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How Do US Passenger Vehicle Fuel Economy Standards Affect Purchases of New and Used Vehicles?

Joshua Linn and Xiaoya Dou

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Abstract

Like many energy efficiency standards, passenger vehicle fuel economy and greenhouse gas standards apply to new but not existing vehicles. In theory, such vintage differentiated regulation could raise demand for used vehicles, which would reduce the social welfare gains of tighter vehicle standards. Using household data from 1996 through 2016, which includes periods of stable and tightening standards, we provide the first direct evidence of the effects of standards on choices of new and used vehicles. Tighter standards induce statistically and economically significant shifts from new to used vehicle purchases, which raises welfare costs of tighter standards.

Key Words: passenger vehicles, fuel tax, fuel economy standard, greenhouse gas emissions standard, consumer demand, vintage differentiated regulation, used vehicles

JEL codes: D12, L62, Q41

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* Linn: University of Maryland, linn@umd.edu and Resources for the Future, linn@rff.org; Dou: University of Maryland. We thank Benjamin Leard and participants at the 2018 World Congress of Environmental and Resource Economists for helpful comments.

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1. Introduction

Many consumer durable goods, such as home appliances, are subject to energy efficiency or greenhouse gas (GHG) emissions standards. Typically, these standards apply to new products when they are first sold to consumers, but products that were sold previously are subject to weaker standards or sometimes no standards at all. Passenger vehicles are a prominent example of this form of vintage differentiated regulation of consumer products. Most new passenger vehicles sold globally are subject to fuel economy or GHG emissions standards, and many countries are tightening standards as part of their efforts to reduce GHG emissions and climate change.

Setting standards for new but not existing products can increase or decrease aggregate new product demand. On the one hand, tighter standards raise the relative cost of producing the new products. If manufacturers pass regulatory costs to consumers by raising prices, the higher prices reduce purchases of the new products and increase purchases of secondhand products. Regardless of whether a secondhand market exists, higher new product prices delay scrappage of existing products (Gruenspecht 1982; Stavins 2006; Jacobsen and van Benthem 2015).[1] For example, tailpipe emissions standards for trucks that were introduced in 2007 may have reduced sales of new trucks and delayed scrappage of older trucks and engines that were not subject to new standards.

On the other hand, regulating new products can increase new product sales for two reasons. First, as with any regulation that specifies a market-wide or manufacturer average rate of energy consumption or emissions, vehicle standards implicitly subsidize vehicles that exceed the standards, which could increase aggregate new vehicle sales (Holland et al. 2009; Horowitz and Linn 2015). Second, if the unregulated market underprovides fuel economy from consumers' perspectives, consumers may value the fuel savings from higher fuel economy more than the vehicle price increases, in which case total new vehicle demand could increase (NHTSA 2012).[2]

If tighter standards affect aggregate new vehicle demand, there could be several welfare implications. As standards tighten, new vehicles with low fuel consumption and emissions rates replace older vehicles with higher rates. Lower sales caused by tighter standards would delay the turnover of the fleet to cleaner vehicles. Moreover, because vehicle manufacturers typically earn a markup to recover fixed costs of vehicle design and production (Berry et al. 1995), lower vehicle

[1] Vintage differentiated regulations affect many markets besides consumer products, such as electricity generation (Bushnell and Wolfram 2012). The passenger vehicle literature has identified several other inefficiencies of vehicle standards relative to fuel consumption or emissions taxes. For instance, passenger vehicle fuel economy standards do not account for differences in driving intensity across vehicles (Jacobsen et al. 2017). Standards increase driving by reducing fuel costs—that is, they induce a rebound effect (Chan and Gillingham 2015; Borenstein 2015). Inefficiencies of regulating emissions and fuel consumption rates rather than levels of fuel consumption and emissions pertain to regulating fuels (Holland et al. 2009), power plants (Linn et al. 2014), and other products.

[2] This argument presumes that consumers choose new vehicles based on most or all of the discounted fuel cost savings. The argument is at odds with the arguments that the Department of Transportation makes elsewhere that consumers appear to undervalue fuel cost savings (NHTSA 2012).

sales can reduce profits in the short run. In fact, many manufacturers claim this has occurred during recent tightening of passenger vehicle standards, and in proposing to weaken federal fuel economy and GHG standards the Trump administration argues that higher new vehicle prices reduce total sales (EPA and NHTSA 2018).[3]

Despite these possibilities, we are not aware of any direct empirical evidence of the effects of such regulation on secondary markets. Jacobsen and van Benthem (2015) estimate the effects of gasoline prices on vehicle scrappage, but they do not estimate the effects of vehicle *standards* on scrappage or used vehicle markets. Other articles on the welfare effects of standards either ignore used vehicle markets and scrappage altogether (e.g., Goldberg 1998; Klier and Linn 2012) or calibrate structural models to observed equilibriums rather than estimate parameters from observed effects of standards on consumer choices (e.g., Jacobsen 2013). This lack of empirical evidence on standards contrasts with the substantial evidence on consumer responses to fuel prices.[4]

Focusing on US passenger vehicles, we provide the first evidence of vintage differentiated regulation of consumer products on purchases of new and used products, and we quantify two welfare implications of those effects. Hypothetically increasing standards by 0.1 percent in the year 2016 reduces new vehicle purchases by 0.02 percent, or about 4,000 units. The lower number of purchases reduces manufacturer profits and raises the welfare costs of achieving tighter standards by 15 percent. This cost has not been considered previously either in the literature or in the regulatory agencies' benefit-cost analysis.

After describing the data in Section 2, in Section 3 we present estimates of the effects of new vehicle standards on purchases of new and used vehicles. The empirical analysis uses recent variation in fuel economy standards and a novel data set of household-level purchase decisions from 1996 through 2016. We collected data on about 160,000 households from the Consumer Expenditure Survey (CEX), which includes household demographics and detailed information about the vehicles they obtained. We combined the CEX data with gasoline prices from the Bureau of Labor Statistics (BLS) and vehicle attributes from Wards Auto.

We follow Klier and Linn (2010, 2016) in measuring the stringency of the fuel economy standards for each vehicle in the data. For new vehicles, stringency is defined as the log ratio of the fuel economy requirement the vehicle faces and the vehicle's actual fuel economy. Considering a particular new vehicle type, stringency varies over time as the standards tighten. In addition, stringency varies across new vehicles in a given year because of cross-sectional fuel economy variation and the fact that each vehicle's fuel economy requirement depends on its footprint (roughly, the area

[3] Changes in aggregate vehicle sales could have other welfare implications, such as the number and severity of traffic accidents (because newer vehicles tend to be safer than older ones). Implications other than the two mentioned in the text lie outside the scope of the paper.

[4] Among others, see Klier and Linn (2010) and Leard et al. (2017a) on gasoline prices and new vehicle sales; Li et al. (2009) on gasoline prices and fleetwide fuel economy; and Busse et al. (2013), Allcott and Wozny (2014), and Sallee et al. (2016) on gasoline prices and vehicle prices and sales. See Gillingham (2014) and West et al. (2017) for recent evidence on the effects of fuel costs on miles traveled. Several recent articles (e.g., Houde, forthcoming) examine welfare effects of efficiency standards for appliances that do not have large secondary markets.

defined by the four wheels) and its class (car or light truck). The variable is defined so as to be proportional to the shadow cost on fuel economy that the standards create. We test whether the stringency faced by a particular new vehicle affects that new vehicle's purchases. Separately, we test whether the average stringency of new vehicles in a particular year affects used vehicle purchases.

Tightening the standards reduces aggregate new vehicle purchases. This estimate reflects changes in household decisions to purchase new vehicles or purchase used vehicles. The results are robust to adding additional controls for demand and supply shocks that could be correlated with the stringency variation in the data.

Section 4 discusses the implications of the decline in new vehicle purchases caused by tighter standards. We consider tighter standards in 2016, the final year of our sample. As we argue in Section 4, under certain assumptions the social benefits of tighter standards are proportional to aggregate new vehicle purchases. The reduction in aggregate purchases therefore reduces fuel economy and GHG benefits by 0.02 percent, which is a small amount.

However, the decline in aggregate new vehicle purchases substantially reduces manufacturer profits. We estimate that the lower number of purchases reduces manufacturer profits by roughly 15 percent in the first year of tighter standards. The lower profits constitute a cost to manufacturers besides the direct costs of adding fuel-saving technologies to meet tighter standards. Accounting for the reduction in aggregate new vehicle purchases reduces net benefits (the difference between benefits and costs) of tighter standards by 26 percent. Despite the higher costs, net benefits of tighter standards in 2016 remain positive according to our calculations. For reasons we discuss in Section 4, the lower profits persist only in the short run, up to about five years after standards tighten.

By providing the first direct estimates of the effects of vehicle standards on new vehicle purchases, our analysis complements recent papers that analyze the aggregate welfare and distributional consequences of standards and taxes. Jacobsen and van Benthem (2015) is perhaps most closely related to our analysis. The authors estimate the effects of used vehicle prices on scrappage and use a calibrated model to estimate the effects of standards on new vehicle purchases as well as on used vehicle scrappage. In contrast, we directly estimate the effects of standards on new vehicle purchases and hold scrappage fixed. In addition, we believe our analysis is the first to estimate the short-run reduction in manufacturer profits caused by the decline in purchases.

Our results also have implications for the recent literature on the distributional effects of tighter standards (e.g., Davis and Knittel 2017; Levinson 2016). We find that low-income households respond more to standards than high-income households, suggesting that low-income households may view new and used vehicles as closer substitutes to one another than do high-income households. This difference should be considered when assessing the distributional effects of tighter standards, because it implies that low-income households may suffer lower welfare losses when they substitute to used vehicles than do high-income households; the literature has generally not accounted for this possibility.

2. Data and Background

2.1. Data

This subsection describes the main data set used in the analysis, which we constructed primarily using the CEX. We chose the CEX

because the data are unique in being publicly available, occurring at an annual frequency, containing detailed information about vehicles and demographics, and including a large sample of vehicle buyers.[5]

To assemble the data set, we collected the public CEX microdata from 1996 through 2016. We constructed each household's stock of vehicles, along with the household's income, family size, number of children, census region, gasoline expenditure, total expenditure, population density of the area, and urbanization status, as well as certain attributes of the household head (age, education, marital status, employment status, and race). For each of the household's vehicles, we obtained the vehicle's age, brand, class, fuel type, purchase year and month, model year, and purchase price.[6] We merged footprint and fuel economy from Wards with the CEX data by model year, class, and brand.

We narrowed the sample to observations of recently purchased new and used vehicles. Having constructed each household's stock of vehicles at the time of the survey, we discarded vehicles that were obtained more than 18 months before being surveyed. The 18-month window balances a trade-off between statistical precision and bias. On the one hand, using a longer window increases the sample size and precision of the estimates. On the other hand, a longer window may introduce bias. The survey asks only about vehicles belonging to the household at the time of the survey. Therefore, we do not observe vehicles that were obtained and then either sold or scrapped prior to the survey. For example, if a household surveyed in 2005 bought a used vehicle in 2002 and sold the vehicle in 2004, the survey data would not include information about the vehicle. Consequently, the longer the window, the greater the likelihood that the household obtained and discarded a vehicle within the window. Such behavior would introduce bias, because the sample would include only those vehicles that were obtained within the window and held until the time of the survey; the magnitude of the bias is likely to increase with the window length.

We chose the 18-month window based on a comparison of CEX data with vehicle sales data from Wards Auto, which includes national new vehicle sales data by month, model, and fuel type. We used Wards data from 1996 through 2016 to compute the share of light trucks in total sales. The CEX data match the Wards new light truck share closely using an 18-month window (correlation of 0.9) and more closely than using other windows. Moreover, CEX purchases by brand and class match Wards sales more closely using the 18-month window than using other windows.

We dropped observations with missing demographics and vehicle attributes. We have about 160,000 household observations.

For reasons explained in Section 3, we aggregate the household data. We compute the count of used vehicles purchased by income group, brand, class, fuel type, age category, and year. We aggregate by income group to

[5] The National Household Travel Survey contains a large sample of US households purchasing new and used vehicles. However, because the survey is conducted roughly every eight years rather than annually, only two survey waves have occurred since the recent tightening of the standards. This yields insufficient variation to identify the effects of the standards on new and used vehicle purchases.

[6] A particular company may sell multiple brands. For example, Ford sells vehicles under the Lincoln and Mercury brands, among others. Many companies have a luxury or high-end brand, such as the Infiniti brand sold by Nissan.

allow us to assess whether responses to standards vary across income groups. We use vehicle age groups rather than single-year age bands because of the CEX sample size. For used vehicles, we define three age categories: less than 5 years old, 5–9 years old, and at least 10 years old. The categories are constructed to include roughly the same number of used vehicles in each category over the entire 1996–2016 period. We merge fuel economy and footprint from Wards for the corresponding brand, class, age category, and year. For example, for used Honda cars 5–9 years old purchased in 1999, we use Wards fuel economy and footprint for new Honda cars sold between 1990 and 1994.[7]

2.2. Background and Summary Statistics

This subsection presents background information about gasoline prices, fuel economy regulation, and demographics of vehicle buyers during the sample period. A vehicle's fuel costs depend on fuel prices and fuel economy. For 1996 through 2016, Figure 1 plots annual average gasoline prices by region. In all four regions, prices were stable and low in the late 1990s, increased through much of the 2000s, were volatile and high in the late 2000s and early 2010s, and decreased at the end of the sample. Also plotted in the figure is the West Texas Intermediate price of crude oil, which is a national benchmark price. The figure shows that gasoline prices tracked crude oil prices closely through most of the sample. The figure also shows that regional gasoline price differences persist through much of the sample, as prices in the Northeast and West tend to be higher than prices in the South or Midwest. The differences among regional prices vary somewhat over time, however, as a result of temporary transportation bottlenecks or other factors. As we explain below, the empirical strategy uses temporal and regional variation in gasoline prices to identify the effects of a vehicle's fuel costs on its purchases.

Figure 2 shows the fuel economy standards for cars and light trucks between 1996 and 2016. The National Highway Traffic Safety Administration (NHTSA) has set separate standards for cars and light trucks since 1978. For cars or trucks sold by manufacturer k, the harmonic mean fuel economy is computed as

$$\frac{\sum_{j \in J_k} q_j}{\sum_{j \in J_k} q_j / m_j} \tag{1}$$

where j indexes the vehicles sold by manufacturer k, J_k is the set of vehicles the manufacturer sells, q_j is the vehicle's purchases, and m_j is the vehicle's fuel economy. The manufacturer is in compliance if its harmonic mean exceeds the corresponding standard.[8] The manufacturers can borrow and bank credits to a limited degree. Since 2011, manufacturers can use car compliance credits to meet the light truck standards and vice versa, although regulations limit the amount of these credit transfers. Manufacturers can also trade credits with one another; see Leard and McConnell (2017) for a description of the fuel economy regulation and the changes in crediting provisions.

[7] This approach introduces some measurement error for used vehicles. Implicitly, we assume that the average fuel economy and footprint of used vehicles purchased in a particular year and age group are equal to the averages of the corresponding brand and class when the vehicles were purchased new. This assumption would not hold if, for example, larger vehicles in a given brand and class are more likely to be sold soon after purchase than are smaller vehicles belonging to the same brand and class.

[8] Imported vehicles are treated separately for compliance. As in most of the literature on fuel economy regulation, we do not distinguish between imports and domestic production because of data limitations.

The standards were phased in between 1978 and the late 1980s (not shown), and they did not change between 1990 and the mid-2000s. Beginning in 2005, the standards for light trucks began increasing by about one mile per gallon (mpg) per year. Then in 2011, the light truck standards began increasing more quickly. The car standards began increasing in 2011. Between 2010 and 2016, the light truck standards increased by 23 percent, and the car standards increased by 37 percent.

An important feature of the standards is that during the last few years of the sample, they depended on a vehicle's footprint, which is roughly the area defined by the four wheels. Until 2012, each manufacturer faced a single standard for its cars that did not depend on any attributes of its cars; for example, all manufacturers faced a standard of 27.5 mpg in 2002. Likewise, for light trucks, each manufacturer faced the same standard until 2011. Starting in 2011 for light trucks and 2012 for cars, each vehicle's fuel economy requirement depends on its footprint, with larger vehicles having a lower fuel economy requirement. The footprint-based standard is defined as

$$\frac{\sum_{j \in J_k} q_j}{\sum_{j \in J_k} q_j / M_c(fp_j)} \qquad (2)$$

where fp_j is the vehicle's footprint and $M_c(fp_j)$ is the function mapping footprint to the fuel economy requirement. Note that the function differs across classes, c. If one manufacturer sells larger vehicles than another, the manufacturer selling larger vehicles faces a lower fuel economy standard.[9]

Thus there are several sources of variation in the fuel economy requirement a particular vehicle faces. First, the overall standards began tightening in 2005 for light trucks and 2011 for cars. Second, starting in 2011 for light trucks and 2012 for cars, the fuel economy requirement for a particular vehicle in a particular year depends on its footprint and class.

Figure 3 illustrates the stringency variation in the data. As described in the next section, we measure a vehicle's stringency as the log ratio of its fuel economy requirement to its fuel economy in a particular period. Tighter standards increase the stringency variable. A vehicle's stringency is mechanically correlated with the vehicle's fuel economy and footprint. For new vehicles, the figure plots the footprint (Panel A), log of vehicle purchases (Panel B), and prices (Panel C) in 2004 against the stringency in 2016. We chose the year 2004 because it was just prior to the tightening of the standards. Each data point represents a unique vehicle, and the dashed lines indicate fitted values. Panel A shows that stringency is positively correlated with footprint, and Panel B shows that stringency is negatively correlated with purchases. These correlations are small, less than 0.2 in magnitude. Panel C indicates a positive correlation between stringency and vehicle prices. Although not shown, this correlation disappears if we condition on fuel economy.

[9] Between 2008 and 2010, manufacturers had the option of meeting a footprint or uniform standard for light trucks. A manufacturer's fuel economy standard depends on the production of each of its vehicles (note that we treat sales, purchases, and production as synonymous, as is customary in the literature). For example, an increase in a manufacturer's sales of small cars would raise its fuel economy standard. Therefore, the regulations do not fix each manufacturer's fuel economy standard. In Figure 2, we illustrate the standards under the sales assumption that EPA and NHTSA made in their benefit-cost analysis of the 2012–16 standards (EPA and NHTSA 2011). The actual standards differed from these levels because of unexpected changes in gasoline prices and other factors (Leard et al. 2017a).

Figures 4 and 5, as well as Tables 1 and 2, present summary statistics for household heads among purchasers of cars and light trucks. The data indicate substantial differences between car and truck buyers and between new and used vehicle buyers. Figure 4 shows the share of buyers of a particular vehicle type who belong to the indicated demographic group. For example, the leftmost solid bar in Panel A is the share of new car buyers who belong to the lowest household income group. Panel A shows that higher-income groups account for larger shares of new vehicle purchases than of used vehicle purchases. Panel B shows that new vehicle buyers tend to belong to higher education categories (based on the respondent's highest degree attained) than do used vehicle buyers. Within new and used vehicles, car and light truck buyers tend to have similar education levels to one another. Panel C indicates that households in areas with large population sizes account for larger shares of new vehicle purchases than they do of used vehicle purchases. Households in areas with small population sizes account for larger shares of used light truck purchases than do households in areas with large population sizes.

Table 1 illustrates large and statistically significant differences between car and truck buyers for other demographics, including whether the household head is married, retired, or employed. Households buying light trucks tend to be larger and have more vehicles than households buying cars. For new vehicle consumers, the variation in demographics across vehicle types is consistent with the findings of Klier et al. (2016), who use data from the National Household Travel Survey.

Table 2 shows variation across income groups in fuel economy, the probability of buying a new car, and purchase price. The probability of buying a new car is the share of new cars in total purchases. The table indicates relatively little variation in fuel economy across income groups, compared with variation in the new car share and purchase price. Higher-income households are more likely to purchase new vehicles and pay more for those vehicles than lower-income households. Average prices of new vehicles is roughly constant across the three lowest household income groups.

Figure 5 shows estimated density functions for the distribution of fuel economy of new and used vehicles. Although mean fuel economy of new vehicles is somewhat higher than that for used vehicles (24.4 mpg versus 23.2 mpg), the distributions overlap considerably, which indicates that consumers can choose similar levels of fuel economy among new and used vehicles.

3. Effects of Standards on Vehicle Purchases

This section presents estimates of the effects of standards on purchases of used and new vehicles. We first discuss the empirical strategy, then present the coefficient estimates, and finally provide evidence supporting the main identification assumption.

3.1. Estimation Equation

A household belonging to a particular income quintile that purchases a vehicle may choose either a new or a used vehicle. We aim to estimate the effects of stringency on both possible outcomes.

We estimate the reduced-form effect of standards on equilibrium purchases of new and used vehicles. By reduced form, we mean that we do not attempt to estimate the mechanism by which standards affect purchases. Instead, our objective is to estimate the equilibrium effect during our sample period.

We could begin by defining the choice set as including each new vehicle and each used

vehicle. In that case, for each new and used vehicle, we could define the dependent variable as the log of ratio of total purchases by vehicle and income quintile to the total number of new and used purchases. Leard et al. (2017a) use a similar dependent variable, except that the denominator includes only total new vehicle purchases, because they are interested in the effects of fuel costs on new vehicle purchases.

We take a slightly different approach. Specifically, we include interactions of income quintile and year as independent variables, and we use the log of purchases for each new and used vehicle, rather than the log of the share, as the dependent variable. Because the dependent variable is in logs, the coefficients on the other independent variables are identical to those we would obtain if we defined the dependent variable as the log share.[10] Consequently, we are able to estimate the effects of stringency on purchases of new and used vehicles.

We measure purchases by income quintile (q), vehicle (j), and purchase year (t), where a vehicle is a unique brand, class, and age group. We include new and used vehicles in the analysis. The estimating equation is

$$lns_{qjt} = \beta_s S_{qjt} + \beta_u \bar{S}_{qct} * U_j + \beta_{fc} f_{qjt} + \beta_{fc\_n} f_{qjt} * N_j + \beta_{g\_n} p_{qt} * N_j + \tau_{qt} + \mu_{qj} + X_{qjt}\mu + \epsilon_{jt} \quad (3)$$

Each new vehicle faces a stringency, S_{qjt}, which we define similarly to Klier and Linn (2016). The variable is the log ratio of the vehicle's fuel economy requirement to its initial fuel economy: $\ln(\frac{M_{ct}(fp_{qj})}{m_{qj}})$. Standards introduce a shadow price on fuel economy (Jacobsen 2013). This functional form is motivated by the fact that all else equal, higher stringency implies a higher shadow price on fuel economy for the manufacturer. The variable equals zero for used vehicles.

The stringency variable uses the vehicle's footprint (fp_{qj}) and fuel economy (m_{qj}) measured in the first year the vehicle is purchased by the particular income quintile. Therefore, stringency varies over time because of changes in the regulatory stringency over time (see Figure 2). The requirement depends on the vehicle's class and footprint, as larger vehicles have lower fuel economy requirements. Given this structure of the regulation, stringency varies across vehicles and quintiles as well as over time. For example, average stringency is lower for Toyota vehicles than for Ford vehicles because Toyota vehicles tend to have higher fuel economy relative to their requirements than do Ford vehicles.

The stringency coefficient could have a positive or negative sign. An automaker facing a positive level of stringency can comply by adopting fuel-saving technology to raise the fuel economy of its vehicles or by adjusting vehicle prices to increase the purchases-weighted average fuel economy. If the manufacturer adds fuel-saving technology and raises fuel economy, the technology would simultaneously raise demand for that vehicle (because consumers value the fuel economy) and raise the cost of producing the vehicle. As Leard et al. (2017b) explain, the net effect could be an increase or decrease in equilibrium vehicle purchases, implying a positive or negative coefficient.

[10] Mathematically, let s_{qjt} equal purchases by income quintile (q), vehicle (j), and purchase year (t), and let M_{qt} equal the number of households in quintile q that purchase a new or used vehicle. The log ratio variable is $\ln\left(\frac{s_{qjt}}{M_{qt}}\right) = lns_{qjt} - lnM_{qt}$. The variable M_{qt} is absorbed by the quintile-year interactions.

The other manufacturer response, adjusting vehicle prices to increase purchases-weighted mean fuel economy, implies a negative coefficient. In equilibrium, higher stringency raises the profit-maximizing price for each vehicle. To see this, consider a manufacturer selling two types of vehicles, one with low fuel economy and the other with high fuel economy ($m_L < m_H$). Suppose that the manufacturer initially does not face fuel economy standards and chooses the profit-maximizing prices for both vehicles and that equilibrium purchases of the two vehicles equal one another. Then the government introduces the same fuel economy requirement for each vehicle, such that the standard is higher than the average fuel economy of the two vehicles and lower than m_H. The profit-maximizing response is for the manufacturer to raise the price of the vehicle with low fuel economy and reduce the price of the vehicle with high fuel economy (Goldberg 1998) to increase the purchases-weighted average fuel economy of its vehicles. That is, the price increases for the vehicle with higher stringency (i.e., that with low fuel economy).

We construct a second stringency variable, $\bar{S}_{qct} * U_j$, to measure the effect of average new vehicle stringency on used vehicle purchases. To construct the variable, we compute the average stringency of all new vehicles by income quintile, class, and year, $\bar{S}_{qct}$. We interact this variable with an indicator equal to one for used vehicles, U_j (the main effects of the independent variables are absorbed by the time interactions and vehicle age fixed effects in X_{qjt}). The interaction variable is positive for used vehicles in a year when new vehicles have positive stringency, and it is zero for new vehicles. The coefficient would be positive if higher new vehicle stringency raises demand and equilibrium purchases for used vehicles; the coefficient measures the average effect of the stringency on used vehicle purchases. Thus S_{qjt} and $\bar{S}_{qct} * U_j$ allow standards to have different effects on new and used vehicles.

Because fuel costs are correlated with stringency, it is important to control for changes in fuel costs that are caused by factors other than stringency, such as gasoline price variation. We define the fuel cost variable similarly to other papers that estimate the effects of fuel costs on vehicle sales (e.g., Leard et al. 2017a). Fuel costs, f_{qjt}, are the ratio of the regional fuel price price (p_{qt}) to the vehicle's fuel economy (m_{qj}), where the fuel price varies by income quintile and year and fuel economy varies by income quintile and vehicle. Note that the fuel economy we use to compute fuel costs does not vary over time. Consequently, if a change in stringency causes a change in fuel economy, the new fuel economy would identify the stringency and not the fuel economy coefficient. Interactions of fixed effects for vehicles and income quintiles (μ_{qj}) control for time-invariant vehicle quality correlated with fuel economy. For example, a luxury brand (Infiniti) may offer lower fuel economy than the standard brand (Nissan), and the fixed effects control for time invariant differences in quality between Infiniti and Nissan.[11]

[11] The fuel cost coefficient is identified by variation over time and within brand caused by fuel costs. Klier and Linn (2010), Busse et al. (2013), and others use within-vehicle variation in fuel prices, where vehicle may be defined as a model, trim, or something else. Unfortunately, this is not possible with CEX data because we observe only the brand and class, not model or trim. As noted below, the demographics control for changes in vehicle attributes or quality correlated with demographics.

The fuel cost coefficient, β_{fc}, should be negative. Because fuel economy used to calculate fuel costs does not vary over time, variation in fuel costs arises from time series variation in fuel prices interacting with cross sectional variation in fuel economy. The quintile by year fixed effects (τ_{qt}) in equation (3) play an important role in the identification of the fuel cost coefficient. For example, suppose fuel prices increase between one year and the next. The price increase raises fuel costs more for vehicles with low fuel economy than for vehicles with high fuel economy.[12] Consequently, consumer demand shifts from low to high fuel economy vehicles, and fuel costs have a negative effect on vehicle purchases. Importantly, the negative coefficient does not imply that when gasoline prices increase, the purchases of all vehicles decline; rather, the coefficient implies a decline in the purchases of high fuel cost vehicles relative to low fuel cost vehicles. These changes are measured relative to the mean change, which is absorbed by the year-quintile fixed effects. Because the fuel cost coefficient is identified by both fuel economy and fuel price variation, it reflects the average effect of fuel costs on prices including both sources of variation.

If we included a single fuel cost variable in equation (3), we would be assuming a linear relationship between fuel costs and log purchases across all vehicles. However, Busse et al. (2013) find that used vehicle purchases respond less to gasoline prices than do new vehicle purchases, which they argue is due to the different supply conditions across used and new vehicle markets. We allow for the possibility that fuel costs affect used vehicle purchases differently from new vehicle purchases by adding two variables: the interaction between fuel costs and an indicator variable for new vehicles ($f_{qjt} * N_j$) and the interaction between gasoline prices and the new vehicle indicator variable ($p_{qt} * N_j$). The fuel cost interaction allows for a different proportionality between new and used vehicles in the fuel cost–price relationship. The estimates in Busse et al. (2013) suggest that the coefficient on the fuel cost interaction term should be negative, indicating that fuel costs have a more negative effect on new vehicle purchases than they do on used vehicle purchases. The fuel cost interaction is identified by variation in fuel costs across new vehicles. The interaction of fuel prices and new vehicles is an estimate of the average effect of fuel prices on new vehicle purchases, relative to used vehicles. The main effect of the fuel price variable is collinear with the year-quintile fixed effects and is omitted from the regression.

The controls in X_{qjt} include vehicle age category fixed effects, demographics, unemployment rates, and consumer confidence. The age category fixed effects control for time-invariant differences in vehicle quality across age groups. The demographics are computed by vehicle age category, income quintile, and year. We construct the demographics by vehicle age category rather than by vehicle to avoid using vehicle purchases to weight the household demographics, in which case the weights would be mechanically related to the dependent variable. Notwithstanding this concern, the results are similar if we compute vehicle-specific demographics.

[12] For example, if a consumer drives 10,000 miles per year, a $1 per gallon gasoline price increase would raise gasoline expenditures by $500 if the consumer's vehicle achieves 20 mpg and by half as much ($250) if the vehicle achieves 40 mpg.

Demographics include indicator variables for urban households, married respondents, retired respondents, and employed respondents, as well as fixed effects for respondent age quintiles, family size (topcoded at 5), number of children (topcoded at 4), population size quintiles of the primary sampling unit, number of vehicles (topcoded at 4), and education category. The demographics control for cross-household differences in vehicle preferences, bargaining power, or dealer price discrimination, as well as for the possibility that variation over time and across regions in macroeconomic conditions and other factors affects the demographic composition of vehicle buyers. We also include interactions of the demographics with indicator variables for light trucks and for new vehicles, which allows these changes in preferences to vary differentially across vehicle types.

We estimate a version of equation (3) that imposes the restrictions that all stringency and fuel cost coefficients are the same across income quintiles. These coefficients therefore represent the average effects across quintiles. We also estimate a version of equation (3) that allows the coefficients to vary across income quintiles, to consider whether members of different income groups respond to vehicle standards differently from one another.

In equation (3), the identifying assumption is that after controlling for the other independent variables, unobserved supply or demand shocks are uncorrelated with stringency. The interactions of vehicle fixed effects with income quintile fixed effects control for time-invariant vehicle quality and other attributes for each income quintile. For example, they allow for the possibility that high-income groups have higher valuation of a luxury brand's quality than do low-income groups. Importantly, the stringency variables are constructed using the vehicle's initial level of fuel economy, because of which changes in fuel economy caused by unobserved demand or supply shocks would not bias the estimates.

The demographics control for demand or supply shocks that are correlated with demographics. For example, larger households typically have higher demand for minivans than do other households (Klier et al. 2017), because of which the demographics in equation (3) control for demand shocks for minivans that are correlated with changes in household size. The demographics also control for changes in vehicle attributes, vehicle quality, and model entry and exit, which may be correlated with demographics.

Equation (3) includes interactions of demographics with indicators for light trucks and new vehicles. These interactions allow changes in demand correlated with demographics to vary across cars and light trucks and across new and used vehicles. Returning to the minivan example, the estimates would not be biased even if demand for minivans among new vehicle buyers declines relative to demand among used vehicle buyers.

The unemployment and consumer confidence controls address aggregate demand shocks correlated with the macro economy. For example, an economic downturn could reduce demand for new vehicles, which would not bias the stringency coefficients because equation (3) includes interactions of the unemployment and consumer confidence controls with indicator variables for new vehicles.

Because unobserved vehicle quality is likely to be positively correlated with purchases and prices, if quality were correlated with stringency, we would expect purchases and prices to be correlated with stringency. For that reason, it is reassuring that correlations among purchases, prices, and stringency are weak (see Figure 3). Of course, we cannot test whether stringency is

correlated with unobserved vehicle quality. However, in Section 3.3, we provide several additional pieces of evidence that the main results are unlikely to be biased by unobserved quality shocks.

3.2. Coefficient Estimates

This subsection presents the coefficient estimates and discusses their statistical significance; Section 4 discusses economic significance. Table 3 presents the main estimation results, reporting coefficients on stringency, fuel costs, and gasoline prices from equation (3). In column 1, we report the first estimation, in which we estimate the equations while imposing the restriction that each coefficient does not vary across income groups. For columns 2 through 6, we report a second estimation, in which we reestimate equation (3) and allow the key coefficients to vary across income groups. Columns 2 through 6 report the coefficients for the income quintile indicated in the column heading. Note that columns 2 through 6 report estimates from a single regression that includes all observations from column 1. Both regressions in Table 3 include the additional independent variables indicated in the notes to Appendix Table 1. Standard errors are clustered by vehicle and quintile.[13]

In column 1, higher stringency (S_{qjt}) increases purchases of that vehicle (significant at the 10 percent level). In columns 2 through 6, new vehicle stringency increases purchases of new vehicles for the lowest income group and reduces purchases of new vehicles for the highest income group.

In column 1, higher average stringency of new vehicles ($\bar{S}_{qct} * U_j$) increases used vehicle purchases. Because of the definition of the dependent and stringency variables, the coefficient implies that on average, a 1 percent increase in the average fuel economy requirement for all new vehicles increases purchases of each used vehicle by 0.61 log points, which is significant at the 1 percent level.

When we estimate the effect of $\bar{S}_{qct} * U_j$ on purchases by income group, the effect generally diminishes with income. The coefficient estimates for the two lowest income groups are statistically significant at the 10 percent level. Purchases of used vehicles respond more to $\bar{S}_{qct} * U_j$ for low-income households than for other households, except in the case of the highest income group, for which an increase in mean stringency reduces used purchases (the estimate is small and is not statistically significant).

Next, we discuss the interpretation of the coefficients on S_{qjt} and $\bar{S}_{qct} * U_j$. The magnitude of the $\bar{S}_{qct} * U_j$ coefficient diminishes across income groups, suggesting that lower-income households may consider new and used vehicles to be closer substitutes than do higher-income households.

[13] To allow the coefficients to vary across income quintiles, we include the main effects of the fuel cost, gasoline price, and stringency variables reported in the tables, as well as the interactions of the variables with a set of fixed effects for income quintiles two through five. In this specification, the coefficients on the main effects are reported in column 2 of Table 3, and they represent the effect of the variable on purchases for the first income quintile. For the other quintiles, the effect of each variable on purchases is equal to the main effect plus the corresponding interaction term. Appendix Table 1 reports the estimated main effects and interaction terms. Columns 3 through 6 in Table 3 report the sums of the main effects and interaction terms, with standard errors in parentheses.

Care must be taken when comparing the two stringency coefficients with one another, because both coefficients are positive. To make this comparison, we consider a hypothetical increase in the stringency of all new vehicles in the sample. The coefficient on $\bar{S}_{qct} * U_j$ is larger than that on S_{qjt}, which indicates that the higher stringency causes used vehicle purchases to increase relative to new vehicle purchases. However, because equation (3) includes interactions of income quintile and year, the regression holds fixed total new and used purchases by income quintile and year. The only way that used purchases can increase relative to new purchases, and total new plus used purchases are unchanged, is for total used purchases to increase and total new purchases to decrease. Therefore, the stringency coefficients indicate that an increase in new vehicle stringency raises used vehicle purchases and reduces new vehicle purchases. In other words, in our data, an increase in stringency causes the interactions of income quintile and year to decrease (not reported). The combined effect of this decrease and the positive stringency coefficient is negative, meaning that higher stringency reduces new vehicle purchases.

When S_{qjt} changes over time, the magnitude of the stringency increase varies across new vehicles, being larger for some vehicles than for others. The positive coefficient on S_{qjt} implies that purchases of new vehicles with a larger increase in stringency rise *relative* to vehicles with a smaller increase in stringency. Thus, purchases of all new vehicles decrease, and the magnitude of the decrease is smallest among vehicles for which the stringency increased the most.

This discussion raises the question as to why purchases fall by a smaller amount for new vehicles experiencing a large stringency increase between one year and the next. Recall that higher stringency incentivizes manufacturers to adopt fuel-saving technology, which raises the vehicle's fuel economy as well as its production costs. Purchases of that vehicle increase proportionately to consumer demand for fuel economy and inversely proportionately to the cost increase. Therefore, it may be the case that vehicles experiencing larger stringency increases tend to have higher consumer demand or lower cost increases.

As noted above, tighter stringency can cause manufacturers to raise prices of vehicles with low stringency. This would cause a negative coefficient on S_{qjt}. However, in practice, Klier and Linn (2012) conclude that adjusting vehicle prices and sales mix is an expensive compliance measure for the US standards, and Reynaert (2017) finds that manufacturers adjust sales mix relatively little in response to European carbon dioxide emissions standards for new passenger cars. Thus the fact that we do not estimate a large and negative coefficient for new vehicles is consistent with prior studies.

To interpret the fuel cost and gasoline price coefficients, we need to consider them jointly. In column 1, the gasoline price interaction coefficient is positive and statistically significant at the 1 percent level. This result indicates that an increase in gasoline prices raises the share of new vehicles in total purchases. The fuel cost coefficient is small and is not statistically significant, which indicates that an increase in gasoline prices does not affect relative market shares within used vehicles; in other words, an increase in gasoline prices causes a proportional reduction in purchases across all used vehicles. The fuel cost interaction coefficient is negative and statistically significant, which indicates that an increase in gasoline prices causes consumers to shift from new vehicles with low fuel economy to new vehicles with high fuel economy. This result is consistent with Leard et al. (2017a) and the

results for new and used vehicles are consistent with Busse et al. (2013). Putting these results together implies that an increase in gasoline prices between one year and the next causes a proportional shift from used to new vehicles, and a shift within new vehicles toward vehicles with high fuel economy.

Columns 2 through 6 also show that this pattern holds for all income groups, which has not been documented previously. Moreover the lowest three income groups—especially the second and third—respond more to fuel costs than do the upper two income groups. This result is consistent with the finding that lower-income groups respond more to stringency than do higher-income groups, in that both the fuel cost and stringency results suggest that low-income households consider new and used vehicles to be closer substitutes for one another than do high-income households.

3.3. Robustness to Adding Further Controls

As noted in Section 3.1, the central identification assumption is that the two stringency variables (S_{qjt} and $\bar{S}_{qct} * U_j$) are uncorrelated with unobserved demand and supply shocks, after controlling for fuel costs, demographics, macroeconomic conditions, and time trends as in equation (3). This subsection provides evidence supporting this assumption.

First, we consider omitted variables that may be correlated with the two variables that determine a vehicle's stringency: footprint and fuel economy. Because stringency increases over time, trends in preferences for these attributes or the supply of these attributes could bias our results. For example, Figure 3 shows a mildly positive correlation between footprint and stringency. Because changes in stringency over time are correlated with the vehicle's initial footprint, if demand for footprint changes over time, such as an increase in demand for larger vehicles, changes in stringency over time could be correlated with changes in demand for footprint. A similar argument would pertain to fuel economy, which is negatively correlated with stringency. Although we cannot observe changes in demand for fuel economy and stringency, we can allow for the possibility that these changes occur linearly over time by interacting each vehicle's footprint and fuel economy with linear time trends. With these controls included, discrete changes in stringency identify the stringency coefficients. Columns 2 and 3 of Table 4 show that the coefficient on $\bar{S}_{qct} * U_j$ remains statistically significant, and the magnitude is broadly similar to the baseline.

Second, we consider the possibility that changes in demand for used vehicles over time are correlated with changes in the stringency variables. As before, we focus on demand for footprint and fuel economy. Columns 4 and 5 include interactions of footprint or fuel economy with a linear time trend, vehicle class, and an indicator for whether the vehicle is new. (All lower-order interaction terms are also included in these regressions.) These interactions allow for differential trends in demand across class and whether the vehicle is new or used. For example, these specifications allow for the possibility that among consumers buying used light trucks, demand for footprint or fuel economy has changed over time, both in absolute terms and relative to demand among consumers buying used cars or those buying new cars or trucks. The coefficients on the used vehicle stringency variable are larger than the baseline and remain statistically significant.

Third, we consider the potential cross-sectional correlation between vehicle quality and stringency. Figure 3 showed that stringency is weakly correlated with vehicle prices (which are positively correlated with quality). The weak correlation does not rule

out the possibility that quality is correlated with stringency. As an additional test, we add placebo measures of stringency to the baseline, which are equal to the vehicle's stringency in the year 2016 (i.e., we construct a separate placebo measure for S_{qjt} and $\bar{S}_{qct} * U_j$). These placebo variables do not vary over time, but they do vary cross-sectionally. Therefore, if stringency were correlated with quality in the cross section, adding these variables would affect the main estimates. Column 6 of Table 4 shows that the stringency coefficients are larger than the baseline and are statistically significant at the 1 percent level. The larger coefficients imply that stringency may be negatively correlated with quality in the cross section, implying that the baseline estimates understate the effects of stringency on purchases. Nonetheless, the placebo stringency variables do not suggest that the baseline estimates are spurious.

Finally, we allow for the possibility of more flexible consumer responses to fuel costs than in the baseline. Busse et al. (2013) show that responses to gasoline prices depend on fuel economy. The baseline specification allows fuel cost responses to vary across new and used vehicles but imposes the assumption that those responses are inversely proportional to fuel economy within new and used vehicles. Because fuel economy is correlated with stringency, failing to account for heterogeneous responses to fuel costs could bias the stringency coefficient estimates. However, column 7 suggests that this is not a major concern, as the main results are not affected by interacting both fuel cost variables with fixed effects for the vehicle's fuel economy quartile. (Busse et al. 2013 similarly use fuel economy quartiles.)

Thus the $\bar{S}_{qct} * U_j$ coefficient does not appear to reflect spurious correlations among stringency, proxies for supply and demand shocks, and quality in the cross section. Moreover, allowing more flexible consumer responses to fuel costs than in equation (3) yields similar results. If anything, these estimates suggest that the baseline estimate in column 1 may underestimate the effects of stringency on used vehicle purchases.

4. Welfare Implications of Substitution between New and Used Vehicles

The previous section showed that tighter stringency reduces purchases of new vehicles and increases purchases of used vehicles. In this section, we discuss the economic significance of the coefficients and implications for benefits and costs of tighter vehicle standards.

4.1. Interpreting the Magnitudes of the Stringency and Fuel Cost Coefficients

In this subsection, we discuss three ways of interpreting the magnitudes of the stringency and fuel cost coefficient estimates in Table 3. The interpretations suggest economically large effects of stringency on vehicle purchases, as well as economically large effects of fuel costs on new vehicle purchases. The magnitudes of the fuel cost coefficients are similar to those reported elsewhere in the literature.

First, we consider the effects of a hypothetical tightening of the fuel economy requirements. For the new vehicles in the sample subject to the new fuel economy standards (that is, after 2004 for light trucks and after 2011 for cars), we increase each vehicle's fuel economy requirement by 0.1 percentage points, or about 0.2 mpg. For other new vehicles in the sample, the stringency variable does not change. This definition of the counterfactual mimics the variation that identifies the stringency coefficients. Higher fuel economy requirements cause both

stringency variables to increase.[14] In these simulations and those in Section 4.2, we use the coefficient estimates from column 1 of Table 3. We use the changes in both stringency variables to compare predicted vehicle purchases across the observed and counterfactual scenarios.[15]

In Table 5, the first row of Panel A reports the fuel economy change caused by higher fuel economy requirements. This calculation includes two effects, the first of which is the higher fuel economy of new vehicles caused by the higher requirements. The second effect includes shifts in vehicle purchases caused by the higher requirements. Accounting for both effects, we find that tightening fuel economy requirements by 0.1 percent raises average fuel economy across the full sample by 0.03 percent. Recall that the higher requirements affect only a subset of vehicles in the sample, which largely explains why the overall fuel economy increase is so much smaller than the increase of the fuel economy requirement.

The second row shows that the higher fuel economy requirement reduces the share of new vehicles in total vehicle purchases. New vehicle purchases decline by 0.009 percent, or about 1,400 units. In the next subsection, we quantify the effects of the lower number of purchases on manufacturer profits.

Second, we consider the effects of a hypothetical gasoline price increase. Starting from the gasoline prices observed in the sample, we suppose that gasoline prices throughout the sample had been $0.10 per gallon higher. The price increase affects the entire sample for consistency with the estimation of the fuel cost coefficients, which rely on price variation throughout the sample. Panel B of Table 5 reports the results. We use the estimated coefficients from equation (3) to predict counterfactual vehicle purchases given the higher counterfactual gasoline prices. Specifically, the predictions depend on the coefficients on fuel costs, the interaction of fuel costs with a new vehicle dummy, and the interaction of the gasoline price with a new vehicle dummy. We compare these predicted vehicle purchases with the predicted vehicle purchases using observed gasoline prices.

Panel B of the table reports substantial effects of gasoline prices on vehicle purchases. The first row shows that higher gasoline prices raise the average fuel economy of all vehicles sold by about 0.03 percent. Although not reported, the increase in average fuel economy of new vehicles (0.02 percent) is similar to that reported in Leard et al. (2017a), after adjusting for the fact that they consider larger price changes. The higher fuel economy arises from shifting purchases of

[14] The fuel costs of each vehicle do not change in these simulations. This is because the fuel cost coefficient is computed using the vehicle's initial fuel economy. As noted in Section 3.1, this construction implies that changes in fuel economy over time caused by changes in stringency do not affect the fuel cost variable and therefore do not identify the fuel cost coefficient. Therefore, for consistency with the estimation, we hold the vehicle's fuel costs fixed in the stringency simulations. Note that if we estimated a dynamic model rather than equation (3), we could account for changes in fuel economy of both new and used vehicles caused by changes in stringency.

[15] For the counterfactuals in this subsection and in 4.2, we renormalize the predicted counterfactual purchases so that total purchases of new and used vehicles are the same as the total predicted purchases using the observed stringency levels. We assume full compliance with the tighter stringency so that all new vehicles affected by the stringency change increase their fuel economy 0.1 percent. Relaxing this assumption would require modeling manufacturer responses to stringency, which lies outside the scope of this paper.

new vehicles to higher fuel economy brands, as well as shifting from used to new vehicles. The second row shows that the gasoline price increase causes new vehicle purchases to increase by about 0.8 percent overall and by varying amounts for each income group. Thus the estimated fuel cost and gasoline price coefficients suggest economically important effects of gasoline prices on vehicle purchases.

Third, we consider the effects of fuel costs on new vehicle purchases. The point estimate for the full sample in Table 3 suggests that a 1 percent increase in a vehicle's fuel costs reduces purchases of the vehicle by about 1 percent. Leard et al. (2017a) use a similar time period but different data and report similar magnitudes.[16] Thus the estimated effects of fuel costs are similar to those reported in other papers that have used different data and identification strategies. This similarity supports our identification strategy to the extent that the fuel cost coefficients do not appear to be biased by unobserved supply or demand shocks.

4.2. Implications for Costs and Benefits of Fuel Economy Standards

Using the EPA and NHTSA benefit and cost estimates of the 2012–16 standards as a starting point, in this subsection we estimate the changes in benefits and costs implied by the decrease in aggregate new vehicle purchases caused by higher stringency. In EPA and NHTSA (2010), three categories of benefits and costs account for nearly all the total costs and benefits: (a) the benefits to consumers of lower fuel expenditures; (b) the benefits to society of lower GHG emissions; and (c) the costs of adding fuel-saving technologies to vehicles. Implicitly, the agencies assumed that standards do not affect driving or scrappage of vehicles that have already been sold. Under this assumption, the benefits of tighter standards are directly proportional to total new vehicle purchases: the larger the number of vehicle purchases, the larger the benefits.

Like the agencies, we assume that standards do not affect driving of vehicles that have already been sold prior to the standards. According to our estimates, some households buy new vehicles with observed but not counterfactual standards. We assume that in the counterfactual, these households choose vehicles with the same level of fuel economy they are observed to choose. Under these assumptions, the aggregate benefits of the standards are directly proportional to aggregate new vehicle purchases.

The first assumption, which is that standards do not affect driving of vehicles that were sold previously, may be strong. Nonetheless, it permits a direct comparison with the agencies' analysis of the standards that manufacturers faced during our sample period. The second assumption is necessary for performing the calculation, but in practice it has a negligible effect on the results. The reason the results are so insensitive to the second assumption is that the change in new vehicle purchases caused by the change in standards is small relative to the aggregate new vehicle purchases.

Unlike the counterfactual in the previous subsection, which raised fuel economy requirements for new vehicles in multiple years, in this subsection we consider a

[16] Leard et al. (2017a) use data from Wards Auto. The data measure national new vehicle sales by model and power type (for example, distinguishing plug-in and gasoline versions of the Ford Fusion). Relative to CEX data, the Wards data are less aggregated across vehicles and more aggregated across demographic groups, suggesting that the most important consumer substitution margin is between new and used vehicles rather than between purchasing and not purchasing vehicles.

hypothetical 0.1 percent increase for new vehicles sold in 2016 only. This setup is similar to the EPA and NHTSA analysis of the 2012–16 standards, which estimates benefits and costs for new vehicles sold in each of those years. The higher fuel economy requirements reduce new vehicle purchases in 2016 by 0.02 percent, or about 4,000 units.

Table 6 reports the estimated benefits from the higher fuel economy requirements. The fuel cost and GHG benefits are computed using EPA and NHTSA (2010) assumptions on miles traveled and scrappage rates, which derive from Lu (2006). For simplicity we assume that costs of raising fuel economy are incurred in the 2016 model year. The fuel savings and GHG emissions benefits occur over the lifetimes of the vehicles. To compute the net present value of the benefits we use a 3 percent discount rate and the 2015 US government estimates of the social cost of carbon. Whereas the agencies use projections of fuel prices made in 2011, we use projections from 2016, which are substantially lower. The first row shows benefits assuming that tighter standards do not affect new vehicle purchases, and the second row shows benefits using the estimated 0.02 percent reduction in purchases. As the bottom row shows, accounting for the lower number of vehicle purchases reduces benefits by 0.02 percent, a negligible amount. The fact that this percentage equals the percentage of reduction in purchases is by assumption; nonetheless, it is clear that because the change is so small, relaxing these assumptions would yield small changes in benefits compared with the benefits in the first row.

Under the same two assumptions, higher fuel economy requirements affect costs in two ways. First, higher requirements raise average production costs per vehicle. Based on the cost estimates in EPA and NHTSA (2010), Leard et al. (forthcoming) compute that increasing fuel economy requirements by 0.1 percent would raise average per-vehicle production costs by $9. Because 2016 new vehicle sales were about 17 million units, the estimated cost of adopting technology is about $157 million.

The second cost implication of the reduction in vehicle purchases is that manufacturers earn lower profits by selling fewer vehicles. Manufacturers typically redesign vehicles every five to seven years. Each manufacturer staggers the redesigns of its vehicles over time, so that in any particular year it is redesigning only a subset of its vehicles. During the redesign, manufacturers incur fixed (largely sunk) costs of designing, testing, and producing vehicles with new technologies. In a conventional model of imperfect competition with free entry and exit, manufacturers can recover the fixed costs by charging a markup over marginal costs. Long-run profits are zero because of free entry and exit.

The decline in aggregate new vehicle purchases caused by higher fuel economy requirements reduces profits. Assuming Bertrand pricing and an average own-price elasticity of demand equal to –5 implies a 20 percent average markup. Using the observed vehicle prices in our data, the lower number of purchases reduces annual profits by about $23 million, or 15 percent of the technology

costs.[17] Thus the decline in aggregate purchases reduces benefits marginally and raises costs by 15 percent, implying a 26 percent reduction in net benefits. Importantly, Table 6 shows that net benefits remain positive even after considering these effects.[18]

However, the lower profits occur only in the short run. The lower profits cause manufacturers to reduce fixed costs and remove vehicles from the market so that long-run profits return to zero. For example, suppose the standards tighten and manufacturers begin adding fuel-saving technology, which raises vehicle prices and reduces equilibrium sales. Suppose further than a manufacturer decides to remove a low-selling version of a model from the market. Because most fixed costs are incurred when the vehicle is redesigned, the manufacturer may remove the vehicle at the time it would have been redesigned. This action would reduce the manufacturer's fixed costs (since the redesign is avoided), and it could increase sales of competing vehicles.[19] Given redesign cycles of five to seven years and typical lead times of two years for setting new vehicle standards, exit is likely to occur within five years of the tighter standards. That is, the reduction in profits persists for up to five years.

Note that in their benefit-cost analysis, the agencies assume that manufacturer profits are zero, even in the short run. In their most recent analysis of standards, EPA and NHTSA (2018) assume that tighter standards reduce aggregate sales, but they do not consider the short-run reduction in profits. Consequently, it is appropriate to add the forgone profits to the costs they include in their analysis.

Jacobsen and van Benthem (2015) estimate the reduction in benefits of tighter standards caused by delayed scrappage. As noted in the introduction, their estimates are based on observed scrappage decisions, combined with a calibrated model of consumer choice. Our estimates are based on observed new and used vehicle purchases, combined with assumptions on vehicle utilization. Our estimates complement theirs; the lower benefits they attribute to delayed scrappage are added to the additional costs we attribute to the decline in aggregate new vehicle purchases.

[17] This calculation includes the sales reduction calculated above. The assumed elasticity is similar to that reported elsewhere in the literature, such as by Berry et al. (1995). The decrease in profits is proportional to the assumed markup, and a smaller elasticity (in absolute value) would imply a higher markup and a larger decrease in profits. The calculation also assumes that the tighter standards do not affect the average markup. Given the magnitude of the change in standards considered here, it is unlikely that the markup would change sufficiently to appreciably affect the main conclusions.

[18] In setting standards through the year 2021, NHTSA (2012) assumed that standards do not reduce aggregate purchases. As noted in the introduction, NHTSA argued that if anything, tighter standards increase aggregate new vehicle purchases. The agency reasoned that tighter standards generate discounted fuel cost savings that exceed the vehicle price increases, raising the value of a new vehicle to consumers. In more recent analysis, EPA and NHTSA (2018) estimate that a $1,000 increase in average vehicle prices immediately reduces aggregate new vehicle purchases by 170,000 units, and further reduces purchases over subsequent years. This estimate includes other factors affecting average vehicle prices besides fuel economy standards, such as improving safety and entertainment features, and cannot be compared directly with the estimates in this paper, which include only the effects of fuel economy on aggregate new vehicle purchases.

[19] In this example, if the increase in sales for the manufacturer's competing version is sufficiently large, the manufacturer may decide to remove the low-selling vehicle from the market prior to redesign. This behavior does not affect the conclusion that lower profits are likely to persist for no more than 5 years.

5. Conclusions

Leveraging recent changes in fuel economy and GHG standards, we have provided the first direct evidence of the effects of standards on vehicle purchases. Recently tightened standards have reduced aggregate new vehicle purchases. In particular, a 0.1 percent increase in new vehicle standards reduced new vehicle purchases in the year 2016 by 0.02 percent. EPA and NHTSA (2010) find that nearly all benefits are directly proportional to aggregate new vehicle purchases, implying that the decline in new vehicle purchases marginally reduces benefits. The lower number of purchases reduces short-run vehicle manufacturer profits, which raises the costs of the standards by about 15 percent. The higher costs do not overturn the conclusion that marginally tightening the 2016 standards would have raised social welfare.

Importantly, the increase in costs is a short-run effect that arises because of sunk costs of vehicle design, production, and purchases. As we argued in Section 4, the short run may persist for up to five years. Because EPA and NHTSA do not include the forgone profits in their benefit-cost analysis, their costs should be adjusted upward by 15 percent in the first year of tighter standards. However, because this is a short-run effect, the proportion of forgone profits in total costs declines with the time horizon considered in the benefit-cost analysis.

Note that standards may delay purchases of new vehicles, which would reduce the long-term implications of our results in addition to the reasons just discussed. Future work could examine this possibility, such as by comparing consumer behavior in periods of stable standards that follow periods of tightening standards.

This paper assesses the effects of passenger vehicle fuel economy and GHG standards on purchases of new and used vehicles. Given the finding that tighter standards raise demand for used vehicles, standards may affect prices of new and used vehicles. Such price effects would have further welfare implications beyond those discussed in this paper. Although the vehicle purchases in the CEX match data from other sources closely, unfortunately the CEX vehicle prices do not match closely. This measurement error precludes an analysis of vehicle prices using CEX data. Also outside the scope of this paper is the potential effect of the decline in aggregate vehicle purchases on traffic accidents, given safety differences between newer and older vehicles. We leave such an analysis for future work.

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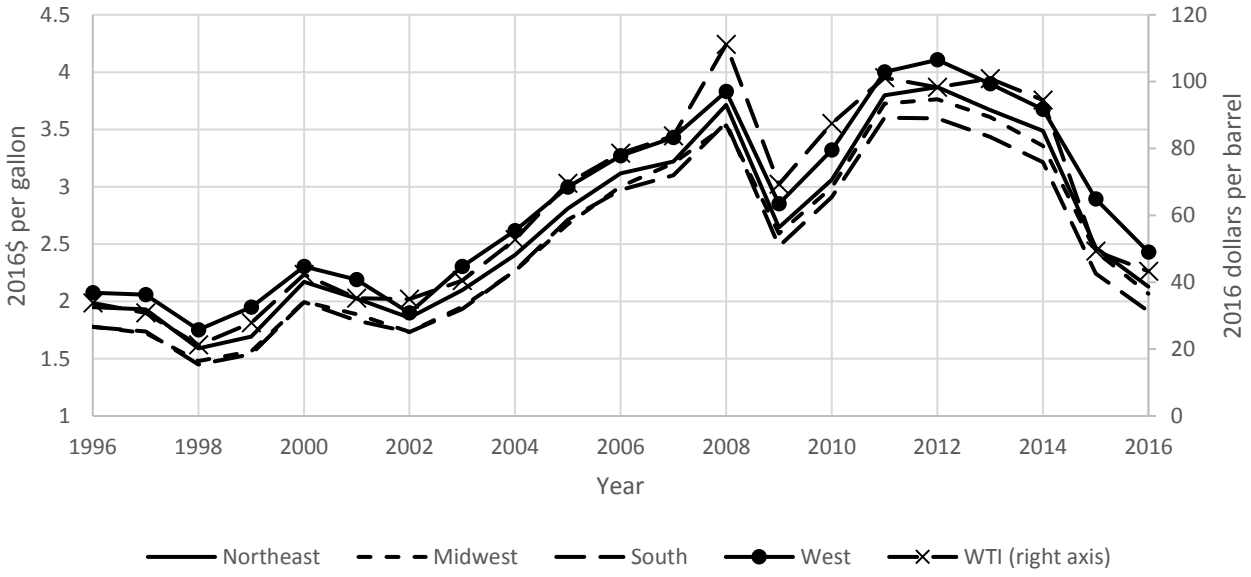
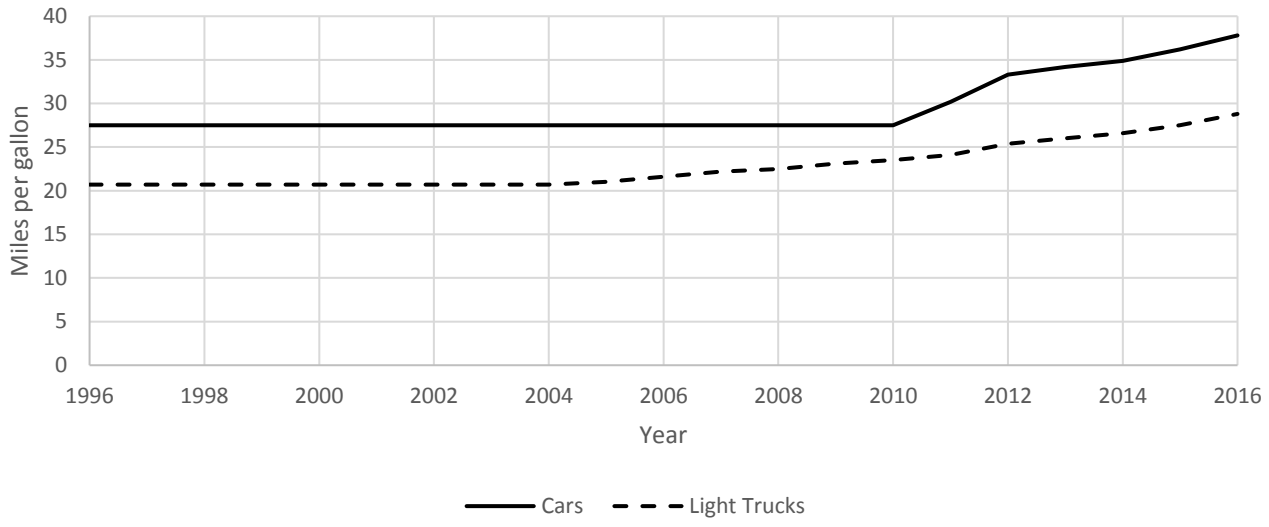
Figure 1. Regional Gasoline Prices and Crude Oil Prices



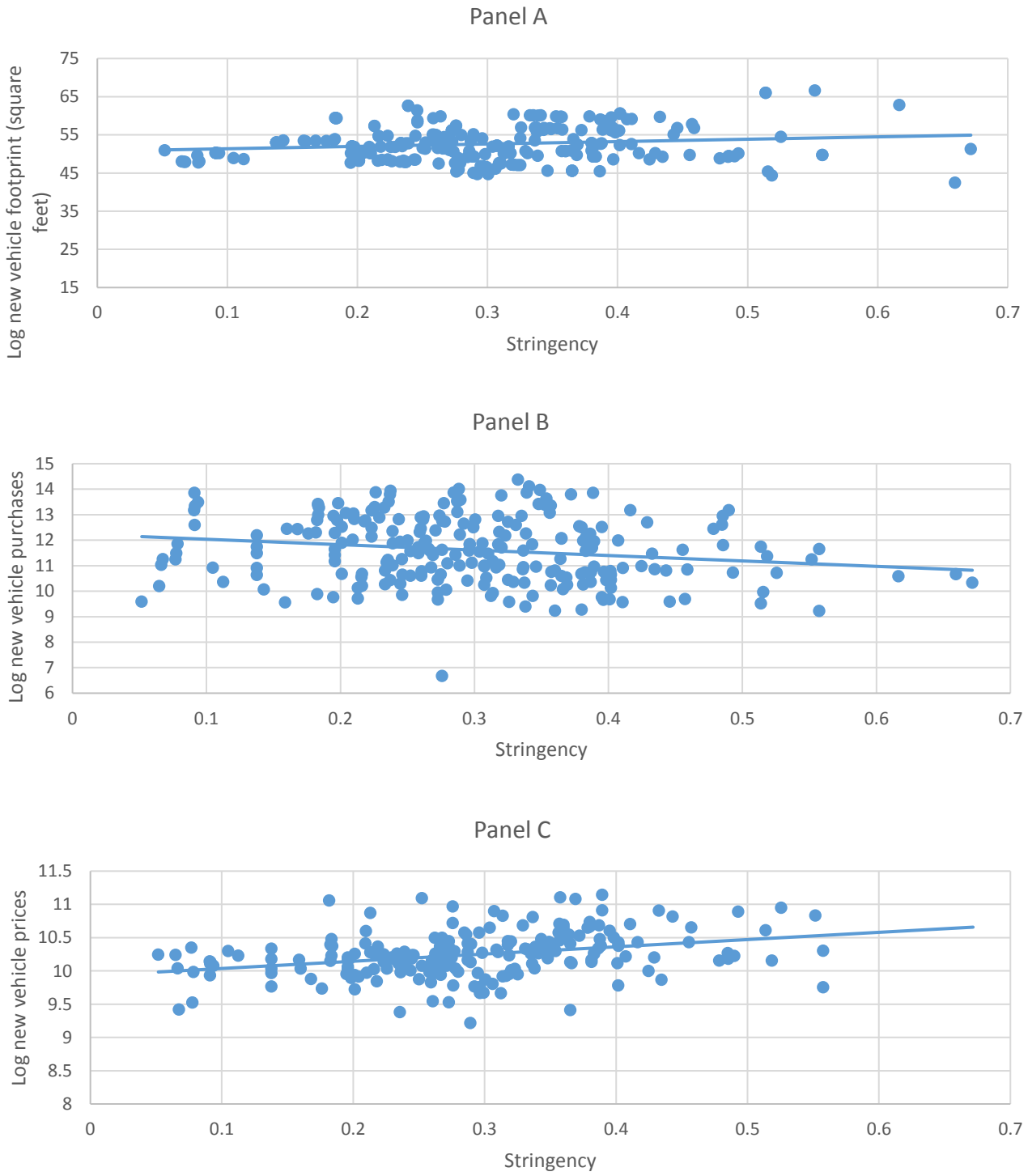
Notes: Gasoline prices are plotted on the left axis, in 2016$ per gallon. Nominal monthly regional prices are from the Bureau of Labor Statistics (BLS) and are converted to 2016$ using the BLS Consumer Price Index (CPI). The prices are merged with the Consumer Expenditure Survey (CEX) household data by the month in which the vehicle was purchased, and annual prices are computed using household survey weights. The West Texas Intermediate (WTI) price is plotted on the right axis in 2016$ per barrel. The annual nominal WTI price is from the Energy Information Administration and is converted to 2016$ using the CPI.

Figure 2. Fuel Economy Standards for Cars and Light Trucks



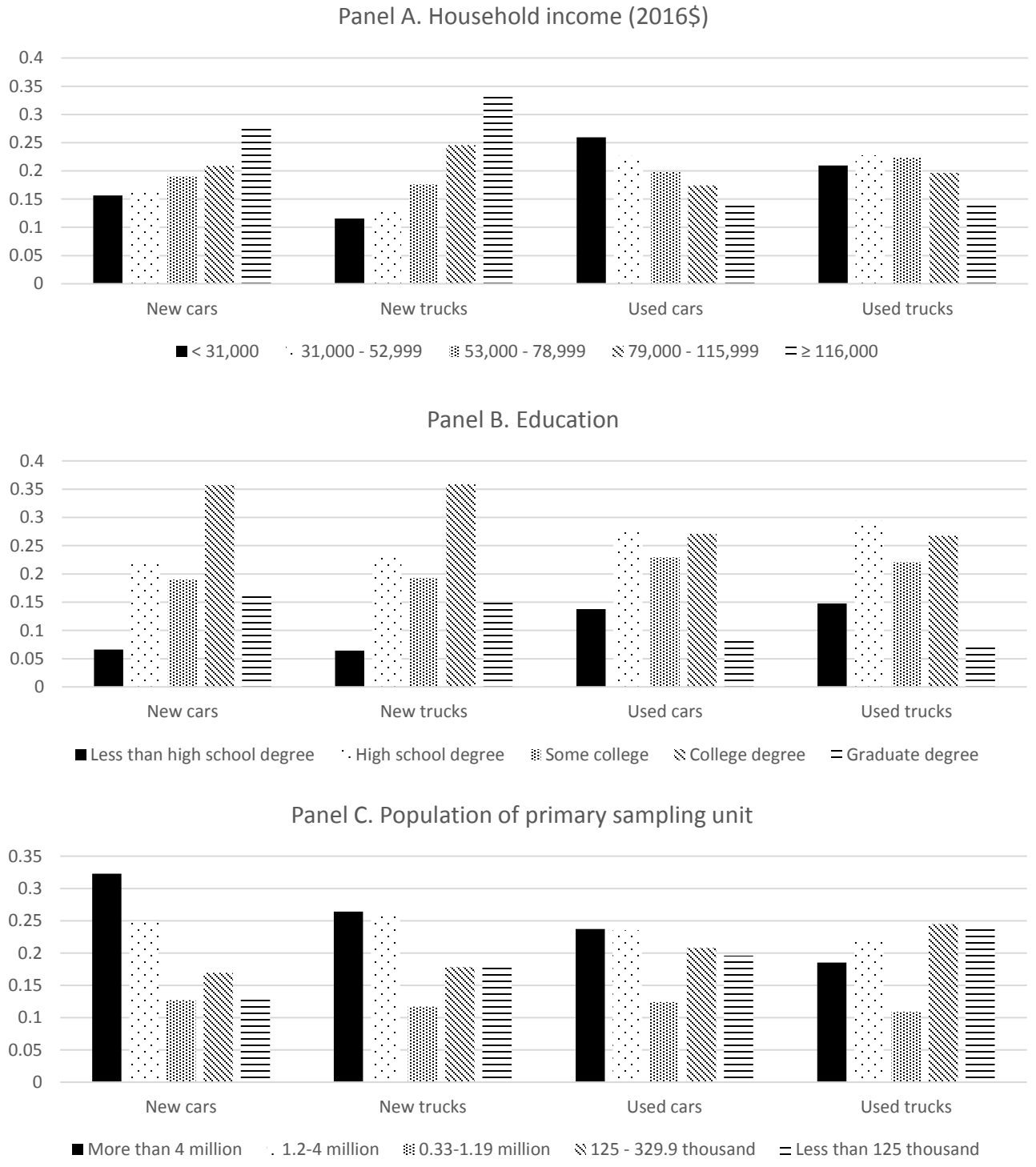
Note: The fuel economy standards for cars and light trucks are from the *Federal Register* , in miles per gallon.

Figure 3. Log New Vehicle Footprint, Purchases, and Prices in 2004 versus Stringency in 2016



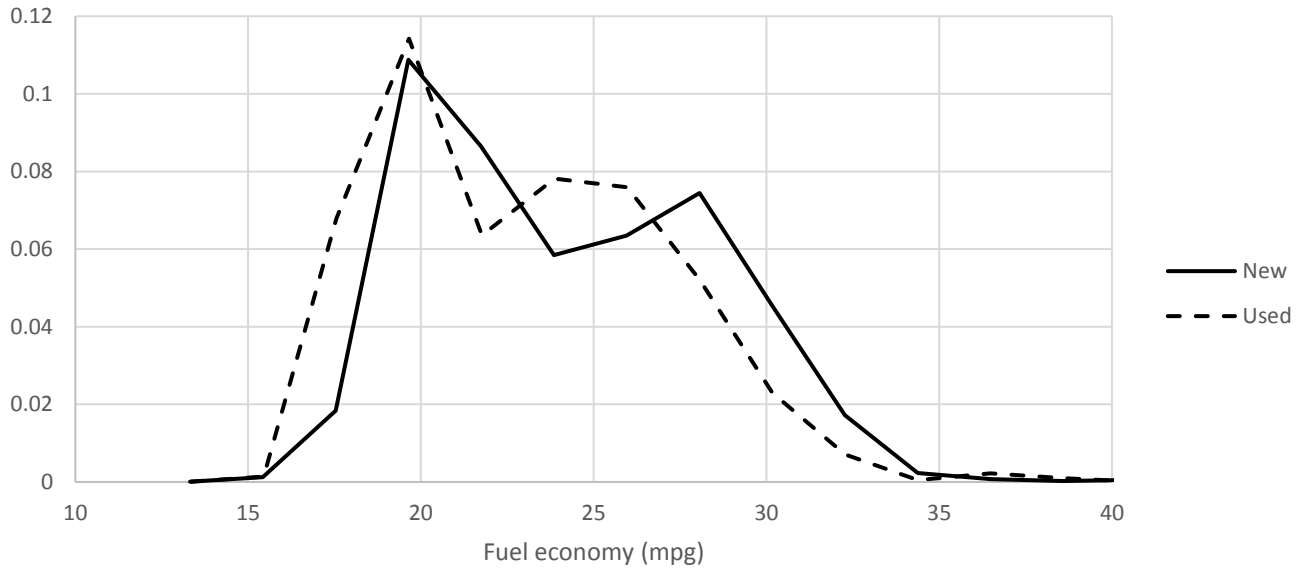
Notes: Each point represents a new vehicle, which is defined as a unique brand, fuel type, and class. Panel A plots the log of footprint against stringency, where stringency is defined in the text. Panel B plots the log of vehicle purchase against stringency. Panel C plots the log of new vehicle prices against stringency. The dashed lines are linear trend lines, which are estimated including a constant term. Footprint, purchases, and prices are measured in 2004, and stringency is measured in 2016.

Figure 4. Shares of Vehicle Buyers by Income, Education, or Population Density



Notes: The figure shows the share of households that belong to the indicated demographic group that purchases the indicated type of vehicle (car or truck, new or used). For example, the leftmost bar in Panel A shows that about 16 percent of new car buyers belong to the lowest income group. In Panel A, income categories are based on the household's income in 2016$. In Panel C, households are assigned to categories based on the population size of the area from which they were sampled.

Figure 5. Estimated Density Functions of New and Used Vehicle Fuel Economy



Note: The figure plots estimated density functions for fuel economy of new and used vehicles purchased between 1996 and 2016.

Table 1. Demographics for New and Used Vehicle Buyers, Cars and Light Trucks

	(1)	(2)	(3)	(4)	(5)	(6)
	New vehicles			Used vehicles		
	Cars	Light trucks	P-value of equality for cars and trucks	Cars	Light trucks	P-value of equality for cars and trucks
	Panel A. Indicator variables (share of households with these characteristics)					
Married	0.67	0.78	0.000	0.57	0.68	0.000
Retired	0.18	0.15	0.000	0.08	0.08	0.009
Employed	0.75	0.78	0.000	0.81	0.80	0.000
Urban	0.92	0.88	0.000	0.89	0.84	0.000
	Panel B. Means of continuous variables					
Age	50.07	48.51	0.000	43.77	43.75	0.851
Family size	2.60	2.90	0.000	2.97	3.21	0.000
Number of children	0.53	0.79	0.000	0.85	1.08	0.000
Number of vehicles	2.52	2.85	0.000	2.63	2.87	0.000

Notes: In Panel A, columns 1, 2, 4, and 5 report the shares of households that have the demographic characteristics indicated in the row heading. Columns 3 and 6 report p-values on a test of equality between the means for car and light truck purchasers. Panel B reports means of the continuous variables indicated in the row columns, with p-values of the corresponding tests in columns 3 and 6.

Table 2. Mean Fuel Economy, New Vehicle Share, and Vehicle Price by Income Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	First (lowest) income quintile	Second quintile	Third quintile	Fourth quintile	Fifth (highest) quintile
Household income range (2016$)		< 31,000	31,000 - 52,999	53,000 - 78,999	79,000 - 115,999	≥ 116,000
Average fuel economy (mpg)	23.75	23.82	23.68	23.63	23.68	23.93
Probability vehicle is new	0.30	0.20	0.22	0.28	0.35	0.48
New vehicle price (2016$)	28,175	26,313	25,406	26,552	27,940	31,416
Used vehicle price (2016$)	12,310	8,720	10,714	12,409	14,051	17,175

Notes: The table reports the sample-weighted means of the variables indicated by the row headings for each income group. The first row provides the income ranges for each income group, in 2016$. The sample includes all households purchasing a vehicle within 18 months of the survey. The probability a vehicle is new is the share of new vehicles in total vehicle purchases. Fuel economy is in miles per gallon, and vehicle price is in 2016$.

Table 3. Effects of Standards and Fuel Costs on Vehicle Purchases

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable is log purchases					
	Full sample	First (lowest) income quintile	Second quintile	Third quintile	Fourth quintile	Fifth (highest) quintile
New vehicle stringency	0.27	0.88	0.60	0.38	0.35	-0.40
	(0.16)	(0.40)	(0.36)	(0.40)	(0.40)	(0.24)
Mean stringency X used	0.61	1.32	0.92	0.56	0.68	-0.07
	(0.21)	(0.45)	(0.41)	(0.48)	(0.51)	(0.41)
Fuel costs	1.06	-0.96	4.49	5.89	-1.40	-1.87
	(1.13)	(2.43)	(2.62)	(2.24)	(2.56)	(2.08)
Fuel costs X new	-8.32	-7.48	-17.12	-11.48	-8.35	-3.93
	(1.18)	(2.14)	(2.36)	(2.39)	(2.52)	(2.49)
Gas price X new	0.47	0.45	0.79	0.58	0.47	0.33
	(0.06)	(0.10)	(0.11)	(0.11)	(0.11)	(0.11)

Notes: The table reports regression coefficients from equation (3) with standard errors in parentheses, clustered by brand, class, and age category. The first column includes the full sample. Columns 2-6 report estimates based on the underlying coefficient estimates reported in Appendix Table 1. The numbers in column 2 are the same as in column 1 of the appendix table, and the numbers in columns 3-6 are the sum of the numbers in column 1 of the appendix table and the numbers in the appendix table with the corresponding income group. Column 1 includes the same unreported independent variables as in the appendix table, and the variable definitions are the same as in that table.

Table 4. Robustness to Adding Further Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	Footprint X trend	Fuel economy X trend	Footprint X trend X new X class	Fuel economy X trend X new X class	Placebo stringency	Interact fuel costs with fuel economy quartiles
New vehicle stringency	0.27	0.34	0.28	0.01	0.71	0.45	0.34
	(0.16)	(0.17)	(0.18)	(0.15)	(0.23)	(0.17)	(0.17)
Mean stringency X used	0.61	0.78	0.61	0.65	1.03	1.11	0.54
	(0.21)	(0.22)	(0.23)	(0.30)	(0.32)	(0.23)	(0.22)

Notes: The table reports regression coefficients with standard errors in parentheses, clustered by brand, class, and age category. The first column repeats the specification from column 1 of Table 3. Specifications in columns 2 through 7 are the same as the baseline except that they include the additional control variables described in the column headings. Columns 2 and 3 include interactions of footprint and fuel economy with a linear time trend. Column 4 includes interactions of footprint, a linear time trend, an indicator for new vehicles, and class. Column 5 is the same as column 4 except using fuel economy rather than footprint. Column 6 includes two stringency placebo variables. The first is the same as new vehicle stringency, except that it does not vary over time. The second is the average of the first, interacted with used vehicles and set equal to zero for new vehicles. Column 7 includes interactions of fuel costs, an indicator for new vehicles, and fixed effects for the vehicle's quartile of fuel economy.

Table 5. Effects of Stringency and Fuel Price Increases						
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Percentage changes caused by 0.1 percent fuel economy requirement increase					
Average fuel economy	0.030	0.024	0.024	0.029	0.035	0.048
New vehcle market share	-0.009	-0.012	-0.010	-0.005	-0.009	-0.008
	Panel B: Percentage changes caused by $0.10 per gallon fuel price increase					
Average fuel economy	0.033	0.047	0.004	-0.026	0.078	0.076
New vehcle market share	0.781	1.041	0.484	0.566	0.766	0.867

Notes: Panel A reports the changes in new and used vehicle prices caused by increasing the fuel economy requirement of new vehicles by 0.1 percent, using coefficient estimates from Table 3. Panel B reports the same outcomes for a simulation of an increase in fuel prices of $0.10 per gallon, using coefficient estimates from Table 3.

Table 6. Implications of Lower New Vehicle Sales for Welfare Effects of Increasing Fuel Economy Requirements by 0.1 Percent

	Fuel cost and greenhouse gas benefits (million 2016$)	Technology costs (million 2016$)	Net benefits (benefits - costs, million 2016$)
Assume no change in vehicle sales	247.43	157.18	90.25
Include lower new vehicle sales	247.37	180.10	67.27
Percent change due to lower sales	-0.02	14.58	-25.46

Notes: The table reports benefits and costs from the stringency scenario considered in Panel A of Table 5. All benefits and costs are in million 2016$. Fuel cost and greenhouse gas benefits are calculated using assumptions on miles traveled and scrappage from EPA and NHTSA (2010), combined with 2016 sales and fuel prices from CEX. Technology costs are computed as in Leard et al. (forthcoming). Net benefits are the difference between benefits and costs. The first row reports benefits and costs assuming no change in new vehicle sales. The second row reports benefits and costs given the change in aggregate new vehicle sales reported in Table 5. The third row reports the percentage changes between the second and first rows.

Appendix Table 1. Purchase Equation Coefficients

	(1)	(2)	(3)	(4)	(5)
		Dependent variable is log purchases			
	First (lowest) income quintile	Second quintile	Third quintile	Fourth quintile	Fifth (highest) quintile
New vehicle stringency	0.88	-0.29	-0.50	-0.53	-1.29
	(0.40)	(0.52)	(0.55)	(0.56)	(0.46)
Mean stringency X used	1.32	-0.40	-0.75	-0.64	-1.39
	(0.45)	(0.58)	(0.62)	(0.68)	(0.59)
Fuel costs	-0.96	5.45	6.85	-0.43	-0.91
	(2.43)	(3.50)	(3.30)	(3.51)	(3.15)
Fuel costs X new	-7.48	-9.63	-4.00	-0.87	3.55
	(2.14)	(3.16)	(3.12)	(3.21)	(3.28)
Gas price X new	0.45	0.34	0.12	0.02	-0.13
	(0.10)	(0.14)	(0.14)	(0.14)	(0.15)

Notes: The table reports coefficients from a single regression (sample size = 16,706). The first column reports coefficients on the variables indicated in the row headings. Columns 2-5 report coefficients on the interaction of the variable in the row heading with an indicator variable equal to one for the income quintile indicated in the column heading. Observations are by brand, fuel type, class, and year. Stringency is the log of the ratio of the model's 2016 fuel economy requirement to its initial fuel economy. Stringency is equal to zero for all used vehicles. To construct mean stringency, we first compute average stringency for the corresponding class, year, and income quintile. The variable is interacted with an indicator for used vehicles (see text for details on the stringency variables). Fuel costs are in 2016$ per mile and are the ratio of the gasoline price to fuel economy. Fuel costs and gasoline prices are interacted with a new vehicle indicator variable. All regressions include interactions of brand, class, fuel type, and income quintile; age category fixed effects; interactions of year and income quintile; and average demographics by vehicle age group and year: income, age, family size, number of children, hours worked, employed, retired, married, urban, regional unemployment rate, and regional consumer confidence. All regressions also include interactions of demographics with a light truck dummy and with a new vehicle dummy.