

Climate Policy Measures: What do people prefer ?¹

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Abstract

Several countries are responding to the climate change threat with various policy measures (e.g., taxes, permit trading, regulations, information campaigns, etc). While the effectiveness of different measures (instruments) has been studied extensively, very little research exists related to *public preferences* for alternative measures. This paper describes the results of a pilot study to determine whether a choice experiment might be a feasible approach for measuring preferences for carbon dioxide reduction policies, while ensuring careful consideration of the budget constraint facing households. We focus on estimating the public's marginal utilities and implicit prices for a select group of attributes that describe climate policy measures in general. The results from the pilot study indicate that when respondents trade-off the cost of alternative and unlabeled policy measures, they are willing to pay for those that encourage (1) the development of environmentally-friendly technology and (2) climate awareness among the Swedish population. Finding (1) could be interpreted to mean public support for market-based measures (e.g., taxes and permit trading) while finding (2) seems to support the use of information in the design of climate policy measures in order to encourage carbon dioxide-reducing behavior. Finally, our pilot study assumed that respondents' preferences for the cost-sharing burden (equity) of measures might be defined in terms of an individual's *ability to pay*. Given this assumption, our results indicate weak preferences for *non-regressive* cost distribution, but *progressive* cost distribution had no effect on choice. We offer several possible conclusions from this preliminary investigation into climate policy preferences.

Keywords: market-based mechanisms, information effects, equity, choice experiment, preferences.

JEL classification: Q48, Q54, Q55, Q5

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1 INTRODUCTION

Climate policy is a salient issue. Since the Kyoto Protocol was signed in December 1997 (entering into force in 2005) Annex I countries² have pledged to reduce the collective emissions of greenhouse gases (GHGs) by 5.2% compared to the 1990 levels by the year 2012. The primary GHG is carbon dioxide (CO₂). Emissions of carbon dioxide have grown exponentially on a global scale³ in recent years and have been linked to the burning of fossil fuels. Mounting evidence suggests that rising global temperatures are likely to lead to negative ecological and economic impacts. Various reports have noted that the economic impacts of inaction (“business as usual”) are potentially very high (Stern et al, 2006).

To prevent both environmental damage and potentially deteriorating economic conditions, Annex I countries are responding with various types of policy measures or instruments⁴ (e.g., taxes, emissions permit trading, subsidies, regulations, information campaigns, etc). In Sweden, the greening of the tax system began in 1991 when *corrective* environmental taxes (e.g., carbon tax, electricity tax, sulfur tax, etc) were implemented in order to reduce carbon dioxide emissions and simultaneously reduce the levels of existing *distortionary* income taxes (Brännlund and Kriström, 1997; Brännlund and Nordström, 1994). In the U.S. (which is not an Annex I country and has not ratified the Kyoto protocol), Congressional action to reduce GHGs appears likely in the near future, though the debate over which type of measure to implement is fierce. Congress has shown a preference for an emission permit trading system,⁵ although some business groups and others have argued that a carbon tax at the point of importation or extraction would be a more effective policy instrument (USEPA 2007; Williams and Zabel, 2008).

Given that countries have a variety of climate policy measures at their disposal, how might they select among competing instruments to achieve carbon reduction? One approach might be to assess the cost-effectiveness of alternative measures. Measures that reduce emissions at the lowest cost – while accounting for uncertainty about future probability of success – might provide the best instrument. Given this approach and criteria it is well known that a broad-based carbon tax or

² The Kyoto protocol divides countries into three categories: Annex I (40 industrial countries plus the EU), Annex II (developed countries that pay for developing countries), and Annex III (developing countries).

³ While the global trend has been exponential growth, emissions in Sweden have been an exception, showing a steady decline since 1970 (see Brännlund 2008).

⁴ We use the terms “measures” and “instruments” and “mechanisms” interchangeably throughout.

⁵ A cap and trade system is a form of emissions permit trading where the government sets the total limit of allowable emissions for some pollutant (e.g., carbon dioxide) and auctions (or gives away) “emission permits” that grant each owner the right to emit a certain amount of a pollutant. By allowing the emission permits to be traded on a market, it will encourage emission reductions at the lowest marginal cost.

broad-based cap and trade program will be the most preferred measures. These measures will result in an emission reduction at the lowest cost. However, in spite of this we see that many countries are hesitant to use such cost-efficient measures or at least to use them to their full potential extent. One reason for this is that the decision-makers and/or the public encounter some kind of disutility, pecuniary or non-pecuniary, with the tax or the cap and trade program. For example, there may be a disutility related to taxes in general ("stigma effect"), and/or the distribution of a measures' economic burden may not correspond to the decision-makers' preferences of a "fair" distribution of welfare. In other words, there may be a trade-off between efficiency and "fairness" or some other pecuniary or non-pecuniary effect. As a result it is no longer given that the choice of instrument should be based on cost-efficiency only.

Thus, an alternative approach, which is pursued in this paper, is to assess citizens' preferences for different types of measures (by "citizens" we mean the general public⁶). The motivation for this approach is that given a government's stated goal of reducing carbon dioxide emissions,⁷ measures that reflect public preferences are more likely to receive general support, thus facilitating carbon reduction policies. If instead policy-makers developed or try to implement "cost-effective" carbon reduction policies that fail to reflect citizen preferences, the measures may fail to accomplish the stated objective (or lead the public to vote their (democratic) government out of office). In simple terms, a policy-maker's agenda that mirrors the public's concerns will lead to trust, mutual understanding and more successful outcomes.

Previous research has queried the public about their disposition toward alternative climate policy measures. For example, Hammar och Jagers (2002) administered a survey that asked Swedish citizens whether they preferred taxes against an environmental "bad" (e.g., carbon dioxide emissions) or subsidies to encourage an environmental "good" (e.g., renewable energy). But, as the authors admit, the approach was flawed because the survey failed to make clear to respondents that both an *income* in the form of a "green subsidy" and an *expenditure* in the form of a "green tax" would impose costs on the government and, therefore, their taxpaying household. Not surprisingly, most respondents preferred to receive, rather than spend, money. A better approach for measuring citizen preferences would consider the cost that comes with alternative preferences

⁶ Previous studies have examined whether preferences for environmental policies differ across different parts of society (e.g., policy makers, experts, and the general public). Carlsson et al (2007) find that preferences for environmental policies do in fact differ between administrators and the public. In a somewhat related paper Colombo et al., (2007) find that the relative importance that experts and citizens ascribe to different attributes of a right of way conservation program in England are very similar, suggesting that public preferences for changes to this program may be obtained more cost-effectively by querying a group of experts.

⁷ Sweden has pledged to reduce carbon dioxide reduction by four percent by 2010 (Swedish Ministry of the Environment, 2008). Most Annex I countries have similar goals.

(i.e., how much income a respondent is willing to trade-off in return for a preferred measure or characteristic of a measure).

A US study surveyed 1,500 adults in 2007 to assess preferences for specific climate policy measures while ensuring that respondents considered the costs of the measures (Bannon et al., 2007). The study elicits preferences for three types of measures by name: regulation, emissions tax, and an emissions trading system (the survey referred to these three types of measures within both the electricity and vehicle use sectors). Respondents were told that the government wants to reduce greenhouse emissions by five percent by 2020 and the survey asks for their preferences for different ways of achieving this. The emissions reduction was held constant and the costs of alternative policy measures varied. The survey described “standards/regulations” as the government telling industry what they can and cannot do. The other two measures are described similarly in the sense that both would cause industry to come up with new ways of creating electricity/developing gasoline. Interestingly, despite the fact that the economic literature suggests that “taxes” and “emissions trading” approaches are more likely to lead to innovation and technological development, the respondents preferred the regulatory approach – but only at very low cost levels.⁸ The authors suggested that the American public may not trust market forces to accomplish carbon dioxide reduction, preferring instead concrete government action via a rule-making. An alternative interpretation might be that Americans dislike taxes.

The purpose of this research is to develop an improved method for measuring public preferences for climate policy measures. To account for the weakness in the Hammar and Jagars (2002) study, our approach will ask respondents to state their preferences while accounting for the household’s budget constraint *vis a vis* taxes and spending. Furthermore, to avoid the stigma that is often associated with the name of certain measures, we will describe climate policy measures in terms of characteristics or attributes rather than by name. Our method relies on a choice experiment (CE) approach, which asks survey respondents to choose a good (a climate policy measure) from a list of alternatives that are described by general characteristics. Respondents face repeated CE questions, where the levels of the attributes are varied in each question. By manipulating the variables of interest (attributes), we can observe the impact this has on the response variable: choice. By including a cost attribute, we can estimate a monetary value for the trade-offs respondents inherently make when choosing between goods. These implicit trade-offs provide

⁸ The survey instrument did not inform respondents of the economic benefits (e.g., technological development) of the tax and cap-and-trade approaches. This may have been intentional on the part of the authors.

policy-makers with useful information in the development of carbon reduction policies that garner public support.

The advantage of a choice experiment is that it mimics decisions in actual markets. For example, when consumers make a choice to purchase a good, they compare the attributes of the different alternatives such as taste, packaging, brand name, price, etc and choose one to purchase. Importantly, CE surveys rely on the fact that respondents are inherently “well-trained” in trading-off relevant attributes for a good they frequently purchase. For example, such surveys are commonly used to assess how fisherman trade-off size or quantity of catch against the cost of a particular site (Paulrud, 2004). Our social experiment makes a similar assumption, namely that citizens are able to weigh the attributes (including cost) of alternative climate policy measures and choose the one that they prefer. An equally important assumption is that we have identified all the most relevant attributes that citizens consider when selecting among alternative climate policy measures.⁹

The rest of the paper is organized as follows. Section 2 provides the economic and statistical theory for a choice experiment. Section 3 explains the development of our survey instrument and data collection and Section 4 provides our empirical choice model specification. Section 5 summarizes the results of the statistical analysis and Section 6 provides a discussion.

2 ECONOMIC THEORY

Economists often begin with two key assumptions describing human behavior and choice: (1) individuals act rationally and consider all information at their disposal and (2) individuals make decisions that maximize their utility, subject to their constraints (e.g., income, time, other purchases already made, etc). Here we employ these assumptions through a random utility model (RUM). Each individual faces a choice among M alternatives. For each alternative the individual, say individual n , obtains a certain level of utility which we can denote as U_{nm} , $m = 1, \dots, M$. A utility-maximizing individual will then choose the alternative that provides the highest utility. The difference between alternatives, with respect to utility, follows from differences in attributes between the alternatives. Here we denote the vector of attributes, facing individual n , associated to choice m as \mathbf{x}_{nm} . Thus the behavioral model becomes: chose alternative m if and only if $U_{nm} > U_{nk}$,

⁹ More specifically we will assume that the effects of attributes not included are uncorrelated with the effects of attributes that are included.

$m \neq k$. This is to say that if the attributes associated with alternative m provides a utility higher than any other alternative, an individual will choose m .

However, as analysts we cannot peer inside the mind of the decision maker, and therefore we must develop a model that can handle the *unobservable* factors affecting an individual's choice. As analysts we can observe the attributes for each alternative that each individual is facing, labeled \mathbf{x}_{nm} , as well as (some) of the attributes characterizing the individual, labeled s_n . Given this we can in principle partition the utility function, U_{nm} , into a deterministic or representative part, V_{nm} , and a (for the analyst) unobservable or random part, ε_{nm} , i.e.;

$$U_{nm} = V(x_{nm}, s_n) + \varepsilon_{nm}, \quad n = 1, \dots, N; \quad m = 1, \dots, M \quad (1)$$

The probability that individual n chooses alternative m over other alternatives can then be written as:

$$\begin{aligned} P_{nm} &= \Pr(m|C) = \Pr(U_{nm} > U_{nk}, \text{ all } k \in C) \\ &= \Pr(V_{nm} + \varepsilon_{nm} > V_{nk} + \varepsilon_{nk}, \text{ all } k \in C) \quad , \\ &= \Pr(\varepsilon_{nk} - \varepsilon_{nm} < V_{nm} - V_{nk}, \text{ all } k \in C) \end{aligned} \quad (2)$$

where C is the complete choice set.

From (2) it is clear that it is the utility difference between alternatives that matters for the choice and, due to the unobserved part, we can only assign probabilities of an individual's choice. By making some assumptions about the distribution of random terms, and making use of some specific parametric function of $V(\mathbf{x}_{nm}, \mathbf{s}_n)$, we can statistically estimate the probabilities.

Thus, to make the *random utility model* functional, we must make some assumption about the error term associated with each alternative-specific utility function.¹⁰ ε_m represents some random and unknown contribution to an individual's utility function of selecting alternative m . To estimate the parameters in the utility function, we must enforce two key assumptions upon it. The first (maintained) assumption refers to the unobserved component of utility *for each individual* and states that this component is located on some (unknown) distribution and randomly assigned to each person. Therefore, our task is to assume some distribution and then determine how to assign each individual the portion of their utility associated with the unobserved component. The second

¹⁰ There is a theoretical indirect utility function for each and every alternative the respondent faces. Note that in our case of an unlabeled experiment, there is no specific alternative (see Hensher et al. 2005)

(testable) assumption refers to the unobserved component of utility *for each alternative* and states that its component is also part of some unknown distribution with individuals assigned locations randomly (within the distribution that defines utility values). Together, these assumptions classify the random component as being independent and identically distributed, or IID. The IID assumption says that each ε_m has its own unique mean value across alternatives, but¹¹ that each alternative's ε is not correlated with the ε from another alternative (*independent*) and that the distribution of all ε 's (regardless of the alternative) are the same (*identically distributed*). Another way to think about this is that there are different sources of uncertainty when predicting the probability of a choice outcome, but we apply a strict assumption that all of the uncertainty is captured by a single unknown, ε (note we drop the alternative-specific index). By collapsing all uncertainty into a single variable we are assuming these uncertainties are not related in any systematic way.

Here we will follow the usual assumption that the errors are extreme value IID distributed (McFadden, 1974). Given this, we will, after some manipulations, obtain the logistic choice probability model (see Train, 2003).

$$P_{nm} = \frac{e^{V_{nm}}}{\sum_{k \in C} e^{V_{nk}}} \quad (3)$$

Assuming that V is linear in the attributes \mathbf{x} , and characteristics, \mathbf{s} , gives us then¹²:

$$P_{nm} = \frac{e^{\beta' \mathbf{z}_{nm}}}{\sum_{k \in C} e^{\beta' \mathbf{z}_{nk}}}, \quad (4)$$

where $\mathbf{z}_{nm} = [\mathbf{x}_{nm}, \mathbf{s}_n]$, and β is the corresponding parameter vector.

¹¹ The IID assumption is a data assumption. An equally important assumption in an MNL model (involving at least three alternatives) is a behavioral assumption known as Independence of Irrelevant Alternatives (IIA). This assumption maintains that if an individual prefers Alternative A to Alternative B (in the choice set [A,B]), then introducing a third alternative X (which expands the choice set to [A,B,X]) must not make B preferable to A (Laitila, 2007). Unlike the IID assumption, IIA can be tested. However, it is not relevant in our empirical application below which involves only two choice alternatives (Carlsson, 2009).

¹² Note that $\beta = \beta^*/\sigma$, where σ is the variance of the unobserved factors. Thus β reflects the effect of the observed variable, relative to the variance in the unobserved factors. Furthermore, it is clear that the ratio between any two parameters is independent of the scale factor, σ .

If we now let $y_{nm} = 1$ if individual n chooses m , and zero otherwise, we can write the probability for an individual choosing the alternative he/she was observed to chose as $\prod_m (P_{nm})^{y_m}$, which means that the likelihood function can be written as:

$$L(\boldsymbol{\beta}) = \prod_n \prod_m (P_{nm})^{y_m} \quad (5)$$

Maximizing (5) with respect to $\boldsymbol{\beta}$ gives us the estimate of the $\boldsymbol{\beta}$ vector.

Given the linear utility index it is easy to see that factors (variables) that do not change over the alternatives do not affect the choice probabilities. This means that under a linear specification individual characteristics do not affect the choice probabilities.

3 DATA AND SURVEY DEVELOPMENT

Data for our theoretical model was collected through a choice experiment survey. The survey instrument required development and testing as well as consideration of the experimental design. The attributes and attribute levels (along with the survey format and information presentation) were tested in four focus groups between November 2007 and January 2008, comprising a total of 31 individuals (13 male; 19 female).¹³ The goal of survey development was to describe climate policy measures to the public in an easy to understand manner. Without clear attributes and attribute levels that distinguish one policy measure from one another, it may be difficult for respondents to identify how each measure affects their utility, which would cause our theoretical model to fail in predicting choice. Below we describe the attributes used in this study and summarize the key aspects of the experimental (statistical) design.

The International Panel on Climate Change, IPCC, identifies seven types of climate policy mitigation measures¹⁴ (IPCC 2007). We studied these measures and developed generic attributes (and attribute levels) that could be used to describe these measures. This attribute-based approach led to an initial list of attributes describing climate policy measures as summarized in **Fel! Hittar inte referenskölla**. To avoid survey complexity we reduced this initial list based on various considerations. Some measures were difficult to measure (e.g., attribute levels for *timing* were subjective and difficult to estimate from the experience of existing policy measures), while others were too similar and therefore difficult for respondents to distinguish (e.g., *future flexibility* and

¹³ See Cole 2008 for more information about focus group participants and conclusions from focus group testing.

¹⁴ These include (1) Regulations & Standards, (2) Price Mechanism – Taxes/Charges, (3) Price Mechanism – Tradable Permits, (4) Financial Incentives (subsidies), (5) Voluntary Agreements, (6) Information instruments, and (7) Public R&D.

regulatory flexibility). The attribute “*effectiveness of carbon dioxide reduction*” is a seemingly relevant attribute that could be used to test for trade-offs related to a measure’s likelihood of success. However, we decided that the relevant policy context in Sweden is based on the government’s de facto decision to reduce carbon dioxide emissions by four percent by the year 2010 (Swedish Ministry of the Environment, 2008). Thus, varying this attribute did not seem realistic. Therefore, we asked respondents to choose the measure they think is best to accomplish the given four percent reduction (note this renders the opt-out option irrelevant).

Table 1 Initial list of climate policy measure attributes.

Potential Attribute	Description	Potential attribute level
<i>Effectiveness of carbon dioxide reduction</i>	How effective (and/or certain) is the measure at reducing carbon dioxide emissions.	Percent reductions in carbon dioxide (compared to 1990 levels by 2012)
<i>Future flexibility</i>	Flexibility of measure to adapt to changing <i>future</i> conditions (e.g., inflation, measure’s cost, technological progress, newly identified carbon dioxide sources)	Low, medium, high flexibility
<i>Regulatory flexibility</i>	Regulated entity’s flexibility to choose method to meet compliance	Low, medium, high flexibility
<i>Effect on Technological Progress</i>	Whether the measure encourage/hinders the spread of new ideas and solutions	Hinders, encourages, neutral effect
<i>Effect on environmental awareness</i>	A measure of the information effect that arises from the policy measure (e.g., some measures can affect peoples’ awareness of how their actions affect the climate, which can lead to people acting more climate friendly)	Low, medium, high effect on awareness
<i>Cost</i>	Cost to an individual to implement the measure.	Annual/monthly cost (in 3 levels)
<i>Distribution of Costs</i>	A measure of how the cost measure is distributed among the population	Progressive, proportional or regressive
<i>Effect on (net) government revenue</i>	Whether or not the instrument is likely to lead to a net gain in government revenue	Positive, negative, neutral effect
<i>Cost-effectiveness</i>	A measure of the cost per unit reduction of carbon dioxide	X SEK per ton of carbon dioxide reduced (in 3 levels)
<i>Enforcement mechanism</i>	Identifies how/if government enforces the measure	Compulsory, voluntary
<i>Timing</i>	How long it takes for the measure to reach its objective of carbon dioxide reduction	Number of years

The final attributes used in this study are shown in **Fel! Hittar inte referenskölla.** The *cost* attribute is an important part of any choice experiment because its parameter allows us to convert utility into its monetary equivalent. We told respondents that each measure would result in an increase in the cost of the goods and services they regularly buy each month. *Cost distribution* is to investigate preferences for the distribution of the cost associated with climate policy measures. In our experiment we have chosen to define burden-sharing in terms of a citizen’s *ability to pay*

for proposed environmental improvements.¹⁵ We told respondents that measures could be distributed in one of three ways (regressive, proportional, progressive) and provided simple warm-up questions to ensure they understood the difference. The *effect on environmental awareness* attribute has been selected to investigate the potential impact of information effects in carbon dioxide reduction policies. We explained this attribute to respondents by saying that some climate policy measures impact peoples’ awareness of how their actions affect the climate and therefore can lead people to act more ”climate-friendly,” resulting in reduced emissions. Finally, the *effect on technological progress* can be used as a means to distinguish theoretically between a regulatory standard and a market measure such as a tax or tradeable permits. A market measure has the effect of increasing the marginal cost of producing carbon dioxide, which creates an incentive for polluters to innovate and find ways of avoiding this cost, while still maintaining production. This innovation is thought to lead to technological improvements. We explained this attribute to respondents by saying that some measures can affect a company’s willingness to invest in technologies, which can make it easier and more effective to reduce emissions in the future.

Table 2. Summary of attributes used in the survey.

Attribute	Description provided to respondent	Attribute levels used
Effect on the development of environmentally-friendly technologies (TECH)	Some measures can affect a company’s willingness to invest in new technologies, which can make it easier and more effective to reduce emissions in the future	Positive No effect Negative
Has a positive effect on the Swedish populations’ awareness of climate change (AWARE)	Some measures can affect peoples’ awareness of how their actions affect the climate, which can lead to people acting more climate friendly, thus reducing emissions	Yes No
Social distribution of costs (DIST)	A reduction of carbon dioxide emissions imposes a cost on society, but this cost can be distributed across society in different ways.	All citizens pay the same <u>amount</u> , independent of income; All citizens pay the <u>same percentage</u> of income; Higher income citizens pay a <u>higher percentage</u> of income
Monthly cost to you until 2010 (COST)	The measure would impose a monthly cost in terms of an increase in the cost of goods and services your household buys each month	100 SEK ^a 300 SEK 1000 SEK

Final attribute levels were determined through discussion and focus group testing. To determine the level of the bid amounts we considered a study that estimated the costs of the Swedish

¹⁵ A strict interpretation of one’s true “ability to pay” might be 100% of one’s income. Under such a rigid interpretation one might suggest that we instead refer to our burden-sharing criteria in terms of one’s “willingness to pay” instead. However, we feel that this interpretation ignores practical costs of living considerations. Therefore, we assume that one’s income is a proxy for one’s “ability to pay,” even though some low income earners might be “willing” to pay more of their income than high income earners for an environmental good.

government's four percent carbon dioxide reduction policy (Carlén, 2004) at between 5 and 8 billion SEK. Given a population of nine million in Sweden, this is approximately 600 to 1,000 SEK per person (dividing by households would give a higher per unit figure). Although this gives us a starting point for bid amounts, we acknowledge that *the cost* of achieving this reduction may or may not be a reasonable proxy for an individual's *welfare change* associated with this reduction.

3.1.1 *Generating the experimental design*

Experimental design refers to the process of ensuring that respondents face sufficient variation of (uncorrelated) attribute levels across surveys so that we can develop testable hypotheses (e.g., about individuals' preferences for policy measure attributes). In addition, the experimental design stage has a direct impact on the statistical efficiency of the model parameters we wish to estimate (For this reason, it is sometimes referred to as the "statistical design"). Developing an experimental design necessarily involves trade-offs between attribute level correlation, parameter efficiency (minimum variance of model estimators), respondent burden/complexity, and cost (e.g. number of choice experiment questions and/or number of survey versions).

Our experimental design relies upon a simple orthogonal¹⁶ main effects plan (OMEP) that is generated by SPSS's *generate orthogonal design* feature, based upon the number of parameters we wish to estimate (four). We then (non-randomly) group the generated alternatives (profiles) into our two-alternative choice sets (see strategy #1 in Street et al 2005). This simple design approach, combined with the consideration of the required degrees of freedom described below, ensures the uncorrelated estimation of the *main effect* parameters only, which means we have chosen *a priori* to ignore possible *interaction effects* that may exist between our attributes.¹⁷ The details of our experimental design are provided in Appendix 1.

The drawback to an OMEP design is the possibility of unwanted overlap of the attributes in our pairings (i.e., attribute levels will occasionally repeat themselves in our design) and/or the lack of balance between attribute levels (i.e., levels may not appear with equal frequency in our design). The existence of these drawbacks implies that an OMEP design may not maximize the amount of

¹⁶ An orthogonal design is a mathematical constraint that ensures all attribute levels are statistically independent of one another. In labeled experiments it is important that attributes are uncorrelated across alternatives. In our unlabeled experiment (or labeled experiments that estimate generic parameters), the criterion is less strict: within-alternative orthogonality is the objective.

¹⁷ We acknowledge that our assumption that the parameters associated with all interaction effects (including two- and three- way interaction effects) are zero is a strong assumption and cannot be tested. The implication is that if these parameters are *not* statistically insignificant, then our model will produce suboptimal results.

information we can obtain from our design.¹⁸ However, rather than assigning alternatives to choice sets at random, we attempt to allocate them in a way that minimizes (but does not completely remove) overlap and balance problems (see Appendix 1).

While an OMEP is a necessary starting point in an experimental design (it ensures uncorrelated attributes or attributes levels), it only applies in cases of linear models and it does not address the issue of statistical efficiency. That is, if we have information *a priori* about how respondents think about the attributes in our experiment (i.e., we suspect that respondents prefer attribute level 1 to attribute level 2), then we can theoretically improve the statistical efficiency of our design. However, designs capable of including this type of information are rarely used (Carlsson Lecture, 2008).

Therefore, although our OMEP design is capable of uncorrelated estimation of main effects, it (1) assumes all interaction effects are zero and (2) does not necessarily ensure the minimum variance of our estimators. That is, a more sophisticated design might be capable of estimating selected interaction effects (if we believe they exist) and of generating model parameters with a smaller standard error (see for example strategy 5 in Street et al 2005). However, a more sophisticated design would also entail greater costs in the form of additional choice questions (or - in the case of blocking - additional surveys). We chose instead for this pilot study to maintain a simple experimental design based on an OMEP.

The survey instrument includes warm-up questions and other information to be used as additional explanatory variables. We include what Hensher et al, (2005) refer to as “proxies for an individual’s tastes” which describe a respondent’s inherent characteristics such as income, age, gender, occupation, etc (individual characteristics). We also include additional variables of interest such as a respondent’s environmental attitudes and behaviors, whether they are members in an environmental organization or drive a car to work everyday. This information can be used to determine whether some attributes are more preferable by certain segments of the population.

¹⁸ Note the trade-off here: If we ensure no *overlap* and perfect *balance*, we may be increasing the *complexity* of the design due to the fact that a comparison of two alternatives necessarily implies that all attribute levels are different, which can increase the cognitive burden of the respondent (see Swait and Adamowicz (1997), Cameron and Englin (1997) for more on complexity).

4 EMPIRICAL SPECIFICATION

The utility of each choice alternative is assumed to be a linear function of the attributes we use to describe the policy measures (see **Fel! Hittar inte referenskälla.**), as well as the socioeconomic and/or behavioral characteristics of the individuals. Our utility index V can then be written as:

Our empirical model is a utility function for individual n who chooses among M alternatives:

$$U_{nm} = \beta \mathbf{X}_{nm} + \alpha \mathbf{S}_n + \varepsilon_{nm} \quad (6)$$

As discussed in the theory section above, \mathbf{X}_{nm} is the vector of policy measure attributes with β as the corresponding parameter vector (including a constant). While the parameters β indicate the importance of each of the k attributes within the individual's choice (across alternatives), the parameters α indicate the importance of socioeconomic characteristics in choices made by the population (across individuals).

The basic formulation in (6) means that we have four parameters to be estimated that are related to the attributes. Depending on specification, the number of parameters related to these attributes vary. Two of the attributes – representing a measure's effect on environmentally-friendly technology and a measure's distribution of costs, respectively - have been dummy coded to represent a possible nonlinear relationship in the attribute levels, whereas the other two variables are assumed to be linear in the attribute level (the AWARE attribute has only two attribute levels, thus precluding nonlinear testing and the COST attribute was coded in SEK terms in order to estimate the monetary values of trade-offs). The constant can then be interpreted as the utility index level for the case when all dummies equal zero. The implication of the dummy coding is that the final specification has six parameters related to the four attributes (see Table 3).

The β parameters represent the weight associated with each attribute and can be interpreted in several ways. First, the parameter values are called the *main effects*¹⁹, as they measure the effect that a particular attribute level has on the dependent variable, choice (all else equal). Alternatively, they are referred to as *marginal utilities* or *part-worths* because they represent the partial value of utility represented by that attribute. A measurement of the utility change on an ordinal scale is not

¹⁹ The other commonly measured effect in the choice literature is an interactive effect, which is the effect on choice obtained by combining two or more attributes which would not have been observed had each of the attributes been estimated separately. We made the decision *a priori* to focus on main effects for this simple pilot study to avoid the additional data collection demands required for interactive effects.

directly relevant to policy makers, who are usually interested in the *value* of policy outcomes. Thus, a third way to interpret parameters is the marginal rate of substitution (ratio of parameters) between an attribute's level and money, which gives the monetary equivalent to the utility change a person experiences from the change in the attribute level (e.g., attribute's implicit price).

Table 3. Explanation of parameters used in empirical model

Parameters (β)	Description	Marginal change in attribute level (base level \rightarrow new level)
Constant (β_0)	a constant that reflects the utility level when dummy variables = 0	n/a
β_{T_NEG}	Policy measure's effect on the development of environmentally - friendly technologies (TECH)	"No effect" \rightarrow "Negative effect"
β_{T_POS}		"No effect" \rightarrow "Positive effect"
β_{AWARE}	Policy measure's affect on improving environmental awareness (AWARE)	"No effect" \rightarrow "Effect" ^a
β_{D_PROP}	Policy measure's cost distribution across society based on ability to pay (DIST)	"Regressive" \rightarrow "Proportional"
β_{D_PROG}		"Regressive" \rightarrow "Progressive"
β_{COST}	Policy measure's cost (COST)	100 SEK \rightarrow 300 SEK \rightarrow 1,000 SEK ^a
α_q	various socioeconomic characteristics of the individual (see section 5)	(varies)

^a represents a simple linear relationship between attribute levels

A number of important considerations relate to our type of *unlabeled* experiment (Hensher et al., 2005, Section 5.3). In general, *labeled* experiments are ideal when the brand name or label of an alternative is expected to be an important consideration in an individual's choice. Including such a label is assumed to increase predictive validity and may also be more realistic to respondents (Blamey et al. 2000). The drawback to a labeled experiment is that a respondent may focus on the label and fail to consider the attributes closely. An *unlabeled* experiment is preferable in studies that focus on the marginal rate of substitution between attributes (as in this study) because it "*encourages respondents [to give] more discerning and discriminating responses*" rather than rely on a label when choosing (quote from Blamey et al. 2000; see also Alpizar et al 2001). Furthermore, when labels attached to alternatives represent a "significant emotional and symbolic content" (e.g., the stigma associated with a carbon tax), the use of unlabeled experiments will make it easier for respondents to focus on the merits of the individual attributes themselves. Blamey et al. 2000 compare model results from a split sample choice experiment where each sample was either a labeled or unlabeled alternatives. They conclude that "the inclusion of policy labels appears to have reduced the attention respondents give to the attributes."

5 RESULTS AND DATA ANALYSIS

Following revisions from focus group testing, we distributed 228 surveys into mail boxes in urban and rural areas in Umeå in February 2008. This included 114 surveys of Block A and B respectively. For more information see Cole, 2008.

After 15 days, 79 surveys were returned, of which 76 (33%) were complete and available for our data analysis (42 from Block A and 34 from Block B). Given four choice questions with two alternatives each, this equates into 608 rows of choice data ($4 \times 2 \times 76 = 608$).

Table 4. shows some of the more interesting descriptive statistics among the 76 respondents, along with information on the national average in Sweden. As shown, the sample is not entirely representative of Sweden. Our sample was older, more educated, had larger size households, and was “greener” than average (based on membership in environmental organizations). Our average income was approximately equal to the average from 1997.

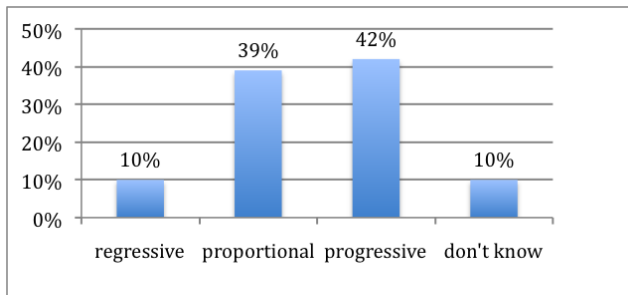
Table 4. Comparison of pilot sample characteristics to national average.

Variable	Our Sample ¹	Typical Swedish House Owners ²
Gender (% male)	48	51
Age (% share older than 65)	65	24 (1997)
Average household income (SEK/month)	30,000	32,000 (1997)
Households with 2 or more incomes (%)	74	(unknown)
Membership in an environmental organization (%)	11	4 (2002)
Household share with 2 or more children (%)	33	16 (2006)
Education (% with university degree)	59	24 (1997)
Commute to work each day by car (%)	70	(unknown)
Believe that current government expenditures on environmental protection is too low	64	(unknown)
¹ From our sample size of 76 respondents.		
² Source: Statistics Sweden (see also Ek, 2002)		

Several questions in the survey were related to the choice experiment questions themselves (Figure 1). First, a warm-up question described three possible attribute levels for the DIST attribute in the form of a question (similar questions existed for the other three attributes). The question gave an illustrative example of a regressive, proportional, and progressive cost distribution and asked respondents which one represented the Swedish income tax system. As shown, there was varying interpretation by respondents, though most economists would agree that the system is progressive. The second question was a “follow-up” to the choice experiment questions and asked respondents

to rate the difficulty of the six choice experiment questions on a four level ordinal scale. As shown, more than two-thirds found the questions to be “pretty difficult” or “very difficult.”

Which cost distribution alternative is most consistent with the Swedish income tax system?*



*The question referred to “alternatives 1 through 3” which illustrated regressive, proportional, and progressive, cost distributions, respectively, without referring to them as such.

How easy/difficult was it for you to choose between the two [choice experiment] alternatives above?

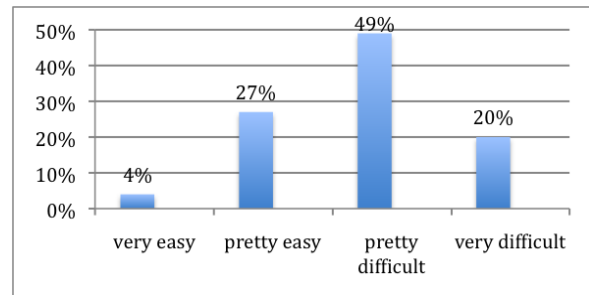


Figure 1. Responses to the warm-up and follow-up questions.

As a follow-up to the (unlabeled) choice scenarios, we asked respondents which type of (labeled) climate policy measure respondents prefer, given a government decision to reduce carbon dioxide emissions from motor vehicles. The multiple-choice answers included labeled climate policy measures as follows (percent of respondents selecting that answer):

- (13%) *Higher tax* on gasoline/diesel (making these fuels more expensive)
- (46%) *Information campaign* (informing people how their transport choices affect the climate)
- (31%) *Regulation* that forces people to use certain types of fuels (e.g., ethanol) or vehicles
- (10%) *Higher income tax* to subsidize public transportation (train and bus)

The purpose of this question was to determine whether respondents who favor a certain climate policy measure by name (labeled question above) also exhibited consistent preferences for the attribute levels that best described that (unnamed) measure in the choice experiment. A strict comparison of preferences is difficult because the CE questions related to carbon dioxide reduction in general, while this follow-up question related specifically to the transport sector. However the lack of an “opt-out/I don’t agree” response was consistent across both questions.

5.1 Empirical Results

Our non-linear-in-the-attributes model is presented in Table 6. The table provides a comparison of the magnitude, signs, standard errors, and estimated t-values across each of the parameters. It also

includes the *conditional*²⁰ implicit attribute prices, along with the standard errors and corresponding 95% confidence intervals. In addition, we provide the pseudo r^2 and the LL Value²¹ for the logistic regression.

In addition to this “attributes only” model, we estimate several models that included parameters for socioeconomic and attitudinal variables. We interacted the following variables with the AWARE attribute to test this model: membership in an environmental organization, beliefs about current governmental environmental expenditures, gender, age, education, income, number of children in household, number of income-earning adults in household, and location (urban/rural), but none of these parameters were significant in our regression. Therefore, we rely on an “attributes only” logistic model.

All parameters in the model are significantly different from zero at the 90 percent level and have the expected signs, with the exception of β_{D_PROG} which is discussed further below. For example, compared to policy measures that have “no effect” on the spread of environmentally-friendly technologies, measures that have a “negative effect” reduce respondents’ utility (β_{T_NEG}) but measures that have a “positive effect” increase respondents’ utility (β_{T_POS}). Interestingly, the utility increase associated with the latter is twice as big as the utility decrease for the former, indicating that respondents are particularly keen to support climate policy measures that encourage technological development. This is also shown by the relatively high conditional implicit price for the β_{T_POS} attribute. The positive sign on the β_{AWARE} parameter indicates that respondents have a clear preference for measures that improve peoples’ awareness of how their actions affect the climate, compared to measures that have no effect. This parameter was highly significant and robust across several different model specifications and has the smallest standard error. In general, respondents seemed less concerned about the distributional impacts of measures. For example, compared to a measure whose cost is regressively distributed, respondents are indifferent about a change to a progressive distribution (β_{D_PROG}). There is a weak statistical relationship indicating an increase in utility when the marginal change is from a regressive to a proportional distributional

²⁰ The implicit values are conditional on the fact that respondents must choose measure A or B without opting out.

²¹ The pseudo r^2 in a logistic regression provides a measure of fit relative to some base model, which is usually specified as a constants-only model. It has a similar interpretation to the r^2 from a linear regression: higher values indicate better model fits. According to Domencish and McFadden 1975 (Henscher et al 2005, 10.3.6) a pseudo r^2 of 0.3 is a pretty good logistic fit and corresponds to an r^2 of approximately 0.6 in a linear regression. The formula for computing a pseudo r^2 between an estimated model and an (assumed) base model is a simple function of the LL values (pseudo $r^2 = [1 - (LL_{estimated\ model}/LL_{base\ model})]$).

(β_{D_PROP}).²² This result does not entirely match our *a priori* assumption that respondents would prefer progressive cost distributions based on an ability-to-pay criterion (i.e., we assumed that $utility_{progressive} > utility_{proportional} > utility_{regressive}$).²³

The relative implicit prices calculated in LIMDEP are significant at the 10 percent level for three attributes: respondents value the marginal change from regressive to proportional cost distribution (138 SEK) lower than the positive effect on environmentally-friendly technologies (324 SEK) or the improvement in environmental awareness (300 SEK). Finally the (linear) β_{COST} parameter is negative, as expected, indicating a decrease in utility as the attribute levels are increased.²⁴

Table 6. Attributes-only non-linear model

Parameter	Coeff. (SE of Coefficient)	t-value	Conditional Implicit Price in SEK (WTP) ^{1,2} (SE of WTP)	Confidence Intervals (95%) ²
Constant (β_0)	.66 (.2143)	3.12	257 (85.18)	90 - 424
β_{T_NEG}	-.38 (.2437)	-1.57	-147 (93.55)	-331 - 36
β_{T_POS}	.84 (.2973)	2.83	324 * (90.00)	148 - 501
β_{AWARE}	.78 (.1226)	6.37	300 * (39.33)	223 - 377
β_{D_PROP}	.36 (.2266)	1.59	138 * (82.21)	-22 - 300
β_{D_PROG}	.10 (.2645)	.39	40 SEK (97.46)	-151 - 231
β_{COST}	-.0026 (.0004)	-5.92	-	
Obs.	608			
Pseudo R	.104			
LL Value	-377.57 (Restricted LL ³ = - 421.44)			
¹ At the time of the survey 1 SEK equaled approximately 0.1 Euros ² Standard error of the WTP was calculated using the WALD command for a random variable in LIMDEP; confidence intervals were calculated using $WTP \pm (1.96) * (SE)$ ³ Restricted LL is a “constants-only” model * significant at the 10 % level Parameters for the various socioeconomic and behavioral variables were not significant and therefore excluded from our model				

²² Henscher et al. 2005 discuss situations where one of two nonlinear dummy parameters is significant (p. 350). In general, it is reasonable to include both in the specification even if one of the parameters is not significant.

²³ We also tried a nonlinear specification where the “proportional” distribution was the base level (instead of regressive), but found that the parameter indicating the marginal change from “proportional → progressive” was not significant.

²⁴ We also tried a nonlinear specification with the cost variable, where “100 SEK” was the base level and COST1 indicated a move to 300 SEK and COST2 indicated a move to 1,000 SEK. We found that the parameter on COST1 was not significant, but the parameter on COST2 was significant. Because our model is designed to estimate the implicit price of the other attributes, we code the COST parameter in Swedish kronor.

Next we test restrictions on our simple model above. The results of a Wald test (performed using LIMDEP 8.0) of linear restrictions indicate that the nonlinear relationships estimated in Table 6 *cannot* be rejected. We reject the null hypothesis for the Wald test that the dummy parameters are equal to zero (e.g., $\beta_{T_NEG} = \beta_{T_POS} = 0$) based on a chi squared test statistic of 17.28. Thus, the slopes are indeed different (this is intuitive given our finding above that respondents received twice the utility gain from the β_{T_POS} parameter compared to the β_{T_NEG} parameter). A Wald test between the β_{D_PROP} and β_{D_PROG} parameters indicates that we cannot reject the null, which implies that a linear relationship might be appropriate here. However, we conduct a simultaneous Wald test where we restrict all four parameters at the same time (e.g., $\beta_{T_NEG} = \beta_{T_POS} = 0$ and $\beta_{D_PROP} = \beta_{D_PROG} = 0$), and reject the null hypothesis, indicating that the nonlinear relationship shown in Table 6 between these variables appears to be best.

6 DISCUSSION

This study provides policy-makers with guidance in how to develop carbon reduction policies that garner public support. The pilot study indicated that a choice experiment is reasonable and workable approach for measuring citizen preferences for different types of properties associated with climate change measures. The survey data enabled us to estimate a statistical choice model that resulted in reasonable interpretations of our four attributes. The focus on attributes of alternative policy measures – rather than the welfare change from implementation of a carbon dioxide reduction policy – is both interesting and policy relevant. The results of this pilot study, therefore, suggest continued development of a nationwide survey along the same lines.

This survey elicited preferences for the properties of climate policy measures by asking respondents to trade-off the cost of certain measures against other beneficial side-effects of the policy (e.g., its effect on technological development, climate awareness, and distributional impacts on society). Because the choice experiment approach enforces a budget constraint upon respondents it avoids meaningless preference comparisons between “taxes on environmental bads” and “subsidies for environmental goods.” Importantly, the preferences revealed from our study assume that respondents chose between two equally-effective measures²⁵ (both reduce carbon dioxide emissions by four percent), where some measures carry with them certain characteristics. Our survey results are intended to help inform *the design* of future policy measures so that they

²⁵ Note that we could have varied this variable to see how respondents value cost-effectiveness, but our focus was on preferences for reaching the current four percent target.

reflect public preferences, rather than argue for specific instruments (e.g., tax, permit, regulation, etc). By incorporating public preferences, policymakers are more likely to garner support for their carbon reduction policies.

The implications of the statistical analysis of the Umeå pilot study – which may or may not be replicated when tested within a more representative national Swedish sample – can be summarized in four points. A conclusion common to all four points is the "all else equal assumption" regarding the other three attribute levels i.e., changing these attribute levels may alter the preferences for attribute in question.

First, awareness-raising is an important aspect of climate policy measures for which respondents are willing to pay. This result is consistent with respondents' answers to the follow-up *labeled* question, where nearly half of the respondents (46%) choose an "information campaign" as their favored policy for reducing motor vehicle emissions. The implication is that measures that educate the public about how human activities impact the climate is a preferred mechanism for inducing climate-friendly behavior. This preference implies nothing about whether people will respond more favorably to information campaigns compared to taxes per se; rather that citizens have positive preferences for measures that exhibit "awareness-raising" characteristics.

Second, respondents favor climate measures that have a positive side-effect on technological development and are willing to pay for this attribute. Their preferences are non-linear. That is, compared to measures that have "no effect" on technological development, measures that have a "positive effect" provide twice as much utility to respondents as measures that have a "negative effect". One interpretation of this result is that the public has a preference for measures that exhibit characteristics of market mechanisms (e.g., taxes and permits), which has the effect of increasing the marginal cost of producing carbon dioxide, leading to innovation and technological development. Thus, this result seems to indicate that citizens value the cost-effectiveness criteria in selecting a policy measure, where cost effectiveness is defined by our TECH attribute in Table 2.

Third, our statistical model indicates that respondents have a weak preference against measures with a regressive cost distribution. Interestingly, the impact of having a progressive cost distribution (higher income people carry a higher burden) had no effect on choice, which is counter-intuitive from our "ability to pay" *a priori* assumption about equity concerns. Possible interpretations include (1) respondents did not fully understand the difference between our

attribute levels and therefore were unable to state a clear preference with respect to the three levels; (2) people expressed a preference (although weakly) against regressive measures; (3) our non-representative pilot study sample perhaps skewed these results; or (4) we failed to define environmental equity in terms of how the majority of respondents think about fairness related to carbon dioxide emissions. It might be that respondents think of environmental equity in terms of the “polluter pays principle” (PPP) rather than the “ability to pay” (ATP) criterion. For example, Atkinson and Dietz, 2005 and Atkinson et al 2000 found that equity issues are important considerations in citizens’ acceptance of environmental policies (e.g., local air pollution abatement). It could be that our survey failed to test whether respondents prefer policies that distribute a higher burden to individuals or sectors in Sweden that are (relatively) more responsible for pollution. In other words, we may have a missing, or poorly-defined, attribute problem, which results in less reliable model.

Fourth, it is possible that we failed to capture another important attribute of climate policy measures: namely where the carbon dioxide reduction takes place. Because carbon dioxide reduction policies create global environmental benefits, one might assume that the country in which the reduction is implemented is meaningless. But do respondents think this way? In a future version of this survey we plan to include an attribute that identifies the level at which the policy is implemented (e.g., Sweden, Europe, outside of Europe) in order to determine whether this has an impact on choice (e.g., do Swedes feel a moral obligation to reduce carbon dioxide at home or are they willing to reduce abroad?). To the extent this is an attribute that citizens consider important, our decision to exclude it in the choice experiment implies that our utility specification is wrong. Thus, this variance is captured by the error term.

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Appendix 1: Experimental Design

Our experimental design relies upon a simple orthogonal²⁶ main effects plan (OMEF) that is generated by SPSS's *generate orthogonal design* feature, based upon the number of parameters we wish to estimate (four). We then (non-randomly) group the generated alternatives (profiles or "treatment combinations") into our two-alternative choice sets (see strategy #1 in Street et al 2005). This simple design approach ensures the uncorrelated estimation of the *main effect* parameters only.

In order to generate an OMEF, we first identify basic assumptions about our model. We assume an unlabeled experiment consisting of two alternatives in each choice set. Each choice set has four attributes, of which one is described using two attribute levels, and the other three rely on three levels. A full factorial design – an enumeration of all possible combinations of attribute levels – would result in 54 treatment combinations ($2^1 \times 3^3 = 54$), or 27 choice experiment questions in our case. This is an unrealistic respondent burden. To reduce the size of the experimental design we present respondents with only a "fraction" of the full treatment combinations by arranging our survey attributes into an orthogonal design and then (non-randomly) selecting attribute levels to fill the treatment combination rows.

Before generating an orthogonal design we must first determine the minimum number of degrees of freedom (i.e., treatment combinations) required for our model. We assume we will estimate four parameters (one for each attribute) based on a linear utility function.²⁷ Thus, the minimum treatment combinations required for our experiment is given by (Hensher et al., (2005) Table 5.10):

Minimum number of treatment combinations for an unlabeled choice experiment – <i>fractional factorial design with main effects and nonlinear effects</i> <i>in attribute levels</i>	= (L-1) x A + 1 (9)
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where A is the number of attributes and L is the number of attribute levels. Thus, in our experiment, we require *at least* 10 treatment combinations (degrees of freedom): three attributes have three levels (TECH, DIST, and COST) and therefore require 8 degrees of freedom [(3-1) x 3+1 = 8] and the other attribute requires 2 degrees of freedom [(2-1) x 1+1 = 2]. Then we add

²⁶ An orthogonal design is a mathematical constraint that ensures all attribute levels are statistically independent of one another. In labeled experiments it is important that attributes are uncorrelated across alternatives. In our unlabeled experiment (or labeled experiments that estimate generic parameters), the criterion is less strict: within-alternative orthogonality is the objective.

²⁷ This is required for our MNL model. We will utilize effects coding to allow for nonlinear effects associated with changes in the attribute levels.

them together ($8+2=10$). Because this is a pilot study, we want the survey instrument to look reasonably similar to the eventual larger survey study. Because this larger study will likely test for interactive effects (requiring additional degrees of freedom), we felt it would be appropriate to include two extra treatment combinations for a minimum of 12.

Authors Note: In the original thesis, I assumed this to be 12 treatment combinations instead of the minimum of 10. The effect of requiring 12 combinations instead of the true minimum of 10 was to increase the respondent burden. However, it is likely that by increasing the “minimum treatment combinations” we were able to capture additional information from our design, which may have improved the statistical efficiency of our model. In what follows below I explain the statistical design assuming that we require at least 12 treatment combinations.

Next we rely on SPSS’s *Generate Orthogonal Design* function, which uses the minimum number of treatment combinations required in our design as the main input, along with a coding structure for the attributes and a blocking variable.²⁸ The SPSS program output tells us that the minimum number of treatment combinations *that ensures orthogonality* in our experiment is 16. This implies eight choice experiment questions per survey. However, we divide the survey into two blocks (Block A and Block B), which means each survey will have four choice experiment questions and that two respondents are required to complete the block.²⁹

As a final step, we distribute these treatment combinations into the actual CE questions in the survey in a non-random manner. Assigning these treatment combinations randomly into our choice experiment questions might lead to overlap (i.e., attributes levels may repeat themselves in a choice set) or imbalance (level may appear with unequal frequency in our design). Thus, by assigning these treatment combinations non-randomly we can address these issues of overlap and imbalance. In addition, we can reduce choice sets that are highly complex (where “complex” means all attribute levels are changing within a choice set, thus increasing the cognitive burden). For more detailed information on how treatment combinations were created see the appendices in Cole 2008.

²⁸ The blocking variable divides the surveys into two blocks: A and B such that two surveys must be answered to complete a block. The purpose of blocking is to reduce the number of choice sets each respondent faces.

²⁹ Note that our mailed survey actually had six choice experiment questions per survey. The fifth CE question was taken from the opposite Block to test for differences between blocks and the sixth CE question presented an “unrealistically dominant” alternative to see whether respondents were rationally considering the attribute levels when choosing.