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Vehicle Miles (Not) Traveled: Why Fuel Economy Requirements Don't Increase Household Driving

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ABSTRACT

A major concern with addressing the negative externalities of gasoline consumption by regulating fuel economy, rather than increasing fuel taxes, is that households respond by driving more. This paper exploits a discrete threshold in the eligibility for Cash for Clunkers to show that fuel economy restrictions lead households to purchase vehicles that have lower cost-per-mile, but are also smaller and lower-performance. Whereas the former effect can increase driving, the latter effect can reduce it. Results indicate these households do not drive more, suggesting that behavioral responses do not necessarily undermine the effectiveness of fuel economy restrictions at reducing gasoline consumption.

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1 Introduction

Negative externalities from gasoline consumption are well-documented, ranging from the local effects of automobile pollution on health [Currie and Walker, 2011; Knittel, Miller, and Sanders, 2011] to the global impact of vehicle emissions on climate change [Interagency Working Group, 2013]. The current level of gasoline taxes in the United States is generally thought to be insufficient to correct for these externalities [McConnell, 2013], but the direct policy solution – increasing these Pigouvian taxes – remains politically unfeasible. As a result, U.S. transportation policy addresses fuel consumption externalities primarily by regulating the fuel efficiency of new vehicles via Corporate Average Fuel Economy (CAFE) requirements.¹

Although CAFE standards remained largely constant for nearly two decades, the federal government has set ambitious new targets for the fuel economy of the future fleet. Regulators project that these new standards will increase the average fleet-wide fuel economy of new light-duty vehicles to 46.2 miles per gallon by 2025, compared to 25.9 miles per gallon in 2010 [NHTSA, 2012]. In the absence of behavioral changes, these projections amount to a substantial reduction in gasoline consumption.

However, policy analysts argue that increasing the fuel economy of the vehicle fleet will not necessarily lead to a proportionate reduction in fuel consumption (e.g. National Research Council [2013]). The intuition underlying this concern is straightforward: because vehicles with higher fuel economy travel farther per gallon of fuel, the cost of driving each mile is comparatively lower in fuel-efficient vehicles, and this lower cost-per-mile may result in an increase in the quantity of miles traveled. This has been called the “rebound effect”.

Despite the simplicity of this argument at a conceptual level, researchers have struggled to quantify the extent of the rebound effect that arises from an increase in fuel efficiency [Gillingham, Kotchen, Rapson, and Wagner, 2013a]. The fundamental challenge has been a lack of exogenous variation in fuel economy. Vehicle owners select the vehicles they purchase in part based on their expected driving behavior, so disentangling the causal impact of fuel economy on driving is empirically problematic. To circumvent these endogeneity issues, most research on the rebound effect exploits variation in fuel *prices* – rather than fuel economy – to identify the relationship between vehicle miles traveled (VMT) and the price-per-mile of driving. As we argue in the following section, there are several reasons why the impact of fuel prices on consumption may differ from the rebound effect for *fuel economy*, at least in the short run.

¹See Knittel [2013] for a history of the (lack of) political support for increasing the gasoline tax dating back to the Nixon administration. Extensive research examines the inefficiencies associated with using fuel economy standards rather than a gasoline tax (e.g. Portney, Parry, Gruenspect, and Harrington, 2003; Fischer, Harrington, and Parry, 2007; Anderson, Parry, Sallee, and Fischer, 2011; Jacobsen, 2013).

The primary difference between rebound effects caused by fuel prices and fuel economy is that in contrast to fuel prices, fuel economy is highly – and typically negatively – correlated with other desirable vehicle attributes, such as vehicle performance (e.g., horsepower) and safety (e.g., vehicle size). Thus, while both gasoline prices and fuel economy alter the *cost* per mile of driving, fuel economy restrictions may also affect the *benefit* per mile traveled. More formally, a change in fuel prices induces movement along the demand curve for VMT because the price per mile varies but vehicle characteristics are held constant. However, a change in fuel economy induces both a shifting of *and* a movement along the demand curve. For example, if a household purchases a more fuel efficient but smaller and lower-performing vehicle, then the change in vehicle characteristics shifts VMT demand *in* and the decrease in the price per mile moves the household *down* the demand curve. Therefore, the sign of the effect of fuel economy standards on VMT is theoretically ambiguous. As a result of this logic, we argue that variation in fuel prices is better suited to predicting the efficacy of changing gasoline taxes, but that exogenous variation in fuel economy, coupled with correlated vehicle attributes, is necessary in order to better understand the impact of CAFE standards as they are implemented in the United States.

With this objective, we use administrative household-level data from Texas to study a unique natural experiment in which some households were quasi-randomly induced to buy more fuel efficient vehicles. We do so by exploiting a discontinuity in the eligibility requirements for the 2009 U.S. “Cash for Clunkers” (CfC) program, which incentivized eligible households to purchase more fuel-efficient vehicles. Specifically, we use a regression discontinuity design to assess the household driving response to the exogenous variation in new vehicle fuel economy induced by the program’s requirement that a “clunker” have an EPA rating of no more than 18 miles per gallon (MPG). Households that owned clunkers with a fuel economy of 18 MPG or less were eligible for the subsidy, while households owning clunkers with an MPG of 19 or more were ineligible. Our empirical strategy is to compare the fuel economy of vehicle purchases and subsequent vehicle miles traveled of barely eligible households to those households who were barely ineligible. The key identifying assumption is that all determinants of fuel economy and miles driven are smooth through the eligibility criteria, with the exception program eligibility.

Importantly, although the program ran for less than two months, we use *all* households who bought new vehicles within one year of the start of the program, which was the maximum time that any household shifted purchases forward [Hoekstra, Puller, and West, 2015]. Thus, by construction this time frame was such that there was no effect of the program on the likelihood of purchase; all households in our sample were going to buy a vehicle sometime in the next year, but some were incentivized to buy somewhat sooner and purchase different

vehicles within that time frame. As we show in Section 4.4, households who purchased during this one-year time window have very similar demographic and previous purchasing and driving characteristics across the eligibility cutoff.² To our knowledge, this is the first study to use quasi-experimental variation in fuel economy to estimate how household driving behavior and fuel consumption respond to policy-induced improvements in fuel economy. We find this approach to be considerably more compelling than one based on panel data, where one might worry that a change in household fuel economy over time is caused by changes in unobserved income or commute distance, which themselves would affect vehicle miles traveled.

We find a meaningful discontinuity in the fuel economy of new vehicles purchased by CfC-eligible households relative to ineligible households. However, we also find that the more fuel efficient vehicles purchased by the eligible households were cheaper, smaller, and lower-performing. This suggests that given current technological limitations and the cost of fuel-saving technologies such as hybrids, households respond to fuel economy restrictions by purchasing vehicles that are more fuel efficient, but are less desirable along other dimensions.

Results indicate that households induced to purchase more fuel efficient (but cheaper, smaller, and lower-performing vehicles) do not drive any additional miles after purchase. Thus, we find no evidence of a rebound effect in response to improved fuel economy. We argue that this is consistent with a shifting in of the VMT demand curve due to changing vehicle characteristics, coupled with a movement down the demand curve for VMT because improved fuel economy reduces the price-per-mile of driving.

This paper makes three primary contributions to the literature. First, we believe this to be the first paper to exploit credibly exogenous variation in household fuel economy to identify the effect on driving behavior. As a result, we are able to obtain estimates that are causal under reasonable assumptions, without the need to impose stronger assumptions required to model vehicle purchase and driving decisions.

Second, our finding of no rebound effect from increased fuel economy is directly relevant for policies such as CAFE, given that auto manufacturers are likely to “downsize” the new vehicle fleet by selling smaller cars than they otherwise would, in order to comply with the new set of CAFE standards (Knittel [2011]). The NHTSA assumes a 10% rebound effect, based in part on the existing literature, when calibrating the CAFE standards ([NHTSA, 2012]). However, as we discuss below, much of the existing literature on the rebound effect

²Specifically, we first use the 2009 National Household Travel Survey to show that Texas households in general look very similar across the CfC eligibility threshold. Second, we demonstrate that households who purchased new vehicles during the 12-month period we study owned similar fleets and exhibited similar driving patterns *prior to* Cash for Clunkers. Finally, we perform falsification tests for new vehicle attributes and subsequent driving outcomes for the households that purchased new vehicles during 2008 – the year prior to CfC.

does not incorporate the effect of downsizing on driving. Our results suggest that if future fuel economy standards require households to downsize vehicles, then estimates of rebound that do not account for changes in vehicle characteristics are likely to be overstated.

Finally, these results have implications for evaluating the welfare comparisons that are frequently made between price-based policies such as a gasoline tax and quantity-based regulations such as CAFE. Quantity-based regulations such as fuel economy standards have been criticized as inefficient on the intensive margin for distorting vehicle utilization relative to the first-best policy of imposing a Pigouvian tax to fully internalize the externalities of driving. This paper makes an important point: extensive margin policies can have countervailing effects on intensive marginal utilization decisions. One effect of increasing fuel economy is captured by a price elasticity of driving – altering the fuel efficiency of the fleet reduces the price-per-mile of driving. A second effect is a vehicle-attribute elasticity of driving – shifting households to fuel efficient cars with less desirable characteristics can reduce the utility-per-mile of driving and thus the amount of driving. Both of these effects must be captured by a complete welfare analysis to compare a particular policy to first-best.³

This paper is organized as follows. Section 2 reviews the literature on the rebound effect and bolsters our argument regarding the distinction between variation in fuel prices versus fuel economy. Section 3 provides an overview of the U.S. Cash for Clunkers program, describes the data included in our study, and details our empirical strategy. Our findings are presented in Section 4, along with the identification checks and falsification exercises. We conclude in Section 5.

2 The Energy Consumption Rebound Effect

Personal vehicles are a major target of U.S. energy and environmental policy. Personal light-duty vehicles generate 16% of U.S. greenhouse gas emissions and consume nearly 10% of world petroleum liquids.⁴ It is widely believed that the externalities from gasoline consumption are not internalized into gasoline prices (McConnell [2013]). Because standard Pigouvian solutions such as a gasoline tax are politically impractical, policy often targets energy consumption with standards on the energy efficiency of vehicles. The primary policy in the U.S. since 1978 has been the Corporate Average Fuel Economy (CAFE) standards that set minimum fuel economy requirements on new vehicles. However, many analysts and policy-

³Our empirical approach places strong emphasis on identifying causal impacts of fuel economy by exploiting quasi-random variation in fuel economy, which to our knowledge is new to the literature. A limitation of this approach is that we are not in a position to estimate the relative magnitudes of these two elasticities or to calculate welfare measures. However, our analysis does suggest that one effect can mitigate the other.

⁴See Environmental Protection Agency [2015] and Energy Information Administration [2014].

makers have noted that increasing the fuel economy of the vehicle fleet will not necessarily lead to a proportionate reduction in fuel consumption. An increase in fuel economy reduces the price-per-mile of driving, which is the price per gallon of fuel divided by the miles per gallon fuel economy. If households respond to the lower marginal cost of driving by increasing vehicle miles traveled, then the effectiveness of this policy in reducing fuel consumption is undermined. This problem, originally called the “Jevons Paradox” and later articulated by Khazzoum [1980], is a more general shortcoming of energy efficiency standards. NHTSA assumes a rebound effect of 10% in formulating CAFE standards and academic literature reviews cite rebound effects in the range from 5-30%, for example see Gillingham et al. [2013b], Hymel and Small [2013], and Greening et al. [2000].

The rebound effect that we study is more precisely called the “direct rebound” effect.⁵ It measures the effect of improving the energy-efficiency of a durable good on the total energy consumed by that good.⁶ To see this more formally, consider a model of a household’s choice of VMT and the resulting consumption of gasoline. Take a household with a vehicle fleet characterized by its fuel economy MPG_i and other characteristics of the vehicle X_i .⁷ VMT is an input to the production of household transportation services, hence it is a derived demand, given by: $VMT_i = f(\frac{\$}{mile_i}, X_i, W_i)$ where $\frac{\$}{mile_i}$ is the price per mile of driving, X_i are vehicle characteristics, and W_i are demographic characteristics of household i . The price-per-mile of driving is the price per gallon of gasoline divided by the fuel economy in miles per gallon, so $\frac{\$}{mile_i} = \frac{p_{gas}}{MPG_i}$. Importantly, we allow for there to be a technological relationship between fuel economy and other vehicle characteristics, $X_i(MPG_i)$.

Given this setup, we can find how the total amount of gasoline consumption changes when there is a (exogenous) increase in fuel economy. A household’s total gasoline consumption is $gallons_i \equiv \frac{VMT_i(\frac{p_{gas}}{MPG_i}, X_i(MPG_i), W_i)}{MPG_i}$. Taking logs and differentiating with respect to MPG yields the elasticity of gasoline consumption with respect to fuel economy ($E_{gallon-MPG}$). This elasticity tells us the percentage reduction in gasoline consumption that one achieves with a given percent increase in fuel economy:

⁵The literature also has studied the “indirect rebound” effect which incorporates the effect of changing the efficiency of one durable good on the energy consumed by other durable goods that the household owns. See Borenstein [2015] for a detailed discussion of the different components of the total rebound effect. In this paper, we do not explore whether households receiving the subsidy and purchasing less expensive vehicles, increased energy consumption via consumption outside personal vehicle transportation.

⁶This direct rebound effect can include both a substitution and an income effect; we do not decompose the two effects.

⁷For simplicity of exposition, assume that households own only one vehicle, but our empirical analysis will allow for multi-vehicle fleets. In addition, assume for exposition that vehicle characteristics X_i are a scalar, though more generally X_i could represent a vector of characteristics.

$$E_{gallon-MPG} = -1 + \underbrace{-E_{VMT-\frac{\$}{mile}}}_{\text{standard rebound}} + \underbrace{\frac{1}{gal_i} \cdot \frac{\partial VMT_i}{\partial X_i} \cdot \frac{\partial X_i}{\partial MPG_i}}_{\text{attribute-based adjustment}} \quad (1)$$

If the two terms in braces are zero, then an increase in fuel economy leads to a one-for-one proportionate decrease in fuel consumption – there is no rebound effect. The two terms in braces capture different behavioral adjustments that can create a response that is not one-for-one. The first term captures the amount that driving increases when the price-per-mile falls *but vehicle characteristics X_i remain constant*. This term – which is positive – has been the focus of much of the literature that estimates rebound. If this were the only behavioral adjustment, then an elasticity of VMT with respect to the price-per-mile of -0.10 would imply that a 10% increase in fuel economy would lead to only a 9% decrease in gasoline consumption.

However, a second behavioral adjustment can occur, as captured by the second term in braces. The second term captures complementarities between vehicle attributes and energy consumption. Specifically, it incorporates how changes in vehicle characteristics affect VMT, conditional on the price-per-mile of driving. There are a variety of channels through which specific vehicle characteristics can be complementary to driving. First, larger vehicles are more spacious and can make driving a more comfortable experience. Second, passengers in heavier vehicles experience lower fatality rates in the event of an accident ([Anderson and Auffhammer \[2014\]](#)). Finally, consumers value the improved acceleration that comes from vehicles with higher horsepower-per-pound, and generally horsepower-per-pound is lower in more fuel efficient vehicles. As we show below, fuel economy is negatively correlated with a number of vehicle characteristics that are complementary to driving.

Visually, this decomposition of the fuel consumption response to energy efficiency improvements corresponds to both a movement along and shifting of the derived demand for gasoline. Figure 1 provides an example. Consider a vehicle that is both more energy efficient but also provides lower ‘performance’ (e.g. horsepower per pound or size). The effect of the efficiency improvement $MPG'_i > MPG_i$ is to reduce the price-per-mile of driving, which shifts households down the derived demand function (holding characteristics constant). But the lower performance characteristics $X'_i \prec X_i$ causes a shift ‘in’ of the derived demand. Depending on the size of the two effects, the net effect on fuel consumption is ambiguous.

In many formulations of the rebound effect that are used for empirical analysis, it is assumed that the energy efficiency improvement does not change any of the other attributes of the service delivered by the durable good. This implicitly assumes that this second term

– the attribute-based adjustment – is zero. In some settings that have been studied this assumption may be valid, as in the case of water heaters where a more energy efficient model has more upfront cost to improve efficiency but still delivers the same volume and temperature of hot water (Allcott and Sweeney [2015]).

However, this “attribute-based adjustment” is likely to be negative in the case of vehicles, which would mitigate the size of the standard rebound effect. The more fuel efficient cars offered by manufacturers tend to have different, arguably less desirable, characteristics. As we show in Section 4.2, more fuel efficient vehicles are smaller, have less horsepower, and generally are less valuable as proxied by sales price. These tradeoffs are driven by technology – Knittel [2011] documents with historical data that improvements in fuel economy requires sacrificing vehicle characteristics such as horsepower, size, and weight. Thus for our setting, it is quite possible that this term is negative because an improvement in fuel economy reduces safety/comfort/size characteristics of vehicles, and that reduces the derived demand for VMT. Importantly, this second term works in the opposite direction of the standard rebound effect and implies that gasoline consumption reductions will be closer to proportional to energy efficiency improvements than one would infer from the standard rebound effect.

We should note that it is not the case that all higher fuel economy cars are smaller vehicles with less desirable characteristics. For example, the Tesla Model S (with the 2015 sticker price \$69,900) is a high performance vehicle, so purchasing a Tesla could both move down and shift *out* the derived demand for VMT. However, improving the desirability of a vehicle by increasing fuel economy is more the exception than the rule. Among the models currently offered, there is a negative correlation between fuel economy and various metrics of quality, as we document in Section 4.2. And, importantly, we show that when provided subsidies to purchase more fuel efficient vehicles during the Cash for Clunkers program, most households chose to downsize. Thus, while we cannot rule out technological progress to produce the contrary, it appears very likely that future fuel economy standards will cause households to move down and shift *in* the derived demand for VMT.

If gasoline taxes were the relevant policy instrument, then the standard rebound effect is most relevant. This effect captures the impact of raising the price of gasoline via a tax, while keeping drivers in cars with the same characteristics. This effect is likely to capture the impact of a gasoline tax, at least in the short-run before households adjust by purchasing different vehicles.

However, as we discuss above, fuel economy standards are likely to be the primary policy tool for reducing gasoline consumption. These policies are likely to change the characteristics of households’ vehicle fleets. Under Cash for Clunkers, households chose to achieve the fuel economy target by choosing smaller vehicles. In the longer run with CAFE standards,

compliance is likely to involve downsizing as well. Manufacturers are likely to comply with fuel economy standards by selling vehicles that have less powerful engines, are less spacious, and are lighter.⁸ Consequently, an understanding of the effect of fuel economy standards on gasoline consumption needs to account for both the standard rebound effect and attribute-based adjustments.

The existing empirical literature has focused on estimating the standard rebound effect.⁹ Much of this literature exploits variation in the price of gasoline, which generates variation in the price-per-mile of driving holding vehicle characteristics constant. (In part, the rationale for exploiting changes in gasoline prices is that it provides quasi-random variation in the price-per-mile of driving, while sources of credibly exogenous variation in fuel economy are difficult to find.) Thus, the existing empirical literature on rebound, while speaking to the effects of gasoline taxes, is not well-positioned to assess the impact of fuel economy policies on driving behavior and fuel consumption.

In this paper, we estimate the net effect of both the standard rebound effect and attribute-based adjustments in the years immediately after an exogenous increase in fuel economy. We estimate how households change their driving behavior in response to vehicles that are both more fuel efficient *and* smaller and less powerful, as dictated by the technological tradeoffs of vehicle manufacturing. This is a different form of “rebound” that addresses a different policy question than the rebound effect estimated in much of the existing literature. [Gillingham et al. \[2013b\]](#) refer to this form of rebound as a “policy-induced improvement” and argue that the size of this effect is more relevant for understanding the effects of energy efficiency policy such as CAFE.¹⁰

It is important to note that the size of the driving response that we estimate should not be interpreted as estimating the welfare implications of energy efficiency improvements. Even if households were to respond by driving more miles, a full welfare calculation would need to account for the utility of the additional driving. Ultimately, the welfare implications depend upon whether the household response to increased energy efficiency mitigates distortions from first-best levels of driving, which is beyond the scope of this paper. This paper documents

⁸See [Knittel \[2011\]](#) and [Klier and Linn \[2012\]](#) for an analysis of the technological tradeoffs of fuel economy standards.

⁹Estimates of rebound that receive considerable policy attention are from recent papers by [Small and van Dender \[2007\]](#) and [Hymel and Small \[2013\]](#). These papers use a representative consumer model that is aggregated to match state-level panel data and simultaneously model the choice of vehicles, vehicle miles traveled, and fuel economy. Surveys of research on the rebound effect include [Gillingham, Rapson, and Wagner \[2013b\]](#), [Austin \[2008\]](#) and [Greening, Greene, and Difiglio \[2000\]](#). In addition, a rich literature has modeled the choice and utilization of vehicles in the process of addressing a host of other policy questions; for example see [Mannering and Winston \[1985\]](#), [Goldberg \[1998\]](#), [West \[2004\]](#), [Fullerton and Gan \[2005\]](#), [Bento, Goulder, Jacobsen, and von Haefen \[2009\]](#), [Gillingham \[2012\]](#), and [Allcott and Wozny \[2014\]](#).

¹⁰See [Gillingham et al. \[2013b\]](#) for a thorough discussion of the definitions, estimation, and caveats of interpreting rebound effects.

how a factor not receiving attention in the literature – the vehicle-attribute elasticity of driving – can counteract any price-per-mile elasticity of driving. This new driving elasticity should be incorporated into both welfare analyses and to policy design that targets gasoline consumption with fuel economy standards.

3 Background and Empirical Strategy

3.1 The Cash for Clunkers Program

We exploit the Cash for Clunkers program as a quasi-random source of variation in the fuel economy of a household’s vehicle fleet. The program, formally known as the Consumer Assistance to Recycle and Save (CARS) Program, created incentives for households to replace used, fuel inefficient vehicles with new, fuel efficient vehicles. The program lasted for eight weeks during the summer of 2009 and offered households a rebate of \$3,500 or \$4,500 towards the purchase of the new fuel efficient car when they scrapped their “clunker.” A requirement of the program was that the clunker had to be taken off the road and scrapped; thus the rebate could be viewed as the trade-in value of the old car from the perspective of the household. Due to the scrappage requirement, the program attracted relatively older and low value vehicles. The average age of scrapped clunkers was 13.8 years.

The CARS Act was signed into law on June 24, 2009 and transactions first became eligible for rebates on July 1, 2009. Initial take-up of the program was substantial, and the \$1 billion that was allocated under the law quickly ran out. Congress allocated an additional \$2 billion on August 7, and those funds quickly were exhausted as well. The program ended on August 24 with over 677,000 vehicles purchased, 44,000 of which were in Texas.

The criteria for eligibility provide us with a cutoff for our regression discontinuity research design. The clunker must have had a combined EPA fuel economy rating of 18 MPG or less.¹¹ The vehicle purchased must have been a new vehicle; used vehicles did not qualify for the rebate. If the new vehicle was a passenger vehicle, it must have a combined fuel economy of at least 22 MPG. In the case of passenger vehicles, if the difference in fuel economy between the new passenger car and clunker was between 4 and 9 MPG, the rebate was \$3500, and if the difference was 10 MPG or more, the rebate was \$4500. If the new vehicle was a Category 1 Truck (e.g. SUV or small to medium pickup truck), a 2-5 MPG difference between the new truck and clunker generated a \$3500 rebate while an improvement of 5 or more MPG generated a \$4500 rebate.¹² [Busse, Knittel, Silva-Risso, and Zettelmeyer \[2012a\]](#) find that

¹¹There were additional requirements that the clunker be in drivable condition, no more than 25 years old, and continuously insured and registered in the same owner’s name for one year prior to the transaction.

¹²Separate criteria applied to Category 2 (large pickups or large vans) and Category 3 trucks (work trucks),

dealerships passed on nearly 100% of the rebates to customers.

These criteria create a discontinuous eligibility threshold – households with clunkers that had fuel economy of 18 MPG or less were eligible for CfC rebates whereas households with 19 or more MPG clunkers were not eligible. Below, we describe how we use our data to classify each household’s eligibility status.

CfC transactions resulted in an increase in the fuel economy of the vehicle fleet for those households that purchased under the program. The average fuel economy of the scrapped clunker was 15.8 MPG while the average fuel economy of new cars purchased under the program was 24.9 MPG.¹³ We should note that we do not evaluate the CfC program directly; rather we use the program design as a source of quasi-random variation in fuel economy. A separate literature has evaluated how well CfC achieved program objectives (for example, see Knittel [2009], Copeland and Kahn [2013], Busse et al. [2012a], Mian and Sufi [2012], Li, Linn, and Spiller [2013], and [Hoekstra, Puller, and West, 2015]).

3.2 Empirical Strategy

We use a regression discontinuity design to estimate the impact on vehicles miles traveled of an exogenous shift of households to more fuel efficient vehicles. We compare differences in the behavior of households whose “clunker” was barely eligible for the CfC subsidy to households whose clunker was barely ineligible. Intuitively, households that are barely eligible and barely ineligible are very similar in their preferences and driving characteristics except that the program induced barely eligible households to purchase more fuel efficient, and as it turns out “downsized”, vehicles. As we document below, the barely eligible and barely ineligible households are very similar in a number of characteristics, which supports our identifying assumption. Importantly, we focus on new car buyers, rather than all car owners. We do this because we otherwise cannot disentangle the effect of driving a more fuel efficient car from the effect of driving a *new* car.¹⁴

Our empirical strategy has two steps, both of which use household-level data on vehicle ownership and utilization as we describe in Section 3.3. First, we identify the set of households who, over some time period, would have purchased a new vehicle independent of the Cash for Clunkers program. Our rationale is the following: the program may have induced some

but we do not discuss those here because there were so few of these vehicles. For a complete set of eligibility criteria, see the NHTSA rules in the Federal Register available at: <http://www.nhtsa.gov/CARS-archive/official-information/day-one.pdf>

¹³C.A.R.S. Program Statistics, 2009, are available from <http://www.nhtsa.gov/Laws+&+Regulations/CARS+Program+Official+Information>.

¹⁴In addition, the number of new cars purchased under the Cash for Clunkers program is small relative to the total stock of vehicles in Texas, making the increase in fuel efficiency across all households at the eligibility cutoff statistically and economically undetectable.

barely eligible households to accelerate their purchases to the two-month program period in order to take advantage of the subsidy. In contrast, the program did not have such an effect on the barely ineligible households. As a result, if we were to study only the households who purchased during the program, one might be concerned that the set of barely eligible purchasers is not similar to the set of barely ineligible purchasers.

We overcome this problem by first estimating the “pull forward window”. We find the period of time beginning with the first two months of the program during which a barely eligible and barely ineligible household were equally likely to purchase a new vehicle. Because the probability of purchase over that time period was similar for barely eligible and ineligible households – by construction all were going to buy new vehicles during the window – there is little reason to expect any pre-existing differences in the composition or preferences of those barely eligible and ineligible buyers. We then focus our subsequent empirical analysis on the households that purchased during this “pull forward window”.

The second step is to take the set of households purchasing during this pull forward time window and compare the purchasing and subsequent driving behavior of barely eligible and barely ineligible households. The barely eligible serve as our ‘intent-to-treat’ group and the barely ineligible serve as our ‘control’ group. Specifically, we measure the extent to which the program induced households to purchase vehicles that are more fuel efficient, but also smaller and lower-performance. And then we test whether the households induced to purchase these different types of vehicles subsequently drove more miles in the year after purchase.

More formally, we compare households whose clunker was barely above the CfC eligibility cutoff of eighteen miles per gallon to those who barely qualified. We estimate the reduced-form discontinuities at the eligibility threshold using the following equation:

$$\begin{aligned} \text{Outcome}_i = & \beta_0 + \beta_1 * f(\text{distance-to-cutoff}_i) * \text{eligible}_i + \\ & \beta_2 * f(\text{distance-to-cutoff}_i) * (1 - \text{eligible}_i) + \beta_3 * \text{eligible}_i + \epsilon_i \end{aligned} \quad (2)$$

eligible_i is an indicator equal to one if the household is classified as being eligible for the program (i.e., the most trade-in-likely vehicle had an MPG rating of eighteen or less). We describe how our data identify a household’s eligibility status in Section 3.3. We allow for separate relationships between the running variable and the outcome on each side of the eligibility threshold. We estimate Equation (2) with least squares and standard errors are clustered at the level of the running variable [Lee and Card, 2008]. The coefficient of interest is β_3 , which measures the jump in the outcome when going from barely-ineligible to barely-eligible for the Cash for Clunkers program.

We use this specification to estimate both the “pull-forward” window and the effect of the program on the cars purchased and miles driven.

3.2.1 “Pull-Forward” Window

In order to estimate the “pull-forward” window, we follow the approach in [Hoekstra, Puller, and West, 2015]. We use a sample of *all* households in Texas. We estimate the number of months after the beginning of the two month program for which the probability of purchasing a new vehicle is equalized across the eligibility threshold. We begin by estimating the probability that a barely eligible and barely ineligible household purchased during the program in July-August 2009. (Not surprisingly, the barely eligibles were more likely to purchase a new vehicle during the two program months.) Then we expand the time window sequentially to include more months (i.e. July-September, July-October, July-November, ...) and estimate when the barely ineligible households “catch up”. More formally, for each time window, we estimate Equation (2) with household-level data where the dependent variable is an indicator of whether the household purchased a new vehicle during the time window. Our “pull-forward” window is defined as the shortest period beginning in July 2009 for which the probability of purchasing a new vehicle is equalized between the barely eligible and barely ineligible.

Once we define this “pull-forward” window, the households that purchase during this window serve as the households that we include in our primary analysis. For these households, because the purchase probability is equalized, it is reasonable to assume that the Cash for Clunkers program did not affect *whether* the household purchased a new vehicle but only the *timing* and *type* of purchase within this window. Thus, there is little reason to expect differences in the underlying vehicle preferences and driving behavior of the new-car-buying households on either side of the cutoff. We provide empirical support for this assumption in Section 4.4.

The “pull-forward” window that we estimate in Section 4.1 is 12 months. This short pull-forward period is very similar to findings in multiple other studies including Mian and Sufi [2012], Li et al. [2013], Copeland and Kahn [2013], and [Hoekstra, Puller, and West, 2015].¹⁵

¹⁵We should note that in this paper we use a slightly longer pull-forward window than our other paper ([Hoekstra, Puller, and West, 2015]). We do so to be conservative in our estimates and ensure smoothness of unobservables across the discontinuity. By extending our window, at worst we add never-takers to our sample, which should not affect inference. We note that in our other paper, we illustrate robustness to slightly longer and shorter pull-forward windows and show that results are unchanged.

3.2.2 VMT Effects of Owning Smaller and More Fuel Efficient Cars

After focusing on households that purchase a new vehicle during the “pull-forward” window, we measure discontinuities in the types of vehicles purchased and the subsequent driving in the year after purchase. We do so by estimating Equation (2) with different outcome variables. First, we estimate the effect on types of cars purchased by defining the outcome variable as fuel economy and various vehicle characteristics such as horse power, curb weight, size, number of cylinders, engine displacement, and four wheel drive. This will estimate the extent to which the program quasi-randomly shifted households into more fuel efficient and smaller, lower performance vehicles.

Second, we estimate the effect on the number of miles driven by defining the outcome variable to be annual vehicle miles traveled by the household (across all vehicles). We also test for whether households shift miles among vehicles in the household’s fleet by defining the outcome variable to be the fraction of total household miles driven in the newly purchased vehicle.¹⁶

The identifying assumption of our analysis is that for households purchasing a vehicle over a period of time when there is no discontinuity in the probability of purchase, all household-level determinants of vehicle miles traveled after 2009 are continuous across the eligibility threshold. Under that assumption, any discontinuity in vehicle miles traveled at the cutoff is properly interpreted as the causal effect of shifting households into more fuel efficient and downsized vehicles.

We find this identifying assumption to be reasonable for several reasons. First, the nature of the program makes manipulation very unlikely. Because households were required to own the “clunker” for one year prior to trade-in, there was little scope for households to manipulate where they were relative to the cutoff. Moreover, the fuel economy that determines eligibility is determined by the vehicle’s EPA fuel economy rating and is independent of any driving behavior by the household.

Second, we find it difficult to construct a mechanism that would violate this assumption. For example, while it is possible to imagine why barely eligible households would be different from ineligible households who bought *during the program*, it is hard to think why this would be true over this longer time horizon. By construction this longer time horizon contains a

¹⁶Within-household substitution of driving across vehicles can lead to biased estimates when using vehicle-level data not linked at the household level. For example, if a household replaces a medium-MPG minivan with a high-MPG small sedan, it may well substitute miles toward its other vehicle – say, a low-MPG SUV – which would cause the researcher with vehicle-level data, unable to observe this shift, to overstate the fuel savings. On the other hand, the household may instead substitute miles from the low-MPG SUV to the high-MPG sedan, which would yield larger fuel savings than expected. Knittel and Sandler [2013] show evidence of within-household substitution of miles between vehicles. A strength of our household-level data is that we can quantify any within-household substitution.

similar number of new vehicle buyers across the cutoff – the only difference is that some of those with clunkers rated at eighteen MPG or below were incentivized to purchase earlier during that time window than the other households.¹⁷

The identifying assumption is also consistent with empirical evidence. We show that there is no compelling evidence of discontinuities with respect to household characteristics or pre-treatment purchase and driving behavior. For example, we find no differences in the demographic characteristics of households that own “clunkers” just above and below the eligibility threshold. Likewise, we compare the driving and gasoline consumption of the households in our sample in the year prior to Cash for Clunkers and find no significant discontinuities.

3.3 Data

Our empirical setting is Texas, the second largest state in the U.S. as measured either by population or consumption of gasoline for transportation.¹⁸ We use several large administrative databases in Texas for our study.

To determine household-level vehicle fleets over time, we use confidential vehicle registration records maintained by the Texas Department of Motor Vehicles (DMV). This database allows us to identify the vehicles in a household’s fleet and when the household purchased each vehicle so that we can trace the evolution of each household’s fleet. In addition to providing a measure of fleets, these records include the unique vehicle identification number (VIN) for each registered vehicle. The VIN information in the DMV data allow us to measure a variety of characteristics for each vehicle, such as EPA-rated fuel economy and horsepower. We use a database obtained from DataOne Software to “decode” the VIN of each car in our sample. Importantly, our data on fuel economy is the same information that was used to determine eligibility for the CfC program.

We compute our measure of vehicle miles traveled (VMT) primarily from odometer readings recorded during annual vehicle emissions tests, which we link by VIN. An important institutional feature for our study is that emissions tests are required annually in seventeen EPA non-attainment counties in Texas for each vehicle older than two years, a more stringent requirement than that mandated by many states. These counties include the areas

¹⁷An example that would violate the identifying assumption is if the program were to accelerate some purchases by (say) two years, while simultaneously causing a similar number of eligible households to delay their purchases by more than a year. If that were the case – and it does seem far-fetched – the rate at which households bought vehicles over the “pull-forward” window might be similar across the cutoff, even though household characteristics would be different.

¹⁸Measures of state-level gasoline consumption by end use are available from the U.S. Energy Information Administration at http://www.eia.gov/state/seds/sep_fuel/html/pdf/fuel_mg.pdf.

surrounding Houston, Dallas-Fort Worth, Austin, and El Paso.¹⁹ Although Texas is sometimes stereotyped as having more trucks and heavy vehicles than other states, the mix of vehicles in these four urban areas is very similar in terms of fuel economy to that in many urban areas across the U.S. (see, e.g., [Busse, Knittel, and Zettelmeyer \[2012b\]](#)’s Figure 9 on fuel economy for each Census tract in the country). From these two databases, we calculate household vehicle ownership, vehicle characteristics, annual VMT, and annual fuel consumption. We provide details on this process in Appendix A. Our data on household VMT are quite complete – we observe annual VMT for over 98% of households that purchased new vehicles during the 12-month pull-forward window.

We use a simple approach to classify each household’s distance from the CfC eligibility cutoff – the running variable in our regression discontinuity design. Our goal in doing so is to determine which vehicle in a household’s fleet is most likely to be removed from the fleet when a new car is purchased, and use the fuel economy of that “clunker” to classify the household relative to the eligibility cutoff. We expect these vehicles to be older, lower-value vehicles given the requirement that they be scrapped to qualify for a CfC subsidy. We define the clunker for each household as the oldest vehicle that the household owns, measured by the vehicle model year, as of June 30, 2009. In the rare case that a household owns two vehicles with the same model year, we use the vehicle that the household has owned for the most days. This simple method of defining clunkers yields remarkably similar predictions as that using a more complex propensity score method, while requiring less completeness of data on vehicle characteristics.

In addition, we impose several sample restrictions. Because the focus of our study is on household drivers, rather than institutional fleets, we follow [Knittel and Sandler \[2011\]](#) in excluding a small number of households that owned more than seven vehicles as of June 2009 (just before CfC). Because CfC offered a maximum subsidy of \$4500, we require that the household’s clunker be at least five model years old to exclude higher value vehicles that were unlikely to be scrapped. We include only households that had owned their clunker since at least July 2008, as one condition for CfC transactions was that the vehicle had been owned by the household for at least a full year. Finally, we restrict our sample to households whose potential clunker had an EPA combined rating of between ten and twenty-seven miles per gallon, which spans the largest bandwidth used in our regression discontinuity specifications.

In some specifications, we use demographic data from the Census. These data include Census tract-level economic and demographic characteristics from the 2000 decennial Census, which we link using address information in the administrative database. Finally, in tests of

¹⁹The Texas Commission on Environmental Quality (TCEQ) provided us with emissions test records for vehicles in EPA non-attainment counties in Texas. These counties include four of the largest metropolitan areas and nearly 60% of the state’s population.

the identification strategy, we use a separate dataset from the spring 2009 National Household Travel Survey (NHTS). Although the NHTS does not include information allowing for direct matching to our data at the household-level, it includes a random sample of the households in Texas, so we can use the rich survey information in NHTS to test our identifying assumption.

We estimate discontinuities for households that purchased a new vehicle during the 12-month pull-forward window – the period spanning from the start of CfC in July 2009 through June 2010. As we show in Section 4.1, the barely-eligible and barely-ineligible households were equally likely to purchase a new vehicle during this time window. Summary statistics for this sample are presented in Table 1. There are 153,821 households purchasing new vehicles in our sample. The mean rated fuel economy of the new vehicles is 22.1 MPG. As far as driving behavior, the mean annual VMT for a household summed across all vehicles in the household is 32,177 miles and the mean annual gasoline consumption is 1675 gallons. This table also summarizes Census Tract characteristics such as demographics and income, which we use as control variables.

4 Results

4.1 Pull-Forward Window

The first step of our empirical analysis is to estimate the time period for which the Cash for Clunkers program did not affect the probability that a household purchased a new vehicle. The program likely induced some households that would soon be in the market for a new car to pull the sales forward so as to qualify for the subsidy. We estimate this pull-forward window and use the sample of households purchasing during this time window in our primary analysis. We have a priori reasons to believe that this set of households is very likely to satisfy our identification assumption, and we show evidence of the identification assumption in Section 4.4.

Intuitively, we find the time window, beginning with the first month of the two month program, where households with barely eligible clunkers are equally likely to purchase a new vehicle as households with barely ineligible clunkers. Thus we start with a dataset that includes *all households* in Texas (in EPA non-attainment counties) and investigate the probability that a household purchases a new vehicle.

Results are shown in Figure 2, which take the same form as subsequent figures. The x-axis shows the running variable of the MPG of the household’s clunker, and the y-axis shows the outcome variable. Households just to the left of the vertical line own clunkers with fuel economy of 18 MPG and are barely eligible, while households just to the right of the

vertical line are barely ineligible. The circles and triangles represent local averages, where the marker size corresponds to the number of households in the MPG bin.

Panel (a) of Figure 2 shows the probability that a household in Texas purchased a new vehicle *during the two months of the Cash for Clunkers program*. There is a clear discontinuity at the eligibility cutoff, suggesting that the program increased the likelihood of purchasing a new vehicle by more than one half of a percentage point. Thus, it is clear that Cash for Clunkers accelerated the timing of new car purchasing by the eligible households.

However, as one can see from the other panels in Figure 2 that show progressively longer time windows, the ineligible households have an equal purchase probability by the end of the first half of 2009. The purchase probability is nearly equalized for the time window July 2009-March 2010 (panel (b)), and appears to be fully equalized by the late spring of 2010 (panels (c)-(e)).

Although the purchase probability is equalized by late spring of 2010, we choose to use a full year – July 2009-June 2010 – as the pull-forward window for our subsequent analysis. We do so to be conservative in our estimates and ensure smoothness of unobservables across the discontinuity. By extending our window, at worst we add never-takers to our sample, which should not affect inference. Thus our analysis below focuses attention on the subset of households in Texas that purchased new vehicles during this 12-month pull-forward window.

4.2 New Vehicle Characteristics

Our regression discontinuity analysis shows strong evidence that the Cash for Clunkers program induced households to purchase vehicles that were both more fuel efficient and “down-sized”. Results are shown in Figure 3. Panel (a) shows visually compelling evidence that the barely eligible purchased vehicles that were more fuel efficient than the barely ineligible. Corresponding regression estimates are shown in the first row of Table 2. Column (1) of the table shows regression results for a cubic polynomial fit to the data in Figure 3. The barely eligible purchased vehicles with a fuel economy rating 0.87 MPG higher than the barely ineligible. Thus, program eligibility increased the fuel economy of new car purchases by 3.9%.

Importantly, the magnitude of our estimate is unaffected by the inclusion of controls, as shown in column (2), which is consistent with our identifying assumption. The additional columns show the regression discontinuity estimates for smaller bandwidths and other functional forms of the polynomial. Across all specifications, the results show robust evidence that program eligibility induced households to purchase vehicles that were more fuel efficient. This suggests that a standard rebound effect may be present and that gasoline consumption would respond less than proportionately to fuel economy *if* there were no corresponding changes in vehicle attributes.

However, the vehicles purchased by the barely eligible did not only differ in fuel economy – the vehicles were downsized relative to the purchases of the barely ineligible. We use various metrics of vehicle characteristics to illustrate the downsizing. First, we use Book Value as a composite measure of the value of the vehicle. Panel (b) of Figure 3 shows that program eligibility induced households to purchase vehicles that are distinctly cheaper. Corresponding regression estimates in the second row of Table 2 show that the Manufacturer Suggested Retail Price (MSRP) for vehicles purchased by the barely eligible was nearly \$1900 smaller than vehicles purchased by the barely ineligible. The other specifications of the regression discontinuity confirm a robust relationship; program eligibility caused households to purchase lower value cars.

We can assess the specific vehicle characteristics that comprised the downsizing. Panels (c) and (d) of Figure 3 illustrate two of these characteristics. The cars purchased by the barely eligible have a lower curb weight, which as shown in Anderson and Auffhammer [2014], increases the fatality risk in the event of an accident. In addition, the cars purchased by the barely eligible have less horsepower-per-pound, a proxy for driving performance.

Table 2 shows regression estimates for the effect of program eligibility on a full set of vehicle characteristics. One dimension of downsizing involves characteristics associated with comfort and safety. The barely eligible purchase vehicles with a curb weight that is 150-200 pounds lighter (see row 3 of Table 2). The footprint of the wheelbase is slightly over 1 square foot smaller (row 4). And the vehicle size, as measured by height*width*length, is smaller by 15-20 cubic feet.

A more complete set of performance-related characteristics related to downsizing are shown in the remaining rows of Table 2. Consistent with panel (d) of Figure 3, horsepower-per-pound is estimated to be significantly lower among the barely eligible car buyers. Engine displacement is around 0.2L smaller as well. Finally, the barely eligible are around 9% less likely to purchase a vehicle with at least 6 cylinders, and 1-2% less likely to purchase a vehicle that is 4-wheel or all-wheel drive.

These results offer compelling evidence that program eligibility caused households to purchase vehicles that are both more fuel efficient and also have characteristics that are associated with less comfort, safety, and vehicle performance. We are unable to separately explore the effects of any of these individual vehicle characteristics on outcomes such as driving behavior because they are highly collinear with fuel efficiency. However, the upshot is that both individual characteristics and a composite measure of value suggest that the barely eligible downsized. If this set of vehicle attributes is complementary to the utility of driving ($\frac{\partial VMT_i}{\partial X_i} > 0$ in Equation(1)), then the increase in fuel economy could cause an attribute-based adjustment that counteracts the standard rebound effect. In the next section, we estimate

the joint effect of increased fuel economy and downsizing on household vehicle miles traveled.

4.3 Household Driving Outcomes

We next turn to whether the households barely eligible for Cash for Clunkers were likely to drive more miles in the year after purchasing a new vehicle, as compared to households that were barely ineligible. As we discussed in Section 2, while the impact of reducing the price-per-mile should increase vehicle miles traveled, that increase could be offset or even reversed by the impact of the associated change in other vehicle attributes.

Importantly, we analyze the effect of purchasing a new vehicle on all driving by the household.²⁰ Because we measure a household’s entire vehicle stock, we can measure the effects on driving both the new car and any within-household substitution of miles across vehicles in multi-vehicle households.

Results are shown in Figure 4. Panel (a) shows discontinuities in the total number of annual household miles driven in the year following purchase. It is clear that the barely eligible households do not appear to drive more total household miles than the barely ineligible. In fact, if there is any impact, the barely eligible households appear to drive fewer miles after purchasing a relatively more efficient and downsized vehicle. Regression estimates of the discontinuity are shown in the first row of Table 3. The estimated discontinuity varies slightly depending upon the polynomial used to fit the data and the bandwidth. However, in no specification is the estimate of the effect positive. In fact, point estimates indicate that driving *decreased* by 1-4% as a result of purchasing a more fuel efficient and downsized vehicle, though only three of the seven coefficients are significant at the 5% level.

These findings have important implications when viewed in context of the components of the gasoline consumption elasticity of fuel economy that we describe in Equation (1). This estimated driving response reflects the two countervailing effects of policy-induced increases in fuel economy – reducing the price-per-mile and reducing complementary attributes. In this setting, it appears that the attribute-based adjustment fully counteracts any rebound effect. While the estimated effect size varies somewhat across specifications, the important policy message is clear: increasing fuel economy, coupled with associated changes in vehicle characteristics, does not increase the amount of driving.

As we discuss above, we are not able to decompose the two components of the elastic-

²⁰A small number of households purchased more than one vehicle during the pull-forward window; we analyze the driving outcomes summed across all vehicles. Also, one might worry that our ability to measure a vehicle’s VMT could differ across the discontinuity because the barely eligible are likely to purchase earlier in the pull-forward window, which impacts when the vehicles are ‘due’ for their emission tests. We think this is unlikely, but test whether the availability of annual VMT data changes across the discontinuity. The fourth row of Table 3 shows no evidence of differences in data completeness across the eligibility threshold.

ity. For technological reasons, fuel economy is highly correlated with many of the vehicle attributes. The set of vehicles purchased does not provide sufficient independent variation in fuel economy and attributes. However, if future CAFE standards are achieved through vehicle downsizing, the policy relevant effect is the total elasticity, which we find is not positive, as suggested by the existing literature.

We also estimate the effect of program eligibility on household gasoline consumption. Given that program eligibility increases fuel economy without increasing vehicle miles traveled, the gasoline response to increases in fuel economy will not be less than proportional. The estimated effect of program eligibility on total household annual gasoline consumption is shown in the second row of Table 3. Our measure of gasoline consumption is calculated as each vehicle’s annual VMT divided by the EPA rated fuel economy and then summed across all vehicles in the household. Given the mechanical relationship between VMT, MPG, and fuel consumption, there is little surprise that estimates in the second row are negative. Eligibility for the program induced new vehicle purchasers to reduce gasoline consumption by 44 to 97 gallons per year, depending upon specification. These figures represent reductions ranging from 3-7% across the specifications, and all of the estimates are statistically significant at the 5% level.

Finally, we test whether the purchase of more fuel efficient vehicles causes households with more than one vehicle to shift the utilization of their fleet. On one hand, a household may have an incentive to shift miles to the more efficient vehicles. However, the vehicles with higher fuel economy also have characteristics that may lower the utility of driving. To directly test for substitution in either direction, we estimate the impact of program eligibility on the fraction of VMT driven by the new car. Of course, any household is likely to shift miles from older vehicles to a new vehicle. We test if this tendency to shift miles to the new car is relatively larger for the barely eligible households who purchased more fuel efficient cars. Results are shown in the third row of Table 3. Estimates are not statistically different from zero and are economically small, with point estimates smaller than 1% of miles. In short, there is little evidence that the fuel economy (and associated characteristics) affects in net how miles are shifted towards new vehicles.

4.4 Tests of the Identification Strategy

The identifying assumption of our empirical strategy is that all determinants of VMT in the year after purchase vary smoothly across the Cash for Clunkers eligibility cutoff among the households that purchased new vehicles from July 2009-July 2010. In this section we test for a variety of potential threats to identification.

The identification assumption could be violated, for instance, if politicians endogenously

selected the 18 MPG eligibility threshold based on types of vehicles that would qualify. We test this assumption in several ways. We use data from the 2009 National Household Travel Survey to test whether there are discontinuities in demographic characteristics among the population of vehicle owners. As shown in Figure 5, this is little compelling visual evidence of discontinuities in vehicle owner characteristics such as number of adults in household, number of weekly travel days, household income, living in an urban area, living in a single-family house, or race, which is consistent with our identifying assumption. In Appendix Table A.1, we test formally for discontinuities in nine demographic characteristics, and find that none are statistically different from zero.

As described earlier, our analysis focuses on new car buyers, rather than all car owners. We do this in part because we otherwise cannot disentangle the effect of driving a more fuel efficient vehicle from the effect of driving a new vehicle. As a result, our identifying assumption requires that for households that bought a new vehicle during the 12-month pull-forward window, all determinants of VMT in the year after purchase vary smoothly across the eligibility cutoff. We implement several tests where we focus on new car buyers using our DMV registration data.

Our first test examines whether the households that purchased during the pull-forward window were different in the *year prior* to the Cash for Clunkers program. Results are shown in Figure 6 with corresponding regression estimates in Table 4. The first row of the table shows the discontinuity in the fuel economy of the household’s vehicle fleet *excluding the clunker*.²¹ Estimates indicate that if anything, barely eligible households may have a preference for slightly lower MPG vehicles, which suggests that our treatment effect estimates presented earlier may somewhat understate the increase in new vehicle fuel economy due to the program. However, we emphasize that the estimated difference is economically small and is not robust to alternative specifications – only four of seven estimates are statistically significant at the 5% level, and two estimates are positive rather than negative.

The second row of Table 4 analyzes total household VMT in the year prior to treatment – estimates of the discontinuity are small and not statistically different. And, finally, the barely eligible households that purchased during the pull-forward window did not consume more gasoline in the year prior to treatment as compared to the barely ineligible, as shown in the third row.

As another identification test, we consider the possibility that there is some general underlying difference between new car buyers with “clunkers” on either side of the 18 MPG threshold. Here, we test whether new-car buying households just below the cutoff *always*

²¹If we included the fuel economy of the clunker - which defines the running variable - then mechanically the relationship would be smooth through the 18 MPG clunker threshold.

tend to buy vehicles that are more efficient, but are also smaller and lower-performance. To test for this possibility, we analyze the purchase and driving behavior of households in 2008 – the year prior to Cash for Clunkers. Figure 7 and Table 5 show new vehicle characteristic discontinuity estimates for households that purchased a new vehicle in calendar year 2008 as a function of the household’s “clunker” in 2008.²² Visually there are no discontinuities in new vehicle characteristics in Figure 7. Formal estimates in Table 5 show little evidence of discontinuities for any of the 10 vehicle characteristics capturing value, MPG, size, safety, and performance. Only 12 of the 70 estimates shown are significant at the 5% level, and for no characteristic is the estimate statistically significant at the 5% level in more than 3 of the 7 specifications.

Results for driving outcomes are shown in Figure 8, with corresponding regression estimates shown in Table 6. There is little evidence of a discontinuity in the fraction of miles driven in the newly-purchased vehicle; only one of seven coefficients is significant at conventional levels. However, there is some evidence that households just below the 18 MPG threshold drive fewer miles even before the program; four of the seven coefficients shown in Table 6 are statistically significant at the 5% level. We note, however, that the estimates are highly sensitive to specification; in general, smaller bandwidths and lower-order polynomials result in estimates that are economically and statistically indistinguishable from zero. In addition, none of the estimates in row (2) of Table 6 are meaningfully more negative than the corresponding treatment estimates from Table 3. Thus, even if one differences out these estimates from the estimates in Table 3, there is little evidence to indicate that barely eligible households in 2009 drove more miles as a result of being induced to drive more fuel efficient, but downsized, vehicles.

5 Conclusion

A critical energy policy question is whether increases in fuel economy will increase miles driven and thus partially undo gains from those increases, exacerbating externalities associated with driving and gasoline consumption. To our knowledge, this is the first paper to address this question using quasi-random variation in a household’s fuel economy. We show that while households that were barely eligible for the subsidy purchased significantly more fuel efficient and downsized vehicles, they did not respond by driving more miles. As a result, we find that there is no evidence of a rebound effect that would offset some or all of the reduction in fuel consumption that would arise when households are induced to drive more efficient vehicles.

²²To define the household’s clunker in 2008, we use a similar approach to define the clunker, except that we use the oldest vehicle in a household’s vehicle stock as of December 2007.

This paper has important implications for policies that target gasoline consumption with fuel economy standards. Policies such as CAFE are likely to change not only the price-per-mile of driving but also other characteristics of vehicles, as dictated by the technology of vehicle production. If policymakers were to mistakenly focus on estimates of the traditional rebound effect – reducing the price per mile of driving while holding constant vehicle characteristics – they would ignore that more fuel efficient vehicles also have characteristics that impact the utility of driving. We argue that the policy-relevant effect is not the “partial elasticity” of miles driven with respect to the price per mile of driving. Rather policymakers should consider the effect on driving of shifting households into vehicles that differ in many characteristics. In the case of Cash for Clunkers, we find that households did not drive more miles in response to purchasing vehicles with higher fuel economy but lower size and performance.

Under other policies such as CAFE, the magnitude of the effect could differ depending on the choice sets available when CAFE standards take effect. Nevertheless, the upshot of our result is clear – inducing households to purchase more fuel efficient cars will not necessarily increase driving and exacerbate driving-related externalities. This suggests that using rebound estimates that hold vehicle characteristics constant can overstate the driving response to fuel economy standards. This has implications for policy design, as the National Highway Traffic Safety Administration (NHTSA) explicitly accounted for a rebound effect of 10% when it was designing the 2012 Corporate Average Fuel Economy (CAFE) standards. More generally, our results give policymakers some cause for optimism, as it suggests that second-best strategies such as CAFE used to combat the negative externalities associated with gasoline consumption are more effective than previously thought.

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Figure 1: Illustration of Two Components of Policy-Induced Improvement in Fuel Economy

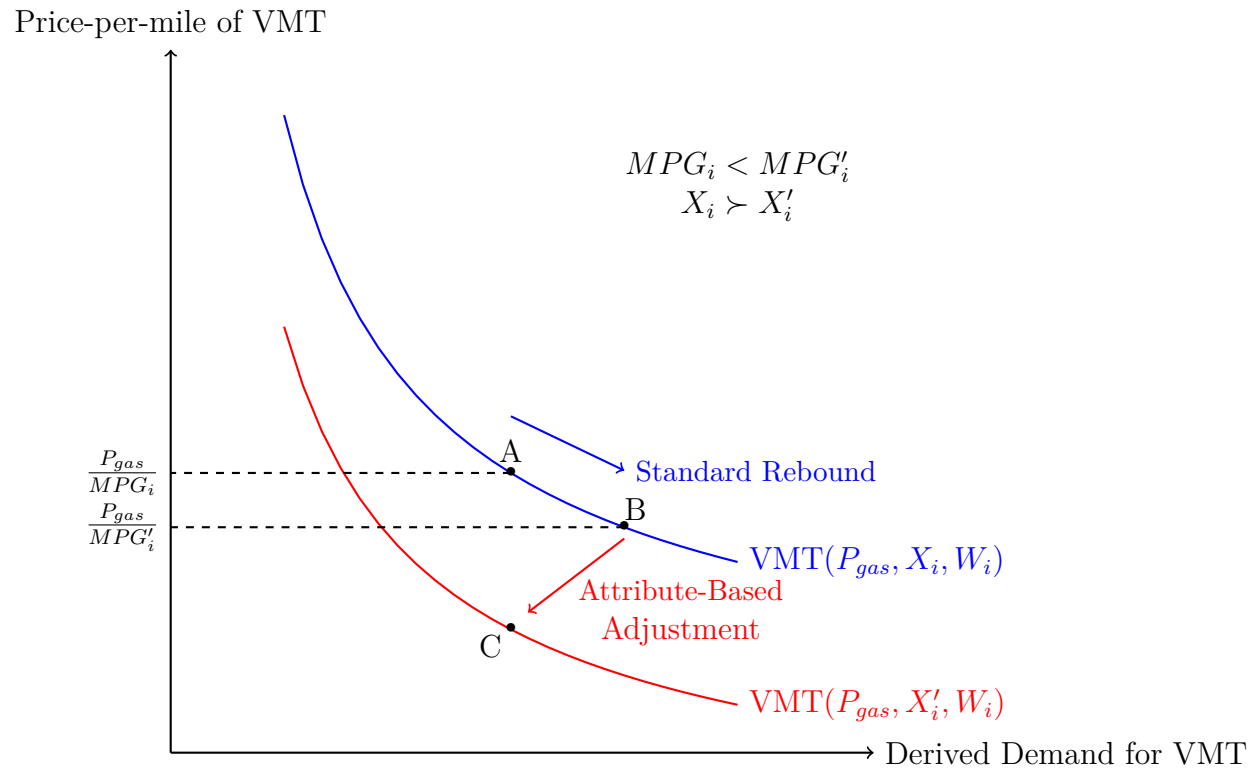
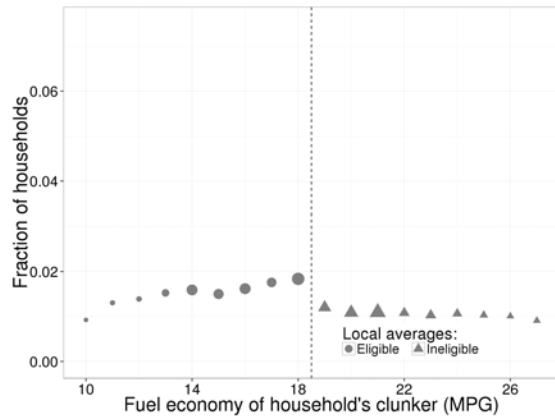
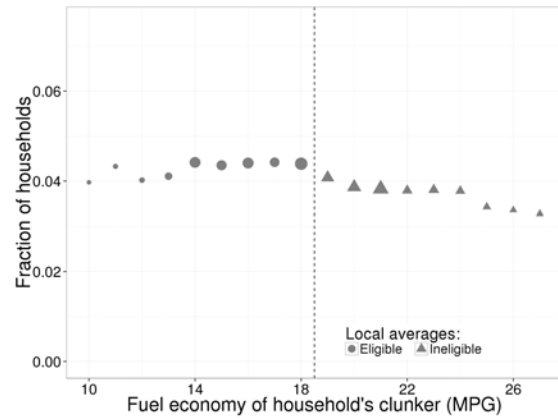


Figure 2: Defining the Pull-Forward Window: Cumulative fraction of households purchasing any new vehicle by time period

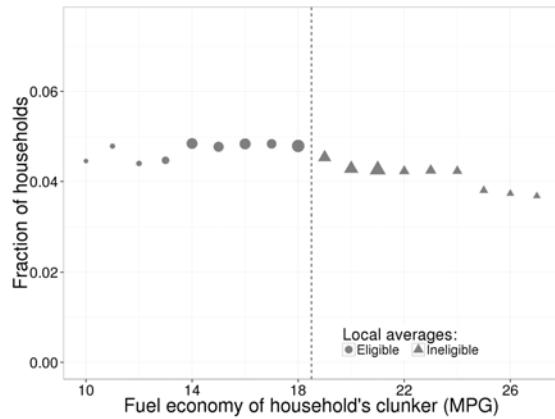
(a) July 2009 - August 2009 (Cash for Clunkers)



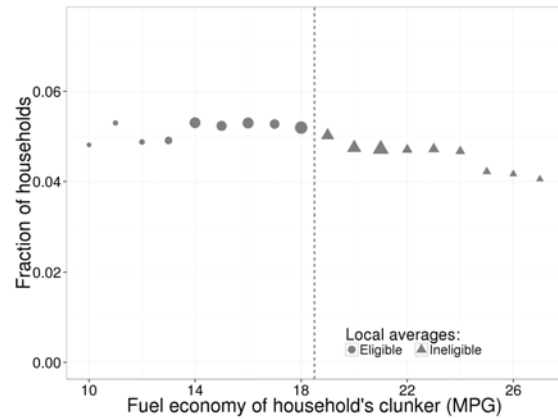
(b) July 2009 - March 2010 (9 months)



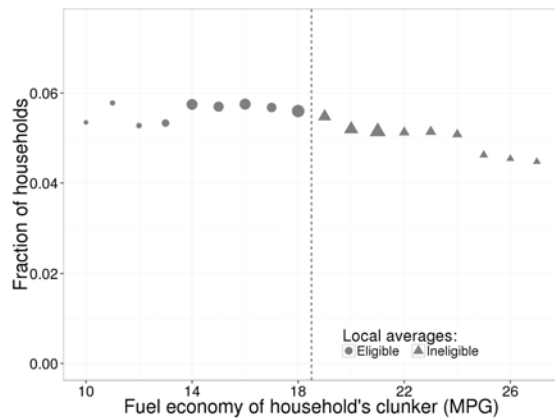
(c) July 2009 - April 2010 (10 months)



(d) July 2009 - May 2010 (11 months)



(e) July 2009 - June 2010 (12 months)



(f) July 2009 - July 2010 (13 months)

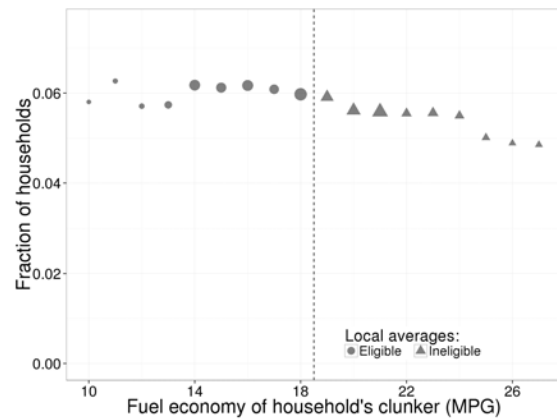
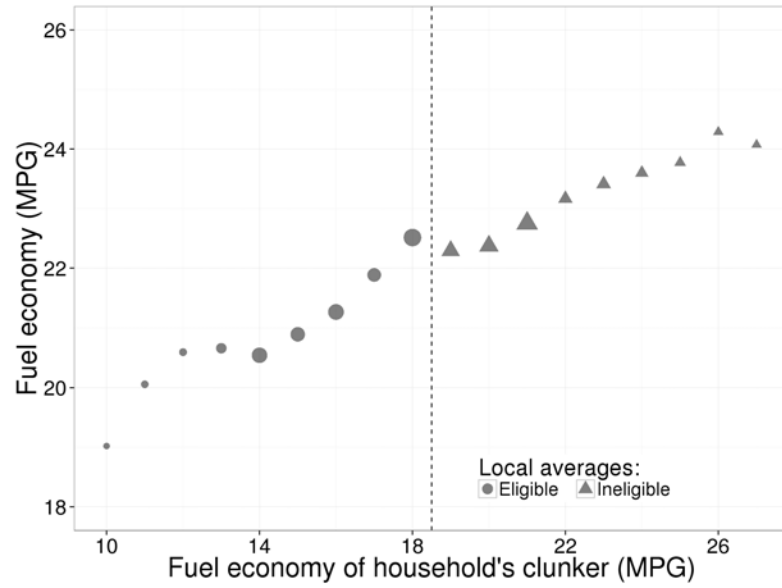
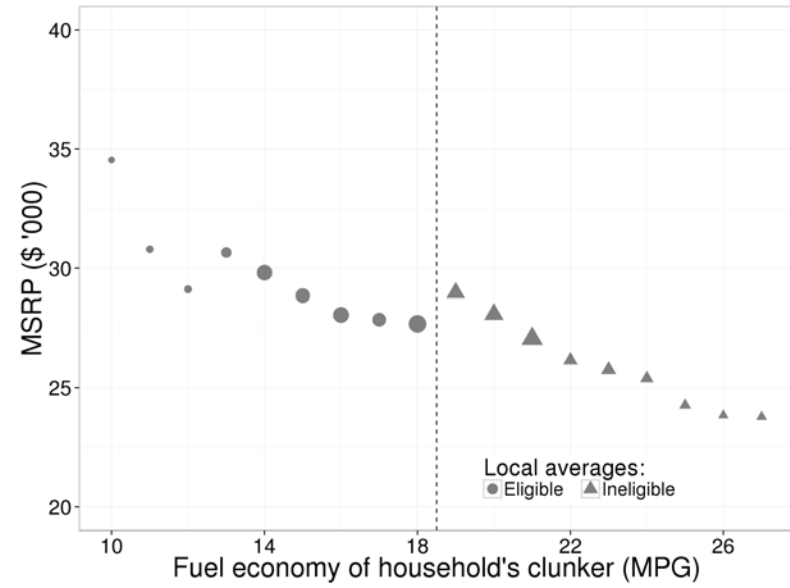


Figure 3: Discontinuities in new vehicle characteristics for purchases during July 2009 - June 2010

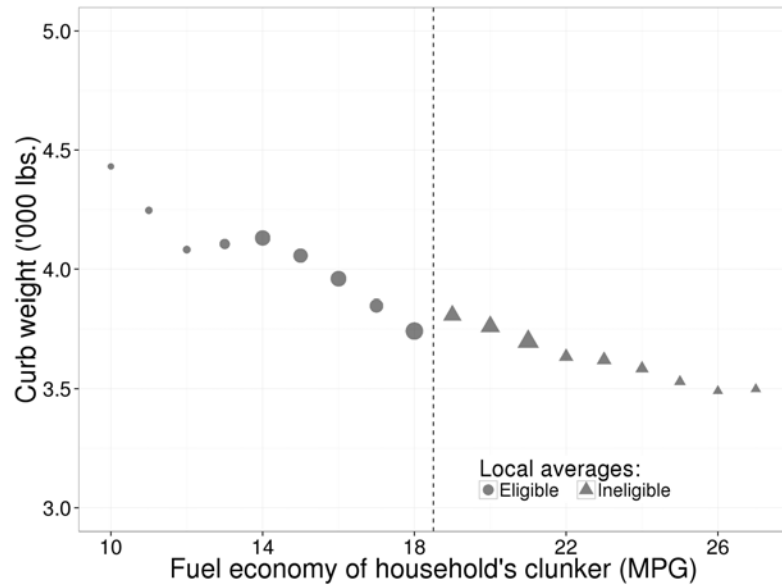
(a) Fuel efficiency (miles per gallon)



(b) Book value (manufacturer suggested retail price)



(c) Safety (curb weight)



(d) Performance (horsepower-per-pound)

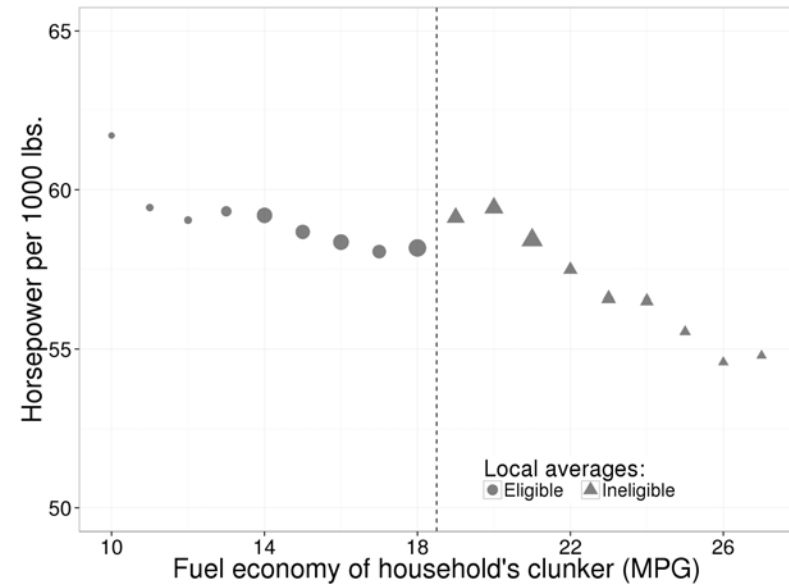
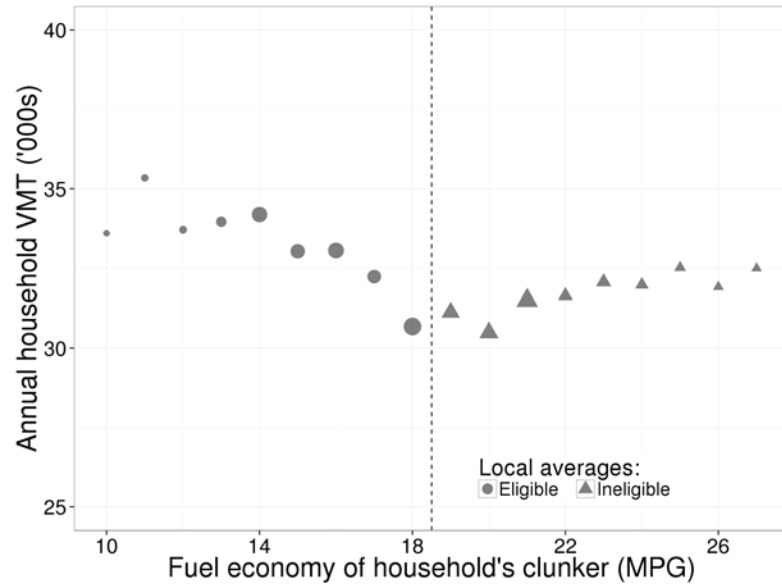
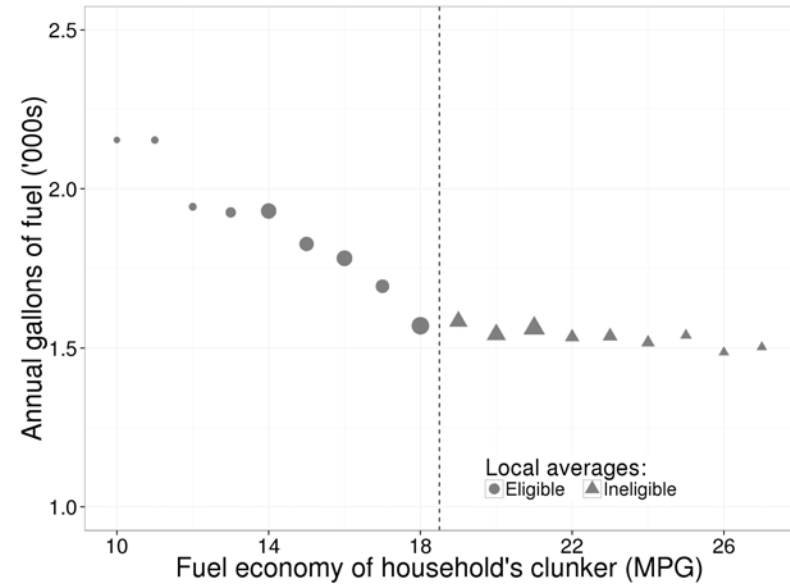


Figure 4: Discontinuities in subsequent household driving outcomes for purchases during July 2009 - June 2010

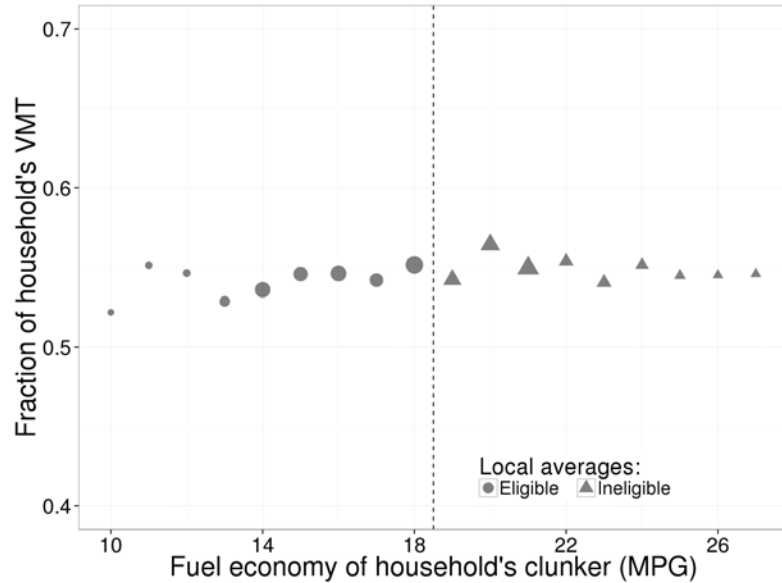
(a) Annual total household vehicle miles traveled (VMT)



(b) Annual total gallons of fuel consumed by household



(c) Fraction of miles driven in newly-purchased vehicle



(d) Household's vehicle miles traveled is observed

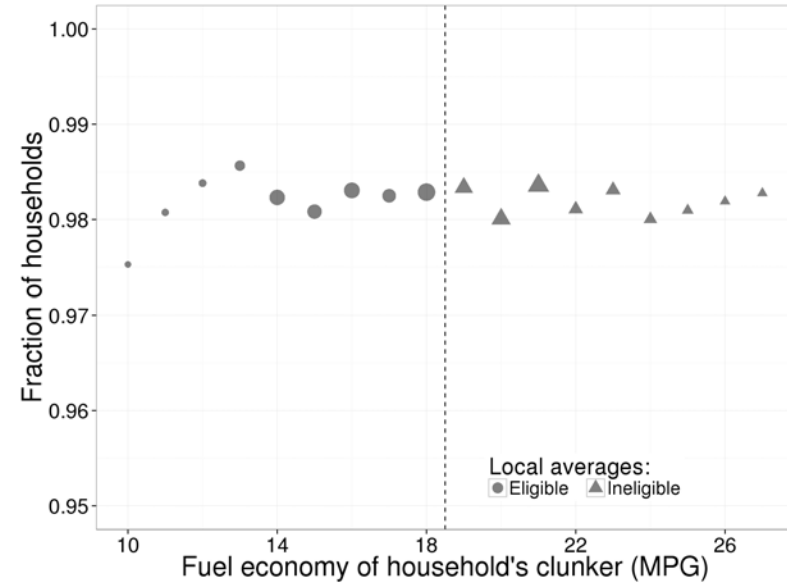


Figure 5: Identification check: National Household Travel Survey (spring 2009)

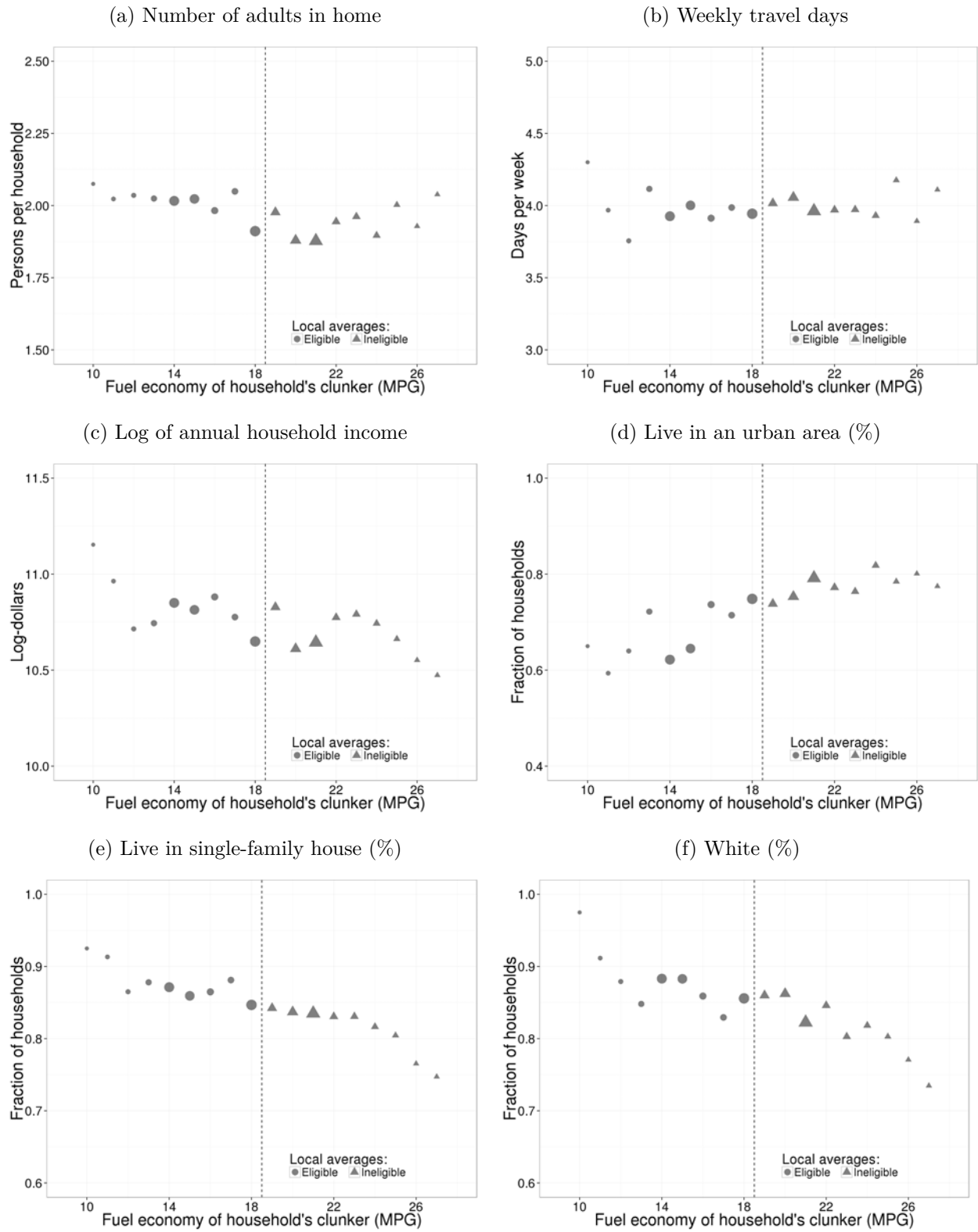


Figure 6: Identification check: Discontinuities in pre-treatment characteristics for purchases during July 2009 - June 2010

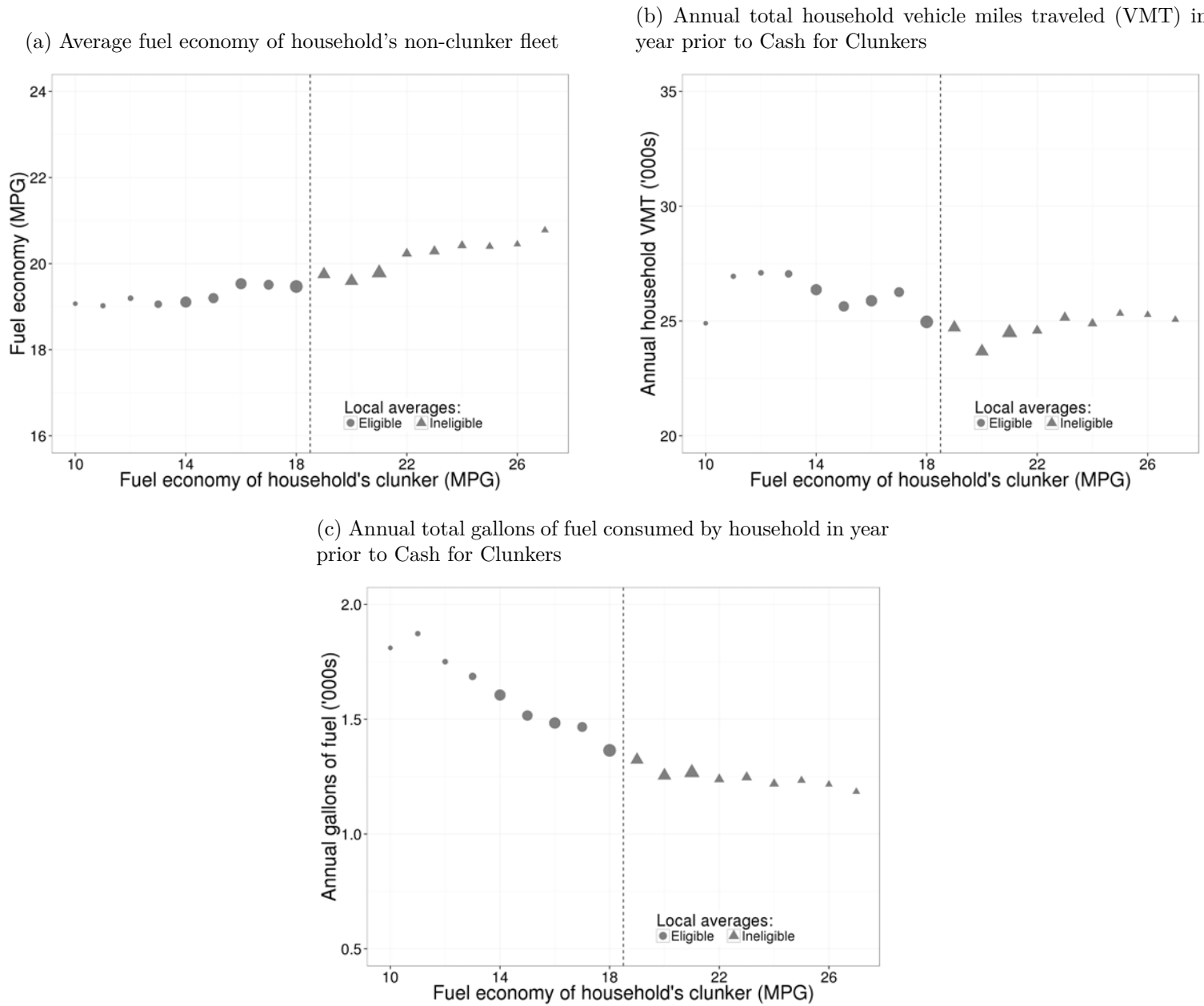


Figure 7: Identification check: Discontinuities in new vehicle characteristics for households purchasing in year prior to CfC

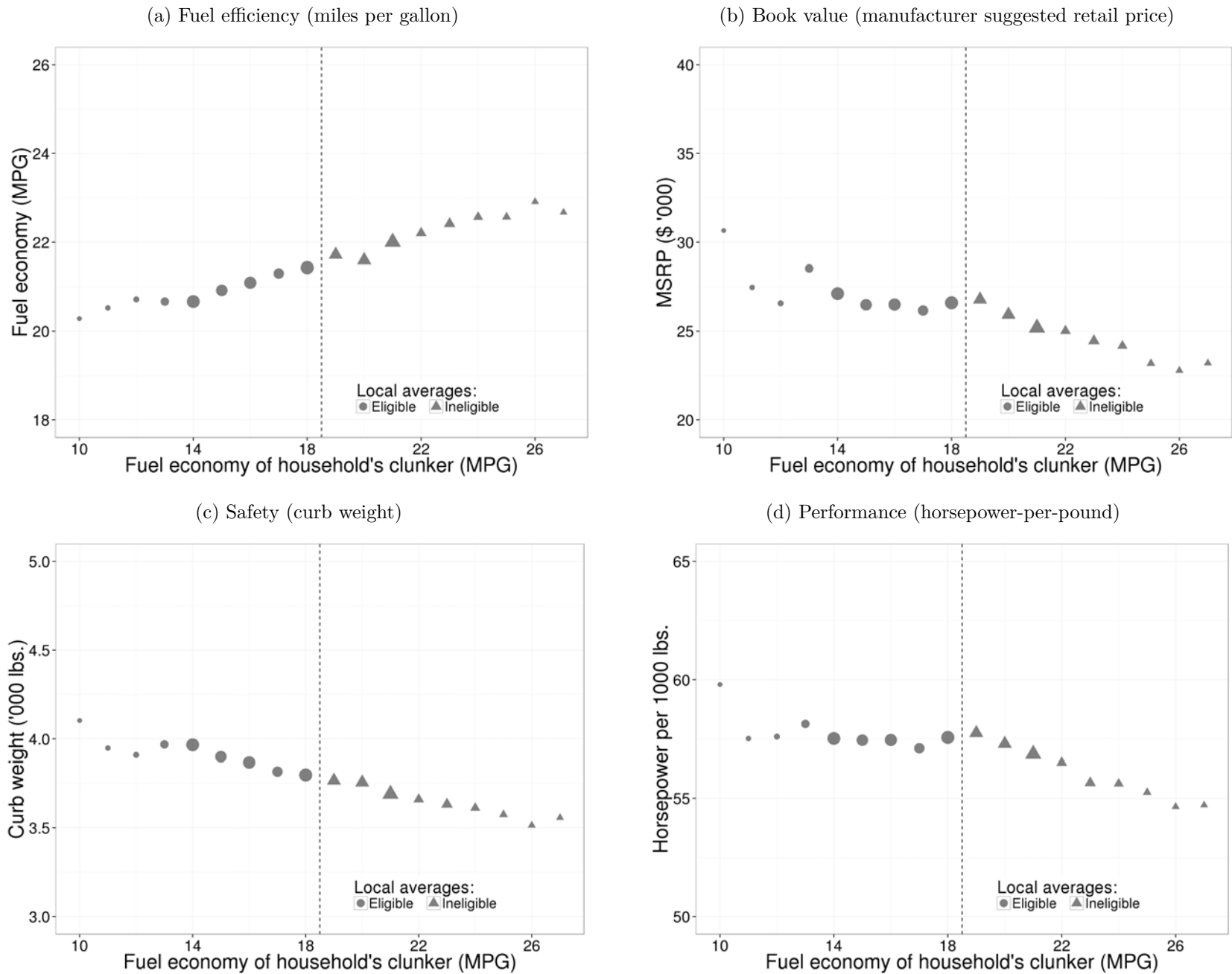


Figure 8: Identification check: Discontinuities in subsequent driving outcomes for households purchasing in year prior to CfC

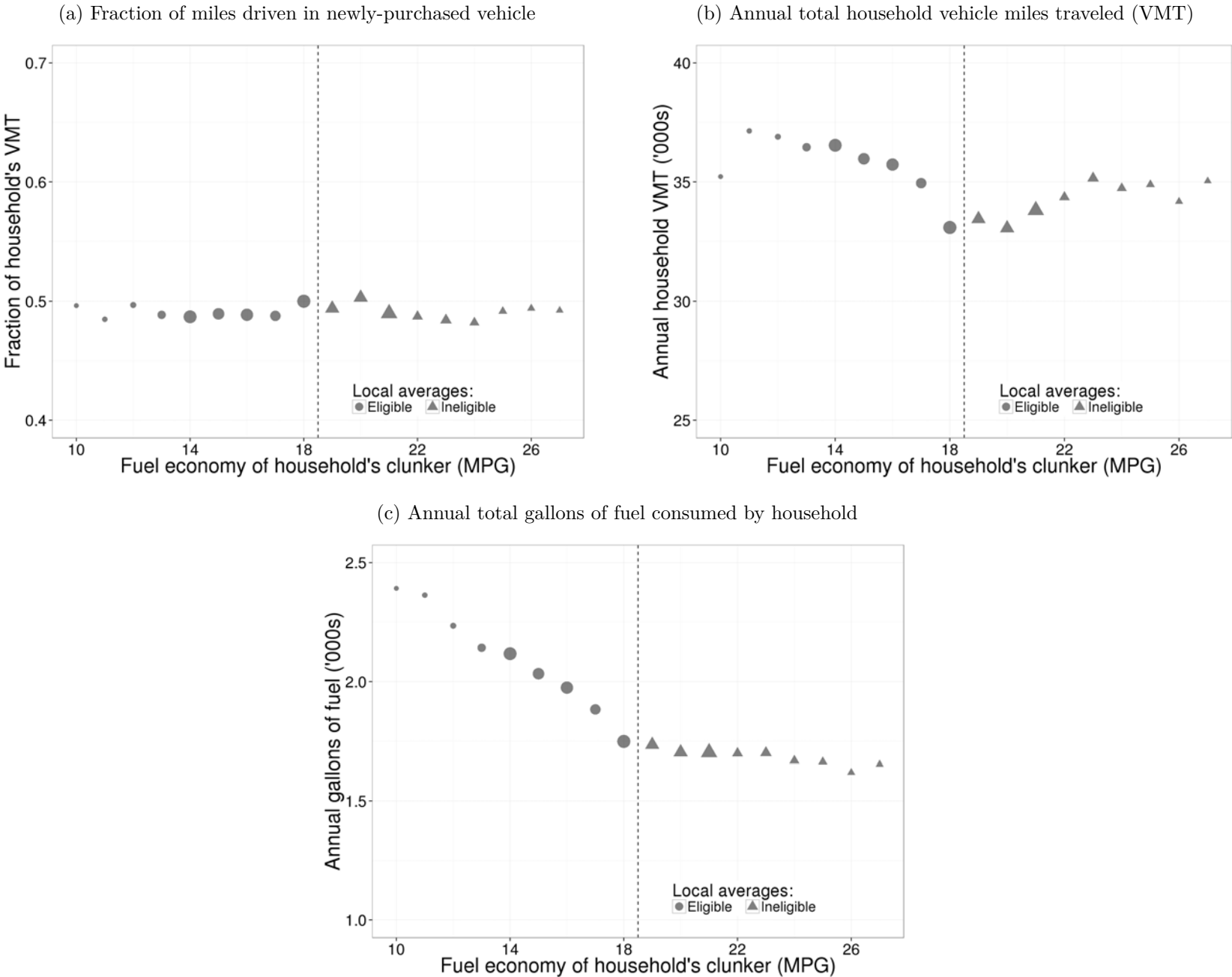


Table 1: Summary statistics for buyers of new vehicles July 2009 - June 2010

	Median	Mean	Std. Dev.
Number of households		153,821	
Number of new vehicles		166,536	
Characteristics of new vehicles			
<i>Fuel efficiency (MPG)</i>	21	22.1	5.886
<i>Book value (MSRP \$'000s)</i>	25.8	27.8	11.12
<i>Safety/Comfort</i>			
Curb weight ('000s lbs.)	3.61	3.84	0.877
Footprint (ft ²)	55.6	58.2	17.48
Size (HxWxL ft ³)	491.1	529.8	190.8
<i>Performance</i>			
Horsepower	211	226	76.83
Horsepower/1000 lbs.	55.7	58.3	13.12
Engine disp. (L)	3	3.26	1.221
I{6+ cylinders}	1	0.55	0.498
I{4WD or AWD}	0	0.079	0.271
Household driving outcomes			
Fraction of VMT in new vehicle	0.50	0.55	0.272
Total annual household VMT	28,168	32,177	21,532
Annual household fuel consumption (gallons)	1429.2	1675.2	1212.5
Census Tract characteristics			
Population	6087	6781.2	3346.2
Median age	33.5	34.1	4.643
White (%)	81.9	76.0	18.36
Black (%)	4.20	9.04	14.06
Asian (%)	2.60	4.52	5.528
Hispanic (%)	11.7	19.9	20.68
Household size	2.92	2.87	0.414
Housing units	2244	2481.3	1177.8
Owner-occupied (%)	81.3	75.0	19.75
Median Income (\$ '000s)	60.3	63.5	25.96
Median Home value (\$ '000s)	114.9	133.3	84.49

Notes: Statistics reported for Texas households residing in an EPA non-attainment county that purchased a new vehicle either during Cash for Clunkers or during the subsequent ten months (12 months from July 2009 through June 2010 in total). The Census Tract-level characteristics are from the 2000 Decennial Census.

Table 2: Discontinuities in new vehicle characteristics for purchases during July 2009 - June 2010

Bandwidth	9 MPG			7 MPG		5 MPG	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fuel efficiency (MPG)	0.872*** (0.107)	0.854*** (0.0976)	0.795*** (0.126)	0.627*** (0.0585)	0.876*** (0.108)	0.721*** (0.0663)	0.630*** (0.125)
Book value (MSRP \$'000s)	-1.892*** (0.264)	-1.641*** (0.208)	-1.802*** (0.150)	-1.223*** (0.187)	-1.862*** (0.219)	-1.551*** (0.106)	-1.756*** (0.158)
Curb weight ('000s lbs.)	-0.196*** (0.0216)	-0.189*** (0.0204)	-0.159*** (0.0125)	-0.149*** (0.00997)	-0.183*** (0.0163)	-0.164*** (0.00874)	-0.125*** (0.0105)
Footprint (ft ²)	-1.399*** (0.435)	-1.390*** (0.428)	-0.915** (0.358)	-1.080*** (0.334)	-1.253** (0.416)	-1.173*** (0.322)	-0.485 (0.274)
Size (HxWxL ft ³)	-20.17*** (4.950)	-19.90*** (4.790)	-14.79*** (3.108)	-13.24*** (3.385)	-19.09*** (4.255)	-16.33*** (3.038)	-8.942*** (2.299)
Horsepower	-15.20*** (2.035)	-14.17*** (1.889)	-15.15*** (2.079)	-10.04*** (1.245)	-15.81*** (2.125)	-12.38*** (1.242)	-13.82*** (1.556)
Horsepower/1000 lbs.	-1.124*** (0.306)	-0.965*** (0.298)	-1.683*** (0.496)	-0.436* (0.235)	-1.504*** (0.455)	-0.853*** (0.243)	-1.888*** (0.494)
Engine disp. (L)	-0.213*** (0.0344)	-0.205*** (0.0324)	-0.204*** (0.0294)	-0.131*** (0.0185)	-0.224*** (0.0329)	-0.166*** (0.0170)	-0.175*** (0.0198)
6+ cylinders	-0.0988*** (0.0127)	-0.0934*** (0.0118)	-0.105*** (0.0182)	-0.0632*** (0.00692)	-0.106*** (0.0158)	-0.0779*** (0.00565)	-0.0946*** (0.0146)
4WD or AWD	-0.0197*** (0.00420)	-0.0166*** (0.00355)	-0.0175*** (0.00411)	-0.0118*** (0.00286)	-0.0171*** (0.00314)	-0.0146*** (0.00176)	-0.0129** (0.00470)
Polynomial	Cubic	Cubic	Quadratic	Cubic	Quadratic	Quadratic	Linear
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	166,536	166,536	166,536	158,540	158,540	138,073	138,073

Notes: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Each coefficient represents a separate regression of the dependent variable (in rows) on an indicator for CARS eligibility, which is β_3 in Equation (2). Heteroskedasticity-robust standard errors, clustered on the running variable, are reported in parentheses. Figure 3 presents graphs corresponding to selected of these results. Controls include county fixed effects and Census tract measures of population, median age, median income, percent that are white, black, Asian and Hispanic, average household size, total number of housing units, percent of housing units that are owner occupied, and median home value.

Table 3: Discontinuities in subsequent household driving outcomes for purchases during July 2009 - June 2010

Bandwidth	9 MPG			7 MPG		5 MPG	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total annual household VMT [Mean: 28,168]	-1053.5** (394.4)	-1031.4** (379.7)	-469.7 (448.5)	-1184.7*** (371.5)	-742.0* (415.7)	-867.4* (396.6)	-174.3 (448.7)
Annual fuel consumption (gallons) [Mean: 1429]	-96.73*** (15.74)	-95.16*** (15.42)	-55.48*** (18.93)	-73.92*** (17.30)	-81.33*** (14.24)	-69.78*** (16.91)	-43.82** (17.36)
Fraction of VMT in new vehicle [Mean: 0.5464]	0.00988 (0.00806)	0.00742 (0.00725)	0.00246 (0.00903)	0.0116 (0.00750)	0.00366 (0.00867)	0.00871 (0.00661)	-0.00244 (0.00926)
Household Post VMT observed [Mean: 0.9832]	0.00188 (0.00180)	0.00202 (0.00174)	0.00008 (0.00123)	-0.00007 (0.00176)	0.00146 (0.00147)	0.00024 (0.00146)	0.0009 (0.00130)
Polynomial	Cubic	Cubic	Quadratic	Cubic	Quadratic	Quadratic	Linear
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	153,821	153,821	153,821	146,470	146,470	127,633	127,633

Notes: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Each coefficient represents a separate regression of the dependent variable (in rows) on an indicator for CARS eligibility, which is β_3 in Equation (2). Heteroskedasticity-robust standard errors, clustered on the running variable, are reported in parentheses. Figure 4 presents graphs corresponding to these results. Controls include county fixed effects and Census tract measures of population, median age, median income, percent that are white, black, Asian and Hispanic, average household size, total number of housing units, percent of housing units that are owner occupied, and median home value.

Table 4: Identification check: Discontinuities in pre-treatment characteristics for purchases during July 2009 - June 2010

Bandwidth	9 MPG			7 MPG		5 MPG	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Non-clunker household MPG	-0.307** (0.133)	-0.314** (0.126)	0.0311 (0.163)	-0.590*** (0.0661)	-0.0467 (0.166)	-0.323*** (0.0969)	0.0940 (0.161)
Total annual household VMT	156.0 (513.5)	96.44 (497.6)	647.1 (605.5)	-595.8 (433.1)	553.5 (589.4)	-25.93 (450.8)	875.5 (605.1)
Annual fuel consumption (gallons)	2.001 (23.26)	-1.390 (22.46)	31.49 (27.72)	-22.27 (21.82)	20.72 (25.41)	0.818 (21.97)	29.48 (26.11)
Polynomial	Cubic	Cubic	Quadratic	Cubic	Quadratic	Quadratic	Linear
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	143,665	143,665	143,665	136,876	136,876	119,280	119,280

Notes: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Each coefficient represents a separate regression of the dependent variable (in rows) on an indicator for CARS eligibility, which is β_3 in Equation (2). Heteroskedasticity-robust standard errors, clustered on the running variable, are reported in parentheses. Figure 6 presents graphs corresponding to these results. Controls include county fixed effects and Census tract measures of population, median age, median income, percent that are white, black, Asian and Hispanic, average household size, total number of housing units, percent of housing units that are owner occupied, and median home value.

Table 5: Identification check: Discontinuities in new vehicle characteristics for households purchasing in year prior to CfC

Bandwidth	9 MPG			7 MPG		5 MPG	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fuel efficiency (MPG)	-0.112 (0.108)	-0.0994 (0.103)	0.111 (0.155)	-0.289*** (0.0870)	0.0814 (0.149)	-0.117 (0.0990)	0.0722 (0.130)
Book value (MSRP \$'000s)	-0.310 (0.327)	-0.157 (0.227)	-0.350** (0.161)	-0.130 (0.208)	-0.286 (0.208)	-0.325*** (0.0975)	-0.274 (0.174)
Curb weight ('000s lbs.)	-0.0120 (0.0179)	-0.0118 (0.0173)	-0.0333 (0.0195)	0.0211 (0.0132)	-0.0353 (0.0214)	-0.00436 (0.0123)	-0.0235 (0.0145)
Footprint (ft ²)	0.141 (0.347)	0.0666 (0.323)	-0.500 (0.407)	0.707** (0.287)	-0.525 (0.440)	0.243 (0.256)	-0.406 (0.347)
Size (HxWxL ft ³)	0.208 (3.895)	-0.725 (3.645)	-6.109 (4.418)	6.770* (3.262)	-6.740 (4.863)	1.865 (2.817)	-4.390 (3.548)
Horsepower	-2.343 (1.455)	-1.848 (1.281)	-3.592** (1.468)	0.630 (0.803)	-3.536** (1.507)	-1.308 (0.788)	-2.964** (1.100)
Horsepower/1000 lbs.	-0.322* (0.171)	-0.193 (0.134)	-0.395** (0.165)	0.0253 (0.135)	-0.344** (0.156)	-0.155 (0.119)	-0.421** (0.154)
Engine disp. (L)	0.000505 (0.0298)	-0.000343 (0.0285)	-0.0404 (0.0350)	0.0641*** (0.0207)	-0.0428 (0.0377)	0.0168 (0.0215)	-0.0295 (0.0284)
6+ cylinders	0.00303 (0.00972)	0.00449 (0.00941)	-0.0165 (0.0158)	0.0246*** (0.00634)	-0.0125 (0.0144)	0.00707 (0.00819)	-0.0145 (0.0139)
4WD or AWD	-0.00661 (0.00444)	-0.00484 (0.00355)	-0.00306 (0.00345)	-0.00362 (0.00373)	-0.00268 (0.00306)	-0.00296 (0.00234)	-0.00255 (0.00245)
Polynomial	Cubic	Cubic	Quadratic	Cubic	Quadratic	Quadratic	Linear
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	148,628	148,628	148,628	141,593	141,593	122,168	122,168

Notes: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Each coefficient represents a separate regression of the dependent variable (in rows) on an indicator for CARS eligibility, which is β_3 in Equation (2). Heteroskedasticity-robust standard errors, clustered on the running variable, are reported in parentheses. Figure 7 presents graphs corresponding to selected of these results. Controls include county fixed effects and Census tract measures of population, median age, median income, percent that are white, black, Asian and Hispanic, average household size, total number of housing units, percent of housing units that are owner occupied, and median home value.

Table 6: Identification check: Discontinuities in subsequent driving outcomes for households purchasing in year prior to CfC

Bandwidth	9 MPG			7 MPG		5 MPG	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fraction of VMT in new vehicle	0.00767 (0.00535)	0.00695 (0.00457)	-0.00199 (0.00611)	0.0124*** (0.00367)	0.000816 (0.00583)	0.00575 (0.00429)	-0.00344 (0.00573)
Total annual household VMT	-1033.3** (367.6)	-1102.4*** (364.8)	-275.4 (528.6)	-1821.3*** (199.4)	-518.5 (498.7)	-1260.0*** (251.7)	184.1 (579.7)
Annual fuel consumption (gallons)	-68.12*** (11.88)	-72.92*** (11.23)	-24.97 (20.30)	-87.03*** (6.726)	-43.93** (15.58)	-64.99*** (9.668)	-11.80 (21.25)
Polynomial	Cubic	Cubic	Quadratic	Cubic	Quadratic	Quadratic	Linear
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	139,046	139,046	139,046	132,480	132,480	114,450	114,450

Notes: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Each coefficient represents a separate regression of the dependent variable (in rows) on an indicator for CARS eligibility, which is β_3 in Equation (2). Heteroskedasticity-robust standard errors, clustered on the running variable, are reported in parentheses. Figure 8 presents graphs corresponding to these results. Controls include county fixed effects and Census tract measures of population, median age, median income, percent that are white, black, Asian and Hispanic, average household size, total number of housing units, percent of housing units that are owner occupied, and median home value.

A Data Appendix

A.1 Defining a Household’s Fleet

The Texas Department of Motor Vehicles (DMV) provided us with confidential access to all Texas vehicle registrations for the years spanning our study. From these records, we attribute individual vehicles to households as follows. First, we used ESRI’s ArcMAP software to geocode the population of entered registration addresses to the North American Address Locator database. Of importance, this process additionally returns the standardized postal address for each specific matched location, thereby correcting for database entry errors. For these standardized addresses, we drop records at any address to which more than 700 unique vehicles (VIN17) were registered within a single calendar year, as these are almost exclusively commercial or institutional registrants. For similar reasons, we drop records for which the last name consists of some variation of a commercial, industrial, or other non-household registrant (e.g. corporation, association, dealer, school, etc.). We drop another roughly one percent of DMV records for the following reasons: (1) we could not match the record to a standardized postal address; (2) the record is missing a sale date; or (3) the record is missing a last name in both last name fields. Finally, we drop records for non-consumer vehicle identification numbers that are not included in EPA fuel economy data (e.g. tractor trailers).

We attribute a pair of vehicles to the same household if either of the following sets of conditions are met: (1) the pair of vehicles is sequentially and jointly registered at multiple locations (i.e. a household moves to a new address); or (2) the pair of vehicles is registered at the same address to the same “fuzzy” last name.²³ After determining pairs of vehicles belonging to the same household, we chain these connections to allocate the population of vehicles to households for each date included in our data.

Because DMV registrations are better suited for tracking vehicle purchases than exits from a household’s fleet, we make two additional adjustments to households’ duration of vehicle ownership. We remove a vehicle from a household’s fleet if the latest observed registration (in Texas) has lapsed by six months. And, because car dealerships often do not appear in the same DMV registration database as households, we backdate a vehicle’s end date for a household if: (1) the vehicle is later sold by a used car dealership, and (2) the former registered household purchased a new vehicle within six months preceding this sale date. This treats the former registrant’s new vehicle purchase transaction date as a trade-in date for the used vehicle.

A.2 Calculating Household VMT

We calculate vehicle miles traveled for each unique vehicle (VIN17) using three sources of odometer readings. Primarily, we use data from annual vehicle emissions tests/safety inspections conducted

²³We use a dynamic Levenshtein distance metric to match last names. First, we trim each of the two last name fields to fifteen letters. Then, we match them pairwise using a Levenshtein critical value of 0.34. The most common entry errors for names in the database are omitted letters (an L-distance of one) and transposed letters (an L-distance of two). For a six letter last name, an L-distance of two requires a critical value of 0.34 to correct. A nine letter last name is allowed three transformations under this critical value.

in the seventeen EPA non-attainment counties in Texas, which were provided to us by the Texas Commission on Environmental Quality (TCEQ) for January 1, 2004 through August 20, 2012. In these counties, Texas law requires personal vehicles to undergo emissions testing annually beginning at the vehicle’s second year.²⁴ New residents are allowed thirty days to obtain a vehicle emissions test. We augment these odometer readings with data from the Texas DMV database, which reports the odometer value for each vehicle transaction involving a Texas buyer. Finally, for a fairly small set of vehicles we append odometer readings reported to the U.S. DOT for vehicles scrapped in the Car Allowance Rebate System (CARS, or “Cash for Clunkers”).²⁵

We determine the temporal duration and total VMT between each sequential pair of odometer readings for each VIN. As many of the odometer readings were at some point manually entered into a database, we attempt to correct for entry errors using several types of adjustments: (1) multiply the reported odometer value by ten; (2) divide the reported odometer value by ten; (3) drop the leading digit of the reported odometer value; (4) subtract one from the leading digit of the reported odometer value; or (5) leave the reported odometer unadjusted. We allow for the adjustment to be made to either the first or the second reading in every sequential pair of odometer values. As a selection metric, for each possible transformation we iteratively compute the equally-weighted average of the absolute value differences between the previous and current, and the current and following readings. In essence, this metric seeks the smoothest path within each set of consecutive three readings. Following this, we drop approximately three percent of remaining readings that imply negative VMT or a daily VMT of less than one or greater than 700. Additionally, at this point we drop readings of fewer than fifty miles apart (which are likely retests of failed inspections) and vehicles for which we observe only a single odometer reading.

Matching on VIN to the DMV registration database, we aggregate vehicle-level VMT to an annual household-level based on vehicle ownership. Within each calendar year, we sum total observed miles driven by the household as well as total days of observed VMT. From these, we compute the average daily VMT per-vehicle for each household for each calendar year. Then, we multiply this value by the total number of “vehicle ownership days” for the household over the calendar year, thus measuring total household-level annual VMT.²⁶

Similarly, we calculate the quantity of gasoline consumed by each household within each calendar year. For each vehicle owned by a household, we divide the total observed miles driven in that vehicle within a year by the vehicle’s EPA rating for combined fuel economy. Then, we divide this value by the number of observed days of VMT within the year to obtain the gallons consumed per day for each vehicle. We multiply this by the number of days in the year for which the household owned the vehicle, and sum across the household’s set of vehicles to determine total gallons consumed per year.

²⁴The annual emissions inspection requirement is waived for vehicles older than twenty-four years. More information on Texas emissions testing requirements is provided by the Texas Department of Public Safety at <http://www.txdps.state.tx.us/InternetForms/Forms/VI-51.pdf>

²⁵The CARS data are available from the [National Highway Traffic Safety Administration](#).

²⁶This approach does extrapolate VMT for some vehicles, but the nature of this calculation restricts extrapolation to within a calendar year. In light of non-compliance, households moving out of emissions testing counties, and other factors precluding odometer observations, we view this as a reasonable trade-off. The overall fraction of VMT determined using such extrapolation is relatively small and is mostly concentrated in the book-ending years of our data, which are not included in our empirical study.

Table A.1: Identification check: Discontinuities in Vehicle Owner Demographics

Bandwidth	9 MPG	8 MPG	7 MPG	6 MPG	5 MPG
Vehicle count	0.0510 (0.0697)	0.0382 (0.0684)	0.0148 (0.0622)	0.00579 (0.0646)	-0.0267 (0.0598)
Driver count	0.0155 (0.0380)	0.00965 (0.0377)	0.00508 (0.0347)	-0.00753 (0.0321)	-0.0217 (0.0295)
Worker count	0.00983 (0.0773)	0.0128 (0.0806)	0.0122 (0.0847)	0.0158 (0.0895)	-0.0319 (0.0782)
Weekly travel days	0.0198 (0.0881)	0.00182 (0.0855)	-0.0599 (0.0666)	-0.0983 (0.0619)	-0.136*** (0.0411)
Number of adults	0.0259 (0.0374)	0.0191 (0.0369)	0.0153 (0.0375)	-0.00173 (0.0342)	-0.0228 (0.0287)
Log of income	-0.0888 (0.0787)	-0.0727 (0.0852)	-0.0699 (0.0908)	-0.0787 (0.0990)	-0.123 (0.0931)
Live in house (%)	-0.0231 (0.0139)	-0.0172 (0.0148)	-0.00893 (0.0116)	-0.0167 (0.0104)	-0.0108 (0.0104)
Live in urban area (%)	-0.00264 (0.0180)	-0.000640 (0.0166)	-0.000193 (0.0175)	-0.000623 (0.0196)	0.00103 (0.0250)
White (%)	-0.0191 (0.0153)	-0.0214 (0.0157)	-0.0229 (0.0170)	-0.0356* (0.0188)	-0.0419 (0.0235)
Polynomial	Linear	Linear	Linear	Linear	Linear
Observations	6335	6252	6060	5763	5303

Notes: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$ Data are from 2009 NHTS for households in Texas. Each coefficient represents a separate regression of the dependent variable (in rows) on an indicator for CARS eligibility, which is β_3 in Equation (2). Heteroskedasticity-robust standard errors, clustered on the running variable, are reported in parentheses.