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Tanga Mohr
Appalachian State University

John C. Whitehead
Appalachian State University

Department of Economics
Appalachian State University
Boone, NC 28608
Phone: (828) 262-2148
Fax: (828) 262-6105
www.business.appstate.edu/economics

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Tanga M. Mohr and John C. Whitehead*

Department of Economics

Appalachian State University

Boone, NC 28608

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*Corresponding author: whiteheadjc@appstate.edu

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Abstract

We consider differences in hypothetical and real payoff laboratory experiments using attribute non-attendance methods. Attribute non-attendance is an empirical approach that measures and accounts for when survey respondents ignore attributes in stated preference surveys. We use attribute non-attendance methods with data from an emissions permit experiment with real and hypothetical payments. Our conjecture is that attribute non-attendance may be more pronounced in hypothetical sessions and, once accounted for, hypothetical decisions and real decisions influenced by monetary payoffs will be more similar. In both treatments we find that the effect of the cost of an emissions permit on behavior differs if the cost is implicit or explicit. In inferred attribute non-attendance models with the real treatment data we find two classes of respondents with different behavior but no evidence of attribute non-attendance. With the hypothetical treatment data we find two classes of respondents with different behavior and evidence of attribute non-attendance on two of the four choice attributes.

Key words: attribute non-attendance, emissions permits, laboratory experiment, stated preferences

Introduction

Stated preference (SP) methods are a common approach for measuring economic values for market and non-market goods in the environmental economics, health economics, marketing, and transportation literatures. One type of SP method is a discreate choice experiment (DCE) where a survey respondent is presented with a choice between two or more alternatives that are described in terms of multiple attributes, including cost. The respondent is asked to choose the alternative that is most-preferred. Under the rational choice model, the individual will choose the alternative that yields the highest utility. In the DCE literature, recognition of the limitations of the rational choice model and efforts to better address the potential for deviations from it have grown over the last two decades.

One limitation that has been observed in DCEs is known as attribute non-attendance (Lew and Whitehead 2020a). Attribute non-attendance (ANA) occurs when survey respondents ignore one or more of the choice attributes. There is no tradeoff between money and changes in other attributes for respondents who ignore the cost attribute. Welfare estimation is precluded for these respondents. When these respondents are included in the valuation model, cost coefficients will be biased towards zero and welfare estimates biased upwards. Estimation of welfare for other attributes, such as environmental quality, are precluded for respondents that ignore them (e.g., Giguere, Moore and Whitehead 2020). If these quality ignoring respondents are included in the full sample the attribute coefficients and welfare estimates will be biased downwards. Thus, if ANA is present but not accounted for, welfare estimates in general will be biased.

The ANA literature includes two primary approaches for identifying and accounting for ANA behavior in choice experiments (Lew and Whitehead 2020b). In the inferred ANA approach, researchers employ econometric models to identify behavior that is consistent with respondents who ignore or give less than full attention to attributes. In the stated ANA approach, survey respondents provide self-reported information about choice experiment attributes that have been ignored or given less than full attention. Lew and Whitehead (2020a) review the literature and identify 86 empirical articles with a focus on ANA in the economics literature over the past 10 years. Sixty-eight percent of the ANA studies employ inferred ANA methods and 49% employ stated ANA methods.

The external validity of stated preference methods is essentially a test for whether hypothetical questions can accurately predict real behavior in similar situations. In the contingent valuation method literature, hypothetical bias results when hypothetical willingness to pay is greater than willingness to pay in a real situation. Penn and Wu (2018) conduct a meta-analysis of these studies and find the mean ratio of hypothetical WTP to real WTP is greater than two. They determine experimental design features that can be used to mitigate the bias. For example, recoding the hypothetical responses for respondents who are uncertain tends to reduce hypothetical WTP so that it is statistically equivalent to real WTP (e.g., Blomquist et al. 2009).

In the contingent behavior literature, hypothetical bias exists if there are differences in real and hypothetical behavior. For example, Grijalva, et al. (2002) conducted recreation surveys before and after closure of a popular rocking climbing area in Texas. They find some evidence that hypothetical climbing trips with the closure accurately predict the number of actual trips after the closure, suggesting that hypothetical behavior responses have external validity. Whitehead (2005) conducted hurricane evacuation surveys before and after a major storm and finds some evidence that hypothetical evacuation behavior data has external validity.

Studies of external validity of DCEs require real payments or behavior with multiple varying choice attributes, which are logistically difficult to design. Most of the existing studies are laboratory experiments.[1] Carlsson and Martinsson (2001) find no differences between real and hypothetical donations to the World Wildlife Fund projects in different locations. The real treatment followed the hypothetical treatment for each respondent. Grebitus, Lusk, and Nayga (2013) conduct a laboratory choice experiment where the attributes are product (apples, wine) price and miles traveled (i.e., pollution) and find differences between the real and hypothetical data. In the health economics literature, Quaife et al. (2018) consider six DCE studies and find some evidence of external validity. de Bekker-Grob et al. (2019), in a health DCE, find that hypothetical behavior models make good predictions if preference heterogeneity is taken into account.

Externally validity tests in the stated preference literature are logistically challenging. In the absence of the opportunity for an external validity test, we consider evidence from a laboratory experiment that was designed to mimic a DCE. One of the tenets of experimental economics is that payoffs matter. Smith and Walker (1993) review 31 experimental studies and find that as monetary rewards increase lab decision-making moves towards the theoretical predictions with decreasing variance. Camerer and Hogart (1999) reviewed 74 studies where financial incentives were either zero, low or high and conclude "the effect of incentives is mixed and complicated." They suggest incentives may affect average performance when effort is involved and intrinsic motivation is not high. Camerer and Hogart (1999) agree with Smith and

[1] A few field experiment studies exist but most of these do not include hypothetical choices (e.g., Newell and Swallow 2013).

Walker (1993) that financial incentives often reduce response variance.

In general, there are several factors that can interact with incentives and affect outcomes in laboratory experiments. Subjects' intrinsic motivation, the attractiveness and/or simplicity of the task, and the subject's experience or cognitive ability can affect the impact of incentives on experimental outcomes. Wilcox (1993) finds support for decision cost models when the task environment is complex. Using decision time to measure subjects willingness to incur decision costs, subjects were more likely to incur cost for higher rewards, but not for simple tasks. Moreover, rewards affected lottery choices only for complex tasks. Bonner et al. (2000) find a positive incentive effect in about half of the experiments they review, noting that the type of task and skill levels matter. McDaniel and Rutström (2001) do not find evidence that extrinsic rewards crowd out intrinsic motivation for their subjects, but there is a lot of individual variation. Taylor et al. (2001) find equivalence between real and hypothetical choices with induced values and referendum voting. Bonner et al. and Camerer and Hogart find limited improvement from incentives when decisions become sufficiently complex. Financial rewards may thus be beneficial when added complexity requires extra effort, but only up to a point.

For our experiment, subjects will likely have mixed levels of knowledge about the task, and thus different opinions regarding the task complexity. The framing of the task which includes the purchase of pollution permits could also affect motivation as it triggers thoughts of climate change. Thus, it is not immediately obvious that financial incentives will improve subject's performance in the experiment. Our expectation is in line with the Smith and Walker (1993) suggestion that subjects make trade-offs between the benefits and costs of error reduction, and that the marginal incentives in a real payment scenario will increase the subject's benefit of extra effort. Therefore, we expect subjects facing marginal incentives to be more attentive to task details. To the extent that the task is not too cognitively demanding, additional attention should move subjects closer to theoretical predictions.

If models that incorporate ANA provide a better model of hypothetical behavior then ANA should improve external validity. None of the ANA studies identified by Lew and Whitehead (2020a) conduct an external validity test in which stated behavior are compared to real behavior. In this paper we conduct a hypothetical (non-incentivized) and real (incentivized) experiment and analyze the data with an inferred ANA model. The experiment that we conduct is similar to the Wråke et al. (2010) emissions permit experiments and conducted with the Veconlab online software (Holt et al. 2010).[2] Our conjecture is that ANA will be more pronounced in hypothetical experiments and, once accounted for with the ANA model, the hypothetical and real decisions influenced by monetary payoffs will be more similar. In the next section we describe the Veconlab experiment. This is followed by a description of the data, the empirical model and results. Conclusions and suggestions for future research follow.

Experiment

The experiments that we conduct are based on the supply and opportunity cost individual experiment in the Veconlab software. Subjects are faced with a task of choosing how much output to produce given an output price and input cost. In addition, respondents must hold an emissions permit in order to produce. In one treatment the permits can be purchased for a price (i.e., an emissions tax). In another treatment, permits are given to the subject (i.e., grandfathered)

[2] The Veconlab experiments are available at http://veconlab.econ.virginia.edu.

and can be used to produce or sold back in the emissions permit market. The latter treatment introduces an implicit opportunity cost. If the implicit cost is not ignored then the supply curves will be equivalent in the two treatments. A screenshot of the decision screen in grandfathered permit round is provided in Figure 1.

More formally, in the emissions permit experiment research subjects decide whether to produce a unit of output ($y = 1$) or not ($y = 0$). The incentive to produce is the difference between the output price and the sum of a fuel input cost and the cost of the required emissions permit. The output price is randomly drawn from a uniform distribution: $p \sim U[2,11]$. There are three production decisions in each decision-making round. The input cost increases with each unit of output, $x = 1, 2, 3$. The input costs are randomly drawn from uniform distributions: $c_1 \sim U[0,2]$, $c_2 \sim U[2,4]$, and $c_3 \sim U[4,6]$.

In each experimental session there are two treatments with 8 rounds each, and subjects participated in both treatments (in-sample design). In treatment 1 two "free" emissions permits are provided (i.e., grandfathered). If the research subject wants to produce the third unit they must purchase a permit from the market. In treatment 2 there are no grandfathered permits. If the subject wants to produce a unit of output then a permit must be purchased from the market. The cost of an emissions permit (i.e., allowance), A, is randomly drawn from a uniform distribution: $c_A \sim U[2,4]$.

In the treatment 1 rounds the cost of the first two grandfathered permits is implicit (i.e., an opportunity cost) and the cost of the third permit is explicit. Participants can sell their grandfathered permits in the permit market instead of producing. In the treatment 2 rounds there are no grandfathered permits. Participants must purchase a permit from the market so that the cost of each permit is explicit. In each round the decision of whether to produce is:

$$Prob(y = 1) = Prob(p_x - c_x - c_A > 0),\ x = 1,2,3.$$

Six sessions of the Veconlab experiment were conducted on-campus in the Appalachian Experimental Economics Laboratory (AppEEL) in February 2020. In three of the sessions, subjects received a show up fee and the earnings were hypothetical. In the other three sessions the payoffs were "real." Two additional sessions were conducted online in March 2021. One of these had real payoffs and the other was hypothetical. The average payoff in the real payoff sessions was $22. There are 16 rounds of decisions in each experimental session. There are 6 sessions with treatment 1 (grandfathered permits) in rounds 1-8 and treatment 2 in rounds 9-16. Two of the on-campus lab sessions reversed the order of the treatments.

Data

The sample size is $n = 138$ with 70 participants in the real payoff sessions and 68 participants in the hypothetical sessions. The sample demographics are presented in Table 1. All of the subjects are university students. Forty-six percent of the sample is male and the average age is 20 years. Eighty percent of the sample is white and has attended university an average of 2.5 years. Forty-one percent of the respondents are business majors. We find no statistically significant differences in demographics in laboratory vs. online or real vs. hypothetical sessions.

The sample size in each session ranges from 16 to 22 (Table 2). There are four different samples of subjects for comparison. These are (1) real payment sessions and rounds with the grandfathered permit treatment (i.e., implicit cost of a permit), (2) real payment sessions with the taxed permit treatment (i.e., explicit cost of a permit), (3) hypothetical payment sessions and grandfathered permits and (4) hypothetical payments and taxed permits.

The theoretical proportions based on the random draws of prices and costs are in the predicted column of Table 3.[3] A difference in proportions test for sample 1 vs. sample 2 and sample 3 vs. sample 4 tests whether subjects more easily identify the payoff-maximizing production strategy when they receive no free allocations of permits. This test is the primary focus of Wråke et al. (2010). We find differences in the proportions of sales for subjects in both real ($z = 2.52$) and hypothetical ($z = 2.42$) sessions. Subjects in both real and hypothetical treatments overproduce when the allowance cost is implicit. This result suggests that the opportunity cost of selling emissions allowances may be ignored by subjects. In the stated preference data context this may interpreted as attribute non-attendance.

Model

We treat each output decision as a discrete choice and estimate panel data models with $t = 48$ observations per subject (16 rounds × 3 decisions per round). We estimate a logistic regression model with unit sales as the dependent variable and the output price, input cost, and implicit and explicit permit costs as independent variables. Estimation is based on a linear utility

[3] The experimental software does not have a constraint requiring that unit 1 must be purchased before unit 2 or 3, or that unit 2 be purchased before unit 3. With 3 units, there are 8 possible purchase vectors: [0,0,0] [1,0,0] [1,1,0] [1,1,1,] [0,1,0] [0,0,1] [0,1,1] and [1,0,1] where 1 indicates unit i=1, 2, 3 was purchased, and 0 indicates it was not. With increasing marginal costs, the first four vectors are rational orders while the last four are not. Overall, these error vectors occur approximately 11.9% of the time. There is no difference in the number of errors in real treatments and significantly more when permits are grandfathered ($p<0.10$).

function:

$$v_{ns} = \beta' x_{it} + \varepsilon_{it}$$

The observable portion of individual *i*'s utility in round *t* is a linear function of the attribute vector x_{it} and coefficient vector *β*, which determines profit. Total utility is the sum of observable profit and an additive component that is unobservable to the researcher (ε_{it}).

Assuming the unobservable portion of utility ε_{ijt} is distributed type 1 extreme value, the probability that individual i will choose to produce the product in situation t is

$$\pi(y_{\text{it}} = 1) = \frac{1}{1 + exp(\beta' x_{\text{it}})}$$

Our departure from the standard model is the inferred ANA model which relies on maximum likelihood to place respondents into different behavioral classes. Each class is defined by a set of parameter restrictions, potentially setting coefficients equal to zero when a respondent indicated they did not attend to the corresponding attributes.

For *k* attributes describing a discrete alternative there are a total of 2^k possible attribute (non-) attendance classes. Thus, for our empirical setting with four attributes (price, fuel cost, permit tax, opportunity cost) there are 16 possible classes of attribute non-attendance to which individual *n* may belong. In preliminary models we find that this equality-constrained latent class model does not produce reliable estimates of the class probabilities. For example, in the hypothetical model, most combinations of ANA constraints lead to class probabilities of zero and all but one of the attribute coefficients are not statistically different from zero. We abstract away from the full set of possibilities and focus on two classes as suggested by Malone and Lusk

(2018): total attendance and total non-attendance.

The probability of observing the individual choosing alternative *j* in a 2-class model is

$$\pi(y_{\mathrm{ijt}}) = \sum_{c=1}^{2}\left[\frac{exp(\theta_c)}{\sum_c^C exp(\theta_l)} \times \frac{1}{1 + exp([a_i\beta_j]'x_{\mathrm{ijt}})}\right]$$

where θ_c is a vector of estimated parameters, Class membership is unknown to the analyst and is instead treated probabilistically. Estimation requires specifying the ANA class probabilities, which are the probabilities that individual *i* belongs to class *c*. The left-most term in the right-hand side equation is the probability of membership in latent class q, where θ_c is a class-specific constant parameter to be estimated, $\sum \frac{exp(\theta_c)}{\sum_c^C exp(\theta_l)} = 1.$ In this model the β vector is modified by a_i indicating which elements of β_j are restricted to zero for the i^{th} respondent due to ANA. The coefficient vector modifier a is equal to 0 if the respondent is assumed to exhibit non-attendance behavior. In the Malone and Lusk (2018) model each of the attributes has a zero coefficient in one of the classes.

Results

We first estimate fixed coefficient logit models and find that production increases with the output price and decreases with the input cost and explicit permit costs in both the real and hypothetical data models. In both models the coefficient on the implicit permit (opportunity) cost is not statistically different from zero. This result suggests that the implicit permit cost was not a factor in any of the subjects' decision-making during the experiment.[4]

We next estimate latent class models separately with the real and hypothetical payments data. The first of these models has two classes with zero restrictions on coefficients. This model is statistically preferred to the fixed coefficient logit model with lower prediction error as measured by the Akaike information criterion (AIC).[5] Following this estimation, we iteratively explore additional patterns of ANA and choose a best model based on the likelihood ratio test and the AIC (Table 4).

With the real payment data, the Malone and Lusk (2018) model, with a zero constraint on each attribute coefficient in one of the subject classes, produces a higher AIC and a likelihood ratio χ^2 statistic that indicates the model is not an improvement. We then impose three zero constraints on attributes other than the output price. This is followed by a two-constraint model with the tax and opportunity cost coefficients constrained to zero and a one constraint model with the opportunity cost coefficient constrained to zero. None of these models are a statistical improvement over the two-class model with freely estimated coefficients in both classes. This indicates that there is no statistical evidence of attribute non-attendance behavior in the model from the real payment session data.

A similar process is conducted with the data from the hypothetical payment sessions. In this case the model with zero coefficient constraints on the tax and opportunity cost attributes in the second respondent class is superior to the model with freely estimated constraints. In this model the AIC statistic is lower than in the zero-constraint model and the likelihood ratio test

[4] These results are available upon request.
[5] We limit our attention to models with 2 classes given the small number of respondents.

indicates statistical equivalence between the two models (χ^2 = 2.89 [2 df]). This indicates that there is a positive probability that subjects in the hypothetical payment sessions ignored the tax and opportunity cost attribute levels.

We present the two statistically preferred models in Table 5. Theoretically, the effect of each type of cost on the decision to produce the product should be equal because a one dollar change in the price has the same effect on profit as a one dollar change in any of the costs in the other direction. However, we find significant differences. Also, the constant should not be statistically different from zero because, in the logit model, if the profit is zero the constant should also be zero so that the probability of a sale is equal to 50% (i.e., indifference).

In Table 5a we present the latent class logit model estimated with the real payment sessions data. The probability that a subject will be in the attending class (Class 1) is 68%. In this class we find that the probability of production increases with the output price and decreases with the fuel input cost, the explicit permit costs (tax) and the implicit permit opportunity cost as expected. The constant is not significantly different from zero.

The magnitude of the coefficients do not follow the theoretical prediction of equivalence. In the attending class the output price coefficient is only 75% of the fuel input cost coefficient. The fuel input cost has 1.7 times and 4.2 times larger effects than the explicit permit and implicit permit (opportunity) costs. The explicit permit cost has a 2.5 times larger effect than the implicit permit cost. Five of the six pairwise comparisons of the coefficients indicate that they are statistically different at the p=0.01 level. Only the price and tax coefficients are of similar magnitude as the difference cannot be rejected at the p=0.103 level.

There is a 32% chance that a real payments session subject will be in the second class, which is non-attending to the attributes relative to the first class. Only the price and fuel input cost coefficients are statistically significant and these are significantly lower than the coefficients in the first class. The price and fuel input cost coefficients are 7.4 and 3.1 times larger in the first class than the second class. The t-statistics for differences in coefficients across classes are 2.42 and 2.12 for the price and fuel input cost coefficient. While we did not find statistical evidence of non-attendance, the coefficients on the tax and opportunity cost attributes are not statistically different from zero indicating that these attributes did not have much impact on decisions in this respondent class. The constant is statistically different from zero indicating that respondents in this class are biased in favor of producing the product. The probability of selling the product when profit is zero is 66% relative to the 50% prediction.

The probability that a subject will be in the first (attending) class is 58% in the latent class logit model estimated with the hypothetical payment sessions data (Table 5b). As in the real payments model, in this class we find that the probability of production increases with the output price and decreases with the fuel input cost, the explicit permit costs (tax) and the implicit permit opportunity cost, as expected. The constant is not significantly different from zero.

The magnitudes of the coefficients on price, fuel input cost and the explicit permit cost are of similar magnitude. The largest difference is between the fuel input cost and explicit permit cost coefficients with the input cost being 22% higher. The coefficient on the implicit permit cost is less than half of the other attributes indicating that it has the lowest impact on the production decisions. Four of the 6 pairwise comparisons of the coefficients indicate that they are statistically different at the p=0.01 level. The magnitudes of the price and explicit permit cost coefficients (p=0.79) and the fuel input cost and explicit permit cost coefficients (p=0.11) are not statistically different. While the difference in the price and fuel input costs is only 18%, the coefficients are estimated so precisely that this difference is statistically significant.

There is a 42% chance that a hypothetical payments session subject will be in the second class, which is non-attending to the attributes relative to the first class. This probability is 31% higher than in the real payment session data model. In addition to the tax and opportunity cost coefficients being constrained to equal zero, the price and fuel input cost coefficients are significantly lower than their counterparts in the first respondent class. The price and input cost coefficients are only 10% and 30% of the magnitude of the coefficients in the first class. The t-statistics for differences in price and input cost coefficients across classes are 3.05 and 2.39. As in the previous model, the constant is statistically different from zero indicating that respondents in this class are biased in favor of producing the product. The probability of selling the product when profit is zero is 69%.

As in the fixed coefficients logit model, the real and hypothetical sessions data latent class models have several similarities. None of the corresponding coefficients in the first (attending) and second (non-attending) classes of the models are statistically different. The only difference across models is the zero coefficient constraints on the tax and implicit permit opportunity) cost coefficients in the hypothetical payment sessions data models.

Conclusions

In induced value experiments of regulated firm behavior we find that the effect of the cost of an emissions permit on behavior differs if the cost is implicit or explicit. In a fixed logit model, we find no differences in laboratory experiment participant behavior in sessions with hypothetical and real payoffs. In the real payoff experimental session we find that there are two classes of subjects with different behavior. But, we find no statistical evidence of attribute non-attendance. In the hypothetical experimental session we find two classes of subjects with different behaviors and evidence of attribute non-attendance for both the explicit and implicit costs of emissions permits. In the hypothetical session data model the probability that a subject will be in the non-attending class is higher than in the real payments session data model and we cannot reject zero constraints on regulatory cost coefficients in this non-attending class.

ANA models provide estimates of the probability that an experimental session participant will ignore experimental attributes and we find evidence of differences between sessions with real and hypothetical payoffs. These results have implications for stated preference research where it is more difficult to test external validity. In stated preference models with hypothetical data it is more likely that survey respondents will exhibit attribute non-attendance behavior. As in these experimental results, ANA empirical models can be used to identify respondents who probabilistically will behave more in accordance with economic theory. These ANA models are likely to provide improved welfare estimates over naïve models that ignore the possibility of attribute non-attendance.

While the results in this paper are suggestive, future experimental and stated preference research with real and hypothetical payments will provide more evidence on the external validity of ANA models. In experimental economics, future research could be conducted with different experiments to determine if ANA behavior exists in the lab. For example, experimental first price auctions with induced values often find that bidding behavior exhibits risk aversion. Latent class models can be used to estimate if there is risk heterogeneity in the subject pool and the extent to which subjects ignored induced values. In the environmental, health, marketing and transportation fields future field experiment studies could be paired with hypothetical surveys to conduct external validity tests. Also, these tests could be conducted without the collection of any new data. Inferred ANA latent class models can be applied to any existing data set (Petrolia and Hwang, 2020). Any previous study that has compared real and hypothetical data with induced values or attribute values, especially those that find a difference, is a candidate for an external validity test with inferred ANA empirical models.

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Figure 1. Round 4 Screen Shot of the Veconlab “Production Cost” Experiment

Submit Decision for Round 4, ID: 24

- You have the capacity to produce and sell up to 3 units of output.
- Each unit actually sold results in an additional fuel input cost: **$0.50** for unit 1, **$3.50** for unit 2, **$4.00** for unit 3.
- Each unit that you sell also requires a single allowance. You have been allocated **2 allowances** at no cost in this round., and if you use additional allowances, they must be purchased for **$3.50** each. If you sell fewer than 2 units of output and hence do not use all of your allowances, you can sell them and earn **$3.50** for such sale.
- **Note:** The going market price in this round is **$2.69** and all units that you sell will be sold at this price.
- Now choose a production quantity by deciding whether to pruduce or not for each of your 3 units of capacity.

	Unit 1	Unit 2	Unit 3	
Production Decision:	select	select	select	Submit

Round	Fuel Input Costs	Emissions Allowance Price	Your Production	Market Price	Sales Quantity	Sales Revenue	Production Cost	Emissions Allowance Revenue or Expenditure	Your Earnings	Total Earnings
current round	**1:$0.50 2:$3.50 3:$4.00**	**see emissions allowance price above**	*	**see market price above**	*	*	*	*	*	*
3	1:$1.00 2:$2.50 3:$5.00	$2.50 for all units	1 1 0	$5.52	2	$11.04	$3.50	$0.00	$ 7.54	$ 24.02
2	1:$0.00 2:$2.50 3:$4.50	$4.00 for all units	1 0 0	$4.48	1	$4.48	$0.00	$4.00	$ 8.48	$ 16.48
1	1:$1.50 2:$3.00 3:$4.50	$4.00 for all units	0 0 0	$4.75	0	$0.00	$0.00	$8.00	$ 8.00	$ 8.00

Table 1. Sample Demographics					
		Laboratory		Online	
Variable	Overall	Real	Hypothetical	Real	Hypothetical
Gender (male = 1)	46%	54%	42%	44%	41%
Age (years)	20.38	20.50	20.28	20.22	20.5
Race (white = 1)	80%	79%	84%	83%	73%
College (years)	2.51	2.42	2.38	2.61	2.91
Business major	41%	46%	44%	22%	36%
Sample size	138	48	50	18	22

Table 2. Experimental Sessions

Session		Sample Size
1	Lab, Real	16
2	Lab, Hypothetical	17
3	Lab, Hypothetical	17
4	Lab, Real	15
5	Lab, Real	17
6	Lab, Hypothetical	16
7	Online, Real	18
8	Online, Hypothetical	22

Table 3. Attribute Means, Predicted and Actual Sales and z-score for Difference in Proportions Test											
Group	Real	Grand fathered	Price	Fuel cost	Tax	Opportunity cost	Predicted (%)	Actual (%)	Mean difference	Sample Size	z-score
1	Yes	Yes	6.67	3.00	1.13	1.91	58.15	62.98	.052	1680	2.52
2	Yes	No	6.64	3.02	2.94	0	57.86	52.86	-.032	1680	
3	No	Yes	6.51	2.99	1.13	1.91	54.66	60.97	.036	1632	2.42
4	No	No	6.54	3.00	2.93	0	57.11	53.86	-.051	1632	

Table 4. Latent Class Logit Model Fit with Attribute Non-attendance Constraints

	Real			Hypothetical		
Constraints	LL	AIC	χ^2	LL	AIC	χ^2
0	-1521.98	3066.0		-1517.58	3057.2	
4	-1554.15	3122.3	73.13	-1574.94	3163.9	114.72
3	-1554.03	3124.1	72.90	-1568.22	3152.4	101.28
2	-1564.51	3147.0	93.86	-1519.03	3056.1	2.89
1	-1524.78	3069.6	14.39	-1525.51	3071.0	15.85

Table 5a. Latent Class Logit Models (dependent variable is Sale = 1): Real Sessions										
	Class 1					Class 2				
	Coefficient	S.E.	t-stat.	95% Conf. Int.		Coefficient	S.E.	t-stat.	95% Conf. Int.	
Constant	0.362	0.338	1.07	-0.300	1.025	0.686	0.368	1.86	-0.036	1.408
Price	0.764	0.045	16.99	0.676	0.852	0.103	0.030	3.46	0.045	0.161
Fuel cost	-1.024	0.061	-16.89	-1.143	-0.905	-0.328	0.047	-6.97	-0.420	-0.236
Explicit permit cost	-0.605	0.105	-5.78	-0.811	-0.400	-0.116	0.098	-1.19	-0.308	0.075
Implicit permit cost	-0.245	0.109	-2.25	-0.459	-0.031	0.037	0.103	0.35	-0.166	0.239
Class probability	0.679	0.061	11.09	0.559	0.799	0.321	0.061	5.23	0.201	0.441
LL	-1521.98									
AIC	3066									
Pseudo-R^2	0.347									
Subjects	70									
Decisions	48									
Sample Size	3360									

Table 5b. Latent Class Logit Models (dependent variable is Sale = 1): Hypothetical Sessions										
	Class 1					Class 2				
	Coefficient	S.E.	t-stat.	95% Conf. Int.		Coefficient	S.E.	t-stat.	95% Conf. Int.	
Constant	0.135	0.398	0.34	-0.646	0.916	0.788	0.189	4.17	0.418	1.158
Price	0.927	0.051	18.03	0.826	1.028	0.091	0.024	3.81	0.044	0.137
Fuel cost	-1.096	0.067	-16.26	-1.228	-0.964	-0.333	0.035	-9.60	-0.402	-0.265
Explicit permit cost	-0.896	0.121	-7.39	-1.134	-0.659	0 [fixed]				
Implicit permit cost	-0.462	0.118	-3.92	-0.694	-0.231	0 [fixed]				
Class probability	0.576	0.061	9.38	0.456	0.697	0.424	0.061	6.90	0.303	0.544
LL	-1519.03									
AIC	3056.1									
Pseudo-R^2	0.329									
Subjects	68									
Decisions	48									
Sample Size	3264									