Evaluating ''Cash-for-Clunkers'': Program Effects on Auto Sales and the Environment

Shanjun Li, Joshua Linn, Elisheba Spiller

www.elsevier.com/locate/jeem

PII: S0095-0696(12)00067-8
DOI: http://dx.doi.org/10.1016/j.jeem.2012.07.004
Reference: YJEEM1746

To appear in: *Journal of Environmental Economics and Management*

Received date: 3 October 2011
Revised date: 9 July 2012
Accepted date: 13 July 2012

Cite this article as: Shanjun Li, Joshua Linn and Elisheba Spiller, Evaluating ''Cash-for-Clunkers'': Program Effects on Auto Sales and the Environment, *Journal of Environmental Economics and Management,* http://dx.doi.org/10.1016/j.jeem.2012.07.004

Evaluating "Cash-for-Clunkers": Program Effects on Auto Sales and the Environment[1]

Shanjun Li, Joshua Linn, and Elisheba Spiller

Abstract

"Cash-for-Clunkers" was a $3 billion program that attempted to stimulate the U.S. economy and improve the environment by encouraging consumers to retire older vehicles and purchase fuel-efficient new vehicles. We investigate the effects of this program on new vehicle sales and the environment. Using Canada as the control group in a difference-in-differences framework, we find that, of the 0.68 million transactions that occurred under the program, the program increased new vehicle sales only by about 0.37 million during July and August of 2009, implying that approximately 45 percent of the spending went to consumers who would have purchased a new vehicle anyway. Our results cannot reject the hypothesis that there is little or no gain in sales beyond 2009. The program will reduce CO_2 emissions by only 9 to 28.2 million tons based on upper and lower bounds of the estimate of the program effect on sales, implying a cost per ton ranging from $92 to $288 even after accounting for reduced criteria pollutants.

Keywords: Stimulus, Cash-for-Clunkers, Auto Demand, CO_2 emissions
JEL Classifications: Q50, H23, L62

---

[1] Shanjun Li is an Assistant Professor in the Dyson School of Applied Economics and Management at Cornell University, 424 Warren Hall, Ithaca, NY, 14853, email: sl2248@cornell.edu, phone: (607)255-1832, fax: (607)255-9984. Joshua Linn is a Fellow at Resources for the Future (RFF), 1616 P Street NW, Washington, DC, 20036, email: linn@rff.org, phone: (202)328-5047, fax: (202)939-3460. Elisheba Spiller is a Post-Doctoral Research Fellow at RFF, email spiller@rff.org, phone: (202)328-5147, fax: (202)939-3460. We thank Soren Anderson, Antonio Bento, Maureen Cropper, Robert Hammond, Paul Portney, Kevin Roth, and Chris Timmins for helpful comments and Jeffrey Ferris, Marissa Meir and Shun Chonabayashi for excellent research assistance. This paper supersedes RFF Discussion paper 10-39 titled Evaluating "Cash for Clunkers": Program Effects on Auto Sales, Jobs and the Environment.

1. Introduction

Amid a major recession and growing concerns about the environment, many countries have adopted programs that encourage consumers to trade in their old, inefficient vehicles in exchange for more efficient ones. In the United States, the Cash-for-Clunkers program provided eligible consumers a $3,500 or $4,500 rebate when trading in an old vehicle and purchasing or leasing a new vehicle. Many other countries, such as France, the United Kingdom, and Germany, have similar programs, which generally share the same goals: to provide stimulus to the economy by increasing auto sales, and to improve the environment. The U.S. program received enormous media attention and many considered the program to be a great success; during the program's nearly one-month run, it generated 678,359 eligible transactions and had a cost of $2.85 billion.[2] But as a matter of economic theory, it is typically quite difficult to achieve multiple goals with a single policy. The large fiscal cost and public enthusiasm for these programs, and their widespread use around the world, raise the question of just how effective they are at meeting their economic and environmental goals.

This study estimates the composition of the fleet of vehicles that would have been sold in the absence of the program, permitting a comprehensive evaluation of the program effect on vehicle sales, the environment and economic activity. First, we examine the program effects on the quantity and composition of new vehicle sales both during the program and in the several months before and after the program. Many observers of the program were concerned that it would primarily pull demand from adjacent months, thus providing little short-term stimulus, while others believed that the program would pull demand from several years in the future (Council of Economic Advisors, 2009). Furthermore, we are interested in analyzing whether the program affected the fuel economy distribution of new vehicle sales. Because Cash for Clunkers was promoted for stimulus and environmental reasons, we focus on two types of changes in consumer behavior caused by the program: switching from purchasing low fuel-efficiency to

[2] Transportation Secretary LaHood declared the program to be "wildly successful" at the end of the program, while two Op-Ed articles in the Wall Street Journal on August 2nd and 3rd raised doubts about whether the program truly increased sales and stimulated the economy. They argued that the program would most likely result in the shifting of future vehicle demand to the present and could hurt the sales of other goods.

high fuel-efficiency vehicles, and shifting the purchase time to take advantage of the program's incentives.

Second, we evaluate the program's cost-effectiveness in reducing gasoline consumption and carbon dioxide (CO_2) emissions by comparing total gasoline consumption as well as emissions of CO_2 and criteria pollutants with and without the program. There exist many federal subsidy programs aiming to reduce U.S. gasoline consumption and CO_2 emissions such as tax credits for ethanol blending and income tax incentives for purchasing hybrid vehicles. Our cost-effectiveness analysis permits a comparison across these different programs.

The basis for these evaluations is the difference-in-differences (DID) analysis in a vehicle demand framework based on monthly sales of new vehicles by model from 2007 to 2009. The U.S. market constitutes the treatment group in the analysis. We use Canada as the control group based on two observations as well as some statistical evidence. First, Canada did not have a similar program, while nearly a dozen European countries did in 2008 and 2009. Second, the Canadian auto market is probably the most similar to the U.S. market: in both countries in recent years before the recession, about 13-14 percent of households annually purchased a new vehicle; characteristics of vehicles sold are similar; and pre-program time trends are similar. The two main identifying assumptions are that the program did not affect the Canadian vehicle market and that the differential effect of the 2008-2009 recession across the two countries' vehicle markets did not vary over the months in 2009. We provide some evidence supporting these assumptions in Section 3.3.

The DID analysis shows that the program increased sales of vehicles that were eligible for the rebate (eligible vehicles) and lowered sales of ineligible vehicles during the program period. Furthermore, within eligible vehicles, the positive effect was larger for those with higher fuel efficiency – which yield a higher rebate under the program. The negative effect on ineligible vehicles was stronger for those that barely missed the eligibility requirement, implying that the program caused consumers to substitute from these vehicles to eligible vehicles. We find that the program reduced sales in the months before and especially after the program, and that the effect on sales weakened over time. The empirical results thus suggest that the program shifted consumer demand from ineligible vehicles to eligible ones as well as from pre- and post-program periods to program periods, with the inter-temporal shift having the strongest impact.

With the parameter estimates from the DID analysis, we simulate vehicle sales in the counterfactual scenario of no program. We find that the program increased sales by only 0.37 million during July and August of 2009, implying that of the 0.66 million vehicles in our sample that were purchased under the program, 0.29 million would have been purchased anyway during these two months. The program effect on vehicle sales erodes further when we look at a longer time horizon: the increase in vehicle sales during June to December of 2009 was practically zero. In addition, our simulation results show that Toyota, Honda and Nissan benefited from the program disproportionally more than other firms: with a combined market share of around 38 percent before the program, they accounted for more than 50 percent of the increased sales. The U.S.-based automakers and their dealers were facing especially low revenues prior to the program, and although sales of the vehicles produced by these automakers increased by about 14 percent in July and August, over the June-December period sales increased by less than one percent. Therefore, we conclude that the program provided little economic stimulus beyond late 2009.

Based on the simulation results for vehicle sales, we estimate the differences in total gasoline consumption, CO_2 emissions, and four criteria pollutant emissions (carbon monoxide, volatile organic compounds, nitrogen oxides and exhaust particulates) with and without the program. We provide the results for 12 different cases, across which parameter and behavior assumptions vary. Over the vehicles' lifetimes, the reduction in gasoline consumption ranges from 924.5 to 2,907.3 million gallons while that in CO_2 emissions ranges from 9 to 28.2 million tons. By comparison, in 2009, U.S. gasoline consumption was 141 billion gallons and CO_2 emissions from passenger vehicles were 1.1 billion metric tons. After accounting for the program's benefit in reducing criteria pollutants, we estimate that the program's cost of CO_2 emissions reduction ranged from \$92 to \$288 per ton of CO_2 while that of gasoline consumption reduction ranged from \$0.89 to \$2.80 per gallon.

Several recent studies have evaluated particular aspects of the Cash-for-Clunkers program. Knittel (2009) estimates the implied cost of the program in reducing CO_2 emissions. Council of Economic Advisors (CEA 2009) and Cooper et al. (2010) analyze program impacts on vehicle sales and employment. National Highway and Traffic Safety Administration (NHTSA, 2009) also examines program effects on gasoline consumption and the environment. The major difference between our analysis and the aforementioned studies lies in the fact that we use the DID approach to estimate counterfactual sales by vehicle model in the absence of the program. Knittel (2009) does not establish the counterfactual and does not examine program effects on vehicle sales. The other three studies estimate the sales effect based on heuristic rules and aggregate sales data and do not examine consumer substitution across models and over time.

A recent study by Mian and Sufi (2010) is more closely related to ours in that we both establish counterfactual outcomes by exploiting variations in program exposure across different areas. Rather than using Canada as a control group, that paper uses the number of "clunkers" registered in U.S. cities prior to the program as a measure of ex-ante program exposure. Variation in this measure identifies the program effect, and the paper shows an almost identical short-term effect (July and August) to ours. They argue that by as early as March 2011, the program effect was completely reversed. Copeland and Kahn (2011) use a time-series approach to examine the program effect on sales and on production. They find a slightly larger short-term effect on vehicles sales but they also conclude that by January 2010, the cumulative effect of the program on sales was essentially zero. Neither of these papers examines environmental outcomes.

Carefully analyzing the counterfactual is important for estimating the environmental benefits of the program. For example, we find a smaller cost per ton of CO_2 reduction than Knittel (2009) because we account for the difference between total CO_2 emissions during the remaining lifetime of the trade-in vehicles and the emissions from the new vehicles purchased to replace them, and the fact that the fleet of new vehicles purchased under the program is more fuel efficient than that without the program; Knittel (2009) only considers the first effect. Failing to analyze the counterfactual fleet without the program can thus underestimate the program's environmental benefit.

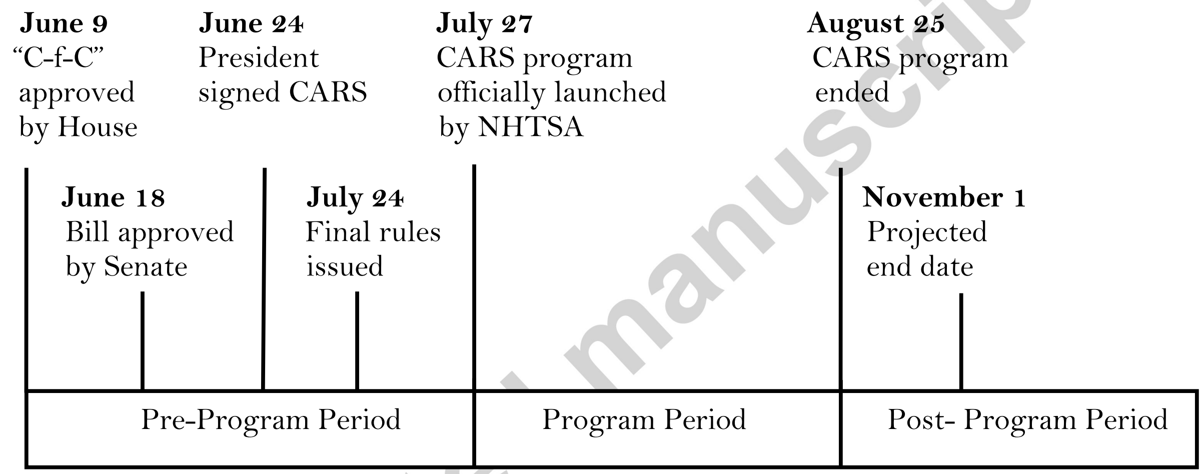
2. Background and Data

In this section, we first discuss the background of the Cash-for-Clunkers program, including the timeline and eligibility rules. Next, we present the data that are used in the empirical analysis.

2.1 Program Description

As Figure 1 shows, the Consumer Assistance to Recycle and Save (CARS) Act was passed by the House of Representatives on June 9th, 2009 and by the Senate on June 18th, and was signed into law by the President on June 24th. The law established the Cash-for-Clunkers program, a temporary program granting subsidies to individuals who trade in their older, fuel inefficient, vehicle to purchase a new and more efficient vehicle. The traded-in vehicle would then be dismantled in order to ensure that it does not return to the road. The program was officially launched on July 27th, 2009 and terminated ahead of schedule on August 25th, 2009. It generated 678,359 eligible transactions at a cost of $2.85 billion.[3] Originally, the program was planned as a $1 billion program with an end date of November 1st, 2009.

Figure 1: Timeline of the Cash-for-Clunkers Program



The Cash-for-Clunkers program was intended to reduce the number of old and less fuel efficient vehicles (i.e. clunkers) on the roads as well as shift demand towards more fuel efficient new vehicles. The program outlined four requirements that the trade-in would have to meet in order to be eligible, as shown in Table 1A. These requirements varied according to the size and class of the vehicle. The first three requirements ensured that the traded-in vehicle would otherwise be on the road had it not been for the program: the trade-in vehicle must be drivable; it must have been continually insured and registered by the same owner for the past year; and it must be less than 25 years old. The fourth rule ensured that the vehicle is in fact a "clunker": it

[3] Statistics are from press releases at http://www.cars.gov/official-information.

must have a combined fuel efficiency of 18 mpg or less (the latter two requirements were different for category 3 trucks).[4]

Table 1B shows the minimum MPG a new vehicle needed to qualify. The MPG requirement was 22 for passenger automobiles, 18 for category 1 trucks, and 15 for category 2 trucks. Category 3 trucks, on the other hand, had no minimum fuel efficiency requirement, but they could only be traded in for category 3 trucks. Finally, the manufacturer's suggested retail price (MSRP) of the new vehicle could not exceed $45,000. Table 1B shows that the stringency of the MPG requirement was greatest for passenger cars and decreased across the truck categories. For example, a new passenger car must have an MPG improvement of at least 4 over the trade-in vehicle in order to qualify for the $3,500 rebate while a 10 MPG improvement is needed for the $4,500 rebate. For a new vehicle in category 1, the requirements on the MPG improvement are 2 and 5 for the two rebate levels. The requirements become even less stringent for category 2 and 3 vehicles.

2.2 Data Description

We collect data on monthly vehicle sales for all models in the United States and Canada from 2007 to 2009 from Automotive News. We combine these data with vehicle MPG data from the Environmental Protection Agency's fuel economy database as well as vehicle prices and other characteristics from Wards' Automotive Yearbook. Our data include 16,776 observations of monthly vehicle sales. We define a model as a country-vintage-nameplate (e.g., a 2007 Toyota Camry in the United States) and we have 1,436 models in the data. Almost all models sold in Canada are available in the United States.

Table 2 provides summary statistics of the data set. Based on the eligibility rules, 1,008 of the 1,436 vehicle models meet the requirement and could be eligible for the rebate during the program (henceforth, eligible vehicles). Among the 16,776 observations, about 70 percent of sales in both countries are for eligible vehicles. As shown in the table, the eligible vehicles have much higher sales than ineligible ones. Although average sales per model in the United States are

[4] Category 1 trucks are "non-passenger automobiles" including SUVs, medium-duty passenger vehicles, pickup trucks, minivans and cargo vans. Category 2 trucks are large vans or large pickup trucks whose wheelbase exceeds 115 inches for pickups and 124 for vans. Category 3 trucks include very large pickup trucks and cargo vans.

much higher than in Canada, the number of new vehicles sold per household is 13-14 percent in both countries. On average, the eligible vehicles are cheaper and, by definition, more fuel-efficient than the ineligible ones. The average prices (sales-weighted) are very similar in the two countries across both categories. Because the share of light trucks in total sales is larger in Canada than the United States, the average fuel efficiency of vehicles in Canada in both categories is lower than in the United States.

To examine the effectiveness of the program on energy consumption and the environment, we use the public database for the Cash-for-Clunkers program from www.cars.gov. The data set provides (dealer-reported) information on the trade-in and new vehicles for each transaction during the program. There are 678,539 transactions in the data set. We remove transactions that are subject to reporting error (e.g., reported MPG that does not meet the eligibility criteria). In addition, we delete 2,278 category 3 vehicles and 6,169 leased vehicles in order to be consistent with our demand analysis of new vehicles. After removing 18,959 records, there are 659,400 observations of trade-in and new vehicles under the program.

Table 3 shows the summary statistics on trade-in and new vehicles. This table demonstrates that consumers were trading in more light trucks than cars, and that these trucks were newer than the cars. Our final sample has an average rebate amount of $4,214 and a total payment of $2.78 billion (out of $2.85 billion for all transactions in the full data set).

3. Empirical Strategy

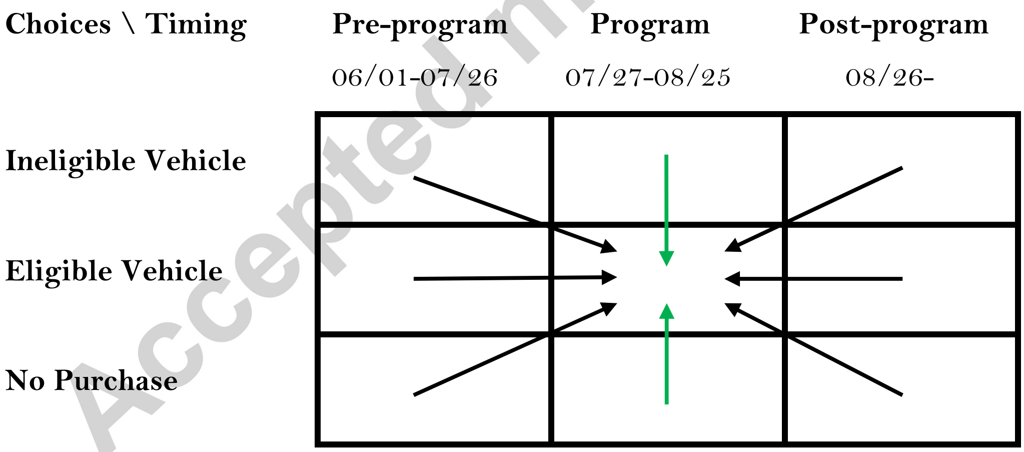
In this section, we first discuss the channels through which the program could affect vehicle sales. We then describe our empirical model.

3.1 Potential Program Effects

In our analysis, we assume that the program did not affect vehicles sales prior to June 2009. Although some consumers may have known about the bill before the House passed it on June 9th, we expect that the uncertainty surrounding the eligibility requirements as well as the bill's final passage would greatly limit its effect before June 9th. In fact, our estimation results show that there is no significant effect on sales even in June. The program period is defined from July 27th to August 25th. Although the program retrospectively recognized qualified sales from July 1st until the official start date, the total number of these pre-program sales was only 30,317, which is less than the average daily sales during the first week of the program.

Because an automobile is a durable good, the program could affect vehicle sales before, during, or after the program period. During the program period, some consumers who would have purchased an ineligible model or chosen not to purchase a new vehicle may choose to purchase an eligible model instead, as depicted in Figure 2. In addition, the program could result in consumers changing the purchase time in order to coincide with the program period (i.e., intertemporal substitution). In the absence of the program, these consumers could have purchased an eligible or an ineligible vehicle in other periods. Both channels would increase total vehicle sales and likely improve fleet fuel-efficiency. To a large extent, the design of the program in achieving the stimulus purpose was to pull demand forward from a *sufficiently* distant future when the economy was expected to be stronger. Thus, the time horizon over which the intertemporal substitution occurs is crucially important to the stimulus purpose but not so for the environmental purpose. The graph below illustrates the different substitution channels.

Figure 2: Diagram of Program Effects



The degree of these substitutions could vary over product space as well as over time for several reasons. First, there could be a stronger substitution to eligible vehicles from vehicles that barely miss the MPG requirement, compared to the substitution from vehicles that have much lower fuel efficiency. This is due to the fact that higher fuel-efficiency vehicles tend to compromise on certain amenities such as horsepower and engine size, and thus a consumer would face a smaller trade off in amenities by only marginally increasing fuel efficiency. In addition, because high MPG vehicles could be eligible for a higher rebate ($4,500 versus $3,500) the program could have a stronger effect on the vehicles eligible for the higher rebate. Second, the substitution could exhibit heterogeneity over time. Intuitively, the intertemporal substitution should be stronger right before or after the program than farther away from the program. Moreover, because the length of the program is not fixed and runs out when the designated amount of stimulus money is used up, the program could have a stronger stimulus effect at the beginning of the program period. In fact, the initial one billion dollars were used up within a week while the additional two billion dollars lasted for three weeks. Thus, we explicitly model and measure these substitution patterns in our estimation.

3.2 Empirical Model

We implement the DID method in a regression framework where the Canadian auto market is used as the control group for the U.S. market. Our DID regression estimates how the program affected vehicle sales before, during, and after the program period on a monthly basis given the vehicle's eligibility and other characteristics. The causal interpretation hinges on the identifying assumption that (unobserved) demand and supply shocks at the time of the program are the same across the two countries. Section 3.3 presents analysis suggesting that Canada is a valid control group to estimate underlying trends that are not affected by the program but that do affect vehicle sales (such as economic shocks that occur at the same time as the program).

The regression model is based on monthly sales of new vehicles by vehicle model. Let c index country (United States or Canada), t index year, m index month, and j index vehicle nameplate (e.g., Ford Focus). We define a vehicle model as a country-year-nameplate (e.g., a 2009 Ford Focus in the United States) and use ctj as the index. By including interactions of month dummies with eligibility in a regression framework, the program can have different effects across months and eligibility status. This allows us to identify both the intertemporal and cross-model substitution patterns discussed in the previous section.

We define E_{ctj} as the eligibility dummy, equal to one for any vehicle in either country that meets the program requirement (irrespective of whether the program is in effect) and zero otherwise. I_{ctj} is a dummy variable for ineligible vehicles and is equal to one for any vehicle in either country that does not meet the program requirement. P_{ctm} is a dummy variable equal to one for months when the program may have had an effect (e.g., June to December of 2009) in the United States and zero otherwise. The interaction of the program dummy with eligibility dummies indicates models that are in the program and are eligible for the rebate. Equation (1) allows us to disentangle monthly program effect on sales for eligible and ineligible vehicles.

$$log\left(q_{ctmj}\right) = E_{ctj}P_{ctm}\alpha^{E}_{tm} + I_{ctj}P_{ctm}\alpha^{I}_{tm}$$

$$+x_{ctmj}\alpha + \xi_{ctj} + E_{ctj}\eta^{E}_{cm} + I_{ctj}\eta^{I}_{cm} + E_{ctj}\delta^{E}_{tm} + I_{ctj}\delta^{I}_{tm} + \varepsilon_{ctmj}\ , \quad \textbf{(1)}$$

where q_{ctmj} is the sales of vehicle model *j*.[5] The first term on the right side captures the program effect on eligible vehicles during the relevant months while the second term captures that on ineligible vehicles. Instead of estimating the program effects for pre-program, during-program, and post-program periods, we estimate the effects month by month using this flexible specification for two reasons: (1) the program period does not coincide with a full month and our data are at the monthly level; and (2) the nature of intertemporal substitution would imply a diminishing impact farther away from (either before or after) the program period.

The first two terms capture the program effect on vehicle sales in the United States and these two terms are zero for the observations in Canada. However, interpreting these coefficients as causal program effects hinges on the assumption that Canada is a valid control group. The other variables in the equation help identify the impact of the program on sales by controlling for observed and unobserved country and vehicle attributes.

Because the program affected demand for eligible vehicles in proportion to their fuel efficiency, we must control for the effect on sales of fuel costs, which also depends on fuel efficiency.

[5] For all the regressions presented in the paper, we also estimate a multinomial logit model in the linear form (Berry 1994) where we assume that consumers have a total of *J* vehicle models plus an outside good indexed by *0* (i.e., not purchasing a new vehicle) to choose from in a given month. The dependent variable is $\log(S_{ctmj}) - \log(S_{ctm0})$ with S_{ctmj} and S_{ctm0} being the market shares of model *j* and the outside good that captures the decision of not purchasing a new vehicle. The market size is the number of households in the two countries. The results are very close to the results from the linear models shown in Section 4.

Variables in x_{ctmj} include dollars per mile (gasoline price/MPG), which is proportional to the lifetime fuel costs of the vehicle assuming the price of gasoline follows a random walk. Several recent empirical studies have documented a negative relationship between fuel costs and vehicle sales (e.g., Busse et al. 2009, Li et al. 2009, and Klier and Linn 2010). We allow the coefficient on dollars per mile to be different in these two countries. Since we control for model (country-year-nameplate) fixed effects, vehicle MPG itself is subsumed in these fixed effects.

ξ_{ctj} denotes model (i.e., country-year-nameplate) fixed effects, which control for month-invariant observed and unobserved vehicle attributes (such as horsepower, weight, and product quality), as well as month-invariant demand shocks at the model level. η^E_{cm} and η^I_{cm} are country-month fixed effects to capture country-specific seasonality for eligible and ineligible vehicles (such as the December holiday effect). ξ_{ctj}, η^E_{cm}, and η^I_{cm} are all country-specific fixed effects, controlling for country-specific demand and supply shocks that affect the level of vehicle sales (these would be equivalent to household or firm dummies in a canonical DID example). Because the fixed effects vary by nameplate-year-country, we allow for the possibility that the recession or other shocks affected the U.S. and Canadian vehicle markets differently *each year*. δ^E_{tm} and δ^I_{tm} are year-month fixed effects for eligible and ineligible vehicles (these would be equivalent to time dummies in a canonical DID example) common across countries. Because these fixed effects are used to capture demand shocks for the two groups of vehicles that are common in the two automobile markets, they give rise to the control group interpretation for the Canadian market.[6] Finally, ε_{ctmj} is the random demand shock.

Although equation (1) provides a starting point for our analysis, it does not allow heterogeneous program effects across vehicles within the same eligibility category. Heterogeneous effects could exist among both eligible and ineligible vehicles for the reasons noted in the previous section. First, since there are two rebate levels ($3,500 and $4,500) and the size of the rebate depends on the difference between the MPG of the new vehicle and that of the trade-in vehicle, consumers may substitute towards eligible vehicles with higher MPGs as these vehicles are more likely to provide them with a $4,500 rebate. Second, the program effect on ineligible vehicles could be correlated with fuel efficiency as well: consumers are more likely to switch from barely ineligible vehicles to eligible vehicles, rather than substitute away from vehicles much farther from the eligibility cut-off. Due to the trade-offs between vehicle size/horsepower and fuel efficiency, consumers likely suffer a smaller sacrifice in vehicle size or horsepower by switching from barely ineligible vehicles to eligible ones, rather than from

[6] Because not all models are available in both countries, we cannot use year-month-model fixed effects.

vehicles that are far below the MPG requirements. To capture the heterogeneous effect, we estimate equation (2) as our main specification.

$$log\left(q_{ctmj}\right) = E_{ctj}P_{ctm}\alpha^{E}_{tm} + E_{ctj}P_{ctm}\left|\Delta GPM_{ctj}\right|\beta^{E}_{tm}$$

$$+I_{ctj}P_{ctm}\alpha^{I}_{tm} + I_{ctj}P_{ctm}\left|\Delta GPM_{ctj}\right|\beta^{I}_{tm}$$

$$+x_{ctmj}\alpha + \xi_{ctj} + E_{ctj}\eta^{E}_{cm} + I_{ctj}\eta^{I}_{cm} + E_{ctj}\delta^{E}_{tm} + I_{ctj}\delta^{I}_{tm} + \varepsilon_{ctmj} \quad \textbf{. (2)}$$

GPM is gallons per mile and $\Delta GPM_j = 1/MPG_j - 1/MPG^*$, where MPG^* is the MPG requirement for rebate eligibility, which varies across vehicle categories as discussed in Section 2.1. Thus, $\left|\Delta GPM_{ctj}\right|$ measures how far a vehicle's fuel efficiency is from the eligibility requirement. The farther away a vehicle's fuel efficiency is from the requirement (for either an eligible or ineligible vehicle), the larger the variable is. The second and fourth terms on the right side of the equation capture the heterogeneous program effect for vehicles within the same eligibility category. As the results show below, although equations (1) and (2) provide similar estimation results for the program effect on vehicle sales, equation (2) leads to a much larger effect on average vehicle fuel economy (and hence a larger environmental benefit and energy savings).

3.3 Canada as the Control Group

In this section, we provide qualitative and quantitative support for using Canada as the control group. First, Canada did not have a similar program, whereas many European countries including Germany, France, Italy and Spain did in 2008 and 2009. Although Canada has a Retire Your Ride Program that started in January 2009, the program is not comparable to the Cash-for-Clunkers program for at least three reasons. First, the program provides only CA$300 worth of credit for eligible participants (owners of pre-1996 model-year vehicles that are in running condition), compared to $3,500 or $4,500 offered in the United States. Second, the goal of the Canadian program is to improve air quality by encouraging people to use environmental-friendly transportation, so the program is not tied to new vehicle purchases. Depending on the province, the credit can be a public transit pass, a membership to a car-sharing program, cash, or a rebate on the purchase of a 2004 or newer vehicle. Third, the program only retired about 60,000 vehicles during the first 15 months. Therefore, its effect on new vehicle sales (about 1.6 million annually) should be negligible.

The second justification for using the Canadian auto market as the control group is that it is probably the most similar to the U.S. market. About 13-14 percent of households purchased a new vehicle in recent years before the economic downturn in both countries. Table 2 also shows that the vehicles sold have similar characteristics, although the U.S. market has a larger set of models. Figure 3 depicts monthly sales in logarithm of all, eligible, and ineligible new vehicles in the two countries from 2007 to 2009. By and large, the two series track each other well. A noticeable difference is that sales in Canada seem to have stronger seasonality (e.g., a larger hump during March-May each year), suggesting the importance of controlling for country-specific seasonality in our analysis.

Our empirical models given in equations (1) and (2) control for unobservables in several dimensions by including model fixed effects ξ_{ctj}, common year-month fixed effects δ_{tm}, and country-specific seasonality η_{cm}. Nevertheless, as we discussed above, the unbiasedness of the coefficient estimates hinges on the identifying assumption that the time trends in demand and supply are the same in the two countries. Otherwise, we risk interpreting preexisting differences in time trends as the effect of the program.

The economic downturn that started in the second half of 2008 raises a particular concern that the demand and supply trends were not similar in the two markets. The recession in the United States was driven by the housing market crisis; the mortgage default rate increased dramatically and housing prices fell sharply at the onset of the crisis. By comparison, housing prices in Canada continued to increase until late 2008. In addition, the credit market in Canada was not impaired and did not experience the same "credit crunch" as the United States. As a result, the downturn in Canada was milder and the auto market in Canada did not contract as much as in the United States. [7,8]

It is important to note that because equation (2) includes nameplate-country-year fixed effects, our model allows for the possibility that the recession differentially affected the U.S. and Canadian markets. For example, the estimated program effects would not be biased if the auto

[7] We examine the trends of durable goods sales across both countries during this time and find similar pre-trend declines in spending across electronics, appliances, and new vehicles.
[8] Although GDP growth, employment, and household spending slowed in Canada, the decrease in total new vehicle sales in the second half of 2008 was less severe in Canada: sales dropped 1.1% in Canada in 2008 (against a 1.5% increase in 2007) while they dropped 18% in the United States (against a 3% drop in 2007).

market contracted in 2008 to different degrees in the U.S. and Canada. Nevertheless, we must maintain the assumption that the differences across countries in the effects of the recession do not vary over the year, which raises two concerns. First, we control for seasonality by including country-month fixed effects, but the estimated fixed effects would be biased if, for example, the downturn in August 2008 was more severe than in other months in 2008; in turn this would bias the estimated program effects. To address this concern, we drop the data from June to December of 2008 as an alternative to the estimation using the full data set. If the downturn were causing significant bias, we would expect to obtain different results by omitting these observations. As we show below, we obtain qualitatively similar results from these two estimations. The second concern about the recession is that the effects of the recession immediately before, during, or after the program may have been different than the effects at other times during 2009. In the difference-in-difference framework, we maintain the identifying assumption that the relative effects of the recession on the United States and Canada were similar throughout 2009. If the recession had a larger negative effect on the U.S. market during the months prior to the program than during the program, the estimated program effects would be biased away from zero.

This identifying assumption cannot be directly tested, but we can take advantage of the data before the program period to examine differences in pre-existing trends. Similarity before the program would support the assumption that the trends are the same during and afterwards. To that end, we estimate equation (2) without the first four terms on the right hand side using data before June 2009 (i.e., before the program affected the market). Figure 4 plots the aggregate monthly sales after removing the effects from observed variables (x_{ctmj}), time trends δ_{tm}, and seasonality η_{cm}. The three panels show that underlying vehicles sales (i.e., residuals) track each other quite well in the two countries before the program.

To gauge the importance of using the Canadian market as the control group, we estimate a model without the control group and present the results in the online appendix (Appendix Tables 2 and 3) posted at the journal's online repository of supplemental material, which can be accessed via www.aere.org/journals). We find much larger program effects on vehicle sales from this analysis than the DID analysis in both the short and long run. In addition, we do not find that the positive sales effect erodes over time, suggesting a lack of evidence of intertemporal substitution. This demonstrates the importance of having a valid control group; ignoring the underlying trend biases the results and causes the program to look much more successful than it truly was. Thus, in the main text, we focus on the DID analysis as our main specification.

4. Estimation Results

We first present parameter estimates for equations (1) and (2). We then discuss program effects on vehicle sales and fuel economy implied by these parameter estimates.

4.1 Difference-in-Differences Results

Table 4 reports parameter estimates and standard errors for three regressions. The first regression is equation (1) while the second one is equation (2), both using the full sample. The third estimation is for equation (2) based on the sample without the second half of 2008 (shorter sample). We only report the coefficient estimates associated with program effects (June to December of 2009) for the two groups of vehicles, noting that the full set of control variables described after equation (1) is included in the regressions; estimates of the other coefficients have the expected signs and are available upon request.[9] In the second and third regressions, we include the interaction of the vehicle eligible dummy and $|\Delta GPM|$ to allow for heterogeneous effects across vehicles.[10] For example, in the top panel, the first row shows the effect of the program on sales of eligible vehicles in June, i.e., before the program begins. The second row shows whether the effect is larger for vehicles that are further from the MPG requirement. Subsequent rows show analogous coefficients for other months, and the bottom panel reports coefficient estimates for ineligible vehicles. Throughout the paper, standard errors are constructed using block bootstrap and are robust to heteroskedasticity and serial correlation within a vehicle model (country-year-nameplate). We also estimate standard errors with two alternative block definitions: country-nameplate and nameplate. The standard errors are slightly smaller under both alternative definitions. As a conservative measure, we present the standard errors using a vehicle model as a block in the main text. The alternative standard errors for the simulations of program effects are presented in the online appendix (Appendix Table 4).

Overall, the parameter estimates have the expected signs. The directions of the program effect on sales suggested by the parameter estimates are similar across all three estimations, and

[9] For the second regression, the coefficient estimates on dollars per mile are -9.877 (1.249) for Canada, and -10.075 (1.254) for the United States. The estimates are negative as well in the other two regressions.

[10] The mean of $|\Delta GPM|$ for eligible vehicles in 2009 in the United States is 0.67 with a range from 0 to 2.61. The mean of $|\Delta GPM|$ for ineligible vehicles is 0.67 with a range from 0.21 to 1.70.

we focus on the full-sample results. The fourth column in the top panel in Table 4 shows the parameter estimates using the full sample. The two coefficient estimates for June suggest that the program reduced sales of eligible vehicles but the reduction is smaller for high MPG vehicles, both without statistical significance. The two coefficient estimates for July capture the combined effects from the pre-program period (July 1^{st}-26^{th}) and the program period (27^{th}-31^{st}). We would expect a decrease in sales during the pre-program period and an increase during the program period. Therefore, the combined effect could be positive or negative. The coefficient estimates using the full sample suggests that the program reduced the sales of eligible vehicles with low MPG while it increased the sales of those with high MPG. Similarly, the coefficients for August capture the combined effect during the program (August 1^{st}-25^{th}) and post-program (August 26^{th}-31^{th}). The coefficient estimates imply that the combined effect on eligible vehicles was positive and that the increase in sales was larger for eligible vehicles with high MPG. These results imply that the positive program effect outweighed the negative intertemporal substitution effect in both July and August.

The coefficient estimates for September suggest that the program reduced sales of eligible vehicles and that the decrease in sales was larger for eligible vehicles with high MPGs, consistent with consumers moving purchases forward to take advantage of the program. The parameter estimates for October and November suggest a negative effect on sales but the estimates are not statistically significant.

For ineligible vehicles, the parameter estimates suggest a negative effect from July to December and a larger effect for vehicles that miss the MPG requirement by a smaller margin (e.g., a smaller $|\Delta GPM|$). This is consistent with the fact that when consumers switch from these vehicles to eligible vehicles, they do not need to make a large sacrifice in other vehicle attributes such as horsepower and size, as discussed in Section 3. The third column shows that the results are qualitatively similar for the short sample, both for eligible and ineligible vehicles.

It is important to point out that our empirical model assumes that there are no interactions between the two markets (e.g., the Cash-for-Clunkers program does not affect the Canadian market). Sales may be correlated across countries for a variety of reasons, but a particular concern for the empirical strategy would be if demand in the U.S. affects the availability or prices of vehicles in Canada. For example, during the program the greater U.S. demand for eligible vehicles could cause manufacturers to divert to the U.S. market eligible vehicles that would otherwise have been supplied to Canada.[11] This would decrease sales of eligible vehicles in Canada, and potentially bias the estimated program effects away from zero. In that sense we consider the reported estimates to be an upper bound, which strengthens the main conclusion that the program had a very small effect on total sales.[12]

4.2 Program Effect on New Vehicle Sales and Fuel Efficiency

Based on the parameter estimates from Table 4, we simulate new vehicle sales under the counterfactual scenario without the Cash-for-Clunkers program. The two plots in Figure 5 show sales effects for eligible and ineligible vehicles from June to December of 2009 for the full sample based on parameter estimates from equation (2). Dashed curves represent the 90 percent confidence intervals estimated by bootstrap. The point estimates show the differences between observed and simulated sales. The corresponding plots in Figure 6 are based on parameter estimates using the short pre-program sample.

The results in both figures demonstrate the two channels through which the program affects vehicles sales (as discussed in Section 3.1). First, the sales of eligible vehicles increased in July and August but decreased in adjacent months, implying that some consumers shifted their purchase timing. Second, the program had a strong positive effect for eligible vehicles in August but a negative effect for ineligible vehicles from July to December, especially in August, suggesting that some consumers switched from ineligible vehicles to eligible vehicles.

The effect on sales in June was negative but not statistically different from zero in both estimations, supporting our modeling assumption that the program effect before June was negligible. Because the program was implemented from July 27th-August 25th, the effect on total sales in July and August captures the (positive) effect during the program period and the (negative) effect due to intertemporal substitution just before or after the program. The net effects are both positive in July and August, although the effect in July is not statistically

[11] The abrupt start of the program, the short program-period, and the large vehicle inventories before the program started make this less likely to have occurred.
[12] Another interaction between the two countries can occur if manufacturers made strategic pricing decisions across both countries, thus the boom in demand due to the program in the United States could potentially affect supply in Canada. However, the short timing of the program and the relatively small amount of extra vehicles sold during the period most likely mitigates this effect.

significant in the second estimation. The sales effects are all negative in September to November from both figures, particularly in the second estimation.

Figure 7 shows the cumulative effects over different time horizons. The left-most point shows the cumulative effect during July-August. The points to the right show that the positive effects eroded over time. The top plot (based on the full sample) shows that the net effect is not statistically different from zero by the end of October. The bottom plot (based on the short pre-program sample) shows the same result by the end of September. Both plots show that the program likely had a short-lived effect on total vehicle sales.

Panel 1 of Table 5 reports monthly observed and simulated sales of new vehicles from June to December of 2009. Column (1) gives the observed sales while columns (2) and (3) provide the estimated sales effects and standard errors based on the parameter estimates from equation (1) using the full sample. Columns (4) to (5) provide results based on the parameter estimates from equation (2) using the full sample. Columns (6) and (7) are results using the short pre-program sample.

The estimated sales effects are similar across all three regressions. The cumulative effect on sales during July and August is estimated to range from 345,000 units to 405,000. Specification two provides an estimate of 370,000, in the middle of the range. This suggests that out of the 660,000 program participants, about 290,000 would have purchased a new vehicle during July and August even without the program. This underscores that one cannot take the number of vehicles sold through the program as the net program effect on vehicle sales. In addition, the estimate suggests that about 45 percent of the total spending ($1.4 billion) went to consumers who would have purchased a new vehicle anyway. Looking at a longer horizon, neither of the estimates suggests a net gain in sales during the period from June to December. Our estimate of the short-term effect on sales of about 360,000 is essentially identical to that of Main and Sufi (2010), despite the fact that different control groups are used. The point estimate is smaller than the 450,000 units from Copeland and Kahn (2011), but their estimate is within the 90 percent confidence interval of ours. In addition, all three studies broadly conclude that the program effect on sales is short-lived, with ours suggesting an even shorter effect.[13]

[13] Copeland and Kahn (2011) argue that Canada had a milder downturn than the United States and as a result the rebound in the second half of 2009 could be milder as well. If our model was not able to address

The second and third panels in Table 5 show the program effect on the average MPG and GPM (gallons per 100 miles) of the new vehicles for two time horizons: July-August, and June-December. Although the three regressions provide similar estimates of the program's effect on sales, the regression based on equation (1) yields a smaller estimate of the program's effect on vehicle fuel efficiency. During July and August, the program increased the average MPG of new vehicles by 0.65 (from 22.72 to 23.37) based on the second regression, compared to only 0.23 based on the first regression. This highlights the importance of allowing heterogeneous effects as in equation (2) in evaluating the program impact. Over a longer time horizon, the effect on average MPG diminished: although the program increased sales of high MPG vehicles in July and August, it actually reduced sales of those vehicles in other months. Although our results suggest that the net effect of the program on vehicle sales was likely zero by the end of 2009, the program did increase the average MPG of new vehicles purchased.

Table 6 reports the sales effects for individual firms during July-August, and June-December of 2009. Toyota experienced the biggest increase in sales while Chrysler experienced the smallest in both time horizons based on the results from the full sample. Although accounting for less than 40 percent of the market share, the three Japanese firms accounted for over 50 percent of the sales increase because they offer more fuel-efficient models than the U.S.-based firms. Although the results for the period of June-December provide evidence that the program did not lead to significant shifts in market shares among automakers, the (relatively small) increase in sales could have provided important, although short-lived, support to cash-stricken GM and Chrysler (Hortaçsu et al. 2011).

5. Program Effects on Gasoline Consumption and the Environment

This section evaluates the effectiveness of the program in reducing gasoline consumption and CO_2 emissions. To that end, we compare the observed outcomes (i.e., gasoline consumption and CO_2 emissions) with the counterfactual outcomes in the absence of the program. In this section, we first discuss our method and then present the results.

this, we should have under-estimated the negative effect on vehicle sales from September to December of 2009.

5.1 Method

The program affected gasoline consumption and pollution through two channels. First, the program changed the fleet of new vehicles by causing some consumers to switch from fuel-inefficient vehicles to fuel-efficient vehicles, and by causing other consumers to purchase a new vehicle when they would not have otherwise. Second, it affected the fleet of used vehicles because the trade-in vehicles were scrapped. A complete analysis of the two channels would involve an equilibrium model of the auto market (including both new and used vehicles) that includes the dynamic effects of the program on both channels in a unifying framework.

Instead, we investigate the two channels based on the results from the previous section together with some simplifying assumptions. The first assumption is that the scrappage of the trade-in vehicles did not affect the remaining fleet of used vehicles. To the extent that the program reduced the availability of used vehicles in the second-hand market and hence increased used vehicle prices and prolonged their service, our analysis would over-estimate the energy and environmental benefits of the program.

The second assumption concerns the long-term program effect on vehicle sales. Because of the difficulty in obtaining precise estimates of the sales effect during June to December, we estimate the environmental and energy impacts based on two alternative scenarios. Our results based on both samples cannot reject a zero net effect during June to December of 2009. That is, total sales of new vehicles under the counterfactual would be the same as the observed total sales. We maintain this assumption in the first scenario, which we report in the main text.

In an alternative scenario, which we report in the online appendix 3, we allow for the possibility that the program increased June-December sales. In constructing this scenario, we note that the 90% upper bound for the sales effect in June to December is 834,337 units, while that for July-August is 566,145. Because of intertemporal substitution the program effect is unlikely to be larger for longer time horizons; therefore, we use 566,145 as the estimate for the June-December effect in the alternate scenario.

To estimate the environmental and energy impacts, we compare gasoline consumption in the actual (with the policy) and counterfactual (without the policy) scenarios. This difference can be represented by the difference between the objects in (3) and (4). Equation (3) is the actual gasoline consumption over the lifetime of the vehicles sold from June to December of 2009:

$$GAS = \sum_j (q_j * VMT_j * GPM_j), \quad (3)$$

where q_j is the total sales of vehicles of model *j* during the period, and VMT_j is the lifetime vehicle miles traveled for model *j*. Lu (2006) estimates that the average lifetime VMT for passenger cars is 152,137 and that for light trucks is 179,954 based on the 2001 National Household Travel Survey. GPM_j is fuel consumption, which is measured in gallons per mile.

Under the two assumptions discussed above, there are two components of counterfactual gasoline consumption: (1) the amount consumed over their remaining lifetime by the clunkers that were not traded in; and (2) the amount consumed by the new vehicles that would have been purchased from June to December of 2009 (with the time horizon to be discussed further below):

$$\tilde{GAS} = \sum_{k=1}^{K} RVMT_k * GPM_k + \sum_j \tilde{q}_j * VMT_j * GPM_j, \quad (4)$$

where $RVMT_k$ is the remaining VMT of the trade-in vehicle *k*. We estimate the remaining VMT of each of the trade-in vehicles based on Lu (2006)'s estimates of age-specific survival probabilities and estimated annual VMT for passenger cars and light trucks as shown in Appendix Table 1 in the online appendix. With this information, we predict age-specific remaining VMT for each type of vehicle, which is also shown in that table. Based on this method, the average remaining VMT of trade-in vehicles is 59,716 with an average remaining lifetime of 7 years.[14] The second term in equation (4) is the total lifetime gasoline consumption of new vehicles sold from June to December in the absence of the program. $\tilde{q}_j$ is the simulated sales of model *j* based on estimation results in the previous section. We adjust $\tilde{q}_j$ proportionally so that total sales of new vehicles are the same under the two scenarios. This analysis amounts to the assumption that consumers as a whole would have kept their trade-in vehicles for their remaining lifetime and in addition purchased the same number of new vehicles during June to December as in the with-policy scenario. The total effect of the policy during the 7-month period

[14] We compared the trade-in vehicles to the vehicles from the 2001 National Household Survey (NHTS), which is a national survey on vehicle holdings and travel behavior. On average, the trade-in vehicles have higher mileage than the vehicles with the same age from the 2001 NHTS. The difference is larger for relatively new vehicles. Therefore, our analysis could overestimate the remaining lifetime of the trade-in vehicles and the environmental benefit of the program. Nevertheless, the majority of the trade-in vehicles are 10-20 years old and the average MPG of these vehicles are quite close in these two data sets.

corresponds to the effect of taking the trade-in vehicles off the road and the effect of increasing the fuel economy of new vehicles sold during this period.

In the first scenario, counterfactual sales are estimated for June-December using the estimates from equation (2). Recall that in the second scenario we allow for the possibility that the program increased June-December sales. Because these sales are pulled forward from after the estimation sample (i.e., after 2009), we need to take a different approach to estimate counterfactual sales than for the first scenario, in which we assume that the program did not affect June-December sales. We discuss this approach in more detail in the online appendix, though the results are very similar for the two scenarios.

We conduct our analysis under two cases regarding VMT_j in the second term of equation (4). In the first case, we use lifetime VMT for cars and light trucks. This assumes that without the Cash-for-Clunkers program, people would drive more (by the amount of VMT over the remaining lifetime of the clunkers). In the second case, we adjust VMT_j for these new vehicles so that their total VMT and the VMT of the clunkers under the counterfactual would be the same as the total VMT from new vehicles sold June-December of 2009 under the program. To the extent that having more vehicles (e.g., a new vehicle and a clunker) may induce extra travel under the counterfactual, the results from these two cases may bound the true effect on gasoline consumption.

5.2 Results

Table 7 presents the results for the cost-effectiveness analysis. Panels 1 and 2 are based on the estimation results from the full sample while panels 3 and 4 are based on the short pre-program sample. Panels 1 and 3 compare the lifetime gasoline consumption of new vehicles sold June-December of 2009 with the lifetime gasoline consumption of the less fuel efficient new vehicles that would have been sold June-December without the program, plus gasoline consumption from the trade-in vehicles during their remaining lifetime. Panels 2 and 4 adjust the VMT of new vehicles under the counterfactual scenario so that the total VMT under the two scenarios are the same.

Case 1 assumes that passenger cars have an average lifetime VMT of 152,137 and light trucks of 179,954. The results show that the reduction in total gasoline consumption is about 2,930 million gallons, which is about 8 days of current U.S. gasoline consumption. The reduction occurs because of the difference between the fuel economy of traded-in vehicles and that of new vehicles (see Table 3), and because of the increase in fuel economy of the new vehicles (see Table 6). Cases 2 and 3 allow more fuel-efficient vehicles to have a higher VMT due to the lower fuel cost per mile of travel, i.e., the rebound effect. Earlier studies often find a long-run rebound effect around 0.20-0.30 while a recent study by Small and van Dender (2007) shows that the rebound effect could be declining largely due to income growth: their estimate of the rebound effect from 1966 to 2001 is 0.22 and that from 1997-2001 is 0.11. We incorporate a rebound effect of 0.1 and 0.5 in the second and third cases.[15] Because the vehicle fleet under the program is more fuel efficient than in the absence of the program, a positive rebound effect would result in a higher total VMT under the program. This would weaken the program effectiveness in reducing gasoline consumption. Therefore, the larger the rebound effect, the smaller the reduction in total gasoline consumption.

Columns (3) to (6) present the fiscal cost of a per unit reduction in gasoline consumption and CO_2 emissions. In calculating the unit cost, columns (3) and (4) take into account the benefit of the program in reducing four criteria pollutants (carbon monoxide, volatile organic compounds, nitrogen oxides, and exhaust particulates, i.e., CO, VOCs, NO_x, and exhaust $PM_{2.5}$). The emissions of these pollutants per mile of travel for trade-in vehicles are from MOBILE6, a computer program maintained by EPA that calculates emission factors for different types of vehicles. The model takes into account the fact that as a vehicle ages, the emissions level per unit of travel can increase dramatically, especially for older vehicles. Because the counterfactual scenario would lead to higher overall emissions of these criteria pollutants due to the clunkers not being scrapped, we estimate through MOBILE6 how many tons of these four pollutants are reduced due to the program. To translate these reductions into monetary terms, we assume that the average damage per ton of the four pollutants is \$74.5, \$180, \$250, and \$1,170, respectively. The average cost for carbon monoxide is the average of the range reported by McCubbin and Delucchi (1994). The other three cost parameters are the median marginal damages from Muller and Mendelsohn (2009).

[15] The average MPG of passenger cars was 21.89 and that of light trucks was 17.45 in 2000. We use these values as the average MPGs corresponding to the lifetime VMT of 152,137 for passenger cars and 179,954 for light trucks (2001 NHTS).

Columns (3) and (4) of Table 7 report the dollar costs of reducing one gallon of gasoline consumed and one ton of CO_2 through the program, with the co-benefit of reduced criteria pollutants. These costs range from \$0.89 to \$2.80 for each gallon of gasoline while the cost of reducing one ton of CO_2 ranges from \$92 to \$288. Without taking into account the co-benefit of reducing criteria pollutants, the unit costs increase as shown in columns (5) and (6): the range for the cost per gallon of reducing gasoline consumption becomes \$1.03 to \$3.24 while that for CO_2 reductions becomes \$106 to \$335.

The implied fiscal cost of CO_2 reduction from the program is much larger than the social cost of CO_2 (social marginal damages) recently estimated by the United States Government Interagency Working Group (2010). Based on three integrated assessment models, the Working Group provides a range of \$5 to \$65 per ton for 2010 emissions (in 2007 dollars) with a central value of \$21. In addition, the implied cost of CO_2 reduction from our analysis is far greater than projected marginal costs under several recent legislative proposals. For example, the allowance price for CO_2 under the Waxman-Markey cap-and-trade bill is projected to be \$17-\$22 per metric ton in 2020 in EPA's analysis in 2020 and \$28 in CBO's analysis. This suggests that there are less costly alternatives in reducing CO_2 to achieve the level of reduction in the bill (i.e., by 2020 a 17 percent reduction from 2005 emissions). However, since the Cash-for-Clunkers program also provides the benefit of stimulating the economy and the estimated cost is a cost to the government rather than the marginal abatement cost, it is perhaps not fair to compare the implied carbon cost of the program to the allowance price in a national cap-and-trade program.

To put our results in perspective, we compare the cost-effectiveness of the program with two other federal programs that use tax expenditure to reduce gasoline consumption and CO_2 emissions. The first is an excise tax credit of 51 cents per gallon of ethanol blended with gasoline (generally at a 10 percent rate) that expired at the end of 2011. Metcalf (2008) estimates that the cost of reducing gasoline consumption under the ethanol credit is about \$2 per gallon and that of reducing CO_2 emissions is over \$1,700 per ton in 2005. The second policy for comparison is the income tax credit of up to \$3,400 for hybrid vehicle purchases. Beresteanu and Li (2009) estimate that the cost of reducing gasoline consumption is about \$1.80 per gallon and the cost of reducing CO_2 emissions is \$177 per ton. Thus, the unit cost estimates of reducing gasoline consumption for both programs are comparable to the cost of the Cash-for-Clunkers program.

However, for reducing CO_2 emissions, the tax credit for ethanol is clearly dominated by the other two programs.

6. Conclusion

As part of the stimulus effort, the Cash-for-Clunkers program was so popular that it exhausted its original allocation of $1 billion within one week despite initial projections that the program would last three months. Nevertheless, while many considered the program to be a great success as a short-term stimulus measure, critics argued that the increased sales observed during the program period could be merely borrowed from immediate future months so that even the short-term effect on vehicle sales may not have been significant. Many have also raised doubts over the potential impact of the program on energy consumption and the environment.

Using a difference-in-differences approach with Canada as the control group, we have examined program effects on vehicle sales for different time-horizons as well as its impacts on pollutant emissions and gasoline consumption. The approach relies on the assumption that the program did not affect the Canadian vehicles market and that the differential effect of the recession on the U.S. vehicles market was stable throughout 2009. We find that a large portion of vehicles sold under the program was a result of demand switching from months surrounding the program: although the program increased vehicle sales by 0.37 million during July and August, the estimated net effect on sales became practically zero by the end of 2009, noting that the estimate is rather imprecise, perhaps reflecting the inherent difficulty in estimating the longer-term effect. Furthermore, if the program were to be judged as an environmental program, the implied costs of reducing gasoline consumption and CO_2 emissions are quite high: the best-case scenario suggests a cost of over $92 in government expenditure for each ton of CO_2 avoided and almost 90 cents for each gallon of reduced gasoline consumption. The analysis does not include the costs of destroying the still-useful capital of the traded-in vehicles and the environmental costs of constructing the new vehicles; including these costs would underscore the main conclusions.

These evaluations of the program reflect the inherent difficulty of using a single policy to simultaneously accomplish multiple objectives. It would be important to examine whether alternative program designs can improve effectiveness and social welfare. This is out of the scope of our static framework since a structural model would be needed that incorporates both the new and used vehicle markets. Nevertheless, some observations regarding program design can be made. First, given the unexpected popularity of the program and the much shorter program period than projected, it should be possible to achieve better environmental outcomes without hindering the stimulus effects by increasing the fuel economy requirements for new vehicles. Second, our analysis shows that about 45 percent of program expenditure was spent on consumers who would have purchased a new vehicle even in the absence of the program. This speaks to the challenge of isolating potential buyers who would not have otherwise purchased a new vehicle. In addition, the short-lived effect on sales implies that the intertemporal substitution occurred over a rather short time horizon. To the extent that the vehicle scrappage rates vary with vehicle attributes (such as class, size or fuel economy) and new vehicles are purchased to replace used vehicles, setting age thresholds based on the attributes of used vehicles could improve targeting and pull demand from a more distant future.

References:

Abrams, Burton and George Parsons, "Is Cars a Clunker?" *The Economist's Voice*, August 2009.

Beresteanu, Arie, and Shanjun Li, "Gasoline Prices, Government Support, and the Demand for Hybrid Vehicles in the U.S.," *International Economic Review*, forthcoming.

Berry, Steven, "Estimating Discrete Choice Models of Product Differentiation," *Rand Journal of Economics*, 25 (1994), 242-262.

Busse, Meghan R., Christopher R. Knittel, and Florian Zettelmeyer. 2009. Pain at the Pump: How Gasoline Prices Affect Automobile Purchasing in New and Used Markets. NBER working paper no. 15590. Cambridge, MA: National Bureau of Economic Research.

Cooper, Adam, Yen Chen and Sean McAlinden, "CAR Research Memorandum: The Economic and Fiscal Contributions of the 'Cash-for-Clunkers' Program – National and State Effects." Center for Automotive Research (2010).

Copeland, Adam and James Kahn, "The Production Impact of "Cash-for-Clunkers": Implications for Stabilization Policy." Federal Reserve Bank of New York Staff Report (2011).

Council of Economic Advisors. "Economic Analysis of the Car Allowance Rebate System ('Cash-for-Clunkers')." Executive Office of the President of the United States (2009).

Hortaçsu, Ali, Gregor Matvos, Chaehee Shin, Chad Syverson and Sriram Venkataraman. 2011. Is an Automaker's Road to Bankruptcy Paved with Customers' Beliefs?" *American Economic Review*, *101*(3), 93-97.

Interagency Working Group on Social Cost of Carbon, "Social Cost of Carbon for Regulatory Impact Analysis under Executive Order 12866," 2010.

Klier, Thomas, and Joshua Linn. 2010. The Price of Gasoline and New Vehicle Fuel Economy: Evidence from Monthly Sales Data. *American Economic Journal: Economic Policy* August: 134–153.

Knittel, Christopher, "The Implied Cost of Carbon Dioxide under the Cash-for-Clunkers Program," University of California-Davis Working Paper, 2009.

S. Li, C. Timmins, and R. von Haefen (2009). How Do Gasoline Prices Affect Fleet Fuel Economy? American Economic Journal: Economic Policy 1(2): 113-37.

Lu, S, "Vehicle Scrappage and Travel Mileage Schedules," National Highway Traffic Safety Administration Technical Report, January 2006.

Ludvigson, Sydney, "Consumer Confidence and Consumer Spending," *Journal of Economic Perspectives,* 18:2 (2004), 29-50.

McCubbin, Donald and Mark Delucchi, "The Health Costs of Motor-Vehicle Related Air Pollution," *Journal of Transportation Economics and Policy*, 33(1999), 253-286.

Metcalf, Gilbert, "Using Tax Expenditures to Achieve Energy Policy Goals," *American Economic Review Papers and Proceedings*, 98(2008), 90-94.

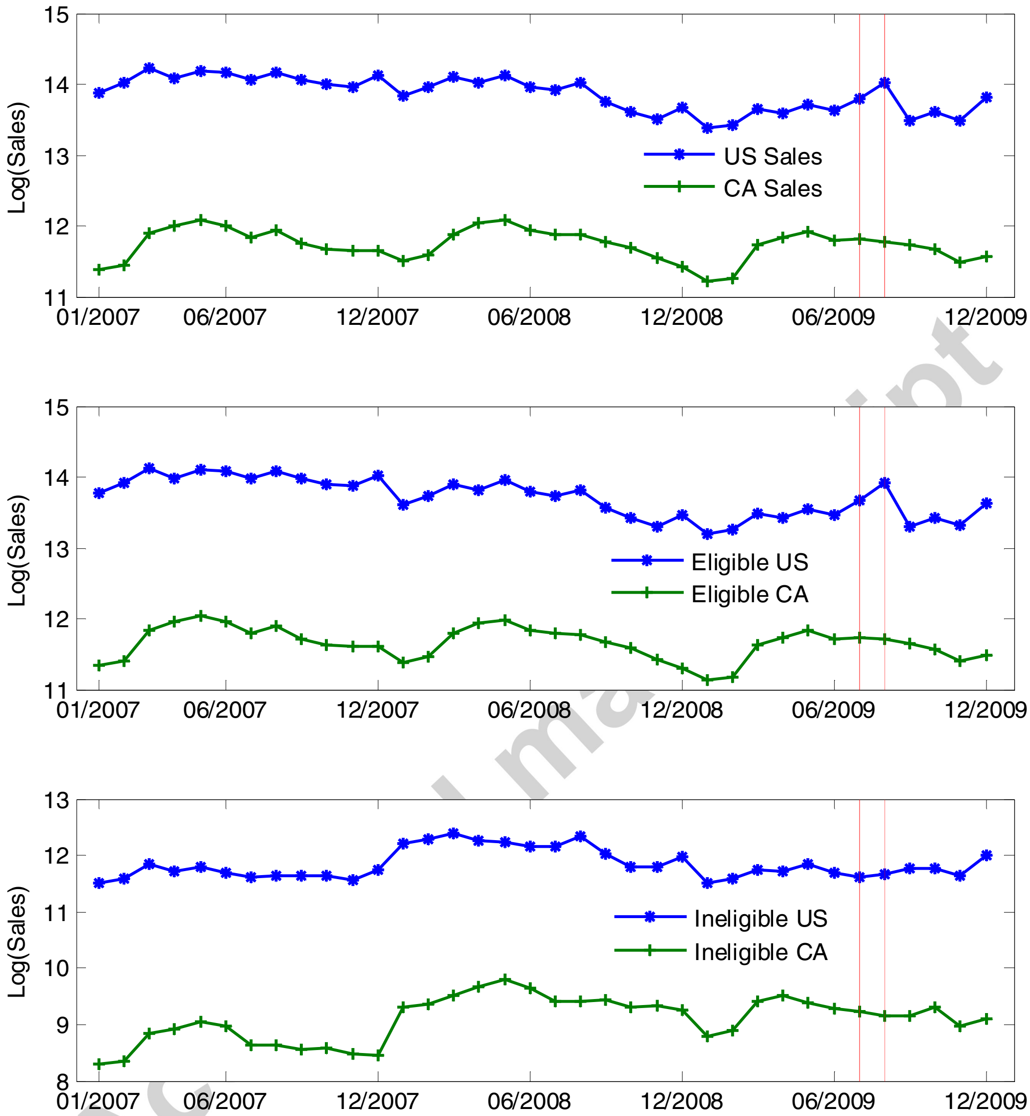
Mian, Atif and Amir Sufi, "The Effects of Fiscal Stimulus: Evidence from the 2009 'Cash-for-Clunkers' Program," (2010), NBER Working Paper.

Muller, Nick and Robert Mendelsohn, "Efficient Pollution Regulation: Getting the Prices Right," *American Economic Review*, 99 (2009), 1714-1739.

National Highway Traffic Safety Administration. "Consumer Assistance to Recycle and Save Act of 2009: Report to Congress." U.S, Department of Transportation, December 2009.

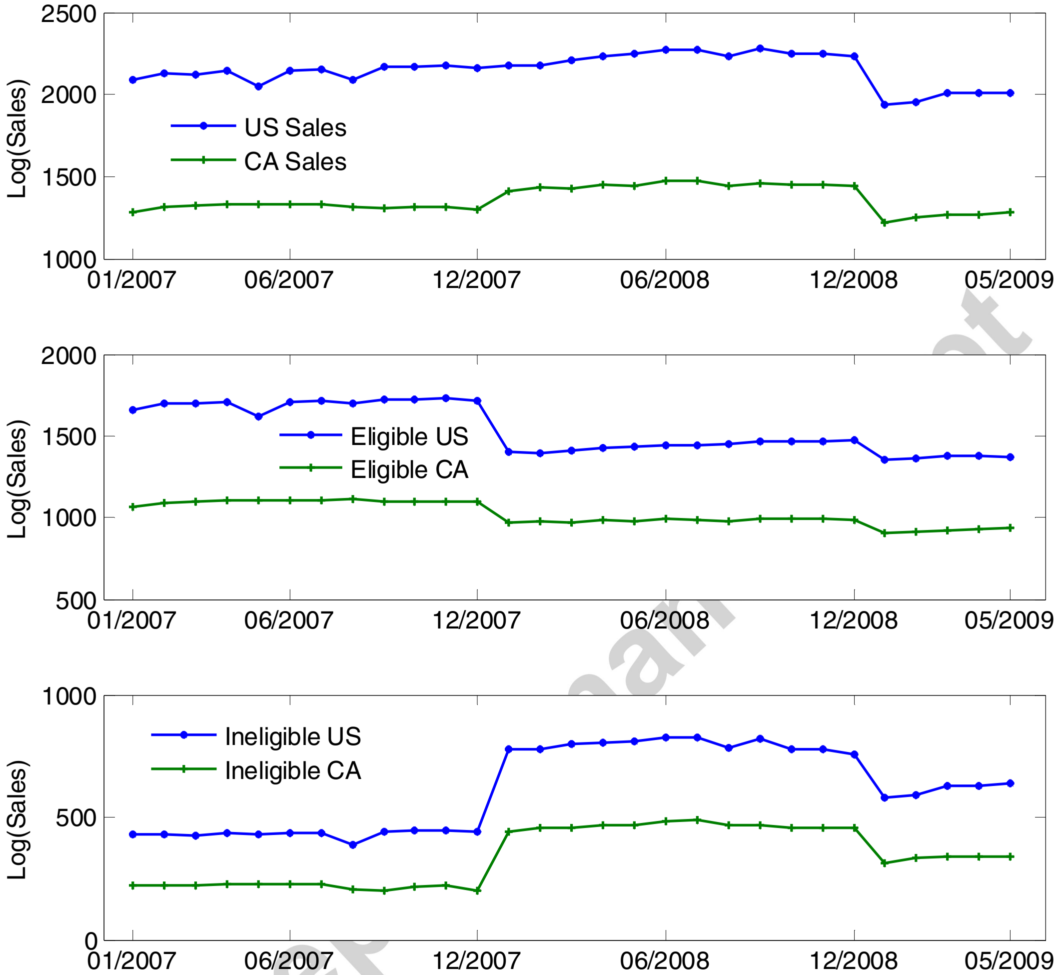
Small, Kenneth and Kurt van Dender, "Fuel Efficiency and Motor Vehicle Travel: The Declining Rebound Effect," *Energy Journal*, 28(2007), 25-51.

Figure 3: Monthly New Vehicle Sales in the United States and Canada from 2007 to 2009



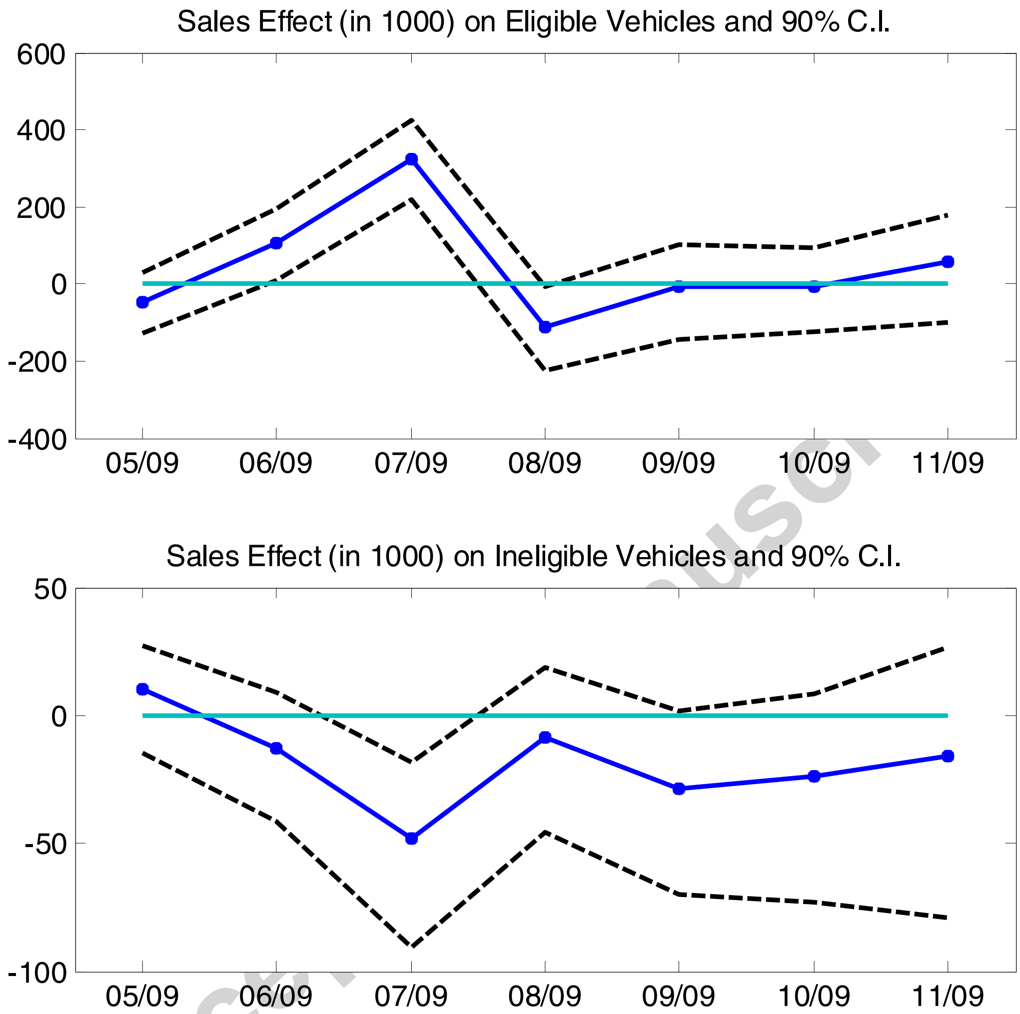
Note: The plots show total monthly sales in logarithm for all, eligible, and ineligible vehicles.

Figure 4: Pre-program Monthly New Vehicle Sales after Controlling for Observables



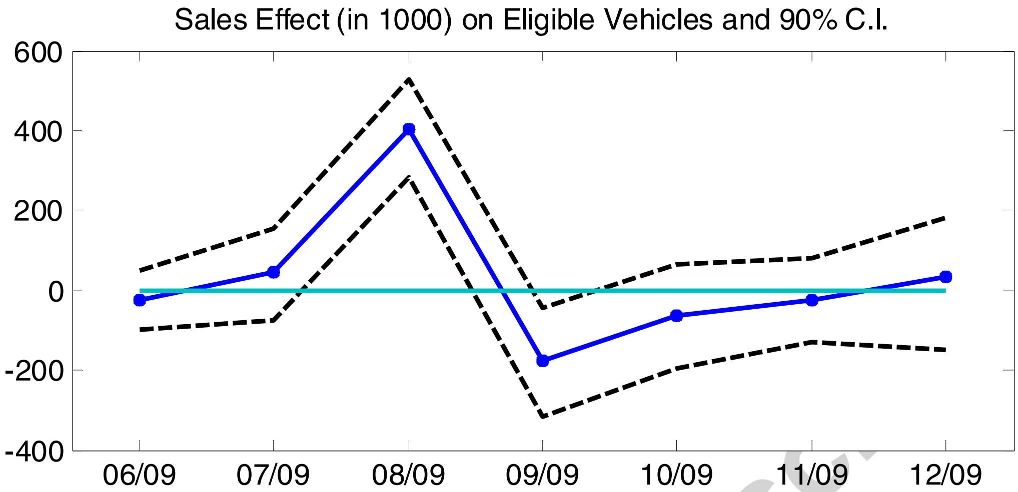
Note: the plots are aggregate monthly sales pre-program after removing the effects of explanatory variables using the same specification as equation (2). The control variables are country-specific seasonality by eligibility type, and year-month common trends by eligibility type, dollars per mile interacting with country dummy, and the gasoline price.

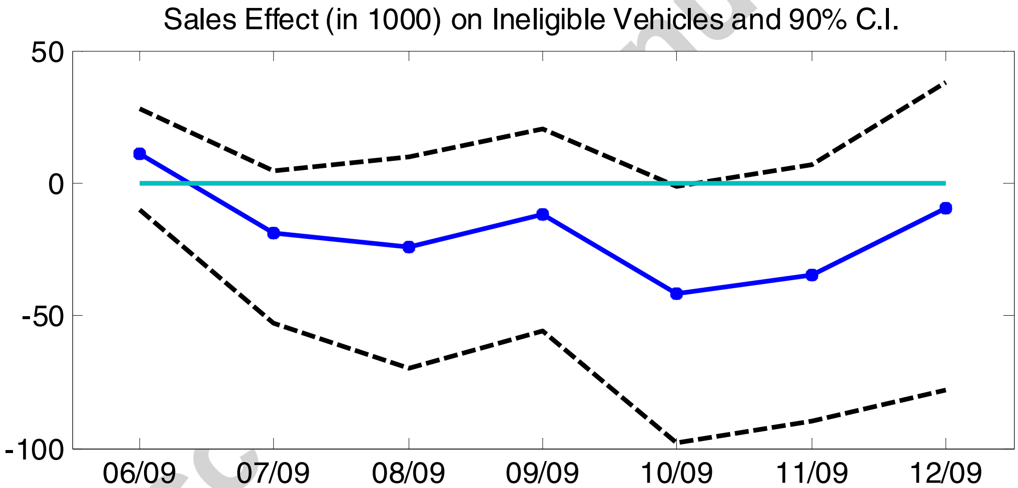
Figure 5: Sales Effect over Time Using the Full Sample



Note: The plots show the program effect on vehicle sales from June to December of 2009. The estimates are based on parameter estimates in Table 4 using the full sample. The 90% confidence intervals are estimated from block (i.e., country-year-nameplate) bootstrap with 500 repetitions.

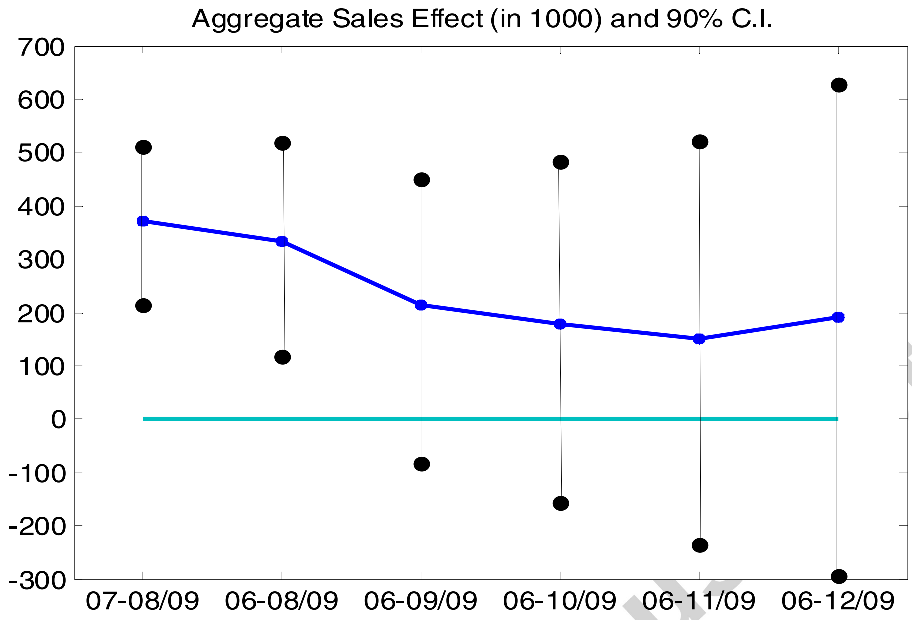
Figure 6: Sales Effect over Time Using the Short Pre-Program Sample

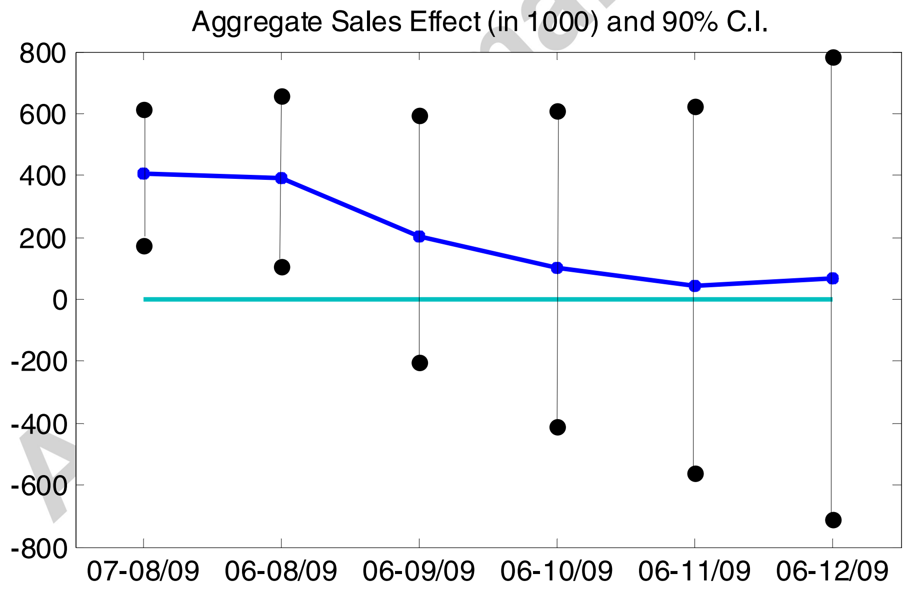




Note: The plots show the program effect on vehicle sales from June to December of 2009. The estimates are based on parameter estimates in Table 4 using the smaller sample. The 90% confidence intervals are estimated from block (i.e., country-year-nameplate) bootstrap with 500 repetitions.

Figure 7: Aggregate Sales Effect over Different Time Horizons





Note: The top figure is based on estimates from the full sample while the bottom figure is based on the short pre-program sample. The 90% confidence intervals are estimated from block (i.e., country-year-nameplate) bootstrap with 500 repetitions.

Table 1A: Rebate Eligibility Requirements

Trade-in Vehicle	• Is in drivable condition • Has been both continuously insured, consistent with state laws, and continuously registered to the same owner for at least one year immediately prior to trade-in under the CARS program • Was manufactured less than 25 years before the date of trade (i.e., before mid- to late- 1984) and, in the case of category 3 trucks, not later than model year 2001 • Has a <u>combined</u> MPG of 18 or less (this does not apply to category 3 trucks, i.e., very large pickup trucks and cargo vans)
New Vehicle (Purchased or Leased)	• Is new (i.e., legal title has not been transferred to anyone) • Has manufacturer's suggested retail price of $45,000 or less

Table 1B: Rebate Amounts

Incentive Amounts					
If the type of new vehicle you want is a…	The combined MPG* of the new vehicle must be…	The type of vehicle you trade-in must be a…	Amount of Incentive: If the difference in combined MPG between new vehicle and trade-in vehicle is…	Amount of Incentive: The incentive is…	
Passenger Automobile • All passenger cars	At least 22 MPG	Passenger car, category 1 or category 2 truck	4-9 MPG	3500	
			10 MPG or more		4500
Category 1 Truck† • All SUVs w/ GVWR <= 10,000 lbs • All pickups w/ GVWR < 8,500 lbs & wheelbase <= 115 inches • Passenger vans and cargo vans w/ GVWR < 8,500 lbs and wheelbase <= 124 inches	At least 18 MPG	Passenger car, category 1 or category 2 truck	2-5 MPG	3500	
			5 MPG or more		4500
Category 2 Truck† • Pickups w/ GVWR <= 8,500 lbs & wheelbase > 115 inches • Passenger vans and cargo vans w/ GVWR <= 8,500 lbs and wheelbase > 124 inches	At least 15 MPG	Category 2 truck	1 MPG	3,500	
			2 MPG or more		4,500
		Category 3 truck	NA‡	3,500	
Category 3 Truck† • Trucks w/ GVWR 8,500 – 10,000 lbs that are either pickup trucks with cargo beds 72" or longer or very large cargo vans	NA‡	Category 3 truck	NA‡ However, the new vehicle must be similar in size or smaller than the trade-in	3,500	

* MPG requirements are based on EPA's combined city/highway rating
† GVWR = Gross Vehicle Weight Rating
‡ Not applicable: Category 3 trucks do not have EPA MPG ratings
Source: www.cars.gov.

Table 2: Summary Statistics of Vehicle Data

	No. of Observations			Monthly Sales per Model		
	Eligible	Ineligible	All	Eligible	Ineligible	All
United States	6,394	2,742	9,136	5,254	1,878	4,241
Canada	5,476	2,202	7,678	774	157	597
	Average Vehicle Price			**Average MPG**		
	Eligible	Ineligible	All	Eligible	Ineligible	All
United States	24,780	43,678	30,452	23.28	18.01	21.70
Canada	24,071	42,920	29,477	22.62	17.96	21.28

Note: The table provides statistics for variables at the country-year-nameplate (i.e., model) level for years from 2007 to 2009.

Table 3: Trade-in and New Vehicles Participating in the Program

	Cars		Trucks		All	
	Mean	**S.D.**	**Mean**	**S.D.**	**Mean**	**S.D.**
Panel 1: Trade-in Vehicles						
MPG	17.55	1.01	15.50	1.75	15.81	1.81
Age	15.60	4.20	13.45	3.99	13.78	4.10
VMT	140,833	53,940	150,432	53,284	148,982	53,494
Observations	99,624		559,776		659,400	
Panel 2: New Vehicles						
MPG	27.96	5.21	20.73	3.17	25.00	5.73
Rebate ($)	4,224	451	4,200	462	4,214	456
Observations	388,809		270,591		659,400	

Note: The program included 678,359 transactions (www.cars.gov). In order to be consistent with our analysis of the new vehicle market, we delete 18,959 transactions, which include 2,278 category 3 new vehicles, 6,169 leases, and 10,512 observations with data errors (e.g., out-of-range MPG data). The total rebate amount for the remaining 659,400 transactions is about $2.78 billion, compared to a total program payment of $2.85 billion.

Table 4: Difference-in-Differences Regression Results

		Full Sample				Shorter Sample	
		Simple Model		Full Model		Full Model	
	Variables	Est. (1)	S.E. (2)	Est. (3)	S.E. (4)	Est. (5)	S.E. (6)
Eligible Veh. In US	June	-0.068	0.063	-0.107	0.083	-0.054	0.087
	June*|ΔGPM|			0.058	0.061	0.029	0.059
	July	0.113	0.078	-0.084	0.099	-0.148	0.116
	July*|ΔGPM|			0.295	0.085	0.270	0.074
	August	0.324	0.085	0.121	0.110	0.247	0.119
	Aug.*|ΔGPM|			0.304	0.108	0.276	0.078
	Sept.	-0.167	0.091	-0.095	0.120	-0.164	0.117
	Sept*|ΔGPM|			-0.106	0.126	-0.132	0.087
	Oct.	-0.009	0.102	0.060	0.133	0.004	0.121
	Oct.*|ΔGPM|			-0.098	0.127	-0.124	0.080
	Nov.	-0.010	0.103	0.016	0.118	0.010	0.108
	Nov.*|ΔGPM|			-0.034	0.092	-0.067	0.068
	Dec.	0.068	0.113	0.114	0.148	0.108	0.133
	Dec.*|ΔGPM|			-0.062	0.128	-0.093	0.091
Ineligible Veh. In US	June	0.077	0.107	0.119	0.126	0.106	0.150
	June*|ΔGPM|			-0.060	0.088	-0.022	0.087
	July	-0.098	0.115	-0.157	0.143	-0.225	0.174
	July*|ΔGPM|			0.090	0.118	0.121	0.109
	August	-0.318	0.125	-0.426	0.161	-0.288	0.223
	Aug.*|ΔGPM|			0.161	0.135	0.198	0.126
	Sept.	-0.004	0.130	-0.245	0.168	-0.284	0.207
	Sept*|ΔGPM|			0.359	0.134	0.392	0.104
	Oct.	-0.143	0.139	-0.392	0.185	-0.491	0.191
	Oct.*|ΔGPM|			0.371	0.146	0.405	0.117
	Nov.	-0.139	0.165	-0.365	0.217	-0.459	0.217
	Nov.*|ΔGPM|			0.341	0.176	0.381	0.124
	Dec.	-0.035	0.172	-0.300	0.224	-0.281	0.217
	Dec.*|ΔGPM|			0.400	0.184	0.438	0.126
R-squared		0.945		0.946		0.947	

Notes: The results in columns (1) and (2) are based on equation (1) while columns (3) to (6) are based on equation (2). The dependent variable is the logarithm of vehicle sales. The number of observations is 16,776 in the full sample and 13,976 in the short pre-program sample. The full set of control variables described in equation (1) is included in the regressions: model fixed effects, country-specific seasonality by eligibility type, year-month common trends by eligibility type, gasoline price, and dollars per mile by country. |ΔGPM| is the absolute difference between the GPM of the vehicle and the eligibility requirement: the farther away an eligible (or ineligible) vehicle's MPG is away from the requirement, the larger it is. The shorter sample is the one without the second half of 2008. The standard errors are computed from block (i.e., country-year-nameplate) bootstrap with 500 repetitions.

Table 5: Program Effects during June- December 2009 in the United States

		Full Sample				Shorter Sample	
		Simple Model		Full Model		Full Model	
	Observed	Effect	S.E.	Effect	S.E.	Effect	S.E.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pane 1: Effect on Vehicle Sales							
June	828,286	-41,160	49,046	-37,532	48,317	-12,973	53,940
July	970,490	80,673	61,308	94,822	58,963	24,764	89,549
August	1,231,137	264,288	72,410	277,159	69,864	380,082	89,745
September	719,795	-108,362	64,537	-119,409	66,106	-187,088	89,962
October	810,066	-25,494	73,980	-35,928	75,200	-103,525	88,244
November	719,140	-22,929	68,903	-29,058	69,749	-57,642	69,705
December	992,053	48,426	95,581	39,832	96,603	23,964	103,142
July-August	2,201,627	344,960	123,475	371,981	118,393	404,845	151,923
June-December	6,270,967	195,442	392,912	189,886	392,958	67,583	411,438
Panel 2: Effect on Vehicle MPG							
July-August	23.37	0.28	0.05	0.65	0.09	0.59	0.13
June-December	22.75	0.13	0.03	0.21	0.04	0.16	0.10
Panel 3: Effect on Vehicle gallons per 100 miles (GPM)							
July-August	4.47	-0.06	0.01	-0.11	0.02	-0.10	0.03
June-December	4.59	-0.03	0.01	-0.04	0.01	-0.03	0.02

Note: Program effects are calculated as the difference between the observed outcomes and the counterfactual outcomes without the program. The shorter sample is the one without the second half of 2008. The standard errors are computed from block (i.e., country-year-nameplate) bootstrap with 500 repetitions.

Table 6: Program Effects on Sales for the Industry and Automakers

		Full Sample				Shorter Sample	
		Simple Model		Full Model		Full Model	
	Observed	Effect	S.E.	Effect	S.E.	Effect	S.E.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Industry	2,201,627	344,960	123,599	371,981	118,512	404,845	152,075
GM	405,394	65,265	22,751	65,886	22,143	72,460	28,310
Ford	318,573	42,505	17,698	46,542	17,043	50,788	22,554
Chrysler	181,846	20,186	10,314	15,176	10,776	18,333	14,641
Toyota	402,317	72,572	23,247	95,045	21,389	98,638	25,877
Honda	276,003	51,781	15,970	62,493	14,898	65,467	18,073
Nissan	176,931	30,842	10,012	35,135	9,456	38,071	11,790
Panel 2: Effects during June to December 2009							
Industry	6,270,967	195,442	393,306	189,886	393,352	67,583	411,850
GM	1,189,728	36,813	74,661	33,300	74,742	11,167	78,601
Ford	963,950	20,355	59,195	13,948	60,103	-4,991	62,222
Chrysler	511,564	7,404	31,147	-5,820	33,372	-13,633	35,297
Toyota	1,130,300	48,424	75,346	64,776	78,279	33,626	76,743
Honda	720,035	35,410	46,843	40,773	46,778	25,277	47,955
Nissan	479,840	19,445	30,494	23,514	30,223	14,631	31,447

Note: Program effects are calculated as the difference between the observed outcomes and the counterfactual outcomes without the program. The shorter sample is the one without the second half of 2008. The standard errors are computed from block (i.e., country-year-nameplate) bootstrap with 500 repetitions.

Table 7: Cost-Effectiveness Analysis

	Total Reductions		Cost ($) w/ Co-benefit		Cost ($)w/o Co-benefit	
	Gasoline (mil gallons)	CO2 (mil tons)	Gasoline (per gallon)	CO2 (per ton)	Gasoline (per gallon)	CO2 (per ton)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Lifetime VMT (Full Sample)						
Case 1: No rebound effect	2,907.3	28.2	0.89	91.7	1.03	106.4
Case 2: Rebound elasticity = 0.1	2,836.2	27.5	0.91	94.0	1.06	109.0
Case 3: Rebound elasticity = 0.5	2,552.0	24.8	1.01	104.4	1.18	121.2
Panel 2: Adjusted VMT (Full Sample)						
Case 4: No rebound effect	1,077.4	10.5	2.40	247.4	2.78	287.1
Case 5: Rebound elasticity = 0.1	1,061.8	10.3	2.43	251.0	2.83	291.3
Case 6: Rebound elasticity = 0.2	995.4	9.7	2.60	267.8	3.01	310.7
Panel 3: Lifetime VMT (Shorter Sample)						
Case 7: No rebound effect	2,835.6	27.5	0.91	94.0	1.06	109.1
Case 8: Rebound elasticity = 0.1	2,774.7	26.9	0.93	96.1	1.08	111.5
Case 9: Rebound elasticity = 0.5	2,530.9	24.5	1.02	105.3	1.19	122.2
Panel 4: Adjusted VMT (Shorter Sample)						
Case 10: No rebound effect	1,024.7	9.9	2.52	260.1	2.93	301.8
Case 11: Rebound elasticity = 0.1	1,005.5	9.8	2.57	265.1	2.98	307.6
Case 12: Rebound elasticity = 0.2	924.5	9.0	2.80	288.3	3.24	334.5

Note: Panels 1 and 2 are based on the estimation results from the full sample while panels 3 and 4 are based on the shorter sample. Panels 1 and 3 compare the lifetime gasoline consumption of new vehicles sold June-December of 2008 with the lifetime gasoline consumption of (less fuel efficient) new vehicles that would have been sold June-December without the program plus gasoline consumption from the trade-in vehicles during their remaining lifetime. Panels 2 and 4 adjust the VMT of new vehicles under the counterfactual scenario so that the total VMT under the two scenarios are the same.