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Final Report

Mileage-Based User Fee Demonstration Project:

Pay-As-You-Drive Experimental Findings

MINNESOTA DEPARTMENT OF TRANSPORTATION

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The Federal Highway Administration and the Minnesota Department of Transportation co-sponsored a demonstration to test how consumers would change their driving behavior if some of the fixed costs of owning and operating a car were to be converted to variable costs. One hundred and thirty participants were given devices that recorded mileage and time of travel. Prices per mile were assigned randomly to each participant, ranging from 5 cents per mile to 25 cents per mile. The findings indicate that per mile pricing does result in measurable, but small reductions in driving. The largest effect is on weekend driving and on peak weekday travel (as some participants were able to substitute mass transit for their vehicle). One key finding in this experiment is that those households that are willing to change their driving behavior will do so with low per mile cost incentives. On the other hand it was also determined that those households unable to change their behavior do not do so even under relatively higher cost incentives. Therefore, the marginal effect of per mile prices seems to drop off dramatically after some point in the lower range of prices.

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GeoStats
MarketLine Research

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Executive Summary

INTRODUCTION

The Pay-As-You-Drive experiment results described in this report are one component of a larger project to test the feasibility of converting the fixed costs of a personal auto to variable costs through pay-as-you-drive charges. This may be one means of using consumer price signals to reduce vehicle-miles traveled and ultimately highway congestion. The overall objectives of the project were to:

1. Simulate the replacement of the fixed costs of vehicle ownership and operation with variable costs that give drivers explicit price signals about travel decisions and alternatives;
2. Develop the best possible understanding of transportation price elasticities and how they vary by vehicle ownership/lease arrangement, income, location, annual mileage driven, and other factors;
3. Develop an understanding about driver acceptance of use-based fees and appropriate price signals necessary to affect travel behavior changes; and
4. Identify strategies and recommendations that might be employed to mainstream or institutionalize policies or techniques learned from the demonstration.

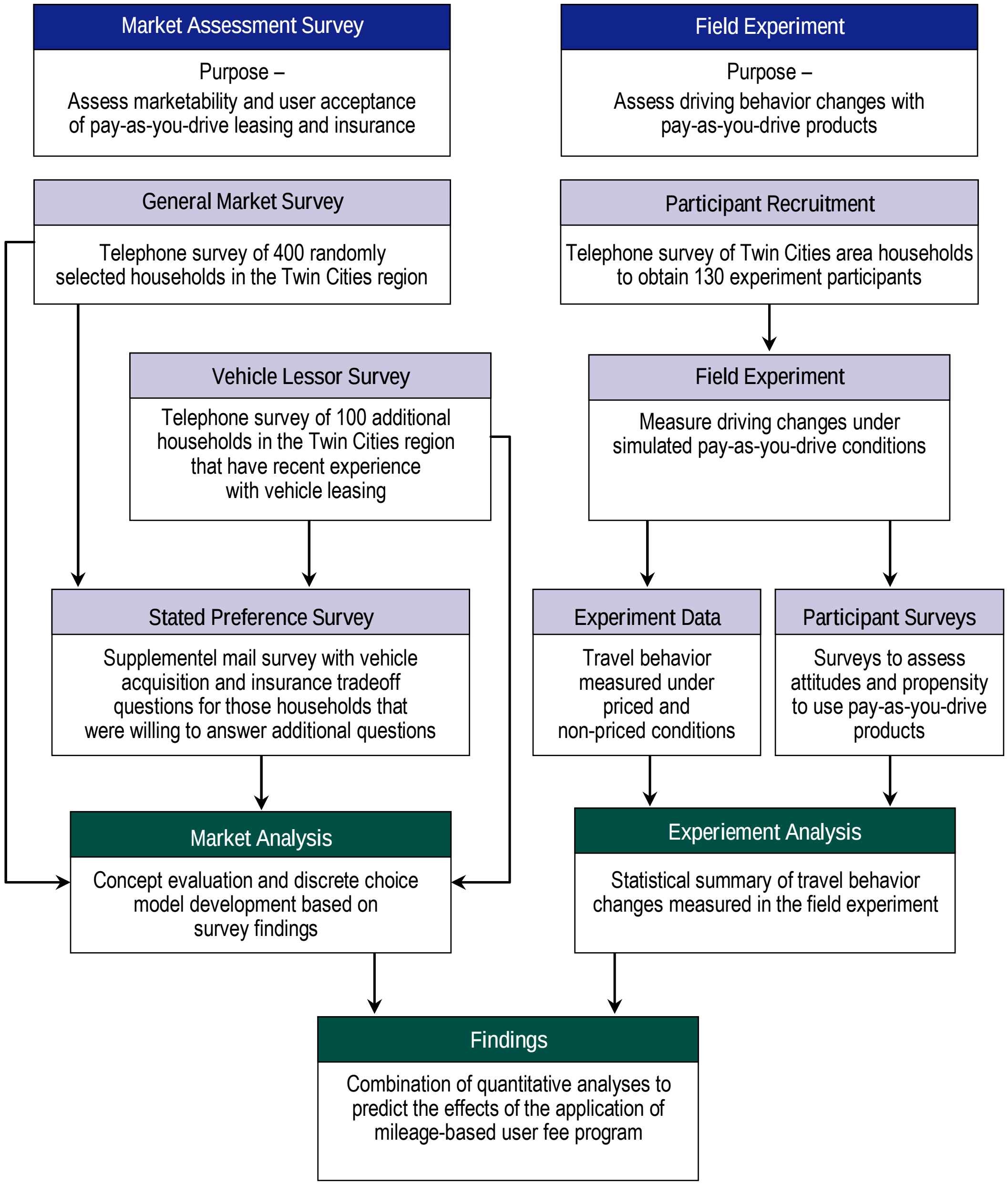
Previously, the project team conducted qualitative research on the pay-as-you-drive concept, investigated private sector interest in commercial products related to mileage-based pricing, and conducted broad market assessment surveys of the general population.

The market assessment survey and the pay-as-you-drive experiment comprise the quantitative element of the study (see Figure ES.1), intended to estimate:

- The level of interest in pay-as-you-drive products;
- The nature of the market for the concept;
- The response of drivers to price signals (price elasticities) that are based on miles driven; and
- The overall effect of the program on vehicle-miles traveled and traffic congestion.

The market assessment surveys provided general feedback on the concept and supplemented information derived from earlier focus groups. The results of the market research work were documented in a previous technical memorandum, *Market Assessment Survey Results, Mileage-Based User Fee Demonstration Project.* This technical memorandum focuses on the field experiment portion of the work.

Figure ES.1 Quantitative Elements of the Pay-As-You-Drive Study



Although there are already variable components to owning and operating a vehicle (e.g., price and consumption of gasoline, maintenance costs, etc.), the largest single outlay is typically the initial purchase price. Insuring a vehicle is another fixed cost with little relation to miles driven. Drivers make decisions about how they use their vehicles based on a cost structure that is heavily weighted toward this large "sunk" cost with relatively low-marginal costs for their use. As a society we have become accustomed to considerable freedom to drive where and when we please. Important life decisions are made about where to live and work based on the expectation of unlimited access to a personal automobile. Consumers have responded rationally to the price signals and incentives now available to them. This is manifested in the fact that many of us tend to drive a lot, drive alone, and live far from our place of work. The pay-as-you-drive approach studied here challenges this paradigm by more explicitly varying the costs of auto ownership and insurance by auto use.

Overview of the Field Experiment

The study team used a telephone survey to recruit a small sample of people that were willing to participate in a field experiment related to driving. Most of the households were recruited to be subject to pricing experiments (targeted at 100 participants) while others were recruited to be a pure control group, with no pricing (targeted at 30 people).

All participants' automobiles were outfitted with an electronic device called a CarChip, which recorded data about each trip the participant made, including mileage and time of travel. We asked the participants to swap their CarChips occasionally so that we could obtain their trip-making data. In addition, participants were asked to track and report odometer readings for other nonpriced vehicles in the household so that the impact of vehicle substitution could be measured. Participants were not penalized for substitution. This self-reported data is naturally considered to be less reliable than the CarChip data, but the project budget was not sufficient to instrument all vehicles in a household.

After an initial control period to obtain baseline driving behavior data, half of the "experiment" group were given simulated pricing experiments. We told them that starting on a certain date, any miles they drove in the monitored vehicle would be charged a certain rate per mile. We gave them a mileage budget set so that if they drove the same amount of miles as in the initial control period, they would use the entire budget. The experiment period was scheduled to last three months.

At the end of the three-month experiment period, those that had been "priced" would revert to an unpriced status, while those who had remained in "control" would have pricing experiments initiated for another three months.

At the end of the experiment, we administered an exit survey to evaluate participant attitudes toward the experiment itself, and toward pay-as-you-drive pricing products. We then analyzed the data to evaluate how driving patterns changed.

Experiment Design

The experiment design was driven by a desire to conduct the most robust, statistically valid experiment on potential changes to driving behavior due to pay-as-you-drive pricing given the availability and capability of the technology to record mileage, as well as time and budget constraints. The experiment was conducted by giving each participant household a monetary budget and a rate for each mile driven. Any money left in the budget at the end of the experiment was theirs to keep. We then analyzed the data to estimate changes that took place in driving behavior as a result of the pricing experiment.

The challenge was to reliably estimate the impact of the treatment (pricing). Since an individual can be observed in only one state (treated or untreated) at any time, the change cannot be computed for any particular individual. Consequently, we needed to consider the behavior of groups of individuals, and the behavior of individuals in one time period compared to another.

In considering different experiment designs, we conducted an extensive literature review that addressed the methodological problems associated with this kind of study. We conducted an eight-month experiment, as follows:

- One hundred thirty (130) households were recruited using a random digit dialing technique from households in the eight-county Minneapolis/St. Paul metropolitan area.
- Of these, 30 households were randomly assigned to the "control group." Their mileage would be tracked over the course of the experiment, but they would not be subjected to pricing experiments.
- After all participants drove for two months while being monitored with CarChips, one-half of the 100-person experiment group (50 households) were given a pricing experiment. The other half remained with no pricing. Pricing protocols were assigned randomly and ranged from $0.05 to $0.25. Mileage budgets were set based on the number of miles driven during the first month of travel with the CarChip. Priced households drove for three months with simulated prices. Pricing for some households was varied for peak and non-peak travel.
- At the beginning of the fifth month, experiment-group participants that were still not priced were given pricing experiments. Those that had been priced reverted to nonpriced status.

Experiment Implementation

The recruit survey was conducted in February and March of 2004. A welcome letter was sent to participants on behalf of the Mn/DOT project manager Deployment team members were then sent to the households to install the CarChips and to provide introductory study materials. This letter explained that participants would receive their first CarChip swap package in either one month (for the experiment participants) or two months (for the control group) and that they would receive $10 as an initial incentive payment within the next few weeks. A toll-free phone number was provided to all study participants if they had any questions. Installations occurred from March 7 through March 29. Each household's start date in the study was based on the installation date, with the dates scheduled for the start of the second and third study phases staggered accordingly.

The experiment design called for the collection of driver data during both control and experiment conditions during an 8-month/35-week period. Participating households, therefore, were divided into three groups based on variations of control and experiment time periods (the actual number of participants is shown in parentheses).

- **Control-Control-Control (CCC) –** The CCC group had no treatment throughout the experiment (31 households).
- **Control-Experiment-Control (CEC) –** The CEC group was to be monitored under no-treatment (control) for two months, then experience three months in treatment (with per-mile pricing), and then finished the final three months of the experiment time with no treatment (48 households).
- **Control-Control-Experiment (CCE) –** The CCE group was to be subjected to no treatment for the first five months of the experiment, but then switched to treated or priced conditions for the remaining three months (51 households).

The following issues occurred during the course of the study:

1. A few CarChips were returned with no data.
2. Some households did not report odometer readings for all vehicles. Letters were sent to these households explaining the need to report odometer readings for all vehicles, not just the CarChip vehicles. A few households decided that they did not want to participate and withdrew from the study.
3. Several households reported that the CarChip appeared to be causing operating problems with their vehicle. Although the CarChip manufacturer, Davis Instruments, denied this possibility, these households were convinced there was a problem and withdrew from the study.
4. A few households did not return their first swap package. These households were contacted and told that it was important to rotate their CarChips as scheduled.
5. During the first experiment period, it became apparent that expecting the households to swap twice a month might be too difficult. Reminder letters and calls were made to those who were the furthest behind in their rotations. Throughout this three-month period, as CEC CarChips were returned, statements were generated for all previous experiment periods covered by the CarChip data. Households that had not performed any CarChip swaps after installation were dropped from the study.
6. During the second experiment period, a personnel issue on the study team prevented swap packages from being mailed. Unfortunately, this was not discovered until several months later, when it became obvious that swap packages were no longer coming back from the field. Further investigation revealed that swap packages for all three groups were behind schedule and that the CCE group had not yet started its experiment. All participants in the study were contacted and told that the study was still underway and that

new swap packages would be forthcoming. Each household was asked to report the participation incentives received to date so that these payments could all be brought up to the current amount due. Upon return and download of the last CarChips deployed, it was discovered that some data for some household vehicles were lost during the previous months due to the extended deployment period without a download. Further contributing to the data loss was the discovery that the extended memory CarChips had been shipped from the factory with default settings that caused them to log unnecessary engine parameters resulting in a shorter than expected logging duration.

It was decided to extend the study from the originally scheduled end date in November, 2004 to a new end date in February, 2005 that would allow the CCE group to participate in a full three-month experiment period starting on November 6 and ending on February 5. The CCE group would still get the $100 participation incentive paid out over the total duration of the study. In addition, the remaining CEC participants were asked to continue in the study through February 2005. Those that stayed would receive a total of $120 for study participation. This group was told that a subset of them would be asked to enter an additional, shorter experiment period within the last few months of the study, thus creating three subgroups within the CEC category:

- The CECx subgroup, which ended their participation in November;
- The CECc subgroup, which had an additional control period; and
- The CECe subgroup, which had an additional experiment period.

Figure ES.2 schematically shows the control and pricing period schedules for the different groups of participants.

Going into the final experiment period in November, there were 41 out of 51 households remaining in the CCE group, 32 out of 48 households remaining in the CEC group, and 27 out of 31 remaining in the CCC group. Table ES.1 shows the final completion and dropout numbers by group.

With the gap in data retrieval in the middle of the experiment, we were concerned about whether the data would be adequate for use in analyzing behavior patterns. We evaluated the data available for each household and found the data to be adequate for the study.

Figure ES.2 Schedule of Control and Experiment Periods by Study Group

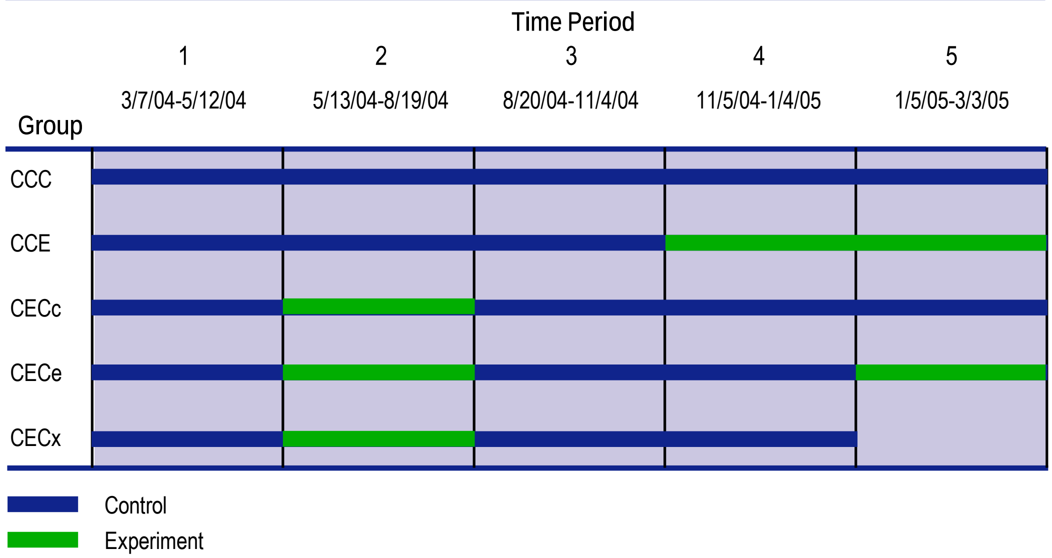


Table ES.1 Final Disposition of Participant Households

Group	Complete	Drop Out
CCC	27	4
CCE	41	10
CECc	23	14
CECe	9	
CECx	2	

EXPERIMENT FINDINGS

The experimental design provided many different ways to measure the change in mileage due to the pricing experiments. The section "Survey Analsyis" below summarizes the results of the recruit surveys and the exit surveys and the following section "Analysis of Driving Behavior" describes the analysis of the driving data. The main findings of this analysis are as follows:

- Wide-scale per-mile pricing would result in a measurable, but small, reduction in vehicle mileage.
- On a percentage basis, the biggest reduction in mileage would be on weekends, which would presumably have the highest percentage of discretionary travel purposes, but weekday peak-period travel would be reduced by more than weekday off-peak period mileage.

- Mileage reductions from per-mile pricing would vary by season, with the highest reductions during the summer.
- Some households could reduce their mileage under per-mile pricing at significantly higher levels than most households. Specifically, households that could reduce their mileage the most are those that:
  - Have other unpriced vehicles to which they could transfer their trips;
  - Have leased vehicles, probably because they are more accustomed to monitoring the mileage on vehicles; or
  - Have household members that actively think about auto ownership and operating costs.
- Households that are less likely to reduce their mileage under per-mile pricing are those that:
  - Share the use of one or more of their vehicles among household members; or
  - Have a head of household who is more than 65 years old.
- Higher per-mile charges do not necessarily seem to increase the mileage reduction of households. Those households that are willing/able to reduce their mileage apparently will do so even with low- to mid-level per-mile prices. Those that do not reduce their mileage do not seem to be able to do so even with higher incentives, at least to the extent offered in this experiment.

Survey Analysis

The recruitment survey collected information on respondent characteristics that help us to understand the overall Twin Cities vehicle travel market and to ensure that participants represent the overall market well. The exit surveys of those who agreed to participate in the driving study allowed us to understand how participants reacted to the study and how the pricing affected participants' attitudes toward pay-as-you-drive pricing.

Recruitment Survey

In February 2004, interviewers from MarketLine Research contacted households in the Twin Cities metropolitan area to collect vehicle usage information and to recruit study participants. There were 2,320 completed surveys for a response rate of 43.1 percent. Most of the 2,320 willing survey respondents were screened out of participating in the experiment for various reasons. Of those remaining, 660 telephone respondents were asked to participate in the study, and 186 agreed to do so (28 percent). The characteristics of the cooperating respondents were similar to those who declined to participate in the study, and to those who did not qualify for the study.

Participant Exit Survey

At the conclusion of the pricing study, all participants were asked to complete a survey that covered the conduct of the study, their behavior during the study, attitudes toward travel, and their assessments of pay-as-you-drive leasing and insurance concepts.

The exit surveys began by asking participants to evaluate their overall experience in the study on a 1 (poor) to 5 (excellent) scale. More than 60 percent of both the control group participants and the experiment participants found the experiment to be "Very Good" (rating 4) or "Excellent" (rating 5). Only 11 percent of each of the groups rated the experience as "Poor" (rating 1) or "Fair" (rating 2). These high ratings would tend to indicate that the logistical problems with the study were resolved to the satisfaction of most participants. The distribution of responses did not indicate a statistically measurable difference between the responses of the control group and experiment group participants.

The survey then asked respondents to provide evaluations of different elements of the study. Among the control group there was no basis for change in travel patterns due to pricing during the study. Ninety-three percent of the control group, versus 69 percent of the experiment group, agreed that the study did not affect their driving habits.

Both experiment and control groups felt that price uncertainty would be an important factor in considering whether to try pay-as-you-drive insurance and leasing. Another important factor was the potential cost savings. The control group felt that the ability to control costs by reducing mileage was not as important as the experiment group. Compared to the experiment group, the control group felt that privacy concerns were a more important consideration in their adoption of pay-as-you-drive insurance and leasing.

Exposure to the experiment made respondents more receptive to consider alternate modes of insurance and vehicle purchases. Consistently, the experiment group was more likely to choose pay-as-you-drive insurance and leasing if available. In addition, the experiment group was more likely than the control group to consider pay-as-you-drive insurance and leasing if features such as variable mileage pricing by time of day and yearly audits were offered. An overwhelming majority of the participants said that they were more likely to choose pay-as-you-drive insurance if they could switch back to traditional insurance without penalties.

Analysis of Driving Behavior

The most straightforward method for measuring the effects of the experiment was to compare the vehicle mileage of all the vehicles that were being priced to the vehicle mileage of all the vehicles that were not being priced. This aggregate measure of mileage differences was then enhanced by looking at the experiment-versus-control average mileages over the different experiment time periods and for different experimental pricing levels, as well as by examining the change in mileage of every group of participants separately over time. These analyses are described as mileage comparisons by group, below.

Because the participants represented a fairly diverse group, the differences measured in the straightforward group comparisons may be influenced by the individual characteristics of the participant households. To account for this issue, we also analyzed the mileage differences for individual vehicles in the experiment. For each vehicle, we compared the average mileage driven under the initial control period with the average mileage driven during the following experimental and control periods. We also sought to explain these mileage differences and the propensity to change mileage in terms of household and vehicle characteristics through the use of regression analysis. These are described in the section "Mileage Comparisons for Individual Vehicles."

Finally, we matched the household and vehicle characteristics of the experiment group to those of the control group, compared the mileage differences between them, and calculated the elasticities of miles driven with respect to price. Through the matching method, we sought to account for both the exogenous differences within the groups and the important effects of mileage changes between time periods.

Mileage Comparisons by Group

The simplest comparison is between total miles driven by the vehicles that were subject to the pricing with those by nonpriced vehicles. This analysis compared the mileage from all the vehicles in the unpriced "C" periods to the mileage from all the priced "E" periods. As shown in Figure ES.3, during the course of the study, the average daily mileage of unpriced vehicles was 47.5 miles, compared to an average of 45.4 miles for the priced vehicles (4.4 percent difference). Comparatively larger differences in percentage terms were measured for weekend trips (8.1 percent) and for weekday peak-period trips (6.6 percent). In all of the comparison cases, the average mileage during priced periods was lower than for unpriced periods, which may indicate that the pricing had a measurable effect on vehicle-miles traveled.

Figure ES.4 shows the differences in average daily miles separately for the five distinct experiment time periods. Time Period 1 was unpriced for all participants. The average daily vehicle mileage for this period was 46.7. During Period 2, the average unpriced mileage increased to 49.8 miles, and priced vehicle mileage was slightly less, at 46.4 miles. In Period 3, there were no pricing data due to the data collection problems, but the average unpriced vehicle had almost the same mileage as the unpriced vehicle in Period 2. During the fourth and fifth periods, there were almost no differences in the priced and unpriced averages. Compared to the seasonal differences for the unpriced vehicles, the differences between the unpriced and priced vehicles within the same time periods are small.

Figure ES.3 Comparison of Average Miles Driven for Vehicles
Entire Study Timeframe

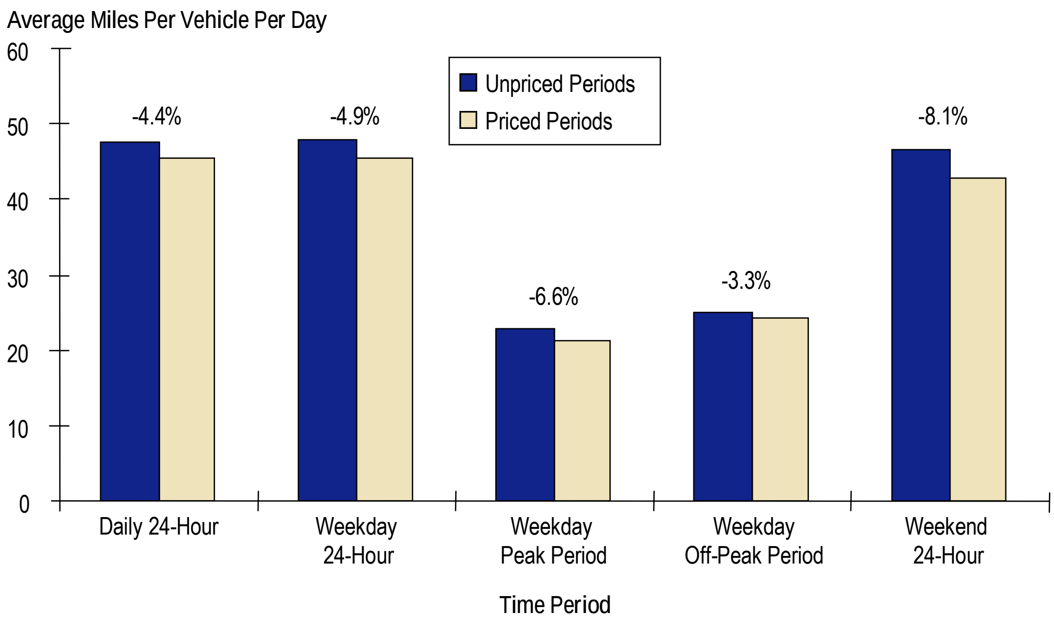
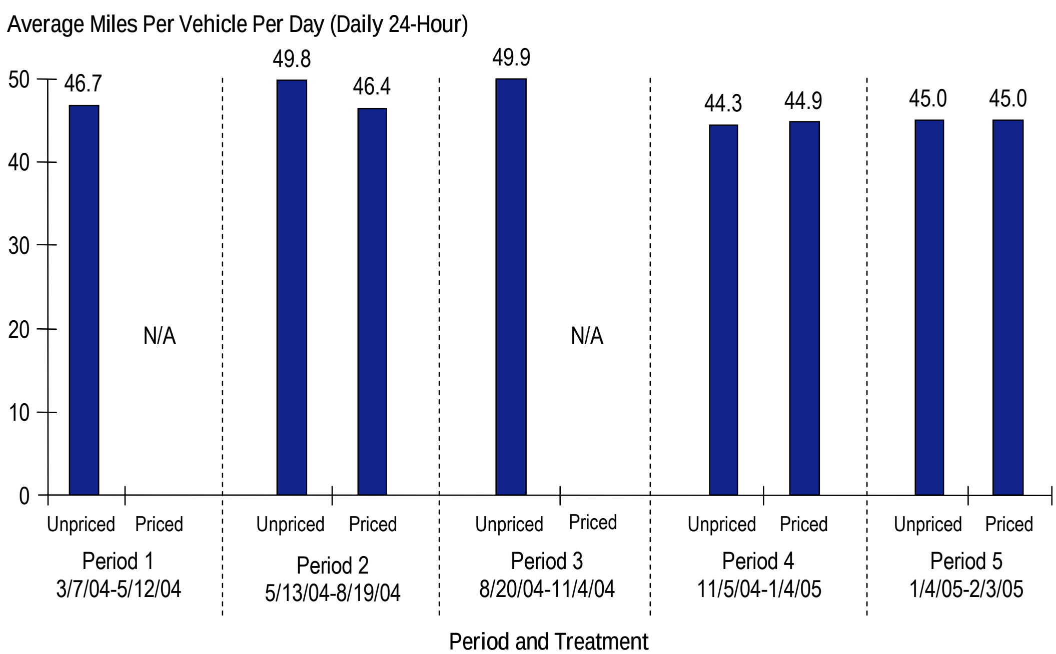


Figure ES.4 Average Miles Per Vehicle Per Day by Calendar Period



A second way in which the initial calculation of differences oversimplifies the effects of the experiment is that it did not account for pricing levels. The experiment allowed for the measurement of differences under several pricing treatments, but with small sample sizes. We would expect the average mileage measures in these figures to decrease as the pricing levels are increased, but the mileage pattern appears to be almost random. Average daily mileage for vehicles being priced at $0.05 per mile and for vehicles being priced at $0.20 per mile on a flat rate basis are 12 percent higher and 23 percent higher, respectively, than the average unpriced mileage. The time-of-day pricing treatments show similar results, with some of the highest pricing levels showing increases in the average daily miles traveled. These curious results are most likely due to the small sample sizes in each pricing category.

Still another way to evaluate the effect of the pricing treatments is to examine every group of participants separately and evaluate their mileage changes over the different calendar periods of the study. Table ES.2 shows that the pattern of mileage changes due to the pricing makes sense in general. All groups decrease their average mileage during the periods when they are priced. The CCE group reduce their mileage by one mile per day in Period 4 and 0.9 mile per day in Period 5 (priced periods for CCE), but surprisingly decrease their mileage by 5.8 miles in Period 2 (unpriced period for CCE). The CECc group reduce their mileage by 8.7 miles during Period 2 which is priced, and the CECe group reduce their mileage by 4.2 miles in Period 2 and 1.6 miles in Period 5 (priced periods for CECe).

Table ES.2 Row-wise Group Difference from Period 1
Netting out Seasonality Effect

	Time Periods				
Group	3/7/04 to 5/12/04	5/13/04 to 8/19/04	8/20/04 to 11/4/04	11/5/04 to 1/4/05	1/5/05 to 3/3/05
CCC	N/A	N/A	N/A	N/A	N/A
CCE	N/A	-5.8	4.5	***-1.0***	***-0.9***
CECc	N/A	***-8.7***	-0.6	-1.0	-4.4
CECe	N/A	***-4.2***	2.9	1.8	***-1.6***

Note: Cell values that are in ***bold and italics*** refer to periods when the corresponding group was subjected to pricing.

The CEC groups seem to be more responsive to the pricing treatments than the CCE group. The data quality problems that were encountered during the summer period may be partially responsible for this difference, but because of the extensive data cleaning that was performed it is our conclusion that the differences are valid. It is likely that the ability to reduce travel is seasonal, with a greater percentage of discretionary trips in the summer. One would assume that these discretionary trips are more likely to be foregone with the pricing incentive in effect.

Mileage Comparisons for Individual Vehicles

A second way to consider the mileage effects of the pricing is to evaluate the mileage differences for each vehicle in the experiment individually. The vehicles that were in the control phase in Period 2 increased their mileage compared with their initial control period by a statistically significant amount. Those who went into the experiment phase during Period 2 had on average a slight decrease in mileage. Thus, as the previous analysis had determined, the differences between the Period 2 control and experiment groups are significant. The statistical t-score was 1.95, representing a 93 percent confidence level. For the other experimental periods – Periods 4 and 5 – the differences were found to be more minor and statistically insignificant.

We modeled the effect of the prices on driving behavior at a disaggregate level through regression analysis. For this analysis, we examined all vehicles that were in treatment during Periods 2, 4, and 5. For every vehicle in treatment, we defined the reduction in mileage as the vehicle's daily mileage during the treatment period minus its daily mileage during the first control period (Period 1). Then we tried to relate the reduction in average daily mileage to the peak and off-peak charges, time period of the experiment, vehicle characteristics (such as the level of comfort), and socioeconomic characteristics of the household (such as age, income, vehicle availability, and attitudes toward driving in general and its associated cost). The vehicle and socioeconomic characteristics were obtained from the recruit and exit surveys, in addition to the experiment databases.

The following conclusions can be drawn from this regression:

- The negative coefficients of the peak price variables indicate that relative to a base peak price of $0.05 per mile, pricing at higher rates causes households to reduce their driving of the priced vehicle(s). Furthermore, the higher the peak price is, the higher is the reduction in average daily mileage.
- The coefficients of the period variables indicate that, with everything else the same, there was overall more reduction in mileage during Periods 4 and 5 than during Period 2.
- The coefficient of "unpriced vehicles in hh" is negative, indicating a substitution effect between vehicles available to the household; if one or more unpriced vehicles are available, the household can shift some of the driving from the priced to the unpriced vehicle(s).
- If one or more vehicles in the household are shared among household members, the prices do not affect the mileage (mileage slightly increases) possibly because of the difficulty in coordinating the driving of the priced and shared vehicle(s).
- If one or more vehicles in the household are leased, the household is very likely to reduce driving. This effect is strong and makes sense because households that already are used to leasing autos are more aware of the associated costs.

Mileage Comparisons Using Matching Methods

Matching is a common method that is used to evaluate the impact of a treatment. The method of matching computes the mean effect of a treatment by matching the units (households) in the treatment sample to other nontreated units in a comparison sample and then computing the change in outcomes (mileage) between the matched units. A unit in a treatment group can be matched to one or more units in the comparison (nontreated) group based on similar observed characteristics or on similar probabilities of participation in the program.

While many participants reduced their mileage as expected, several others increased their mileage when subjected to pricing. One would expect that as the per-mile price charged increases, the household would reduce the mileage. This holds for several but not all of the price categories, probably because of the small sample size in the experiment. The following conclusions can be drawn from the regressions performed on the data, which generally support other conclusions drawn from the previous analyses:

- The intercepts in both regressions are negative, indicating a negative base effect (decrease in mileage with pricing).
- The effect of having one or more leased vehicles in the household is that the household responds by reducing mileage on the priced vehicle (negative coefficient in regression) because households that already are used to leasing autos are more aware of the associated costs.
- The presence of unpriced vehicles in the household causes a decrease in mileage for the priced vehicle because of there is some mileage substitution between the priced and unpriced vehicles, hence the negative coefficient in the regression. However, this is not a uniform phenomenon (only 39 percent of such households increased mileage on the unpriced vehicle) and does not account for all of the mileage reduction in the priced vehicles.

Finally, we conducted an outlier analysis by relating the increase in mileage for some of the participants to their answers to the recruit and exit surveys. The sample of respondents used in this analysis are those that increased their mileage by 20 percent or more in their priced periods (Periods 2 and 4 are used for this analysis) relative to the first control period. Most of these respondents agreed that the per-mile price made no effect on their driving patterns in the short term, they did not reduce mileage during any time period (even discretionary travel), and did not try shifting to other modes/patterns (shifting to unpriced vehicles, chaining trips, using bus/walk) to reduce mileage on the priced vehicles. The individuals' driving patterns do not indicate anything peculiar about those respondents who increased their mileage when their vehicles were priced. However, the outliers were more likely than the mainstream participants to have the following characteristics:

- Live in Chisago County on the outskirts of the Metro area where people are likely to be very auto dependent;
- Have two to three vehicles in the household indicating a high level of auto dependency and use;
- Have a college-graduate or post-graduate head of household; and
- Share one or more vehicles among household members making it harder to manage mileage budgets.

1.0 Introduction

The pay-as-you-drive experiment results described in this report are one component of a larger project to test the feasibility of converting the fixed costs of a personal auto to variable costs through pay-as-you-drive charges. This may be one means of providing consumers with stronger price signals upon which to make their travel decisions. The ultimate outcome of pay-as-you-drive pricing may be to reduce or eliminate lower value trips, or provide consumers with added incentives for trip-chaining, thus reducing vehicle-miles traveled and ultimately helping to reduce some highway congestion. The overall objectives of the project were to:

1. Simulate the replacement of the fixed costs of vehicle ownership and operation with variable costs that give drivers explicit price signals about travel decisions and alternatives;
2. Develop the best possible understanding of transportation price elasticities and how they vary by vehicle ownership/lease arrangement, income, location, annual mileage driven, and other factors;
3. Develop an understanding about driver acceptance of use-based fees and appropriate price signals necessary to affect travel behavior changes; and
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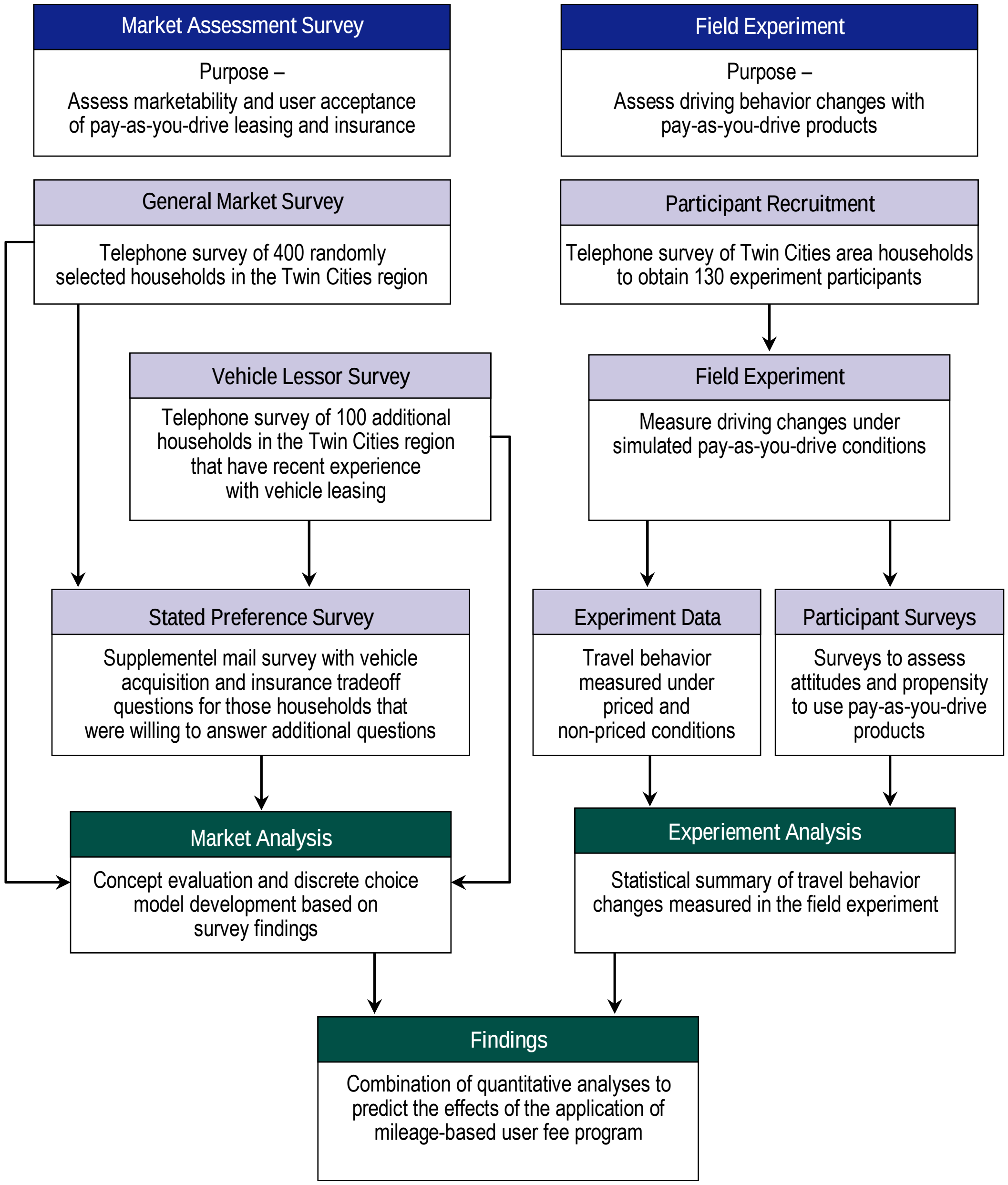
- The response of drivers to price signals (price elasticities) that are based on miles driven; and
- The overall effect of the program on vehicle-miles traveled and traffic congestion.

The market assessment surveys provided general feedback on the concept and supplemented information derived from earlier focus groups. The results of the market research work were documented in a previous technical memorandum, *Market Assessment Survey Results, Mileage-Based User Fee Demonstration Project.* This technical memorandum focuses on the field experiment portion of the work.

Figure 1.1 Quantitative Elements of the Pay-As-You-Drive Study



Although there are already variable components to owning and operating a vehicle (e.g., price and consumption of gasoline, maintenance costs, etc.), the largest single outlay is typically the initial purchase price. Insuring a vehicle is another fixed cost with little relation to miles driven. Drivers make decisions about how they use their vehicles based on a cost structure that is heavily weighted toward this large "sunk" cost with relatively low-marginal costs for their use. As a society we have become accustomed to considerable freedom to drive where and when we please. Important life decisions are made about where to live and work based on the expectation of unlimited access to a personal automobile. Consumers have responded rationally to the price signals and incentives now available to them. This is manifested in the fact that many of us tend to drive a lot, drive alone, and live far from our place of work. The pay-as-you-drive approach studied here challenges this paradigm by more explicitly varying the costs of auto ownership and insurance by auto use.

1.1 Overview of the Field Experiment

This section provides general context to readers as they read the details of the experiment concept and results in subsequent sections. The right side of Figure 1.1 shows the overall flow of the experiment.

The study team used a telephone survey to recruit a small sample of people that were willing to participate in a field experiment related to driving. Most of the households were recruited to be subject to pricing experiments (targeted at 100 participants) while others were recruited to be a pure control group, with no pricing (targeted at 30 people).

All participants' automobiles were outfitted with an electronic device called a CarChip, which recorded data about each trip the participant made, including mileage and time of travel. We asked the participants to swap their CarChips occasionally so that we could obtain their trip-making data.

After an initial control period to obtain baseline driving behavior data, half of the "experiment" group were given simulated pricing experiments. We told them that starting on a certain date, any miles they drove in the monitored vehicle would be charged a certain rate per mile. We gave them a mileage budget set so that if they drove the same amount of miles as in the initial control period, they would use the entire budget. The experiment period was scheduled to last three months, but was extended because of inadvertent data loses.

At the end of the three-month experiment period, those that had been "priced" would revert to an unpriced status, while those who had remained in "control" would have pricing experiments initiated for another three months.

At the end of the experiment, we administered an exit survey to evaluate participant attitudes towards the experiment itself, and towards pay-as-you-drive pricing products. We then analyzed the data to evaluate how driving patterns changed.

2.0 Experiment Design

The experiment design was driven by the desire to conduct the most robust, statistically valid experiment on potential changes to driving behavior due to pay-as-you-drive pricing given the availability and capability of the technology to record mileage, as well as time and budget constraints.

2.1 In-Vehicle Technology

GeoStats did an evaluation of different technology options and presented that evaluation to the study Advisory Committee.[1] The decision that emerged from that process was to use an in-vehicle device called CarChip. This is an off-the-shelf product that connects to a car's on-board diagnostic port (OBD II). This is the same port used by mechanics to get diagnostic readings. Although these devices do not have wireless transmission capabilities, they are easily swapped out by participants, and were inexpensive enough to maximize the sample size. CarChips also capture time-of-day data, which is an important part of the evaluation of the effectiveness of mileage-based fees at reducing peak-period congestion.

Other potential solutions could not compete on the basis of price and readiness for use in this application. GeoStats estimated that the next best solution (the Benefon Trackbox) would require a minimum of six months of development time, with no guarantees that the schedule could be met.

The CarChip approach did not permit capture of route choice data. That would have required a far more expensive and time-consuming technology solution. Evaluating route choice was never a goal of this project, but is being tested in another FHWA-funded project in the Seattle region.

2.2 Overall Experiment Structure

The purpose of the experiment was to understand how people might change driving behavior if they are charged for driving on a per-mile basis. The experiment was intended to simulate conversion of a portion of either leasing or insurance costs to a mileage basis, although by implication other costs could be assumed as well such as per-mile road user charges or vehicle depreciation. The simulation was achieved by giving each participant household a monetary budget and a rate for each mile driven. Any money left in the budget at the end

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[1] GeoStats LP, *Mn/DOT Mileage-Based User Fee Demonstration Project Technology Inventory and Assessment*, July 2003.

of the experiment was theirs to keep. We then analyzed the data to estimate changes that took place in driving behavior as a result of the pricing experiment.

The challenge was to reliably estimate the impact of the pricing (treatment). Since an individual can be observed in only one state (priced, or treated, and unpriced, or untreated) at any time, the change in mileage cannot be computed for any particular individual at any time. Consequently, we needed to consider the behavior of groups of individuals, the behavior of individuals in one time compared to another, and the behavior of individuals compared to other individuals with similar characteristics at a given time.

In considering different experiment designs, we conducted an extensive literature review that addressed the methodological problems associated with this kind of study. A summary of the literature review is provided below, with details in Appendix A.

We needed to compare the priced (treatment) and unpriced (no-treatment) mileage (outcome) of those that were subjected to pricing so that we could evaluate the impact of the pricing. The major issue in evaluating social programs is to estimate the no-treatment outcome (i.e., unpriced mileage, which is not observed at the time of the pricing) of those participants that are subjected to treatment. Two methods have been used to estimate the no-treatment outcome of a social program: experimental and nonexperimental.

The experimental method consists of randomly assigning a group of participants to a control group that does not receive treatment and the remaining participants to a treatment group. Assuming that nonrandom attrition from the experiment does not occur and that the control group members do not find a close substitute to the treatment elsewhere, one can then evaluate the mean impact of the treatment (pricing) by comparing the outcome (mileage) of the treatment and control groups. The no-treatment outcome of the experimental treatment group is then approximated by the outcome of the experimental control group.

Nonexperimental (or econometric) methods rely on the formulation of statistical and behavioral models of the outcome and participation decision processes. Such methods should be used if there is no experimental control group, or if there is nonrandom attrition from the experiment. The outcome of a nonparticipant comparison group is used to approximate the no-treatment outcome of the treatment group. Some of the common nonexperimental methods include the 1) method of matching, where a participant's outcome (mileage) is compared to the outcome of one or more nonparticipants with similar observed characteristics, and 2) the instrumental variable method, where participation is modeled first and a regression model of outcome is then estimated using the predicted participation as an additional explanatory variable.

We tried to design our experiment to allow both experimental and nonexperimental analyses to be used in the evaluation of the experiment data.

To accommodate this structure, we devised an eight-month experiment, as follows:

- One hundred thirty (130) households would be recruited using a random digit dialing technique from households in the eight-county Minneapolis/St. Paul metropolitan area.
- Of these, 30 households would be randomly assigned to the "control group." Their mileage would be tracked over the course of the experiment, but they would not be subjected to pricing experiments.
- After all participants would drive for two months while being monitored with CarChips, one-half of the 100-person experiment group (50 households) would be given a pricing experiment. The other half would remain with no pricing. Pricing protocols would be assigned randomly (see further detail below). Mileage budgets would be set based on the number of miles driven during the first month of travel with the CarChip. Priced households would drive for three months with simulated prices.
- At the beginning of the fifth month, experiment-group participants that were still not priced were given pricing experiments. Those that had been priced reverted to nonpriced status.

2.3 Experiment Design Details

The details of the experiment design and plan for implementation is described in this section, covering these topics:

- Recruitment and incentives;
- Initiation and data retrieval;
- Participant communications and price signals;
- Sizing the mileage fees;
- Pricing experiments;
- Mileage budgets; and
- Exit survey.

2.3.1 Recruitment and Incentives

Participants were recruited using a random digit dialing technique for households in the eight-county Minneapolis/St. Paul metropolitan area. In order to qualify for the experiment, respondents had to:

- Be a resident of the metropolitan area for at least six months (to ensure some measure of stability of travel patterns);
- Have at least one valid drivers' license in the household;

- Have driven on state highways and freeways in the past month;
- Typically drive at least 100 miles per week (total for all cars in household);
- Have one, two, or three vehicles in the household;
- Have at least one 1996 or newer vehicle in the household (to ensure that there was at least one vehicle that could accept the CarChip);
- Have no plans to acquire a vehicle in the next year (to reduce the potential for attrition due to a vehicle change that could influence driving behavior);
- Have no plans to move to a new address, stop working, or retire in the next year; and
- Not be employed in a way that might introduce bias (e.g., Mn/DOT, automobile dealership, market research firm).

We developed a recruit survey that obtained household demographic, auto ownership, and travel characteristic data from the respondents. If the respondent qualified, they were invited to participate in the "Mn/DOT Driving Study." They were not told that they would be given pricing experiments, so as not to influence their behavior during the control period. When inviting people to participate, we sought to achieve target participation rates for categories of different household characteristics based on estimates obtained from analysis of the National Household Travel Survey (NHTS).[2]

Since we expected that people's responses to the experiment would include changes in vehicle miles traveled and substitution between household vehicles, we tried to ensure that the participants included a mix of respondents among a few different categories:

- Number of vehicles (one, two, or three vehicles);
- Mileage level per vehicle (lower than median annual miles, higher than median annual miles); and
- Number of vehicles compared to number of licensed drivers (more drivers than vehicles, the same or fewer drivers than vehicles).

As shown in Table 2.1, we developed an initial recruitment plan to obtain households of each combination of categories. The recruit survey can be found in Appendix B; and a detailed discussion of how the experiment recruitment targets were established can be found in Appendix C.

[2] Bureau of Transportation Statistics/Federal Highway Administration, 2001 National Household Travel Survey, http://www.bts.gov/programs/national_household_travel_survey/.

Table 2.1 Proposed Participant Groups and Incidence

Participant Group	Workers in Household	Total Vehicles Available	1996 and Newer Vehicles	Percent of Households[a]	Proposed Number in Experiment Group	Proposed Number in Control Group
100	0	1	1	14.4%	10	3
110	1	1	1	17.5%	15	4
120	2+	1	1	6.6%	10	3
211	0, 1	2	2	5.9%	10	3
212	0, 1	2	1	8.7%	10	3
221	2+	2	2	12.7%	10	3
222	2+	2	1	18.7%	15	4
310	0, 1	3	1-3	3.4%	5	2
320	2+	3	1-3	12.1%	15	4
Total				100.0%	100	29

[a] The incidence of households of this type as a percentage of all eligible households, based on NHTS data.

Once the households had been recruited, we assigned the households in each category to different experiment protocols. The households within a group were assigned different pricing levels and different experiment timing periods, and within the groups, some households were asked to include all of their vehicles in the per-mile pricing, while other households were asked only to include their newest vehicle and to simply report the mileage of their other vehicles.

The actual distribution of willing participants varied from the proposed target, because of variations in cooperation rates among households with different numbers of vehicles and because of differences in reported and actual vehicle ownership levels. Respondents in households with two or more vehicles were more willing to complete the initial recruitment survey and participate in the experiment than those in households with one vehicle. In addition, some respondents misinterpreted the recruitment survey question regarding the number of vehicles available to their household members, so that when they entered the actual experiment their vehicle ownership levels were different than they had reported. The actual distribution of participant households is summarized in Table 2.2.

Table 2.2 Vehicle Availability Levels for Participant Households

Vehicles Available	Target Number in Experiment	Actual Number in Experiment	Target Number in Control Group	Actual Number in Control Group
1 vehicle	35	29	10	3
2 vehicle	45	49	13	18
3 vehicles	20	21	7	10

In order to encourage participation, recruits were offered incentives. For those that were to be subjected to pricing experiments, the incentive was $100 for completing the experiment. An incentive of $30 was offered for those in the pure control group. The experiment participants were also told that they would have an opportunity to make more money during the course of the study, but they were not explicitly told of the mileage budgets and per-mile pricing until the beginning of the pricing period.

2.3.2 Selection of Vehicles to Instrument

We wanted to understand the extent to which participants respond to pricing by switching travel from a priced vehicle to a nonpriced vehicle. This is important because a real-world product might allow people this opportunity. Therefore, it was desirable that all vehicle mileage in a household be measured so that we could easily track these shifts, so we asked participants to manually record the mileage on all automobiles in their household that were not priced.

Most of the participants only had one vehicle priced. A few households had all vehicles priced, so we could measure the difference in response. We expected that some participants would shift mileage from the priced to the nonpriced vehicles. A commercial vendor of a pricing product might not care that this happens – in fact, they, too, would expect it. From a public policy perspective, however, it is important to understand the extent to which such products only result in mileage shifting among household vehicles, as opposed to reductions in overall household driving.

2.3.3 Initiation and Data Retrieval

Once an interview respondent agreed to participate, they were initiated into the study. Initiation involved:

- A letter from the Mn/DOT project manager to the participants thanking them for their involvement in the study, and telling them to expect a call from GeoStats within two weeks to set up an appointment to install the CarChips (see Appendix D);
- A letter from GeoStats describing the experiment in detail, including the rules and the incentive schedule (see Appendix E); and
- A visit from GeoStats staff to install the CarChip in the vehicle(s), instruct participants in how to detach and reinstall replacement CarChips, and communicate other record keeping requirements.

When it was time for participants to report their mileage, GeoStats sent a new CarChip scheduled to arrive at least a day after the desired data collection period. The participants were instructed to remove the CarChip already installed in their car(s), and immediately replace it with the new CarChip. Participants put the old CarChip into a postage-prepaid envelope and mailed it back to GeoStats. Participants also were instructed to manually record the odometer readings on all vehicles in their household (see Appendix F).

The data retrieval occurred generally on the following schedule:

- All participants: once per month for the first two months;
- Participants in a pricing experiment period: twice per month; and
- Participants in a control period: once per month.

2.3.4 Participant Communications and Price Signals

A key element of the study was how participants respond to price signals regarding the cost of their driving. To provide these price signals, GeoStats retrieved CarChips from the participants; downloaded and analyzed the data; and created a statement showing the miles traveled, dollars expended, and dollars remaining in their account.

Statements were made available on-line, through a password-protected web site developed by GeoStats. GeoStats sent an e-mail to each participant advising them when their statements were available. The e-mails included a link to the web site, with instructions for accessing the account information.

The on-line statements included several levels of detail, including:

- Household odometer data, providing total mileage by vehicle;
- Vehicle activity statement summary, providing aggregate vehicle activity for the period and for the entire experiment, plus information on the dollar balance in the account;
- Detail view of mileage and cost by day; and
- Daily trip detail, showing each trip and the cost of those trips.

2.3.5 Sizing the Mileage Fees and Budgets

The previous work conducted with General Motors and during the project focus groups provided insight into establishing the range of the mileage fees that were analyzed. For a typical midsize car, the variable component of a pay-as-you-drive lease was estimated to vary between $0.10 and $0.15 per mile. The variable component of typical pay-as-you-drive insurance expenses is about $0.02 to $0.10 per mile. In the stated-preference survey portion of the market assessment survey work, we used pricing that matches reasonable levels that might be offered by a private leasing company. The prices were based on the make and model (or price range) of the next car the respondent planned to acquire and respondents' expected vehicle acquisition cost and insurance cost.

For the experiment, we were more interested in finding elasticity values for the per-mile prices that would be in the realm of reasonableness for the person's current vehicle, plus their insurance if converted to a mileage basis. Therefore, we established per-mile charges of between $0.05 and $0.25.

Since we wanted to test the mileage reduction differences at different times of day, some respondents were priced at higher per-mile levels during the weekday peak periods than at other times while other respondents were priced per mile the same amount at all times. Table 2.3 shows the 11 per-mile pricing levels that were tested in the experiment.

Table 2.3 Experiment Pricing Levels

Level	Type	Price Per Mile – Weekday Peak Hours	Price Per Mile – Other Times of Day	Number of Households	Number of Vehicles
1	Flat	$0.05	$0.05	10	10
2	Flat	$0.10	$0.10	22	22
3	Flat	$0.15	$0.15	11	12
4	Flat	$0.20	$0.20	10	10
5	Peak/Off-peak	$0.10	$0.05	11	14
6	Peak/Off-peak	$0.15	$0.05	9	10
7	Peak/Off-peak	$0.15	$0.10	9	11
8	Peak/Off-peak	$0.20	$0.05	4	5
9	Peak/Off-peak	$0.20	$0.10	5	6
10	Peak/Off-peak	$0.25	$0.05	4	6
11	Peak/Off-peak	$0.25	$0.10	2	2

Each household in the experiment was assigned a per-mile pricing level on a random basis. A few reallocations were then made to ensure that households with similar levels of vehicle availability, licensed drivers, and reported annual vehicle mileage were assigned to different pricing protocols.

Once initial vehicle mileage data were collected in the first two months of the study and the pricing levels were established for each household, we calculated household-specific mileage budgets. These budgets were set so that if a participant drove the same amount of miles as they had during the initial period and the per-mile amount assigned to their household was deducted from their mileage budget, they would end up close to zero dollars.

When participants were asked to begin their pricing periods, they were provided with an account with the initial budget amount and then this amount was reduced based on the miles they drove. At the end of the pricing period, any funds left in the account were given to the participants. If a participant drove more than their budget, their mileage budget was zeroed out – they were not required to pay any of their own money.

2.3.6 Incentive Schedule

The incentive schedule was devised with the intent of keeping the participants interested throughout the course of the experiment. This involved providing incentives at various milestones in the project, and then holding the final incentive until the exit survey was completed, as follows:

Experiment Group Participants

- $10 upon installation of the CarChip;
- $20 at the end of Month 3;
- $20 at the end of Month 5;
- $20 at the end of Month 8; and
- $30 upon the return of the final CarChip and completion of the exit survey.

Control Group Participants

- $10 upon installation of the CarChip;
- $20 at the end of Month 4; and
- $20 upon the return of the final CarChip and completion of the exit survey.

2.3.7 Exit Survey

All participants were asked to fill out an exit survey. The purpose of the exit survey was to provide a context for matching observed changes in driving behavior to participant attitudes and opinions. It also provided a way to relate responses from the market research and stated-preference surveys done with a different participant group to those that were exposed to a simulation of pay-as-you-drive products. Finally, it provided a way to get feedback from the participants about the mechanics of the study.

Similar exit survey instruments were developed for those that were subjected to pricing experiments and those that were in the pure control group.

3.0 Experiment Implementation

MarketLine conducted the recruit survey in February and March of 2004. As households were recruited, the recruit data were provided to GeoStats so that CarChip installations could be scheduled. GeoStats sent a welcome letter to participants on behalf of the Mn/DOT project manager (Appendix D). GeoStats' deployment team members were then sent to the households to install the CarChips and to provide introductory study materials (see Appendix E). This letter explained that participants would receive their first CarChip swap package in either one month (for the experiment participants) or two months (for the control group) and that they would receive $10 as an initial incentive payment within the next few weeks. GeoStats also provided a toll-free phone number to all study participants if they had any questions.

Installations occurred from March 7 through March 29. Each household's start date in the study was based on the installation date, with the dates scheduled for the start of the second and third study phases staggered accordingly.

The experiment design called for the collection of driver data during both control and experiment conditions during an 8-month/35-week period. Participating households, therefore, were divided into three groups based on variations of control and experiment time periods.

- **Control-Control-Control (CCC) –** The CCC group had no treatment throughout the experiment.
- **Control-Experiment-Control (CEC) –** The CEC group was to be monitored under no-treatment (control) for two months, then experience three months in treatment (with per-mile pricing), and then finished the final three months of the experiment time with no treatment.
- **Control-Control-Experiment (CCE) –** The CCE group were to be subjected to no treatment for the first five months of the experiment, but then switched to treated or priced conditions for the remaining three months.

After all installations were complete, the breakdown of households by group was:

- 31 households in the CCC group (control group throughout entire eight-month study period).
- 48 households in the CEC group (initial two-month control period, followed by three-month experiment period, followed by three-month control period).
- 51 households in the CCE group (initial two-month control period, followed by three-month control period, followed by three-month experiment period).

Minor implementation issues caused these numbers to vary slightly from the target of 30 CCC, 50 CEC, and 50 CCE households.

The swap schedule established for these groups was:

- Participants of the CEC and CCE groups would swap CarChips once a month during their control periods and twice a month during their experiment periods.
- Participants of the CCC group did not need to swap CarChips as frequently, since no price signals were needed, so they swapped CarChips every two months.

The CarChips were available in two versions – one that supported up to 75 operating hours of trip recording and the extended memory version that supported up to 300 operating hours. The extended memory CarChip was more expensive, and due to budgetary constraints, it was decided that 150 units of each type would be acquired. Given this mix of CarChip types, it also was decided that those participants in the CCC group would get the extended memory CarChips given their two-month rotation schedule, with participants in the CEC and CCE groups who had higher mileages targeted for the remaining extended memory CarChips.

3.1 Period 1 – Initial Two-Month Control Period (March-May 2004)

As the one-month mark approached for each household in the CEC and CCE groups, swap packages were created for each household, including replacement CarChip(s), a cover letter explaining the swap process (see Appendix F), and a control sheet for recording old and new CarChip serial numbers as well as the odometer readings for all household vehicles (see Appendix G). The swap packages were mailed to arrive one to two days after the one-month anniversary date.

During this initial two-month control period, several issues arose:

- A few CarChips were returned with no data. This might have been caused by a vehicle/CarChip incompatibility problem. When the CarChip was selected in August 2003, there were only a few vehicles on the CarChip incompatibility list. However, by the time the study started in the spring of 2004, many additional vehicle types had been added to the list. It was decided to wait until the end of the second swap rotation to confirm incompatibility. If it was then determined that the CarChip was not compatible with the vehicle, the household was notified by letter and a final incentive was offered to the household upon return of the last CarChip.
- Some households were not reporting odometer readings for all vehicles. Letters were sent to these households explaining the need to report odometer readings for all vehicles, not just the CarChip vehicles. At this point, a few households decided that they did not want to participate and withdrew from the study.

- Several households reported that the CarChip appeared to be causing operating problems with their vehicle. Although the CarChip manufacturer, Davis Instruments, denied this possibility, these households were convinced there was a problem and withdrew from the study.
- A few households did not return their first swap package. These households were contacted and told that it was important to rotate their CarChips as scheduled. Those who had not returned their swap packages by May were sent letters and return envelopes asking them to either perform the last swap or to withdraw from the study.

As the CarChips were returned during the first rotation in April, mileage details were sent to Cambridge Systematics so that initial mileage budgets and peak/off-peak mileage rates could be established for the CEC study group.

3.2 Period 2 – First Experiment Period (May-August 2004)

The initial control period ended with the second swap that occurred in May. At this time, households in the CEC group were sent swap packages along with letters explaining the experiment phase of the study (see Appendix H). Details of this letter included the initial mileage budget for each CarChipped vehicle as well as the mileage rates assigned for peak/off peak time periods. Participants were told that this phase would last exactly three months and that whatever balance was left in their mileage budget would be theirs to keep upon successful completion of the study in January 2004. The other two groups, the CCE and CCC groups, received letters asking them to make their swap and to continue reporting odometer readings for all household vehicles.

Fifteen days into the experiment period, the CEC households that had returned their last swap package were sent their next swap package. In this package, they also were provided with the web site address, logon user ID, and logon password for viewing their vehicle activity statements. Examples of the web site pages are provided in Appendix I.

At this point in the study, it became apparent that expecting the households to swap twice a month might be too difficult. Reminder letters and calls were made to those who were the furthest behind in their rotations. Throughout this three-month period, as CEC CarChips were returned, statements were generated for all previous experiment periods covered by the CarChip data. Households that had not performed any CarChip swaps after installation were dropped from the study.

3.3 Periods 3 through 5 – Second Experiment Period (Original: August-November 2004, Actual November 2004-February 2005)

Swap packages and pricing letters for the CCE group were prepared in August. However, a personnel issue at GeoStats prevented these packages from being mailed. Unfortunately, this was not discovered until early October, when it became obvious that swap packages were no longer coming back from the field. Further investigation revealed that swap packages for all three groups were behind schedule and that the CCE group had not yet started their experiment.

In mid October, all participants in the study were contacted by GeoStats and told that the study was still underway and that new swap packages would be forthcoming. Each household was asked to report the participation incentives received to date so that these payments could all be brought up to the current amount due. Upon return and download of the last CarChips deployed, it was discovered that some data for some household vehicles were lost over the summer months due to the extended deployment period without a download. Further contributing to the data loss was the discovery that the extended memory CarChips had been shipped from the factory with default settings that caused them to log unnecessary engine parameters – which effectively resulted in a logging duration similar to the 75-operating hour CarChips.

After discussions among the project managers at GeoStats, Cambridge Systematics, and Mn/DOT, it was decided to extend the study from the originally scheduled end date in November to a new end date in February that would allow the CCE group to participate in a full three-month experiment period starting on November 6 and ending on February 5. The CCE group would still get the $100 participation incentive paid out over the total duration of the study.

In addition, the remaining CEC participants were asked to continue in the study through February 2005. Those that stayed would receive a total of $120 for study participation. This group was told that a subset of them would be asked to enter an additional, shorter experiment period within the last few months of the study, thus creating three subgroups within the CEC category:

- The CECx subgroup, which ended their participation in November;
- The CECc subgroup, which had an additional control period; and
- The CECe subgroup, which had an additional experiment period.

Mileage budget balances as of the end of the experiment in August were then paid in December. The CCC group was informed that the study was being extended to early February and that they would receive an additional $10 for their participation, bringing their total participation incentive to $60.

Nine CEC households were selected for another pricing experiment (CECe). The remainder in this group (CECc and CECx) received surveys asking them about their experience in the experiment as well as other pay-as-you-go pricing concepts. (See Appendix J.)

Going into the final experiment period in November, there were 41 out of 51 households remaining in the CCE group, 32 out of 48 households remaining in the CEC group, and 27 out of 31 remaining in the CCC group.

3.4 Study End (February 2005)

All households remaining in the study in November 2004 successfully completed the study in February 2005. For the CEC group, swap packages and statements were generated and distributed on a monthly basis to help improve CarChip rotation rates. After all CarChips were returned in February along with a completed exit survey, final participation incentives as well as any remaining mileage balances for the CCE and CECe households were paid. Table 3.1 shows the final completion and dropout numbers by group.

Table 3.1 Final Disposition of Participant Households

Group	Complete	Drop Out
CCC	27	4
CCE	41	10
CECc	23	
CECe	9	14
CECx	2	

3.5 Adequacy of Data for Use in Analysis

With the gap in data retrieval in the middle of the experiment, we were concerned about whether the data would be adequate for use in analyzing behavior patterns. GeoStats evaluated the data available for each household and found the data to be adequate for the study (see Appendix K).

4.0 Experiment Findings

The experimental design provided many different ways to measure the change in mileage due to the pricing experiments. Section 4.1 summarizes the results of the recruit surveys and the exit surveys.

Section 4.2 describes the analysis of the driving data. In that section, several evaluation and comparison methods for analyzing the experiment findings are summarized.

4.1 Survey Analysis

The recruitment survey collected information on respondent characteristics that help us to understand the overall Twin Cities vehicle travel market and to ensure that participants represent the overall market well. The exit surveys of those who agreed to participate in the driving study allowed us to understand how participants reacted to the study and how the pricing affected participants' attitudes toward pay-as-you-drive pricing.

4.1.1 Recruitment Survey

In February 2004, interviewers from MarketLine Research contacted households in the Twin Cities metropolitan area to collect vehicle usage information and to recruit study participants. The survey interview script is shown in Appendix B. Table 4.1 summarizes the survey returns.

Table 4.1 Distribution of Recruitment Survey Outcomes

	Telephone Numbers	
Survey Outcome	Number	Percent
Completed Survey	2,320	14.3%
Refusal (includes initial refusals, terminations, and language problems)	2,291	14.1%
Inactive Number – Unable to contact after maximum number of attempts	1,901	11.7%
Invalid Telephone Number (includes nonworking, nonhousehold, and outside target geography)	6,622	40.8%
Active Telephone Numbers when survey targets were reached	3,091	19.1%
TOTAL	16,225	100%
Response Rate	2,320/(2,320 + 2,281 + 780)	43.1%

Source: MarketLine Recruitment Survey, February 2004.

The survey response rate was 43 percent. This rate is similar to those that we have achieved in other Twin Cities area surveys. The response rate calculation allocates proportions of the 1,901 inactive numbers with unknown dispositions to the refusals category and to the out-of-scope category (invalid telephone number) based on the percentage of numbers with known dispositions. So, of the 1,901 numbers, we expect 40.8 percent ((2,320 + 2,291)/(2,320 + 2,291 + 6,622)), or 780 numbers were actually valid numbers and therefore refusals. The response rate is then 2,320/(2,320 + 2,291 + 780) or 43.1 percent. Active numbers that were still being worked by MarketLine at the end of the survey are not included in this calculation.

Most of the 2,320 willing survey respondents were screened out of participating in the experiment for various reasons, including:

- 222 had an affiliation with a business that might affect their experiment outcomes;
- 131 could not provide the number of licensed drivers in their household, their household's county, their length of residency, or the number of household vehicles;
- 269 either had no vehicles in their household or more than three vehicles in their household;
- 268 had no vehicles newer than model year 1996 that could be outfitted with a CarChip;
- 540 did not drive their vehicle more than 100 miles in the past seven days; and
- 230 expected to change their address, obtain a new vehicle, or retire during the experiment period.

The remaining 660 telephone respondents were asked to participate in the study, and 186 agreed to do so (28 percent).

The cooperating respondents were similar in terms of the questions asked in the first part of the survey to those who declined to participate in the study, and not very different than those who did not qualify for the study. Table 4.2 summarizes the differences in household characteristics between all the survey respondents, those respondents that were not screened out before being asked to participate, and those who agreed to participate. Table 4.3 shows a similar comparison for the travel characteristics questions, and Table 4.4 summarizes the differences in the attitudinal questions asked prior to the invitation to participate in the study.

Table 4.5 summarizes the vehicle ownership characteristics of the qualified respondents who agreed to participate in the experiment and who did not drop out later. These questions were asked only of those respondents who qualified to participate in the study.

Table 4.2 Household Characteristics of Recruitment Survey Respondents

Respondent Characteristics		Respondents: All	Respondents: Qualified for Study	Respondents: Agreeing to Participate
Average number of licensed drivers in household	N	2,098	660	186
	Average	2.02	2.03	2.03
Average number of vehicles in household	N	1,968	660	186
	Average	2.27	2.10	2.14
Average number of years in the Twin Cities	N	1,985	660	186
	Average	31.8	30.1	29.7
Household distribution by county	N	2,066	660	186
– Anoka	Percents	12.4	13.2	15.6
– Carver		2.0	2.3	1.6
– Chisago		0.5	0.9	1.1
– Dakota		19.3	21.8	19.9
– Hennepin		30.8	30.0	33.9
– Ramsey		17.7	15.2	14.5
– Scott		4.9	7.1	4.8
– Washington		8.5	9.5	8.6
– Other/Refused		3.9	0.0	0.0

Source: MarketLine Recruitment Survey, February 2004.

Table 4.3 Travel Characteristics of Recruitment Survey Respondents

Respondent Characteristics		Respondents: All	Respondents: Qualified for Study	Respondents: Agreeing to Participate
Primary mode is private vehicle	N	1,968	660	186
	Percent	97.0	98.9	99.5
Use freeways in traveling around Twin Cities	N	1,968	660	186
	Percent	92.3	94.7	95.2
One or more work or school commuters in household	N	1,968	660	186
	Percent	64.2	67.8	71.5
Commute during the a.m. peak period	N	1,264	459	133
	Percent	79.7	80.6	81.2
Commute during the p.m. peak period	N	1,264	459	133
	Percent	84.5	86.3	87.2
Pay to park at work/school	N	1,264	459	133
	Percent	22.2	23.5	27.1

Source: MarketLine Recruitment Survey, February 2004.

Table 4.4 Attitudes of Recruitment Survey Respondents

Respondent Characteristics		Respondents: All	Respondents: Qualified for Study	Respondents: Agreeing to Participate
Believe congestion has increased in past three years	N	1,968	660	186
	Percent	82.1	83.5	86.0
Believe congestion has decreased in past three years	N	1,968	660	186
	Percent	0.8	0.6	1.1
Believe congestion has stayed same in past three years	N	1,968	660	186
	Percent	14.0	13.9	11.3
Tolerance level for congestion: Rated intolerable (Rating 8-10)	N	1,968	660	186
	Percent	24.5	22.9	26.4
Tolerance level for congestion: Rated tolerable (Rating 1-3)	N	1,968	660	186
	Percent	20.7	17.6	17.8
Convenient public transit: Rated very convenient	N	1,968	660	186
	Percent	25.8	21.1	19.9
Convenient public transit: Rated very inconvenient	N	1,968	660	186
	Percent	28.6	32.9	36.0

Source: MarketLine Recruitment Survey, February 2004.

Table 4.5 Auto Ownership and Demographic Characteristics of Program Participants

Participant Group Code	Workers in Household	Total Vehicles Available	1996 and Newer Vehicles	Actual Number in Experiment Group	Actual Number in Control Group
100	0	1	1	6	0
110	1	1	1	10	1
120	2+	1	1	4	1
211	0, 1	2	2	21	4
212	0, 1	2	1	7	2
221	2+	2	2	8	9
222	2+	2	1	2	2
310	0, 1	3	1-3	9	2
320	2+	3	1-3	10	6
Total				77	27

Source: MarketLine Recruitment Survey, February 2004.

In addition to providing descriptive statistics of participants and nonparticipants to demonstrate reasonable similarities between these groups, we also used these data to build an experiment participation model. This model was a necessary component when we used the matching methods described later. This model is described in Appendix L, and its application in the analysis of the pricing experiments is described in later discussions in this section.

4.1.2 Participant Exit Survey

At the conclusion of the pricing study, all participants were asked to complete a survey that covered how the study was conducted, their behavior during the study, attitudes toward travel, and their assessments of pay-as-you-drive leasing and insurance concepts. The experiment group and control group mail survey instruments are shown in Appendix J.

Experience in the Study

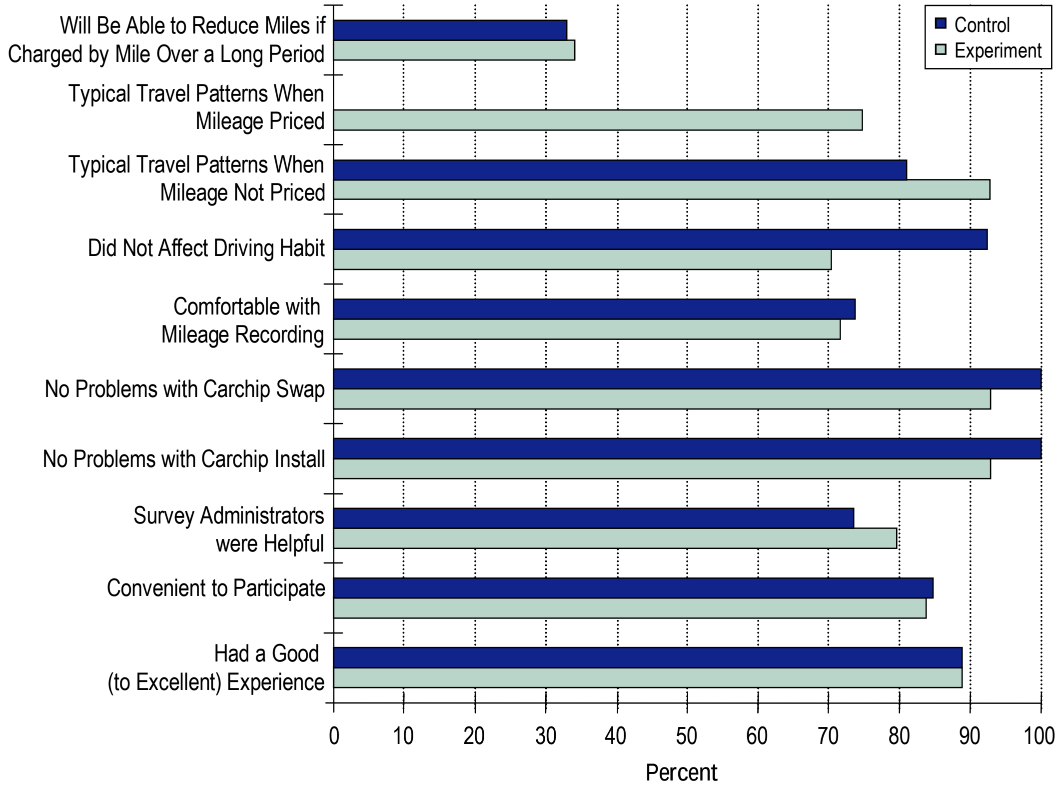
The exit surveys began by asking participants to evaluate their overall experience in the study on a 1 (poor) to 5 (excellent) scale. More than 60 percent of both the control group participants and the experiment participants found the experiment to be “Very Good” (rating 4) or “Excellent” (rating 5). Only 11 percent of each of the groups rated the experience as “Poor” (rating 1) or “Fair” (rating 2). These high ratings would tend to indicate that the logistical problems with the study were resolved to the satisfaction of most participants.

The distribution of responses did not indicate a statistically measurable difference between the responses of the control group and experiment group participants.

Overall Study Experience and Perception of Behavior Change

The survey then asked respondents to provide evaluations of different elements of the study. Figure 4.1 shows the study experience of the control and experiment groups. Among the control group there was no basis for change in travel patterns due to pricing during the study. However, for the experimental group, the percent of respondents who agreed that their travel patterns were typical dropped from 92 percent when not priced to 74 percent when priced. Ninety-three percent of the control group, versus 69 percent of the experiment group, agreed that the study did not affect their driving habits.

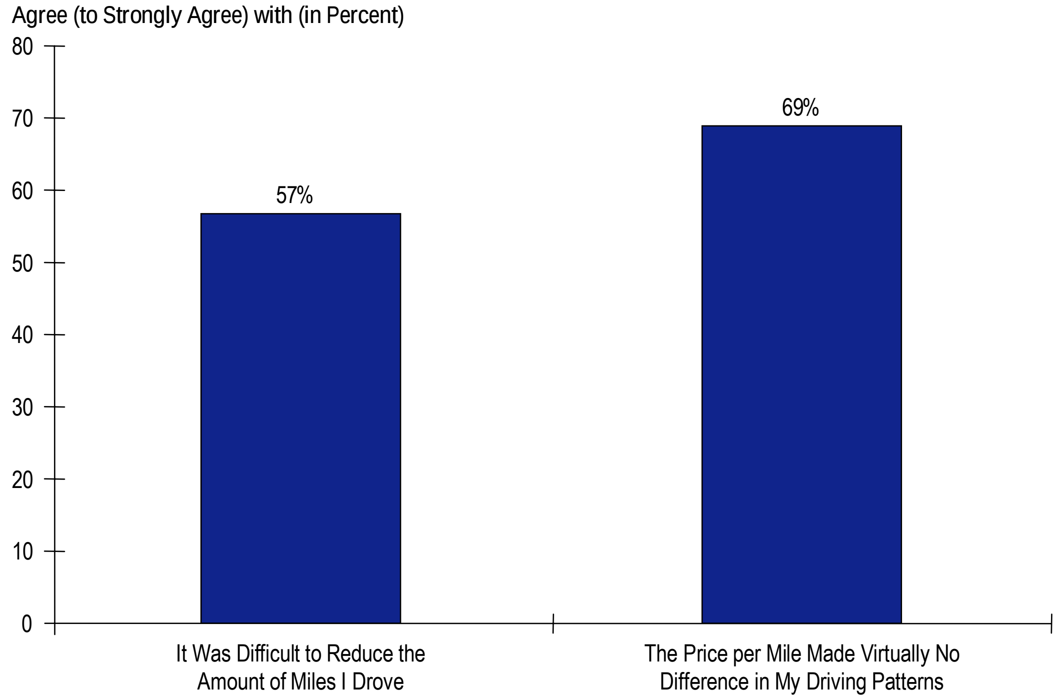
Figure 4.1 Overall Study Experience



Note: "Control" refers to the pure control group only (CCC).

Figure 4.2 reports on participants' perceived ability to reduce miles while priced in the simulation. About 69 percent said that there was virtually no change in driving patterns, and about 57 percent of the participants found it very difficult to limit or reduce the amount of miles that they drove.

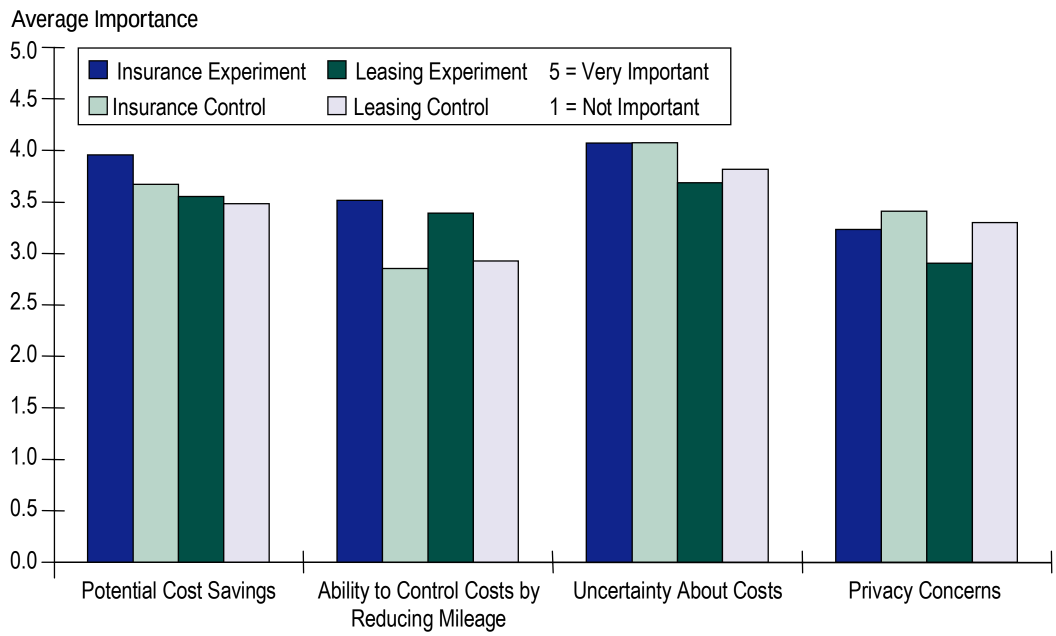
Figure 4.2 Perceived Effect of Pricing on Travel Behavior



Participant Attitudes Towards Pay-As-You-Drive Products

Both experiment and control groups felt that price uncertainty would be an important factor in considering whether to try pay-as-you-drive insurance and leasing (Figure 4.3). Another important factor was the potential cost savings. The control group felt that the ability to control costs by reducing mileage was not as important as the experiment group by a statistically significant margin. Compared to the experiment group, the control group felt that privacy concerns were a more important consideration in their adoption of pay-as-you-drive insurance and leasing.

Figure 4.3 Factors Affecting Participants' Choice of Pay-As-You-Drive Insurance and Leasing



Figures 4.4 and 4.5 show the differences between the control and experiment group respondents to choosing pay-as-you-drive insurance and leasing based on potential features of these products. Exposure to the experiment made respondents more receptive to consider alternate modes of insurance and vehicle purchases. Consistently, the experiment group was more likely to choose pay-as-you-drive insurance and leasing if available. In addition, the experiment group was more likely than the control group to consider pay-as-you-drive insurance and leasing if features such as variable mileage pricing by time of day and yearly audits were offered. An overwhelming majority of the participants said that they were more likely to choose pay-as-you-drive insurance if they could switch to traditional insurance without penalties.

Figure 4.4 Features Influencing the Likelihood of Choosing Pay-As-You-Drive Insurance

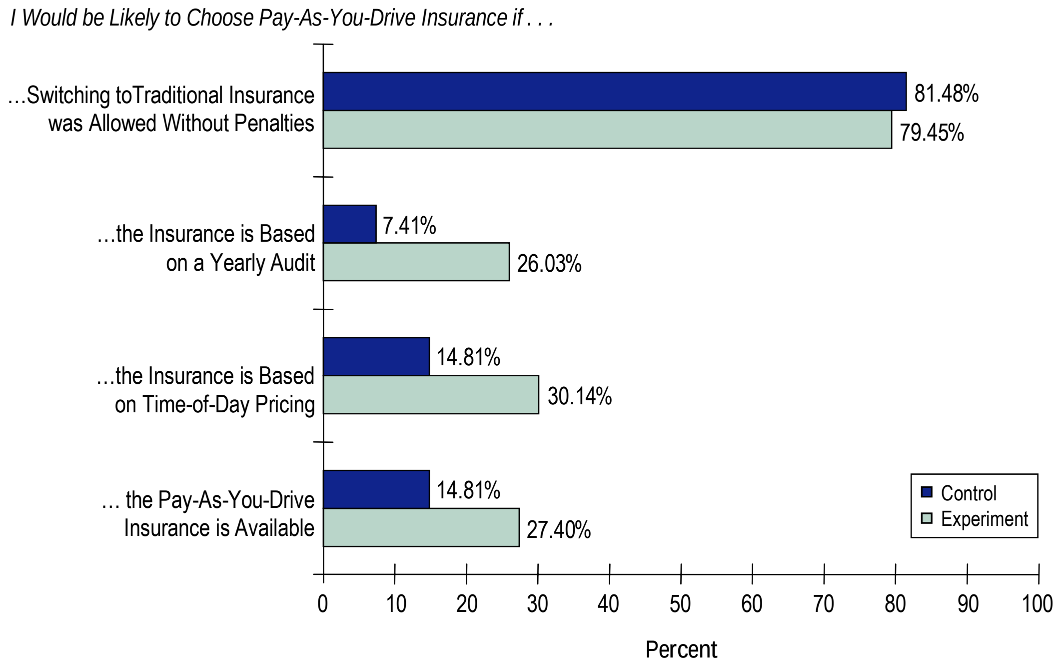
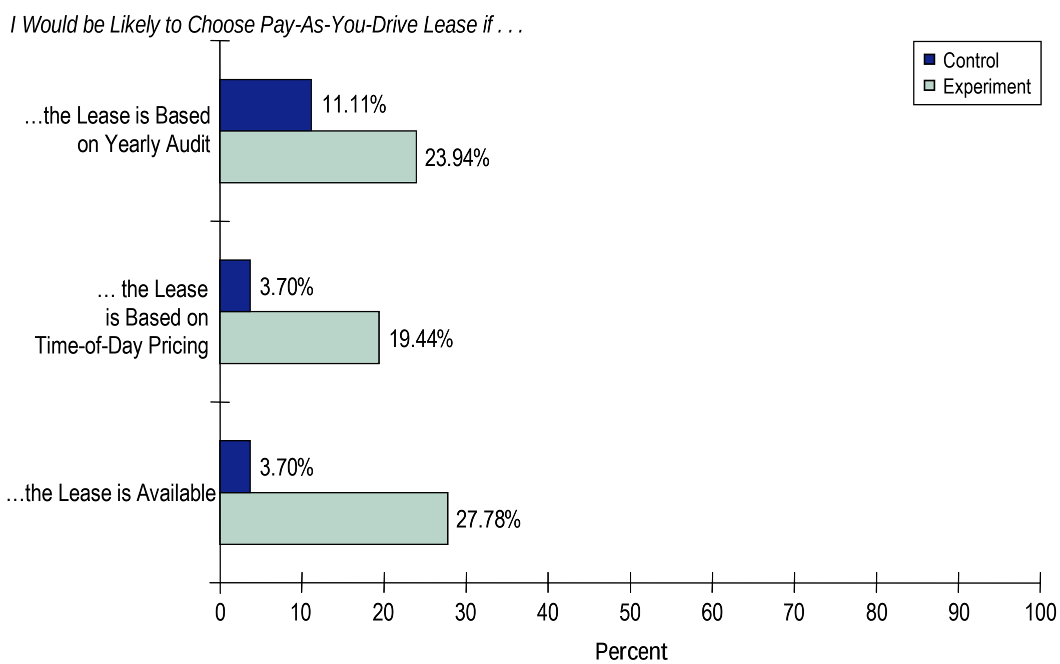


Figure 4.5 Features Influencing the Likelihood of Choosing Pay-As-You-Drive Lease



Conclusions from the Exit Survey

Based on the exit interviews, overwhelming majorities of both the control and experiment group participants felt that it was very difficult to limit or reduce the number of miles they drove or change their travel patterns. Uncertainty about the potential costs and savings, and ability to control costs by reducing mileage were some of the main factors affecting the choice of pay-as-you-drive insurance and leasing.

Another finding from the study was that exposure to the program may be an important factor in making it successful. Consistently, experiment group participants were more willing to consider pay-as-you-drive insurance and lease programs compared to the control group participants. It would therefore seem that a key component to the success of the pay-as-you-drive concept, if it were to be adopted, is education about the concept and possible exposure to the program. Exposure to the concept also made experiment participants more receptive to solutions such as variable mileage pricing by time of day to reduce travel costs and contribute to the overall reduction in congestion. Both groups of participants were willing to consider pay-as-you-drive insurance if they were allowed to switch to traditional insurance products without incurring any penalty.

4.2 ANALYSIS OF DRIVING BEHAVIOR

The most straightforward method for measuring the effects of the experiment was to compare the vehicle mileage of all the vehicles that were being priced to the vehicle mileage of all the vehicles that were not being priced. This aggregate measure of mileage differences was then enhanced by looking at the experiment-versus-control average mileages over the different experiment time periods and for different experimental pricing levels, as well as by examining the change in mileage of every group of participants separately over time. These analyses are described as mileage comparisons by group, and comprise the first analyses presented below.

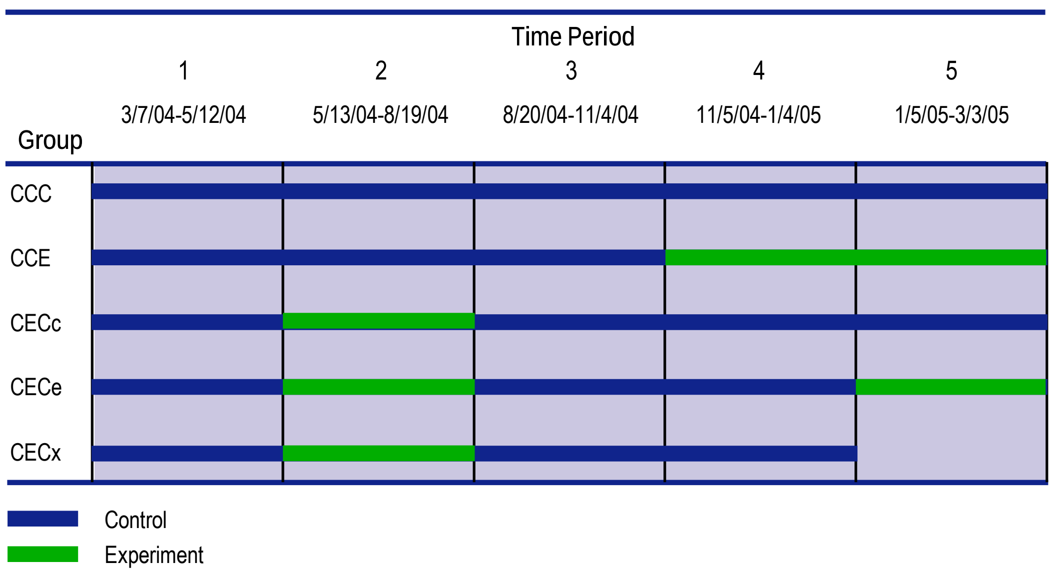
Because the participants represented a fairly diverse group, the differences measured in the straightforward group comparisons may mask behavioral changes that could be influenced by the individual characteristics of the participant households. To account for this issue, we also analyzed the mileage differences for individual vehicles in the experiment. For each vehicle, we compared the average mileage driven under the initial control period with the average mileage driven during the following experimental and control periods. We also sought to explain these mileage differences and the propensity to change mileage in terms of household and vehicle characteristics through the use of regression analysis. These are described in the section "Mileage Comparisons for Individual Vehicles."

Next, we matched the household and vehicle characteristics of the experiment group to those of the control group, compared the mileage differences between them, and calculated the elasticities of miles driven with respect to price. Through the matching method, we sought to account for both the exogenous differences within the groups and the important effects of mileage changes between time periods. Finally, we present our analyses of some issues that seem to affect the different comparisons.

4.2.1 Mileage Comparisons by Group

The pricing study was designed for participants to experience unpriced control periods and experiment periods in which some respondents were subjected to simulated pay-as-you-drive pricing. Figure 4.6 schematically shows the control and pricing period schedules for the different groups of participants.

Figure 4.6 Schedule of Control and Experiment Periods by Study Group

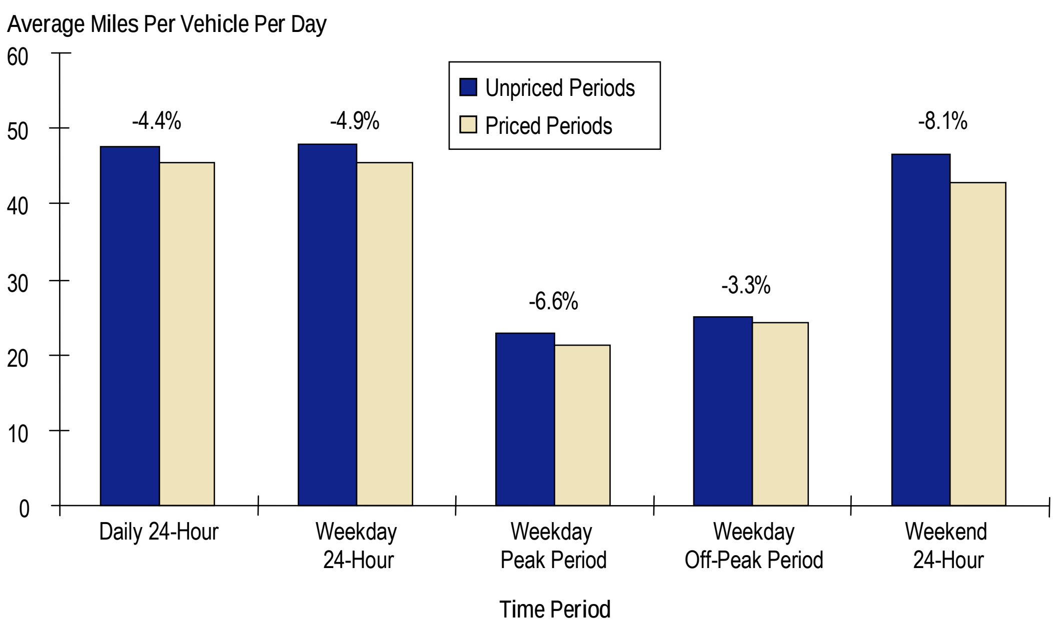


All participants were monitored but not priced during Period 1, the initial budget-setting period control (or "C") period. During Period 2, some participants were introduced to pricing (an experiment, or "E" period), while others continue to be monitored, but not priced (another "C" period). The initial design called for the continuation of a mix of priced and unpriced vehicles for Period 3, but the implementation problems described in Section 3.0 required us to suspend the pricing during this period. Thus, all the participant groups were monitored, but not priced in Period 3, between August 20, 2004 and November 4, 2004. There also were considerable data gaps during this period. During Period 4, we again collected a mix of data, with the CCE group (control-control-experiment) entering the pricing part of the experiment. Period 5 was added to collect additional data to make up for lost data during the summer implementation problems. Those participants who were in an experiment phase during Period 4 (CCE group) continued to be administered the pricing in Period 5. Most of those in the control phase in Period 4 (CEC group) were also asked to continue in control for Period 5 (CECc), but a small number of participants were asked to participate in a second pricing phase with new pricing levels (CECe).

Comparison of Total Miles Driven

The simplest comparison is between total miles driven by the vehicles that were subject to the pricing with those by nonpriced vehicles. This analysis compared the mileage from all the vehicles in the unpriced "C" periods to the mileage from all the priced "E" periods shown in Figure 4.7. Over the course of the study, the average daily mileage of unpriced vehicles was 47.5 miles, compared to an average of 45.4 miles for the priced vehicles (4.4 percent difference). Comparatively larger differences in percentage terms were measured for weekend trips (8.1 percent) and for weekday peak-period trips (6.6 percent). In all the comparison cases, the average mileage during priced periods was lower than for unpriced periods, which may indicate that the pricing had a measurable effect on vehicle-miles traveled.

Figure 4.7 Comparison of Average Miles Driven for Vehicles
Entire Study Timeframe

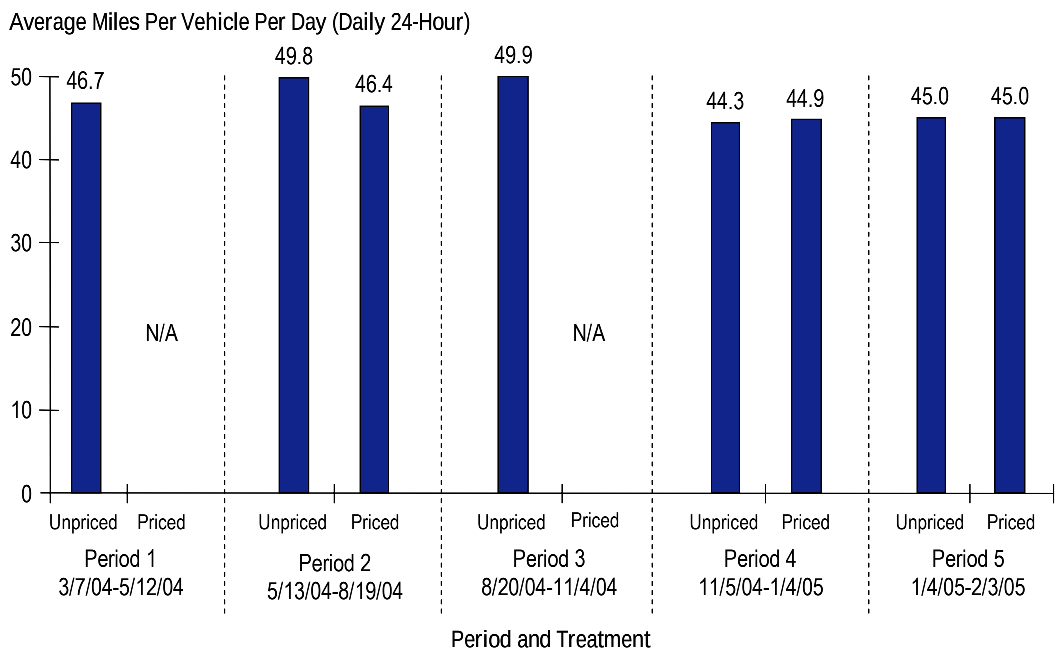


Comparison of Mileage by Calendar Period

This simple analysis may mask several aspects of background variability in travel that should be considered in measuring the efficacy of the pricing in terms of mileage reduction. One complexity is that miles driven naturally vary over the course of the year.

Figure 4.8 shows the differences in average daily miles separately for the five distinct experiment time periods. Time Period 1 was unpriced for all participants. The average daily vehicle mileage for this period was 46.7. During Period 2, the average unpriced mileage increased to 49.8 miles, and priced vehicle mileage was slightly less, at 46.4 miles. In Period 3, there were no pricing data due to the data collection problems, but the average unpriced vehicle had almost the same mileage as the unpriced vehicle in Period 2. During the fourth and fifth periods, there were almost no differences (statistically insignificant) in the priced and unpriced averages. Compared to the seasonal differences for the unpriced vehicles, the differences between the unpriced and priced vehicles within the same time periods are small.

Figure 4.8 Average Miles Per Vehicle Per Day by Calendar Period



Figures 4.9 and 4.10 indicate similar small differences in average priced and unpriced mileages for weekdays and weekend days, respectively, with Period 2 showing larger differences.

Comparison of Mileage by Pricing Level

A second way in which the initial calculation of differences probably oversimplifies the effects of the experiment is that it did not account for pricing levels. The experiment allowed for the measurement of differences under several pricing treatments, albeit with small sample sizes. Figure 4.11 shows the effect of the pricing levels on average mileage for the flat rate pricing treatments, in which vehicles are charged the same amount per mile in the peak and off-peak periods. Figure 4.12 shows the mileage differences for the different time-of-day pricing treatments.

Figure 4.9 Average Weekday Miles Per Vehicle Per Day by Calendar Period

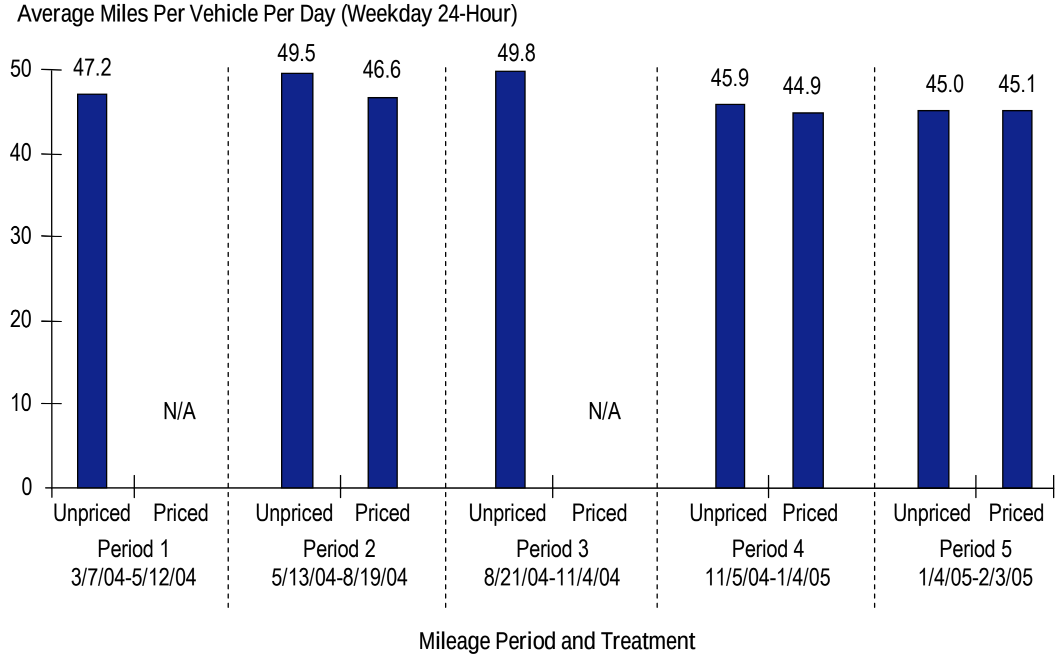


Figure 4.10 Average Weekend Miles Per Vehicle Per Day by Calendar Period

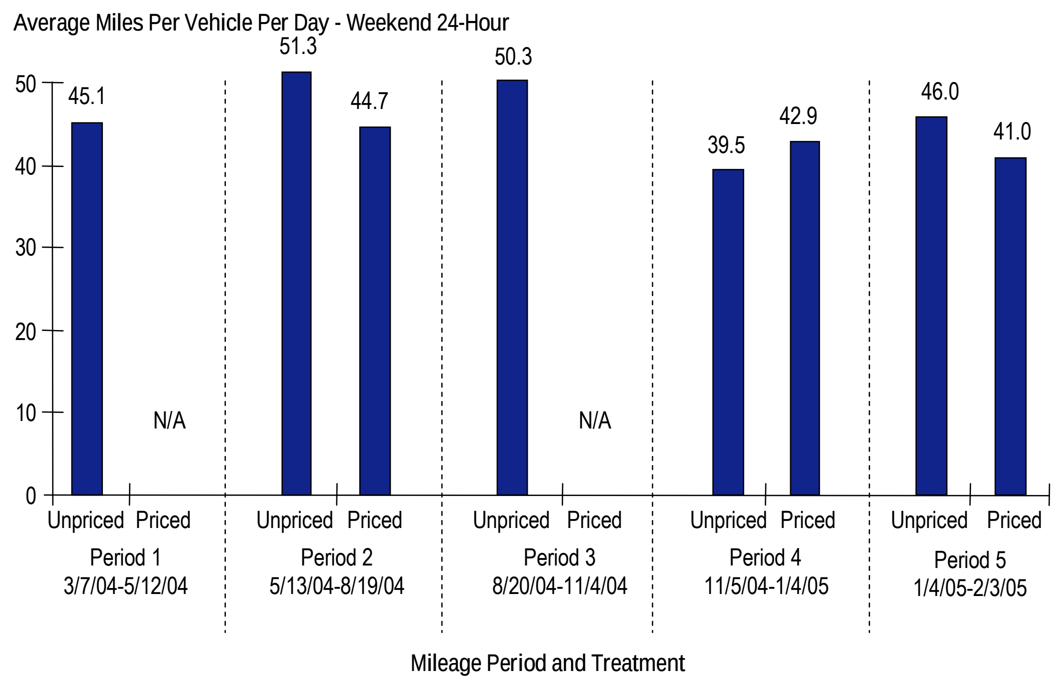


Figure 4.11 Average Miles Per Day for Standard (Flat) Pricing Treatments

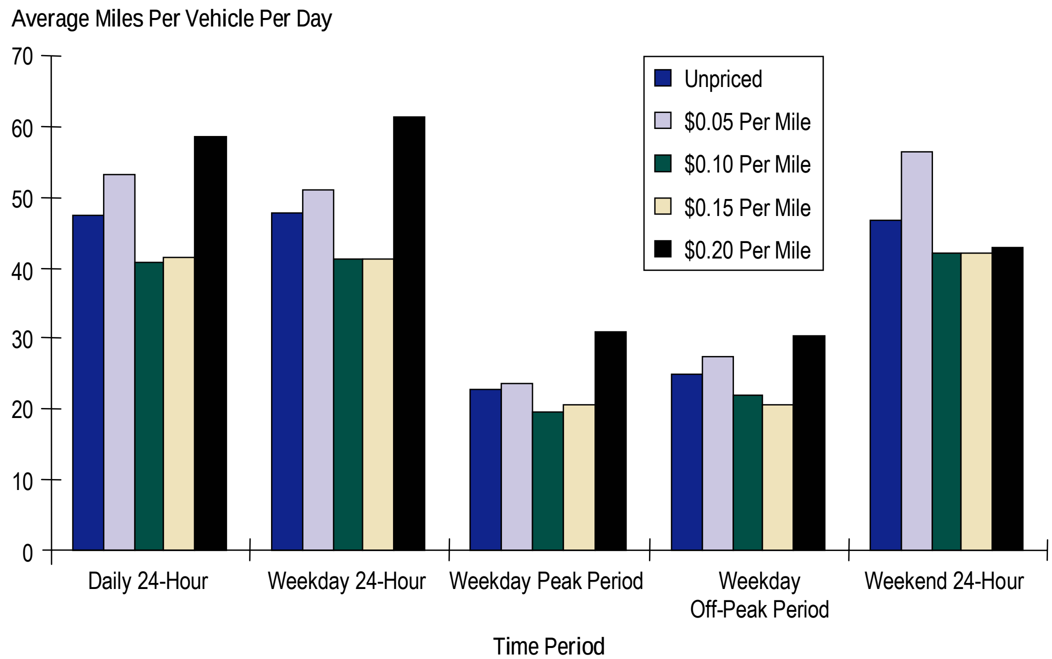
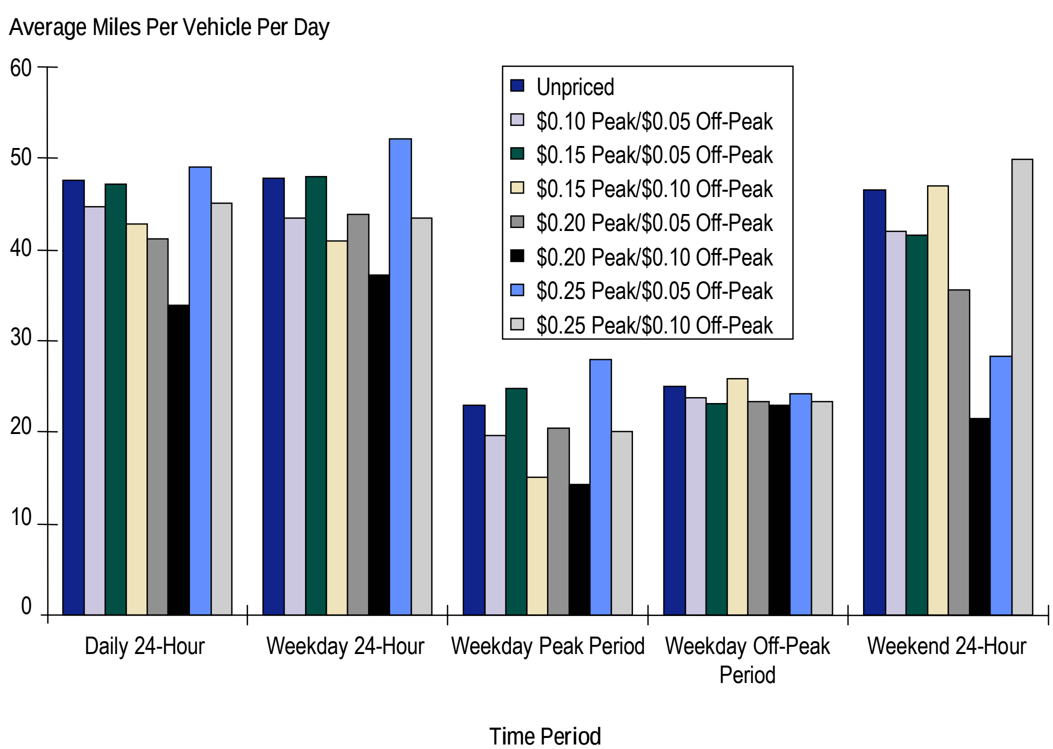


Figure 4.12 Average Miles Per Day for Time-of-Day Pricing Treatments



We would expect the average mileage measures in these figures to decrease as the pricing levels are increased, but the mileage pattern appears to be almost random. Average daily mileage for vehicles being priced at $0.05 per mile and for vehicles being priced at $0.20 per mile on a flat rate basis are 12 percent higher and 23 percent higher, respectively, than the average unpriced mileage. The time-of-day pricing treatments show similar results, with some of the highest pricing levels showing increases in the average daily miles traveled. These curious results are most likely due to the small sample sizes in each pricing category.

Mileage Differences by Participant Group and Calendar Period

Still another way to evaluate the effect of the pricing treatments is to examine every group of participants separately and evaluate their mileage changes over the different calendar periods of the study. Table 4.6 shows the average daily mileage by group and calendar period, and Table 4.7 shows the standard deviation of the average daily mileage. The mileage pattern of the control-only group (CCC) can be used to track mileage changes that are due to seasonality effects only since this group was not subjected to pricing. This pattern shows that people drive more in the summer (Period 2) compared to the spring (Period 1), and then reduce their mileage again in the fall and winter seasons, with the minimum average mileage occurring between the months of November and January. For the other groups, the mileage changes include both a pricing effect and a seasonality effect. The standard deviation of the average daily mileage ranges from 2.9 to 6.4 miles, and the significance of this variation is captured through the statistical analysis described next.

Table 4.6 Average Daily Mileage by Group and Calendar Period

	Time Periods				
Group	3/7/04 to 5/12/04	5/13/04 to 8/19/04	8/20/04 to 11/4/04	11/5/04 to 1/4/05	1/5/05 to 3/3/05
CCC	44.7	51.2	46.0	42.9	44.5
CCE	47.7	48.5	53.5	44.9	46.5
CECc	50.0	47.9	50.8	47.3	45.5
CECe	40.4	42.9	44.7	40.6	38.7

Table 4.7 Standard Deviation of Average Daily Mileage by Group and Calendar Period

	Time Periods				
Group	3/7/04 to 5/12/04	5/13/04 to 8/19/04	8/20/04 to 11/4/04	11/5/04 to 1/4/05	1/5/05 to 3/3/05
CCC	3.6	4.6	3.4	2.9	3.7
CCE	3.6	5.6	4.3	3.0	4.5
CECc	5.7	3.9	5.4	4.3	6.4
CECe	4.6	5.0	5.1	6.0	4.7

Tables 4.8, 4.9, and 4.10 compare every group of participants to the CCC group by time period. Table 4.8 shows the difference between each group's average daily mileage in a given time period and the average daily mileage of the CCC group during the same time period. Table 4.9 shows the standard errors[3] of these differences for the purpose of statistical analysis. Table 4.10 shows the t-statistics corresponding to these differences (obtained by dividing the differences in mileage from Table 4.8 by the standard errors of these differences from Table 4.9). All the t-statistics are less than 1.96 in absolute value, which means that there are no statistically significant differences at the 95 percent level of confidence between a given group (CCE, CECc, or CECe) and the control-only group (CCC) in any time period.

Table 4.8 Column-wise Difference in Average Daily Mileage *Group Mileage Minus CCC Mileage*

	Time Periods				
Group	3/7/04 to 5/12/04	5/13/04 to 8/19/04	8/20/04 to 11/4/04	11/5/04 to 1/4/05	1/5/05 to 3/3/05
CCC					
CCE	3.0	-2.8	7.5	***2.0***	***2.1***
CECc	5.4	***-3.3***	4.8	4.4	1.0
CECe	-4.2	***-8.4***	-1.3	-2.4	***-5.8***

Note: Cell values that are in ***bold and italics*** refer to periods when the corresponding group was subjected to pricing.

[3] The standard error of a numerical estimate of a characteristic is a measure of the uncertainty associated with the estimate due to sampling.

Table 4.9 Standard Error of Column-wise Difference in Average Daily Mileage
Group Mileage Minus CCC Mileage

	Time Periods				
Group	3/7/04 to 5/12/04	5/13/04 to 8/19/04	8/20/04 to 11/4/04	11/5/04 to 1/4/05	1/5/05 to 3/3/05
CCC					
CCE	5.1	7.3	5.5	***4.2***	***5.8***
CECc	6.8	***6.1***	6.4	5.2	7.4
CECe	5.9	***6.8***	6.1	6.7	***6.0***

Note: Cell values that are in ***bold and italics*** refer to periods when the corresponding group was subjected to pricing.

Table 4.10 T-statistics of the Differences in Average Daily Mileage

	Time Periods				
Group	3/7/04 to 5/12/04	5/13/04 to 8/19/04	8/20/04 to 11/4/04	11/5/04 to 1/4/05	1/5/05 to 3/3/05
CCC					
CCE	0.6	-0.4	1.4	***0.5***	***0.4***
CECc	0.8	***-0.6***	0.8	0.8	0.1
CECe	-0.7	***-1.2***	-0.2	-0.4	***-1.0***

Note: Cell values that are in ***bold and italics*** refer to periods when the corresponding group was subjected to pricing.

Table 4.11 shows the difference between average daily mileage for a study group in a given time period (Periods 2 to 5) and the average daily mileage of that group in Period 1. Except for the CCC group, this difference consists of a seasonality effect and a price effect. To compute the price effect, we net out the seasonality effect from this difference as shown in Table 4.12. This is illustrated through an example. For the CECc group, Table 4.11 shows that the difference in average daily mileage between Periods 1 and 2 is -2.2 miles (a decrease in mileage in Period 2). Between these two time periods, the CCC group increased their average daily mileage by 6.6 miles, which can be considered as the seasonality effect for the CECc group also (as shown previously, there is no statistical significance between these two groups). Therefore, the true price effect for the CECc group in Period 2 is the total change in mileage minus the seasonality effect, i.e., -2.2 - 6.6 = -8.7 miles (rounded). Thus, on average, the CECc group decreased their average daily mileage by 8.7 miles when they were subjected to pricing.

Table 4.11 Total Row-wise Group Difference from Period 1
Seasonality Effect Plus Price Effect

	Time Periods				
Group	3/7/04 to 5/12/04	5/13/04 to 8/19/04	8/20/04 to 11/4/04	11/5/04 to 1/4/05	1/5/05 to 3/3/05
CCC	N/A	6.6	1.3	-1.7	-0.2
CCE	N/A	0.8	5.8	*-2.7*	*-1.1*
CECc	N/A	*-2.2*	0.8	-2.7	-4.6
CECe	N/A	*2.4*	4.2	0.1	*-1.8*

Note Cell values that are in ***bold and italics*** refer to periods when the corresponding group was subjected to pricing.

Table 4.12 Row-wise Group Difference from Period 1
Netting out Seasonality Effect

	Time Periods				
Group	3/7/04 to 5/12/04	5/13/04 to 8/19/04	8/20/04 to 11/4/04	11/5/04 to 1/4/05	1/5/05 to 3/3/05
CCC	N/A	N/A	N/A	N/A	N/A
CCE	N/A	-5.8	4.5	*-1.0*	*-0.9*
CECc	N/A	*-8.7*	-0.6	-1.0	-4.4
CECe	N/A	*-4.2*	2.9	1.8	*-1.6*

Note: Cell values that are in ***bold and italics*** refer to periods when the corresponding group was subjected to pricing.

Table 4.12 shows that the pattern of mileage changes due to the pricing makes sense in general. All groups decrease their average mileage during the periods when they are priced. The CCE group reduce their mileage by one mile per day in Period 4 and 0.9 mile per day in Period 5 (priced periods for CCE), but surprisingly decrease their mileage by 5.8 miles in Period 2 (unpriced period for CCE). The CECc group reduce their mileage by 8.7 miles during Period 2 which is priced, and the CECe group reduce their mileage by 4.2 miles in Period 2 and 1.6 miles in Period 5 (priced periods for CECe).

Based on the group analysis, the CEC groups seem to be more responsive to the pricing treatments than the CCE group. The data quality problems that were encountered during the summer period may be partially responsible for this difference, but because of the extensive data cleaning that was performed it is our conclusion that the differences between these groups are valid. It is likely that the ability to reduce travel is seasonal, with a greater percentage of discretionary trips in the summer. One would assume that these discretionary trips are more likely to be foregone with the pricing incentive in effect. It may also be the case that some of the reduction in driving during the warmer months can be attributed to alternative transportation which might be considered by many to be a more reasonable option during that time of year, since warmer weather and longer daylight generally improve walking, cycling, and transit waiting conditions.

4.2.2 Mileage Comparisons for Individual Vehicles

The group analyses described above measured aggregate differences between the different groups. A second way to consider the mileage effects of the pricing is to evaluate the mileage differences for each vehicle in the experiment individually.

Figures 4.13 through 4.15 show the average miles for each vehicle in the experiment by calendar period. Figure 4.13 shows the mileage for the all-unpriced CCC group. Figure 4.14 shows the differences for vehicles assigned to the "CCE" group. These vehicles were not priced in Periods 1 through 3, but were priced in Periods 4 and 5. Figure 4.15 includes the differences for the "CECc" and "CECe" groups. The "CECc" group vehicles were priced in Period 2. The "CECe" vehicles were priced in Periods 2 and 5.

We calculated the means and standard deviations of the mileage differences for the control and experiment combinations. Figure 4.16 shows the confidence intervals for the mileage difference estimates for the different time periods. The vehicles that were in the control phase in Period 2 increased their mileage compared with their initial control period by a statistically significant amount. Those who went into the experiment phase during Period 2 had on average a slight decrease in mileage. Thus, as the previous analysis had determined, the differences between the Period 2 control and experiment groups are significant. The statistical t-score was 1.95, representing a 93 percent confidence level.

For the other experimental periods – Periods 4 and 5 – the differences were found to be more minor and statistically insignificant.

Figure 4.13 Average Daily Mileage for Experiment Participants by Mileage Period
Control Group

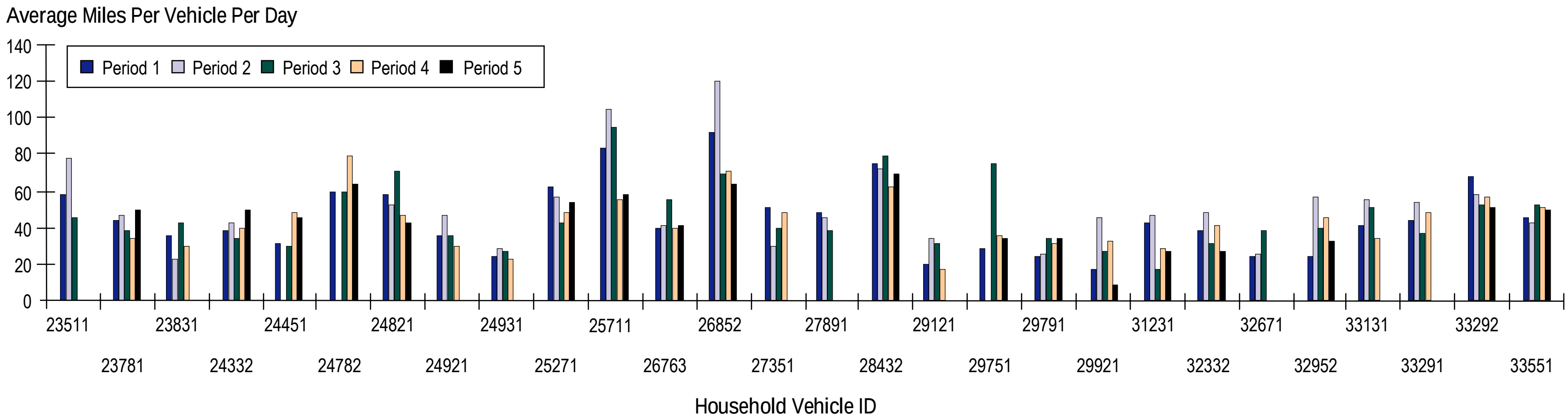


Figure 4.14 Average Daily Mileage for Experiment Participants by Mileage Period
"CCE" Group

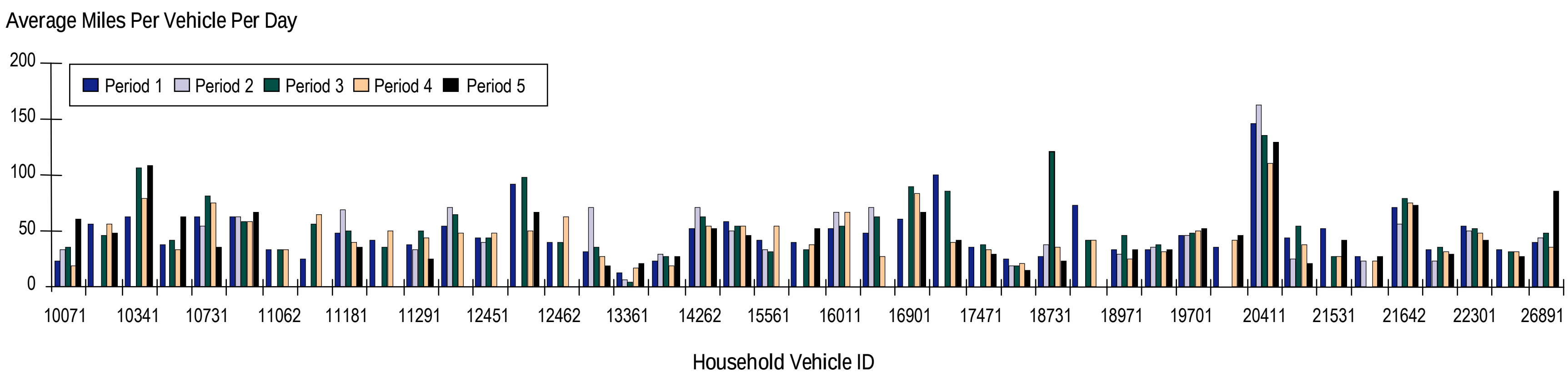


Figure 4.15 Average Daily Mileage for Experiment Participants by Mileage Period
"CEC" Group

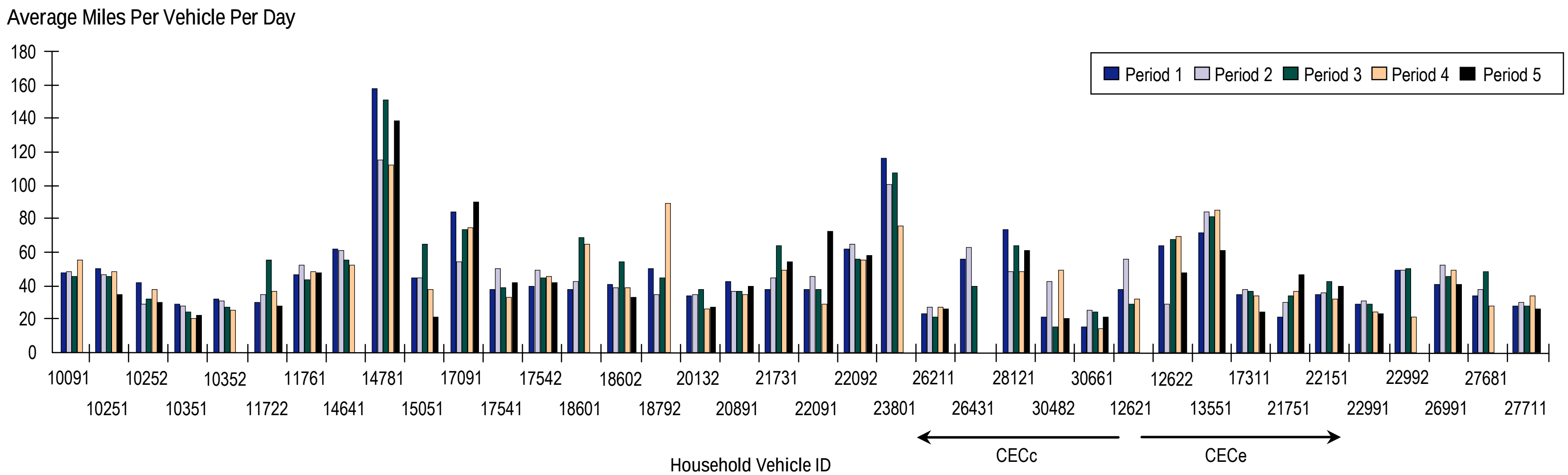
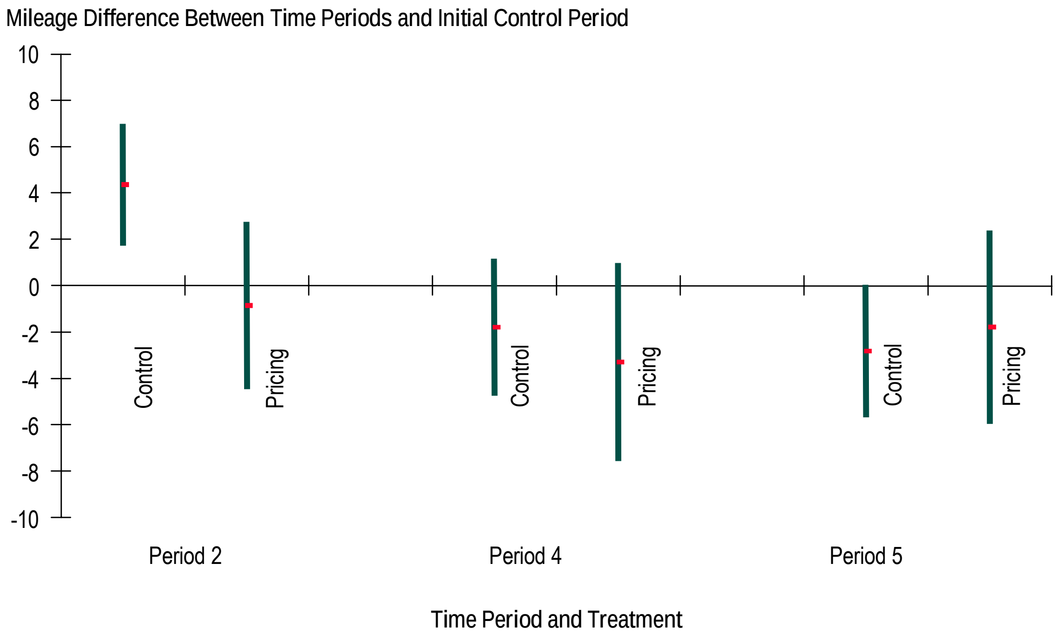


Figure 4.16 Confidence Intervals for Mileage Differences between Experimental Time Periods and the Initial Control Period



Regression-Based Mileage Comparisons

We modeled the effect of the prices on driving behavior at a disaggregate level through regression analysis. For this analysis, we examined all vehicles that were in treatment during Periods 2, 4, and 5. For every vehicle in treatment, we defined the reduction in mileage as the vehicle's daily mileage during the treatment period minus its daily mileage during the first control period (Period 1). Then we tried to relate the reduction in average daily mileage to the peak and off-peak charges, time period of the experiment, vehicle characteristics (such as the level of comfort), and socioeconomic characteristics of the household (such as age, income, vehicle availability, and attitudes towards driving in general and its associated cost). The vehicle and socioeconomic characteristics were obtained from the recruit and exit surveys, in addition to the experiment databases.

Following Train's approach,[4] a selectivity variable that accounts for participation bias was introduced into the regression so that the coefficient estimates are not biased by differences between participants and nonparticipants. The selectivity variable is a function of the probability of participation of a household in the experiment. For this purpose, a participation model was developed and is

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[4] Train, K. (1986). *Qualitative Choice Analysis: Theory Econometrics, and an Application to Automobile Demand.* The MIT Press.

described in Appendix K. Note that the selectivity variable does not get included during the application stage and can be construed as a statistical tool to account for participation bias.

Table 4.13 shows the results of the regression, where the dependent variable is the treatment period average daily mileage minus the average daily mileage in Period 1. Every observation used in the regression corresponds to a priced vehicle. The table shows the parameter estimates corresponding to the variables listed in the table as well as the t-statistics.[5]

Table 4.13 Regression Results Using the Selectivity Correction Approach

Variable	Coefficient Estimate	t-statistic
Intercept	12.97	1.21
Peak price = 0.10	-2.90	-0.51
Peak price = 0.15	-5.32	-0.90
Peak price ≥ 0.2	-8.12	-1.35
Vehicle priced in Period 4	-1.87	-0.45
Vehicle priced in Period 5	-1.14	-0.27
Selectivity correction[a]	2.52	0.89
Presence of unpriced vehicles in household	-2.28	-0.57
Shared car(s) in hh	0.03	0.01
Leased car(s) in hh	-13.96	-2.72
Age of head of household above 65	7.43	1.48
Strongly agree that: "I like driving whenever and wherever I like without worrying about the cost"	-1.92	-0.60
Strongly agree that: "I actively think about ways to reduce my auto operating and ownership costs"	-0.33	-0.10

[a] The selectivity correction factor is defined as:
$[(1-P(\text{participation})) * \log_e(1- P(\text{participation}))]/P(\text{participation}) + \log_e(P(\text{participation}))]$.

The following conclusions can be drawn from this regression:

- The negative coefficients of the peak price variables indicate that relative to a base peak price of $0.05 per mile, pricing at higher rates causes households to reduce their driving of the priced vehicle(s). Furthermore, the higher the peak price is, the higher is the reduction in average daily mileage. The reasonable relative values of these model coefficients are particularly inter-

[5] A t-statistic greater than or equal to 1.96 in absolute value indicates a variable that is statistically significantly different from zero at the 95 percent level of confidence.

esting, because the comparison of the experiment and control groups as entire units did not reveal this pattern. The regression model helps to isolate the effects of the individual explanatory variables.

- The coefficients of the period variables indicate that, with everything else the same, there was overall more reduction in mileage during Periods 4 and 5 than during Period 2. This finding is also contrary to the conclusions of the analyses of the groups. When other factors are accounted for in the regression, the seeming large group wide differences between periods 2 and the two later experimental periods are diminished (the low t-statistics on these coefficients indicate that there is not likely much difference between the periods in terms of explaining differences in individual vehicle mileage). As with the pricing variables, the regression findings for the period variables seem to be quite reasonable.
- The coefficient of "unpriced vehicles in hh" is negative, which may indicate a substitution effect between vehicles available to the household; if one or more unpriced vehicles are available, the household can shift some of the driving from the priced to the unpriced vehicle(s).
- If one or more vehicles in the household are shared among household members, the prices do not affect the mileage (mileage slightly increases) possibly because of the difficulty in coordinating the driving of the priced and shared vehicle(s). However, this effect is quite small.
- If one or more vehicles in the household are leased, the household is much more likely to reduce driving on their priced vehicle. This effect is strong and makes sense because households that already are used to leasing autos are more aware of the associated costs of traveling more miles.
- If the head of household is more than 65 years old, the household is more likely to increase driving. Pricing does not seem to reduce the driving of those households because of the mobility needs of senior people, or simply because those households were less willing to change their travel patterns than others.
- The attitudinal variables included from the exit survey indicate that people who like to drive without worrying about the costs actually decrease their mileage to a surprising extent; moreover, and less surprisingly people who actively think about reducing their auto ownership and operating costs also reduce their mileage.

4.2.3 Mileage Comparisons Using Matching Methods

Matching is a common method that is used to evaluate the impact of a treatment. This section defines the method of matching and describes how it has been applied to evaluate the pricing experiment.

What Is Matching?

The method of matching computes the mean effect of a treatment by matching the units (households) in the treatment sample to other nontreated units in a comparison sample and then computing the change in outcomes (mileage) between the matched units. A unit in a treatment group can be matched to one or more units in the comparison (nontreated) group based on similar observed characteristics or on similar probabilities of participation in the program. The basic assumptions used in matching are that 1) individuals do not enter the program on the basis of gains unobserved by analysts. In other words, it is assumed that the factors that drive participation are observable characteristics of the individual/household, and 2) both treated and nontreated units are available with the same (or similar) observed characteristics X over which the effect of the treatment is to be measured. Given these assumptions, selectivity bias can be removed if one matches units with similar observed characteristics or similar probabilities of participation.

Different matching methods exist, including:

- **Nearest Neighbor Matching –** This involves matching a unit in treatment to one (or more) unit(s) in the comparison group with the closest observed characteristics (or closest probability of participation).
- **Caliper Matching –** This involves matching a unit in treatment to all units in the comparison group where the difference between the observed characteristics is less than a certain caliper (threshold).
- **Kernel Matching –** This involves using some or all of the comparison group members to form a match to a unit in treatment by using Kernel weights[6] applied to the comparison group.

We experimented using the nearest neighbor matching method (matching every treatment household to one comparison household or to five comparison households based on the probability of participation) and the Kernel matching method (including some form of caliper matching). Unfortunately, the estimated savings in mileage were sensitive to the matching method used. Due to the limited size of the comparison group, we have chosen to use the Kernel matching method so that we can use all observations in the comparison group as a match to a household in treatment.

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[6] A Kernel weight is a constant or a function multiplied by some function of the difference in observed characteristics or probability of participation between an individual in the comparison group and an individual in the treatment group.

Application to the Pricing Experiment

Applying the matching method to the pricing experiment involves three procedures: 1) developing a participation model; 2) doing the match; and 3) estimating a model of mileage reduction/elasticity.

Matching the probability of participation reduces the problem of matching to a scalar (one value) instead of matching on a set of observed characteristics. To obtain the probability of participation, the participation model must include all variables that are likely to influence participation. The participation model is described in Appendix L.

Each of the other two components (doing the match and estimating a model of mileage reduction/elasticity) is described below.

Matching Treatment to Control

The next step after estimating the participation model is to match every household in a treatment group to one or more households in a nontreatment group, such as the experimental control group or a group of eligible nonparticipants. Since mileage data are not available for nonparticipants, we use the experimental control groups as the comparison group from which the matches are drawn.

Since we have three treatment samples corresponding to the Periods 2, 4, and 5, we did the matching separately for each of those three time periods. For each of these three cases, the comparison group is all households that are in control during that time period. Matching treatment to control group members in one time period ensures that when comparing the treatment to the control group mileage, there is no issue of seasonality effects.

The matching was done based on two criteria: probability of participation (which was a substitute for a set of observed characteristics) and mileage during the initial control period. In other words, for every household in a treatment group, we formed a weighted match from the comparison group by assigning a weight to every household of the comparison group so that:

- The weighted probability of participation of the comparison group is equal to the probability of participation of the household in the treatment group;
- The weighted average daily mileage of the comparison group in the first time period is equal to the average daily mileage (in the first time period) of the household in the treatment group; and
- The sum of the weights assigned to all members of the comparison group is 1.0.

The mathematics of this matching method are given in Appendix M.

Table 4.14 shows the reduction in mileage (obtained after doing the matching) for all households that were in treatment.

Table 4.14 Matching Results

HHID	VEHID	Period	Trip Peak Charge	Trip Off-Peak Charge	Comparison Mileage	Treatment Mileage	Reduction in Mileage
1034	1	5	0.10	0.05	53.90	107.73	-53.83
2689	1	5	0.15	0.15	39.54	85.08	-45.54
1007	1	5	0.05	0.05	26.11	61.02	-34.91
1113	2	4	0.10	0.10	32.15	64.81	-32.66
1034	1	4	0.10	0.05	48.48	78.70	-30.22
2175	1	5	0.15	0.10	21.71	46.61	-24.90
2164	2	4	0.20	0.20	51.35	75.93	-24.58
1073	1	4	0.10	0.05	52.24	74.33	-22.09
1246	2	4	0.25	0.05	41.25	62.03	-20.79
1046	2	5	0.15	0.10	42.94	62.59	-19.65
1690	1	4	0.05	0.05	64.74	83.21	-18.47
1690	1	5	0.05	0.05	48.43	66.02	-17.59
1262	1	2	0.10	0.05	38.48	55.66	-17.18
2164	2	5	0.20	0.20	59.75	73.78	-14.03
1556	1	4	0.20	0.20	42.02	54.04	-12.01
1590	1	5	0.10	0.05	39.38	51.33	-11.95
2001	1	5	0.25	0.10	33.83	45.74	-11.91
1078	1	5	0.10	0.10	54.50	66.14	-11.63
1970	1	5	0.10	0.10	41.03	52.30	-11.26
1426	2	4	0.05	0.05	43.04	53.88	-10.84
2001	1	4	0.25	0.10	31.11	40.65	-9.54
1860	1	2	0.15	0.10	33.39	42.44	-9.05
2768	1	2	0.15	0.10	29.16	37.62	-8.46
1601	1	4	0.15	0.15	57.38	65.71	-8.34
2215	1	5	0.10	0.10	30.93	39.26	-8.33
1078	1	4	0.10	0.10	50.98	58.75	-7.77
2699	1	2	0.20	0.20	44.26	51.85	-7.60
1754	1	2	0.15	0.10	42.54	49.84	-7.31
2209	1	2	0.10	0.05	38.74	45.59	-6.85
1128	1	4	0.25	0.10	42.88	49.19	-6.31
1754	2	2	0.15	0.10	43.60	49.46	-5.86
1970	1	4	0.10	0.10	44.45	50.28	-5.83
1426	2	5	0.05	0.05	46.16	51.88	-5.72
1129	1	4	0.10	0.10	39.12	44.75	-5.62
1106	2	4	0.10	0.10	27.38	32.92	-5.54

HHID	VEHID	Period	Trip Peak Charge	Trip Off-Peak Charge	Comparison Mileage	Treatment Mileage	Reduction in Mileage
1245	1	4	0.10	0.10	41.74	47.02	-5.28
1897	1	5	0.15	0.10	27.47	32.57	-5.10
1747	1	4	0.10	0.10	27.83	32.49	-4.66
1873	1	4	0.05	0.05	30.43	35.02	-4.58
1936	2	5	0.25	0.05	29.97	33.97	-4.00
2209	2	2	0.10	0.05	61.51	65.14	-3.63
1010	1	4	0.20	0.20	53.04	56.24	-3.20
1246	1	5	0.25	0.05	63.96	66.86	-2.89
1936	2	4	0.25	0.05	28.91	31.57	-2.66
2643	1	2	0.15	0.05	60.45	62.92	-2.47
2699	1	5	0.15	0.15	38.88	40.86	-1.98
2013	2	2	0.10	0.10	33.13	34.98	-1.86
1009	1	2	0.15	0.15	46.86	48.56	-1.70
2215	1	2	0.05	0.05	34.42	36.04	-1.62
2173	1	2	0.15	0.05	43.02	44.43	-1.41
1336	2	5	0.10	0.05	27.02	27.62	-0.60
2194	1	4	0.15	0.05	31.60	31.19	0.41
1010	1	5	0.20	0.20	49.51	47.68	1.84
1590	1	4	0.10	0.05	39.64	37.63	2.01
1897	1	4	0.15	0.10	28.10	25.93	2.17
2299	1	5	0.10	0.05	26.94	23.67	3.27
2771	1	2	0.10	0.10	34.41	30.06	4.35
1046	2	4	0.15	0.10	38.00	33.20	4.80
2158	1	5	0.15	0.15	31.03	26.21	4.82
1204	1	4	0.05	0.05	52.60	47.65	4.95
1860	2	2	0.15	0.10	43.42	38.39	5.03
2689	1	4	0.15	0.15	40.12	34.91	5.20
2149	1	4	0.10	0.10	42.64	36.71	5.93
1262	2	5	0.15	0.10	53.90	47.71	6.19
2194	1	5	0.15	0.05	34.51	28.14	6.36
1549	2	5	0.10	0.10	51.62	44.80	6.82
1118	1	5	0.15	0.15	42.69	35.85	6.83
2230	1	4	0.15	0.10	55.00	48.16	6.84
2299	1	2	0.20	0.10	37.99	31.01	6.98
2284	1	4	0.10	0.10	38.04	31.05	6.99
2230	1	5	0.15	0.10	48.20	41.04	7.16

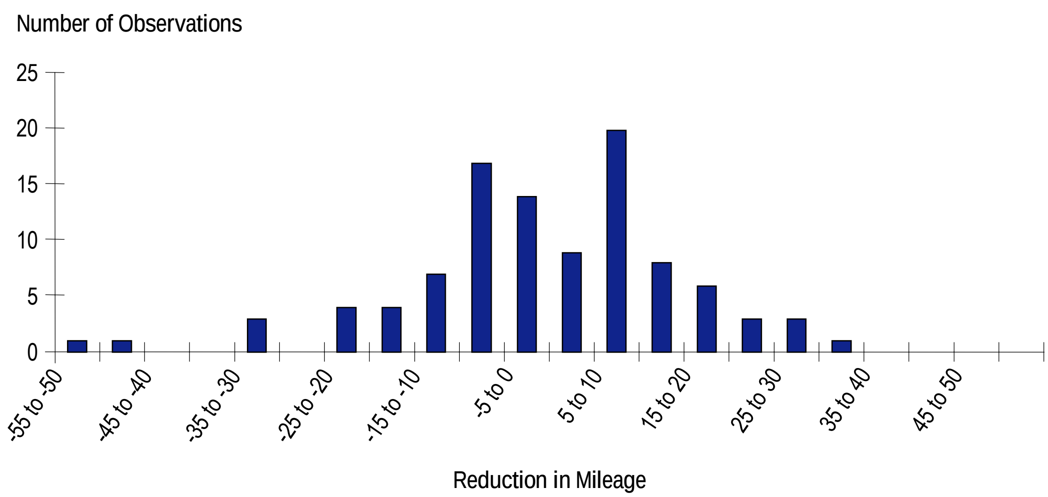
HHID	VEHID	Period	Trip Peak Charge	Trip Off-Peak Charge	Comparison Mileage	Treatment Mileage	Reduction in Mileage
2621	1	2	0.15	0.15	34.58	26.99	7.59
2771	1	5	0.15	0.15	33.76	26.03	7.73
1549	2	4	0.10	0.10	61.37	53.33	8.04
2158	1	4	0.15	0.15	30.84	22.36	8.48
1035	1	2	0.15	0.15	36.50	27.87	8.63
1873	1	5	0.05	0.05	31.94	22.89	9.05
1874	1	4	0.15	0.05	51.04	41.69	9.35
2284	1	5	0.10	0.10	36.03	26.59	9.44
1172	2	2	0.10	0.10	45.09	35.30	9.79
1025	2	2	0.20	0.05	40.07	28.57	11.49
2299	2	2	0.20	0.10	60.98	49.41	11.57
1276	1	4	0.20	0.20	38.24	26.66	11.58
1007	1	4	0.05	0.05	31.30	19.67	11.63
1747	1	5	0.10	0.10	41.46	29.62	11.85
1336	2	4	0.10	0.05	30.78	18.11	12.67
1844	1	4	0.10	0.10	33.00	20.01	12.99
1025	1	2	0.20	0.05	60.33	46.18	14.15
1844	1	5	0.10	0.10	29.61	14.60	15.01
1118	1	4	0.15	0.15	55.42	40.35	15.07
1129	1	5	0.10	0.10	40.68	25.03	15.65
2149	1	5	0.10	0.10	39.20	21.67	17.53
1276	1	5	0.20	0.20	37.40	18.97	18.42
1246	1	4	0.25	0.05	69.72	50.37	19.35
1073	1	5	0.10	0.05	55.33	35.22	20.11
2153	1	5	0.15	0.05	64.93	41.92	23.01
1658	1	4	0.10	0.05	51.47	27.81	23.66
1879	2	2	0.10	0.05	61.56	34.57	26.99
1262	2	2	0.10	0.05	56.56	28.60	27.96
1035	2	2	0.15	0.15	59.45	30.64	28.82
2153	1	4	0.15	0.05	58.94	26.05	32.89

The table shows the following variables:

- Household ID;
- Vehicle ID;
- The period during which the household was in treatment;
- The average daily mileage that the household's vehicle had while in treatment (labeled as "Treatment mileage");
- The average daily mileage of the matched comparison group in that period labeled as "Comparison mileage" (which represents the expected mileage for the household's vehicle had it been in control during that period);
- The reduction in mileage for that household's vehicle (which is equal to treatment mileage minus comparison mileage); and
- The trip peak and off-peak charges that the vehicle was subjected to while in treatment.

Figure 4.17 shows the distribution of reduction in mileage as computed in Table 4.14. Positive values indicate a reduction in mileage when subjected to pricing, while negative values indicate an increase in mileage when subjected to pricing. While many participants reduced their mileage as expected, several others increased their mileage when subjected to pricing.

Figure 4.17 Distribution of Reduction in Mileage due to Pricing



Note: Positive numbers indicate a reduction in mileage. Negative numbers are negative reductions, or an increase.

Regression Analysis of Matching

Since the experiment involved multiple treatments (different price levels by time of day) and the sample size per treatment is small, the average reduction in mileage for every treatment level might not be a reliable indicator of the effect of the treatment. Therefore, we developed a regression to relate the reduction in mileage to the prices that the households were subjected to, using all vehicles that were in treatment (shown previously in Table 4.14).

Table 4.15 shows the results of this regression. The dependent variable is the reduction in mileage, and the independent variables are the different combinations of peak and off-peak charges. All peak and off-peak price combinations are included except for a peak price of $0.05 and an off-peak price of $0.05. The regression results, also shown graphically in Figure 4.18, can be interpreted as follows:

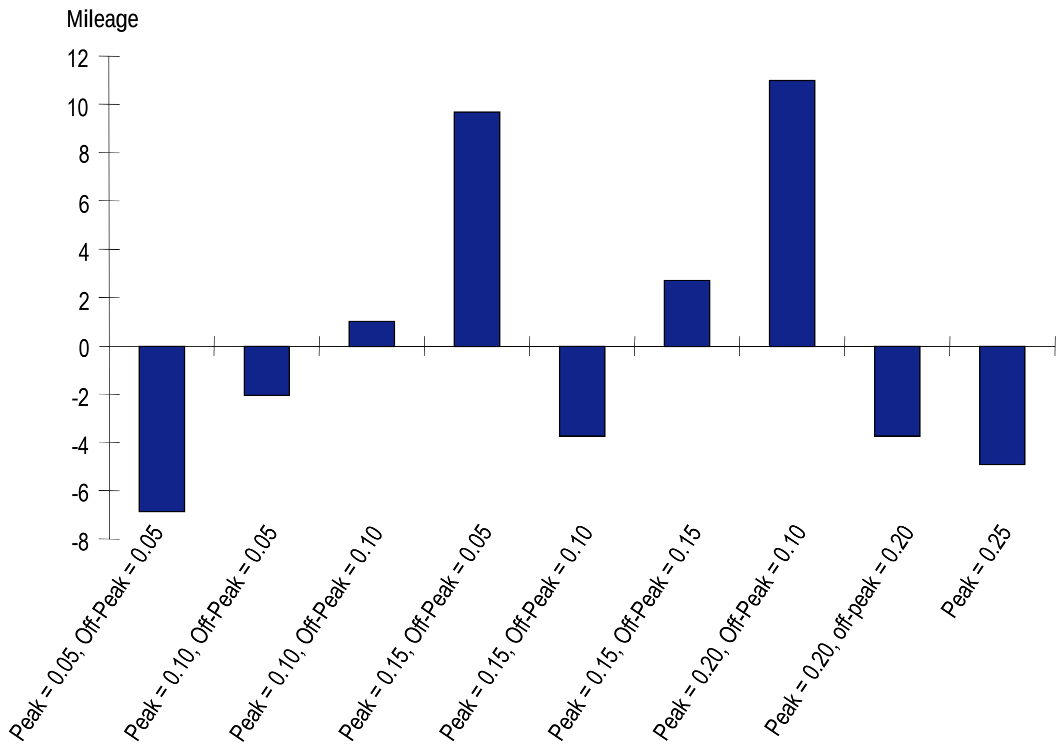
- The negative intercept in the regression indicates that those that were priced $0.05 in both the peak and off-peak periods actually increased their mileage by an average of 6.81 miles per day.
- The positive coefficients of the price variables indicate that relative to a peak, off-peak price combination of $0.05, higher prices are associated with more reduction in mileage. For example, households that were charged $0.15 in the peak period and $0.15 in the off-peak period decreased their mileage by an average of 2.74 miles per day (= -6.81 + 9.55). The reduction in mileage for other price combinations is shown in Figure 4.18.

Table 4.15 Results of the Regression of Reduction in Mileage

Variable	Coefficient Estimate	t-statistic
Intercept	-6.81	-1.44
Peak price = 0.05, off-peak price = 0.05	0.00	base
Peak price = 0.1, off-peak price = 0.05	4.83	0.79
Peak price = 0.1, off-peak price = 0.1	7.85	1.39
Peak price = 0.15, off-peak price = 0.05	16.54	2.25
Peak price = 0.15, off-peak price = 0.1	3.11	0.50
Peak price = 0.15, off-peak price = 0.15	9.55	1.52
Peak price = 0.2, off-peak price ≤ 0.1	17.86	2.02
Peak price = 0.2, off-peak price = 0.2	3.11	0.44
Peak price = 0.25	1.96	0.28

One would expect that as the per-mile price charged increases, the household would reduce the mileage. This holds for several but not all of the price categories, probably because of the small sample size in the experiment.

Figure 4.18 Reduction in Mileage as a Function of Peak and Off-Peak Prices



Note: Positive numbers indicate a reduction in mileage. Negative numbers are negative reductions, or an increase.

Elasticity Analysis of Matching

In addition to the reduction in mileage, we computed peak and off-peak elasticities of mileage with respect to price. The elasticity is defined as follows:

Elasticity = $[(M_t - M_c)/M_c]/[(P_t - P_c)/P_c]$,

where M_t is (peak or off-peak) mileage when in treatment, M_c is the mileage when in control, P_t is the cost per mile when in treatment, and P_c is the cost per mile when in control.

For a given vehicle in treatment, M_c is obtained from the mileage data of the matched comparison group, $P_t - P_c$ is equal to the (peak or off-peak) price that the vehicle is charged per mile, and P_c is assumed to be $0.10 per mile.

Figures 4.19 and 4.20 show the distributions of peak and off-peak elasticities, respectively, among the household vehicles that were subjected to pricing. A few outlier observations with large positive elasticities were removed for the purpose of this analysis. These graphs show that there are mixed reactions to pricing. Some people decrease their mileage while some others increase their mileage.

Figure 4.19 Distribution of Peak Elasticity among Vehicles in Treatment

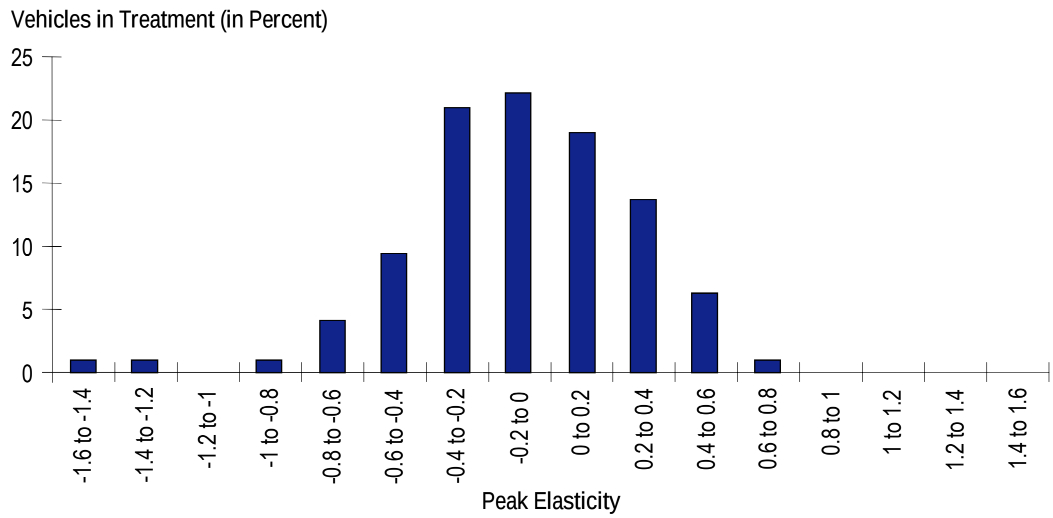
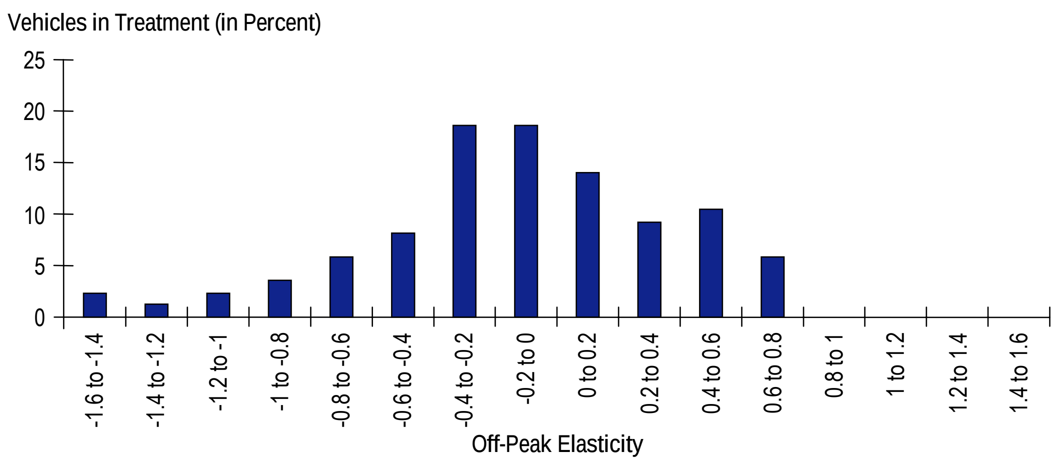


Figure 4.20 Distribution of Off-Peak Elasticity among Vehicles in Treatment



We then regressed the elasticity of mileage with respect to price against several variables, including the presence of a leased car in the household, the presence of unpriced vehicles in the household, and mileage during the initial control period. The results of these regressions are shown in Tables 4.16 and 4.17 for peak and off-peak conditions, respectively.

Table 4.16 Regression for the Elasticity of Peak Mileage with Respect to Peak Price

Variable	Coefficient Estimate	t-statistic
Intercept	-0.37	-2.39
Leased car(s) in hh	-0.07	-0.64
Presence of unpriced vehicle(s) in household	-0.06	-0.75
Peak mileage in Period 1	0.02	1.37
Peak mileage in Period 1 squared	-0.0001	-0.56

Table 4.17 Regression for the Elasticity of Off-Peak Mileage with Respect to Off-Peak Price

Variable	Coefficient Estimate	t-statistic
Intercept	-0.61	-2.44
Leased car(s) in hh	-0.03	-0.17
Presence of unpriced vehicle(s) in household	-0.12	-1.07
Off-peak mileage in Period 1	0.04	2.18
Off-peak mileage in Period 1 squared	-0.0005	-1.63

The following conclusions can be drawn from these regressions, which generally support other conclusions drawn from the previous analyses:

- The intercepts in both regressions are negative, indicating a negative base effect (decrease in mileage with pricing).
- The effect of having one or more leased vehicles in the household is that the household responds by reducing mileage on the priced vehicle (negative coefficient in regression) because households that already are used to leasing autos are more aware of the associated costs.
- The presence of unpriced vehicles in the household causes a decrease in mileage for the priced vehicle possibly because some mileage substitution between the priced and unpriced vehicles, hence the negative coefficient in the regression.
- Finally, the (peak or off-peak) mileage during the initial control period has a strong effect as well on the elasticity of mileage to price. A quadratic function of control period mileage is used as an explanatory variable. The result is that for the typical distance ranges that are driven in the peak and off-peak periods, as a household's base mileage (in the control period) is larger, the less likely the household is to reduce the mileage when subjected to pricing. However, as the control period mileage exceeds a certain threshold, the household becomes more sensitive to pricing.

4.2.4 Additional Analyses

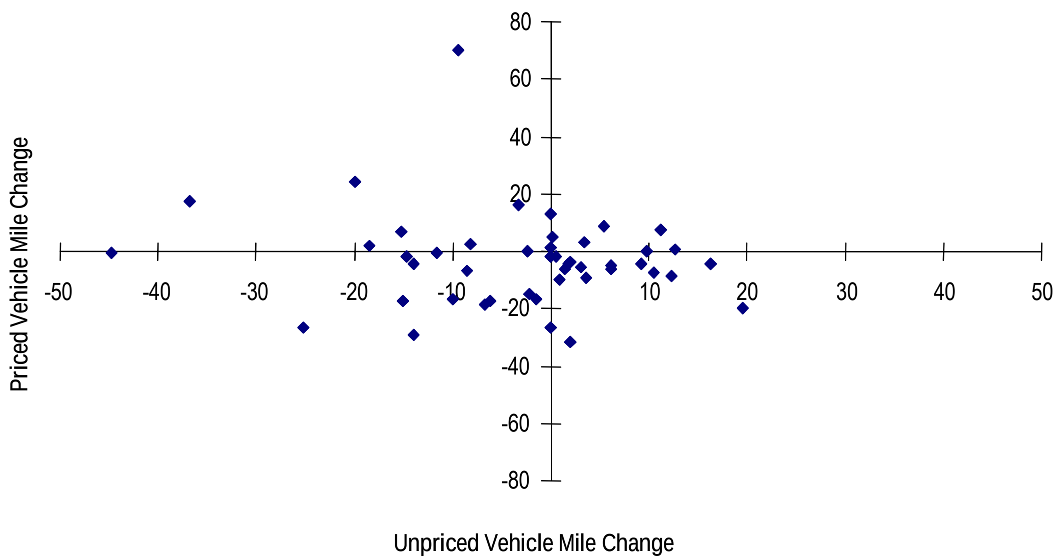
Substitution of Mileage to Unpriced Vehicles

The regression analyses performed as part of the individual vehicle analysis and as part of the matching analysis indicated that the presence of other nonpriced vehicles in a household increases the amount of mileage reduction for the priced vehicles. This could indicate that participant households shifted their vehicle travel to the unpriced vehicles during the pricing periods. The implication of this is that the net vehicle mileage reduction would be reduced if future participants of PAYD programs are able to keep a combination of participating and nonparticipating vehicles.

To evaluate the level of substitution between priced and unpriced vehicles within households, we requested participants provide periodic odometer readings from all household vehicles, including both priced and unpriced vehicles. These data were generally less clean and more likely to be missing than the CarChip data, because they relied on participants' collection, so some data interpolation and judgment were needed in processing them.

Nevertheless, we developed reasonable odometer readings for all the vehicles in 46 households that included a combination of priced and unpriced vehicles. Figure 4.21 plots the mileage changes of priced vehicles compared to the Period 1 control against the mileage changes of unpriced vehicles in the same household compared to their Period 1 mileage.

Figure 4.21 Comparison of Mileage Changes for Priced and Unpriced Vehicles in the Same Households



The 18 households (39 percent of all represented households) in the lower right quadrant of the graph reduced mileage on their priced vehicle(s) while increasing their mileage on unpriced vehicle(s). Another 14 households actually reduced mileage traveled in both their priced and unpriced vehicles. The remaining 14 households increased the amount they drove their priced vehicles. Among these 46 households, the average mileage change for unpriced vehicles is actually very similar to the change for priced vehicles, but as the figure shows, this calculation is heavily influenced by a handful of households that reported significant reductions in the usage of their unpriced vehicles.

If we discount these records, we reach the conclusion that although substitution does take place within some households, this substitution is not solely responsible for the reduction in priced vehicle mileage. If the experiment findings can be generalized, we can conclude that voluntary opt-in PAYD programs will not, at least in the short term, be taken advantage of to a large extent by multiple vehicle households who can keep some vehicles with standard fixed costs and others with PAYD costs.

Outlier Analysis

As discussed above, some of the experiment participants did not seem to react to the exposure to per-mile pricing as would be expected. To better understand the behavior of this group, we compared these participants to the participants as a whole, based on their answers to the recruit and exit surveys. We classified the 17 respondents that increased their mileage by 20 percent or more in their priced periods (Periods 2 and 4 were used for this analysis) relative to the first control period as "outliers." We then compared those respondents' characteristics to the overall experimental sample.

From the recruit survey, the following observations can be made regarding those participants classified as outliers:

- 58.8 percent of these participants live in Chisago County compared to 34.6 percent of the overall experimental sample who live in Chisago County;
- 94.1 percent of these participants have two or three vehicles available in the household compared to 84.0 percent of the overall experimental sample who have two or three vehicles available in the household;
- 82.4 percent of these participants have a full-time or part-time employed head of household compared to 80.2 percent of the overall experimental sample who have a full-time or part-time employed head of household;
- 82.4 percent of these participants have a college graduate or post graduate head of household compared to 65.4 percent of the overall experimental sample who have a college graduate or post graduate head of household;

- 64.7 percent of these participants have a high household income ($65,000 or above) compared to 63.0 percent of the overall experimental sample who have a high household income;
- 70.6 percent of these participants share one or more vehicles among household members compared to 48.2 percent of the overall experimental sample who share vehicles among household members; and
- 70.6 percent of these participants have a total annual household mileage of 15,000 miles or more compared to 71.5 percent of the overall experimental sample who have a total annual household mileage of 15,000 miles or more.

Table 4.18 summarizes the characteristics and experience of those respondents classified as outliers in comparison to the overall experimental sample as described in the exit survey.

Because there are only 17 outlier participants, it is difficult to identify statistically significant differences between the outliers and the overall sample. As for the overall sample, almost all of the outliers felt that their driving behavior during unpriced periods was fairly typical, but the outliers were less likely to agree (65 percent versus 74 percent) that their travel patterns were typical during the priced periods. There is no real indication that this group did not try as hard as the overall sample to restrict their mileage. While 63 percent of the overall sample said they did not try hard to reduce their mileage, only 53 percent of the outliers did not. In addition, the outliers were more likely to say they were aware of the pricing during the priced period (53 percent versus 39 percent).

The outliers were more likely to say that they need to drive to different destinations (59 percent versus 49 percent), less likely to have the same driving patterns from week to week (53 percent versus 65 percent), and far less likely to say that each driver in their household has a particular vehicle they more or less drive all the time (47 percent versus 77 percent).

Based on these observations, the generalized profile of the outliers that emerges is a group of active households that had little perceived control over the derived demands for their auto travel during the priced period. These participants would be taking on the risk of higher than average month-to-month costs if they participated in a PAYD program, but interestingly, they were more confident at the end of the experiment than the overall sample that, over a longer period of time, they could reduce the number of miles that they drove in response to per-mile charges (59 percent versus 39 percent).

Table 4.18 Summary of Outliers' Characteristics and Experience from the Exit Survey

Question	Rating	Percentage of Outlier Respondents	Percentage of Experimental Respondents
Part 1: Participants' Study Experience			
Being in the experiment affected my or my family's driving habits	Disagree	64.7	70.7
During the time when our mileage was not priced, the amount we drove and our travel patterns were fairly typical	Agree	94.1	93.4
During the time when our mileage was priced, the amount we drove and our travel patterns were pretty typical	Agree	64.7	73.7
During the time when our mileage was priced, I was aware of the price when I drove the car	Agree	52.9	39.5
During the time when our mileage was priced, I felt restricted in terms of where and when I drove	Disagree	76.5	84.2
It was difficult to reduce the amount of miles I drove	Agree	52.9	56.6
I tried hard to reduce the number of miles I drove	Disagree	52.9	63.2
The price per mile made virtually no difference in my driving patterns	Agree	64.7	68.4
If I were charged by the mile, I would be able to reduce the number of miles I drive over a long period, like a year	Agree	58.8	38.7
During the pricing period, I reduced weekday rush hour driving to save miles on the priced vehicle	Disagree	70.6	78.9
During the pricing period, I reduced weekday driving at times other than rush hours to save miles on the priced vehicle	Disagree	64.7	71.1
During the pricing period, I reduced weekend driving to save miles on the priced vehicle	Disagree	70.6	68.4
During the pricing period, I combined driving trips to save miles on the priced vehicle	Disagree	52.9	47.4
During the pricing period, I used other unpriced vehicles to save miles on the priced vehicle	Disagree	76.5	77.6
During the pricing period, I walked, biked, and/or used public transit to save miles on the priced vehicle	Disagree	88.2	92.1
During the pricing period, other household members reduced their driving to save miles on the priced vehicle	Disagree	70.6	78.9
Part 2: Participants' Driving Attitudes			
The automobile gives me a lot of flexibility in my daily life	Agree	70.6	75.3
I need to drive to different destinations as part of my busy daily schedule	Agree	58.8	49.3
I like driving whenever and wherever I like without worrying about the cost	Agree	47.1	52.0
My driving patterns are pretty close to the same from week to week	Agree	52.9	64.9
If the car I usually drive is unavailable for some reason, I can usually use a different vehicle to make the trips I need to	Agree	52.9	51.4
I don't like having to rely on others to take me to where I need to go	Agree	58.8	73.7
Each driver in our household has a particular vehicle that they more or less drive all the time	Agree	47.1	77.3

4.3 Summary

The conclusions of the individual analyses vary, but we believe the overall findings of the experiment may be summarized as follows:

- Wide-scale per-mile pricing would result in a measurable, but small, reduction in vehicle mileage. In magnitude, this reduction is probably within the regular variation that occurs from season to season, but if these reductions were generalized to the entire population per-mile pricing would reduce VMT and congestion measurably. This was done in the *Public Policy Implications* report. The results compare favorably to the typical results of major transit capital investments in terms of overall reductions in VMT.
- On a percentage basis, the biggest reduction in mileage would be on weekends, which presumably have the highest percentage of discretionary travel purposes, but weekday peak-period travel would be reduced by more than weekday off-peak period mileage. The higher reduction in the peak period may be due to better availability of alternative travel options (transit, carpooling) during these times compared to off-peak times. The evaluation of time-of-day pricing protocols, in which peak-period mileage charges were set to be higher than off-peak charges, did not indicate that this type of pricing increases the peak-period mileage reduction. In fact, the vehicles that had a single mileage price per day actually reduced peak-period mileage more than those that were priced with time-of-day pricing schemes. It was not possible to perceive from the data any significant amount of time shifting from the peak to the off-peak.
- Mileage reductions from per-mile pricing would vary by season, with the highest reductions during the warmer months.
- Some households could reduce their mileage under per-mile pricing at significantly higher levels than most households. Specifically, households that could reduce their mileage the most are those that:
  - Have other unpriced vehicles to which they could transfer their trips;
  - Have leased vehicles, probably because they are more accustomed to monitoring the mileage on vehicles; or
  - Have household members that actively think about auto ownership and operating costs.
- Households that are less likely to reduce their mileage under per-mile pricing are those that:
  - Share the use of one or more of their vehicles among household members; or
  - Have a head of household who is more than 65 years old.

- Based on the somewhat disparate results of the different analyses, higher per-mile charges do not necessarily seem to increase the mileage reduction of households. Those households that are willing/able to reduce their mileage apparently will do so even with low- to mid-level per-mile prices. Those that do not reduce their mileage do not seem to be able to do so even with higher incentives, at least within the pricing parameters tested in this study.

Appendix A

Evaluation of Social Programs: Literature Review

Evaluation of Social Programs

Literature Review

February 9, 2004

Table of Contents

1.0 Introduction

Evaluation of social programs consists of identifying and applying methods that reliably estimate the impacts of a social program to assist in decision-making on program initiation, expansion, or termination. Two approaches for program evaluation have been widely discussed in the literature. The first approach is the experimental method, which is based on the randomization of a pool of participants into a treatment group and a control group, and the direct comparison of outcomes from the two groups to assess program impacts. The second approach is the nonexperimental method, which relies on "microdata sources, statistical methods, and behavioral models" to allow the comparison of the outcomes of participants and nonparticipants in social programs (Heckman and Smith, 1995). This report provides a review of these program evaluation methods, discusses their advantages and limitations, and provides examples of studies where these methods have been used.

■ 1.1 The Evaluation Problem

Let Y denote an outcome of interest, and suppose than an individual can be in one of two states: "1" if the individual receives treatment and "0" otherwise. Y_1 is the outcome associated with receipt of treatment, and Y_0 is the outcome in the no-treatment state. The gain of an individual from participating in a program is the change in outcomes between the treatment and no-treatment states, defined as:

$$\Delta = Y_1 - Y_0 . \tag{1.1}$$

Since at any time an individual can be observed in only one state (treated or untreated), the gain cannot be computed for any particular individual. Consequently, the focus in the evaluation literature has been on the estimation of the distribution of impacts among individuals, or certain aspects of the distribution, particularly mean impacts. This report will also focus on methods that estimate mean impacts. In voluntary programs and those that target specific groups in the population, the parameter of interest is normally the mean effect of treatment on program participants, defined as:

$$E(\Delta|D=1) = E(Y_1|D=1) - E(Y_0|D=1), \tag{1.2}$$

where $E(.)$ denotes expected value and D is an indicator of participation ($D=1$ for participants and 0 for nonparticipants).

The term $E(Y_0|D=1)$ which represents the mean outcome of participants had they not participated, also called a counterfactual, is not observed. The estimation of the desired counterfactual lies at the heart of the evaluation problem.

1.2 Report Organization

The remainder of this report is organized as follows. Section 2 discusses how social experiments solve the evaluation problem and presents their limitations. Section 3 summarizes various econometric methods that have been used in the evaluation of social programs. Section 4 presents examples of actual social programs that have been evaluated in practice, and describes the methods that were applied to assess the programs. Section 5 concludes the report. Appendix A of this report provides further technical detail to the problem of attrition bias often encountered in evaluations of social programs.

2.0 Experimental Methods

This section is organized as follows. Section 2.1 describes how social experiments solve the evaluation problem and presents their underlying assumptions. Section 2.2 presents the limitations of experimental methods and motivates the need for nonexperimental methods, which are discussed in Section 3 of this report.

2.1 Definitions and Assumptions

Experimental methods for program evaluation operate in the context of social experiments, where participants in the experiment are divided into two groups: an experimental treatment group that receives treatment, and an experimental control group whose members are randomly denied access to the service or treatment being evaluated. Experimental methods evaluate program impact by making direct comparisons of outcomes (usually means) between the treatment and control groups without the need for functional form specifications of outcome and participation processes. Under ideal conditions (discussed below), experimental estimators provide unbiased estimates of the mean impact of treatment on the treated. The mean outcome of the treatment group is used to estimate the treatment outcome of participants in the program, and the mean outcome of the control group is used to estimate the no-treatment outcome of participants in the program. Let R be an indicator of receipt of treatment conditional on participation in the experiment, where $R=1$ for the treatment group and $R=0$ for the control group, and let t denote a post-program period. The experimental estimator of mean impact of treatment on the treated is given by the following expression:

$$E\left(Y_{1t}\middle|D=1,R=1\right)-E\left(Y_{0t}\middle|D=1,R=0\right). \tag{2.1}$$

The experimental method provides unbiased estimates of the mean impact of treatment on the treated provided the following conditions are satisfied (Heckman and Smith, 1995):

- No randomization bias: Randomization is the process of randomly allocating participants to treatment or control groups, resulting in two statistically identical sets of individuals. Randomization bias occurs if the selection process into the program is altered, so that "those who participate during an experiment differ from those who would have participated in the absence of an experiment."
- No substitution bias: Substitution bias occurs when control group members obtain close substitutes for the treatment elsewhere, thus rendering the outcome of the control group inappropriate as a proxy for outcome in the no-treatment state. The experimental estimate in this case evaluates the program being evaluated in reference

to other existing programs (which can be called the effect of the program), rather than as compared to no program at all (i.e., rather than the effect of training, for example, in the job training program evaluation context) (Smith (2000), Heckman et al. (2000)).

Note that randomization does not remove selection bias. Rather it balances the bias between the treatment and control groups so that it cancels out (Heckman and Smith, 1995). This can be seen by considering the following simple common coefficient model:

$$Y = \alpha + \delta D + \varepsilon , \tag{2.2}$$

where α is the mean outcome in the absence of the program, δ measures a common treatment effect, and ε represents unobserved individual characteristics that affect outcome. Selection bias occurs if participation, as indicated by D, is correlated with unobserved characteristics ε. In each of the treatment and control groups, $E(\varepsilon | D = 1)$ might be different from zero (i.e., there is selection bias). By taking the difference in mean outcomes between the treatment and control groups, the term $E(\varepsilon | D = 1)$ drops out thus canceling the bias.

2.2 Limitations of Experimental Methods

Several difficulties are associated with the use of experimental methods to estimate program impacts. First, experimental methods allow the estimation of a limited set of parameters that are of interest to policy makers. The estimation of several parameters, such as the proportion of people harmed by the program, requires the joint distribution of outcomes in the treatment and no-treatment states. Heckman and Smith (1995) show a method for bounding those estimates, but emphasize the significant variability in the ranges implied by those bounds.

Second, in the case where multiple treatment types are administered to participants, experimental methods cannot assess the impacts of separate treatments unless multi-stage randomization is employed.

Third, participants (both treatment and control group members) might attrit from the sample due to change of residence, loss of interest, or other factors, and data on those who attrit are lost. If attrition is nonrandom between members of the treatment and control groups, the experimental estimate of mean impact is biased due to the correlation between the experimental status R and the likelihood of being in the sample. In this case, nonexperimental methods utilizing attrition models should be used. To minimize attrition, random assignment should be done as close as possible to the actual initiation of treatment (Heckman and Smith, 1995). A review of attrition bias and methods to correct for it are presented in Appendix A.

Fourth, treatment group dropouts also pose a problem for experimental evaluation methods (Heckman et al. (1998 c), Heckman et al. (1999 a), Heckman et al. (2000)). The difference between dropouts and attrition is that dropping out applies to treatment group members only, while attrition could apply to both treatment and control group members. Furthermore, it is assumed that data on outcomes are still available for treatment group members that drop out, but are no longer available for those who attrit. The characteristics of treatment group members that drop out of the experiment are normally different from the characteristics of those that actually receive treatment. In the presence of dropout, experimental mean difference estimates are biased estimates of the impact of full treatment on the fully treated. That is, in the presence of dropout, the experimental mean difference estimates what is called the "intent to treat," which is the mean effect of the offer of treatment, rather than the term $E(\Delta|X, D=1)$. Heckman et al. (1998 c, 1999 a) show that if treatment group members drop out before receiving any treatment, an unbiased estimate could still be obtained under certain assumptions, with some adjustments of the formula for the experimental estimate. However, in the case of dropouts with partial treatment, the key assumption justifying an instrumental variable estimator, commonly used in the case of dropouts with no treatment at all, is unlikely to hold. Heckman et al. (1998 c) discuss the use of exclusion restrictions but argue that identification of the effect of full treatment on the fully treated based on exclusion restrictions "is a delicate operation that is not robust to small perturbations in the assumptions," and make the case that parameters other than the effect of full treatment on the fully treated might be of interest.

3.0 Nonexperimental Methods

3.1 What Are Nonexperimental Methods?

Recall that the mean effect of treatment on the treated was given in expression (1.2) as follows:

$$E(\Delta|D=1)=E(Y_1|D=1)-E(Y_0|D=1). \tag{1.2}$$

In the case of social experiments, the counterfactual $E(Y_0|D=1)$ is obtained from outcome data of the experimental control group. When any of the assumptions justifying the use of experimental methods fails to hold, as discussed in Section 2, nonexperimental methods should be used in impact assessments. Nonexperimental methods use a comparison group, usually of eligible nonparticipants, to estimate the outcomes of participants in the no-treatment state (i.e., the counterfactual $E(Y_0|D=1)$). That is, the effect of treatment on the treated is estimated as follows:

$$\hat{E}(\Delta|D=1)=E(Y_1|D=1)-E(Y_0|D=0). \tag{3.1}$$

The bias that results from using the outcome data of a nonexperimental comparison group is given by:

$$B=E(Y_0|D=1)-E(Y_0|D=0). \tag{3.2}$$

Nonexperimental (or econometric) methods rely on the use of statistical and behavioral models to minimize the bias as given by expression (3.2). Most of these methods rely on the assumption that models of the outcome and participation processes could be formulated.

The remainder of this section is organized as follows. Section 3.2 provides a decomposition of the bias that arises when comparing participants to nonparticipants. Section 3.3 presents general guidelines for constructing a nonexperimental comparison group. Sections 3.4 presents the methods and assumptions of several nonexperimental estimators that have been widely used in the literature of program evaluation.

3.2 Decomposition of Bias

The following discussion is based on Heckman et al. (1998 a). The conventional measure of bias can be decomposed into three terms:

$$B = B_1 + B_2 + B_3 . \tag{3.3}$$

The first bias term B_1 results from comparing incomparable people. In other words, it refers to failure to find individuals in the comparison group that are similar in observed characteristics or probability of participation to individuals in the experimental treatment group. This is known as the "common support problem." B_1 is given by the following expression:

$$B_1 = \int_{S_{1X} \setminus S_X} E(Y_0 | X, D = 1) dF(X | D = 1) - \int_{S_{0X} \setminus S_X} E(Y_0 | X, D = 0) dF(X | D = 0), \tag{3.4}$$

where S_{1X} is the support of X in the $D = 1$ population (i.e., the region of X with positive density), S_{0X} is the support of X in the $D = 0$ population, and S_X is the region of common support (i.e., $S_X = S_{1X} \cap S_{0X}$).

The second bias term B_2 is the result of differential weighting of comparison group members within the region of common support. It is given by:

$$B_2 = \int_{S_X} E(Y_0 | X, D = 0) [dF(X | D = 1) - dF(X | D = 0)]. \tag{3.5}$$

The third bias term B_3 is the bias that remains even after controlling for observable differences. It is given by:

$$B_3 = \int_{S_X} \left(E(\varepsilon_0 | X, D = 1) - E(\varepsilon_0 | X, D = 0) \right) dF(X | D = 1), \tag{3.6}$$

where ε_0 is the error term in the outcome equation corresponding to the no-treatment state.

■ 3.3 Constructing a Comparison Group

Several lessons have been learned from the program evaluation research on the sampling of a comparison group for use in nonexperimental methods. We summarize those guidelines as follows:

- Eligibility: A comparison group of nonparticipants should be subjected to the same eligibility criteria used to select participants in the experiment.
- Market and geographical matching: Selecting nonparticipants from the same market conditions and geography as participants helps reduce the bias in nonexperimental estimates of program impacts.
- Comparing comparable people: The comparison group should be large enough to ensure that for every participant in the experimental group there is at least one nonparticipant in the comparison group with a similar set of observable characteristics (or with similar probability of participation). If this condition is not satisfied, the set of participants over which the comparison can be made is reduced, and the estimated effect could be different from the one estimated on the whole set of participants.
- Survey instrument: It is advisable to use the same questionnaire, methods of outcome measurement, and interviewers with the participant and nonparticipant samples.
- Weighting: This refers to the need to weigh the comparison group data if the distribution of observable characteristics is different between participants and nonparticipants in the region of common support.

Heckman et al. (1999 a) note that participants are often oversampled compared to nonparticipants. Several econometric estimators assume random sampling, and therefore require the samples to be reweighed in the case of oversampling of participants.

■ 3.4 Types of Nonexperimental Estimators

Nonexperimental estimators can be categorized in different ways. Estimators are cross-sectional if the comparison is made between participants and nonparticipants at one point in time (e.g., in a post-program period), longitudinal if comparisons are made between the same persons in the untreated and treated states (from pre-program and post-program data), and a hybrid of the two if comparisons are made between different persons and using multiple time periods (Heckman et al., 1999 a). Nonexperimental estimators can also be classified as those that are based on "selection on observables" and those that are based on "selection on unobservables." Methods based on "selection on observables" assume that participation is random conditional on some set of observed covariates, and the difference among estimators that are based on "selection on observables" is in the conditioning process. An example of these estimators is the propensity score matching estimator discussed in Section 3.4.4. Methods based on "selection on unobservables" assume that participation and outcomes are jointly affected by factors other than observed covariates. An example of these estimators is the difference-in-differences estimator discussed in Section 3.4.2 (Smith, 2000).

Note that there is no universal estimator that fits the best, and that different estimators give the same estimate only if there is no selection bias. Also note that Heckman et al. (1999 a) stress the choice of conditioning variables X that are not caused by D (participation) given the vector of potential outcomes. Below we discuss some of the widely considered estimators in the literature on program evaluation.

3.4.1 The Before-After Estimator

A widely used method for evaluating the impacts of a program is the before-after estimator, which compares outcomes before and after a program. The method assumes the availability of longitudinal data on outcomes for participants, or cross-sectional data taken from the same population such that one cross-section is sampled before the program and another is sampled after the program.

The main assumption of this method is that the pre-program outcome of participants is a good proxy of the post-program outcome of participants in the no-treatment state. This can be expressed as follows:

$$E\left(Y_{0t} - Y_{0t'} \middle| D = 1\right) = 0, \tag{3.7}$$

where t' is a pre-program period and t is a post-program period.

The mean effect of treatment on the treated can be expressed as follows:

$$E\left(Y_{1t} - Y_{0t} \middle| D = 1\right) = E\left(Y_{1t} - Y_{0t'} \middle| D = 1\right) + E\left(Y_{0t'} - Y_{0t} \middle| D = 1\right), \tag{3.8}$$

and the before-after estimator of the effect of treatment on the treated is the first term $E\left(Y_{1t} - Y_{0t'} \middle| D = 1\right)$ on the right-hand-side of expression (3.8). A disadvantage of the before-after estimator is that it does not take account of lifecycle factors and other trends that might affect outcome, but rather attributes all changes in outcome to the program being evaluated. If the individual approximation errors, $Y_{0t'} - Y_{0t}$, do not average to zero, the program impact estimate would be biased. A good example of bias caused by using the before-after estimator is in the context of evaluating job training programs, due to a pre-program dip in earnings experienced by participants and referred to as Ashenfelter's dip (see for example Heckman and Smith (1999 b)).

Two solution approaches have been suggested to overcome the limitations of the before-after estimator. The first approach assumes the availability of a time series of outcomes in pre-program periods that would allow the extrapolation of outcome in the post-program period in the no-treatment situation. This method is valid only if population mean outcomes evolve deterministically with time or with macroeconomic variables. The second approach is the difference-in-differences estimator discussed next.

3.4.2 The Difference-in-Differences Estimator

The difference-in-differences estimator uses a comparison group of nonparticipants to control for changes in outcome that are not attributed to the program being evaluated. Data on outcomes and their determinants in the pre-program and post-program periods need to be collected for participants and nonparticipants, using either a longitudinal sample or repeated cross-sections. The main assumption of this method is that mean changes in the no-treatment outcome are the same for participants and nonparticipants, stated formally as:

$$E\left(Y_{0t} - Y_{0t'} \middle| D = 1\right) = E\left(Y_{0t} - Y_{0t'} \middle| D = 0\right). \tag{3.9}$$

Under this assumption, the difference-in-differences estimator is given by the following expression:

$$\hat{E}\left(\Delta \middle| D = 1\right) = E\left(Y_{1t} - Y_{0t'} \middle| D = 1\right) - E\left(Y_{0t} - Y_{0t'} \middle| D = 0\right). \tag{3.10}$$

The difference-in-difference estimator will produce a biased estimate if the time path of no-treatment outcomes differs between participants and nonparticipants. For example, in the context of the job training program, participants experience a pre-program dip in earnings (known as Ashenfelter's dip) and an upward trend in post-program earnings that are different from the earning patterns experienced by nonparticipants, causing the estimates produced by a conventional difference-in-differences estimator to be biased (see for example Heckman and Smith (1999 b)).

The difference-in-differences estimator can be motivated in an alternative way (Smith, 2000). The outcome for individual i in period t can be specified as follows:

$$Y_{it} = \beta X_{it} + \delta_i D_i + \varepsilon_{it}. \tag{3.11}$$

Since participation D might be correlated with ε_{it} (the unobserved characteristics), running OLS would lead to biased estimates of program impact. The difference-in-differences estimator assumes that the error term ε_{it} in the outcome equation can be decomposed into a time-invariant component μ_i (also called fixed effect) and a transitory component ω_{it}, as follows:

$$\varepsilon_{it} = \mu_i + \omega_{it}. \tag{3.12}$$

The difference-in-differences model thus assumes that participation depends on the fixed effect μ_i but not on ω_{it}. The fixed effect μ_i can be differenced out by taking the difference in outcomes between a pre-program and a post-program period, as follows:

$$Y_{it} - Y_{it'} = \beta\left(X_{it} - X_{it'}\right) + \delta_i D_i + \left(\omega_{it} - \omega_{it'}\right). \tag{3.13}$$

A conditional version of the difference-in-differences estimator has also been suggested in the literature and is believed to perform better than the conventional difference-in-differences estimator (see Heckman and Smith (1999 b)). It assumes that, conditional on a vector of observed characteristics X, selection bias is the same in periods before and after the participation decision, stated as follows:

$$E\left(Y_{0t} \middle| X, D=1\right) - E\left(Y_{0t} \middle| X, D=0\right) = E\left(Y_{0t'} \middle| X, D=1\right) - E\left(Y_{0t'} \middle| X, D=0\right) \tag{3.14}$$

The effect of treatment on the treated, $E\left(Y_{1t} - Y_{0t} \middle| X, D=1\right)$, is then estimated using expression (3.10). Under general conditions, conditioning on X can also be replaced by conditioning on the probability of participation $P(X)$ (Rosenbaum and Rubin, 1983).

Finally, note that it is good practice to use the difference-in-differences estimator after matching has been done.

3.4.3 The Cross-Section Estimator

The cross-section estimator produces an estimate of program impact by comparing the outcome of a participant to that of a nonparticipant in a post-program period. This method is based on the assumption that the no-treatment outcome is the same for both participants and nonparticipants, which can be expressed as follows:

$$E\left(Y_{0t} \middle| D=1\right) = E\left(Y_{0t} \middle| D=0\right). \tag{3.15}$$

The cross-section estimator is then given by the following expression:

$$E\left(Y_{1t} \middle| D=1\right) - E\left(Y_{0t} \middle| D=0\right). \tag{3.16}$$

Note that the estimate will be biased if individuals participate in the program based on outcomes in the no-treatment state in the post-program period.

3.4.4 The Method of Matching

The method of matching is based on the idea that the no-treatment outcomes of participants can be estimated from outcomes of nonparticipants with a similar set of observed characteristics. This method is based on two assumptions:

$$\left(Y_0, Y_1\right) \perp D \middle| X, \tag{3.17}$$

and $0 < \Pr\left(D=1 \middle| X\right) < 1,$ (3.18)

where $\perp$ denotes stochastic independence. Assumption (3.17) states that outcome is independent of participation, given conditioning variables X. An implicit implication of this assumption is that individuals do not participate in a program based on gains unobserved by analysts. Assumption (3.18) requires the availability of both participants and nonparticipants with the same X values over which the effect of the treatment is to be measured. If the set of X variables over which comparison can be made is reduced, and if the parameter of interest depends on X, then the estimate generated by matching might be different from the experimental estimate. Note also that the set X of conditioning variables should not contain any variable caused by D (participation) given the unobservables so as not to mask the true effect of D on outcomes (Heckman et al., 1999 a).

Constructing a matching estimate associates with every individual in the treatment group one or more individuals from the comparison group of nonparticipants so that they have the same set of observed characteristics that influence participation. Alternatively, if assumptions (3.17) and (3.18) are satisfied, one can match on the probability of participation (Rosenbaum and Rubin, 1983), which reduces the set of matching criteria to a scalar. Different matching methods have been suggested, such as nearest neighbor matching which assigns only one individual with the closest characteristics from the comparison group to match an individual from the treatment group; caliper matching, which matches one individual from the comparison group to one from the treatment group based on a pre-specified tolerance in the difference in characteristics, and; kernel matching, which uses all members of a comparison group with a weighting strategy to match to an individual from the treatment group. A mean impact estimate based on matching is given by the following expression:

$$m = \frac{1}{N_t}\sum_{i=1}^{N_t}\left(Y_i^t - \overline{Y}_i^c\right) = \frac{1}{N_t}\sum_{i=1}^{N_t}\left(Y_i^t - \sum_{j=1}^{N_c} W(i,j)Y_j^c\right), \tag{3.19}$$

where the subscripts t and c refer to the treatment group and comparison groups, respectively, N_t and N_c are the sample sizes of the treatment and comparison groups, respectively, Y_i^t and Y_j^c represent outcomes in the treatment and comparison groups, respectively, and $W(i, j)$ is the weight assigned to individual j from the comparison group when constructing a match to individual i from the treatment group such that:

$$\sum_{j=1}^{N_c} W(i,j) = 1 \quad \forall i = 1,\ldots,N_t \,. \tag{3.20}$$

For clarity of presentation, we outline the main steps of a widely-used matching method, called propensity score matching (see for example Baker (2000) and Ravallion (2002)).

1. Construct a sample of eligible nonparticipants to serve as a comparison group to a treatment sample. The two samples should be subjected to the same (or very similar) survey instruments.

2. Combine the two samples of participants and nonparticipants to estimate a binary choice participation model (logit has been frequently used) as a function of all variables that are likely to influence participation.

3. Use the estimated model in Step 2 to compute the predicted probability of participation, also called a propensity score, for every participant and nonparticipant.

4. Remove some of the nonparticipant observations whose propensity scores (e.g., those that are very low) fall outside the range of propensity scores of participants. The ranges of probability participation should then be close in the participant and nonparticipant samples.

5. For every individual in the treatment sample, find one (or more) individuals from the nonparticipant sample to match to, based on propensity scores, using some matching technique such as nearest neighbor (or 5 nearest neighbors), etc.

6. Estimate the gain for an individual by taking the difference between the outcome for the individual and the outcome for the matched individuals from the comparison group.

7. Compute the mean overall gain of treatment group members, possibly stratified by some variable of interest.

Note that if participants are oversampled, choice-based sampling methods could be used to correct for that or the odds ratio, defined as $\frac{P}{1-P}$ where P is the probability of participation, could be used for matching (Baker, 2000).

Finally, note that matching eliminates the first two components of bias B_1 and B_2 (see Section 3.2), but the third component of bias B_3 might still be a substantial proportion of the experimental estimate. The reader is also referred to Heckman et al. (1998 b) for a more detailed discussion of matching methods.

3.4.5 Index Sufficient Methods and the Classical Econometric Selection Model

The traditional econometric selection model allows for selection on unobservables. Outcomes in the treatment and no-treatment states are specified using an additive separability assumption, as follows:

$$Y_1 = g_1(X) + \varepsilon_1, \tag{3.21}$$

and $Y_0 = g_0(X) + \varepsilon_0$. (3.22)

The set of observed characteristics X is divided into two sets (Q, Z), where Q is a set of variables determining outcome and Z is a set of variables determining participation. The econometric approach then postulates exclusion restrictions, i.e., that there are variables in Z that are not in Q (Heckman et al. (1998 a), Heckman et al. (1999 a)).

Participation is modeled according to a latent index model of the following form:

$IN = H(Z) - V$, where IN is an index, $H(Z)$ is the mean difference in utilities between the treatment and no-treatment states, and V is independent of Z.

An individual participates in the program if $IN > 0$ and does not participate otherwise. It follows that:

$$\Pr(D = 1|Z) = F_V(H(Z)). \tag{3.23}$$

It is also assumed that the correlation between participation, as indicated by D, and the unobservables, ε_0 and ε_1, occurs through V and not through Q or Z. The "index sufficient" representation characterizes the bias as follows:

$$B(P(Z)) = E(\varepsilon_0|P(Z), D = 1) - E(\varepsilon_0|P(Z), D = 0), \tag{3.24}$$

where $P(Z)$ is the probability of participation in the program.

The effect of treatment on the treated is estimated as follows:

$$E(Y_1 - Y_0|Q, P(Z), D = 1) = g_1(Q) - g_0(Q) + K_1(P(Z)) + K_0(P(Z))\left[\frac{1 - P(Z)}{P(Z)}\right], \tag{3.25}$$

where K_0 and K_1 are control functions that represent the conditional means of the error terms in the outcome equations. Heckman et al. (1999 a) note that the parameter of interest can thus be estimated by making separability, exclusion, and intercept identification assumptions. Moreover, index sufficiency is necessary but not sufficient for the classical index sufficient selection model to be applied in a nonparametric or semi-parametric setting. The reader is referred to Heckman et al. (1998 a) for more details on the use of this method.

3.4.6 The Method of Instrumental Variables

Consider an outcome equation of the following form:

$$Y = \beta X + \delta D + \varepsilon, \tag{3.26}$$

and a participation equation of the form:

$$D = \gamma Z + \nu . \tag{3.27}$$

The method of instrumental variables consists of finding an instrument, defined as an observable source of exogenous variation in program participation but is not already in the outcome equation nor is it correlated with the error term of the outcome equation (Baker, 2000). In terms of the above specifications, Z should contain at least one variable that is not in X and is uncorrelated with ε. One would then estimate the participation model, and use the predicted values of D (conditional on Z) in the outcome equation. One can then apply ordinary least squares to the outcome equation since the predicted value of D depends only on Z which is exogenous and uncorrelated with ε. Note that the standard errors should be adjusted in the second-stage estimation since the values of D that are used are obtained from a previous estimation.

4.0 Social Experiments in Practice

This section presents several examples of social experiments and the methods that have been used in each case to assess the impact of the program. It is organized as follows. Section 4.1 discusses an experiment that has been conducted at a messenger firm in Switzerland to examine labor supply increase in response to wage increase. Section 4.2 presents an experiment done within the context of job training programs in the U.S. Section 4.3 discusses an experiment that has been conducted at a telephone quitline in California to assess the effectiveness of the counseling services offered. Finally, Section 4.4 describes an experiment conducted to evaluate an anti-poverty program in Mexico.

4.1 Experiment 1: Labor Supply Increase in Response to Wage Increase

Fehr and Götte (2002) report the results of a study conducted by the Institute of Empirical Research in Economics at the University of Zurich to determine labor supply increase in response to wage increase. An experiment was conducted with bicycle messengers to examine how messengers would change the number of daily shifts and the effort per shift as a result of a temporary, anticipated, and exogenous wage increase. The bicycle messengers were normally paid a share of the daily revenues they generate (39% of revenues for males and 44% for females at Veloblitz). The experiment consisted of increasing the revenue share by 25%, and since the change was exogenous, it was safe to assume that wage change was not induced by unobserved supply or demand variations. Moreover, messengers normally had the freedom to choose the number of shifts they work a day and the effort per shift, and they were used to daily fluctuations in demand and consequently in earnings.

The study involved two messenger firms, Veloblitz and Flash, but the randomized field experiment was conducted only at Veloblitz where 58 messengers were employed. In order to participate in the experiment, messengers had to fill in at most four questionnaires (at the beginning and end of each treatment period). Of the 58 messengers, 45 participated in the experiment and 13 did not participate. Furthermore, of the 45 participants, one ceased to participate, so dropout in this experiment is not a major concern. The remaining 44 participants were randomly assigned to one of two groups, *A* or *B* (22 in each group). During treatment *A* (September 11 – October 6, 2000), members of group *A* received treatment (wage increase) while members of group *B* served as a control group. During treatment *B* (October 30 – November 24, 2000), members of group *B* received treatment (wage increase) while members of group *A* served as a control group. In addition to participants, a "field control group" was formed, and it consisted of messengers at the nonparticipating firm Flash and nonparticipating messengers at Veloblitz.

Records on messengers' deliveries (both at Veloblitz and Flash) were available for a pre-program period of around one and a half year. In addition, the behavior of participants and records on their deliveries were examined during the experimental period. However, participants did not know that their behavior was being observed (thus, there is no Hawthorne[1] effect) nor did they know that the purpose of the experiment was the study of labor supply behavior. If pressed, the evaluators would tell the messengers that the purpose of the study was to examine the relationship between wage increase and job satisfaction. At the same time, data available from the nonparticipating firm Flash, which operated in the same market as Veloblitz, were useful for investigating any effect that the experiment might have had on the control group at Veloblitz as well as for controlling for daily fluctuations in demand.

As cited by Fehr and Götte (2002), the following treatment effects were of interest:

- <u>The direct treatment effect</u>: is the impact of the wage increase on the treatment group's behavior relative to the experimental and field control group during both treatments.
- <u>The indirect treatment effect</u>: is the impact of the experiment on the behavior of *all* messengers at Veloblitz relative to all the bicycle messengers at Flash during the treatment periods.
- <u>The announcement effect</u>: is the impact of the announcement of the experiment on the participating messengers relative to all the other messengers.

A first indication of the direct treatment effect on the number of shifts is given by a direct comparison of the number of shifts between the treatment and control groups and the computation of an intertemporal elasticity of substitution with respect to shifts. However, it is noted that this direct comparison in computing elasticity might overstate the direct treatment effect due to a working hazard effect, which means that the probability of working a shift is a function of the number of days that have elapsed since the latest shift. To overcome this problem, a "survivor" function (which measures the share of messengers who have not worked for at least a given number of days) is used.

In addition, a Cox regression is performed to examine the determinants of shift in more detail, using the following specification:

$$\text{Prob}\left(i \text{ works today} \middle| \text{hasn't worked } T \text{ days}\right) = \exp\left(\alpha x_{it} + \gamma Treat_{it}\right)\psi_i(T), \tag{4.1}$$

[1] The Hawthorne effect means that subjects change their behavior just because they know that their behavior is being observed.

where x_{it} is a set of control variables, $Treat_{it}$ refers to the treatment variables discussed above, and $\psi_i(T)$ is a function that indicates the baseline probability of working a shift if the messenger hasn't worked for T days (it does not need to be specified in the Cox regression). The regression results indicate a positive and significant direct treatment effect, an insignificant indirect treatment effect, and a positive and significant announcement effect. The main conclusion is that messengers increase their number of shifts when their wage increases.

Similar analyses are performed to assess the impact of the wage increase on the level of effort (revenues) per shift. Unlike the number of shifts, the effort per shift decreases with the increase in wage. This is explained by a loss aversion, reference dependence model, where "workers who temporarily earn higher wages are more likely to exceed the reference income level and, hence, their marginal utility of income is low, inducing them to provide less effort."

4.2 Experiment 2: Job Training Programs

Job training programs in the United States have been evaluated extensively using experimental and nonexperimental methods. This section is based on research conducted in Heckman et al. (1999 a), Heckman and Smith (1999 b), and Heckman et al. (1998 a) as related to the Job Training Partnership Act (JTPA) program.

Four samples were used in the evaluation. An experimental treatment group (two thirds of participants) and an experimental control group (one third of participants; randomized out for 18 months) consisted of persons randomly assigned at four training centers. In addition, two comparison groups were used in nonexperimental evaluations. The first comparison group consisted of eligible nonparticipants (ENP) who chose not to participate. The ENP sample was drawn from the same geographic areas and labor market conditions as participants. The second comparison group was sampled from the 1986 national Survey of Income and Program participation (SIPP) sample.

Several surveys were administered. A long baseline survey (LBS) collected five years of retrospective data on earnings, demographics, etc. for ENP and control group samples. A first follow-up survey was administered to treatment and control group members, as well as to the ENP sample, and covered the period 12 to 24 months after random assignment for the experimental groups and 12 to 48 months after the long baseline survey for the ENP sample. A second follow-up survey was administered to a random sample of the experimental groups and covered the period 24 to 48 months after random assignment. In addition, a background information form was filled by treatment and control group members at the time of random assignment.

Heckman and Smith (1999 b) develop a model of program participation, and based on that, they investigate the performance of a matching estimator and a nonparametric conditional difference-in-differences estimator. In their data, the two estimators they consider reduce selection bias substantially but do not eliminate it because the estimators are based on "selection on observables." In Heckman et al. (1998 a), three nonexperimental estimators are tested: matching, the classical selection bias model with index sufficiency, and a conditional difference-in-differences estimator. In their data, matching reduces but does not eliminate the bias, and the authors refer to other research where matching might increase the estimated bias for some sets of conditioning variables. Their data are consistent with the index sufficiency assumption underlying the classical selection bias model and the assumptions underlying the conditional difference-in-differences estimator.

Lessons learned from evaluating job training programs could be summarized as follows (Heckman et al., 1999 a): 1) Heterogeneity of program impacts affects the choice of an estimator, 2) An estimator should be chosen based on the economics of the problem, the available data, and the evaluation question being addressed, 3) Better data help a lot, as selection bias arises from missing data on the common factors affecting participation and outcomes, 4) It is important to compare comparable people, 5) Different methods for evaluating program impacts produce the same estimates only if there is no problem of selection bias, 6) It is important to administer the same questionnaire to trainees and comparison group members and to place them in the same local labor market and geography, 7) Experimental methods can evaluate the effect of a program for which there are few good substitutes (there should be no disruption of other established procedures), 8) Programs implemented at a national or regional level affect both participants and nonparticipants, 9) When modeling participation in training, it may be important to account for not only individual incentives but also those of the program operators, and 10) The choice of a nonexperimental estimator should be guided by knowledge of the determinants of program participation.

4.3 Experiment 3: Telephone Quitline for Smokers

Zhu et al. (2002) report the results of an experiment conducted at the California Smokers' Helpline to evaluate the effectiveness of the counseling services offered by the quitline. An experiment was conducted from July 11, 1995 to November 4, 1996 and involved 3282 participants who were smokers. Callers were recruited based on the following eligibility criteria: they had to be ready to quit smoking, they wanted counseling, and they agreed to be evaluated.

Callers were assigned to a treatment or a control group only when the number of callers requesting counseling exceeded the quitline's capacity to provide it. As such, 1973 callers (60% of participants) were randomly assigned to treatment and 1309 callers (40% of participants) to control. The treatment consisted of administering counseling sessions for a period of 3 months after initial contact. Furthermore, the control group was divided into two subgroups: control subgroup *A* that consisted of control group members calling back and receiving counseling; and control subgroup *B* that consisted of control group members who did not call back and thus did not receive counseling. This categorization of the control group has the following implication. If the treatment group had not received counseling, it would have been divided into two subgroups: treatment subgroup *A* consisting of those who would have called back to ask for counseling; and treatment subgroup *B* consisting of those who would not have called back to ask for counseling. Therefore, control subgroup *B* and treatment subgroup *B* were equivalent except with respect to service. Consequently, control subgroup *B* served as a control for treatment subgroup *B* in the determination of the effect of counseling.

Follow-up interviews were conducted 2, 4, 7, and 13 months after initial contact. Participants for whom there were no follow-up data were excluded from some analyses. Furthermore, it is indicated that only 72.1% of treatment group members actually received counseling. The analysis, therefore, is an intention-to-treat analysis.

The analysis consisted first of comparing participants to nonparticipants in terms of baseline characteristics to test for any significant differences. Then, experimental treatment and control groups were directly compared at two levels: the whole treatment group (subgroups *A* and b) was compared to the whole control group (subgroups *A* and *B*), and treatment subgroup *B* was compared to control subgroup *B*. The measures used to assess the effectiveness of the program were: rates of prolonged abstinence, rates of quitting attempts, and probability of relapse. The results of the analyses were overall promising in terms of the effectiveness of the quitline's counseling services.

4.4 Experiment 4: The Case of Progresa

Parker and Teruel (2003) report the results of a study conducted to assess the effectiveness of the Mexican anti-poverty program, Progresa, which offers education, health, and nutrition benefits. The program currently serves rural and urban communities, but the evaluation was done at the rural level only at which there is no element of self-selection. At the rural level, beneficiary families are selected into the program through a three-stage targeting mechanism. First, geographic targeting is used to select poor regions or communities. Second, a survey of socioeconomic conditions is conducted to choose households in the selected communities. Finally, agreement is reached among all families in a community on the list of selected households.

The experiment designed to evaluate Progresa was designed to achieve randomization at the community level but not at the household level. Therefore, the evaluation is somewhere between a randomized experiment and a quasi experimental evaluation. A subset of communities eligible to receive Progresa benefits were randomly assigned to a treatment (320 communities) or control group (186 communities). The experiment lasted only for a year and a half whereby the treatment beneficiary households began to receive benefits in May 1998 and control households began to receive benefits in December 1999. Five surveys were conducted throughout: an initial survey to identify beneficiary households (October – November 1997), a baseline survey approximately one month before treatment, and three follow-up surveys (November 1998, May 1999, and November

1999). Furthermore, an important feature of the evaluation design consisted of interviewing all households (including nonparticipants) in a treatment and control community. This has the following two advantages: 1) verifying that nonbeneficiary households living in Progresa communities were not subjected to spill-over aspects, and 2) the potential to use nonbeneficiary households to serve as a control group, particularly in the period after which the control group is incorporated to receive benefits.

At the community level, treatment and control groups appeared to be random since randomization was performed at this level. However, some significant differences in pre-program characteristics between the treatment and control groups were observed at the individual level. Therefore, experimental methods were not used to assess impacts. Instead, nonexperimental methods were employed, including: 1) regression methods with control variables, as opposed to simply comparing mean values between the treatment and control groups, and 2) double difference methods (or cross-section estimators in some cases), using an outcome equation applied only to eligible households/individuals, and impacts were allowed to vary over time.

The evaluation of this program provides several valuable lessons, some of which might be extended to various social programs. First, it is unlikely that randomized designs of social program evaluations last for a long period of time, especially if control group members do not receive any benefits. Second, if a program offers the same package (treatment) to all beneficiaries (participants), it is difficult to isolate the effect that the different components may have on outcomes. For example, in the case of Progresa, it was difficult to isolate the effects on schooling caused by increased educational benefits from those caused by increased health benefits. Third, nonrandom attrition between treatment and control groups could dissipate the benefits of conducting a randomized experiment, and requires information about the whereabouts of the movers to conduct a bias-free analysis of the evaluation of the program. In the case of Progresa, where 16.01% of households and 21.89% of individuals originally interviewed in the fall of 1997 were no longer in the sample by the end of November 2000, attrition was not taken into account in the analyses, and this constituted a serious defect as cited by the authors. Finally, the nature of some social programs produces incentives for reporting bias, and this bias should be taken into account.

5.0 Conclusions

This report has presented the evaluation problem that arises in the context of social programs and provided an overview of methods commonly employed to solve it.

The evaluation problem arises because of the inability to observe the same person in the treated and untreated states at the same time, and hence, the inability to compute the gain for any individual. Experimental and nonexperimental methods have been employed to solve the evaluation problem.

Experimental methods estimate the mean impact of treatment on the treated by directly comparing mean outcomes between an experimental treatment group and an experimental control group. Under ideal conditions, they provide an unbiased estimate of the mean program impact. If randomization intro the treatment and control groups is not done properly, if control group members find close substitutes to the treatment elsewhere, if there is nonrandom attrition between treatment and control group members, or if treatment group members drop out of the program after receiving partial treatment, nonexperimental methods should be used to estimate program impacts. A discussion of attrition models and methods to correct for attrition bias have been presented in the appendix.

Nonexperimental or econometric methods are those methods that rely on functional form specifications of the outcome and participation decision processes, as well as on the use of microdata sources, to estimate program impacts. The outcome from a comparison group of eligible nonparticipants is normally used to approximate the no-treatment outcome of participants. This report has discussed the underlying assumptions, biases, and approaches associated with several nonexperimental estimators, including the before-after estimator, the difference-in-differences estimator (and the conditional version of it), the cross-section estimator, the matching estimator, the index sufficient method, and the method of instrumental variables.

Examples of social programs that have been evaluated in practice, together with the evaluation methods and lessons learned from the evaluations, have also been presented.

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Appendix A: Attrition Bias

This appendix discusses the problem of attrition bias in longitudinal surveys and social experiments. It is organized as follows. Section A.1 defines the problem posed by attrition. Section A.2 discusses ways in which attrition bias can be detected, and Section A.3 presents some methods that have been traditionally used to correct for attrition bias.

A.1 The Problem Posed by Attrition

Attrition is a problem in panel surveys, where individuals are followed over time, and in the context of social experiments. It refers to the loss of sample members with a subsequent loss of data on the attriters, and is caused by factors such as change of residence or job, loss of interest, or the inadequacy of treatment benefits received. (Hausman and Wise, 1979).

Attrition can bias a sample in two ways. First, it might threaten the external validity of the study if the characteristics of the sample are altered in a way that makes it no longer representative of the original sample. Second, it might threaten the internal validity of the study if the correlations between variables in the remaining sample are different from the true correlations in the original sample (Miller and Wright, 1995). In social experiments with random design, attrition not only affects the representativeness of the remaining sample of nonattriters but also negates the randomization of the experiment (Grasdal, 2001).

In the context of social experiments, the parameter of interest most often discussed in the literature is the effect of treatment on the treated. Let D denote participation in the experiment, where D is equal to 1 for a participant (treatment or control groups) and zero otherwise. Let R represent treatment, where R is equal to 1 if a participant is subjected to treatment and zero otherwise. X is a vector of observed characteristics. Let P_T and P_C denote the probability of attrition among members of the treatment group and control group, respectively. A represents attrition, where A is equal to 1 if a participant does not attrit from the sample (stays at follow-up) and is equal to zero otherwise. In the absence of attrition, the effect of treatment on the treated can be defined as:

$$E(\Delta|X, D=1) = E(Y \mid X, D=1, R=1) - E(Y \mid X, D=1, R=0). \tag{A.1}$$

In the presence of attrition, the mean outcomes in the treatment and control group can be expressed as follows (this assumes that the attriters from the treatment group drop out after receiving treatment but prior to filling out the follow-up survey):

$$E(Y \mid X, D=1, R=1) = P_T E(Y \mid X, D=1, R=1, A=0) + (1-P_T) E(Y \mid X, D=1, R=1, A=1), \tag{A.2}$$

and $$E(Y \mid X, D=1, R=0) = P_C E(Y \mid X, D=1, R=0, A=0) + (1-P_C) E(Y \mid X, D=1, R=0, A=1). \tag{A.3}$$

Thus, the effect of treatment on the treated, in the presence of attrition, can be expressed as:

$$\begin{aligned} E(\Delta | X, D=1) &= E(Y \mid X, D=1, R=1) - E(Y \mid X, D=1, R=0) \\ &= P_T E(Y \mid X, D=1, R=1, A=0) + (1-P_T) E(Y \mid X, D=1, R=1, A=1) \\ &- P_C E(Y \mid X, D=1, R=0, A=0) - (1-P_C) E(Y \mid X, D=1, R=0, A=1). \end{aligned} \tag{A.4}$$

Therefore, we have:

$$\begin{aligned} E(\Delta | X, D=1) &= E(Y \mid X, D=1, R=1, A=1) - E(Y \mid X, D=1, R=0, A=1) \\ &+ P_T [E(Y \mid X, D=1, R=1, A=0) - E(Y \mid X, D=1, R=1, A=1)] \\ &+ P_C [E(Y \mid X, D=1, R=0, A=1) - E(Y \mid X, D=1, R=0, A=0)]. \end{aligned} \tag{A.5}$$

If attrition is nonrandom and is related to the endogenous variables, the expressions inside the square brackets (i.e., the difference in outcomes between attriters and nonattriters, for each of the treatment and control groups) will not be equal to zero. Consequently, estimating $E(\Delta | X, D=1)$ from data on nonattriters only will lead to biased results.

A.2 Detecting Attrition Bias

To test for attrition in a sample, it is common to use t-tests to compare the means of certain characteristics for the full sample and for those who dropped out from the sample. Attrition bias is implied by significant differences in the means of one or more of the variables. Another method is to use a binary choice model, such as logit, to estimate the probability of attrition. Statistically significant coefficient estimates of the independent variables indicate that those variables were important determinants of the decision to stay in the sample, and could be an indicator of attrition bias.

Moreover, attrition bias in the relationships between variables can be assessed through a test of the invariance of the correlation matrices between two samples: those individuals that stayed in the sample and those that dropped out (Miller and Wright, 1995).

Other methods for detecting attrition bias rely on statistical tests that can be performed once a correction method has been applied, as discussed further below.

A.3 Correcting for Attrition Bias

A.3.1 Models of Outcome and Attrition

Consider an outcome equation of the form:

$$Y_i = \alpha W_i' + \delta R_i + \varepsilon_i = \beta X_i' + \varepsilon_i, \tag{A.6}$$

where δ is the effect of the treatment, X_i represents observed characteristics for individual i (it includes the treatment dummy variable R_i), ε_i represents unobserved characteristics affecting outcome, β is a vector of coefficients measuring the impact of X_i on outcome. The purpose is to obtain consistent estimates of β.

Outcome Y_i is observed if individual i is a nonattriter (i.e., $A_i = 1$), where A_i is commonly described by a latent index structure of the form:

$$A_i^* = \gamma' Z_i + \nu_i$$
$$A_i = \begin{cases} 1 & \text{if } A_i^* > 0 \\ 0 & \text{otherwise,} \end{cases} \tag{A.7}$$

where Z_i represents a vector of observed characteristics affecting the attrition decision, and ν_i represents unobserved characteristics.

Estimating the coefficients β in expression (A.6) through ordinary least squares applied to the sample of nonattriters leads to biased estimates since:

$$E(Y_i \mid Z_i, D=1, A=1) = \beta X_i' + E(\varepsilon_i \mid Z_i, D=1, A=1), \tag{A.8}$$

and $E(\varepsilon_i \mid Z_i, D=1, A=1) \neq 0$ due to the possible correlation between the unobserved characteristics ε in the outcome equation and the unobserved characteristics ν in the attrition equation.

Various methods have been proposed in the literature to correct for attrition bias. Some of these methods are parametric (i.e., they require specific distributional assumptions about the error terms in the outcome and attrition equations), some are semi-parametric, and some are nonparametric. For example, in Grasdal (2001), where a rehabilitation program is evaluated through a randomized field trial that is significantly affected by attrition, the following sample selection estimators are considered for the case of a continuous outcome: the parametric (Heckman) two-step and maximum likelihood estimators, the semi-parametric two-step series estimators using weighted and unweighted probit index, and the semi-parametric two-step series with the Manski maximum score index. Below we present some of those methods along with their assumptions. The reader is also referred to Vella (1998). It is important to emphasize that the models presented in Sections A.3.2 and A.3.3 are based on one cross-section of a panel, and that the estimators are inconsistent if both state dependence (i.e., the response behavior of an individual in a given time period conditional on the response in previous time periods) and unobserved heterogeneity are present in the attrition process (Nijman and Verbeek, 1992). In Section A.3.4, we present one model with two time periods.

A.3.2 Maximum Likelihood Estimation

A.3.2.1 Parametric Maximum Likelihood

Heckman (1974) proposed a maximum likelihood estimator whose main assumption is that the error terms in the outcome equation and the attrition equation are independently and identically normally distributed, stated formally as:

Assumption 1: ε_i and ν_i are independently and identically distributed $N(0,\Sigma)$, where $\Sigma = \begin{pmatrix} \sigma_\varepsilon^2 & \sigma_{\varepsilon\nu} \\ \sigma_{\varepsilon\nu} & \sigma_\nu^2 \end{pmatrix}$, and (ε_i, ν_i) are independent of Z_i.

Under Assumption 1, the parameters of the model (A.6) – (A.7) can be estimated by maximizing the following average log likelihood function (Vella, 1998):

$$L = \frac{1}{N}\sum_{i=1}^{N}\left\{ A_i * \ln\left[\int_{-(Z_i'\gamma)}^{\infty} \phi_{\varepsilon\nu}(Y_i - X_i'\beta, \nu)\, d\nu\right] + (1-A_i) * \ln\left[\int_{-(Z_i'\gamma)}^{\infty}\int_{-\infty}^{\infty} \phi_{\varepsilon\nu}(\varepsilon,\nu)\, d\varepsilon\, d\nu\right]\right\}, \tag{A.9}$$

where N is the entire sample size and $\phi_{\varepsilon\nu}$ is the probability density function for the bivariate normal distribution.

The parameter estimates due to the maximum likelihood estimation are fully efficient, provided that the model is correctly specified. Some loss in efficiency might result if the parametric assumptions are relaxed.

While Heckman's maximum likelihood procedure imposes normality of the error terms, Lee (1982, 1983) suggests a method which assumes that ε_i and ν_i are drawn from known distributions $F(\varepsilon)$ and $F(\nu)$, not necessarily normal. In this method, ε_i and ν_i are transformed into normal errors, which are then included in the likelihood equation (Vella, 1998).

A.3.2.2 Semi-Parametric Maximum Likelihood

Semi-parametric methods impose no restrictions on the distributions of the error terms in the outcome and attrition equations. For example, Gallant and Nychka (1987) suggest approximating the true joint density of ε and ν through the following expression:

$$b_{\varepsilon\nu} = \left(\sum_{k=0}^{K} \sum_{j=0}^{J} \pi_{kj} \varepsilon^k \nu^j \right) \phi_\varepsilon \phi_\nu , \tag{A.10}$$

where ϕ_ε and ϕ_ν are normal densities for ε and ν, respectively, and π_{kj} represents unknown parameters. This method results in consistent estimates of β and γ under the condition that the number of approximating terms tends to infinity as the sample size increases.

A.3.3 Two-Step Estimation

A.3.3.1 The Parametric Heckman Two-Step Procedure (1979)

Two-step estimators are frequently used in sample selection models. We present below Heckman's (1979) two-step estimator.

Recall that the conditional expectation of outcome is given by expression (A.8):

$$E(Y_i \mid Z_i, D=1, A=1) = \beta X_i' + E(\varepsilon_i \mid Z_i, D=1, A=1), \tag{A.8}$$

and $E(\varepsilon_i \mid Z_i, D=1, A=1) \neq 0$. The basic idea of Heckman's two-step estimator is to obtain an estimate of $E(\varepsilon_i \mid Z_i, D=1, A=1)$ and include it as a regressor in expression (A.6).

Heckman's method assumes that the error terms in the outcome and attrition equation are jointly normally distributed (i.e., that Assumption 1 is satisfied). It can be shown that:

$$E(\varepsilon_i \mid Z_i, D=1, A=1) = \frac{\sigma_{\varepsilon\nu}}{\sigma_\nu^2} \left\{ \frac{\phi(Z_i'\gamma)}{\Phi(Z_i'\gamma)} \right\}, \tag{A.11}$$

where $\frac{\phi(Z_i'\gamma)}{\Phi(Z_i'\gamma)}$ is referred to as the inverse Mills ratio, and ϕ and Φ represent the probability density function and cumulative distribution function, respectively, of the standard normal distribution. The following steps can be followed to produce consistent estimates of β:

1. Run a maximum likelihood Probit (since the error term v_i was assumed to be normally distributed) on the attrition model (7), using all observations (attriters and nonattriters). This would allow the estimation of γ. A common assumption that is made is $\sigma_\nu = 1$.

2. Construct an estimate of the inverse Mills ratio:

$$\hat{\lambda}_i = \frac{\phi(Z_i'\hat{\gamma})}{\Phi(Z_i'\hat{\gamma})}. \tag{A.12}$$

3. Include $\hat{\lambda}_i$ as a regressor in the outcome equation, as follows:

$$Y_i = \beta X_i' + \mu\hat{\lambda}_i + \eta_i\,, \tag{A.13}$$

where μ is equal to $\frac{\sigma_{\varepsilon\nu}}{\sigma_\nu^2}$ (see expression (A.11)), and η_i is an error term with zero mean and is uncorrelated with X_i and $\hat{\lambda}_i$.

4. Run ordinary least squares on expression (A.13) to obtain consistent estimates of β and μ.

To test for the presence of attrition bias, one can do a t-test on the null hypothesis that μ (and consequently $\sigma_{\varepsilon\nu}$) is zero.

There are several concerns associated with the Heckman two-step procedure. First, the standard errors estimated from OLS (Step 4 above) should be adjusted to account for the first step estimation of the inverse Mills ratio. Second, there could be a potential identification problem in the estimation of β and μ in Step 4 above if all the variables in Z_i (attrition equation) are also in X_i (outcome equation), i.e., if there are no exclusion restrictions, since the inverse Mills ratio is linear for certain ranges of the index $Z_i'\gamma$.

A.3.3.2 Relaxing Some Distributional Assumptions

The bivariate normal distribution assumption (Assumption 1) is sometimes relaxed to the following assumption:

<u>Assumption 2</u>: The distribution of ν_i is known and ε_i is a linear function of ν_i.

Olsen (1980) proposes a method which assumes that ν_i is uniformly distributed, thus allowing the estimation of the parameters of the attrition equation by methods other than Probit. The method also requires exclusion restrictions (i.e., that at least one variable appears in Z_i but not in X_i). Olsen (1980) shows that the two-step estimator is consistent and that: $E[\varepsilon_i | Z_i, A_i = 1] = \rho\sigma_\nu (3)^{1/2}(Z_i'\gamma - 1)$

$$E[\varepsilon_i | Z_i, A_i = 1] = \rho\sigma_\nu (3)^{1/2}(Z_i'\gamma - 1), \tag{A.14}$$

where ρ is the correlation coefficient between ε and ν.

A.3.3.3 Semi-Parametric Methods

Semi-parametric methods do not rely on restrictive distributional assumptions for the error terms in the outcome and attrition equations. Assumption 3, known as an index restriction, is normally used to replace Assumption 2:

Assumption 3: $E[\varepsilon_i \mid Z_i, A_i = 1] = g(Z_i'\gamma)$, where g is an unknown function.

Thus, the conditional expectation of the outcome can be written as:

$$E[Y_i \mid Z_i, A_i = 1] = X_i'\beta + g(Z_i'\gamma). \quad \text{(A.15)}$$

The difficulties associated with invoking this assumption are that: 1) the parameter γ can no longer be estimated by assuming a specific distribution of v_i, and 2) the estimation of $E[\varepsilon_i \mid Z_i, A_i = 1]$ cannot rely on distributional assumptions.

To solve the first difficulty, several semi-parametric and nonparametric procedures have been suggested to estimate the binary choice model of attrition (see for example Cosslett (1983), Gallant and Nychka (1987), and Newey (1988)), and consequently the index $Z_i'\gamma$. Heckman and Robb (1985) suggest estimating $g(Z_i'\gamma)$ through a Fourier expansion in terms of $\Pr[A_i = 1 \mid Z_i]$. Newey (1988) estimates $g(Z_i'\gamma)$ as:

$$\hat{g}(Z_i'\gamma) = \sum_{k=1}^{K} \alpha^k (Z_i'\hat{\gamma})^{k-1}, \quad \text{(A.16)}$$

where K is the number of terms in the approximating series.

Ichimura and Lee (1991) propose an iterative nonlinear least squares approach. From expression (A.15), we have:

$$E[(Y_i - X_i'\beta) \mid Z_i'\gamma] = g(Z_i'\gamma). \quad \text{(A.17)}$$

The method first uses estimates of β and γ to estimate $g(.)$ nonparametrically using expression (A.17). Then, β and γ can be reestimated using expression (A.15) and the estimate of $g(.)$, and so on. This method results in consistent and asymptotically normal estimates of β and γ.

In contrast to parametric methods that typically do not require exclusion restrictions (i.e., variables in Z_i that are not in X_i), semi-parametric methods require exclusion restrictions.

A.3.4 Hausman's Maximum Likelihood Method

In Hausman and Wise (1979), an analysis of attrition bias is conducted by considering two time periods. Individuals that attrit from the sample are observed in the first time period but not in the second. The authors provide a mechanism that can be used to analyze cases where there are more than two time periods. The model that they consider is:

$$\begin{aligned} Y_{i1} &= \beta X_{i1} + \varepsilon_{i1} \\ Y_{i2} &= \beta X_{i2} + \varepsilon_{i2} \\ A_i &= \gamma Z_i + \nu_i, \end{aligned} \tag{A.18}$$

where Y_{i1} and Y_{i2} represent outcomes in the first and second period, respectively, and all error terms are normally distributed. Moreover, the error terms in the outcome equations are assumed to consist of an individual effect and a time effect. The authors use a maximum likelihood procedure to estimate asymptotically efficient and consistent parameters, and to test the statistical significance of the correlation between ν_i and ε_{i2} as a test for attrition bias. First, the joint density of attrition and the outcomes in both periods is computed as follows:

$$f\left(A_i = 1, Y_{i1}, Y_{i2}\right) = \Pr\left[A_i = 1 \mid Y_{i1}, Y_{i2}\right] f\left(Y_{i2} \mid Y_{i1}\right) f\left(Y_{i1}\right). \tag{A.19}$$

Next, the joint density of attrition and the outcome in the first period is computed as follows:

$$f\left(A_i = 0, Y_{i1}\right) = \Pr\left[A_i = 0 \mid Y_{i1}\right] f\left(Y_{i1}\right). \tag{A.20}$$

Using expressions (A.19) and (A.20), a log likelihood function is defined.

Hausman and Wise (1979) include the outcome in period 2 as an independent variable in the attrition equation (thus Z_i contains all variables in X_i) but note that Z_i need not include variables besides those that are in X_i, i.e., exclusion restrictions are not needed for identification.

The method is applied to data from an experiment to obtain estimates of potential labor supply and earnings responses to possible income maintenance plans. Their analysis reveals that using the structural model, some attrition bias was present though not substantial to affect the experimental result greatly. On the other hand, estimates obtained from performing simple analysis-of-variance techniques seemed to be greatly affected by attrition bias.

Appendix B

Recruitment Survey

Minnesota Department of Transportation Driving Study

Experiment Recruitment Survey Script, February 20, 2004

1. Hello, my name is ___________, and I'm calling from MarketLine Research on behalf of the Minnesota Department of Transportation. Mn/DOT (pronounced "mindot") is continuously seeking more effective and efficient ways to deliver services; including ways of managing congestion. We are conducting a short survey to better understand how the costs of owning automobiles affects how people drive in the Twin Cities metropolitan area. This information will assist Mn/DOT in determining if there are alternatives that could help reduce congestion and mitigate environmental impacts on our highways. We would like to enlist your help in this market research study.

Screening

2. Do you or does anyone in your household work for…

...The Minnesota Department of Transportation?	YES	NO	***If YES, THANK/TERMINATE***
…An automobile dealership?	YES	NO	***If YES, THANK/TERMINATE***
…An automobile insurance provider or agency?	YES	NO	***If YES, THANK/TERMINATE***
An auto leasing company or financial institution?	YES	NO	***If YES, THANK/TERMINATE***
…A marketing research firm?	YES	NO	***If YES, THANK/TERMINATE***
…A newspaper, radio or TV station?	YES	NO	***If YES, THANK/TERMINATE***
…The Metropolitan Council?	YES	NO	***If YES, THANK/TERMINATE***
…A city or county public works department?	YES	NO	***If YES, THANK/TERMINATE***

3. How many members of your household have a valid driver's license (including yourself)?

Record number with license: ___________________ ***{If no drivers, Thank/Terminate, else Q. 4}***

4. In what county do you live?

Anoka	**{Q. 5}**
Ramsey	**{Q. 5}**
Dakota	**{Q. 5}**
Hennepin	**{Q. 5}**
Chisago	**{Q. 5}**
Washington	**{Q. 5}**
Scott	**{Q. 5}**
Carver	**{Q. 5}**

If Other__________________ ***THANK/TERMINATE***

5. How long have you lived in the Twin Cities area? (If less than one year, enter number of months)

Years: __________________

OR

Months: __________________ ***If less than six months, Thank/Terminate***

{Otherwise, T1}

Travel Behavior

T1. For getting about in the Twin Cities metro area, what is your primary mode of transportation?

Car	( )	
Car pool	( )	
Bus	( )	
Taxi	( )	
Other	( )	[SPECIFY]

T2a. Do you commute either to work or school during the week on a regular basis? [IF BOTH ASK: Which do you do most often?]

Work	( )	
School	( )	
Both	( )	
No	( )	[SKIP TO T6]

T3. Do you regularly commute in the mornings between 6:00 a.m. and 9:00 a.m.?

Yes	( )
No	( )

T4. Do you regularly commute in the afternoons between the hours of 3:00 p.m. and 6:30 p.m.?

Yes ( )
No ( )

ASK EVERYONE

T6. Do you sometimes use a major highway or freeway as part of your trip route? By freeway, we mean highways, such as 35W, 94, 494, 62 Crosstown, etc.

Yes ( )
No ( )

T7. Thinking of the past three years, do you think that the level of congestion on metro area roadways has increased, decreased, or stayed the same?

Increased ( )
Decreased ( )
Stayed same ( )
Don't know ( )

T8. Please describe your level of tolerance for this congestion. Using a scale of 1 to 10 where "1" means you really don't mind and "10" means that the congestion is intolerable; what number represents how you feel about the congestion you experience <u>today</u>?

Don't mind									Intolerable	[DON'T OFFER]
1	2	3	4	5	6	7	8	9	10	Don't know

ASK Q56 ONLY IF "WORK" OR "SCHOOL" SELECTED IN QT2a.

56. Do you or any other household members need to pay for parking at or near your workplaces?

Yes	1	***{Q. 57}***
No	2	***{Q. 57}***

ASK EVERYONE

57. How convenient is public transit service to your household location? Would you say it is…

…Very convenient?	1	***{Q. 58}***
…Somewhat convenient?	2	***{Q. 58}***
…Somewhat inconvenient?	3	***{Q. 58}***
…Very inconvenient?	4	***{Q. 58}***

Vehicle Inventory

Q. In this survey we are trying to speak with households using a variety of vehicle types and number of miles driven that best reflects the State's population. My next series of questions deal with the number and types of vehicles used by members of your household.

6. How many registered motor vehicles and light trucks does your household have available? This includes all cars, vans, pickup trucks, RVs, SUVs, and motorcycles that are owned by you, leased to you, or provided by an employer.

Record vehicles: ___________________ ***If zero vehicles, four or more vehicles, or DK/ RF, Thank/Terminate***

ELSE, {Q.7}

7. How many cars, vans, pickup trucks, or SUVs are there in your household that are model year 1996 or newer?

Record vehicles: _______________ **{If none, Thank/Terminate/ Otherwise, Q. 8}**

8. In the past seven days, would you say that [((*IF Q6:AUTOS=1*:) this vehicle was driven)((*IF Q6:AUTOS>1*:) these vehicles altogether were driven)] more or less than a total of 100 miles? [check one box]

Driven more than 100 miles	1	***{Q. 9}***
Driven less than 100 miles	2	***{Thank/Terminate}***

9. Do you or does anyone else in your household expect one of the following events to occur in the coming year? [READ LIST]

Move to a new address	***1. If yes, Thank/ Terminate***
Add a second car that you will drive fairly often	***2. If yes, Thank/ Terminate***
Stop working or retire	***3. If yes, Thank/ Terminate***

10. In total, including yourself, how many people live in your household?

Record number in household: ___________________ ***{Q. 11}***

11. Which of the following best describes your employment status? [check one box]

Do you work full-time?	***YES***	***NO***
Do you work part-time?	***YES***	***NO***
Are you not employed outside the home?	***YES***	***NO***
Are you retired?	***YES***	***NO***
Are you a student?	***YES***	***NO***
Refused		

15. IF Q6-AUTOS=1, ask: Now, I would like to ask you about your vehicle. What is the body type of the vehicle? Is it an…[check one box]

If Q6-AUTOS=2 or 3, ask: Now, I would like to ask you about each of your <Q5-AUTOS> vehicles, starting with the newest vehicle. What is the vehicle's body type?

…Auto	1	***{Q. 16}***
…Van	2	***{Q. 16}***
…Recreational Vehicle (RV)	3	***{Q. 16}***
…Sport Utility Vehicle (SUV)	4	***{Q. 16}***
…Pick-up Truck	5	***{Q. 16}***
…Other Truck	6	***{Q. 16}***
…Motorcycle	7	***{Q. 16}***
…Other	8	***{Q. 15A}***

15A. Other, specify

Record body type: ___________________ ***{Q. 16}***

16. What is the model year of this vehicle?

Record 4 digit model year: ___________________ ***{Q. 17}***

17. Is this vehicle …[check one box]

…Owned by a household member?	1	***{Q. 18}***
…Leased by a household member?	2	***{Q. 18}***
…Owned by a person not in your household?	3	***{Q. 18}***
…Leased by a person not in your household or by an employer?	4	***{Q. 18}***
DK/RF	9	***{Q. 18}***

13. What is the vehicle's make or manufacturer?

Acura	1	***{Q.14}***
Audi	2	***{Q.14}***
BMW	3	***{Q.14}***
Buick	4	***{Q.14}***
Cadillac	5	***{Q.14}***
Chevrolet	6	***{Q.14}***
Chrysler	7	***{Q.14}***
Dodge	8	***{Q.14}***
Ford	9	***{Q.14}***
Geo	10	***{Q.14}***
GMC	11	***{Q.14}***
Harley Davidson	12	***{Q.14}***
Honda	13	***{Q.14}***
Hyundai	14	***{Q.14}***
Infiniti	15	***{Q.14}***
Isuzu	16	***{Q.14}***
Jaguar	17	***{Q.14}***
Jeep	18	***{Q.14}***
Kawasaki	19	***{Q.14}***
Kia	20	***{Q.14}***
Lexus	21	***{Q.14}***
Lincoln	22	***{Q.14}***
Mazda	23	***{Q.14}***
Mercury	24	***{Q.14}***
Mercedes	25	***{Q.14}***
Mitsubishi	26	***{Q.14}***
Nissan	27	***{Q.14}***
Oldsmobile	28	***{Q.14}***
Plymouth	29	***{Q.14}***
Pontiac	30	***{Q.14}***
Porsche	31	***{Q.14}***
Range Rover	32	***{Q.14}***
Saab	33	***{Q.14}***
Saturn	34	***{Q.14}***
Subaru	35	***{Q.14}***
Suzuki	36	***{Q.14}***
Toyota	37	***{Q.14}***
Volkswagen	38	***{Q.14}***
Volvo	39	***{Q.14}***
Yamaha	40	***{Q.14}***
Daewoo	41	***{Q.14}***
Other	97	***{Q. 13A}***

13A. Other, specify

Record make/manufacturer: ____________________ ***{Q. 14}***

14. And, what model is that vehicle?

Record model (DK/RF=XXX): __________________ ***{Q. 15}***

18. What year did your household acquire this vehicle?

Record 4 digit acquisition year: __________________ ***{Q. 19}***

19. What month of <Q18-Acquire Year> did you acquire this vehicle?

Record 2 digit acquisition month: __________________ ***{Q. 20}***

20. Was the vehicle new or used when you got it? [check one box]

New	1	***{Q. 21}***
Used	2	***{Q. 21}***

21. Approximately, how many miles does this vehicle currently have recorded on its odometer?

Record mileage: __________________ ***{Q. 22}***

22. *IF Q.20=1:New, skip to Q.23; else ask***:** Approximately how many miles did the vehicle have recorded on its odometer when you acquired it?

Record beginning __________________ ***{Q. 23}***

23. In the last 12 months, about how many miles was this vehicle driven?

Record last year mileage: __________________ ***{Q. 24}***

24. On a typical weekday, about how many miles is this vehicle driven during the afternoon peak traffic times between 3:00 p.m. and 6:30 p.m.?

Peak period miles: __________________ ***{Q. 25}***

25. (IF Q.3:Licensed=1, skip to Q.26; IF Q.3:Licensed>1, then ask:) Which of the following statements is the most accurate regarding this vehicle?

This vehicle is almost always driven by one particular household member	1	***{Q. 26}***
This vehicle is driven by more than one person, but is usually driven by one particular household member	2	***{Q. 26}***
This vehicle is driven in equal amounts by more than one household member	3	***{Q. 26}***

26-38 (IF Q6:AUTOS=1, SKIP to Q. 52, ELSE ASK:) Now, please think about the second newest vehicle that your household uses. (ASK Q. 13 – 25 for the second car)

39-51 (IF Q6:AUTOS=2, SKIP to Q. 52, ELSE ASK:) Now, please think about the oldest vehicle that your household uses. (ASK Q. 13 – 25 for the third car)

52a.

Q. Now I'm going to read you some brief statements regarding a Mn/DOT Driving Study that is designed to better understand how the costs of owning and operating motor vehicles affects how people drive in the Twin Cities area. As I mentioned previously, information from this study will assist Mn/DOT in determining if there are alternatives that could help reduce congestion and mitigate environmental impacts on our highways. We would like to enlist your help in this important driving study.

Q. We are seeking metro area drivers who would be willing to volunteer as participants in a 10-month driving study, that would include vehicle mileage reporting and periodic communications with the project team.

52b. Would you like to hear more about this study?

_______If yes (CONTINUE)

______If no (SKIP to Q 10/page 10)

Let me explain this driving study in more detail. As I mentioned, this 10 month study includes periodic communications and vehicle mileage reporting. To simplify this vehicle mileage reporting for you, study participants would agree to install a small mileage recorder into some or all of their vehicles. This device (also known as a CarChip) shows how many miles you drive, not where you drive, and it is placed below the dashboard out of sight. A Mn/DOT representative would schedule a visit to your home or a metro location of your choice (e.g., work) the first time to insert the CarChip into your vehicle's existing diagnostics port. Afterwards, you would need to replace these devices with new ones once or twice per month, record your odometer reading, and send the devices back to us in postage-paid envelopes we will provide. Plugging and unplugging these devices requires no tools or technical expertise.

Lastly, during the Driving Study there would be some periodic communications with you from Mn/DOT representatives. Most of the communication will be by e-mail if you have e-mail, though we may need to call you on the phone a few times.

In appreciation for your household's time and effort, we would offer you or the charity of your choice, $100.00. In addition, during this driving study, you would have some opportunities to receive additional monetary incentives up to $100.00 if you complete the study fully. It is important, that the CarChips be changed and sent to us in a timely manner for you to earn the incentive payment.

Mn/DOT will use the information gathered during the driving study to better understand people's driving choices and how people react to potential driving cost savings. All collected information will remain strictly confidential and anonymous. Results will not be reported by individual or household.

Would you be interested in helping Mn/DOT by participating in this study?

Yes	1	***{Q. 53}***
No	2	***{Q. 53}***

Demographics

Now I'm going to ask you some questions for classification purposes only. We ask these questions to ensure that we have collected opinions from a variety of people.

62. (IF Q11= FT, PT) Would you best describe your job as…? [READ LIST]

- ❑ Executive
- ❑ Administrative / managerial
- ❑ Sales
- ❑ Clerical
- ❑ Technical / scientific
- ❑ Production / manufacturing worker
- ❑ Maintenance
- ❑ Other
- ❑ Don't know, no opinion

Other type of job activity. [RECORD VERBATIM]

63. In which of the following categories does your age fall into. Please stop me when I get to the right range. [READ LIST, SELECT ONLY ONE]

Under 18	45 – 54	
18 – 24	55 – 64	
25 – 34	65 and older	
35 – 44	Refused	**{Q. 63}**

64. Which of the following categories best describes your last grade of school attended. [READ LIST] **{Q. 64}**

- ❑ Some High school or less
- ❑ High School graduate
- ❑ Tech school graduate
- ❑ Some College or technical school
- ❑ College graduate, or
- ❑ Post Graduate
- ❑ [DO NOT READ] Refused

65. Can you tell me what your 2003 total household income before taxes was? Please stop me when I get to the right range.

Under $20,000	$65,000 to $75,000
$20,000 to $35,000	$75,000 to $100, 000
$35,000 to $50,000	$100,000 or more
$50,000 to $65,000	Don't know/Refused

{Q. 65}

Q. Do you have access to the Internet at home or at work?

Yes

No

[IF ACCESS TO INTERNET ASK]

54. Do you have an e-mail address that you check regularly and to which we could send you communication about the Mn/DOT Driving Study?

Yes	**1 {Q. 53}**
No	**2 {Q. 53}**

[ASK ONLY IF Q53 = 1 (YES – AGREE TO PARTICIPATE)]

66. We will need to send you some materials explaining the project in detail. Could you please tell me your ….

Full Name: ____________________

Address: ____________________

E-mail Address: ____________________

Telephone Number: ____________________

What's the best time of day to reach you at this number? (weekday/weekend) ____________________

Could you give us an…

alternate person we can contact to reach you: ____________________

alternate telephone number at which we can reach you: ____________________

[READ IF Q53 = 1 (YES)] **{Q. 67}**

67a. Thank you for your participation in this survey. The MnDOT Project Manager will be in touch with you by mail in approximately one to two weeks with more details on the driving study.

[READ IF Q53 = 2 (NO – TO PARTICIPATE)]

67b. Thank you very much for your time and participation. That's all the questions I have.

68. [DO NOT READ] Record gender

Female Male

Appendix C

Stratification Analysis

Stratification Analysis

We will not have enough households to conduct detailed analyses of subgroups of households, but we still want to ensure that different subgroups are all represented in our experiment data, so that we can properly expand the data and generalize conclusions to the larger population. Ideally, we would like to define strata that are more homogenous than the overall population, so we want to divide our recruits up based on the characteristics that are likely to define their vehicle usage levels well. These predefined strata will be used for sample selection and expansion, but they do not limit our ability to consider a wider range of explanatory variables once we have the data. We used the National Household Travel Survey (N=681 for Minnesota) to better understand household driving behavior and to define the strata we would like to fill in the experiment.

Minnesota Household Auto Availability

Vehicles Available	Newer Vehicles	Older and Model Year Unknown Vehicles[1]	Percent of Households	Ineligible for Experiment	Eligible for All-Vehicles-Priced Experiment	Eligible for Newest-Vehicle-Priced Experiment
0	-	-	4.4%	4.4%		
1	1	0	12.5%		12.5%	12.5%
1	0	1	18.8%	18.8%		
2	2	0	10.7%		10.7%	10.7%
2	1	1	15.8%			15.8%
2	0	2	11.2%	11.2%		
3	3	0	1.2%		1.2%	1.2%
3	2	1	5.3%			5.3%
3	1	2	6.0%			6.0%
3	0	3	5.5%	5.5%		
4	-	-	4.8%	4.8%		
5	-	-	2.1%	2.1%		
6	-	-	1.1%	1.1%		
7	-	-	0.5%	0.5%		
8	-	-	0.1%	0.1%		
Total			**100.0%**	**48.5%**	**24.4%**	**51.5%**

Source: 2001-2002 National Household Travel Survey, December 2003 Microdata Release. (N=681 households; weighted results).

Notes:
[1] Since the survey was conducted in 2001-2002, older vehicles are defined as model year 1995 and earlier.

Roughly half of the households will be eligible to participate in a one-vehicle priced experiment. Only one-fourth will be eligible for the all vehicle experiment. It will be very difficult to find three vehicle households with three instumentable vehicles.

Average Annual Vehicle Miles Traveled for Minnesota Households

Vehicles Available	Newer Vehicles	Older and Model Year Unknown Vehicles[1]	Mean Annual Vehicle Mileage (All HH Vehicles)[2]	N
1	1	0	13,855	69
1	0	1	15,004	72
2	2	0	28,620	58
2	1	1	23,396	87
2	0	2	22,482	58
3	3	0	44,023	9
3	2	1	38,620	19
3	1	2	29,243	23
3	0	3	51,858	14

Source: 2001-2002 NHTS, December 2003 Microdata Release. (N=681 HHs; wtd)

Notes:
[1] Since NHTS was conducted in 01/02, older vehicles are model year 1995 and earlier.
[2] For households for which mileage could be estimated for all vehicles.

See comment below next table

Best Estimate of Annual Mileage for Vehicles In Minnesota Households[1]

Percentile	All Vehicles	Newer Vehicles	Older and Model Year Unknown Vehicles
10	2,500	5,215	1,500
20	4,790	7,120	3,610
30	6,700	9,020	5,420
40	8,125	10,680	6,770
50	10,215	12,410	8,190
60	12,280	15,020	10,500
70	14,740	17,280	12,625
80	18,000	20,200	15,670
90	22,840	24,970	21,040
95	29,270	31,200	25,500
98	41,700	41,800	41,600
Minimum	0	77	0
Maximum	145,000	64,100	145,000
Mean Mileage	12,681	14,460	11,541

Source: 2001-2002 NHTS, December 2003 Microdata Release. (N=681 HHs; wtd)

Notes:
[1] Best estimate is from the ORNL analysis of NHTS data.

Age and Usage of Minnesota Household Vehicles

Vehicles Available	Newer Vehicles	Older and Model Year Unknown Vehicles[1]	Vehicles that are Driven More than the Median Number of Miles Per Year	Vehicles that are Driven Less than the Median Number of Miles Per Year	Percent of Households
1	1	0	0	1	20.1%
1	1	0	1	0	19.8%
1	0	1	0	1	43.0%
1	0	1	1	0	17.1%
Total – 1 Vehicle					100.0%
2	2	0	0	2	6.8%
2	2	0	1	1	13.7%
2	2	0	2	0	7.7%
2	1	1	0	2	19.0%
2	1	1	1	1	16.7%
2	1	1	2	0	6.3%
2	0	2	0	2	15.6%
2	0	2	1	1	9.6%
2	0	2	2	0	4.5%
Total – 2 Vehicles					100.0%
3	3	0	0	3	1.2%
3	3	0	1,2	2,1	4.9%
3	3	0	3	0	0.5%
3	1,2	2,1	0	3	13.3%
3	1,2	2,1	1,2	2,1	48.4%
3	1,2	2,1	3	0	1.5%
3	0	3	0	3	13.1%
3	0	3	1,2	2,1	17.2%
3	0	3	3	0	0.0%
Total – 3 Vehicles					100.0%

Source: 2001-2002 NHTS, December 2003 Microdata Release. (N=681 HHs; wtd)

Notes:
[1] Since NHTS was conducted in 01/02, older vehicles are model year 1995 and earlier.
[2] For households for which mileage could be estimated for all vehicles.

Vehicle mileage differs by age of vehicle, so we should ensure that we get adequate representation from households with different vehicle age distributions. It will also be good to stratify the participants into groups based on their household characteristics, so we can seek to account for the background differences. Vehicle availability is an obvious choice. We had also previously assumed licensed drivers would capture household mileage differences well.

Minnesota Drivers in Households

Vehicles Available	Drivers in Household	Percent of Households
1	One or less	74.8%
1	More than one	25.2%
1	All 1 Vehicle HHs	100.0%
2	Two or less	95.5%
2	More than 2	4.5%
2	All 2 Vehicle HHs	100.0%
3	Three or less	100.0%
3	More than 3	0.0%
3	All 3 Vehicle HHs	100.0%

Source: 2001-2002 NHTS, December 2003 Microdata Release. (N=681 HHs; wtd)

Notes:
More than 85 percent of Minnesota household vehicles have one household member that drives them most of the time.

Turns out there isn't much variability in drivers. Households match drivers and vehicles very closely. So we don't need both. We look for other household factors. See following three tables.

Average Annual Vehicle Miles Traveled for Minnesota Households by Household Size

Vehicles Available	Household Size	N	Percent of Households (Weighted)	Mean Annual Vehicle Miles Traveled-All HH Vehicles	Standard Deviation	Minimum	Maximum
1	1	85	20.5%	10,695	628,174	675	58,184
1	2	39	11.2%	12,537	618,668	1,034	31,957
1	3	12	4.1%	29,687	2,053,674	2,558	70,659
1	4+	5	2.6%	29,421	5,350,012	4,785	145,029
2	1	7	1.4%	12,103	483,171	4,287	32,258
2	2	123	24.1%	24,685	793,368	4,060	79,522
2	3	21	6.8%	24,338	1,127,944	10,425	68,534
2	4+	52	13.6%	25,996	674,342	8,416	71,785
3	1	0	0.0%	-	-	-	-
3	2	19	3.3%	32,911	950,334	9,287	84,691
3	3	22	5.6%	52,599	3,105,039	16,227	163,070
3	4+	24	6.7%	32,149	780,092	14,123	94,316

Source: 2001-2002 National Household Travel Survey, December 2003 Microdata Release. (N=681 households; weighted results)

Notes:
[1] For households for which mileage could be estimated for all vehicles.

Average Annual Vehicle Miles Traveled for Minnesota Households by Household Life Cycle

Vehicles Available	Life Cycle	N	Percent of Households (Weighted)	Mean Annual Vehicle Miles Traveled- All HH Vehicles	Standard Deviation	Minimum	Maximum
1	One adult	48	11.5%	14,020	715,505	2,156	58,184
1	Two or more adults	15	5.9%	24,036	1,995,613	3,028	70,659
1	1 adult, 1+ children	12	4.1%	17,807	757,590	3,131	30,711
1	2+ adults, 1+ children	9	4.0%	22,739	3,879,730	2,558	145,029
1	Retirees	57	13.1%	7,192	364,542	675	24,685
2	One adult	4	0.6%	17,183	541,791	8,861	32,258
2	Two or more adults	80	17.2%	26,964	847,958	8,344	79,522
2	1 adult, 1+ children	2	0.5%	14,501	905,101	8,416	30,145
2	2+ adults, 1+ children	68	18.7%	25,833	830,256	8,878	71,785
2	Retirees	49	8.9%	18,634	603,371	4,060	48,220
3	One adult	0	0.0%	-	-	-	-
3	Two or more adults	10	1.7%	43,844	1,013,747	13,888	84,691
3	1 adult, 1+ children	3	0.7%	23,024	282,894	18,152	27,015
3	2+ adults, 1+ children	40	11.0%	41,948	2,450,746	14,123	163,070
3	Retirees	12	2.1%	29,989	714,915	9,287	46,862

Source: 2001-2002 National Household Travel Survey, December 2003 Microdata Release. (N=681 households; weighted results)

Notes:
[1] For households for which mileage could be estimated for all vehicles.

Average Annual Vehicle Miles Traveled for Minnesota Households by Household Workers

Vehicles Available	Workers	N	Percent of Households (Weighted)	Mean Annual Vehicle Miles Traveled-All HH Vehicles	Standard Deviation	Minimum	Maximum
1	0	63	14.4%	7,531	349,463	675	24,685
1	1	61	17.5%	14,582	737,644	1,265	58,184
1	2+	17	6.6%	29,717	3,185,467	3,028	145,029
2	0	27	4.6%	19,596	714,783	4,060	48,220
2	1	48	10.0%	21,244	621,577	8,344	41,615
2	2+	128	31.4%	26,446	860,025	8,878	79,522
3	0	3	0.6%	19,474	789,862	9,287	39,775
3	1	16	2.9%	31,853	556,642	17,387	46,862
3	2+	46	12.1%	42,460	2,323,805	13,888	163,070

Source: 2001-2002 National Household Travel Survey, December 2003 Microdata Release. (N=681 households; weighted results)

Notes:
[1] For households for which mileage could be estimated for all vehicles.

Of these factors, workers seems to best explain variations in household mileage amounts, so we propose to base our participant groups on vehicles available, workers, and the ages of vehicles in the household.

Proposed Participant Groups and Incidence

Participant Group	Vehicles Available	Workers	Newer Vehicles	Percent of Households (Weighted)	Proposed Number in Experiment	Proposed Number in Control Group
100	1	0	1	14.4%	10	3
110	1	1	1	17.5%	15	4
120	1	2+	1	6.6%	10	3
211	2	0, 1	2	5.9%	10	3
212	2	0, 1	1	8.7%	10	3
221	2	2+	2	12.7%	10	3
222	2	2+	1	18.7%	15	4
310	3	0, 1	1-3	3.4%	5	2
320	3	2+	1-3	12.1%	15	4

Appendix D

Participant Initiation Mailing

MnDOT Driving Study

March 3, 2004

Re: MnDOT Driving Study

Dear,

On behalf of the Minnesota Department of Transportation (MnDOT), I would like to thank you for recently completing our survey on driving and car ownership and offering to participate in the MnDOT Driving Study. This study is an opportunity for us to learn about what factors influence your driving behavior and may reveal alternatives that would reduce congestion and help mitigate environmental impacts on our highways.

The information we collect from you will be kept confidential and used only as research for this study. Your participation is vital to the success of the study and will help determine the future of our State's transportation system.

In this study, we will monitor your household driving mileage over an eight-month period by installing an electronic device called a "CarChip" to the on-board diagnostics port of your cars. This is where your mechanic gets diagnostic information. The CarChip will be attached to one or more of your vehicles to gather information on the time and distance of trips you make. This device cannot record where you go or any other information. Details of the experiment, participant rules, and the specifics of our incentive program are enclosed.

You soon will be contacted by a representative of GeoStats, one of our contractors, who will schedule a time to install the CarChip and show you how to replace it with new ones we will send you during the eight-month period. GeoStats is a company that specializes in data collection for transportation projects nationwide.

Again, I want to thank you for your participation in this study and reiterate that your commitment to serving our community is greatly appreciated! If you have any questions about the project, please call our project toll-free hotline at 1-866-GEOSTATS.

Sincerely,

Kenneth R. Buckeye
Project Manager, MnDOT

Appendix E

Installation Letter

MnDOT Driving Study

Minnesota Department of Transportation
395 John Ireland Boulevard
St. Paul, MN 55155-1899

<INSTDATE>

<First Name> <Last Name>
<Address Line 1>
<Address Line 2>
<City>, <State> <Zip Code>

Dear <First Name> <Last Name>,

Recently you agreed to participate in the MnDOT Driving Study. Thank you for your participation in this study – you are among a select group of people in your area who are participating. Your participation is completely voluntary and there are no penalties for not participating.

Our representative will be installing a CarChip to the OBD (on-board diagnostics) port in each of your vehicles with model year 1996 or newer. A picture of where this port is located in your vehicle(s) will be left with you so that you can perform future swaps of the CarChip(s). Please keep this picture. You will receive replacement CarChip(s) in one month with detailed instructions on how to make this swap yourself. There will also be a green card to record the details of this swap and to return with the old CarChip(s) in a self-addressed stamp envelope at absolutely NO cost to you.

After receiving the first CarChip(s) next month, the next swap will occur one month later. The full schedule of your CarChip swaps appears next:

This study will last for a total of 8 months. At times during this study, you will be instructed to go to our website to view a vehicle activity statement. The web address is www.geostats.com/mndotweb/. You will be provided a username and password to view the information. You are the only one that will be able to view your travel information.

The success of the study relies on you replacing the CarChip promptly when you receive it, and not driving without the CarChip installed. If, when we review the data from the CarChip we find that the car has been driven without the CarChip installed, we may have to terminate your involvement in the study, and you would forfeit the remainder of your compensation.

You will be compensated for your participation in this project. For beginning the study, you will be receiving $10 within the next few weeks. Future compensation will occur on the schedule below, and will be based on completing some simple tasks:

- <Month 3> $20
- <Month 5> $20
- <Month 8> $20
- End of project $30 (upon return of the CarChip)

You will also have the opportunity to earn additional amounts – instructions on how to do this will be provided to you over the course of the project. These extra amounts will be provided upon completion of the study.

If at any time you have any questions, please do not hesitate to call us toll-free at 1-866-GEOSTATS.

Thank you for your important contribution to helping your region plan for its future!

Appendix F

First Swap Cover Letter

Minnesota Department of Transportation
395 John Ireland Boulevard
St. Paul, MN 55155-1899

MnDOT Driving Study

<Household ID> <Date>

<First Name> <Last Name>
<Address Line 1>
<Address Line 2>
<City>, <State> <Zip Code>

Dear <First Name> <Last Name>,

Thank you for your participation in the MnDOT Driving Study. You have been driving with the CarChip installed for about a month now, and it is time to replace it. Please review the package contents and follow the instructions below.

For each vehicle in your household **that has the CarChip installed**:

1. Remove the installed CarChip. Refer to the picture of the CarChip location that was left by the installer.
2. Compare the CarChip ID (located on the end of the CarChip) that you just removed with the number on the green card. Make any corrections on the green card.
3. Make sure that the ID number on the new CarChip sent in this package matches that shown on the green card. Install the new CarChip in the vehicle indicated on the green card..
4. Write down the Odometer reading for each vehicle listed on the green card.

For each vehicle in your household **that does not have the CarChip installed**:

1. Write down the odometer reading on the green card.

When you have finished swapping the CarChip(s):

1. Place each removed CarChip(s) securely in the padded envelope provided.
2. Insert the green card into the envelope with the CarChip(s).
3. Seal the envelope and drop it in any U.S. Postal Service mailbox. Postage has already been paid.

Please leave the CarChips that you just installed in your vehicle(s) until you receive your next package.

If you have any questions or comments, please call 1-866-GeoStats.

Progress of the Study

CarChip installed	X
First payment received: $10	X
First replacement CarChip received	
Second payment (around June): $20	
Third payment (around August): $20	
Fourth payment (around November): $20	
Fifth payment (around January) $30 (after return of the CarChip and completion of a short survey)	

Appendix G

Vehicle/CarChip Control Sheet

Minnesota Department of Transportation
395 John Ireland Boulevard
St. Paul, MN 55155-1899

Driving Behavior Study

<Household ID> <Date>

<First Name> <Last Name>
<Address Line 1>
<Address Line 2>
<City>, <State> <Zip Code>

Enter the information for each vehicle below.

	Make	Model	Year	Current CarChip ID	New CarChip ID	Swap Date	Odometer Reading
1	Chrysler	Town & Country	1996	________	________	________	_________
2	Ford	Explorer	1994	N/A	N/A	________	_________
3						________	_________

Notes:

Odometer readings for all household vehicles must be recorded. This is a requirement for participation.

Each CarChip must be installed in the assigned vehicle throughout the study period. The CarChip data informs us of all install and removal events.

If you have not recently provided us with your e-mail address, please do so here:

____

Please let us know if you are planning a vacation and will not be driving your car for a certain period during this study.

Please note any special circumstances here.

Appendix H

Example Experiment Start Letter

Minnesota Department of Transportation
395 John Ireland Boulevard
St. Paul, MN 55155-1899

MnDOT Driving Study

<Household ID> <Date>

<First Name> <Last Name>
<Address Line 1>
<Address Line 2>
<City>, <State> <Zip Code>

Dear <First Name> <Last Name,

Thank you for your participation in the MnDOT Driving Study. You have been driving with the CarChip installed for about a month now, and it is time to replace it. Please review the package contents and follow the instructions below.

****You are about to enter the next phase of the* MnDOT Driving Study***

We are testing a concept where people would pay for some portion of the price of their car based on how many miles they drive. We want to know if paying in this way alters their travel behavior.

In this part of our project, you can earn additional money above and beyond the participation incentive of $100. You will not be asked to risk any of the $100 we will be giving you or any of your own money Here's how it will work:

1. ***We have set up an account for you with*** *$520.*
2. ***We will deduct some money from your account for each mile you drive in your CarChipped vehicle (****1996 Town & Country****), as follows:***

15 cents per mile **for travel on weekdays** during peak travel times**:**
from 6:00 a.m. to 9:00 a.m. and from 3:00 p.m. to 7:00 p.m.,
10 cents per mile for travel at all other times**.**

If a single trip covers both peak and off-peak periods, we will charge based on the miles driven during each time period.

- ***We will track your mileage for this/these vehicle(s) using the CarChip from*** 5/21/2004 ***to*** 8/20/2004 ***in the same way we have up until now.***
- ***We will prepare a statement of your account twice a month, based on the mileage recorded on the CarChip you have installed. You can view your statement on the web at any time at the following website:*** www.geostats.com/mndotweb/login.aspx.

We will send you an e-mail telling you when the statement is ready to be viewed. Please look at the statement when you receive the e-mail. This is a very important part of the project.

- ***After*** 8/20/2004***, we will not charge you for your mileage, but we still need to track your mileage patterns. This is also very important.***
- ***At the end of the study, you may keep any money remaining in your account. The money will be sent to you along with your final incentive check at the end of this eight-month project upon return of the final CarChip(s).***
- ***We will never charge you any money. If you use your entire budget, you will simply not get an additional incentive. You will still get the $100 participation incentive, distributed over time, as shown in the table at the end of this letter.***

If you do not understand these instructions, please call GeoStats at 1-866-GeoStats

Instructions for Swapping your CarChip

For each vehicle in your household **that has the CarChip installed**:

1. Remove the installed CarChip. Refer to the picture of the CarChip location that was left by the installer.
2. Compare the CarChip ID (located on the end of the CarChip on the blue metallic label) that you just removed with the number on the green card. Make any corrections on the green card.
3. Make sure that the ID number on the new CarChip sent in this package matches that shown on the green card. Install the new CarChip in the vehicle indicated on the green card.
4. Write down the Odometer reading for each vehicle listed on the green card.

For each vehicle in your household **that does not have the CarChip installed**:

1. Write down the odometer reading on the green card.

Important Notes

Keep your CarChip installed. **You must leave the CarChip installed throughout the study period to qualify for the financial incentives. The only time it should be removed is to swap it with a new CarChip, or for maintenance.**

What to do if you take your car to the mechanic. **If you take your vehicle with the CarChip to a mechanic for servicing, please inform them that the CarChip is installed on the OBD port and that they can remove it if necessary for diagnostics, but it must be returned to the OBD port before you depart.**

Odometer readings are important. **Odometer readings for all household vehicles must be reported during each swap – and not just for the vehicles with the CarChips installed.**

When you have finished swapping the CarChip(s):

1. Place each removed CarChip(s) securely in the padded envelope provided.
2. Insert the green card into the envelope with the CarChip(s).
3. Seal the envelope and drop it in any U.S. Postal Service mailbox. Postage has already been paid.

Please leave the CarChips that you just installed in your vehicle(s) until you receive your next package, which should arrive in a few weeks.

If you have any questions or comments, please call 1-866-GeoStats.

Progress of the Study

CarChip installed	X
First payment received: $10	X
First replacement CarChip received	X
Second replacement CarChip and pricing instructions received.	X
Second payment (in June): $20	
Third payment (around August): $20	
Fourth payment (around November): $20	
Fifth payment (around January) $30, plus remaining balance from mileage account, if any (after return of the CarChip and completion of a short survey)	

Thank you again for your participation!

Appendix I

Example Vehicle Activity Statement

Household Information

Jane Doe Anytown, MN 55555	
Vehicle:	1996 Chrysler Town & Country
Peak Mileage Fee:	$0.15
Off-Peak Mileage Fee:	$0.10

Statement Start Date:	5/21/2004
Statement End Date:	6/4/2004
Statement Starting Balance:	$520.00
Statement Ending Balance:	$466.07

Statement Period Data

Total Trips:	96
Total Distance (miles):	445.2
Total Cost:	$53.93

Balance Information

Initial Balance:	$520.00
Current Balance:	$466.07

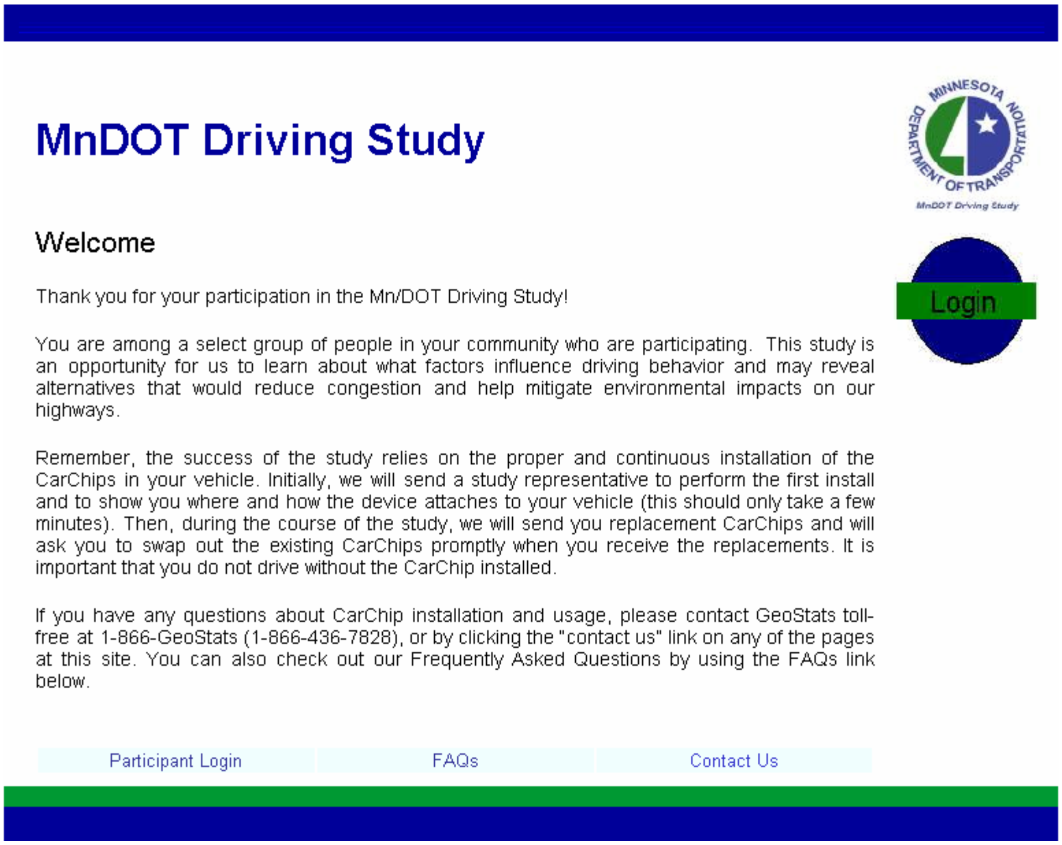
Select Statement Date

2004-05-21 - 2004-06-04 Go

Click 'Go' to see last statement or select previous statement if available.

	Date	# Trips	Off-Peak		Peak		Total Daily Cost
			Miles	Cost	Miles	Cost	
Details	5/21/2004	4	9.3	$0.93	0	$0.00	$0.93
Details	5/22/2004	5	6.9	$0.69	3.3	$0.50	$1.18
Details	5/23/2004	3	43.5	$4.35	0	$0.00	$4.35
Details	5/24/2004	7	47.9	$4.79	0	$0.00	$4.79
Details	5/25/2004	17	33.0	$3.30	27.1	$4.06	$7.36
Details	5/26/2004	5	0	$0.00	29.2	$4.38	$4.38
Details	5/27/2004	6	13.4	$1.34	3.7	$0.56	$1.90
Details	5/28/2004	4	6.6	$0.66	25.5	$3.82	$4.49
Details	5/29/2004	6	14.7	$1.47	15.2	$2.28	$3.75
Details	5/30/2004	8	18.0	$1.80	9.7	$1.46	$3.26
Details	5/31/2004	2	0	$0.00	0	$0.00	$0.00
Details	6/1/2004	8	25.7	$2.57	17.2	$2.58	$5.15
Details	6/2/2004	3	10.0	$1.00	21.7	$3.26	$4.26
Details	6/3/2004	8	22.4	$2.24	16.1	$2.42	$4.66
Details	6/4/2004	10	5.6	$0.56	19.5	$2.92	$3.48
Totals		**96**	**257**	**$25.70**	**188.2**	**$28.23**	**$53.93**

MnDOT Driving Study Web Site – Home Page



MnDOT Driving Study

MINNESOTA DEPARTMENT OF TRANSPORTATION

MnDOT Driving Study

Login

Welcome

Thank you for your participation in the Mn/DOT Driving Study!

You are among a select group of people in your community who are participating. This study is an opportunity for us to learn about what factors influence driving behavior and may reveal alternatives that would reduce congestion and help mitigate environmental impacts on our highways.

Remember, the success of the study relies on the proper and continuous installation of the CarChips in your vehicle. Initially, we will send a study representative to perform the first install and to show you where and how the device attaches to your vehicle (this should only take a few minutes). Then, during the course of the study, we will send you replacement CarChips and will ask you to swap out the existing CarChips promptly when you receive the replacements. It is important that you do not drive without the CarChip installed.

If you have any questions about CarChip installation and usage, please contact GeoStats toll-free at 1-866-GeoStats (1-866-436-7828), or by clicking the "contact us" link on any of the pages at this site. You can also check out our Frequently Asked Questions by using the FAQs link below.

Participant Login | FAQs | Contact Us

Frequently Asked Questions

Frequently Asked Questions

Q: Why is the Minnesota Department of Transportation conducting this study?

A: The MnDOT Driving Study is an opportunity for the DOT to learn about what factors influence your driving behavior and may reveal alternatives that would reduce congestion and help mitigate environmental impacts on our highways.

Q: How long will this study last?

A: This study will last eight months, and will be followed by extensive data analysis. The project Final Report will be produced by MnDOT in September 2005.

Q: Who is doing this study?

A: The MnDOT project manager is Ken Buckeye. The project consulting team consists of Cambridge Systematics, Inc., MarketLine Research, and GeoStats.

Q: What do I do if I have questions about how to swap my CarChips?

A: You will be given a diagram illustrating how to swap and install your CarChips when a GeoStats representative installs the first CarChip to your car (s) OBD port. After referring to this document, if you still have questions you should contact the project toll-free hotline at 1-866-GEOSTATS.

Q: Will the data collected by my CarChip be shared with anyone else?

A: Absolutely not! The information collected will be used strictly as research for the MnDOT Driving Study. We will not use this information for any other purpose after completion of the study, nor will we share it with outside entities. No personal identification information will be kept.

Log In

Log Off

Please choose one of the following menu options:

Travel Statistics

1. View Household Statement
2. View Household Odometer Data

Back To Top | FAQs | Contact Us

Household Odometer Data

Home Log Off

Odometer Data

Vehicle ID	Start Date	End Date	Initial Odometer	Final Odometer
1	3/6/2004	4/5/2004	21439	22187
1	4/6/2004	5/5/2004	22187	22914
1	5/6/2004	6/5/2004	22914	23746

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Household Statement – Select Vehicle

Home Log Off

Household
1172
Minneapolis, MN 55410

View Vehicle Statement
2000 Toyota Camry Go

<< Back

Back To Top FAQs Contact Us

Vehicle Activity Statement Summary

Home | Log Off

Vehicle Activity Statement Summary

MnDOT Driving Study

Household Information	
1172 Minneapolis, MN 55410	
Vehicle:	2000 Toyota Camry
Study Type:	CEC
Peak Mileage Fee:	$0.10
Off-Peak Mileage Fee:	$0.10

Balance Information	
Initial Balance:	$270.00
Current Balance:	$224.19

Select Statement Date

2004-05-14 - 2004-05-28 Go

Click 'Go' to see last statement or select previous statement if available.

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Note: Study type line was only visible to project staff, not to participants.

Vehicle Activity Select Statement

Home | Log Off

Vehicle Activity Statement

MnDOT Driving Study

Household Information	
1172 Minneapolis, MN 55410	
Vehicle:	2000 Toyota Camry
Study Type:	CEC
Peak Mileage Fee:	$0.10
Off-Peak Mileage Fee:	$0.10

Statement Period Data	
Total Trips:	152
Total Distance (miles):	595.4
Total Trip Cost:	$0.00

Statement Start Date:	4/14/2004
Statement End Date:	5/13/2004
Statement Starting Balance:	$0.00
Statement Ending Balance:	$0.00

CarChip Install Date:	4/15/2004
CarChip Removal Date:	5/14/2004

Balance Information	
Initial Balance:	$270.00
Current Balance:	$224.19

Select Statement Date

2004-04-14 - 2004-05-13 Go

Click 'Go' to see last statement or select previous statement if available.

Note: Study type and CarChip installation and removal dates were only visible to project staff, not to participants.

Trips, Total Miles, and Cost by Date

	Date	# Trips	Miles	Cost
Detail View	4/14/2004	6	6.2	$0.00
* Detail View	4/15/2004	7	10.9	$0.00
Detail View	4/16/2004	6	41.9	$0.00
Detail View	4/17/2004	14	17.9	$0.00
Detail View	4/18/2004	13	24.0	$0.00
Detail View	4/19/2004	5	38.8	$0.00
Detail View	4/20/2004	7	16.2	$0.00
Detail View	4/21/2004	3	35.7	$0.00

Note: Simulated statement -- cost of miles not shown.

Daily Trip Detail

Home Log Off

Daily Detail

Vehicle Trips For:	4/14/2004

Total Travel Time (minutes):	21.6
Total Distance (miles):	6.2
Total Cost:	$0.00

Trip Start Time	Trip End Time	Duration (min)	Distance (miles)	Trip Cost
9:00am	9:04am	3.9	1.0	$0.00
9:09am	9:10am	1.1	0.2	$0.00
9:14am	9:18am	3.9	1.0	$0.00
3:32pm	3:35pm	3.4	1.0	$0.00
3:49pm	3:52pm	3.6	1.1	$0.00
4:02pm	4:08pm	5.7	1.9	$0.00
	Totals	21.6	6.2	$0.00

Note: Simulated statement – cost of miles not shown.

Appendix J

Exit Surveys (Control Group and Experiment Group)

Control Group

MnDOT Driving Study

MnDOT Driving Study Participant Survey

1. Using a scale from 1 through 5 where "5" means excellent and "1" means poor, how would you rate your overall experience with the study to date? ***(Please check one box)***

Poor	Fair	Good	Very Good	Excellent
☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

2. What could have been done differently to make the experience better?

____

____

____

____

Using a scale from 1 through 5 where "1" means you strongly disagree and "5" means you strongly agree, please tell us how much you agree with the following statements.

		Strongly Disagree	Disagree	Neither Agree Nor Disagree	Agree	Strongly Agree
3.	Participating in the study has been inconvenient	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
4.	The people administering the study have been helpful	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
5.	Installing the CarChips has been difficult	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
6.	Swapping the CarChips has been difficult	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
7.	Having my and my family's mileage recorded made me uncomfortable	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

		Strongly Disagree	Disagree	Neither Agree Nor Disagree	Agree	Strongly Agree
8.	Being in the study affected my or my family's driving habits	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
9.	During the study, the amount we drove and our travel patterns were pretty typical	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
10.	If I were charged by the mile, I would be able to reduce the number of miles I drive over a long period, like a year	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

We would like to ask you about two potential products that might be priced on a mileage basis—insurance and vehicle leases. We would like your reaction to the following products.

PAY-AS-YOU-DRIVE INSURANCE

Pay as you drive insurance is a product where you pay a flat fee per year that is less than what you pay now. In addition, you would pay a charge for each mile you drive the vehicle. Your mileage would be automatically recorded and reported to a central billing agency by a tamperproof device installed in your vehicle. If you drove less, you would pay less. If you drove more, you would pay more. You would receive an itemized bill (like a phone bill) four times per year for the fixed amount and your mileage charges.

11. How likely is it that you would consider pay-as-you-drive insurance if it was available? Assume when answering this question that all vehicles in your household would be on the same policy, which means that there would not be any savings from shifting mileage from one vehicle to another.

☐	☐	☐	☐	☐
1 (Very Unlikely)	2	3	4	5 (Very Likely)

12. What do you like most about the pay-as-you-drive insurance concept?

____

____

____

____

13. What do you like least about the pay-as-you-drive insurance concept?

____

____

____

____

How important are the following factors in thinking about a pay-as-you-drive insurance?

		Not At All Important	⟵		⟶	**Very Important**
14.	Potential insurance cost savings	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
15.	Ability to control costs by reducing my mileage	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
16.	Uncertainty about what my costs would be	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
17.	Concerns that data about my driving patterns could be used for other unintended purposes	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

If the following features were added to the insurance agreement, would you be more or less likely to choose a pay-as-you-drive insurance product?

		Much Less Likely	Less Likely	Neither More nor Less Likely	More Likely	Much More Likely
18.	Higher per-mile prices during the weekday rush hours, and lower prices at other weekday times and on weekends	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
19.	Instead of the mileage being recorded electronically, it is simply audited once a year at an authorized service location (such as a gas station, insurance agent, or car dealer)	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
20.	Flexibility to switch back to a traditional insurance policy without penalty	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

PAY-AS-YOU-DRIVE LEASING

Pay-as-you-drive leasing is an auto leasing agreement that you would enter when you acquire a vehicle. You pay a flat fee per month that is lower than a standard auto lease. In addition to this you would pay a mileage charge. Your mileage would be automatically recorded and reported to a central billing agency by a tamperproof device installed in your vehicle. If you drove less, you would pay less. If you drove more, you would pay more. You would receive an itemized bill (like a phone bill) each month for the fixed amount of the lease and your mileage charges. At the end of the lease period you would return the vehicle or have an option to purchase it.

21. How likely is it that you would consider pay-as-you-drive leasing if it was available?

☐	☐	☐	☐	☐
1 (Very Unlikely)	2 (Unlikely)	3 (Not sure)	4 (Likely)	5 (Very Likely)

22. What do you like most about the pay-as-you-drive leasing concept?

____

____

____

____

23. What do you like least about the pay-as-you-drive leasing concept?

____

____

____

____

How important are the following factors in thinking about a pay-as-you-drive lease concept?

		Not At All Important	⟵		⟶	**Very Important**
24.	Potential vehicle acquisition cost savings	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
2.5	Ability to control costs by reducing my mileage	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
26.	Uncertainty about what my costs would be	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
27.	Concerns that data about my driving patterns could be used for other unintended purposes	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

If the following features were added to the lease agreement, would you be more or less likely to choose a pay-as-you-drive leasing product?

		Much Less Likely	**Less Likely**	**Neither More nor Less Likely**	**More Likely**	**Much More Likely**
28.	Higher per-mile prices during the weekday rush hours, and lower prices at other weekday times and on weekends	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
29.	Instead of the mileage being recorded electronically, it is simply audited once a year at an authorized service location (such as a gas station or car dealer)	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

The following are some general statements related to driving and the use of technology. Please use a scale of 0 to 10 to indicate how much you agree or disagree with each of the following statements, where a "10" means you completely agree and a "0" means you completely disagree.

30. I drive much more than the typical person

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

31. I like the freedom that the automobile represents

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

32. The automobile gives me a lot of flexibility in my daily life

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10

(Strongly Disagree) (Strongly Agree)

33. I need to drive to different destinations as part of my busy daily schedule

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10

(Strongly Disagree) (Strongly Agree)

34. I like driving whenever and wherever I like without worrying about the cost

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10

(Strongly Disagree) (Strongly Agree)

35. My driving patterns are pretty close to the same from week to week

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10

(Strongly Disagree) (Strongly Agree)

36. I actively think about ways to reduce my auto operating and ownership costs

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10

(Strongly Disagree) (Strongly Agree)

37. Metro area congestion affects where and when I drive

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10

(Strongly Disagree) (Strongly Agree)

38. Metro area congestion affects how often I carpool and use public transit

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10

(Strongly Disagree) (Strongly Agree)

39. I like taking long rides out in the countryside to relax

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10

(Strongly Disagree) (Strongly Agree)

40. I enjoy spending time driving and consider my car as a private space/refuge

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10
(Strongly Disagree)										(Strongly Agree)

41. When purchasing something that requires financing, I always try to make the largest down payment that I can

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10
(Strongly Disagree)										(Strongly Agree)

42. I don't mind complicated transactions if they save me money

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10
(Strongly Disagree)										(Strongly Agree)

43. Leasing an automobile is an expensive way to get a car

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10
(Strongly Disagree)										(Strongly Agree)

44. Leasing an automobile frees you from worrying about resale value

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10
(Strongly Disagree)										(Strongly Agree)

45. Maintenance costs and reliability are important in my choice of vehicles

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10
(Strongly Disagree)										(Strongly Agree)

46. Fuel economy is an important factor for me in choosing a car

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10
(Strongly Disagree)										(Strongly Agree)

47. A one dollar per gallon increase in the cost of gas would not affect my everyday driving

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0	1	2	3	4	5	6	7	8	9	10
(Strongly Disagree)										(Strongly Agree)

48. I enjoy driving a new car every few years

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

49. I believe that the car one drives reflects her or his lifestyle

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

50. Leasing a car allows you to use a new model every few years

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

51. I like driving SUVs

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

52. I like driving vehicles with good gas mileage to help the environment

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

53. I try to avoid unnecessary driving

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

54. People should rideshare, take transit, walk, or bicycle whenever possible to cut down on air pollution and energy consumption

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

55. I go out of my way to buy environmentally friendly products

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

56. I am willing to pay more to buy products that are environmentally friendly

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

57. I consider myself to be politically aware and I closely follow local, regional, and national issues that affect my family and me

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

58. I try to look at least five years into the future when making plans for my family

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

59. If the car I usually drive is unavailable for some reason, I can usually use a different vehicle to make the trips I need to

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

60. I don't like having to rely on others to take me to where I need to go

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

61. Each driver in our household has a particular vehicle that they more or less drive all the time

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

62. I don't like the idea that somebody could be monitoring my daily habits

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

63. Programs that track what an individual does on the Internet are an invasion of privacy

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

64. Thorough searches at airport checkpoints based on visual profiles are an invasion of privacy

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0 (Strongly Disagree)	1	2	3	4	5	6	7	8	9	10 (Strongly Agree)

65. Unsolicited phone calls for the purpose of selling products or services are an invasion of privacy

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0 (Strongly Disagree)	1	2	3	4	5	6	7	8	9	10 (Strongly Agree)

66. I feel comfortable using personal computers

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0 (Strongly Disagree)	1	2	3	4	5	6	7	8	9	10 (Strongly Agree)

67. I am intrigued by new technologies and like to try new gadgets

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0 (Strongly Disagree)	1	2	3	4	5	6	7	8	9	10 (Strongly Agree)

68. I like exploring ways that technology can improve my daily life

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0 (Strongly Disagree)	1	2	3	4	5	6	7	8	9	10 (Strongly Agree)

69. I am concerned about security of transactions while using the Web

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0 (Strongly Disagree)	1	2	3	4	5	6	7	8	9	10 (Strongly Agree)

70. When paying for telephone and electricity services, I would rather pay the same amount every month

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0 (Strongly Disagree)	1	2	3	4	5	6	7	8	9	10 (Strongly Agree)

71. How would you rate your personal knowledge of the vehicle purchasing process?

Poor	**Fair**	**Good**	**Very Good**	**Excellent**
☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

72. How would you rate your personal knowledge of the vehicle leasing process?

Poor	Fair	Good	Very Good	Excellent
☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

73. How would you rate your personal knowledge of auto operating and ownership costs?

Poor	Fair	Good	Very Good	Excellent
☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

The next set of questions has to do with the next time you will acquire a vehicle.

74. When do you and the other members of your household next expect to purchase or lease a vehicle? ***(Please check one box)***

☐ Within the next six months

☐ Six to 12 months from now

☐ One to two years from now

☐ Three to five years from now

☐ More than five years from now or never

75. Will that vehicle be used to replace a vehicle you currently have, or will it be in addition to the vehicles that your household already has? ***(Please check one box)***

☐ Replacement of a vehicle

☐ In addition to current vehicles

76. What is the likelihood that you will get a new vehicle versus a used vehicle? ***(Please check one box)***

☐ Definitely will get a NEW vehicle, rather than a used vehicle

☐ Probably will get a NEW vehicle, rather than a used vehicle

☐ Are uncertain whether you will get a new or used vehicle

☐ Probably will get a USED vehicle, rather than a new vehicle

☐ Definitely will get a USED vehicle, rather than a new vehicle

77. Thinking about the next time you get a vehicle, what is the likelihood that you will buy the vehicle versus lease the vehicle? ***(Please check one box)***

- ☐ Definitely will BUY the vehicle, rather than lease it
- ☐ Probably will BUY the vehicle, rather than lease it
- ☐ Are uncertain whether you will buy or lease it
- ☐ Probably will LEASE the vehicle, rather than buy it
- ☐ Definitely will LEASE the vehicle, rather than buy it

78. What do you expect the purchase price to be for this vehicle? ***(Please check one box. Your best estimate is fine.)***

- ☐ $10,000 or less
- ☐ $10,000 to $15,000
- ☐ $15,000 to $20,000
- ☐ $20,000 to $25,000
- ☐ $25,000 to $30,000
- ☐ $30,000 to $40,000
- ☐ $40,000 to $50,000
- ☐ $50,000 to $70,000
- ☐ More than $70,000

79. How much do you expect that it would cost per year to insure this vehicle with the level of coverage that you would like to have? ***(Your best estimate is fine.)***

$__________

80. How many miles per year would you expect to drive the vehicle? ***(Your best estimate is fine.)***

__________ miles

81. Do you have any additional comments?

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Thank you for your time. Please enclose the completed survey in the envelope provided and mail it back to us as soon as possible.

Experiment Group

MnDOT Driving Study

MnDOT Driving Study Participant Survey

1. Using a scale from 1 through 5 where "5" means excellent and "1" means poor, how would you rate your overall experience with the study to date? ***(Please check one box)***

Poor	Fair	Good	Very Good	Excellent
☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

2. What could have been done differently to make the experience better?

____

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Using a scale from 1 through 5 where "1" means you strongly disagree and "5" means you strongly agree, please tell us how much you agree with the following statements.

		Strongly Disagree	**Disagree**	**Neither Agree Nor Disagree**	**Agree**	**Strongly Agree**
3.	Participating in the experiment has been inconvenient	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
4.	The people administering the experiment have been helpful	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
5.	Installing the CarChips has been difficult	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
6.	Swapping the CarChips has been difficult	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
7.	The website has been easy to use	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
8.	Having my and my family's mileage recorded made me uncomfortable	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

		Strongly Disagree	**Disagree**	**Neither Agree Nor Disagree**	**Agree**	**Strongly Agree**
9.	Being in the experiment affected my or my family's driving habits	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
10.	During the time when our mileage <u>was **not** priced</u>, the amount we drove and our travel patterns were fairly typical	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
11.	During the time when our mileage <u>was priced</u>, the amount we drove and our travel patterns were pretty typical	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
12.	During the time when our mileage <u>was priced</u>, I was aware of the price when I drove the car	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
13.	During the time when our mileage <u>was priced</u>, I felt restricted in terms of where and when I drove	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
14.	I liked being able to control my driving costs	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
15.	It was difficult to reduce the amount of miles I drove	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
16.	I tried hard to reduce the number of miles I drove	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
17.	I was able to reduce the number of miles I drove as much as I expected when the pricing period began	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
18.	The price per mile made virtually no difference in my driving patterns	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
19.	If I were charged by the mile, I would be able to reduce the number of miles I drive over a long period, like a year	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

		Strongly Disagree	Disagree	Neither Agree Nor Disagree	Agree	Strongly Agree
20.	During the pricing period, I reduced weekday rush hour driving to save miles on the priced vehicle	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
21.	During the pricing period, I reduced weekday driving at times other than rush hours to save miles on the priced vehicle	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
22.	During the pricing period, I reduced weekend driving to save miles on the priced vehicle	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
23.	During the pricing period, I combined driving trips to save miles on the priced vehicle	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
24.	During the pricing period, I used other unpriced vehicles to save miles on the priced vehicle	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
25.	During the pricing period, I walked, biked, and/or used public transit to save miles on the priced vehicle	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
26.	During the pricing period, other household members reduced their driving to save miles on the priced vehicle	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

We conducted this study to test how drivers would react to mileage-based charges and to simulate how potential pay-as-you-drive products might work. Two potential products that might be priced on a mileage basis are insurance and vehicle leases. Based on your experience in the MnDOT Driving Study, we would like your reaction to the following products.

PAY-AS-YOU-DRIVE INSURANCE

Pay as you drive insurance is a product where you pay a flat fee per year that is less than what you pay now. In addition, you would pay a charge for each mile you drive the vehicle. Your mileage would be automatically recorded and reported to a central billing agency by a tamperproof device installed in your vehicle. If you drove less, you would pay less. If you drove more, you would pay more. You would receive an itemized bill (like a phone bill) four times per year for the fixed amount and your mileage charges.

27. Based on your experience in this experiment, how likely is it that you would consider pay-as-you-drive insurance if it was available? Assume when answering this question that all vehicles in your household would be on the same policy, which means that there would not be any savings from shifting mileage from one vehicle to another.

☐	☐	☐	☐	☐
1	2	3	4	5
(Very Unlikely)				(Very Likely)

28. What do you like most about the pay-as-you-drive insurance concept?

____

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____

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29. What do you like least about the pay-as-you-drive insurance concept?

____

____

____

____

How important are the following factors in thinking about a pay-as-you-drive insurance?

		Not At All Important	⟵		⟶	**Very Important**
30.	Potential insurance cost savings	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
31.	Ability to control costs by reducing my mileage	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
32.	Uncertainty about what my costs would be	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
33.	Concerns that data about my driving patterns could be used for other unintended purposes	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

If the following features were added to the insurance agreement, would you be more or less likely to choose a pay-as-you-drive insurance product?

		Much Less Likely	**Less Likely**	**Neither More nor Less Likely**	**More Likely**	**Much More Likely**
34.	Higher per-mile prices during the weekday rush hours, and lower prices at other weekday times and on weekends	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
35.	Instead of the mileage being recorded electronically, it is simply audited once a year at an authorized service location (such as a gas station, insurance agent, or car dealer)	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
36.	Flexibility to switch back to a traditional insurance policy without penalty	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

PAY-AS-YOU-DRIVE LEASING

Pay-as-you-drive leasing is an auto leasing agreement that you would enter when you acquire a vehicle. You pay a flat fee per month that is lower than a standard auto lease. In addition to this you would pay a mileage charge. Your mileage would be automatically recorded and reported to a central billing agency by a tamperproof device installed in your vehicle. If you drove less, you would pay less. If you drove more, you would pay more. You would receive an itemized bill (like a phone bill) each month for the fixed amount of the lease and your mileage charges. At the end of the lease period you would return the vehicle or have an option to purchase it.

37. Based on your experience in this experiment, how likely is it that you would consider pay-as-you-drive leasing if it was available?

☐	☐	☐	☐	☐
1	2	3	4	5
(Very Unlikely)	(Unlikely)	(Not sure)	(Likely)	(Very Likely)

38. What do you like most about the pay-as-you-drive leasing concept?

____

____

____

____

39. What do you like least about the pay-as-you-drive leasing concept?

____

____

____

____

How important are the following factors in thinking about a pay-as-you-drive lease concept?

		Not At All Important	⟷			**Very Important**
40.	Potential vehicle acquisition cost savings	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
41.	Ability to control costs by reducing my mileage	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
42.	Uncertainty about what my costs would be	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
43.	Concerns that data about my driving patterns could be used for other unintended purposes	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

If the following features were added to the lease agreement, would you be more or less likely to choose a pay-as-you-drive leasing product?

		Much Less Likely	**Less Likely**	**Neither More nor Less Likely**	**More Likely**	**Much More Likely**
44.	Higher per-mile prices during the weekday rush hours, and lower prices at other weekday times and on weekends	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5
45.	Instead of the mileage being recorded electronically, it is simply audited once a year at an authorized service location (such as a gas station or car dealer)	☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

The following are some general statements related to driving and the use of technology. Please use a scale of 0 to 10 to indicate how much you agree or disagree with each of the following statements, where a "10" means you completely agree and a "0" means you completely disagree.

46. I drive much more than the typical person

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

47. I like the freedom that the automobile represents

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

48. The automobile gives me a lot of flexibility in my daily life

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

49. I need to drive to different destinations as part of my busy daily schedule

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

50. I like driving whenever and wherever I like without worrying about the cost

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

51. My driving patterns are pretty close to the same from week to week

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

52. I actively think about ways to reduce my auto operating and ownership costs

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

53. Metro area congestion affects where and when I drive

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

54. Metro area congestion affects how often I carpool and use public transit

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

55. I like taking long rides out in the countryside to relax

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

56. I enjoy spending time driving and consider my car as a private space/refuge

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

57. When purchasing something that requires financing, I always try to make the largest down payment that I can

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

58. I don't mind complicated transactions if they save me money

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

59. Leasing an automobile is an expensive way to get a car

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

60. Leasing an automobile frees you from worrying about resale value

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

61. Maintenance costs and reliability are important in my choice of vehicles

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

62. Fuel economy is an important factor for me in choosing a car

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

63. A one dollar per gallon increase in the cost of gas would not affect my everyday driving

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

64. I enjoy driving a new car every few years

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

65. I believe that the car one drives reflects her or his lifestyle

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

66. Leasing a car allows you to use a new model every few years

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

67. I like driving SUVs

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

68. I like driving vehicles with good gas mileage to help the environment

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

69. I try to avoid unnecessary driving

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

70. People should rideshare, take transit, walk, or bicycle whenever possible to cut down on air pollution and energy consumption

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

71. I go out of my way to buy environmentally friendly products

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

72. I am willing to pay more to buy products that are environmentally friendly

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

73. I consider myself to be politically aware and I closely follow local, regional, and national issues that affect my family and me

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

74. I try to look at least five years into the future when making plans for my family

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

75. If the car I usually drive is unavailable for some reason, I can usually use a different vehicle to make the trips I need to

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

76. I don't like having to rely on others to take me to where I need to go

☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐ ☐
0 1 2 3 4 5 6 7 8 9 10
(Strongly Disagree) (Strongly Agree)

77. Each driver in our household has a particular vehicle that they more or less drive all the time

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

78. I don't like the idea that somebody could be monitoring my daily habits

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

79. Programs that track what an individual does on the Internet are an invasion of privacy

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

80. Thorough searches at airport checkpoints based on visual profiles are an invasion of privacy

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

81. Unsolicited phone calls for the purpose of selling products or services are an invasion of privacy

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

82. I feel comfortable using personal computers

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

83. I am intrigued by new technologies and like to try new gadgets

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

84. I like exploring ways that technology can improve my daily life

☐ 0 (Strongly Disagree) ☐ 1 ☐ 2 ☐ 3 ☐ 4 ☐ 5 ☐ 6 ☐ 7 ☐ 8 ☐ 9 ☐ 10 (Strongly Agree)

85. I am concerned about security of transactions while using the Web

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0 (Strongly Disagree)	1	2	3	4	5	6	7	8	9	10 (Strongly Agree)

86. When paying for telephone and electricity services, I would rather pay the same amount every month

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
0 (Strongly Disagree)	1	2	3	4	5	6	7	8	9	10 (Strongly Agree)

87. How would you rate your personal knowledge of the vehicle purchasing process?

Poor	**Fair**	**Good**	**Very Good**	**Excellent**
☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

88. How would you rate your personal knowledge of the vehicle leasing process?

Poor	**Fair**	**Good**	**Very Good**	**Excellent**
☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

89. How would you rate your personal knowledge of auto operating and ownership costs?

Poor	**Fair**	**Good**	**Very Good**	**Excellent**
☐ 1	☐ 2	☐ 3	☐ 4	☐ 5

The next set of questions has to do with the next time you will acquire a vehicle.

90. When do you and the other members of your household next expect to purchase or lease a vehicle? ***(Please check one box)***

☐ Within the next six months

☐ Six to 12 months from now

☐ One to two years from now

☐ Three to five years from now

☐ More than five years from now or never

91. Will that vehicle be used to replace a vehicle you currently have, or will it be in addition to the vehicles that your household already has? ***(Please check one box)***

- ☐ Replacement of a vehicle
- ☐ In addition to current vehicles

92. What is the likelihood that you will get a new vehicle versus a used vehicle? ***(Please check one box)***

- ☐ Definitely will get a NEW vehicle, rather than a used vehicle
- ☐ Probably will get a NEW vehicle, rather than a used vehicle
- ☐ Are uncertain whether you will get a new or used vehicle
- ☐ Probably will get a USED vehicle, rather than a new vehicle
- ☐ Definitely will get a USED vehicle, rather than a new vehicle

93. Thinking about the next time you get a vehicle, what is the likelihood that you will buy the vehicle versus lease the vehicle? ***(Please check one box)***

- ☐ Definitely will BUY the vehicle, rather than lease it
- ☐ Probably will BUY the vehicle, rather than lease it
- ☐ Are uncertain whether you will buy or lease it
- ☐ Probably will LEASE the vehicle, rather than buy it
- ☐ Definitely will LEASE the vehicle, rather than buy it

94. What do you expect the purchase price to be for this vehicle? ***(Please check one box. Your best estimate is fine.)***

- ☐ $10,000 or less
- ☐ $10,000 to $15,000
- ☐ $15,000 to $20,000
- ☐ $20,000 to $25,000
- ☐ $25,000 to $30,000
- ☐ $30,000 to $40,000
- ☐ $40,000 to $50,000
- ☐ $50,000 to $70,000
- ☐ More than $70,000

95. How much do you expect that it would cost per year to insure this vehicle with the level of coverage that you would like to have? ***(Your best estimate is fine.)***

$__________

96. How many miles per year would you expect to drive the vehicle? ***(Your best estimate is fine.)***

__________ miles

97. Do you have any additional comments?

____

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Thank you for your time. Please enclose the completed survey in the envelope provided and mail it back to us as soon as possible.

Appendix K

Data Retrieval File

Data Retrieval File

Blank cells represent no data collected during a given week (which could either represent non-use of the vehicle during that week or a data loss for that week), a "'1" indicates CarChip data for one household vehicle is available for that week, and a "2" indicates CarChip data for two household vehicles is available for that week. The last column shows the total weeks of data available per CarChipped vehicle per household. Given the initial eight-month study period, which is equivalent to 35 weeks, the numbers in the last column shows that sufficient data were collected from most households during the extended study period.

	March					April				May				June					July				Aug				Sep					Oct				Nov				Dec					Jan / Feb					totals
CECc			2	2		2	2	2	2	2	2	2	2	2	2	2	2	2	2		1	1	1	1	2	2		2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	42
		2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	1	2	2	1	1	1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	1	1	1	45
		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	47
	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1										1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	40
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			1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1						1	1	1	1	1	1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1	1	1	1	41
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			1	1	1	1	1	1	1	1	1	1	1	1	1				1	1	1	1		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	43
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			2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2					2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	43
			2	2	2	2	2	2	1	1	1	1	2	2	2	2	2	2	2	2	2	2			1	1	1	1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	41
		1	1	1	1	1	1			1	1	1	1	1	1	1	1	1	1						1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	41
			1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1							1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	41
	1	2	2	2	2	2	2	2	2	2	2	2	2		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	60
		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	47
			1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1						1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	42
		2	2	2	2	2	2	1	1	1	1	2	2	2	2	2	2	2				1	1	1	1	1	1	2	2	2	2	2	2	2	2	2		2	2	2	2	2	2	2	2	2	2	2	2	39
			1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1								1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1					36
			1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		1	1	1	1		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	45
		1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1	1	1							1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	41
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				1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	46
CECe			2	2	2	2	2	2	2	2	2	2	2	2													1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	35
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		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1			1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	46
			1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1					1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	43
		1	1	1	1	1	1	1	1	1	1	1	1	1			1	1						1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		1	1	1	1	40
				2	2	2	2	2	2	2	2	2	2	2	2	2	2										1	1	1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	36
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CECx		1	1	1	1	1	1	1	1	1	1	1	1																	1	1	1	1	1	1	1	1	1	1	1	1	1								25
		1	1	1	1	1	1	1	1	1	1	1		1				1	1	1		1		1		1				1			1	1	1	1	1													24
Total	**2**	**16**	**37**	**42**	**40**	**42**	**42**	**39**	**39**	**40**	**40**	**40**	**40**	**38**	**35**	**34**	**35**	**31**	**29**	**16**	**16**	**19**	**15**	**18**	**22**	**27**	**28**	**34**	**36**	**38**	**40**	**40**	**41**	**40**	**41**	**40**	**39**	**40**	**40**	**40**	**39**	**40**	**39**	**39**	**38**	**37**	**36**	**36**	**36**	

	March					April				May				June					July				Aug				Sep					Oct				Nov				Dec					Jan / Feb					totals
CCE	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1									1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	41
	1	1	1	1	1	1	1	1	1	1	1														1	1	1	1	1	1	1	1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1	1	35
		1	1	1	1	1	1	1	1	1																					1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	27
		1	1	1	1	1	1																				1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	29
		1	1	1	1	1	1			1	1	1	1	1	1	1	1	1									1	1	1	1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1	1	1	1	37
	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1											1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	39
					1	1	1	1		1	1																				1	1	1	1				1	1		1	1	1	1	1				1	18
		1	1	1	1	1	1	1	1	1	1	1														1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	35
	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1										1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	40
		1	1	1	1	1	1	1	1	1	1															1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	34
	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1									1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	41
		1	1	1	1	1	1	1	1	1	1	1	1	1	1						1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	43
	1	1	1	1	1	1	1	1	1	1														1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1					33
		1	2	2	2	2	2	2	1	1	1																1	1	1	2	2	1	2	2	2	2	2	2	2	2	2	2	2	2	1	1	1	1	1	27
		1	1	1	1	1	1	1	1	1	1		1	1	1	1	1	1							1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	41
			2	2	2	2	2	2	2	2	2	2	2	2	2	2	1	2	2	2	2	2	2	2	2	2	2	1	2	2	2	2	2	1	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	46
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		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1					1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1	43
		1	1	1	1	1	1	1	1	1															1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	34
		1	1	1	1	1	1	1	1	1	1				1	1	1	1	1	1	1	1				1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	42
		1	1	1	1	1	1		1	1	1	1												1	1	1	1	1	1	1					1	1	1	1	1	1		1	1	1	1	1	1	1	1	31
	1	1	1	1	1	1	1	1	1	1	1															1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	35
			1	1	1	1	1	1	1	1	1															1	1	1	1	1	1	1	1	1	1		1		1	1	1	1	1	1	1	1	1	1	1	31
		1	1	1	1	1	1	1	1	1	1														1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	35
	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1		1	1	1	1	47
	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1															1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	35
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			1	1	1	1	1	1	1	1	1	1													1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	35
		1	1	1	1	1	1	1	1	1	1												1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	37
	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1										1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	40
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	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1										1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	40
		1	1	1	1	1	1	1	1	1	1																	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	32
		1	1	1	1	1	1	1	1	1			1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1				1	1	1	43
	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1														1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	36
		1	1	1	1	1	1	1	1	1	1												1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	37
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		1	1	1	1	1	1	1	1	1	1														1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	35
			1	1	1	1	1	1	1	1	1	1												1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	36
Total	**14**	**35**	**42**	**42**	**43**	**43**	**42**	**40**	**39**	**41**	**37**	**23**	**21**	**20**	**20**	**9**	**7**	**7**	**5**	**5**	**6**	**7**	**8**	**13**	**23**	**30**	**34**	**34**	**38**	**40**	**40**	**40**	**41**	**41**	**41**	**39**	**42**	**41**	**42**	**42**	**42**	**43**	**43**	**40**	**41**	**39**	**40**	**40**	**41**	

	March					April				May				June					July				Aug				Sep					Oct				Nov				Dec					Jan / Feb					total
CCC								1	1	1	1	1	1															1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	28
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	1	1	1	1	1	1	1	1	1	1	1	1	1	1													1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	38
				1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	46
			1	1	1	1	1	1	1	1	1	1														1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	35
				1	1	1	1	1	1	1	1	1																1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	31
				1	1	1	1	1	1	1	1	1															1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	33
						1	1	1	1	1	1	1	1													1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	32
			1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	47
							1	1	1	1	1	1	1	1														1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	30
								1	1	1	1	1	1				1	1	1	1	1	1							1	1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1	1	1	1	32
					1	1	1	1	1	1	1	1			1	1	1	1	1	1	1	1	1						1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	38
					1	1	1	1	1	1	1	1	1												1	1	1	1	1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1	1	1	1	1	33
				1	1	1	1	1	1	1	1	1													1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	34
			1	1	1	1	1	1	1	1	1	1	1	1											1	1	1	1	1	1	1					1	1	1	1	1	1	1	1	1	1	1	1	1	1	33
							1	1	1	1	1	1		1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	42
			1	1	1	1	1	1	1	1	1	1										1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	38
				1	1	1	1	1	1	1	1	1	1													1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	34
				1	1	1	1	1	1	1	1	1	1	1								1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	39
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						1	1	1	1	1	1	1	1							1	1	1	1	1	1	1	1	1	1	1	1			1	1	1	1			1	1	1	1	1	1	1	1	1	1	34
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Total	**1**	**1**	**7**	**15**	**19**	**22**	**24**	**27**	**28**	**28**	**28**	**28**	**18**	**10**	**7**	**7**	**8**	**8**	**8**	**9**	**10**	**13**	**12**	**11**	**16**	**20**	**20**	**23**	**26**	**27**	**27**	**25**	**25**	**27**	**26**	**27**	**28**	**27**	**27**	**28**	**27**	**28**	**28**	**28**	**28**	**28**	**28**	**28**	**28**	

Appendix L

Participation Model

Participation Model

We used the recruit survey to develop a participation model. The recruit survey includes several household and person variables related to socioeconomics, demographics, and detailed auto characteristics for all households that were eligible to participate even if they chose not to participate. The recruit survey allows us to estimate a model which predicts the probability that a given individual agrees to participate in the experiment. However, several individuals who have agreed to participate later dropped out of the experiment. Therefore, we have developed another model which predicts the probability of not dropping out for any given participant. The two models, "agree to participate" and "not drop out," are then used in combination to compute the probability of participating and not dropping out.

Table L.1 shows the "agree to participate" model. The variables shown in this table are included in the utility equation of the "agree to participate" alternative. The utility of the "disagree to participate" alternative is set to zero. The following conclusions can be drawn from the estimation results:

- The constant is negative, indicating that respondents are more likely to disagree to participate.

- Relative to inner counties (Hennepin and Ramsey Counties), respondents in the North/East (Anoka, Chisago, and Washington Counties), and Southern counties (Scott, Carver, and Dakota Counties) are less likely to participate. This is probably because as an area becomes more urban, people look for alternative ways to reduce driving while people in the suburbs are more likely to rely on driving.

- As household size increases, respondents are more likely to participate. Households with one or two autos are less likely to participate in the experiment relative to households with three autos because of the constraints on auto availability.

- The model indicates that if there is one or more leased cars in the household, the household is less likely to participate, while if one or more cars are shared among household members the household is more likely to participate.

- The effect of annual household mileage on the decision to participate is modeled through a power series expansion of degree 6. That is, miles, (miles)2, …, (miles) 6 are included in the utility equation. Table L.1 shows the coefficients of these terms, and Figure L.1 shows a plot of the resulting utility (the utility component corresponding to the mileage function) versus mileage. Figure L.1 indicates that up to a certain mileage (around 30,000 miles), households are more likely to participate as mileage increases but become less likely to participate as mileage increases further; this is expected because households with high mileage are less likely to benefit from the experiment. The variation of the utility as a function of mileage is, however, relatively flat.

- The person variables included in the model indicate that:
  - Females are more likely to participate than males;
  - Older people are more likely to participate than younger people;
  - Workers are less likely to participate than nonworkers because of time constraints;
  - People are more likely to participate as their education level increases.

Table L.1 "Agree to Participate" Model

Variable	Parameter Estimate	t-statistic
Constant	-3.66	-3.84
Household Variables		
North/East County	-0.02	-0.09
South County	-0.38	-1.66
1-person hh	-0.65	-1.59
2-persons hh	-0.37	-1.43
3-persons hh	-0.21	-0.78
1 or 2 autos in hh	-0.10	-0.46
Leased car(s) in hh	-0.40	-1.20
Shared car(s) in hh	0.28	1.38
Annual household miles	5.15E-04	1.49
(Annual household miles)^2	-5.84E-08	-1.15
(Annual household miles)^3	3.32E-12	1.01
(Annual household miles)^4	-9.65E-17	-0.91
(Annual household miles)^5	1.37E-21	0.84
(Annual household miles)^6	-7.52E-27	-0.79
Person Variables		
Female	0.28	1.50
Age: 55 or above	0.21	0.81
Worker	-0.09	-0.36
Education: tech school	1.12	2.80
Education: college/post graduate	1.29	3.39

Variable	**Parameter Estimate**	**t-statistic**
Model Statistics		
Number of observations	627	
Initial likelihood	-434.60	
Final likelihood	-348.70	
Rho-squared w.r.t. zero	0.1977	
Rho-squared w.r.t. constants	0.0701	

Figure L.1 Utility of Participating as a Function of Annual Household Mileage

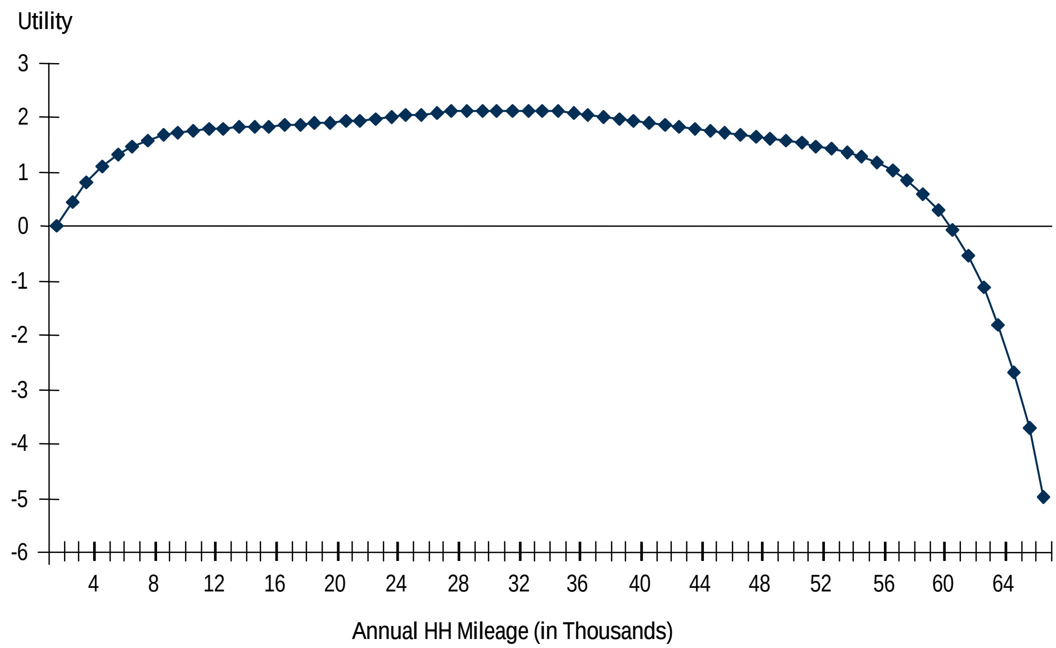


Table L.2 shows the "drop-out" model. The variables shown in this table are included in the utility equation of the "do not drop out" alternative. The utility of the "drop out" alternative is set to zero. The following conclusions can be drawn from the estimation results:

- The constant is positive, indicating that once a household has agreed to participate, the household is more likely to stay in the experiment.

- Similar to the previous model, relative to inner counties (Hennepin and Ramsey Counties), households in the North/East (Anoka, Chisago, and Washington Counties), and Southern counties (Scott, Carver, and Dakota Counties) are more likely to drop out.

- Four-person households are more likely to stay in the experiment relative to smaller households, and one-vehicle households are less likely to stay in the experiment especially because they have no other alternative to their priced vehicle when they are in treatment.

- High-income households are less likely to stay in the experiment (note that a missing income variable is included to account for households that did not provide their income information).

- The presence of a leased or shared car in the household makes the household less likely to stay in the experiment.

- The effect of annual household mileage on the decision to drop out is modeled through a power series expansion of degree 4. Figure L.2, which shows a plot of the resulting utility (the utility component corresponding to the mileage function) versus mileage, indicates that up to a certain mileage (around 10,000 miles) households are more likely to stay in the experiment as mileage increases but become less likely to stay as mileage increases further because of lower chances for mileage reduction.

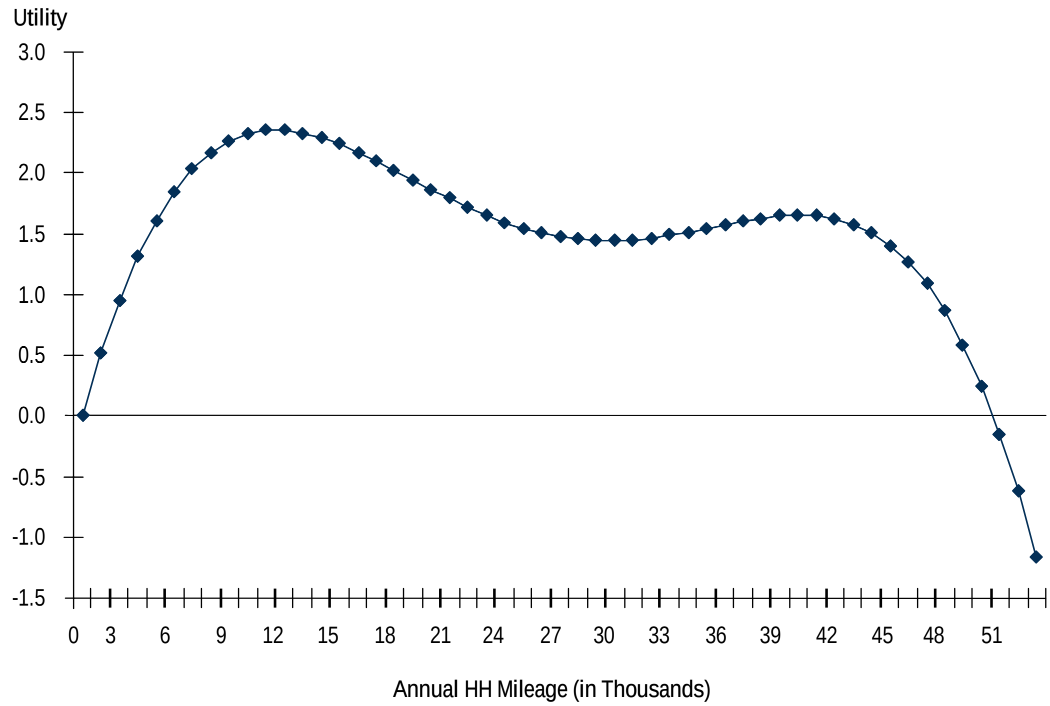
- The person variables included in the model indicate that:

  - Females are more likely to drop out than males;
  - Older people are less likely to drop out than younger people;
  - Full-time workers are more likely to drop out than part-time workers or nonworkers because of time constraints;
  - People are more likely to stay in the experiment as their education level increases.

Table L.2 "Drop-out" Model

Variable	Parameter Estimate	t-statistic
Constant	1.06	0.53
Household Variables		
North/East County	-1.14	-1.71
South County	-0.87	-1.35
1-person hh	-0.84	-0.65
2-persons hh	-1.08	-1.31
3-persons hh	-0.30	-0.39
1 auto in hh	-1.67	-1.64
Income above $75,000	-1.28	-1.85
Missing income	-1.80	-1.60
Leased car(s) in hh	-0.06	-0.06
Shared car(s) in hh	-0.12	-0.21
Annual household miles	5.56E-04	1.28
(Annual household miles)^2	-4.32E-08	-1.17
(Annual household miles)^3	1.23E-12	1.04
(Annual household miles)^4	-1.18E-17	-0.93
Person Variables		
Female	-0.31	-0.54
Age: 35-54	0.64	0.87
Age: 55 or above	1.98	1.86
Full-time worker	-0.70	-1.01
Education: college/post graduate	0.93	1.50
Model Statistics		
Number of observations	126	
Initial likelihood	-87.34	
Final likelihood	-52.45	
Rho-squared w.r.t. zero	0.3995	
Rho-squared w.r.t. constants	0.1823	

Figure L.2 Utility of Not Dropping out as a Function of Annual Household Mileage



Figures L.3 and L.4 show the number and percentage, respectively, of participants and nonparticipants as a function of the predicted probability of participation (obtained from the two models shown above). For nonparticipants, the distribution is skewed towards the left, which makes sense because nonparticipants should have lower probabilities of participation. For participants, the distribution is shifted more towards the right.

Figure L.3 Number of Participants and Nonparticipants as a Function of the Predicted Probability of Participation

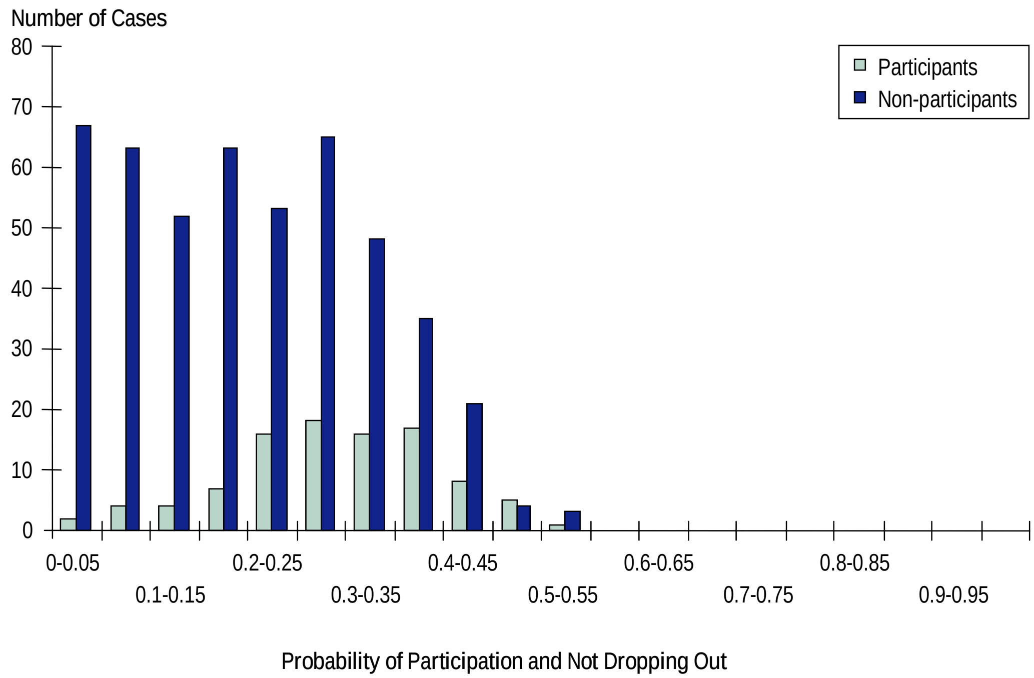
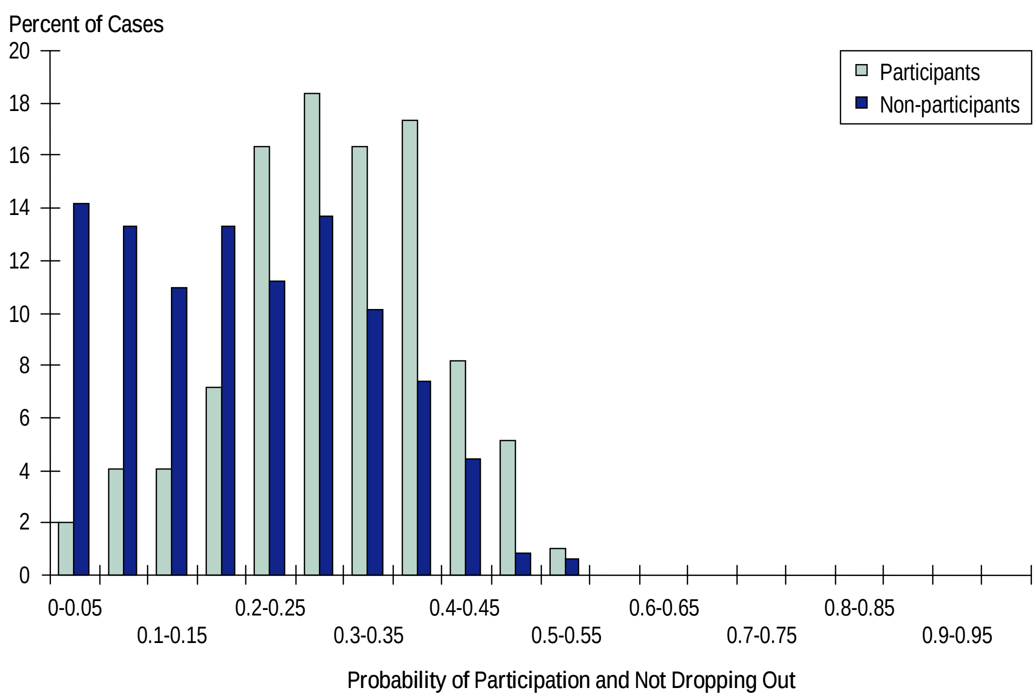


Figure L.4 Percent of Participants and Nonparticipants as a Function of the Predicted Probability of Participation



Appendix M

The Matching Method

The Matching Method

In this section, we show the mathematical details of the implementation of the matching method.

Let i denote a household in a treatment group, and let j denote a household in a comparison group. The weight W_{ij} given to household j is defined as:

$W_{ij} = \dfrac{K}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}}$ ....(1), where K, γ_1, and γ_2 are parameters to be estimated, P_i is the probability of participation for household i, and M_i is the mileage in the initial control period of household i.

K, γ_1, and γ_2 are determined from the following equations:

$\sum_j W_{ij} = 1$ ...(2) (sum of weights over all nontreated households equals 1)

$\sum_j W_{ij} P_j = P_i$ ...(3) (weighted probability of participation of nontreated households equals probability of participation of treated household)

$\sum_j W_{ij} M_j = M_i$ ...(4) (weighted control period mileage of nontreated households equals control period mileage of treated household)

Solution:

From equations (1) and (2), $\sum_j W_{ij} = \sum_j \dfrac{K}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}} = 1$

$$\Rightarrow K = \frac{1}{\sum_j \dfrac{1}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}}} \quad \text{...(5)}$$

Substituting equation (5) in equations (3) and (4),

$$\sum_j W_{ij} P_j = P_i \Rightarrow \sum_j \frac{K}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}} P_j = P_i$$

$$\Rightarrow \sum_j \frac{\dfrac{1}{\sum_j \dfrac{1}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}}}}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}} P_j = P_i$$

$$\Rightarrow \sum_j \frac{1}{\left[\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}\right] \sum_j \dfrac{1}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}}} P_j = P_i \ \ldots\ldots\ldots\ldots\ldots\ldots(6)$$

$$\sum_j W_{ij} M_j = M_i \Rightarrow \sum_j \frac{K}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}} M_j = M_i$$

$$\Rightarrow \sum_j \frac{\dfrac{1}{\sum_j \dfrac{1}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}}}}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}} M_j = M_i$$

$$\Rightarrow \sum_j \frac{1}{\left[\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}\right] \sum_j \dfrac{1}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}}} M_j = M_i \ \ldots\ldots\ldots\ldots\ldots(7)$$

Solving Equations (6) and (7) in Excel is equivalent to using the Solver to minimize the quantity:

$$\left[P_i - \sum_j \frac{1}{\left[\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}\right] \sum_j \dfrac{1}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}}} P_j\right]^2$$

$$+ \left[M_i - \sum_j \frac{1}{\left[\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}\right] \sum_j \dfrac{1}{\left|P_i - P_j\right|^{\gamma_1} + \left|M_i - M_j\right|^{\gamma_2}}} M_j\right]^2$$