the same indirect  
 5 utility function.

6 In summary, full factorials probably can be used in many more applications than  
 7 most SP researchers think, although their use in multiple choice response tasks is  
 8 likely to be limited only to very small problems.

### 9 10.7.3 Orthogonal main effects plans

10 Orthogonal main effects plans (OMEs) are a sample of attribute level combinations  
 11 from the full factorial that have the property that all main effects are independent of  
 12 one another. The advantage of OMEs is that they typically are smaller than other  
 13 designs. The disadvantage, however, is that one must assume that the indirect utility  
 14 function is strictly additive for all respondents. Worse yet, if this assumption is false,  
 15 one cannot test and reject it. It probably is fair to say that OMEs are used much more  
 16 often than they should, particularly in so far as they are the most widely used designs  
 17 in SP research. If one has a type 1 design application, it may be that OMEs are the  
 18 only feasible option to allow rigorous comparisons of individuals. However, for type 2  
 19 design applications, it rarely would be the case that one would need to use an OME,  
 20 and so researchers should consider other options discussed below.

### 21 10.7.4 Designs that allow estimation of main and interaction 22 effects

23 Readers who want to construct and apply these designs should consult reference  
 24 works in the design literature, although a good starting place is LHS (1). These types  
 25 of designs are distinguished by what can be estimated and what must be assumed  
 26 about omitted effects:

- 27 1. Main effects are orthogonal to one another, and are also orthogonal to unobserved  
 28 but potentially significant two-way interactions. These designs protect estimates of  
 29 main effects from two-way interaction effects that cannot be estimated. Their  
 30 advantage is that they typically are relatively small(er), and hence, can be used in  
 31 many applications. Two-way interactions are the most likely interactions to be  
 32 significant and large, and so should be considered a key potential source of bias in  
 33 main effects when they are omitted. Another disadvantage is that one must assume  
 34 that all interactions of higher order than two-way are not significant, and that one  
 35 cannot test this assumption to determine whether it is false. These designs typically  
 36 need to be blocked into versions, but for smaller designs, it may be possible to use  
 37 them in type 1 design applications.
- 38 2. One also can construct designs for problems that involve estimation of all main  
 39 effects and a subset of the two-way interactions (known as ‘selected two-way inter-  
 40 actions’). It is hard to generalize about these designs because they typically are  
 41 constructed on a case-by-case basis, but sources of these designs exist, as noted in  
 42 Street and Burgess (33). The advantage of these designs is that they are smaller than

- 1 the design discussed below. The disadvantage is that designs for the exact subset  
 2 that a particular researcher is interested in may not exist, and one must assume that  
 3 all unobserved interactions are not significant, leaving one open to bias from  
 4 unobserved two-way interactions that are significant.
- 5 3. Main effects and two-way interactions are orthogonal to one another, and both  
 6 types of effects can be simultaneously estimated. These designs often are large,  
 7 especially if the number of attributes and levels is greater than 8–10. However, for  
 8 smaller problems, these designs have the advantage that they allow estimation of  
 9 both main and two-way interaction effects, but at the cost of assuming that all  
 10 other unobserved interaction effects are not significant. It rarely will be the case  
 11 that one can use these designs in a type 1 design application, so the vast majority of  
 12 applications of these designs will be for type 2 design problems.
- 13 4. One also can construct designs that allow one to orthogonalize the main effects and  
 14 two-way interactions to unobserved and potentially significant three-way interac-  
 15 tions. One also can construct designs that allow independent estimation of all  
 16 main, two-way and three-way interactions. These types of designs are rarely used  
 17 because they typically are fairly large, and require considerable design skill.

#### 18 10.7.5 D-optimal designs

19 As previously noted, these designs optimize the determinant of the Fisher Information  
 20 Matrix for the design. D-optimal designs for the case of all effects equal to zero have  
 21 been developed by Street and Burgess (33), and readers should consult this reference  
 22 for construction methods. What appears to be widely misunderstood about these  
 23 designs is the fact that Monte Carlo simulations show that these designs are optimally  
 24 efficient for choice probabilities that are not extreme and that they remain reasonably  
 25 efficient for very large and very small choice probabilities. So, they are a good choice  
 26 for almost all DCE problems. It also is worth noting that if one observes choice prob-  
 27 abilities in the far tails in a DCE, this implies a very poor choice of the attribute levels  
 28 or that the underlying process is almost deterministic. In addition to the Street and  
 29 Burgess designs, designs can be constructed using SAS macros developed by Kufeld  
 30 (38). Comparisons with Street and Burgess designs, however, suggests that the SAS  
 31 designs sometimes do not have diagonal information matrices and can require sub-  
 32 stantial computation time to construct a highly efficient design.

#### 33 10.7.7 Random designs

34 A number of SP researchers use what we call ‘random designs’. These designs are con-  
 35 structed in various ways, but typically one or more sets of starting designs are con-  
 36 structed or one randomly samples from the complete set of all possible choice sets. If  
 37 one uses a set of starting designs, say  $m$  of them, one typically randomly selects an  
 38 attribute level combination from each of the  $m$  simultaneously to create an  $m$ -tuple  
 39 that represents an  $m$ -element choice set. This design procedure was discussed by  
 40 Louviere and Woodworth (37), but modern advances in optimal design of DCEs has  
 41 made them obsolete. Similarly, some commercial DCE software creates choice sets by  
 42 drawing them randomly from the entire set of possible choice sets, which typically is

1 very large. Neither way of designing DCEs is a good idea since one cannot determine  
 2 *a priori* which effects can be estimated with any precision, nor can one identify *a priori*  
 3 what will be identified.<sup>28</sup> This approach also has the disadvantage that differences in  
 4 individuals may be confounded with differences in the choice sets faced. Our advice is  
 5 not to use this approach since better alternatives are available.

## 6 10.8 Using prior information

7 Naturally, it is always better to use whatever prior information one has available to  
 8 construct designs, as noted earlier. So, if one has theoretical or empirical reasons to  
 9 impose sign restrictions on the attribute effects, this will (a) restrict the classes of  
 10 designs to consider and (b) will restrict the indirect utility functions to be estimated.  
 11 For example, suppose there are three two-level attributes, and each has a known sign  
 12 for the main effects. If the indirect utility function is strictly additive, only four  
 13 scenarios are required to estimate the model in a binary discrete choice task. There are  
 14 16 possible binary response patterns that could be observed because each of the four  
 15 scenarios can receive either of the two binary responses ( $2^4$ ). Of these 16 patterns, only  
 16 seven are consistent with additivity and sign restrictions, assuming that basing  
 17 responses on only a single attribute or a pair of attributes is acceptable.

18 If the utility function is not additive, one must use the full factorial ( $2 \times 2 \times 2 = 8$   
 19 attribute level combinations), which greatly increases the allowable response patterns  
 20 to nearly 128, again assuming that one can base one's choices on one or a pair of attri-  
 21 butes as well as all three attributes. It should be obvious that as the number of attri-  
 22 butes and/or the number of response categories increase, the number of possible  
 23 response patterns that can be consistent with a particular set of sign restrictions grows  
 24 exponentially. Thus, as the number of attributes and/or the number of levels increases,  
 25 sign restrictions may not help bound the problem in any practical sense. As such, sign  
 26 restrictions are most useful for smaller problems.<sup>29</sup>

27 Finally, one can use a sequential design approach. In this approach, one uses exper-  
 28 iments on small(er) samples to explore as much of the design space as possible. This  
 29 approach can be viewed as a type of model selection problem where the objective is to  
 30 identify as many possible significant and meaningful effects as possible *a priori*, while  
 31 at the same time eliminating as many non-significant and non-meaningful effects as  
 32 possible. In this way, one can bound the problem, which may allow one to use a  
 33 smaller, special purpose design to identify and estimate the effects that one has *a priori*  
 34 reason to believe will be significant and meaningful.

28 Use of random design is often believed to identify all of the parameters of a model. That is true, however, only asymptotically as the design drawn approaches the full factorial. In the typical application, many parameters will not be statistically identified and other very poorly identified if subsamples receiving particular attribute combinations is small.

29 One also can impose informative priors on the parameters of the utility function. This takes one in the direction of Bayesian designs if uncertainty around the priors is formally quantified. If one is prepared to assume that the parameters are known with certainty, it is possible to determine the design that maximizes D-optimality.

1 **10.9 Desirability and implications of common**  
 2 **design criteria**

3 In this section, we review several design criteria that are frequently discussed in the  
 4 various DCE literatures.<sup>30</sup>

5 **10.9.1 Orthogonality**

6 Orthogonality of the effects to be estimated means that the information matrix is  
 7 block diagonal, and hence, all effects of interest can be estimated independently of one  
 8 another. This is a desirable but not essential criterion. What is essential is that the  
 9 degree of shared covariance between effects to be estimated is low.

10 **10.9.2 Level-balance**

11 This means that each level of an attribute occurs equally often, and more generally,  
 12 this should hold for all attributes. This criterion is associated with the precision of the  
 13 estimate of the attribute levels, such that if level balance holds, the model parameters  
 14 associated with each level will be estimated with equal precision. Again, this criterion  
 15 is desirable, but not essential. However, unless one has good reasons for not satisfying  
 16 level balance, such as one of three levels being far more important to the work than the  
 17 other two, it is desirable to satisfy this criterion. A similar criterion is a balanced level  
 18 co-occurrence. That is, if a design is orthogonal, it will be the case that the levels of  
 19 each pair of attributes will co-occur equally often. This criterion insures minimal  
 20 shared covariances.

21 **10.9.3 Attribute overlap**

22 This refers to correlations among two or more attributes, such that it may not be pos-  
 23 sible to vary them independently. This leads to what are called ‘nested’ factors/  
 24 attributes because the way to deal with these problems is to combine the attribute  
 25 levels into a single attribute that can be varied independently. For example, if a par-  
 26 ticular health service attribute is the amount of use of the system, and a second is the  
 27 cost of using the system, it is likely to be the case that higher levels of use will covary  
 28 with cost, so one would want to combine these two attributes into one.

29 **10.9.4 Elimination of dominated/infeasible alternatives**

30 If all the attributes are numerical and their signs are known *a priori*, any of the stand-  
 31 ard design constructions will lead to dominated and/or infeasible options. However,  
 32 while this happens in practice, our experience suggests that there is far too much con-  
 33 cern about this criterion than should be the case.<sup>31</sup> The first thing a researcher should

30 Viney, Savage, and Louviere (39) look at these concepts in the context of a specific empirical example.

31 In empirical applications, the more serious problem is likely to be that particular attributes are known to have sufficiently high correlation so that the absence of this correlation in attribute bundles is noticeable.

1 do is to determine whether it is reasonable to expect the respondent sample to actually  
 2 know which attribute level combinations are infeasible. Typically, the sample does not  
 3 know this; but the experts, such as doctors or medical researchers do. If the sample  
 4 cannot tell if a scenario or an option is infeasible, a researcher may want to proceed to  
 5 use standard design construction methods as these will provide significantly better  
 6 statistical properties than alternative methods.

7 If many respondents in fact know that something is infeasible and/or there can be  
 8 dominant options in choice sets, then one typically must modify the standard con-  
 9 struction methods to deal with this. For example, one way to deal with dominance is  
 10 to randomly replace one or more levels with levels that are non-dominant. Ideally,  
 11 one should test various random replacements, using those that minimally modify the  
 12 statistical properties of the design. Infeasible options are a different problem as  
 13 they have to be eliminated from the design. One way to do this is to construct the full  
 14 factorial of possible options that can be created from the attributes and levels of each  
 15 choice option (if the options are not generic), then eliminate all the infeasible combi-  
 16 nations and check the statistical properties of the remaining combinations. If the  
 17 shared covariances are not large and the inverse of the information matrix is well-  
 18 conditioned, then use that design. If the statistical properties are poor, then one could  
 19 try to select combinations from the feasible pool with the objective being to select a  
 20 sample that has the best properties.

### 21 10.9.5 Utility balance

22 It is not clear why this criterion is considered important, although it has achieved  
 23 considerable prominence in the marketing literature. Put simply, this criterion means  
 24 that one should try to construct choice sets in such a way that the options in each set  
 25 are as close in utility as possible. While this may seem like an intuitive criterion, if one  
 26 could achieve this objective, there would be **NO** useful statistical information pro-  
 27 vided by the choices. That is, satisfying this criterion is equivalent to making all the  
 28 choice options equally probable because if the option utilities are perfectly balanced,  
 29 the respondents should be totally indifferent to all of them, and so should choose  
 30 randomly. Thus, this criterion should not be used in the design of DCEs.

### 31 10.10 Concluding remarks

32 Experimental design is a key component of a successful choice experiment to help  
 33 evaluate health policy alternatives. It is all too easy to construct and implement designs  
 34 that do not statistically identify the parameters of interest or that greatly diminish the  
 35 precision of the estimates relative to what could have been achieved with an efficient  
 36 design. The underlying statistical theory for generic choice experiments is now well-  
 37 understood (33), and software for producing reasonably high quality designs is now  
 38 available (e.g., Street and Burgess design software that comes with their book); so,  
 39 there is little justification for choosing and using the poor quality designs that appear  
 40 all too often in the current literature.

41 Applied researchers need to think seriously about the attributes of the programs they  
 42 wish to compare and the class of underlying utility functions they wish to estimate.

1 Invest time upfront in extensive qualitative work of the type illustrated by the ICEPOP  
2 Program (31;41), where extensive and iterative qualitative work was used to under-  
3 stand not only attributes, but also key words and phrases. Also plan time for elaborate  
4 and extensive pre-testing to identify tasks and associated survey instruments that  
5 'work'. By 'work' we mean that (a) will be understood by all respondents, (b) will be  
6 meaningful to them, and (c) will simulate the actual choices one wants to observe as  
7 closely as possible. Often it makes sense to ask more choice questions rather than more  
8 complicated choice questions, or to ask more questions about the options in each  
9 choice set instead of more choice sets. Make reasonable restrictions on the nature  
10 of the utility function to reduce the size of the model space. Fix the attribute levels  
11 that are relatively unimportant to the policy issues being evaluated to further reduce  
12 the size of the model space and the task. More generally, one should avoid complex  
13 models and designs unless the project budget allows for extensive pretesting and large  
14 sample sizes.

15 Spend time observing the actual choices made by the population(s) of interest.  
16 Interview these populations and ask them how they make the choices, what are  
17 the pros and cons of each choice, and whether they feel like they have sufficient and/  
18 or 'the right' information to make the right decision(s); if they do not have sufficient  
19 or 'right' information, what would assist them? Relying on experts to tell you how  
20 consumers make decisions rarely is a good idea because few of them actually know  
21 this. If, in fact, the expert is the one who makes the decision for the consumer, then  
22 model the expert; yet, even here, one may want to understand and model how the  
23 expert's decision(s) impact(s) the consumer.

24 Finally, there is a great deal of misinformation and misunderstanding about the  
25 design of DCEs in the applied health economics literature. The literature on the  
26 optimal design of DCEs is highly technical, and it is easy to make mistakes as noted  
27 by (33;43). There are no quick fixes and no easy routes; the literature on the design of  
28 experiments for linear models has evolved over more than 80 years, with some prob-  
29 lems yet to be resolved. So, beware those who claim to have answers for all DCE design  
30 problems. Currently, we barely understand the generic design case for conditional  
31 logit models, and while designs for the alternative-specific case have been around since  
32 Louviere and Woodworth (37), few formal proofs of the properties of these designs  
33 exist even for conditional logit models. Furthermore, there are virtually no results  
34 available to guide those who want to estimate more general choice models than condi-  
35 tional logit, although mixed logit models at least should be identified with current  
36 generic and alternative-specific designs (each person is represented by a conditional  
37 logit model; only the parameters of that model differ across people).

38 It also is worth noting that this chapter has had little to say about types of tasks, task  
39 context, task complexity, methods of survey administration, survey length, incentive  
40 compatibility, sampling strategies, and a host of other issues relevant to whether  
41 any given stated preference DCE survey is reliable and valid. Similarly, we have said  
42 nothing about validating SP model predictions, pooling data from various sources,  
43 taking account of observable and unobservable heterogeneity and many other issues  
44 that are germane to particular applications. The choice modelling and SP literatures  
45 are now extensive on each of these topics, and interested readers should consult the

1 workshop reports from the triennial Invitational Choice Symposia published in  
 2 special issues of the journal *Marketing Letters* (e.g., *Marketing Letters* 1991, 1993,  
 3 1996, 1999, 2002, 2005, 2008), as well as standard reference sources like (1;44) for  
 4 guidance.

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